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REGIONAL REDISTRIBUTION THROUGH THE U.S. MORTGAGE MARKET

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ABSTRACT

Regional shocks are an important feature of the U.S. economy. Households' ability to self-insure against these shocks depends on how they affect local interest rates. In the United States, most borrowing occurs through the mortgage market and is influenced by the presence of government-sponsored enterprises (GSEs). We establish that despite large regional variation in predictable default risk, GSE mortgage rates for otherwise identical loans do not vary spatially. In contrast, the private market does set interest rates that vary with local risk. We use a spatial model of collateralized borrowing to show that the national interest rate policy substantially affects welfare by redistributing resources across regions.

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I Introduction

The Great Recession has led to wide disparities in economic activity across regions within the United States. The extent to which households can borrow to self-insure against these regional shocks depends crucially on the interest rate and how it varies with regional economic conditions. Theoretical models typically assume that regions within a monetary union share a common risk-adjusted interest rate.¹ Yet, there are no papers — of which we are aware — testing whether risk-adjusted interest rates are equated across regions within a monetary union like the United States.

In this paper, we use data on mortgage loans, which represent the bulk of household borrowing, to document two new facts. First, risk-adjusted rates are not equalized across locations within the U.S. monetary union: despite large regional variation in ex-ante predictable default risk, there is no regional variation in mortgage contract rates for loans securitized by government-sponsored enterprises (GSEs). Since GSEs securitize most of the loans in the U.S. mortgage market, this constant *contract rate* in the face of variation in predictable default risk implies that the majority of borrowers face *risk-adjusted rates* which do vary with their locations. Second, this lack of risk-based pricing does not occur because this risk cannot be observed ex ante: we show that otherwise similar non-GSE loans that are securitized in the private market increase (decrease) mortgage rates when ex-ante local default risk rises (falls).

If mortgage rates do not respond to local economic shocks that increase ex-ante default risk, then individuals in those regions face lower borrowing costs than they otherwise would if default risk was priced into interest rates. This reduction in borrowing costs may in turn offset some of the negative local economic shock that increased default risk in the first place. Conversely, individuals in regions with low default risk will face higher borrowing costs than if this low default risk was priced into interest rates. Thus, the constant interest rate “policy” followed by the GSEs results in state-contingent regional transfers. While the first half of our paper concentrates on documenting the constant interest rate policy, the second half of the paper quantifies the size and welfare consequences of these implicit transfers.

Our paper unfolds in three parts. We begin by using detailed loan-level data securitized by the GSEs to show that local characteristics systematically predict future local loan default even after controlling for other observable borrower and loan characteristics. For example, there is medium-run persistence in local default probabilities: conditional on borrower and loan characteristics, regions that experienced higher default rates yesterday are more likely to experience higher default rates tomorrow. These findings hold throughout the entire 2000s and are not limited to the period surrounding the 2008 recession. Despite this finding, we further document that interest rates on loans securitized by the GSEs do not vary at all with this predictable default risk. These patterns hold across different time periods and are robust to many different specifications to predict local mortgage default rates. The

¹The theoretical literature that assumes a constant risk-adjusted (or risk-free) interest rate across regions is extensive. Recent papers making this assumption in the macroeconomics, monetary union, and public finance literatures include: Lustig and Van Nieuwerburgh (2005), Fahri and Werning (2014), Nakamura and Steinsson (2014), Yagan (2014), Zidar (2015), and Beraja et al. (2015).

results are striking. Even though the GSEs charge different interest rates to borrowers who take on greater leverage (i.e., have higher loan-to-value ratios) or who are less creditworthy (i.e., have lower FICO scores), they do not charge higher rates to borrowers in regions with declining economic conditions even though they are much more likely to eventually default. Additionally, we show that local mortgage rates for loans securitized by the GSEs do not vary with other dimensions that could also induce local adjustment for risk, such as local mortgage recourse laws, local bankruptcy laws, or local lender concentration.

In the second part of the paper, we then provide an assessment of the extent to which GSE interest rates should vary spatially, given the large spatial variation in default risk. To do this, we exploit loan-level data containing loans securitized by private agencies. To facilitate comparisons, we focus on a set of loans that we refer to as “prime jumbo” loans. The GSEs are only allowed to securitize loans smaller than some threshold size, known as the conforming loan limit. Our prime jumbo loans are larger than those made by the GSEs but comparable on many other dimensions (in particular, FICO score and LTV ratio). Unlike the interest rate on GSE loans, we document that the interest rate on prime jumbo loans rises dramatically with ex-ante local predicted default risk. Thus, although there is no regional risk-based pricing in the government-backed GSE market, the private market does set interest rates based in part on regional risk factors. This result shows that local risk factors are ex-ante observable by lenders.

Employing a variety of techniques, including a regression discontinuity approach around the conforming limit threshold, we construct counterfactual estimates of the extent to which GSE mortgage rates should have varied across regions within the United States. In particular, we construct these estimates during both the early 2000s and during the Great Recession, assuming that the GSEs priced local risk similarly to the private market. These results are robust to controlling for many potential confounding factors, including the possibility that prepayment propensities or points and fees vary spatially. We also document that loan amounts for GSE and prime jumbo borrowers do not respond differentially to ex-ante predictable default. This suggests that, relative to the private market, the GSE market does not compensate for the lack of spatial variation in mortgage rates by reducing the amount of credit extended.

We explore a number of explanations for why the relationship between mortgage rates and predictable default differs in the GSE and private markets. We conclude that political pressure is the most reasonable explanation for the patterns we observe. The GSEs face a great deal of political scrutiny: we provide evidence showing that multiple times during the past decade the GSEs tried to implement space-based policies but that these efforts were abandoned after backlash from Congress, realtors, and community groups that objected to GSEs using different standards across regions.²

The fact that risk-adjusted mortgage rates are not equalized across regions implies that resources are redistributed across regions through the mortgage market. In the final part of the paper, we quantify the economic impact of the

²This lack of local variation in pricing rules appears in many pricing decisions for the U.S. government. For example, the U.S. Postal Service charges the same flat rate for all first-class mail regardless of the distance traveled. Finkelstein and Poterba (2013) also find that political economy considerations can explain why U.K. insurance providers price nationally despite the presence of local drivers of mortality risk.

transfers induced by the GSEs' constant interest rate policy. We begin with a simple back-of-the-envelope exercise that "marks-to-market" the interest rate on GSE-securitized loans originated during the Great Recession. More precisely, for each loan, we calculate the difference between the actual mortgage payment we observe under the GSE constant interest rate policy and the counterfactual mortgage payment if GSE interest rates instead priced local predicted default like the private market. Summing up these wedges over all loans originated during the Great Recession implies a total redistribution of \$14.5 billion in mortgage payments across regions during the 2007–2009 period. While this calculation already suggests an important redistributive role for the constant interest rate policy, it does not fully account for the total effects of the policy. In particular, it ignores: (1) equilibrium effects of the GSE policy on local income and house prices, (2) equilibrium effects associated with households adjusting their housing and mortgage behavior in response to changes in the GSE pricing rule, and (3) the effect of the policy on loans originated outside of the 2007–2009 period.

In order to provide a more complete account of the welfare consequences of the constant interest rate policy, we build a structural model suitable for counterfactual analysis. This spatial model of collateralized borrowing has households that face region-specific shocks to house prices and labor earnings as well as purely idiosyncratic labor earnings risk. Individuals in the model can choose whether to own a home or to rent, in addition to choosing non-durable consumption and liquid savings over their life cycle. Owner-occupied housing is subject to fixed adjustment costs but serves as collateral against which individuals can borrow to smooth nondurable consumption. In addition, changes in interest rates have effects on local house prices and income.

We use this model to assess the welfare consequences of the GSEs' constant interest rate policy. In particular, we ask what would happen if the GSEs maintained their role in the mortgage market but simply allowed interest rates to vary with local default risk as in the private market.³ Within the model, we compare two scenarios, one in which a common interest rate applies to all regions and one in which interest rates respond to the local default risk within each region. We use the empirical work in the first part of the paper to discipline the counterfactual interest rate policy in which rates respond to local default risk.

In our benchmark calibration, designed to match the regional variation observed during the Great Recession, the GSE's pricing policy generates a present value effect roughly equivalent to a one-time \$1,000 per-household tax on a region with a two-standard-deviation increase in regional activity (i.e., decline in predicted local mortgage default) and generates a one-time subsidy of \$900 for a region with a two-standard-deviation decrease in regional activity (i.e., increase in predicted local mortgage default). This one-time net transfer of \$1,900 per household from regions with two-standard-deviation positive shocks to those with two-standard-deviation negative shocks is larger than the per-household tax rebate checks paid by the U.S. government during the 2001 and 2008 recessions. Thus, our results suggest that the magnitude of redistribution induced by the GSEs through the mortgage market is economically meaningful and compares in size to transfer policies that have received vastly more attention.

³To be clear, we are not evaluating the consequences of eliminating GSEs and are instead considering a simple change in their interest rate policy. Eliminating GSEs would have many important effects on housing markets, as described in Elenev, Landvoigt, and Van Nieuwerburgh (2015).

Rather than focusing on the model’s implications for particular regions during the Great Recession, we can also add up the total transfers across *all* regions. Under our baseline calibration, our model implies that about \$47 billion is transferred via the mortgage market from regions receiving better than average economic shocks to regions receiving worse than average economic shocks. The model-implied transfers are higher than our estimated back-of-the-envelope transfers, in large part because the model allows for the constant interest rate policy to provide an additional benefit to local economic activity by boosting local income and house prices.

We also show that this large average transfer across regions hides substantial heterogeneity in the effects within regions since not all households have equal mortgage exposure. In particular, our model implies that the GSE pricing policy has a much larger effect on middle-aged households than on young households because the young mostly choose to rent and are thus less sensitive to the local mortgage rate. Similarly, the implied transfer is largest for middle-income households within each region, as the poorest households do not own houses and the richest households have little mortgage debt. Thus, the GSE constant interest rate policy has the greatest effects on the middle class.

Our work relates to a number of existing literatures. First, there is a small body of work that studies the extent to which risk is shared across U.S. states through credit markets. For example, Asdrubali et al. (1996) examine risk sharing across U.S. states and suggest that credit markets smooth about 23 percent of regional shocks. In that paper, the key mechanism is general borrowing and lending across regions. Lustig and Van Nieuwerburgh (2010) directly explore the role of housing equity in supporting regional risk sharing. As housing equity increases, households are better able to borrow. The increased ability to borrow relaxes local liquidity constraints, allowing local residents to better insure themselves against local shocks. Lustig and Van Nieuwerburgh find that the extent of regional risk sharing varies with the state of the aggregate housing market. Our paper complements these findings by highlighting a direct mechanism by which the credit market serves to insure regional shocks. This mechanism, as far as we can tell, is a novel addition to the regional risk-sharing literature.⁴

Additionally, our paper speaks to how local shocks are mitigated within monetary and fiscal unions. This question has gained considerable attention in recent years as large disparities in regional outcomes have occurred within both the United States and Europe. There is a large literature arguing that an integrated tax and transfer system together with easy factor mobility can help mitigate local shocks.⁵ Most papers exploring regional variation in economic conditions impose constant interest rates across regions. Since these models typically do not include default risk, this should be interpreted as imposing a common risk-adjusted rate. Our work suggests that institutional features – such as the political pressure faced by GSEs – may lead to violations of this assumption. The bulk of U.S. household borrowing occurs in mortgages securitized by GSEs. We show that loans securitized by the GSEs exhibit

⁴More broadly, our work contributes to the growing literature emphasizing that housing finance has important implications for the U.S. economy. Recent papers in this literature include Agarwal et al. (2012), DiMaggio et al. (2014), Keys et al. (2014), Lustig and Van Nieuwerburgh (2005), Mian, Sufi, and Trebbi (2014), Mian, Rao, and Sufi (2014), Mayer et al. (2009), Piazzesi et al. (2007), and Scharfstein and Sunderam (2013).

⁵See, for example, Farhi and Werning (2013) and the citations within. Additionally, Sala-i-Martin and Sachs (1991) and Asdrubali et al. (1996) explore the role of an integrated fiscal system in smoothing income across U.S. states. For a classic example of the importance of factor mobility, see Blanchard and Katz (1992). Recent examples include Farhi and Werning (2014), Charles, Hurst, and Notowidigdo (2013), and Yagan (2014). Also see Feyrer and Sacerdote (2011) for arguments that the integrated tax and transfer system as well as the ease of factor mobility are reasons for the long-run stability of the monetary union across U.S. states.

contract rate equalization across regions, but that default risk varies substantially across these same regions. This implies that the risk-adjusted rate on these loans varies substantially. This in turn leads to quantitatively important transfers across regions that occur in state-contingent ways.

II Background

Most mortgages in the United States are sold to a secondary market after origination, rather than staying on lenders' balance sheets. For example, from 2004 to 2006, about 80 percent of all mortgages were securitized (Keys et al. 2013). Loans meeting the underwriting standards of Fannie Mae and Freddie Mac are considered “conventional,” and thus eligible for purchase by these government-sponsored enterprises (GSEs). These loans are purchased, packaged, and insured against loss of principal and interest in the resulting mortgage-backed securities. As a premium, lenders pay a “guarantee fee” on each loan, which could potentially vary with features of the borrower (FICO score) or loan (loan-to-value ratio). The interest rate charged on mortgages sold to the GSEs thus reflects the guarantee fee, additional guidelines imposed by the GSEs, and any other charges that could potentially vary with regional risk.

The alternative secondary market for mortgages is known as the non-agency or private mortgage-backed security (MBS) market. In this market, loans that do not meet the standards of the GSEs are purchased, bundled, and sold to investors in the form of securities. These investors do not receive any guarantees against losses of principal or interest on the loans underlying the securities. That is, while investors in GSE securities are insulated from default risk, investors in the private market must accurately price both the risk of default and the risk of early prepayment. The interest rate charged on mortgages sold through the private market thus reflects the guidelines imposed by investors, as well as other charges that could potentially vary with regional risk.

Prior to 2004, roughly 80 percent of the securitized mortgage market was securitized by the GSEs (Fannie Mae, Freddie Mac, and Ginnie Mae). The private market securitized all other loans. The private market includes jumbo mortgages (loans that exceed the conventional mortgage size limits), subprime mortgages (loans for borrowers with poor credit histories), and Alt-A mortgages (loans for borrowers who provide less than full documentation). During the 2004–2006 period, the share of loans securitized by the private market grew at the expense of those loans securitized by the GSEs. However, by late 2007, the private secondary mortgage market dried up, and essentially all securitization of mortgages since that time has been conducted by the GSEs.

Why do the GSEs dominate the conventional mortgage market? Researchers have estimated that the government's implicit guarantee to keep Fannie and Freddie solvent reduces the GSEs' cost of funds relative to the private market. Estimates suggest that mortgage rates for conventional mortgages are 20 to 40 basis points lower than mortgage rates for otherwise similar jumbo mortgages (see, for example, Sherlund 2008). This difference is attributed to both the implicit guarantee and the scale of the GSE market.⁶ This cost differential makes it difficult for the private market to undo any potential mispricing by the GSEs. In particular, if political constraints prevent the GSEs from

⁶For a recent discussion of this literature, see Lehnert et al. (2008).

raising interest rates in declining markets and lowering interest rates in relatively strong markets, the cost differential prevents private markets from competing with lower interest rates in relatively stronger markets. However, this cost differential does provide a bound on the potential mispricing of local risk.

Finally, it is worth discussing who ultimately holds these securities and bears the risk of the mispricing. Although institutional investors may hold both GSE-backed and private mortgage-backed securities, only the private securities face default risk. In contrast, the GSEs guarantee the principal and interest payments of their mortgage-backed securities. Thus, the GSEs directly bear the risk of mispricing. From the investors' perspective, they only face the risk of early prepayment in GSE-backed mortgage securities. When the GSEs were publicly traded, their shareholders also bore the risk that the GSE pricing model was not accurate. After the housing bust caused the GSEs to be put into government conservatorship, losses were ultimately borne by taxpayers. In sum, the costs from failing to price local default risk are first borne directly by the GSEs, who fully insure securities holders against default risk, and then indirectly by taxpayers, who implicitly provide a government backstop.

III Data

We use two main data sources for our empirical work. The first includes a sample of loans securitized by either Fannie Mae or Freddie Mac. Due to issues related to data coverage and comparability, we do not analyze loans securitized by Ginnie Mae. The second includes a sample of jumbo loans securitized by the private market.

III.A Fannie Mae/Freddie Mac Sample

Our primary data sources are Fannie Mae's Single Family Loan Performance Data and Freddie Mac's Single Family Loan-Level Data Set. The population of both data sets includes a subset of the 30-year, fully amortizing, full documentation, single-family, conventional fixed-rate mortgages acquired by the GSEs between 1999 and 2012. The data include both borrower and loan information at the time of origination as well as data on the loan's performance. With respect to information at the time of origination, the data includes the borrower's credit (FICO) score, the date of origination, the loan size, the loan size relative to the house value (LTV ratio), whether the loan is originated for purchase or refinancing, the three-digit zip code of the property, and the interest rate on the mortgage. The loan performance data are provided monthly and include information on the loan's age, the number of months to maturity, the outstanding mortgage balance, whether the loan is delinquent, the number of months delinquent, and whether the loan is prepaid. There is a unique loan identifier code in the data sets that allows a loan to be tracked from inception through its subsequent performance.

When creating our analysis file, we pool data from both the Fannie Mae and Freddie Mac data sets. In doing so, we are exploring the spatial variation in interest rates for conventional loans that are securitized by either GSE. Finally, within our analysis sample, we include loans associated with both new-purchase mortgages and refinancings.⁷

⁷The results are unchanged if we analyze Fannie Mae and Freddie Mac loans separately, or if we exclude refinance loans. The data

In total, our sample includes roughly 13 million loans that were originated during the 2001–2006 period and another roughly 5 million loans that were originated during the 2007–2009 period.

III.B Prime Jumbo Sample

Our second primary data source is the Loan Performance database, which contains loan-level origination and performance data on the near-universe of mortgage loans sold through the private secondary market during the housing boom. Within the Loan Performance database, we focus only on what we term fixed-rate “prime jumbo” mortgages. As noted above, loans securitized by the private market include both subprime and Alt-A mortgages as well as mortgages that are larger than the conforming loan limit.

Specifically, we want to create a set of mortgages securitized by the private market that is as similar as possible to the mortgages in the Fannie/Freddie pool. To do that, our “prime jumbo” mortgages: (1) have an origination value that is between the conforming mortgage limit and two times the conforming mortgage limit in the year of origination, (2) have a fixed interest rate, (3) have an LTV ratio at origination of less than 100 percent, (4) have a FICO score at origination of 620 or higher, (5) provide full documentation at the time of origination, and (6) were originated between 2001 and 2006.⁸ The 2006 end date is necessitated by the fact that the private market effectively disappeared in 2007.

In essence, our prime jumbo loans are designed to be similar to the Fannie/Freddie loans in all respects except that the origination value of the loan is slightly higher. As with GSE mortgages, we include originations for both new purchases and refinancings. Finally, we restrict the sample to include only observations where there are at least five loan originations in an MSA and quarter-of-year cell. Our unit of analysis for exploring spatial variation in mortgage rates is at the MSA level. This restriction ensures that there will be a minimum amount of loans for each MSA-quarter cell. In total, our “prime jumbo” sample includes 70,327 loans originated during the 2001–2006 period.

III.C Additional Sample Restrictions

Table 1 provides descriptive statistics for both our GSE sample (column 1) and our prime jumbo sample (column 4) during the 2001–2006 period without any further restrictions on the GSE sample. A few things are of note about the GSE sample relative to the prime jumbo sample. First, borrower quality looks higher in the GSE sample despite our initial restrictions on the prime jumbo sample. In the full GSE sample, the average FICO score of borrowers is 728. The comparable number in the prime jumbo sample is only 656. Second, the GSE data covers 374 distinct metropolitan statistical areas (MSAs). However, prime jumbo loans are only in 106 distinct MSAs (where at least five loans that meet our definition were originated during a quarter). This is not surprising given that the origination

appendix discusses additional sample restrictions. In particular, we include only mortgages that have a FICO score at origination of at least 620 (the bulk of GSE data), were originated between January 2001 and December 2009, and were originated within one of our included MSAs.

⁸The conforming limit was raised from \$275,000 to \$417,000 between 2001 and 2006. This period pre-dates the FHFA policy to vary loan limits regionally based on “high cost” areas, which began in 2008.

amount on a prime jumbo loan has to exceed a relatively large value. For many MSAs, it is rare for a property to transact above the conforming loan limit. As average property values in an MSA increase, the probability that loans exceed the conforming loan threshold also increases.

To further facilitate comparison between the GSE data and the prime jumbo data, we make two additional sets of restrictions to the GSE data. First, we restrict the GSE data to include only loans for the 106 MSAs where we have at least five observations of prime jumbo data. This ensures that the MSA-quarter coverage between the two samples is identical. The restriction reduces the sample size of GSE loans from 13.1 million loans to 8.1 million loans. Descriptive statistics for this sample are shown in column 2 of Table 1. This restriction does not alter the borrower-quality comparisons at all: It is still the case that the MSA-matched GSE sample had higher FICO scores than the prime jumbo sample.

Our second set of restrictions is more substantial. Here we restrict the GSE sample to match the prime jumbo sample along two additional dimensions. First, we restrict the sample so that the sample sizes match exactly. This is important given that when we measure the variability of interest rates and default rates across MSAs, we want to ensure we have similar power within the two samples. Second, we restrict the GSE sample so that it replicates the FICO and LTV distributions of the prime jumbo sample. As a result, the distribution of borrower quality as measured by FICO scores and LTV ratios will not differ between the two samples.⁹ We refer to this sample as the “matched” GSE sample where the matching occurs on MSA-quarter, FICO score, LTV ratio, and sample size. For each prime jumbo loan we “draw” a similar loan from the GSE sample. Descriptive statistics for the matched GSE sample are shown in column 3 of Table 1. Given the matching procedure, it is not surprising that the median FICO variation, median LTV variation, and the MSA coverage match exactly with the prime jumbo sample. This matched GSE sample will be our main analysis sample going forward.

Table 1 also shows the average interest rate on the loans within each sample. Consistent with the literature, the unconditional interest rate on GSE loans during this period was about 33 basis points lower than the rate on prime jumbo loans (6.33% vs. 6.66%). Throughout the paper, 60+ days delinquent will be our primary measure of default. Table 1 measures the fraction of loans that became 60+ days delinquent at some point during the two years after origination. Unconditionally, 3.0% of the GSE loans in the matched sample become delinquent in the two years after origination, while only 2.1% of the prime jumbo loans become delinquent. As we show below, conditioning on the date of origination and focusing on loans originated around the conforming limit cutoff, the ex-post delinquency measures are nearly identical between the two samples.

⁹All of these sample restrictions were made to ease comparison of the two samples. However, given that all of our estimation procedures also include controls for observable loan and borrower characteristics, the matching did not make much difference. In many of our tables, we show the results with and without restricting the samples to be similar in size and FICO/LTV distributions. The results are nearly identical across the specifications. See the appendix that accompanies the paper for details of the exact selection criteria for our main sample to facilitate replication of our results. In Online Appendix Table A-1, we show that the matching criteria resulted in both the mean and distribution of FICO and LTV being similar between the GSE and prime jumbo sample.

III.D Controlling for Borrower and Loan Characteristics

Throughout the paper, we want to examine spatial variation in mortgage rates and show how this variation correlates with spatial variation in predicted future mortgage default rates in each of our samples. Interest rates and delinquency rates could potentially differ spatially just because borrower or loan characteristics, such as FICO score or date of origination, vary spatially. For example, borrowers with lower credit scores empirically face higher interest rates and are more likely to later default. If borrower creditworthiness varies spatially, this could explain some spatial variation in observed mortgage rates and default rates. Of course, matching the two samples on FICO scores and LTV ratios mitigates some of this concern. What we are after, however, is whether interest rates and the predictable component of default rates vary spatially after conditioning on borrower and loan characteristics. A borrower with a given credit score and LTV ratio may be more likely to default in one region relative to another because overall economic conditions differ across regions. We want to know whether a given borrower would pay a higher interest rate when taking out an otherwise identical loan in a high risk rather than a low risk location.

To formally explore these patterns, we purge the variation in mortgage rates and subsequent delinquency rates of spatial differences in borrower and loan characteristics. To do so, we first estimate the following equations using our loan-level micro data:

$$r_{ikt}^j = \alpha_0^j + \alpha_1^j X_{it} + \alpha_2^j D_t + \alpha_3^j D_t \cdot X_{it} + \eta_{ikt}^j$$

$$y_{ikt}^j = \varphi_0^j + \varphi_1^j X_{it} + \varphi_2^j D_t + \varphi_3^j D_t \cdot X_{it} + \nu_{ikt}^j$$

where r_{ikt}^j is the loan-level mortgage rate for a loan made to borrower i , in MSA k , during period t , and y_{ikt}^j is an indicator variable for whether the loan made by borrower i , in MSA k , during period t , defaulted at some point during the subsequent 24 months. X_{it} is a set of control variables for borrower i in period t . Sample j refers to whether we use individuals from the GSE sample or the private jumbo sample. We run these regressions separately using data from each of our two samples. D_t is a vector of time dummies based on the quarter of origination. The borrower/loan controls include detailed FICO and LTV controls. Specifically, all regressions include quadratics in FICO and LTV, and each of these terms is fully interacted with quarter of origination dummies. The goal of these specifications is to recover η_{ikt}^j and ν_{ikt}^j , the residual mortgage rate and residual ex-post delinquency rate, respectively, for borrower i in MSA k during time t for loans in sample j after controlling for borrower/loan characteristics and time fixed effects.

Once we have the residuals from the above regressions with the full set of controls, we compute location specific average mortgage rates, R_{kt}^j , and location specific average ex-post default rates, Y_{kt}^j . We do this separately for each time period and for each sample. Specifically,

$$R_{kt}^j = \frac{1}{N_{kt}^j} \sum_{i=1}^{N_{kt}^j} \eta_{ikt}^j$$

$$Y_{kt}^j = \frac{1}{N_{kt}^j} \sum_{i=1}^{N_{kt}^j} \nu_{ikt}^j$$

where N_{kt}^j is the number of loans in the MSA k during period t within each sample. Formally, R_{kt}^j (Y_{kt}^j) will be the average mortgage rate residual (ex-post delinquency residual) in an MSA for loans originated during a given period for a given sample.

The bottom rows of Table 1 show the standard deviation of unconditional and conditional mortgage rates and delinquency rates across the MSAs for our matched GSE sample and our prime jumbo sample originated during 2001–2006. The cross-MSA variation in interest rates is reduced dramatically once we condition on borrower, loan, and time controls. Additionally, the conditional cross-MSA standard deviation of mortgage rates is twice as high in the prime jumbo sample as in the matched GSE sample, while the conditional cross-MSA standard deviation of delinquency rates is similar in the two samples. As a starting point, this shows that there is more cross-MSA variation in mortgage rates in privately securitized loans than in GSE loans.

IV Local Mortgage Rates and Predictable Local Default Risk

In this section, we document our key empirical facts. As we will illustrate, GSE mortgage rates do not vary at all with measures of local default risk, while prime jumbo rates do vary with this risk.

IV.A A Metric for Local Economic Activity

In order to examine whether mortgage rates vary with local economic conditions, we need to define measures of local economic activity observable to lenders that could potentially be used in their pricing decisions. Our primary measure of local economic activity is the lagged delinquency rate on loans securitized within each sample. Specifically, within each MSA k in period t , we measure the fraction of loans originated during the prior two-year period that defaulted at some time between their origination and period $t - 1$. Because our time unit of analysis is a quarter, our lagged delinquency measure is the fraction of all loans originated between 9 quarters prior and 1 quarter prior that became 60 days delinquent by the current quarter. We refer to this measure as $E_{k,t-1}^j$, where $E_{k,t-1}$ denotes lagged economic activity in location k prior to the current period. We index this measure by j because we could measure lagged defaults either in the GSE sample or in the prime jumbo sample. We use lagged delinquency as our primary measure of local economic activity both because it is a summary statistic for many economic factors that could predict future default (e.g., weak local labor markets, declining house prices) and because it is easily observable by lenders.¹⁰

To present the data, Figure 1a shows a simple scatter plot of local mortgage rates residuals for the GSE loans,

¹⁰We also used both the lagged local unemployment and lagged housing price growth as measures of local economic activity. Results were generally similar. The one difference was that lagged local house price growth during the early 2000s negatively predicted local mortgage default, while lagged local house price growth during the mid 2000s positively predicted local mortgage default. The latter result was driven by the fact that local house price growth during the mid-2000s predicted local house price declines during the late 2000s, and households are more likely to default when house prices decline.

R_{kt}^{GSE} , in the full GSE sample against lagged local GSE default rates, $E_{k,t-1}^{GSE}$, during the 2001–2006 period. Figure 1b presents the same result for the GSE sample matched on the distribution of FICO scores and LTV ratios. The matched GSE sample, as discussed above, only includes 106 MSAs, while the full sample includes 374 MSAs. Figure 1c analogously shows the scatter plot of local mortgage rates residuals for the prime jumbo loans, R_{kt}^{jumbo} , against lagged local GSE default rates, $E_{k,t-1}^{GSE}$, during the same time period. Each observation in the figures is an MSA-quarter pair.

Figures 1a and 1b show that there is no relationship between lagged local GSE default rates and average local mortgage rates in either the full GSE sample or in the matched GSE sample. Columns (1) and (3) of Table 2 summarize the regression line of the scatter plots in Figures 1a and 1b, respectively. Focusing on the results from column (3) of Table 2, a one-percentage-point increase in lagged GSE default is associated with a (statistically insignificant) increase in local GSE mortgage rates of only 3.5 basis points (i.e., from 6.000 to 6.035).¹¹ The standard deviation of lagged GSE default across MSAs is 0.7 percentage points, which implies that a one-standard-deviation increase in lagged default is associated with only a 2.5-basis-point increase in local GSE mortgage rates. Even adjusting for the standard error of the estimate, this is essentially a precise zero. As seen from comparing the first three columns of Table 2, there is no economically meaningful or statistically significant relationship between lagged GSE default and GSE mortgage rates regardless of the sample used for the GSE data. Finally, columns (5) and (6) show that the 2001–2006 patterns persisted through the 2007–2009 period. During the Great Recession, there was also no economically meaningful relationship between lagged local mortgage default and local mortgage rates in the GSE market.

The pattern in Figure 1c is in stark contrast to those in Figures 1a and 1b. Figure 1c shows that there is a strong positive correlation between lagged GSE default rates and local interest rates for prime jumbo loans. MSAs that had larger GSE defaults in the prior year originate loans with higher interest rates conditional on borrower and loan characteristics. Column (4) of Table 2 shows that a one-percentage-point increase in lagged local GSE default rates was associated with a 31-basis-point increase in local prime jumbo mortgage rates. This coefficient is 10 times larger than the effect on GSE mortgage rates and is highly statistically significant. Importantly, the strong response of interest rates to lagged default in the prime jumbo market shows that this information is available and exploitable by lenders. That is, the lack of risk-based pricing by GSEs cannot arise because this risk was ex-ante unobservable.

IV.B Relationship Between Predicted Default and Mortgage Rates

The previous subsection showed the relationship between lagged economic conditions and current mortgage rates. What lenders are presumably interested in is how past economic conditions translate into future default risk. In this subsection, we assess the extent to which lagged local economic conditions predict subsequent actual default. We

¹¹When fitting a line through the scatter plot or running regressions, we weight each observation by the number of loans originated during the MSA-quarter. As a result, larger MSAs with more loans are weighted more when fitting the line. All results in the paper are weighted in a similar manner.

then assess the cross-region relationship between predicted default and mortgage rates for both the GSE and prime jumbo samples.

We refer to predicted local default for loans in each sample j , in each location k , during each time period t , as \hat{Y}_{kt}^j . We calculate three measures of predicted default. Our first and primary measure predicts the relationship between future default and lagged default conditional on borrower and loan characteristics. In particular, we run the following regression on both the GSE and prime jumbo samples using data from 2001–2006:

$$y_{ikt}^j = \theta_0^j + \theta_1^j X_{it} + \theta_2^j D_t + \theta_3^j D_t \cdot X_{it} + \lambda^j E_{k,t-1}^{GSE} + \nu_{ikt}^j$$

where y_{ikt}^j , X_{it} , D_t , and $E_{k,t-1}^{GSE}$ are defined above. The goal of this regression is to use the underlying micro data to see whether lagged GSE default rates predict subsequent mortgage default (conditional on loan and borrower observables). We use the lagged GSE default rate for both samples so that we capture the response of actual default rates in the two samples to the same underlying economic conditions. The primary coefficient of interest is λ^j , which we can use to define our first measure of predicted local mortgage default:

$$\hat{Y}_{kt}^j = \lambda^j E_{k,t-1}^{GSE}$$

For both samples, λ^j is large and statistically significant, showing that lagged GSE default rates have significant predictive power for future default rates in both the GSE and prime jumbo samples. In particular, for the GSE market, the coefficient is 1.71 (SE=0.24, F-stat=50.5), while for the non-GSE market, the coefficient is 2.55 (SE=0.31, F-stat=68.1).¹² For robustness, we also explore two additional measures of predicted local default. The first we refer to as our “random walk” forecast such that:

$$\hat{Y}_{kt}^j = E_{k,t-1}^j$$

This specification implies that the best forecast of today’s loan default rate is yesterday’s default rate. Notice, for each sample, the lagged default rate is sample specific. This differs from the first predicted default measure where both the future default rates of loans in the GSE sample and the prime jumbo sample depended on the lagged GSE default rate. This allows for lagged default rates on the prime jumbo sample to have better predictive properties for loans in the prime jumbo sample than would lagged GSE default rate. As was the case with the previous results, lagged prime jumbo default rates were highly predictive of future prime jumbo default rates.

Second, we examine a “perfect foresight” prediction of future default such that:

$$\hat{Y}_{kt}^j = Y_{k,t}^j$$

¹²One may wonder if the relationship between lagged GSE default and future default is an artifact of the period we studied. We explored this possibility by re-running the above relationship for various subperiods of our data. For example, within the GSE sample, λ^j was large, statistically significant, and of similar order of magnitude during the 2001–2003 period, the 2004–2006 period, and the 2007–2009 period. In all three subperiods, lagged GSE default positively and significantly predicted future default rates within each loan type.

This perfect foresight specification implies that lenders' best prediction of future default in a given sample in a given location (conditional on observables) is the actual future default rate (which we label $Y_{k,t}^j$ in the above specification).

To examine whether the mortgage rates on GSE loans and the mortgage rates on prime jumbo loans respond similarly to predicted local default, we estimate the following equation separately for each sample during the 2001–2006 period:

$$r_{ikt}^j = \omega_0^j + \omega_1^j X_{it} + \omega_2^j D_t + \omega_3^j D_t \cdot X_{it} + \beta^j \hat{Y}_{kt}^j + \eta_{ist}^j$$

The regression is nearly identical to the ones above explaining mortgage rate variation aside from the addition of the predicted default variable. The coefficients of interest are β^{GSE} and β^{jumbo} (estimated from separate regressions on the GSE data and prime jumbo data, respectively).¹³ Column (1) of Table 3 shows our estimates of β^{GSE} for our three predicted default measures, while the second column shows our estimates of β^{jumbo} . Columns (3) and (4) show the difference between the coefficients ($\beta^{jumbo} - \beta^{GSE}$) as well as the p-value of the difference.

In all cases, mortgage rates in the prime jumbo market respond much more to predicted default than do mortgage rates in the GSE sample. That is, these regressions show that the greater response of jumbo mortgage rates to lagged economic conditions is not driven by greater sensitivity of actual default to these conditions. Furthermore, it is not just that jumbo rates are more responsive than GSE rates: our regression shows that GSE interest rates do not respond in *any* meaningful way to predicted default. A one-percentage-point increase in local predicted default only raises local GSE mortgage rates by 2 basis points, an effect that is statistically indistinguishable from zero.¹⁴ Again, the strong response of jumbo mortgage rates to our measures of predicted default implies that these objects have predictive power and can be meaningfully acted upon by actual lenders.

We can also explore the differential responsiveness of local mortgage rates to measures of local predicted default using a regression discontinuity approach to estimate ($\beta^{jumbo} - \beta^{GSE}$) around the conforming loan threshold. Specifically, we estimate:

$$r_{ikt}^j = \delta_0 + \delta_1 X_{it} + \delta_2 D_t + \delta_3 D_t \cdot X_{it} + (\tilde{\delta}_1 X_{it} + \tilde{\delta}_2 D_t + \tilde{\delta}_3 D_t \cdot X_{it}) D_{it}^{jumbo} + \delta_4 Bin_{it} + \beta Bin_{it} \cdot \hat{Y}_{kt}^j + \eta_{ist}^j$$

For this regression, we pool the prime jumbo sample and the matched GSE sample for the years 2001–2006. D^{jumbo} is a dummy variable indicating that the loan is from the prime jumbo sample, and our specification allows the responsiveness of mortgage rates to observables (FICO, LTV) and time effects to differ across the two samples.

The key additions to this specification are the variables Bin_{it} and $Bin_{it} \cdot \hat{Y}_{kt}^j$. For each loan, we compute a metric of the mortgage size relative to the conforming loan threshold. Loans above the conforming threshold will have a metric that ranges from 1 to 2 (given the prime jumbo sample includes only loans that were originated up to two

¹³To address concerns related to statistical inference with generated regressors, every estimate reported in the paper that relies on predicted defaults uses bootstrapped standard errors (500 repetitions, clustered at the MSA level).

¹⁴It is important to note that because the measures of predicted default are in different units, the coefficients cannot be directly compared across rows within a given column. In the next section, we will show that all three of the lagged default specifications yield similar differential variations in interest rates between the two samples once scaled appropriately by the underlying variation in the predicted default metric.

times the conforming limit). These loans will all be from the prime jumbo sample. Loans below the conforming threshold will have a metric between 0 and 1. The variable Bin_{it} is an indicator variable for the extent to which the loan size differs from the conforming threshold. Specifically, the Bin_{it} variable is defined in 0.2 unit intervals of the ratio of the loan size to the conforming loan limit (e.g., 0.8–1, 1–1.2, 1.2–1.4, etc.). For example, loans in the 1–1.2 bin have an origination value that is between the conforming limit and 20 percent greater than the conforming limit. The regression includes dummy variables for all ten bin values and allows the responsiveness of local interest rates to our measures of local predicted default to differ across the bins. As noted above, we created our matched GSE sample so that it has a similar distribution of loan sizes below the conforming threshold as the prime jumbo sample has above this threshold. This ensures that there are similar numbers of loans in each symmetric bin to the left and right of the threshold.

Selection is a potential concern for any such regression discontinuity approach, and we address it in a number of ways. More specifically, the concern is that loans just above the threshold may be similar on observables but might differ on unobservables that affect their propensity to default. This type of selection would not be surprising given the large financial benefit in terms of lower average interest rates for GSE loans relative to prime jumbo loans. As a result, better borrowers may migrate to the GSE sample by choosing to put up more equity and take out a loan smaller than the conforming threshold. We explore these issues in Figure 2. Figures 2a and 2b show that there is no discrete change in FICO scores or LTV ratios, so observable characteristics do not change across the conforming threshold. This is not surprising given that the samples were matched on exactly these measures.

Figure 2c explores whether there is selection on unobservables at the conforming threshold. It does so by comparing the default rates of the GSE loans right below the threshold with the default rates for the prime jumbo loans right above the threshold. If there was selection, one would imagine that better borrowers (on unobservables) put up more cash so that they secure a loan lower than the conforming threshold. Figure 2c shows that there is a very slight increase in default probabilities for prime jumbo loans in the first bin above the conforming threshold relative to the first bin below the threshold (differential actual default probability = 0.004 with a standard error of 0.001). Although the difference in actual default rates is small, it does appear that some selection is taking place. However, the second bin above the threshold shows no differential default probability relative to the GSE loans just below the threshold. The differential default probability between GSE loans close to the conforming limit and loans in the second bin above the threshold is close to 0.001 with a standard error of 0.001. Similar results hold for the third, fourth, and fifth bins above the threshold. Thus, although there may be a small amount of selection occurring within the first bin above the threshold, there does not seem to be any evidence of selection in the other bins that is correlated with actual loan performance.¹⁵

Figure 3 shows our estimates of β for each of the ten bins using our three default measures. The results are, again, striking. The responsiveness of local mortgage rates to local predicted default rates is essentially zero for all

¹⁵Given that the second bin has a loan value that is, on average, between \$40,000 and \$80,000 above the threshold, it is challenging for most households buying a \$500,000 home to substantially reduce the loan balance so that it could be securitized by the GSEs.

bins below the conforming threshold, regardless of our definition of predicted default. However, for the bins directly above the conforming thresholds, there is a strong positive relationship between local default probabilities and local mortgage rates. The estimated responsiveness is nearly identical in the second, third, and fourth bins above the threshold. The results, combined with the actual default analysis in Figure 2c, show that the pricing behavior of mortgages with respect to local default risk changes discretely between the GSE and prime jumbo samples. Column (5) of Table 3 shows our Regression-Discontinuity (RD) estimates of the differences in responsiveness for our three measures of predicted default. Our RD estimates are very similar to the regression-based estimates shown in column (3) of Table 3.

IV.C How Much Should GSE Loan Rates Have Varied with Predictable Default?

In this subsection, we construct a counterfactual of how much GSE interest rates should have varied across regions if local risk was priced similarly to the prime jumbo sample. Table 4 shows the standard deviation of predicted default for our three default measures. The first and second columns examine the standard deviation of predicted default for our matched GSE sample and our prime jumbo sample during the 2001–2006 period. The last column examines predicted default measures for a sample of GSE loans restricted to the same MSAs as the prime jumbo loans, but during the 2007–2009 instead of the 2001–2006 period.

Table 5 is our key counterfactual table. Given the standard deviation of predicted default rates (shown in Table 4), Table 5 computes how much GSE interest rates should have varied across regions in response to a two-standard-deviation change in predicted default. We use our baseline RD coefficients (column (5) of Table 3) to perform the counterfactual. Table 5, therefore, computes the counterfactual by multiplying our estimate of $(\beta^{jumbo} - \beta^{GSE})$ by two times the relevant standard deviation of predicted default. Our preferred estimates (row (1) of Table 5, which uses the regression measure of predicted default) suggest that a two-standard-deviation shock to predicted default should have resulted in a 16-basis-point variation in GSE mortgage rates across regions during the 2001–2006 period and a 30-basis-point variation in GSE mortgage rates across regions during the 2007–2009 period. The difference between the two periods results from the fact that the variation in predicted default across regions was much higher during the 2007–2009 period.

The other specifications of lagged default give roughly similar estimates. In our modeling section below, we are particularly interested in measuring the extent of resource transfers due to the GSEs’ constant interest rate policy during the Great Recession because regional risk was particularly important during this time period. With this goal in mind, we choose parameters so that a two-standard-deviation shock to local economic activity across regions would generate a 25-basis-point movement in mortgage rates across regions if the GSEs abandoned their constant interest rate policy and allowed mortgage rates to adjust to local default risk as in the private market. Given our counterfactual estimates for the other predicted default measures shown in Table 5, we examine the robustness of our model results when a two-standard-deviation shock causes a 15-basis-point or a 35-basis-point movement in mortgage

rates across regions.

In sections that follow, we will assess the consequences of the GSE constant interest rate policy. We use a simple back of the envelope calculation as well as more formal structural model that accounts for endogenous household decisions in response to interest rate changes and feedback of interest rate policy to local house prices and income. The broad conclusion that emerges from either of these approaches is that the constant interest rate policy induces large and meaningful transfers across regions.

IV.D Robustness and Extensions

Before turning to a formal welfare analysis of the constant interest rate policy, we first briefly discuss the robustness of our empirical results along a number of dimensions. As a summary, none of the robustness specifications we explored altered our conclusions either qualitatively or quantitatively. In the Online Appendix we describe these robustness exercises in much greater detail.

Aside from default risk, the biggest risk lenders face is prepayment risk. If prepayment risk differs dramatically between GSE loans and prime jumbo loans in a way that is correlated with local default risk, the lack of variation in GSE mortgage rates with local default risk may not be surprising. In our data, we can track prepayments and thus create a measure of predicted local prepayment risk (in ways similar to our creation of local default risk). The Online Appendix discusses exactly how we compute the measures of local prepayment risk. We find that predicted prepayment rates, conditional on loan and borrower observables, are very similar for GSE and prime jumbo loans. For example, using our RD approach, predicted annual prepayment rates were only 1 percentage point lower for prime jumbo loans above the conforming threshold than for GSE loans below the threshold (19% vs. 20%). What matters is whether predicted prepayments are differentially correlated with predicted default rates across the two samples in a way that undoes the results documented above. To explore this, we added predicted prepayment rates as an additional control to all our main empirical specifications. Table 6 shows one such specification. Column (1) of Table 6 redisplayes our estimate from column (5) of Table 3 (row (1)). We do this to facilitate comparison across our robustness specifications. Column (2) shows our RD estimates when we add the measure of predicted prepayments as an additional control. Notice that controlling for predicted prepayment risk does not change the RD estimates in any meaningful way. Again, this is not surprising given the fact that conditional prepayment probabilities barely differ between the samples. These results suggest that predicted prepayment differences are not driving the differential interest rate sensitivities to local default risk between the GSE and private samples.

Another potential concern with the interpretation of our previous results is that identification could be driven by across-MSA differences in the composition of GSE versus private loans rather than from differential responses of these loans to common local conditions. To address this concern, we reestimated all our specifications including MSA fixed effects. This allows us to compare GSE loans within an MSA to prime jumbo loans within the same MSA. Column (3) of Table 6 controls for MSA fixed effects in our RD specification, while column (4) controls for

both MSA fixed effects and local prepayment risk. As can be seen from the table, the estimated difference in interest rate responsiveness to local default risk, $(\beta^{jumbo} - \beta^{GSE})$, is essentially unchanged in all the specifications.

Our analysis thus far has only explored the adjustment of mortgage prices in response to spatial variation in regional risk. One may also expect some adjustment to occur on the quantity side—that is, on both the extensive (loan approval) and intensive (loan amount, conditional on approval) margins.¹⁶ Unfortunately, we are not able to directly explore variation on the extensive margin, because the only available data on the extensive margin (HMDA database) does not have borrower-level variables, which are crucial for differentiating borrower-level risk from location specific risk. We are, however, able to explore quantity movements on the intensive margin using our data. Online Appendix Figure A-2 shows the relationship between lagged default rates and LTV residuals for both the GSE sample (top panel) and the prime jumbo sample (bottom panel). These figures are similar to Figure 1. We residualize LTV controlling for FICO score and time effects in a way similar to our residualization of interest rates. As seen from Online Appendix Figure A-2, there is little LTV adjustment across MSAs in response to differences in lagged default rates in either sample. If anything, borrowers in riskier places are slightly more leveraged on average. Moreover, there are no statistical differences in the response rates between the two samples. Additionally, we reestimated our main results on a sample where there is essentially no extensive margin adjustment. Specifically, rejection rates in the GSE sample are close to zero for high-quality borrowers with an initial LTV less than 0.8. In column (5) of Table 6 we show our RD estimate restricting the sample to borrowers with an LTV less than 0.8. As can be seen from the table, the estimated difference in interest rate responsiveness to local default risk, $(\beta^{jumbo} - \beta^{GSE})$, is if anything slightly larger when restricting the sample to borrowers where the potential for quantity adjustments on the extensive margin is close to zero.

Up until this point, we have not examined regional variation in points paid or other loan fees because points and fees are not recorded in our data. It may be the case that mortgage rates do not vary across MSAs in the GSE sample, but that points and other fees do vary with local default risk. To address this concern, we obtained additional data from one of the GSEs to directly estimate the relationship between effective interest rates and regional risk. The measure of effective interest rates in this data nets off any points and fees (including closing costs) that are charged to the borrower. As shown in Online Appendix Table A-2, we find no significant relationship between effective interest rates in the *universe* of GSE loans that meet our sample criteria and regional risk, as measured by lagged GSE default. We also examined whether the difference in risk based pricing still occurs within loans with an origination $LTV \leq 0.8$, which generally do not require private mortgage insurance. As seen from Column (5) of Table 6, even in this restricted sample where primary mortgage insurance (PMI) is not required, our RD coefficients are nearly identical to our base case.¹⁷

¹⁶Note that most models would imply that when lenders reduce loan quantities they would also raise loan prices, so the fact that there is no regional variation in GSE loan prices strongly suggests there is no quantity variation.

¹⁷For additional robustness, we also secured data from LoanSifter, a company that collects loan quotes from various lenders about the interest rate they charge for a given loan type. Loan type is defined as a function of FICO score, fixed interest rate, points charged, and initial LTV. We were able to secure fixed-rate loan quotes from banks for a given size loan (\$300,000), a given LTV (80%), no points, and three FICO score levels (750, 680, 620) during the period of September 2009 through September 2010. The key advantage of this data is that points are held fixed across all loan quotes. Given the loan size, all quotes were for conforming loans eligible for securitization by

In our final robustness analysis, we show that local mortgage rates for loans securitized by the GSEs do not vary with other dimensions that could also induce local adjustment for risk such as local mortgage recourse laws, local bankruptcy laws, or local lender concentration. The details are described in the Online Appendix. States with higher bankruptcy exemptions, higher lender concentration, and less potential for recourse judgments did not have systematically higher mortgage rates in either the GSE or prime jumbo samples.

V Why Do GSE Rates Not Vary with Local Economic Conditions?

Why do the mortgage rates on loans sold to the private market vary with local economic conditions but the mortgage rates on loans sold to GSEs do not?¹⁸ We can easily rule out some alternatives. First, one might argue that the GSE constant interest rate policy occurs because GSE loans are securitized, which allows for better diversification of idiosyncratic and regional risk. However, note that our comparison is with loans in the private market that are also securitized; hence securitization per se cannot explain the absence of regional risk-based pricing in one market and not in the other. Next, one might think that the bigger size of the GSE market relative to the private market, which contributes to its cost advantage, may explain our findings. However, since this cost advantage occurs in all regions, it cannot explain the lack of regional variation in interest rates that we find.

We believe the quasi-public nature of the GSEs may impose political economy constraints on the extent to which they can vary mortgage rates across space. There is some evidence that supports this view. In early 2008, the GSEs attempted to implement a “declining market” policy that restricted credit differentially across U.S. locations. The policy required more equity at the time of origination in markets for which house prices were declining. In non-declining markets, the GSEs would purchase mortgages that had an initial LTV lower than 95%. However, in declining markets, the GSEs would only purchase mortgages where the initial LTV was lower than 90%.¹⁹ The policy did not affect interest rates; it only affected underwriting standards.

The declining market policy was announced in December of 2007 and was implemented in mid-January of 2008. After receiving large amounts of backlash from a varied set of constituents, the policy was abruptly abandoned in May of 2008. Consumer advocacy groups rallied against the policy, arguing that it was a form of space-based discrimination.²⁰ Real estate trade organizations used their political clout to protest the policy because it was hurting business. For example, the *Wall Street Journal* summarized the GSEs’ abandoning the declining market policy by saying, “The change [in GSE policy] comes in response to protests from vital political allies of the government-sponsored provider of funding for mortgages, including the National Association of Realtors, the National Association of the GSEs. Within this data, we find no relationship between quoted mortgage rates for a given contract and local measures of default risk (as measured by lagged GSE default). These results can be seen in Online Appendix Figure A-3.

¹⁸There is nothing in the GSE charters that prevents charging differential interest rates across localities. However, the current charter of the Federal Home Loan Mortgage Corporation states that the GSEs are to “promote access to mortgage credit throughout the Nation...by increasing the liquidity of mortgage investments and improving the distribution of investment capital available for residential mortgage financing.” See Lucas and Torregrosa (2010) for a discussion of the origins of the GSEs being driven in part by the volatility in mortgage access across U.S. subregions in the periods surrounding the Great Depression.

¹⁹Fannie and Freddie had slightly different definitions for what was a declining market. Roughly, declining markets were defined as locations where house prices were declining over the last two to four quarters.

²⁰See “Fannie Mae Sets New Loan Boundaries,” *NPR’s Marketplace*, April 18, 2008.

of Home Builders and organizations that promote affordable housing for low-income people.”²¹ The *Washington Post* reported, “Critics, including the National Association of Realtors and consumer advocacy groups, had charged that Fannie Mae’s policy served to further depress sales and real estate values in areas tainted as declining.”²² Even though it may have been profitable to require different down payments in different areas, Fannie Mae and Freddie Mac succumbed to political pressure and quickly abandoned the policy.

In September of 2012, the Federal Housing Finance Authority (FHFA), which now oversees the GSEs, proposed a new 25-basis-point fee at the time of origination that would differ across locations. The fee was tied to states that had long judicial delays in foreclosures. The rationale was that these states’ institutional features increased the length of the foreclosure process and the associated GSE losses. At the time of its original announcement, the fee would only have applied to loans originated in the five states with the longest foreclosure delays (New York, California, Florida, Connecticut, and Illinois). In late 2012, the FHFA invited comments on the proposal from the public. Like the declining market policy, this policy received a large amount of public backlash. For example, the governor of Illinois wrote a detailed public comment against the new fee. In December 2013, the FHFA announced that despite the backlash, they were going to implement the fee increase in the previously announced states (excluding Illinois) in April 2014. However, in January 2014, after another round of political pressure, the FHFA announced that the policy to charge differential state-based guarantee fees had been delayed indefinitely.

Even though these policies focused on imposing either spatial variation in down payments or spatial variation in loan guarantee fees, they can shed light on reasons why the GSEs may not raise interest rates in riskier markets during a recession. The source of the pushback on charging different interest rates across locations would likely have been the same. Interestingly, the argument against the declining market policy was that it would harm depressed areas by further reducing mortgage activity. This is *exactly* the mechanism we wish to highlight and quantify. By foregoing profit-maximizing behavior and charging a constant interest rate across all regions despite different levels of predictable default risk, the GSEs redistribute resources toward markets with weaker economic activity and greater default risk.

VI Consequences of Constant Interest Rates: A Back-of-the-Envelope Calculation

The fact that GSE mortgage rates do not vary spatially despite regional variation in predictable default rates implies regional differences in risk-adjusted interest rates. The GSEs’ pricing rule redistributes resources across regions in the sense that higher default regions get a subsidy from lower default regions, which allows them to borrow at a lower risk-adjusted rate. We now try to measure the size of these cross-region transfers.

To examine the quantitative impact of constant geographic pricing in the GSE mortgage market, we perform two

²¹See “Fannie Is Poised to Scrap Policy over Down Payments,” *Wall Street Journal*, May 16, 2008.

²²See “Looser Credit on the Way in ‘Declining’ Markets?” *Washington Post*, May 24, 2008.

complementary analyses. We begin with a simple back-of-the-envelope exercise that “marks-to-market” the interest rate on GSE securitized loans originated during the Great Recession. This allows us to calculate the extent to which mortgage payments on loans originated during the 2007-2009 period are transferred across regions. However, this calculation comes with a number of important caveats. First, this back-of-the-envelope calculation only measures the direct transfers that arise from the GSEs’ setting a constant national interest rate. It does not capture any of the indirect general equilibrium forces whereby changes in interest rates affect local income and house prices. By keeping rates low despite rising default probabilities, the GSE policy props up house prices and local income, providing further welfare gains to residents in regions hit by negative shocks. Second, the calculation takes existing mortgage portfolios as given and so may overstate some of the consequences of eliminating the constant interest rate policy due to a standard “Lucas Critique” argument.²³ Finally, newly originated loans are only a fraction of the total stock of mortgages outstanding at a point in time, so focusing on loans at origination understates the total effects of the constant interest rate policy when adding across *all* loans. Addressing these three issues requires moving to a structural model of household behavior, which we do in the following section.

To assess the transfers associated with the constant interest rate policy during the 2007–2009 period, we construct a measure of the implied transfer over the first year of the loan, $Transfer_{ikt}$, for each newly issued GSE loan in our data set during the 2007–2009 period. We begin by first estimating how much the interest rate on each loan would change under a counterfactual in which the GSEs priced regional risk like the private market:

$$\Delta \hat{r}_{kt}^{counterfactual, GSE} = (\beta^{jumbo} - \beta^{GSE}) \hat{Y}_{kt}^{GSE}$$

where $\beta^{jumbo} - \beta^{GSE}$ is the estimated response of mortgage rates in the prime jumbo market relative to the GSE market to a one-standard-deviation change in predicted default. Given we are focusing on the 2007–2009 period, our estimate of $\beta^{jumbo} - \beta^{GSE}$ is 0.148. This number comes from column 2 of Table 5.²⁴ \hat{Y}_{kt}^{GSE} is the predicted local GSE default rate for each MSA-quarter cell during the 2007–2009 period. We use our main predicted local default measure, $\hat{Y}_{kt}^{GSE} = \lambda^{GSE} E_{k,t-1}^{GSE}$, with $\lambda^{GSE} = 1.71$. Both our main predicted default measure and our estimate of λ^{GSE} are discussed in Section 4. Within each quarter, we standardize \hat{Y}_{kt}^{GSE} so that its cross-MSA mean is zero and its cross-MSA standard deviation is 1. This ensures that a change of $\hat{Y}_{kt}^{GSE} = 1$ implies a one-standard-deviation change.

We then multiply this counterfactual change in interest rates by the size of loan i originated in MSA k during 2007–2009 to arrive at the annual change in payment arising from the constant interest rate policy:

$$Transfer_{ikt} = (\beta^{jumbo} - \beta^{GSE}) \hat{Y}_{kt}^{GSE} LoanAmount_{ikt}.$$

²³For example, if the GSE pricing rule was eliminated, households in regions with poor economic conditions would likely delay entry to the housing market and reduce the size of their houses to mitigate some of the negative effect of higher interest rates.

²⁴In column 2 of Table 5, we show the response of GSE mortgage rates to a two-standard-deviation shock. To get the response to a one-standard-deviation shock, we divide the response to a two-standard-deviation shock by two ($0.148 = 0.297 \div 2$).

where $LoanAmount_{ikt}$ is the value of a newly originated loan in our data set. We perform this calculation for every newly issued loan (including both purchase and refi) in our Fannie/Freddie data sets during the 2007–2009 time period. The change in mortgage payments due to the constant interest rate policy in a given location thus depends on the size of loans originated together with the predicted GSE default rate in that location. Note that $Transfer_{ikt}$ measures the change in mortgage payment under the alternative policy during the year of origination, t . If payments are locked in at origination, this same transfer will then also occur in year $t + 1$ and in all future years until the loan is refinanced. That is, locking in a low mortgage rate today yields benefits in future years, and these future effects on loans originated in 2007–2009 should be accounted for when assessing the consequences of the GSEs’ constant interest rate policy. To compute the present value of the transfers associated with each loan originated during the 2007–2009 period, $Transfer_{ikt}^{PV}$, we assume that the first-year transfer persists in perpetuity with a discount rate of 15 percent. We choose a discount rate of 15 percent given that the average mortgage rate during this period was just under 6 percent and the prepayment rate was around 9–12 percent per year.

Figure 4 presents a map of the average $Transfer_{ikt}^{PV}$ within each MSA for loans originated during the 2007–2009 period. Positive transfers mean the MSA was a net recipient of transfers (received subsidies) from other regions. Negative transfers mean the MSA was a net payer of transfers (paid “taxes”) to other regions. The locations that receive a subsidy are in green, while places that were taxed are shown in red. The figure shows that the sand states (excluding coastal California), the Gulf Coast, and parts of the Northeast/Michigan received the largest average subsidies during the 2007–2009 period. In contrast, new loans originated in MSAs in the Northwest, coastal California, and the Midwest paid the largest taxes on average over this period. Across all MSAs, the 10th, 25th, 50th, 75th, and 90th percentiles of the $Transfer_{ikt}^{PV}$ distribution were $-\$680$, $-\$420$, $-\$80$, $\$290$, and $\$780$, respectively.

These transfers are sizable for individuals initiating a loan during this time period. To compare with our model results below, we compute the total value of transfers to regions with predicted default above the mean from regions with predicted default below the mean. To do this, we sum together $Transfer_{ikt}^{PV}$ for all loans originated in MSAs with $\hat{Y}_{kt}^{GSE} > 0$ during the 2007–2009 period and then sum together $Transfer_{ikt}^{PV}$ for all loans originated in MSAs with $\hat{Y}_{kt}^{GSE} < 0$ during the 2007–2009 period. Taking the difference between the transfers to the high-default regions and those to the low-default regions gives us the total present value of net transfers made through the mortgage market due to the GSEs’ constant interest rate policy, for loans originated between 2007 and 2009. We make one final adjustment to this number to ensure that it accurately reflects the total volume of GSE loans made during this period. Our GSE database includes only a selection of GSE loans originated during this time period. Using aggregate data published by Fannie/Freddie, we find that our data set includes about one-quarter of the total loans issued by Fannie/Freddie during the 2007–2009 period. As we discussed above, loans in the Fannie/Freddie data set had to meet certain characteristics. Moreover, even after meeting those characteristics, Fannie/Freddie only made a portion of the loans they originated available online. Given this, and assuming our estimate of $\Delta \hat{r}_{kt}^{factual, GSE}$ is the

same for GSE loans not in our sample, we scale our estimated transfer number by a factor of 4. Again, this scaling results from the fact that the dollar value of loans originated in our sample represents one-quarter of the total dollar value of loans originated by the GSEs during the 2007–2009 period.

Putting all of this together, our back-of-the-envelope estimate suggests that the GSE constant interest rate policy resulted in direct transfers of \$14.5 billion across regions for loans that were *newly originated* during the 2007–2009 period. This number requires little in additional assumptions beyond our baseline empirical strategy, and it already suggests an economically meaningful role for GSE interest rate policy in generating cross-region transfers.²⁵ Again, this back-of-the-envelope calculation misses many important aspects of the constant interest rate policy, including: (1) equilibrium effects associated with the response of local income and house prices to changes in the GSE pricing rule, (2) equilibrium effects associated with households adjusting their housing and mortgage behavior in response to changes in the GSE pricing rule, and (3) the effect of the policy on loans originated outside of the 2007–2009 period.

More rigorous counterfactual analysis that can address these three issues requires moving to a structural model of household behavior. We now turn to exactly such a model in order to provide a more comprehensive account of the welfare consequences of the GSE’s constant interest rate policy. Ultimately, we find that this model implies an even more important role for GSE policy in shaping household welfare than suggested by the \$14.5 billion of direct effects at origination.

VII Consequences of Constant Interest Rates: A Structural Model

In this section, we lay out a quantitative spatial model that captures various salient factors of the U.S. housing market. We develop a multi-region life-cycle consumption model where households face region-specific shocks to house prices and labor earnings as well as purely idiosyncratic labor earnings risk. Individuals in the model can choose whether to own a home or to rent, in addition to choosing nondurable consumption and liquid savings. Owner-occupied housing is subject to fixed adjustment costs but serves as collateral against which individuals can borrow using mortgages. This structural model accounts for endogenous changes in household behavior in response to changes in mortgage rates and thus can be used for counterfactual policy analysis. In addition, it allows us to consider the feedback of local interest rates onto local house prices and local economic activity, which can potentially have large effects on welfare. The model also allows us to measure the distributional consequences of the constant interest rate policy for households with different incomes and ages. This is something we could not explore with the back-of-the-envelope calculation.

The previous sections have shown that GSE mortgage rates do not respond to regional shocks. Accordingly, we initially assume that there is no regional variation in mortgage rates and calibrate the model to match various features of the data. We then use the model to explore what would happen if the constant interest rate policy was removed so that mortgage rates vary with local economic conditions in the manner in which they do in the prime

²⁵We defer a more detailed comparison to other cross-region transfer policies until after presenting our structural results.

jumbo market documented in the prior part of the paper.

Our model allows for regional variation in mortgage rates to affect welfare through three key channels: (1) We assume that households are able to borrow against their houses subject to holding some minimum equity, (2) households typically borrow all but the required down payment when purchasing houses, and (3) increases in mortgage rates depress local house prices and economic activity. If interest rates rise when local conditions deteriorate, the first channel lowers welfare by making it more difficult to smooth consumption. The second channel further lowers welfare, as higher interest rates mean that households in deteriorating regions will delay purchasing housing and reduce the sizes of their eventual purchases. Finally, the third channel amplifies these effects by further driving down economic activity when interest rates rise. A constant interest rate policy eliminates these effects, and it is in this sense that the policy transfers resources toward regions experiencing deteriorating local economic conditions.

VII.A Model Setup

VII.A.1 Demographics and Location

The economy is characterized by a continuum of households indexed by i . Household age is indexed by $j = 1, \dots, J$. Households enter the labor force at age 25 and retire at age 60. After retirement, households face stochastic mortality risk with probability of death d_j . Households live to a maximum age of 85, so $d_{85} = 1$.

Households live in specific regions indexed by k , and we assume that households never move. In our empirical results above, we showed that local economic conditions such as lagged mortgage default predict current mortgage rates in the prime jumbo sample but that there is no such relationship in the GSE sample. While we would want to include various dimensions of regional economic activity in our model, it is intractable to include all the separate dimensions as separate stochastic processes. Instead, within our model, we capture various measures of local economic conditions in a parsimonious manner by collapsing them into a single stochastic process, γ . We assume that this measure of economic activity (γ) in region k and period t follows the following process:

$$\log \gamma_{k,t} = \rho_\gamma \log \gamma_{k,t-1} + \varepsilon_{k,t}$$

In turn, we assume that $\gamma_{k,t}$ affects other local variables such as income and house prices. The effect of this regional shock $\gamma_{k,t}$ on these other aspects of the model will be made concrete below as we describe the evolution of income and house prices.

VII.A.2 Preferences and Household Choices

Household i receives flow utility

$$U_{ijk} = \frac{\left(c_{ijk}^\alpha h_{ijk}^{1-\alpha} \right)^{1-\sigma}}{1-\sigma}$$

from nondurable consumption c_{ijk} and housing services h_{ijk} .²⁶ Households discount expected flow utility over their remaining lifetimes with discount factor β .

VII.A.3 Income Shocks

Time t household labor earnings y for working-age households are given by:

$$\log y_{ijk,t} = \chi_j + z_{i,t} + \phi^y \gamma_{k,t} + \phi_r^y \phi^r \gamma_{k,t}$$

$$\log z_{i,t} = \rho_z \log z_{i,t-1} + \eta_{i,t}$$

where χ_j is a deterministic age profile common to all households, $z_{i,t}$ is a purely idiosyncratic persistent income shock, and $\phi^y \gamma_{k,t}$ is a region-specific shock to income. ϕ^y is a parameter that governs the sensitivity of household income to the underlying latent local economic conditions. One symptom of a depressed local economy will be declines in household income, and this is captured by ϕ^y . It is important to include this channel because changes in household income will directly affect the borrowing decisions of households and, as a result, will affect their response to interest rate variation.

Finally, $\phi_r^y \phi^r \gamma_{k,t}$ is a term that allows for a feedback multiplier from interest rates to local income: when interest rates rise, this may depress local economic activity. As described below, ϕ^r determines the response of interest rates to local economic activity, and ϕ_r^y then determines the response of local income to local interest rates. Interest rates can directly affect local income through their effects on local non-tradable demand and indirectly through their effects on house prices (see, e.g., Mian and Sufi 2014 and Charles et al. 2015). As discussed in our calibration section, we pin down this feedback in the model to match empirical estimates rather than endogenizing local income. In our robustness results we argue that endogenizing local income would substantially complicate our analysis with little effect on our conclusions.

When retired, households receive Social Security benefits. These benefits are based on lifetime earnings prior to retirement, and they are deterministic until household death. We describe the computation of these benefits in the calibration section of the model, but they mirror payments under the actual U.S. Social Security system.

VII.A.4 Housing Markets, Mortgages, and Interest Rates

Housing services can be obtained from owner-occupied housing or through a rental market. Housing can be purchased at price $p_{k,t} = (\gamma_{k,t})^{\phi^h + \phi_r^h \phi^r}$ or rented at price $p_{k,t} r^f$. We assume that house prices move exogenously with local economic activity, and ϕ^h governs the strength of this correlation. ϕ_r^h is a term that captures feedback from interest rates to local house prices in the event that interest rates are not constant ($\phi_r > 0$). If $\phi_r^h = 0$, then interest

²⁶This specification of utility between housing and nondurable consumption is common in the literature. See, for example, Piazzesi et al. (2007) and Davis and Van Nieuwerburgh (2014). Using data from the consumer expenditure survey, Aguiar and Hurst (2013) find that the share of expenditure households allocate to housing out of total expenditure is roughly invariant with either the level of household income or the level of household expenditure.

rates have no effects on housing prices. We estimate this response empirically rather than endogenizing house prices since this would necessitate modeling housing supply and dramatically complicate the model. More importantly, we argue in our robustness results that endogenizing house prices would have little effect on our conclusions. We denote owner-occupied houses as $h_{i,t}$ and rented houses as $h_{i,t}^f$. Buying or selling an owner-occupied house requires paying a fixed cost that is proportional to the current value of the house. That is, the fixed fraction lost for household i when the owners buy or sell their home takes the following form:

$$F_{i,t} = \begin{cases} F & \text{if } h_{i,t+1} \neq h_{i,t} \\ 0 & \text{if } h_{i,t+1} = h_{i,t} . \end{cases}$$

Offsetting the disadvantage that it is costly to adjust one's housing services, owning has two benefits over renting. First, households can borrow against houses subject to a minimum equity requirement:

$$m_{ik,t} \leq (1 - \theta)p_{k,t}h_{i,t},$$

where θ is the minimum down payment or equity that must be held in the house. Second, we assume that the rental stock depreciates at rate $\delta^f > \delta^h$. This is a standard assumption that provides a reason that individuals prefer to own rather than rent. In a competitive equilibrium, the rental price of housing must be equal to the risk-free rate plus the rate of depreciation of the rental stock:

$$r^f = r + \delta^f$$

Thus, $\delta^f > \delta^h$ implies that the imputed rental price of owner-occupied housing is lower than that of renting.

Since the majority of mortgages in the United States have fixed rates with an option to refinance, we assume that mortgages in the model take this form. The current market interest rate on new mortgages is equal to the risk-free rate plus a risk adjustment

$$r_{k,t}^{m,market} = r + \Psi_{k,t}$$

where the risk adjustment is declining in regional economic activity:

$$\log \Psi_{k,t} = \bar{\Psi} - \phi^r \log \gamma_{k,t}.$$

$\bar{\Psi}$ is a fixed risk adjustment associated with mortgage lending that is constant across locations. ϕ^r represents the sensitivity of local mortgage rates to local economic conditions. In our base specification, we set $\phi^r = 0$, consistent with the patterns documented for the GSE loans described above. Our main counterfactuals will be based on changing ϕ^r so that it matches the regional variation in response to predicted local default risk found among the prime jumbo loans.

We assume that households have access to fixed rate mortgages, so the current interest rate that households pay on their mortgages, $r_{k,t}^{m,fixed}$, may differ from the market rate, $r_{k,t}^{m,market}$. We assume that when households move houses or purchase for the first time, then they must reset their rate so that $r_{k,t}^{m,fixed} = r_{k,t}^{m,market}$. When not moving, households have the option of keeping their previous fixed rate, or refinancing to the current market interest rate at cost F^{refi} , which is proportional to the value of the house.²⁷ We extensively discuss the robustness of our results to alternative mortgage arrangements below.

Finally, in addition to borrowing through mortgages and saving through the purchase of durable housing, households can save in a one-period bond b with risk-free rate r . We assume that households are otherwise liquidity constrained in that they can only borrow against the value of their home.

VII.A.5 Household Problem

The household model is solved recursively. Within each period, households choose whether to move houses, to stay in their initial home, or to rent. If they stay in their current owner-occupied home, then they must choose whether to refinance. The adjusters include those homeowners who remain homeowners but change the size of their house, those homeowners who become renters, and those renters who become homeowners. Conditional on their adjustment decision, households choose the level of their consumption, their savings in bonds, and their mortgage debt. For brevity, we leave a formal statement of the the value functions to the Online Appendix, which also discusses in detail the numerical solution of the model.

VII.B Calibration

Our benchmark calibration strategy proceeds in two parts. First, we calibrate parameters that do not depend on regional economic activity to standard values from the literature together with standard moments from wealth data. Second, for parameters that vary with regional activity, we calibrate to match estimates from our previous empirical results. Our model period is one year, and we calibrate the model accordingly.

VII.B.1 Standard Parameters

Following Floden and Linde (2001), we set $\rho_z = 0.91$ and $\sigma_\eta = 0.21$ to match the annual persistence and standard deviation of earnings in the Panel Study of Income Dynamics (PSID). Their calculation conditions on education and age and so captures residual earnings risk. Since households in our model are ex-ante identical, this is the relevant empirical object. To calibrate the life-cycle profile of earnings, χ , we use the age-earnings profile in PSID data estimated by Kaplan and Violante (2010).

During retirement, households receive Social Security benefits, which we calculate using the method of Guvenen and Smith (2013). In reality, Social Security benefits are a function of lifetime earnings, but this would substantially

²⁷The assumption that the refinancing cost is proportional to the size of the house simplifies computations under some parameterizations of the fixed cost, as discussed below. Also, many of the costs of refinancing, like title insurance, are proportional to home value.

complicate the solution of the model because these lifetime earnings would become a state variable. However, a relatively accurate measure of lifetime earnings can be imputed from earnings in the final period of working life given the persistence of the income process. Thus we forecast lifetime income given income in the final period of working and then apply the actual benefit ratios from Social Security charts to this imputed lifetime income.

In addition, following Berger et al. (2015), we capture retirement accounts by introducing a lump sum payment at retirement equal to Ω times final working period income. This allows us to proxy for the fact that retirement accounts are illiquid for working age households but become liquid at retirement. Kaplan and Violante (2014) argue that it is essential to distinguish between liquid and illiquid wealth in order to generate realistic marginal propensities to consume, and this payment at retirement allows us to better capture the life-cycle profile of liquid wealth and so generate realistic levels of household self-insurance.

As is standard in the risk-sharing literature, we set $\sigma = 2$ to generate an intertemporal elasticity of substitution of $1/2$. As stated above, our model period is annual, and we accordingly set the risk-free rate to $r = 0.03$ to match the average real one year Treasury bill rate in the 2000s. In addition, we calibrate an average risk adjustment ($\bar{\Psi}$) of 0.01 to match the average real mortgage rate in our data. We calibrate $\delta_h = 0.03$ to match the average ratio of residential investment to the residential stock in Bureau of Economic Analysis (BEA) data. We set $\theta = 0.20$ so that households are required to have a minimum 20% down payment. We pick $F = 0.05$ so that there is a 5% transaction cost from adjusting housing. This encompasses costs of real estate broker fees, closing costs, and other costs associated with buying/selling a home. In our baseline results, we assume that $F^{refi} = 0$. This simplifies the problem, as the refinancing choice when not moving will simply deliver $r_{k,t}^{m, fixed} = \min(r_{k,t}^{m, market}, r_{k,t-1}^{m, fixed})$. Below, we discuss the results for a number of alternatives and show that among these alternatives, this is the most conservative in terms of generating small implied transfers from the constant interest rate policy.

We jointly pick β, r^f, Ω , and α to match various wealth and home ownership targets. We do so under the assumption that $\phi^r = 0$, which corresponds to the data-generating process under current policy. To estimate these parameters, we target a home-ownership rate of 69% as in the Survey of Consumer Finances (SCF) data. We also separately target liquid wealth net of all debt relative to income for working-age and retired households in the SCF.²⁸ In the SCF data calculation, we exclude retirement accounts in liquid wealth for working age households and include retirement accounts in liquid wealth for retired households. Finally, using BEA data over our sample period, we target a ratio of non-housing consumption to residential investment of 15. In our robustness results, we discuss alternative measures of this expenditure share and argue that our benchmark calibration is conservative in its implications: choosing higher housing shares only amplifies the importance of variation in mortgage rates.

We initialize households in the model to match the distribution of income, liquid wealth net of debt, and housing for 25- to 30-year-old households in the SCF.²⁹ Together, these targets yield $\beta = 0.916$, $r^f = 0.074$, $\Omega = 4.13$, and $\alpha = 0.88$.

²⁸In the baseline model, only assets net of debt are well defined, so we cannot separately match gross assets and gross debt in the data.

²⁹We also assume that the initial distribution of the idiosyncratic and regional income variation matches the observed cross sectional distributions for those variables.

VII.B.2 Calibrating Regional Variation

In addition to these relatively standard parameters, we must calibrate parameters that vary with regional economic conditions. Our baseline calibration uses local employment as our measure of economic activity (γ). Using annual employment data from the Bureau of Labor Statistics (BLS) from 1991 to 2013, we estimate an annual AR(1) process for log MSA employment, which yields $\rho_\gamma = 0.947$ and $\sigma_\varepsilon = 0.018$.³⁰ These findings suggest that shocks to local employment are somewhat persistent. For simplicity, we assume that local labor earnings move one-for-one with local employment so that $\phi^y = 1$, but we assess the importance of this assumption below in our robustness analysis.

To estimate ϕ^h , we use house price data from FHFA and regress log MSA house prices on log MSA employment during the same time period, which yields $\phi^h = 0.48$. We think of this elasticity as a short- to medium-run adjustment in house prices. In our baseline analysis, we are abstracting from housing supply adjustments, which limit the relationship between local employment growth and house price growth in the long run. However, to account for differential long-run supply effects, we show the robustness of our results to values of ϕ^h between zero and 0.48. A value of $\phi^h = 0$ implies a perfectly elastic housing supply curve even in the short run in response to local shocks.

We pick the key policy elasticity ϕ^r so that the regional variation in interest rates in our model when the GSE pricing policy is removed is consistent with that predicted by the prime jumbo data described above. In particular, we pick ϕ^r so that a two-standard-deviation decrease in γ increases local mortgage rates by 25 basis points. This is the variation in local mortgage rates to a two-standard-deviation change in predicted default during the 2007–2009 period for prime jumbo loans (see Table 5). Given this, the implied elasticity of the total borrowing rate ($r + \bar{\Psi}\gamma$) to γ is 0.54. We provide robustness results for both larger and smaller ϕ^r and discuss alternative counterfactuals in the following section.

Finally, when interest rates decline, this is likely to increase demand for housing and put upward pressure on house prices as well as on local employment and earnings, through local multiplier effects. We pin down the strength of these effects using relatively conservative estimates from VAR evidence on the response of house prices and GDP to federal funds rate innovations in Christiano, Eichenbaum, and Evans (1999) and Vargas-Silva (2008). In particular, we calibrate ϕ_r^y and ϕ_r^h so that a 25-basis-point increase in interest rates generates a 0.40 percent decline in house prices and a 0.20 percent decline in GDP. It is worth noting that both the empirical and theoretical size of multiplier effects is contentious, but we show below that even if we conservatively set feedback multipliers to zero, the implied welfare effects of the constant interest rate policy remain large.

VII.B.3 Model Fit

How well does our model fit non-targeted moments? Figure 5 shows the life-cycle profiles in our model compared with the data. Overall the model does a good job of replicating life-cycle patterns in the data. We match the

³⁰When estimating the AR(1) process for MSA employment, we remove permanent differences in employment across MSAs by including MSA fixed effects. Likewise, we remove aggregate business cycle effects by including year fixed effects. We include these same fixed effects when calculating the elasticity of house prices to local employment.

hump-shaped profile of nondurable consumption, as well as the increasing homeownership rate as households age. The model also does a good job of matching the size and life-cycle profile of total wealth net of debt (which includes the value of the housing stock), as well as liquid wealth net of debt (which excludes the value of housing from the measure of wealth). Overall, we think the model provides a close enough fit to the data that we are comfortable using it to assess the counterfactual effects of changes in GSE interest rate policy.

Our goal is to provide a broad estimate of the impact of the GSE constant interest rate policy on the economy. Household reoptimization in response to changing policy is a first-order effect that must be modeled in order to get the impact of this GSE policy roughly correct. We are less concerned that modest departures between our model and data along the above dimensions will dramatically affect our broad policy conclusions.

VII.C Model Results

To examine the effect of a constant interest rate policy on household well-being, we simulate household consumption under both the constant interest rate and the variable interest rate policy. For ease of discussion we label regions with low economic activity and high predicted default “bad” (low γ) regions and regions with high economic activity and low predicted default “good” (high γ) regions. We assume that in the absence of intervention from GSEs, mortgage rates would move with regional economic activity so that good regions would have lower rates and bad regions would have higher rates. This implies that the constant interest rate policy will tend to make households in the bad regions better off and households in the good regions worse off.

To assess the quantitative size of this “transfer” between good and bad regions under the constant interest rate policy, we ask how much households in a given region would be willing to pay in units of consumption to change from a variable interest rate policy to a constant interest rate policy.³¹

Formally, let $V_j^{\text{constant } r}(\mathbf{s}_{jk})$ be the indirect utility obtained from solving the household problem with state \mathbf{s}_{jk} in a world with $\phi_r = 0$. Similarly, let $V_j^{\text{variable } r}(\mathbf{s}_{jk})$ be the indirect utility obtained from solving the model in a world with $\phi_r > 0$, and let \widetilde{c}_{jk} and \widetilde{h}_{jk} be the choice for nondurable consumption and housing services, respectively, that obtain this maximal value. Finally, let $E_{\gamma,z,j}$ denote the expectation of these value functions over values of the idiosyncratic shock and age, conditional on living in a region with economic activity γ .³² We then solve for λ so that:

$$E_{\gamma,z,j} V_j^{\text{constant } r}(\mathbf{s}_{jk}) = E_{\gamma,z,j}^{\text{variable } r} \left\{ U(\widetilde{c}(1 + \lambda), \widetilde{h}(1 + \lambda)) + \beta E_j V_{j+1}^{\text{variable } r}(\mathbf{s}_{jk}) \right\}$$

That is, we compute the one-time percentage change in the consumption aggregate today that, in expectation, makes households indifferent between being in a world with constant $r^{m,market}$ and a world with variable $r^{m,market}$.

Table 7 shows the implied values of λ for various regions. We discretize the distribution of γ and focus on regions that had shocks that were one and two standard deviations above and below the mean region. The first row expresses

³¹The Online Appendix that accompanies the paper discusses exactly how we solve the model.

³²In all calculations, unless otherwise noted, we focus on the utility of working-age households since we want to understand how the mortgage market interacts with household risk. Retired households face no such labor market risk.

the utility gain/loss (λ) from the constant interest rate policy. This is the lifetime consumption gain as a fraction of today's consumption. The second row turns the lifetime consumption equivalent into a dollar amount. To do this, we first calculate the ratio of consumption to income in the model and then multiply this by median household income in the United States.³³ This calculation gives that median household consumption is roughly \$42,000. Although this number represents typical consumption per household, in the model households in bad regions consume less than households in good regions. This implies that the same λ in a bad region represents a smaller amount of consumption in dollars than in a good region, so we account for these differences by using the model's implied consumption difference across regions to scale consumption accordingly.

The worst regions (on the left side of the table) are made substantially better off by the constant interest rate policy, while the best regions (on the right side of the table) are made substantially worse off. Households living in a region with a two-standard-deviation negative shock to economic conditions would pay just over 2 percent of today's consumption to permanently move to a constant interest rate policy. Conversely, households with a two-standard-deviation positive shock would pay 2 percent of today's consumption to avoid the constant interest rate policy. In terms of dollar values, the constant interest rate policy transfers roughly \$870 per household to the worst regions in Table 7 and taxes the best regions \$990. This represents a one-time net transfer of \$1,860 during a period of large regional dispersion similar to the Great Recession.

This \$870 transfer to the most depressed regions (those with a two-standard-deviation negative shock) is similar in size to the tax rebate checks authorized by the U.S. Congress during the 2001 and 2008 recessions, which were also one-time payments during recessions (which ranged from \$500 to \$1,000 per qualifying household). However, it is important to note that our \$870 transfer converts the expected lifetime effects of the constant interest rate policy into a single one-time transfer equivalent. Since tax rebates have now become a common tool for fighting recessions, the present discounted value of future tax rebates is most likely higher than the size of any one rebate check. The other difference is that the transfer provided via the constant interest rate mortgage policy is funded by "taxing" the regions that are doing relatively well by roughly the same amount. While one might think the regions that are doing relatively well could borrow from private lenders to avoid this tax, it is important to remember that this is prevented by the GSEs' overall cost advantage, as noted in Section II.

Rather than focusing on implicit transfers from regions with particular employment shocks, we can also calculate the total resources transferred from *all* regions with positive employment shocks to *all* regions with negative employment shocks.³⁴ The average household in a location with a negative regional employment shock values the constant interest rate policy equivalently to a one-time \$350 payment. At the same time, the average household in a location with a positive employment shock would be willing to make a one-time payment of \$465 to move permanently to a

³³We make the conversion from consumption to income in the model since direct measures of consumption in the data are less reliable than measures of income. The average ratio of consumption to income in the model is 0.85. The Census Bureau estimates that median household income in 2010 was \$49,276. Multiplying the two numbers implies median consumption of \$41,885.

³⁴To compute this average transfer, we compute $\int_{\gamma < 0} f(\gamma) (1 + \lambda(\gamma)) d\gamma$ and $\int_{\gamma > 0} f(\gamma) (1 + \lambda(\gamma)) d\gamma$, where $f(\gamma)$ is the probability of experiencing a given γ shock and $\lambda(\gamma)$ is the welfare evaluation of a constant interest rate policy for a household living in a region with that γ . To provide a better approximation to $f(\gamma)$ we expand the γ grid from 5 to 15 points for this calculation, but we find nearly identical results when using 5 points.

variable interest rate policy. Thus, the total implicit net transfer implied by the constant interest rate policy averages \$815 per household. The aggregate value of the transfers induced by the constant interest rate policy comes to \$47 billion.³⁵

It is important to note that this \$47 billion should be interpreted as a one-time equivalent of the total discounted lifetime transfers across regions. For comparison, the Department of Labor forecasts that total unemployment insurance (UI) benefits paid in 2014 alone will equal roughly \$50 billion. Thus, transfers through the constant interest rate policy are smaller than the large-scale social insurance arising from UI, but are certainly large enough to warrant substantial attention. Our results imply large transfer payments across regions so that the constant interest rate policy has significant redistributive consequences.

The direct effect of abandoning the constant interest rate policy is that interest rates will then rise when economic activity deteriorates. This directly reduces welfare for anyone who will refinance during the period when economic conditions remain depressed, since it makes it more difficult to smooth consumption through home equity loans and also reduces the affordability of housing.³⁶ In our baseline model, we also allow for local multipliers so that increases in mortgage rates further depress local house prices and economic activity. This amplifies the direct effects of higher interest rates for households that refinance but also lowers utility even for households that never plan to refinance. What are the relative contributions of direct vs. indirect multiplier effects? In the third and fourth rows of Table 7 we set $\phi_r^h = \phi_r^y = 0$ and recompute the size of transfers when local multipliers are set to zero. These rows show that direct interest rate effects account for about 60% of the transfers from the constant interest rate policy.

It is also useful to relate these full model results to the back-of-the-envelope results from the previous section. In that section, we showed that when ignoring indirect feedback effects and household reoptimization and focusing only on originations rather than on all loans, we arrived at an already substantial transfer of roughly \$15 billion. Our structural model shows that when accounting for all of these missing effects, implied transfers rise to an even more substantial \$47 billion. Around 40 percent of this increase comes from indirect multiplier effects on local income and house prices, which are not captured by the back-of-the-envelope calculation. If we turn off the indirect multiplier effects in the model, transfers fall to just over \$28 billion. The general equilibrium effects of the interest rate policy on local house prices and therefore local income thus accounts for a substantial portion of the reason why our model is yielding transfers larger than our back of the envelope calculation. The remainder of the difference arises from the fact that the model measures the welfare consequences of the GSE policy for all households rather than just those adjusting their mortgages in a given year and also accounts for household reoptimization. Clearly, the back-of-the-envelope numbers and those from the model are not identical, and including effects that cannot be captured in the simple reduced-form exercise amplifies our conclusions. Nevertheless, the fact that the overall order of magnitude of welfare consequences is similar between the back-of-the-envelope calculation and the model provides

³⁵This value is equal to $\$350 \cdot 115/2 + \$465 \cdot 115/2$ million households. We divide by 2 because half the households in our model live in regions that get positive shocks, while the other half live in regions that get negative shocks.

³⁶Households who do not refinance will also be affected due to the dynamic nature of the problem: changes in interest rates will affect the value of refinancing in the future and thus household continuation values.

some reassurance that our model does not produce implausible results.

Our model also allows us to assess the effects of the policy across different subgroups of the population. We choose to focus on two dimensions of heterogeneity: age and income. Tables 8 shows the constant interest rate policy has the largest effects on middle-income, middle-aged households. In our model, the importance of mortgage rates for household welfare depends on the level of their mortgage debt, and this is closely tied to age and income. The model implies that the GSE pricing policy has a much larger effect on middle-aged households relative to younger households because the young mostly choose to rent and are thus less sensitive to the local mortgage rate. Similarly, the implied transfer is largest for middle-income households within each region, as the poorest households do not own houses, while the richest households have little mortgage debt. Thus, not only are there important transfers at the regional level, there is also important variation in these transfers across subgroups. Our model suggests that in many ways the GSE constant interest rate policy distributes large amounts of resources within middle-class households.

All of our results so far (in Tables 7 and 8) focus on the ex-post redistributive consequences of the GSE pricing rule, because this makes our results more comparable to existing studies of fiscal transfers. For example, studies of state transfers arising from the federal income tax system focus on the transfers from states with high income to those with low income rather than on the ex-ante consequences of the tax system behind the “veil of ignorance.” Similarly, studies of unemployment benefits often look at the effects on individuals who actually become unemployed rather than their ex-ante consequences for utility. Nevertheless, it is straightforward to calculate the ex-ante welfare effects of the GSE constant interest rate policy. With concave utility, if the variable interest rate resulted in a pure mean preserving spread in consumption, it would necessarily lower ex-ante welfare. We find that in our benchmark results, the ex-ante lifetime consumption equivalent is nevertheless very small: households would pay only 0.1 percent of current consumption to move from the variable to the constant interest rate policy. Part of the reason for the small welfare effects is that in general the costs of business cycles are quite small (a point made in Lucas 1987). The welfare costs of the regional business cycles in our model are slightly higher because the regional shocks we estimate are more persistent than the shocks typically estimated for aggregate business cycles. Despite having relatively small ex-ante welfare gains, our model predicts large ex-post transfers across regions. These results are robust to various changes in our parameter specification.

VII.D Robustness and Model Discussion

VII.D.1 Sensitivity of Results to ϕ^r

In our benchmark calibration, we pick ϕ^r to match the variation in interest rates observed in the jumbo market during the Great Recession. However, if the jumbo market piggybacks off of the GSE policy in picking interest rates, the true sensitivity might be larger than what we find in the data. Conversely, our empirical section controlled for various observables around the conforming threshold and argued that observable default varies smoothly across the threshold. However, if borrowers just above the conforming threshold were instead riskier than borrowers just below

the threshold, then our benchmark estimates of ϕ^r might be overstated. Table 9 shows implied transfers under various values of ϕ^r . In particular, we target a 35-basis-point variation in response to a two-standard-deviation regional shock (referred to as “larger variation”) and then separately a 15-basis-point variation in response to a two-standard-deviation regional shock (referred to as “smaller variation”). Unsurprisingly, the level of implied transfers is increasing in ϕ^r . Increasing the variation by 10 basis points in response to a two-standard-deviation change in local economic activity increases the total transfers from good to bad regions by about 50 percent relative to the base specification. Cutting the variation by 10 basis points in response to a two-standard-deviation change in local economic activity reduces the total transfer from good to bad regions by about 50 percent. The key point is that even with a much smaller value of ϕ^r (compared with what we estimate from the prime jumbo data), the transfers from good regions to bad regions through the constant interest rate policy remain quite large.

VII.D.2 Alternative Mortgage Contracts

In our baseline model, we make many assumptions surrounding the mortgage contracts available to households. In particular, we assume that all mortgages are fixed rate, that households can adjust their mortgage balances without resetting the rate on their mortgage, and that households can refinance to a different mortgage rate without paying a cost. These assumptions capture many realistic aspects of mortgage contracts but at the same time make some simplifications in order to increase the tractability of the model solution. For example, most mortgages in the United States are fixed rate. Additionally, most households can reduce their mortgage balance (by making extra payments) without paying any refinancing costs. However, households typically have to pay a refinancing cost to increase mortgage balances (if they do not have a home equity line of credit) or to reset their rate. Several of these assumptions simplify the solution of the model substantially, but how important are they for our conclusions?

In the Online Appendix, we explore the sensitivity of our results to changing the mortgage contracts available to households within the model. In particular, we first explore a version of the model where we assume variable rather than fixed-rate mortgages (i.e., the mortgage rate paid by a household in period t is always equal to $r_{k,t}^{m,market}$.) Next, we allow for costly refinancing by forcing households to pay a fixed cost, F^{refi} every time they refinance. Finally, we solve a version of a much more complicated model with fixed-rate, fixed-balance mortgages and costly refinancing. The details of these robustness exercises can be found in the Online Appendix. Importantly, we find the size of cross-region transfers is quantitatively similar across all these alternative specifications. From this, we conclude that our results are not dependent on the type of mortgage contract we choose to model in our base specification.³⁷

³⁷It might seem surprising that moving from fixed to variable interest rates has little effect on our conclusions. As discussed in the Online Appendix, this occurs because most households make their housing decisions largely based on life-cycle considerations and idiosyncratic reasons rather than timing purchases to take advantage of low rates. Thus, most households end up having a fixed rate mortgage which is not too different from the prevailing rate.

VII.D.3 Modeling default

In our results thus far, we do not explicitly model household default decisions and instead capture the effects of local conditions on credit risk and interest rates through their effects on the risk-adjustment factor (Ψ). While it would be desirable to build a model with endogenous credit risk and interest rates rather than exogenously linking these variables to local conditions, this would also substantially complicate the model. To what extent would such a model change any of our conclusions? Since we are interested in evaluating the effects of a change in interest rate policy, the main way in which endogenizing default could matter would be if default responds strongly to interest rates. These responses might then alter the size of implied transfers. For example, if defaults also rise with interest rates, then abandoning the constant interest rate policy would further increase the response of default risk and interest rates to local conditions, which would amplify our conclusions.

While a full-fledged model of strategic default with endogenous interest rates is beyond the scope of this paper, it is straightforward to provide some sense of the sensitivity of our results along this dimension. In the Online Appendix, we amend our model by endogenizing default using a simple version of Campbell and Cocco (2015). Reassuringly, this model suggests that our results are unlikely to be altered by explicitly modeling local credit risk and endogenous interest rates. This is because in this extended model, default barely responds to local interest rate variation: a one-standard-deviation (cross-MSA) increase in interest rates only increases default by 0.027 (cross-MSA) standard deviations.³⁸ Applying our estimates from the first half of the paper, this increase in expected default should lead to only an additional 1.1-basis-point-increase in interest rates.³⁹ This would increase the size of implied transfers, but in a fairly negligible way that is well within the range of robustness we considered in Table 9. Thus, we conclude that formally modeling default and endogenous interest rates will greatly complicate our model without substantively altering its quantitative implications.

VII.D.4 Labor Mobility

Our base assumption is that labor is immobile across regions. One way to support a regional equilibrium is by allowing factors to move across regions. Two facts make us believe that allowing for such mobility would not alter our results in any meaningful way. First, we estimate our regional income processes on actual data. The underlying data takes into account both the true underlying process driving the regional shocks to income as well as any endogenous response of factors across regions. Our approach cannot distinguish between large regional shocks that are mitigated in part through factor mobility and slightly smaller shocks in a world with less factor mobility. Given that we are using these processes to calibrate our model, any migration that actually occurs will be captured in our estimates. Second, there is a large literature showing that permanent regional shocks lead to sizable migration responses (see, for example, Blanchard and Katz 1992). There is less evidence that regional migration is important in response

³⁸In particular, a one-standard deviation increase in interest rates (12.5 basis points) in the model increases default from 2.58% to 2.66%. Dividing this .08% change by the 2.9% cross-MSA standard deviation in the last row of Table 1 delivers the 0.027-standard-deviation change in default.

³⁹A 1% increase in predicted default implies that interest rates should rise by 13.48 basis points ($0.08 * 13.48 = 1.07$ basis points).

to the kinds of temporary regional shocks captured by our model. With costly migration, individuals may choose to ride out the regional business cycle as opposed to paying the migration cost and moving to another region. In fact, there was very little net migration from regions hit hard during the most recent recession to regions that were hit less hard (see Yagan 2014). For these reasons, we believe that abstracting from migration will not significantly change the model’s main results.

VII.D.5 Endogenous Income and House Prices

In our baseline model, we allow for feedback from interest rates to local house prices and income that is consistent with observed relationships in the data. However, we do not endogenize this feedback, since this would add numerous layers of complication to an already complex model.⁴⁰ Ultimately, the parameters of this more complicated model would need to be picked so as to match empirical relationships between interest rates, income, and house prices. For tractability, we instead skip this intermediate step and match empirical relationships directly.

This could potentially be problematic if the historical relationship between income, house prices, and interest rates would change if the GSE constant interest rate policy was altered. However, this is unlikely to be the case: we are not considering the elimination of the GSEs, which would have large macroeconomic effects on housing markets. Instead we consider a simple change in the pricing of local risk, which is already present for the private segment of the market. Furthermore, in the Online Appendix, we show that our results are insensitive to the overall level of variation in house prices and income. In addition, in the model, housing demand falls only mildly in response to a 25-basis-point increase in interest rates, so that we would not expect any equilibrium effects from changes in GSE interest rate policy to be dramatic or alter historical relationships. Put differently, we believe we have shown that the constant interest rate policy plays an important role in redistributing resources across regions. However, it is unlikely that the introduction of 25-basis point variation in interest rates would generate systemic aggregate effects or change the overall institutional structure of housing markets in a way that would invalidate our modeling strategy.

VII.D.6 Additional Robustness

Finally, in the Online Appendix, we explore the robustness of our model results to changes in a variety of other model parameters and calibration targets. For example, we explore the sensitivity of our results to changes in both ϕ^y and ϕ^h as well as to changes in housing adjustment costs, the calibration of wealth, and housing expenditure shares. As we show, these parameters have little effect on the level of cross-region transfers, and if anything our baseline calibration is relatively conservative.

⁴⁰For example, we would have to take a stand on the importance of nominal frictions, elasticity of housing supply, share of tradeables in local employment, and capital and labor mobility. This would also introduce one or more additional state variables and require forecasting and solving for equilibrium.

VIII Conclusion

Recent business cycles have yielded dramatic disparities in regional outcomes within the United States. While prior research has carefully studied the role of tax and transfer systems in mitigating local shocks, we propose an entirely different mechanism through which federal policy may provide some regional redistribution. In this paper we empirically document the extent to which local mortgage rates (do not) vary with local economic conditions. The United States is unique in the extensive role that government institutions play in the mortgage market. In 2008, when placed into conservatorship, the Federal National Mortgage Association (Fannie Mae) and the Federal Home Loan Mortgage Corporation (Freddie Mac) owned or guaranteed roughly half of the \$12 trillion U.S. mortgage market. We establish empirically that, despite large regional variation in predictable default risk, there is essentially no spatial variation in GSE mortgage rates (conditional on borrower observables). In contrast, we show that mortgage rates in the private “prime jumbo” market, where loans are larger than the conforming limit but comparable on many dimensions to loans backed by the GSEs, were strongly correlated with ex-ante predicted default probabilities across geography. Using a structural spatial model of collateralized borrowing where households face both idiosyncratic and region-specific shocks, we estimate the magnitude of transfers across regions when interest rates are set using a constant national rate rather than in response to local risk. Overall transfers are large, and for the regions hit with particularly bad shocks, they are comparable in size to recent fiscal stimulus programs such as tax rebates and unemployment insurance.

Although a range of consequences to the housing and mortgage markets are often attributed to the presence of Fannie Mae and Freddie Mac, their common national interest rate policy is one important and understudied dimension of their impact on households’ choices. By distributing resources across U.S. regions in a state-contingent way, in addition to providing countercyclical liquidity to the mortgage market, Fannie Mae and Freddie Mac provide meaningful insurance during aggregate downturns. It is worth noting that, so long as the current structure of GSEs persists, we expect this resource redistribution to continue since their lower cost of funds — attributed to both the implicit “too-big-to-fail” guarantee and their scale — makes it difficult for the private market to undo any potential mispricing by the GSEs. In particular, if political constraints prevent the GSEs from raising interest rates in declining markets and lowering interest rates in relatively strong markets, the cost differential prevents private markets from competing with lower interest rates in relatively stronger markets. We hope to better understand the impact of the current structure of GSEs and in particular the constant rate policy on housing market activity and house prices in future work.

We conclude by noting two important caveats of our result. Throughout the analysis, our benchmark for how much mortgage rates should vary with ex-ante default probabilities in the GSE market is the variation we observe in our sample of prime jumbo loans. We feel this is a good comparison group, particularly when we match on factors like MSA, FICO score, LTV ratio, documentation type, fixed-rate, and 30-year term. However, we realize that even in private markets, political economy considerations may still limit the extent to which interest rates can vary

spatially. Additionally, discussions with securitizers of private mortgages suggest that they often attempt to use the same mortgage pricing platforms as the GSEs to increase their pricing models' transparency for secondary market investors. Both of these factors may lead us to understate the spatial variation we would observe in the mortgage market if GSEs fully priced local default probabilities. These factors would in turn imply that our estimates of state-contingent transfers across regions will be a lower bound on the true extent of transfers. It is worth noting, however, that using our model, we re-computed transfers under a number of alternative assumptions about how mortgage rates would vary with local predicted default probabilities across regions and continued to find large implied transfers.

Additionally, we want to stress that we are not saying the GSE policy is the optimal way to transfer resources across regions in state-contingent ways. Many have argued for the potential welfare-reducing role of the GSEs in distorting the allocation of capital (see, e.g., Elenev et al. 2015). Moreover, policy makers have many other tools to transfer resources across regions if they so desire. Our goal in this paper is to study the impact of a current policy as opposed to providing either a full welfare analysis of the existence of the GSEs or discussing the optimal way to transfer resources across regions.

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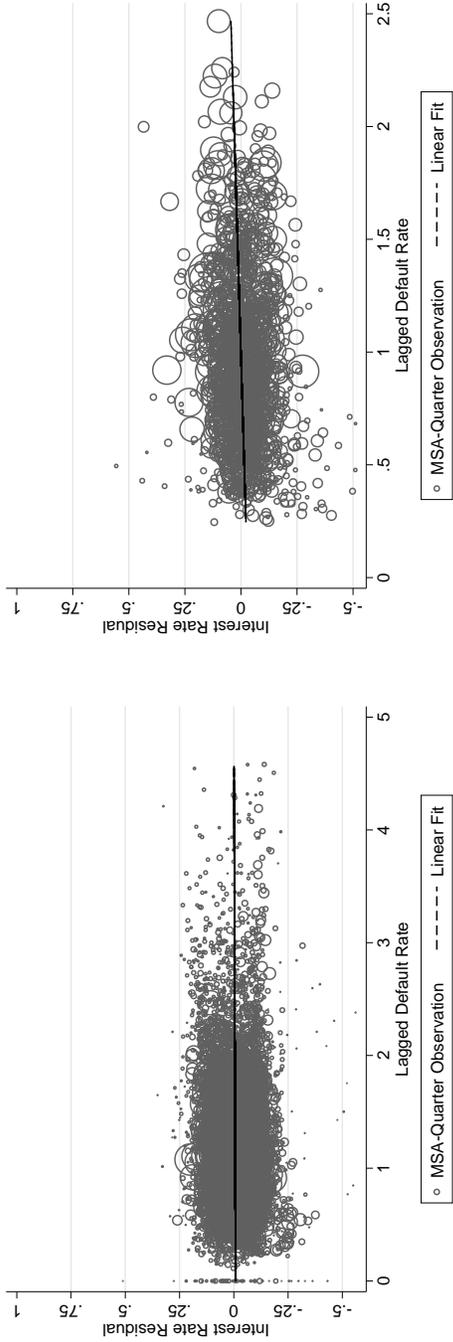
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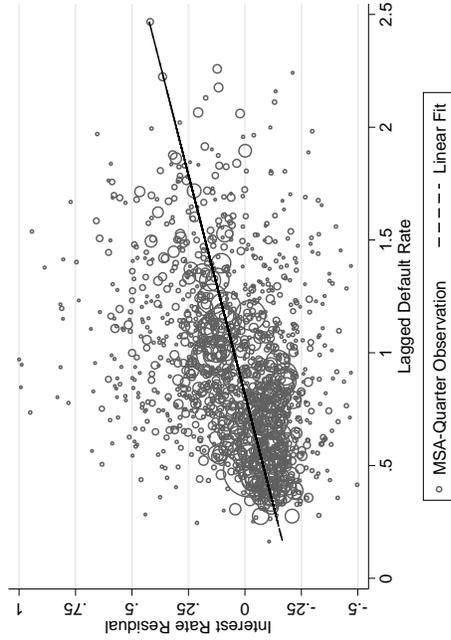
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Figure 1: Relationship between Interest Rates and Lagged Local Default, 2001-2006



(a) GSE Loans

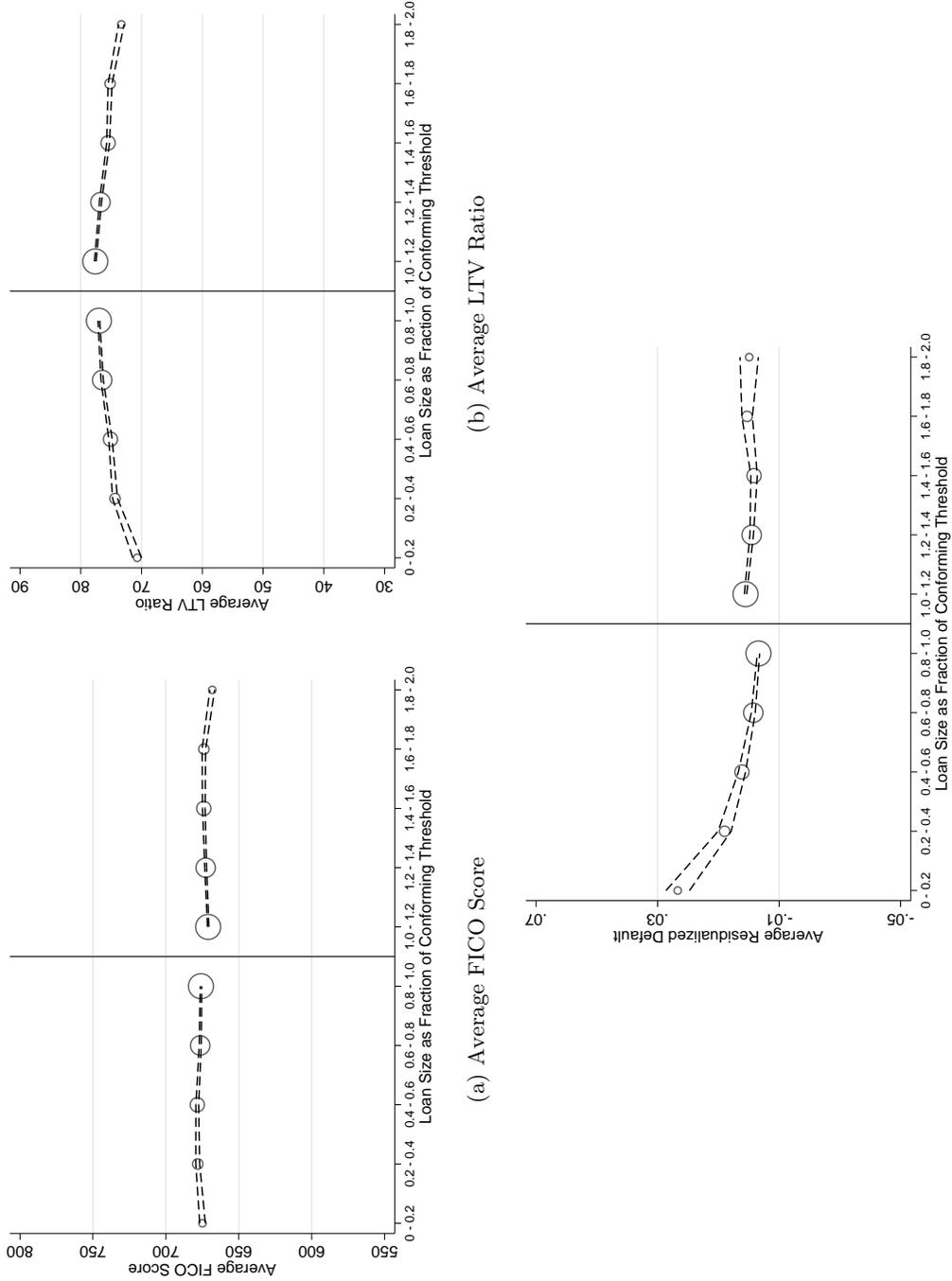
(b) GSE Loans Matched on FICO and LTV



(c) Non-GSE Loans

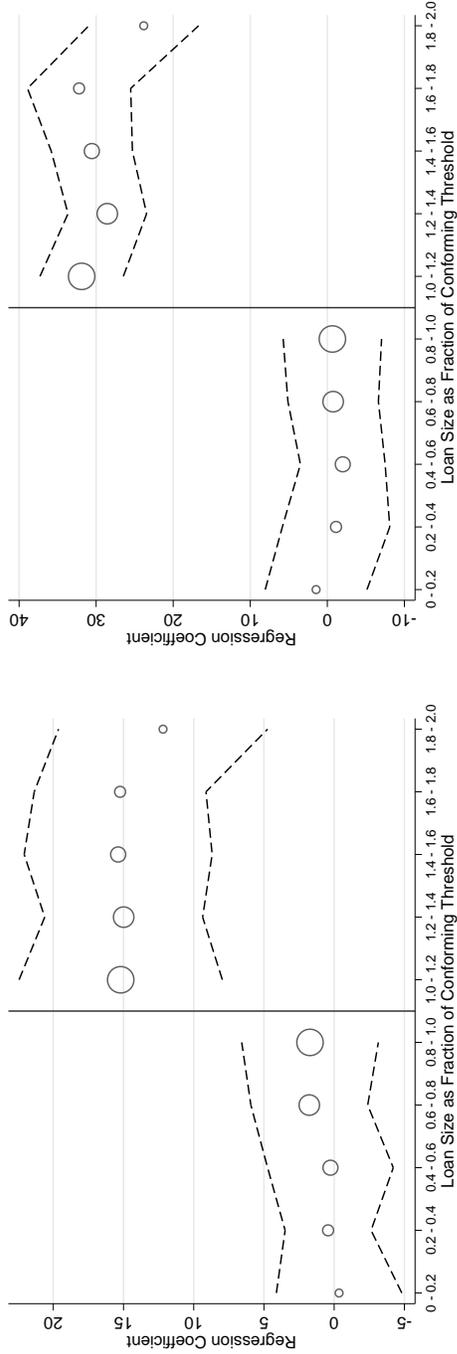
Note: This figure shows the relationship between residualized interest rates and residualized lagged MSA-level default of loans originated within the last two years for three samples. Panel a presents the relationship in the GSE market for all 374 available MSAs. Panel b restricts the GSE loans to the 106 MSAs where non-GSE loans are present, and matched based on the FICO and LTV distributions of non-GSE loans for comparability. Panel c shows the relationship in the non-GSE loan market. The adjusted residual removes year*quarter fixed effects and semi-parametric controls for FICO and LTV interacted with year*quarter fixed effects.

Figure 2: Average FICO Score, LTV Ratio, and Default Rate, by Loan Amount, 2001-2006



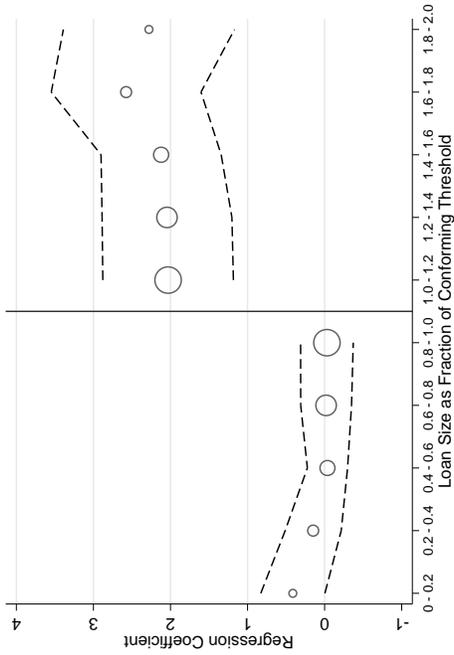
Note: This figure shows (a) the average FICO credit score, (b) the average LTV ratio, and (c) the average residualized default rate in each loan amount bin around the conforming loan limit. Residualized default rate removes year*quarter fixed effects and semi-parametric controls for FICO and LTV interacted with year*quarter fixed effects. To the left of the limit (values ≤ 1), loans are insured and securitized by the GSEs. To the right of the limit (values ≥ 1), loans are securitized by the private non-GSE market. The GSE sample is restricted to the MSAs where non-GSE loans are present, and matched based on the FICO and LTV distributions of non-GSE loans for comparability. Each point in each figure is an average for a loan amount bin representing 10% of the loan amount distribution from \$0 to twice the conforming loan limit. 95 percent confidence intervals are represented by dashed lines. See text for details.

Figure 3: Relationship between Interest Rates and Three Measures of Default, 2001-2006



(a) Predicted Default

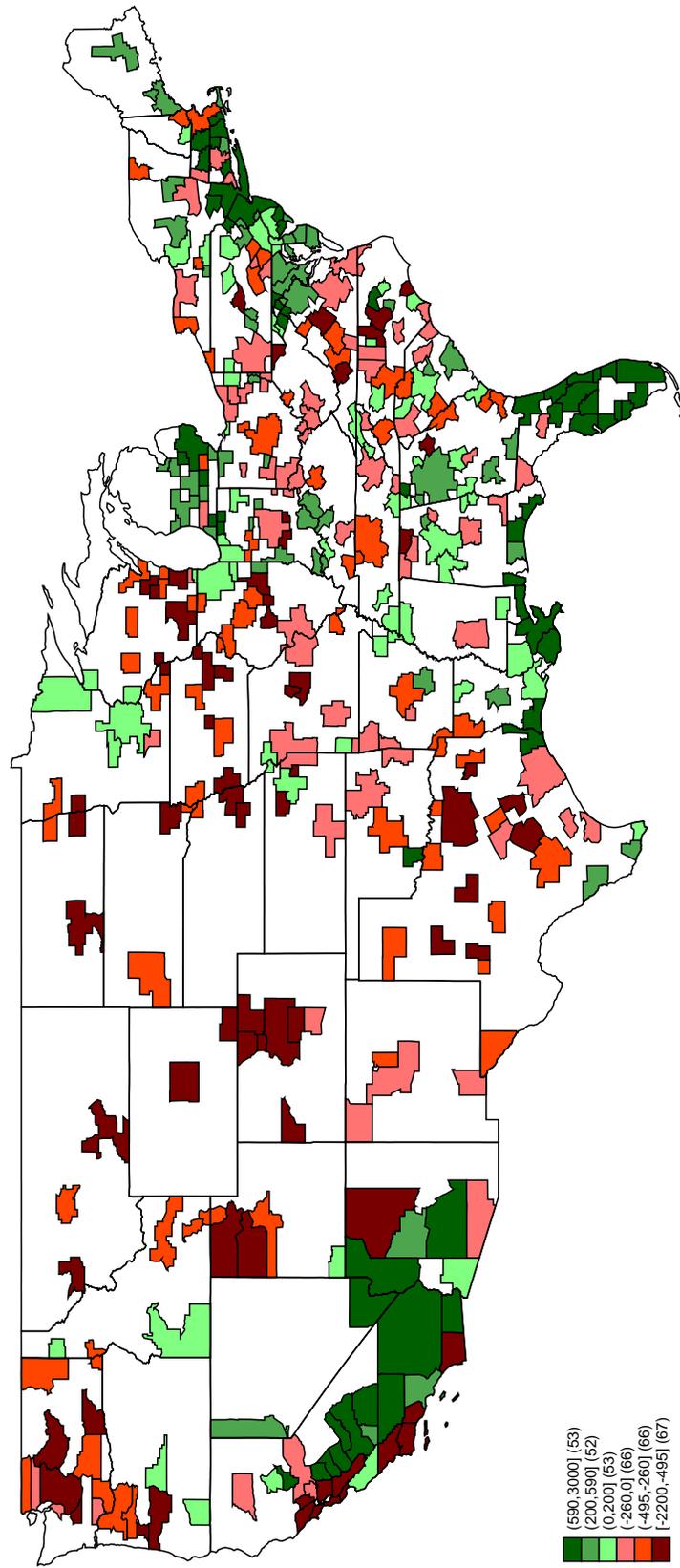
(b) Lagged Default



(c) Actual (Realized) Default

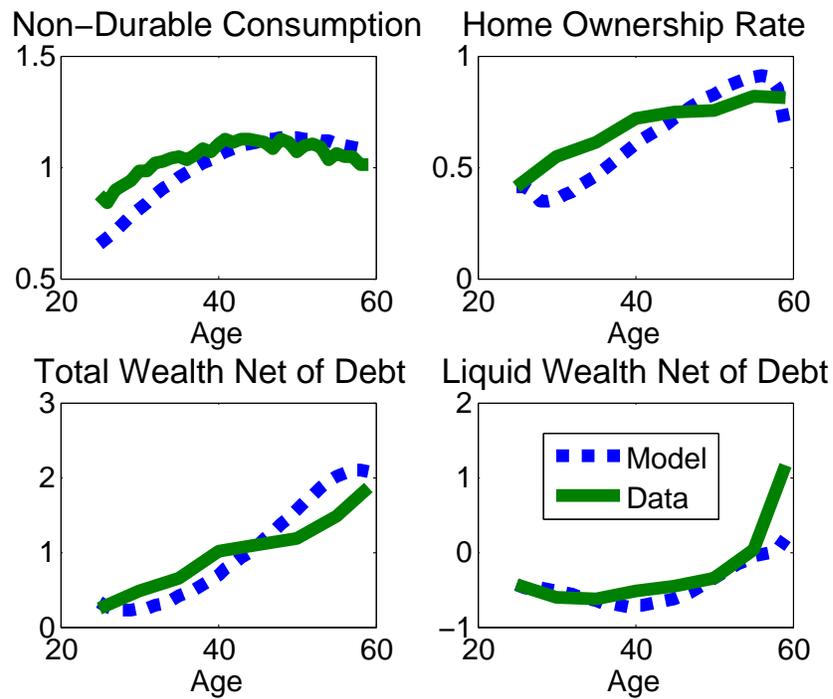
Note: This figure shows the relationship between residualized interest rates and default rates in each loan amount bin around the conforming loan limit. Adjusted residual removes year*quarter fixed effects and semi-parametric controls for FICO and LTV interacted with year*quarter fixed effects. To the left of the limit (values ≤ 1), loans are insured and securitized by the GSEs. To the right of the limit (values ≥ 1), loans are securitized by the private non-GSE market. The GSE sample is restricted to the MSAs where non-GSE loans are present, and matched based on the FICO and LTV distributions of non-GSE loans for comparability. Each point in each figure is a regression coefficient for a loan amount bin representing 10% of the loan amount distribution from \$0 to twice the conforming loan limit. 95 percent confidence intervals based on standard errors clustered at MSA level are represented by dashed lines. Standard errors for results relying on predicted default are bootstrapped (500 repetitions, clustered at MSA level). See text for details.

Figure 4: Transfers by MSA, 2007-2009



Note: This figure shows the average value of transfers $Transfer_{it}^{PV}$ for each MSA over the 2007-2009 period. Positive values represent regions receiving subsidies while negative values represent regions being taxed. See text for additional description.

Figure 5: Average Life Cycle Profiles: Model Simulation vs. Data



Note: This figure shows average lifecycle profiles generated from the model compared with actual data. Data sources: Nondurable consumption comes from Aguiar and Hurst (2013). Home ownership rates are calculated from the March CPS, and wealth statistics are calculated from SCF data. Non-durable consumption and total wealth net of debt are normalized to their life-cycle means. Homeownership rates are unadjusted, and liquid wealth net of debt is normalized by the mean life-cycle income. See text for additional details.

Table 1: Descriptive Statistics

	Time Period					
	2001-2006			2007-2009		
	GSE All	GSE Restricted MSAs	GSE Matched Sample	Prime Jumbo	GSE All	GSE Restricted MSAs
Number of Loans	13,110,212	8,052,967	70,327	70,327	4,861,259	3,677,984
Median FICO	728	727	658	656	756	757
Median LTV	0.78	0.75	0.79	0.80	0.76	0.75
MSAs covered	374	106	106	106	374	106
Mean Interest Rate (%)	6.25	6.22	6.33	6.66	5.65	5.63
Mean 2-Yr Delinquency Rate (%)	1.6	1.4	3.0	2.1	3.8	4.0
<u>Cross MSA SD of Interest Rates</u>						
Unconditional (percentage points)	0.544	0.557	0.578	0.657	0.627	0.623
Conditional (percentage points)	0.076	0.072	0.086	0.165	0.070	0.064
<u>Cross MSA SD of Delinquency Rates</u>						
Unconditional (percentage points)	1.5	1.2	3.2	2.7	4.0	4.3
Conditional (percentage points)	1.3	1.1	2.8	2.5	2.9	2.9

Note: This table provides summary statistics for the samples of GSE and non-GSE (“prime jumbo”) loans. The different columns refer to different samples and different time periods, with the first four columns referring to loans originated between 2001 and 2006, and the last two columns featuring loans originated between 2007 and 2009 (after the non-GSE market ceased large-scale operation). The first column uses all loans in our sample originated by the GSEs, the “Restricted MSA” sample uses only those MSAs with prime jumbo loans present (during 2001 to 2006), and the “GSE Matched Sample” restricts to these 106 MSAs and matches the distribution of FICO scores and LTV ratios in the non-GSE sample. Conditional measure of standard deviation removes year*quarter fixed effects and semi-parametric controls for FICO and LTV interacted with year*quarter fixed effects. See text for details.

Table 2: Responsiveness of Conditional MSA Interest Rates to Lagged GSE Default Rates

	Time Period					
	2001-2006			2007-2009		
	(1)	(2)	(3)	(4)	(5)	(6)
	GSE All	GSE Restricted MSAs	GSE Matched Sample	Prime Jumbo	GSE All	GSE Restricted MSAs
Coefficient on Lagged GSE Default Rate	0.16 (0.29)	2.40 (2.84)	3.54 (2.75)	30.55 (2.49)	1.12 (0.23)	1.09 (0.27)
Implied Basis Point Change in Mortgage Rate to a Two Standard Deviation Change in Lagged GSE Default	0.28	1.78	1.93	14.54	3.18	3.27
Observations	13,109,968	8,052,967	70,327	70,327	4,861,218	3,677,984

Note: This table shows the coefficient from a regression of conditional MSA interest rates during a given quarter on lagged GSE default rates. The different columns refer to different samples and different time periods for which the conditional MSA interest rates and lagged default rates are based. The different sample definitions are discussed in the notes to Table 1. The implied change in interest rate to a two standard deviation change in lagged GSE default is simply the coefficient times the standard deviation of lagged GSE default across the MSAs in the relevant sample. Standard errors in parentheses clustered at the MSA level. See text for details.

Table 3: Relationship Between Conditional MSA Interest Rates and MSA Predicted Defaults, 2001-2006

Predictive Default Measure	Base Specification				Regression Discontinuity Specification
	(1) GSE Matched Sample	(2) Prime Jumbo Sample	(3) Difference in Coefficients	(4) p-value of difference	
Predicted Default Using Lagged Local GSE Default	2.10 (1.78)	12.04 (1.68)	9.94	<0.001	13.48 (4.56)
Lagged Default (Random Walk)	3.56 (2.76)	12.60 (3.16)	9.04	<0.001	13.04 (4.57)
Actual Default (Perfect Foresight)	0.26 (0.14)	2.12 (0.40)	1.86	<0.001	2.06 (0.44)
Observations	70,327	70,327			
Time, FICO, and LTV Controls Included	Yes	Yes	Yes	Yes	

Note: This table presents coefficients from regressions of conditional MSA interest rates on three measures of predictive default: lagged default rates, actual default rates, and predicted default rates. Lagged default is measured within-sample depending on GSE or non-GSE loans. Predicted default rates are constructed using lagged GSE default rates. The sample of GSE loans is restricted to the 106 MSAs where non-GSE loans are present during the time period 2001-2006 and matches the distribution of FICO scores and LTV ratios in the non-GSE sample. The different sample definitions are discussed in the notes to Table 1. The first two columns show the separate OLS estimates, columns 3 and 4 test for differences, while column 5 shows the “regression discontinuity” estimates shown in Figure 3, using bins that are each 20% of the loan amount distribution between \$0 and twice the conforming loan limit. Standard errors in parentheses clustered at the MSA level. Standard errors for results relying on predicted default are bootstrapped (500 repetitions, clustered at MSA level) to account for the generated regressor. See text for details.

Table 4: Standard Deviations of Predicted Default

Predicted Default Measure	Time Period		
	2001-2006		2007-2009
	GSE Matched Sample	Prime Jumbo Sample	GSE Restricted MSAs
Predicted Default Using Lagged Local GSE Default	0.006	0.009	0.011
Lagged Default (Random Walk)	0.004	0.005	0.015
Actual Default (Perfect Foresight)	0.030	0.027	0.043

Note: This table presents the standard deviation of each measure of predicted default for each sample used in the analysis, GSE loans and non-GSE loans originated between 2001 and 2006, and GSE loans originated between 2007 and 2009. The GSE sample during the 2001-2006 period is restricted to the MSAs where non-GSE loans are present and matched on the FICO and LTV distributions of the non-GSE sample for better comparability. The GSE sample during the 2007-2009 period is restricted to the MSAs where non-GSE loans were present during the period 2001-2006. See text for details of sample construction.

Table 5: Predicted Counterfactual Two Standard Deviation Cross MSA Variation in GSE in Interest Rates

Predicted Default Measure	Time Period	
	2001-2006	2007-2009
Predicted Default Using Lagged Local GSE Default	0.162	0.297
Lagged Default (Random Walk)	0.104	0.391
Actual Default (Perfect Foresight)	0.124	0.177

Note: This table presents the interest rate response to a two standard deviation change in each predicted default measure for two time periods, 2001-2006 and 2007-2009. These values are obtained by multiplying the values in Table 3, column 5 with two times the standard deviations found in Table 4 for GSE loans.

Table 6: Robustness of Regression Discontinuity Estimates

Predictive Default Measure	Specification				
	(1)	(2)	(3)	(4)	(5)
Predicted Default Using Lagged Local GSE Default	13.48 (4.56)	12.99 (5.04)	11.73 (4.74)	12.35 (5.03)	15.64 (4.56)
Time, FICO, and LTV Controls Included	Yes	Yes	Yes	Yes	Yes
Predicted Payment Controls Included	No	Yes	No	Yes	No
MSA Fixed Effects Included	No	No	Yes	Yes	No
Restrict to LTV ≤ 0.8	No	No	No	No	Yes

Note: This table presents regression-discontinuity estimates of the difference in the relationship between interest rates and predicted defaults around the conforming loan limit. The regression estimates here are estimated as in Figure 3, using bins that are each 20% of the loan amount distribution between \$0 and twice the conforming loan limit. Lagged default and lagged prepayment measures are constructed within-sample depending on GSE or non-GSE loans. The GSE sample is restricted to the MSAs where non-GSE loans are present and matched on the FICO and LTV distributions of the non-GSE sample for better comparability. Each coefficient represents a separate regression. Standard errors in parentheses clustered at the MSA level. Standard errors for results relying on predicted default are bootstrapped (500 repetitions, clustered at MSA level) to account for the generated regressor. See text for details.

Table 7: One-Time Consumption Equivalent Necessary to Accept Region-Specific Rates

	Regional Employment				
	-2 Standard Deviation	-1 Standard Deviation	0 Standard Deviation	1 Standard Deviation	2 Standard Deviation
<u>Baseline:</u>					
Percent consumption gain	2.26%	1.18%	-0.02%	-1.04%	-2.12%
Dollar per household effect	\$870	\$470	-\$8	-\$457	-\$988
<u>No Feedback Multiplier:</u>					
Percent consumption gain	1.36%	0.70%	-0.04%	-0.64%	-1.26%
Dollar per household effect	\$524	\$279	-\$17	-\$281	-\$586

Note: This table shows the consumption gains estimated from our baseline model. See text for a description of baseline parameters and the policy experiment. The consumption gain in row 1 is equal to $\lambda \times 100$, where λ is the percentage change in consumption that makes a household indifferent between a variable and constant interest rate. To compute dollar equivalents in row 2, we use the formula $(\lambda \times 100)(\lambda \times 41885 \times \frac{C_{region}}{C_{overall}})$, where \$41,885 is average household consumption, adjusted for the fact that consumption varies with local economic activity. Baseline results include both direct interest rate effects as well as indirect feedback effects. The No Feedback Multiplier results include only direct effects. Calculations are restricted to working age households subject to labor market risk.

Table 8: One-Time Consumption Equivalent Necessary to Accept Region-Specific Rates, by Age and Income

	Regional Employment				
	-2 Standard Deviation	-1 Standard Deviation	0	+1 Standard Deviation	+2 Standard Deviation
Consumption gain: Overall	2.26%	1.18%	-0.02%	-1.04%	-2.12%
Consumption gain: Young	1.76%	1.02%	-0.08%	-0.90%	-1.98%
Consumption gain: Middle Aged	2.50%	1.26%	0.00%	-1.14%	-2.18%
Consumption gain: Low Income	1.46%	0.76%	-0.06%	-0.82%	-1.74%
Consumption gain: Middle Income	2.64%	1.36%	-0.02%	-1.16%	-2.32%
Consumption gain: High Income	2.18%	1.22%	0.04%	-0.92%	-1.60%

Note: This table shows the consumption gains estimated from our baseline model, by age and income. See text for a description of baseline parameters and the policy experiment. The consumption gain in each row is equal to $\lambda \times 100$, where λ is the percentage change in consumption that makes a household indifferent between a variable and constant interest rate. Calculations are restricted to working age households subject to labor market risk. We define “young” as households of ages 25-34 and “middle aged” as households of 35-59. Income groups are split into the highest one-third, middle one-third and lowest one-third of the stochastic income process for working households.

Table 9: Sensitivity to Different Values of ϕ^r

	Regional Employment				
	-2 Standard Deviation	-1 Standard Deviation	0	+1 Standard Deviation	+2 Standard Deviation
Consumption gain: Bnchmk (25 bp)	2.26%	1.18%	-0.02%	-1.04%	-2.12%
Larger Variation (35 bp)	3.16%	1.56%	-0.05%	-1.50%	-2.63%
Smaller Variation (15 bp)	1.39%	0.74%	0.00%	-0.67%	-1.32%

Note: This table shows the robustness of our estimated consumption gains to different interest rate sensitivities to local economic conditions. See text for a description of baseline parameters and the policy experiment. The consumption gain in each row is equal to $\lambda \times 100$, where λ is the percentage change in consumption that makes a household indifferent between a variable and constant interest rate. We alter the value of ϕ^r such that the benchmark variation in regional employment produces alternative variability in mortgage rates.

A-1 FOR ONLINE PUBLICATION: Empirical Appendix

In this appendix, we provide more details on our matching procedure and results from a number of robustness exercises.

A-1.A Matching Procedure

As described in Section III.C of the text, our primary set of comparisons are between loans originated in the non-GSE jumbo market and comparable loans originated in the GSE market in the 2000–2006 period since the jumbo market dries up after this period (Figure A-1). In order to make a closer comparison, in addition to our standard sample restrictions (30-year fixed rate full documentation loans made for either purchase or refinance of a single-family residence or condo), we also select our subsample of GSE loans to be from the same markets as the non-GSE loans in our sample. This restricts our sample to using GSE loans from 106 of the 374 possible MSAs.

To further make similar comparisons, we select our subsample of GSE loans to match the distribution of both FICO and LTV scores in the non-GSE market in a way that is symmetric across the loan amount distribution. To do so, we randomly select loans from ranges of FICO scores and LTV scores based on the quartiles of the non-GSE distribution in each “bin” of loan amounts used in the regression-discontinuity style estimates. For instance, there are 32,123 loans in the bin representing loans that are between the conforming loan limit and 1.2 times the conforming loan limit in the non-GSE market. To match FICO and LTV in the bin on the other side of the conforming loan limit (between 0.8 and 1.0 of the limit), we split the GSE loans in that bin into each quartile of the FICO \times LTV distribution, and select roughly 2,000 loans from each of these 16 bins.

This procedure leads to a very close match of the distribution of FICO and LTV scores in the non-GSE market. Appendix Table A-1 shows the results of the procedure. Not only are the means very similar across FICO and LTV, but the entire distributions line up closely because of this procedure.

A-1.B Robustness and Extensions

In this part of the Appendix, we discuss in detail a variety of robustness results we studied. Some of the robustness specifications looked at whether different factors could explain our results (e.g., regional variation in points or fees or regional variation in pre-payment risk). Other robustness specifications looked at different outcome variables (e.g., loan quantity adjustment). Still other robustness exercises looked at other specifications (e.g., exploiting time variation in the conforming limit within a MSA). Finally, our last set of robustness exercises looked at regional variation in GSE and prime jumbo loans to other policies that may affect the risk adjusted return of making a loan (e.g., local lender competition or local bankruptcy laws)

A-1.B.1 Exploring Regional Variation in Mortgage Quantities Instead of Interest Rates

In our primary analysis, we explored the adjustment of mortgage prices in response to spatial variation in regional risk. One may also expect some adjustment to occur on the quantity side, i.e., both on the extensive (loan approval) and intensive (loan amount, conditional on approval) margins.⁴¹ Unfortunately, we are not able to explore variation on the extensive margin, because the only available data on the extensive margin (HMDA database) does not have borrower-level variables, which are crucial for differentiating borrower-level risk from location specific risk. We are, however, able to explore quantity movements on the intensive margin using our data. Appendix Figure A-2 shows the relationship between lagged default rates and LTV residuals for both the GSE sample (top panel) and the prime jumbo sample (bottom panel). These figures are similar to Figure 1 of the main text. We residualize LTV controlling for FICO score and time effects in a way similar to our residualization of interest rates. As seen from Appendix Figure A-2, there is little LTV adjustment across MSAs in response to differences in lagged default rates in either sample. If anything, borrowers in riskier places are slightly more leveraged on average. Moreover, there are no statistical differences in the response rates between the two samples. Given this finding, we focus our analysis only on the price margin and not the quantity margin.

Additionally, we focused on a sample where rejection rates are quite small. Specifically, for high quality borrowers with LTV ratios less than 0.8, the chance of the loan being rejected is very small. Yet, even within this sample, we found that the prime-jumbo interest rates varied significantly with ex-ante predictable local default risk while the GSE interest rates did not. We highlight these results more in the “Points, Fees, and Private Mortgage Insurance” subsection below.

⁴¹Note that most models would imply that when lenders reduce loan quantities they would also raise loan prices, so the fact that there is no regional variation in GSE loan prices strongly suggests there is no quantity variation.

A-1.B.2 Prepayment Risk

One potential concern with our base empirical work is that we did not account for other potential local risks that could affect local loan pricing. In particular, aside from default risk, the biggest risk lenders face is prepayment risk. If prepayment risk differs dramatically between GSE loans and prime jumbo loans in a way that is correlated with local default risk, the lack of variation in GSE mortgage rates with local default risk may not be surprising.

In our data, we can track prepayments and thus create a measure of predicted local prepayment risk. In particular, we follow the same procedure as in the main text for local default risk and create three different measures of predicted prepayment risk: the regression-based approach for both samples using lagged local GSE prepayment rates, a perfect foresight model in which the predicted prepayment rate is the actual local prepayment rate for each sample, and a random walk model in which the predicted prepayment rate is the actual lagged local prepayment rate for each sample. Our first finding is that predicted prepayment rates, conditional on loan and borrower observables, are very similar for GSE and prime jumbo loans. For example, using our RD approach, annual predicted prepayment rates were only 1 percentage points lower for prime jumbo loans above the conforming threshold relative to the GSE loans below the threshold (19% vs. 20%).

What matters is whether predicted prepayments are differentially correlated with predicted default rates across the two samples in a way that undoes the results documented above. To explore this, we added predicted prepayment rates as an additional control to all our main empirical specifications. Table 6 from the main paper shows one such specification. We focus on our base RD specification where predicted local default is based on a regression of actual default on lagged GSE default and loan-level controls. Column (1) of Table 6 redisplayes our estimates from column (5) of Table 3. We do this to facilitate comparison across our robustness specifications. Column (2) shows our RD estimates when we add the regression-based measure of predicted prepayments as an additional control. Notice that controlling for predicted prepayment risk does not change the RD estimates in any meaningful way. Again, this is not surprising given the fact that conditional prepayment probabilities barely differ between the samples. These results suggest that predicted prepayment differences are not driving the differential interest rate sensitivities to local default risk between the GSE and private samples.

A-1.B.3 MSA Fixed Effects

Another potential concern with the interpretation of our previous results is that identification could be driven by across-MSA differences in the composition of GSE versus private loans rather than from differential responses of these loans to common local conditions. To address this concern, we re-estimated all our specifications including MSA fixed effects. This allows us to compare GSE loans within an MSA to prime jumbo loans within the same MSA. Column (3) of Table 6 from the main text controls for MSA fixed effects in our RD specification, while column (4) controls for both MSA fixed effects and local prepayment risk. As can be seen from the table, the estimated difference in interest rate responsiveness to local default risk, $(\beta^{jumbo} - \beta^{GSE})$, is essentially unchanged in all the specifications.

A-1.B.4 Points, Fees, and Private Mortgage Insurance

Up until this point, we have not examined regional variation in points paid or other loan fees because points and fees are not recorded in our data. It may be the case that mortgage rates do not vary across MSAs in the GSE sample, but that points and other fees do vary with local default risk. To address this concern, we obtained additional data from one of the GSEs to directly estimate the relationship between effective interest rates and regional risk. The measure of effective interest rates in this data nets off any points and fees (including closing costs) that are charged to the borrower. As shown in Appendix Table A-2, we find no significant relationship between effective interest rates in the *universe* of GSE loans that meet our sample criteria and regional risk, as measured by lagged GSE default. The effect of a two-standard-deviation increase in regional risk is an insignificant 5 basis points increase in effective interest rate. In results not shown, no component of the effective interest rate (either points or fees) were found to be statistically associated with regional risk for these loans.

One additional concern with our analysis is that the securitized lenders may require borrowers with higher LTVs to purchase private mortgage insurance (PMI). The high LTV borrowers (usually those with an origination LTV greater than 80%) would pay for PMI that would insure the lenders against part of the principal loss during the default. Instead of variation in local predicted default risk showing up as variation in local mortgage rates, it could show up as variation in local PMI premiums. Again, this would affect our results only if the PMI was differentially used between the GSE and prime jumbo samples. Because we do not observe whether the loan had private mortgage insurance, we cannot control for this directly in our analysis. However, we reestimate our key results on a subsample of loans that are explicitly not required by the GSEs to have PMI, namely loans with loan-to-value ratios less than

or equal to 80 percent at origination. When we restrict both the GSE and prime jumbo samples to only loans with $LTV \leq 0.80$, we lose roughly 30 percent of our sample. Column (5) of Table 6 from the main text shows our RD estimates when we restrict the sample to loans with $LTV \leq 0.80$. Notice that, even in this restricted sample where PMI is not required our RD coefficients are nearly identical to our base case. If anything, the differences between GSE and non-GSE loans are slightly larger among this subsample where the GSEs are directly bearing the cost of any default.

As an additional robustness specification, we examine rate quotes from LoanSifter, a firm that collects mortgage contract quotes across a range of U.S. markets and contract types. The advantage of this data is that the prices quoted are intended to be holding points constant (in our case, at zero). We use rate quotes from September 2009 through November 2010 (collapsed to the quarterly level) across 57 metro areas, for quoted prices for a 30-year fixed-rate conforming loan with a 20 percent downpayment (that is, an 80% LTV ratio) and a 750 FICO score. The time period reflects the availability of data and follows the worst of the Great Recession, leading to substantially larger lagged default rates than in prior periods. As shown in Appendix Figure A-3, we observe no relationship between lagged GSE default rates and quoted mortgage rate prices.

A-1.B.5 Additional Robustness Tests

Appendix Table A-3 presents a few additional robustness checks on our main empirical findings. The first column shows our baseline estimates of the regression-discontinuity style effect of the difference in the relationship between interest rates and regional risk across the GSE and non-GSE markets and uses the same sample and same specification as described in the main text. Column (2) restricts our baseline sample to only those markets with at least 10 non-GSE loans in a given quarter. Column (3) includes a set of lagged FICO and lagged LTV controls, thus conditioning on both contemporaneous and prior composition of loan quality in each market. Across all of these additional specifications, the results are consistent in magnitude. These results suggest our main findings are not sensitive to our selection of MSAs or the exact specification of our FICO and LTV controls.

A-1.B.6 Exploiting Time Variation in the Conforming Limit

We also conducted a robustness specification that exploits time variation in the conforming limit, shown in Appendix Table A-4. In particular, we focus on specific set of loans of a given dollar amount that changed between being above the conforming limit to being below the conforming limit during the early 2000s. For instance, a loan amount of \$350,000 was above the limit (and thus excluded from the GSE market) in the period prior to 2005, but in 2005 the conforming loan limit was raised such that a loan of this size could be purchased and insured by Fannie Mae or Freddie Mac. We then ask if interest rates for loans of \$350,000 respond to local predicted default risk prior to 2005 but not respond to local predicted default risk after 2005. To explore this, we reestimate our main specifications using only the set of loans in the range of \$276,000 and \$417,000 that change conforming status between 2001 and 2006. For these loans, prior to the conforming limit change, they responded significantly to local predicted default risk. However, for loans in this same range, interest rates no longer responded to local default risk once they became conforming loans. The magnitudes of the differences in Appendix Table A-4 are consistent with our estimates in Table 2. We view this as further evidence that GSEs do not vary interest rates in response to local predictable default risk.

A-1.B.7 Variation of Mortgage Rates to Other Local Risk Factors

In our final analysis, we show that local mortgage rates for loans securitized by the GSEs do not vary with other dimensions that could also induce local adjustment for risk such as local mortgage recourse laws, local bankruptcy laws, or local lender concentration. In particular, in Appendix Table A-5 (Panel A), following Scharfstein and Sunderam (2013), we use two measures of MSA-level lender concentration, the market share of the four largest lenders and the Herfindahl-Hirschman Index (HHI). As seen in the table, we find no significant relationship between residualized interest rates and lender concentration among GSE loans. In the prime jumbo sample, we find a small statistically significant negative relationship, suggesting that interest rates in this market were, if anything, slightly lower in more concentrated markets.

In Panel B of Appendix Table A-5, we assess if systematic variation in different state laws that capture the ability of lenders to recover their assets in a timely manner could explain the dispersion in residualized interest rates. In particular, we estimate the relationship between rates and whether the state allows for a “deficiency judgment” against the debtor (“recourse”), whether the state requires a judicial procedure to complete a foreclosure, and an indicator for whether the state is in the top half of the distribution in the generosity of homestead exemptions in personal bankruptcy. All of the coefficients on the state laws are statistically insignificant in both the GSE and

prime jumbo samples. In other words, there is no spatial variation in mortgage rates that are associated with local bankruptcy or creditor protection laws.

A-2 FOR ONLINE PUBLICATION: Computational Appendix

In this appendix, we describe the solution to the model described in the body of the text. Reviewing the baseline model setup, the household state vector is defined as $\mathbf{s}_{jk} = (b_j, m_j, h_j, z_j, r_j^{m, fixed}; \gamma_{jk})$, and the model is solved by backward induction from the final period of life. When working, households solve:

$$V_j(\mathbf{s}_{jk}) = \max \left\{ V_j^{adjust}(\mathbf{s}_{jk}), V_j^{noadjust}(\mathbf{s}_{jk}), V_j^{refi}(\mathbf{s}_{jk}), V_j^{rent}(\mathbf{s}_{jk}) \right\}$$

with

$$\begin{aligned} V_j^{adjust}(\mathbf{s}_j) &= \max_{c_j, b_{j+1}, m_{j+1}, h_{j+1}} U_{jk}(c_j, h_{j+1}) + \beta E_j(V_{j+1}(\mathbf{s}_{j+1, k})) \\ & \text{s.t.} \\ c_j &= b_j(1+r) - b_{j+1} + (\chi_j + z_j)(\gamma_{k,j})^{\phi_y + \phi_r^y \phi_r} - (1 + r_{k,j}^{m, market})m_j + m_{j+1} \\ & \quad + (\gamma_{k,t})^{\phi_h + \phi_r^h \phi_r} h_j (1 - \delta^h) (1 - F) - (\gamma_{k,t})^{\phi_h + \phi_r^h \phi_r} h_{j+1} \\ b_{j+1} &\geq 0, \quad m_{j+1} \geq 0 \\ \log z_{j+1} &= \rho_z \log z_j + \eta_{j+1} \\ \log \gamma_{k,j+1} &= \rho_\gamma \log \gamma_{k,j} + \varepsilon_{k,j+1} \\ m_{j+1} &\leq (1 - \theta) (\gamma_{k,t})^{\phi_h + \phi_r^h \phi_r} h_{j+1} \\ r_{k,j}^{m, market} &= r + \bar{\Psi} \gamma_{k,j}^{-\phi_r} \\ r_{j+1}^{m, fixed} &= r_{k,j}^{m, market}, \end{aligned}$$

when households choose to adjust the size of their owner-occupied house. The value function for non-adjusters is given by:

$$\begin{aligned} V_j^{noadjust}(\mathbf{s}_j) &= \max_{c_j, b_{j+1}, m_{j+1}} U_{jk}(c_j, h_j) + \beta E_j(V_{j+1}(\mathbf{s}_{j+1, k})) \\ & \text{s.t.} \\ c_j &= b_j(1+r) - b_{j+1} + (\chi_j + z_j)(\gamma_{k,j})^{\phi_y + \phi_r^y \phi_r} - (1 + r_j^{m, fixed})m_j + m_{j+1} - \delta^h (\gamma_{k,t})^{\phi_h + \phi_r^h \phi_r} h_j \\ b_{j+1} &\geq 0, \quad m_{j+1} \geq 0 \\ \log z_{j+1} &= \rho_z \log z_j + \eta_{j+1} \\ \log \gamma_{k,j+1} &= \rho_\gamma \log \gamma_{k,j} + \varepsilon_{k,j+1} \\ m_{j+1} &\leq (1 - \theta) (\gamma_{k,t})^{\phi_h + \phi_r^h \phi_r} h_j \\ h_{j+1} &= h_j \\ r_{j+1}^{m, fixed} &= r_j^{m, fixed}, \end{aligned}$$

The value function for households who refinance but do not move is given by:

$$\begin{aligned}
V_j^{refi}(\mathbf{s}_j) &= \max_{c_j, b_{j+1}, m_{j+1}} U_{jk}(c_j, h_j) + \beta E_j(V_{j+1}(\mathbf{s}_{j+1, k})) \\
&\text{s.t.} \\
c_j &= b_j(1+r) - b_{j+1} + (\chi_j + z_j)(\gamma_{k,j})^{\phi_y + \phi_r^y \phi_r} - (1 + r_{k,j}^{m,market})m_j + m_{j+1} \\
&\quad - \delta^h (\gamma_{k,t})^{\phi^h + \phi_r^h \phi_r} h_j - F^{refi} (\gamma_{k,t})^{\phi^h + \phi_r^h \phi_r} h_j (1 - \delta^h) \\
b_{j+1} &\geq 0, \quad m_{j+1} \geq 0 \\
\log z_{j+1} &= \rho_z \log z_j + \eta_{j+1} \\
\log \gamma_{k,j+1} &= \rho_\gamma \log \gamma_{k,j} + \varepsilon_{k,j+1} \\
m_{j+1} &\leq (1 - \theta) (\gamma_{k,t})^{\phi^h + \phi_r^h \phi_r} h_j \\
r_{k,j}^{m,market} &= r + \overline{\Psi} \gamma_{k,j}^{-\phi_r} \\
r_{j+1}^{m,fixed} &= r_{k,j}^{m,market} \\
h_{j+1} &= h_j,
\end{aligned}$$

and a household that chooses to sell its current house⁴² and rent has value function

$$\begin{aligned}
V_j^{rent}(\mathbf{s}_j) &= \max_{c_j, b_{j+1}, m_{j+1}, h_{j+1}^f} U_{ijk}(c_j, h_{j+1}^f) + \beta E_j(V_{j+1}(\mathbf{s}_{j+1, k})) \\
&\text{s.t.} \\
c_j &= b_j(1+r) - b_{j+1} + (\chi_j + z_j)(\gamma_{k,j})^{\phi_y + \phi_r^y \phi_r} - (1 + r_{k,j}^{m,market})m_j \\
&\quad + (\gamma_{k,t})^{\phi^h + \phi_r^h \phi_r} h_j (1 - \delta^h) (1 - F) - r^f (\gamma_{k,t})^{\phi^h + \phi_r^h \phi_r} h_{j+1}^f \\
b_{j+1} &\geq 0, \quad m_{j+1} = 0 \\
\log z_{j+1} &= \rho_z \log z_j + \eta_{j+1} \\
\log \gamma_{k,j+1} &= \rho_\gamma \log \gamma_{k,j} + \varepsilon_{k,j+1} \\
r_{k,j}^{m,market} &= r + \overline{\Psi} \gamma_{k,j}^{-\phi_r} \\
h_{j+1} &= 0.
\end{aligned}$$

The problem for a retired household is identical except that social security benefits replace labor earnings, and future payoffs are discounted at rate $\beta(1 - d_j)$ where d_j is an age-specific probability of death.

In order to implement the solution to this model numerically, we proceed as follows. First, we note that since $r_{k,j}^m > r \forall k, j$ it is never optimal to simultaneously hold both positive m and positive b . Thus, we can replace m and b with a single variable a . This financial asset variable is positive if $b > 0$ and negative if $m > 0$, and households face $r_{k,j}^m$ when $a < 0$ and r when $a > 0$. Thus, the household state reduces to $\mathbf{s}_{jk} = (a_j, h_j, z_j, r_j^{m,fixed}; \gamma_{jk})$. Second, notice that if $F^{refi} = 0$ as in our baseline model, then $V_j^{refi}(\mathbf{s}_{jk}) \geq V_j^{noadjust}(\mathbf{s}_{jk})$, so we can eliminate $V_j^{noadjust}(\mathbf{s}_{jk})$ from the problem and simply set $r_{j+1}^{m,fixed} = r_{k,j}^{m,market}$. Similarly, if $F^{refi} = F$, as in our robustness check with high refinancing costs, then $V_j^{adjust}(\mathbf{s}_{jk}) \geq V_j^{refi}(\mathbf{s}_{jk})$ and we can eliminate the refinancing choice from the problem.

In order to rectangularize the choice set and simplify the computational problems imposed by the endogenous liquidity constraint, we follow Diaz and Luengo-Prado (2010) in reformulating our problem in terms of voluntary equity. In particular, define $q_j \equiv a_j + (1 - \theta)p_{k,j}h_j$. After substituting the budget constraint into the utility function to eliminate non-durable consumption as a choice variable, the value function can then be rewritten in terms of two endogenous variables q_j and h_j , the choice of which is restricted to be strictly positive. Note that $q_{j+1} \equiv a_{j+1} + (1 - \theta)\gamma p_{k,j+1}h_{j+1}$ but that a_{j+1} and h_{j+1} are chosen in period j . Thus, shocks to house prices mean that voluntary equity realized at the start of period $j+1$ may differ from that chosen at the end of period j . Define $\widetilde{q}_{j+1} \geq 0$ to be the choice of voluntary equity for period $j+1$ made in period j . The state of realized voluntary equity relevant for $j+1$ then evolves as $q_{j+1} = \widetilde{q}_{j+1} + (1 - \theta)h_{j+1}(p_{k,j+1} - p_{k,j})$. This implies that although households are constrained to always choose $\widetilde{q}_{j+1} \geq 0$, actual voluntary equity can become negative if house prices fall by a

⁴²If previously a renter, the household will start the period with $h_j = 0$ so will have nothing to sell when it chooses to rent again.

large enough amount. To account for this, we solve the model for states that include negative voluntary equity even though households are constrained to choose non-negative values for this variable.⁴³

We discretize the problem so it can be solved on the computer by first discretizing γ and z using the algorithm of Tauchen (1986). We use 13 grid points for z and 5 grid points for γ . We then approximate $V_j^{adjust}(q_j, h_j, z_j, \gamma_j, r_j^{m, fixed})$, $V_j^{refi}(q_j, h_j, z_j, \gamma_j, r_j^{m, fixed})$, and $V_j^{rent}(q_j, h_j, z_j, \gamma_j, r_j^{m, fixed})$ as multilinear functions in the endogenous states. In our benchmark calculation, we use 50 knot points for q_j (we space these points more closely together near the constraint) and 36 knot points for h_j . The presence of fixed adjustment costs on housing together with the borrowing constraint make the household policy function highly non-linear. For this reason, we follow Berger and Vavra (2015) and compute optimal policies for a given state-vector using a Nelder-Meade algorithm initialized from 3 different starting values, to reduce the problem of finding local maxima. The value of adjusting, not adjusting and renting are then compared to generate the overall policy function. We proceed via backward induction from the final period of life.

In order to compare the constant interest rate model to that with risk-based interest rates, we solve the model for both of these scenarios. To compute consumption equivalents, we then simulate a panel of 100,000 households over their life-times to find the endogenous joint-density of household states over regional economic activity. We record average welfare by region in the variable interest rate world and then compute the percentage change in household consumption required to make average welfare in that region equal to average welfare in that region in the constant interest rate simulation.

We consider two extensions of the model that require modifications of the computation. The first extension changes the fixed-rate, variable-balance mortgages in our baseline model to fixed-rate, fixed-balance mortgages. This adds the additional restriction that when not adjusting $m_{j+1} = m_j$. Importantly, in this environment it is no longer true that it is never optimal to simultaneously hold both positive m and positive b . Thus, we must solve the model with an additional state variable rather than the simplified version after substituting a for b and m . In addition, this specification requires an alternative method to rectangularize the state-space. Since $0 \leq m_{j+1} \leq (1 - \theta) p_{k,j} h_j$, applying the analogous previous change of variables $q_j \equiv -m_j + (1 - \theta) p_{k,j} h_j$ delivers the non-rectangular constraint that $0 \leq q_j \leq (1 - \theta) p_{k,j} h_j$. Instead, we transform the state-variable into a leverage ratio: $q = \frac{m}{(1-\theta)p_h}$. The problem can be restated in terms of this leverage ratio with a restriction that $-1 \leq q \leq 0$. Leverage is not defined for renters, but this is not problematic since by definition they will always have $m = 0$, which is enough information to fully characterize the rental problem. This version of the model is substantially more computationally challenging, as it adds an additional continuous state-variable as well as an additional continuous choice variable. Now instead of choosing net-debt, households must separately choose mortgage debt and savings. When adjusting, this means that the choice is now three dimensional: households choose b , q , and h . We modify the Nelder-Meade algorithm accordingly, but in order to compute the model in a reasonable amount of time, grid sizes are reduced from our baseline: we solve the model with 20 grid points in b and q and 22 grid points in h . We also reduce the grid size for z to 11 points. Despite this reduction in grid sizes, this version of the model takes roughly 50 times longer to solve than the baseline. While versions of the model with smaller grid sizes can be solved reasonably quickly, we found that results were sensitive to reducing grid sizes further. Increasing grid sizes modestly did not appear to affect the conclusions.

The second extension introduces the option to default, as in Campbell and Cocco (2015). We assume that any point in time, households can default on fraction Ω of existing mortgage debt (net of the value of housing), but that they must then sell their house and are excluded from owning for the rest of their lives. Since $\theta = 0.2$ and we abstract from heterogeneity in initial LTV, the majority of households in the model have positive housing equity and do not default. Thus, a high value of Ω is required to match observed default rates. We define a two-year lagged default variable as in the data, and setting $\Omega = 0.95$ delivers an average value for this variable of around 2.5%, in line with results for our sample period.⁴⁴ The introduction of the option to default introduces one new value function, which is analogous to those in our baseline problem. It also introduces one additional binary state-variable which tracks whether or not a household has previously defaulted and is thus excluded from the ownership market.

⁴³Shocks to house prices in the model are not large enough to ever reach a situation where q is so negative that households would be unable to choose $\widetilde{q}_{j+1} \geq 0$ without having negative consumption.

⁴⁴Note that nothing necessarily restricts Ω to be less than 1. $\Omega > 1$ could perhaps reflect the value of living in a house for free during foreclosure. Increasing Ω increases default rates but does not affect our conclusions for the size of implicit transfers.

A-3 FOR ONLINE PUBLICATION: Model Robustness

A-3.A Alternative Mortgage Contracts

In this section, we show that our results are robust to a variety of alternative specifications for mortgage contracts. In our baseline model, we assume that mortgages are fixed rate, since this characterizes the majority of mortgage contracts in the U.S. Allowing for the presence of fixed rates is potentially important since it can affect the extent to which households are subject to interest rate fluctuations which might arise from variation in local risk. For example, if local shocks were iid and refinancing was costless, then households with access to fixed rate mortgages might actually prefer a variable interest rate policy, since they could lock in low rates when conditions are good and keep them if conditions decline tomorrow.

In row 2 of Table A-6, we assess the importance of fixed rate mortgages for our conclusions by computing a version of the model where all mortgages are instead adjustable rate. In particular, we assume that the mortgage rate paid by a household in period t is always equal to $r_{k,t}^{m,market}$. This row shows that whether rates are fixed or variable has little quantitative effect on our conclusions. When conditions are bad, the subsidy from the constant interest rate policy is slightly larger under adjustable rate mortgages (2.35%) than under fixed rate mortgages (2.26%) since abandoning the constant interest rate policy makes all rates rise under adjustable rate mortgages. Conversely, the tax from the constant interest rate policy in good regions is slightly larger under fixed rate mortgages (-2.12%) than under adjustable rate mortgages (-1.88%). This is because when conditions are good and interest rates are not constant, fixed rate mortgages allow households to lock in a relatively low rate that remains permanently, while this is not possible with adjustable rate mortgages.

While it may seem surprising that fixed rates make little difference for our conclusions, this is true for a number of reasons. 1) Anyone who is buying a house or refinancing will be subject to the current rate, including all first time home buyers. These agents tend to be more constrained and as a result are more affected by interest rate variation than those who are not adjusting and maintain the old rate. For renting households contemplating buying, the distinction between the FRM and the ARM makes much less difference in terms of how they are affected by current rates; 2) Regional shocks are reasonably persistent; 3) There is an important life-cycle component to housing and mortgage adjustment decisions; and 4) Idiosyncratic shocks are large compared to the regional variation in interest rates.⁴⁵

The last two facts imply that households are largely making their decisions based on life-cycle considerations and idiosyncratic factors rather than timing purchases or delaying purchases to take advantage of low rates. When combined with persistent regional shocks, this means that most households end up having a fixed rate mortgage which is equal to or very close to the prevailing interest rate so that the distinction between fixed and adjustable rates is not particularly important.

In our baseline model, we assume that mortgages have fixed rates but calibrate the model such that these mortgages can be refinanced with no cost. This specification has two advantages: First, it simplifies the solution of the model, since households always refinance when $r_{k,t-1}^{m,fixed} < r_{k,t}^{m,market}$ and never refinance when $r_{k,t-1}^{m,fixed} > r_{k,t}^{m,market}$. Second, this specification eliminates the possibility of lock-in effects which complicate the interpretation of transfers. In the presence of fixed rate mortgages with costless refinancing, households always take advantage of the opportunity to move to lower rates when local conditions improve. If refinancing is instead costly, then households can potentially get locked into the "wrong" rate. For example, if a household happens to refinance for idiosyncratic reasons when conditions are bad, it will continue to pay a high rate until refinancing again, even if local conditions improve.

Row 3 of Table A-6 shows that such lock-in effects are nevertheless quantitatively unimportant. In this row, we impose a very large refinancing cost $F^{refi} = F$, so that mortgages will only be refinanced by moving. Despite this large cost, results are nearly identical to our baseline. This again occurs because life-cycle conditions are the primary determinant of housing decisions: it is not worth staying in a house of the wrong size for several years in order to save 25 basis points or less on the mortgage rate. Since moving decisions are largely made independently of local interest rates, most households end up endogenously facing market rates which are near their current fixed rate. This in turn means that making it more difficult to refinance and take advantage of local rate variation does not have huge effects. Row 4 of Table A-6 similarly shows that increasing the fixed cost of moving also has little effect on our results.

Finally, for tractability, our baseline model assumes that mortgage *rates* are fixed but that mortgage *balances* can be adjusted without triggering a rate reset unless the household actually moves. In reality, the ability to adjust mortgage balances is asymmetric: most fixed rate mortgage contracts allow balances to be paid down more quickly than required without triggering a rate reset, but contracts do not allow mortgage debt to be increased while maintaining the same rate. The symmetry in our baseline model is thus undesirable, but it dramatically simplifies

⁴⁵These conclusions hold under a wide range of empirically realistic calibrations of the local shock process.

the computation of the model. Modeling mortgages with fixed balances requires separately tracking mortgage debt and liquid assets rather than just net debt, and it also introduces an additional continuous choice variable. In order to assess the importance of this simplification, we have solved a version of the more complicated model with fixed-rate, fixed-balance mortgages.⁴⁶ This model is substantially more complicated to solve and requires less precise model solutions, but the fifth row of Table A-6 shows that it delivers nearly identical results. This is not particularly surprising because it turns out that in our baseline model only 1.5% of households actually choose to increase mortgage debt without refinancing or moving. Thus, even though we allow for this unrealistic behavior in our baseline model in order to substantially simplify computations, it is very unusual and so has little quantitative effect on our conclusions.

A-3.B Modeling default

In our result thus far, we do not explicitly model household default decisions and instead capture the effects of local conditions on credit risk and interest rates through their effects on the risk adjustment factor (Ψ). While it would be desirable to build a model with endogenous credit risk and interest rates rather than exogenously linking these variables to local conditions, this would substantially complicate the model. To what extent would such a model change any of our conclusions? Since we are interested in evaluating the effects of a change in interest rate policy, the main way in which endogenizing default could matter would be if default responds strongly to interest rates. These responses might then alter the size of implied transfers.

For example, the first half of the paper shows that under the constant interest rate status quo, default risk rises when local conditions deteriorate. If defaults also rise with interest rates, then abandoning the constant interest rate policy would further increase the response of default risk and interest rates to local conditions, which would amplify our conclusions. Conversely, endogenous default might instead dampen our results if when rates rise, households are able to use default to avoid paying higher interest rates. However, the presence of fixed rate mortgages in our baseline model reduces the importance of this second mechanism, since interest rates do not automatically rise unless households refinance.

What are the quantitative effects of endogenous default? A full-fledged model of strategic default and endogenous interest rates is beyond the scope of this paper, but it is straightforward to provide some sense of the sensitivity of our results to explicitly modeling default using a simple version of Campbell and Cocco (2015).⁴⁷ Reassuringly, this model suggests that our results are unlikely to be altered by explicitly modeling local credit risk. In particular, we find that default responds only very mildly to local interest rate variation: In particular, a one-standard deviation increase in interest rates (12.5 bp) in the model increases default from 2.58% to 2.66%. Dividing this .08% change by the 2.9% cross-MSA standard deviation in the last row of Table 1 implies that this is only 0.027 (cross-MSA) standard deviation change in default. Furthermore, applying our estimates from the first half of the paper that a 1% increase in predicted default should lead interest rates to rise by 13.48 basis points means that a 0.08 percentage point increase in default should only increase interest rates by 1.07 basis points. This would increase the size of implied transfers but in a fairly negligible way that is well within the range of robustness we considered in Table 9. Thus, we conclude that formally modeling default and endogenous interest rates will greatly complicate our model without substantively altering its quantitative implications.

Table A-6 also shows that allowing for the option to default does not mitigate the effects of given interest rate variation. This does not imply that the option to default does not matter for welfare, but rather that any such welfare consequences do not interact strongly with variable interest rates. Put differently, regional risk matters for welfare, and the ability to default interacts with that, but this interaction is not much altered by interest rates which also vary with regional risk.

A-3.C Additional Robustness

Table A-7 explores how the overall level of regional risk interacts with the GSE constant interest rate policy in addition to alternative calibration choices for the model. The benchmark results are redisplayed at the top of the table, and the next rows show the sensitivity of our results to different values of ϕ^y . In our base specification, we set $\phi^y = 1$. As a robustness exercise, we show results for $\phi^y = 0.5$, and $\phi^y = 0.0$. Basically, the different estimates of ϕ^y determine the extent of regional income risk. In the extreme case, $\phi^y = 0$, there is no regional income risk at all. The next rows of the table show the sensitivity of our results to reducing ϕ^h , where ϕ^h is the responsiveness of local house prices to changes in local economic activity. If housing supply is perfectly elastic in all periods, $\phi^h = 0$. To explore the sensitivity of our results, we recompute results setting $\phi^h = 0.25$ and $\phi^h = 0$. The main take-away from

⁴⁶See the computational appendix for details.

⁴⁷See the computational appendix for additional description of this extended model.

Table A-7 is that varying sensitivity of regional income and house prices has essentially no effect on how households evaluate interest rate variation. In other words, although regional income and house price variation matter for the level of risk faced by households, they have little effect on the way that households are affected by changes in the interest rate across the two policies.

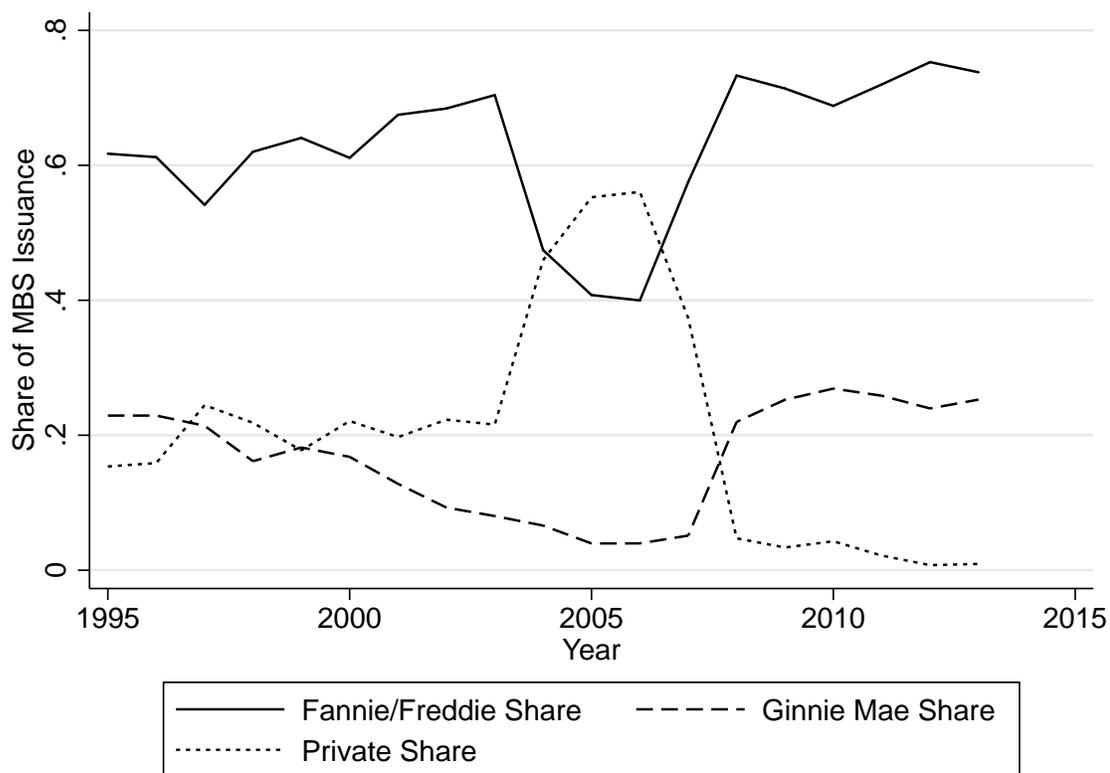
More specifically, the counterfactual experiment asks how much a household would pay to avoid having the interest rate rise today. What the non-interaction with ϕ^y and ϕ^h means is that the answer to this question depends little on whether a household is currently in a high-house-price area, a low-house-price area, a high-income area, or a low-income area. This can be seen most clearly in the second to last row, where both $\phi^h = \phi^y = 0$ so there is no regional variation in income or in house prices. This means that under the constant interest rate policy, regions are identical. The only way that regions vary when $\phi^h = \phi^y = 0$ is that in the variable interest rate counterfactual, some regions will have high interest rates and some regions will have low interest rates. Comparing that row with the benchmark shows that the welfare effects of interest rate variation in a world where house prices and income vary spatially is very similar to one where they do not. This occurs because there are offsetting interactions between regional risk and interest rate variation. For example, higher income makes households more likely to want to buy a big house, so that they dislike high interest rates more than low-income households do. But at the same time, higher income also makes households less likely to borrow, so they care less about high interest rates. The net effect is that these two effects roughly cancel each other out so the level of regional risk has little effect on implicit transfers from interest rate variation.⁴⁸

In our baseline results, we follow Berger et al (2015) in calibrating our parameters to match the life-cycle profile of *liquid* wealth. Kaplan and Violante (2014) argue that matching liquid wealth is essential for generating realistic consumption dynamics. Nevertheless, Table A-7 shows results when we instead target the median *total* wealth-to-income of 1.52 from SCF data (see Kaplan and Violante (2010)). With higher wealth, households are better able to self-insure without borrowing and so the size of transfers is very mildly attenuated, but this effect is extremely minimal. In our baseline results, we match the initial distribution of assets for young households in the SCF, but Table A-7 shows that instead starting all households with zero housing and wealth does not importantly affect our conclusions. Finally, our baseline calibration for the housing share in total consumption is based on matching consumption and residential investment from BEA data and implies a housing expenditure share of around 15%. Davis and Ortalo-Magne (2011) focus on renters using census data and find somewhat larger expenditure shares of 24%.⁴⁹ Table A-7 shows that if we target an expenditure share of 24%, then all of our results are moderately amplified. This is not surprising, since a larger expenditure share on housing implies a larger role for mortgage payments and sensitivity to interest rates.

⁴⁸It is important to note that this non-interaction does not imply that ϕ^y and ϕ^h do not matter for welfare: households are much worse off in the world with regional income variation. But the relevant question is to what extent households' willingness to tolerate interest rate variation interacts with these other regional shocks. Table A-7 shows that there is no substantive interaction.

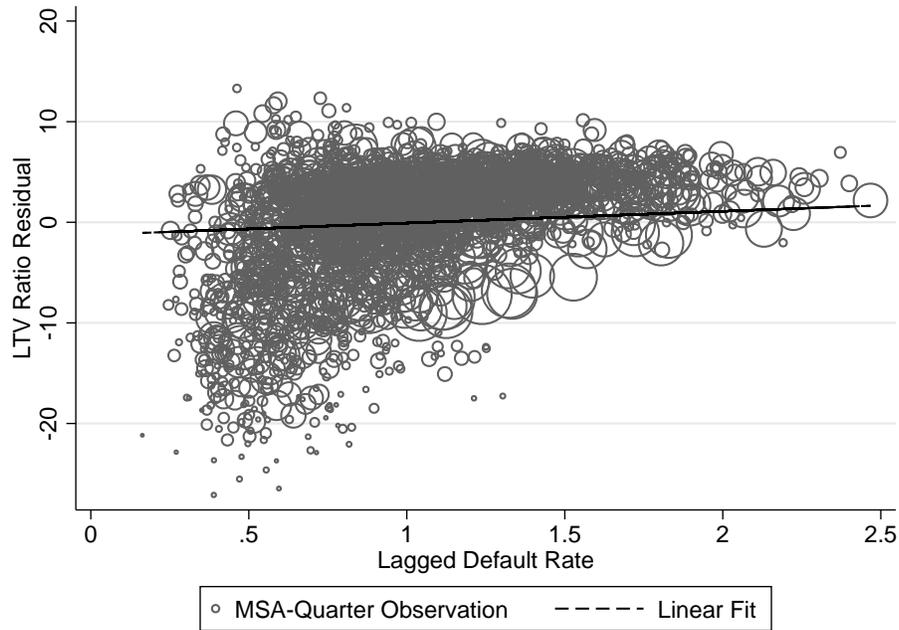
⁴⁹See Section 2 of Davis and Ortalo-Magne (2011) for a discussion of the comparison to BEA data.

Figure A-1: MBS Issuance by Issuer, 1996-2013

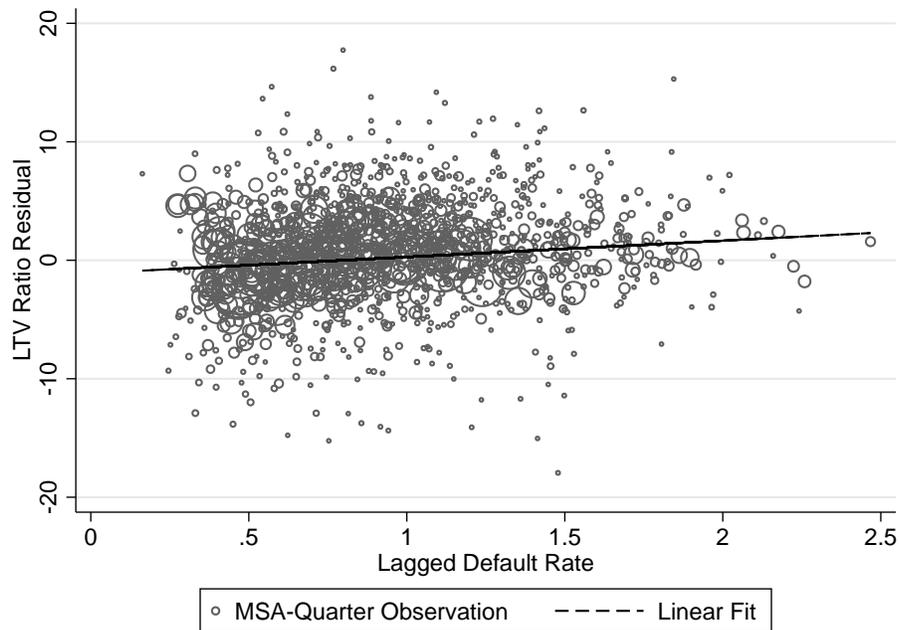


Note: This figure shows the share of mortgage-backed security issuance by the GSEs (Fannie Mae, Freddie Mac, and Ginnie Mae) and non-GSE issuers over the period 1996-2013. The focus of this paper is on the period 2001-2006, when the non-GSE issuance market was especially active, and 2007-2009, during the recession when the non-GSE market collapsed. Source: SIFMA).

Figure A-2: Relationship between LTV Ratio and Regional Risk



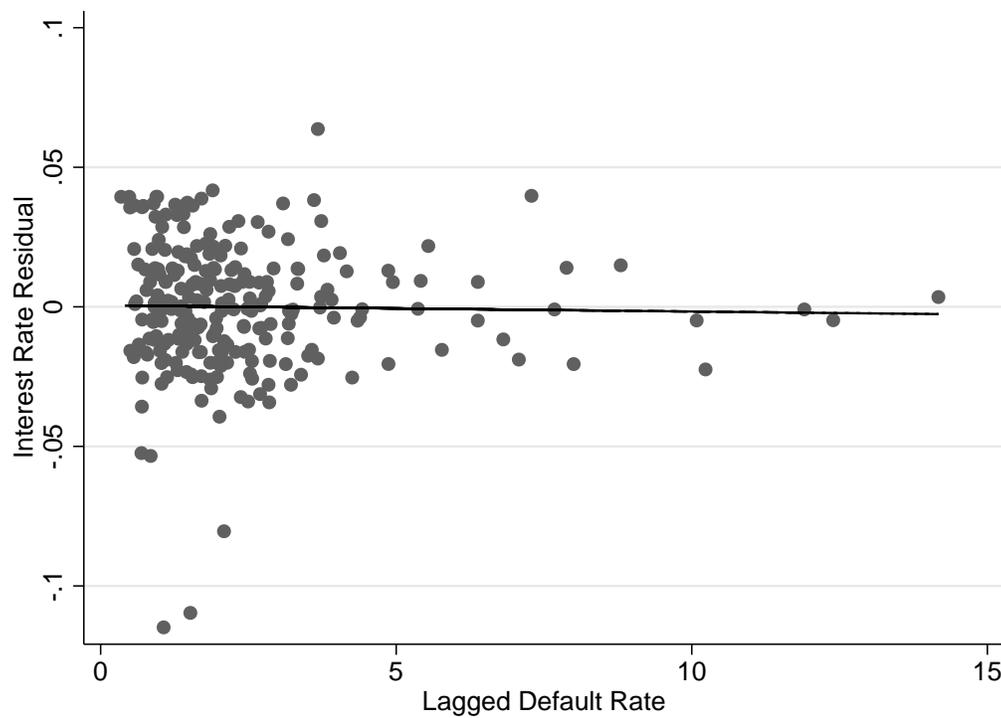
(a) GSE Loans



(b) Non-GSE Loans

Note: The figure presents the relationship between average loan-to-value (LTV) ratios and regional risk for both the GSE (panel a) and non-GSE (panel b) markets during 2001 to 2006. Regional risk is measured by the lagged default rate in the MSA. The estimated relationships are statistically and economically insignificant, and if anything show a slightly positive relationship between leverage and predicted risk during this time period. The LTV measure is residualized to remove time fixed effects and FICO scores. Each dot represents an MSA-quarter average. See text for details.

Figure A-3: Relationship between Interest Rates and Regional Risk, LoanSifter data



Note: The figure uses MSA-level quarterly data from 2009:Q4 through 2010:Q4 from LoanSifter to explore quoted conforming mortgage prices with points and fees held constant at a level of zero. The interest rate measure is residualized to remove time fixed effects. The estimated relationship between interest rates and lagged GSE default are statistically and economically insignificant. See text for details.

Table A-1: Descriptive Statistics of Matched Samples

Percentile	Interest Rate		FICO Score		LTV Ratio	
	Non-GSE	Matched GSE	Non-GSE	Matched GSE	Non-GSE	Matched GSE
10th	5.625	5.5	627	628	60	57
25th	5.99	5.875	637	638	70	70
50th	6.5	6.25	656	658	80	79
75th	7.125	6.875	698	700	85	85
90th	7.95	7.25	745	759	90	95
Mean	6.66	6.32	672	677	76.7	76.1
Observations	70,327	70,327	70,327	70,327	70,327	70,327

Note: The table provides summary statistics for the samples of matched GSE and non-GSE (“prime jumbo”) loans originated between 2001 and 2006. The matched GSE sample uses only those MSAs with prime jumbo loans present (during 2001 to 2006) and matches the distribution of FICO scores and LTV ratios in the non-GSE sample. See text for details.

Table A-2: Relationship between Effective Interest Rates and Regional Risk

	Interest Rate (%)	Effective Interest Rate net of points and fees (%)
Coefficient on Lagged GSE Default Rate	0.372 (0.337)	1.881 (1.414)
Observations (N> 6M)	N	N
Implied Interest Rate Change in Y to a Two Standard Deviation Change in Lagged GSE Default	0.01	0.05

Note: The table provides coefficients from regressions of interest rates and effective interest rates on regional risk as measured by lagged GSE default. Standard errors in parentheses are clustered at the MSA level. The data comes from one of the GSEs (anonymously), and we are not allowed to provide precise sample sizes except to note that there are more than 6 million loans in each specification. The GSE loans included use the same sample restrictions we have made above, and the specifications include quadratics in FICO and LTV interacted with time dummies as above. Column 1 reports the relationship between interest rate and our measure of regional risk. Column 2 uses a measure of “effective” interest rate that nets off any points and fees (including closing costs) paid by borrowers. Neither regression shows a significant association between our measure of regional risk and the cost of borrowing in the GSE mortgage market.

Table A-3: Additional Robustness Results

	Baseline	N>=10	Conditional Lags
Predicted	13.48	13.93	14.48
Default	(4.56)	(3.89)	(4.04)
Lagged GSE	32.54	34.59	29.61
Default	(4.00)	(3.95)	(6.33)
Lagged Own	13.04	13.25	11.88
Default	(4.57)	(4.60)	(5.29)
Actual	2.06	2.72	--
Default	(0.44)	(0.53)	--

Note: The table provides coefficients from a number of specifications of the regression-discontinuity style estimates using different samples or different controls. Column 1 provides the baseline estimates from the main text. Column 2 restricts the sample to only those loans that are not required to purchase private mortgage insurance. Column 3 restricts the sample to use only those markets with at least 10 non-GSE loans in an MSA-quarter of origination cell. Column 4 uses the baseline sample but includes controls for both contemporaneous and lagged measures of loan quality (FICO and LTV). Standard errors in parentheses clustered at the MSA level. Standard errors for results relying on predicted default are bootstrapped (500 repetitions, clustered at MSA level) to account for the generated regressor. See text for details.

Table A-4: Focus on Loans Transitioning Around the Conforming Loan Limit

	Estimated Coefficient			
	GSE Matched Sample	Prime Jumbo Sample	Difference	p-value of difference
Predicted Default Using	-3.38	10.76	14.14	<0.001
Lagged GSE Default	(3.69)	(1.07)		

Note: The table provides coefficients from separate regressions for loans backed by the GSE and non-GSE markets during 2001 through 2006 that had loan amounts between \$276,000 and \$417,000, the range of loans that switched status based on changes in the conforming loan limit. The results show strong and statistically significant correlations between interest rates and regional risk in the non-GSE market, but no meaningful relationship in the GSE market. As in the main text, the regressions control for loan quality using measures of FICO and LTV interacted with quarter of origination. Standard errors in parentheses clustered at the MSA level. Standard errors for results relying on predicted default are bootstrapped (500 repetitions, clustered at MSA level) to account for the generated regressor. See text for details.

Table A-5: Relationship between Interest Rates, Lender Concentration, and State Laws

	GSE Sample	GSE Sample	Prime Jumbo Sample	Prime Jumbo Sample
Lender HHI	-1.15 (0.78)		-5.92 (1.34)	
Top 4 Concentration		-0.171 (0.117)		-0.909 (0.193)
Years	2001-2006	2001-2006	2001-2006	2001-2006
Observations	624	630	461	465
MSAs	105	106	105	106
R-squared	0.03	0.02	0.13	0.13

(a) MSA-level Regressions

	GSE Sample	GSE matched Sample	Prime Jumbo Sample
Recourse Law	0.034 (0.019)	0.036 (0.024)	0.063 (0.045)
Judicial Foreclosure	0.038 (0.040)	-0.007 (0.067)	0.013 (0.039)
Debtor- Friendly	0.023 (0.039)	-0.021 (0.065)	0.02 (0.038)
Years	2001-2006	2001-2006	2001-2006
Observations	1224	809	809
States	51	46	46
R-squared	0.08	0.06	0.013

(b) State-level Regressions

Note: Panel A provides coefficients from regressions of interest rate residuals on lender concentration as measured by the Herfindahl-Hirschman Index (HHI) and the market share of the top 4 lenders in the MSA. The dependent variable is residualized interest rate, following the specifications in the main text, removing FICO, LTV, and higher-order polynomials, all interacted with quarter of origination fixed effects. Panel B provides coefficients from regressions of interest rate residuals on three relevant state laws. The laws measure the ability of lenders to recover their assets in a timely manner. The regression includes measures of whether the state allows for a “deficiency judgment” against the debtor (a.k.a. “recourse”), whether the state requires a judicial procedure to complete a foreclosure, and an indicator for whether the state is in the top half in terms of homestead exemptions in personal bankruptcy.

Table A-6: Sensitivity to Mortgage Contracts and Housing Transaction Costs

	Regional Employment				
	-2 Standard Deviation	-1 Standard Deviation	0	+1 Standard Deviation	+2 Standard Deviation
Consumption gain: Bnchmk	2.26%	1.18%	-0.02%	-1.04%	-2.12%
Adjustable Rate Mortgages	2.35%	1.30%	0.12%	-0.86%	-1.88%
FRM-Large Refinancing Cost	2.42%	1.27%	0.09%	-0.92%	-2.03%
Double Fixed Cost of Moving	2.19%	1.02%	-0.16%	-1.30%	-2.27 %
FRM-Fixed Balance	2.20%	1.24%	-0.14%	-1.57%	-1.94%
Allow for Default	2.26%	1.18%	-0.04%	-1.05%	-2.11%

Note: This table shows the robustness of our estimated consumption gains to different interest rate sensitivities to local economic conditions. See text for a description of baseline parameters and the policy experiment as well as the description of each particular mortgage environment. The consumption gain in each row is equal to $\lambda \times 100$, where λ is the percentage change in consumption that makes a household indifferent between a variable and constant interest rate.

Table A-7: Sensitivity to Regional Economic Variation and Alternative Calibrations

	Regional Employment				
	-2 Standard Deviation	-1 Standard Deviation	0	+1 Standard Deviation	+2 Standard Deviation
Consumption gain: Bnchmrk	2.26%	1.18%	-0.02%	-1.04%	-2.12%
Reduce Regional Income Variation in Half	2.25%	1.16%	-0.05%	-1.16%	-2.25%
Reduce Regional Income Variation to Zero	2.20%	1.11%	-0.07%	-1.20%	-2.25%
Reduce Regional House Price Variation in Half	2.18%	1.16%	0.07%	-1.04%	-2.11%
Reduce Regional House Price Variation to Zero	2.11%	1.02%	-0.11%	-1.30%	-2.25%
Reduce Regional House Price and Income Variation to Zero	2.16%	1.07%	-0.07%	-1.21%	-2.22%
Calibrate to Total Wealth	2.09%	1.05%	-0.02%	-1.14%	-2.13%
No Initial Assets Calibration	2.32%	1.11%	-0.04%	-1.13%	-2.22%
Higher Housing Share	2.64%	1.50%	-0.02%	-1.58%	-2.98%

Note: This table shows the robustness of our implied consumption response to alternative parameterizations. See text for a description of baseline parameters and the policy experiment. The consumption gain in each row is equal to $\lambda \times 100$, where λ is the percentage change in consumption that makes a household indifferent between a variable and constant interest rate. In rows 2 and 3, we reduce the value of ϕ^h , in rows 4 and 5 we reduce the value of ϕ^y , and in row 6 we reduce the value of both elasticities. Row 7 presents results for the baseline model recalibrated to match median total net wealth rather than liquid wealth. Row 8 presents results for model where agents are born with no assets or housing. Row 9 shows results for a calibration matching higher rent share data rather than BEA statistics.