Regressive Mortgage Credit Redistribution in the Post-crisis Era

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Abstract

We document four novel facts about mortgage origination after the 2008-2009 Financial Crisis. First, since 2011, mortgage lenders reduced the origination of conforming loans by 15% and increased the origination of jumbo loans by 21%. Second, the extent of redistribution increased monotonically with the size of the originator. Third, mortgages farther away from the conforming loan limit both above and below drove the redistribution, and hence systematic differences between conforming and non-conforming loans are unlikely to drive the results. Fourth, lending standards – measured as the ratio of households income over loan amount – became stricter for mid-sized loans and were relaxed for jumbo loans, and especially for loans above $700k. Results hold at the individual-loan level and zip-code level, and at the intensive margin and extensive margin. The collapse of the private-label securitization market, banks' risk-management concerns, wealth polarization, post-crisis policies of GSEs, or pre-crisis indebtment are unlikely to explain the results. The results appear consistent with large banks reacting more to the increased costs of origination imposed by financial regulation.

Keywords: Mortgage Market, Financial Crisis, Dodd-Frank, Income Inequality.

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Wealth inequality has been increasing steadily in the United States since the late 1970s. The share of wealth held by the top 1% of the distribution had reached 42% in 2012. Interestingly, wealth inequality has increased dramatically not only before the recent financial crisis, but especially since 2011 (Saez and Zucman, 2016). These dynamics are common to several countries worldwide, but the most recent increase has been higher in the United States compared to other countries. According to Alvaredo, Chancel, Piketty, Saez, and Zucman (2017), "different country-specific policies and institutions matter considerably [for the recent increase in wealth inequality]." The nature and relevance of the channels that explain these dynamics in wealth inequality are still largely unknown.

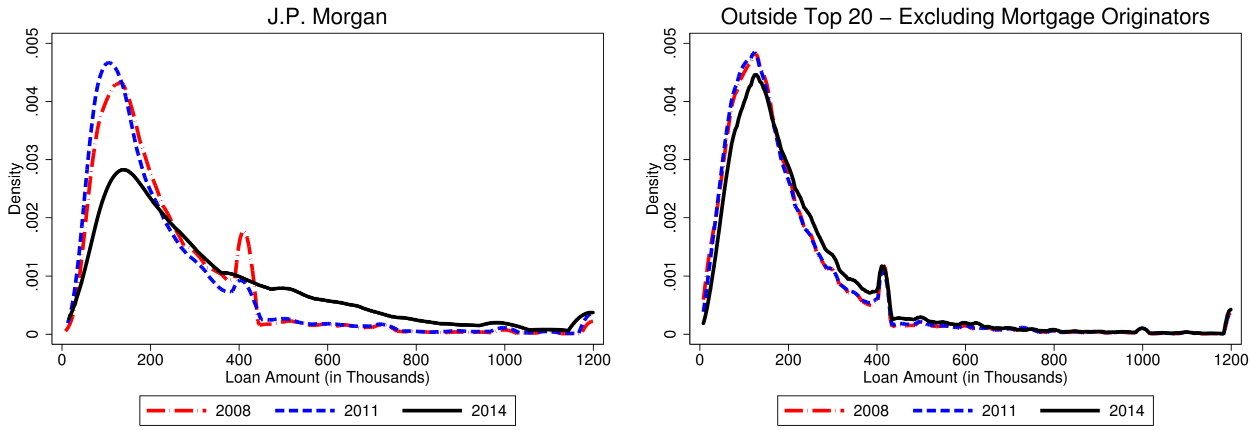
In this paper, we document one channel that might have contributed to the sharp increase in the US wealth inequality since 2011 – a substantial regressive mortgage credit redistribution from middle-class households to wealthy households. A back-of-the-envelope calculation that keeps constant the mortgage-demand characteristics of 2010 shows financial institutions reduced their lending to medium-sized loans by 15% in 2014, and increased lending to large loans by 21%. This redistribution is robust to controlling for individual- and county-level determinants of the demand for mortgages, such as applicants' race and income, the local racial composition and average household income, local house prices, and the local share of foreclosed properties. The results are similar if we control for time-varying local economic shocks related to the demand or supply of mortgages, if we restrict the variation within counties, and if we run the analysis at the individual-loan level or the zip-code level.

The extent of redistribution increased monotonically with the size of the lender. The plot below documents this point, which we discuss further in Section 3. The left panel shows the loan-size distribution of the mortgages originated by J.P. Morgan, one of the top 20 lenders in our sample period. We use J.P. Morgan as a representative example, but the effects described below are similar for other large banks, as we show in Panel A of Figure 1. The right panel plots the distribution across all lenders outside the top 20, excluding mortgage originators.[1] The overall distribution of credit by loan size shifted to the right for J.P. Morgan starting in 2011. The bunching at the conforming loan limit, which was very pronounced in 2008, has virtually disappeared by 2014.[2] The distribution shifted

[1]The results are qualitatively similar if we include mortgage originators (see the right plot in Panel B of Figure 1).

[2]The distribution contracted from 2008 to 2010, and started expanding thereafter. Because of the left shift, the bunching at the conforming loan limit had started reducing between 2008 and 2010.

to the right since 2011 also for smaller lenders, but the shift was less dramatic for them.



To assess whether well-known determinants of the demand for mortgages explain these facts, we perform a multivariate analysis at the loan level. Because households rarely shop for loans outside their county of residence, households in counties with a higher exposure to large lenders were more exposed to the redistribution.[3] Consistent with the aggregate facts, households in counties more exposed to large lenders obtained lower amounts for loans between \$100K and the conforming loan limit of \$417K since 2011. The opposite holds above the conforming loan limit. The results are similar for different definitions of large and small lenders.

A third important feature of the redistribution we document is it was mainly driven by loans farther away from the conforming loan limit from above and below. We observe a substantial decrease in the dollar amount of loans between from \$100K to \$200K, and a substantial increase in the dollar amount of loans above \$700K. Only 6 percent of US counties have a conforming loan limit higher than \$417, and almost all counties have a limit below \$700k. Thus, systematic regulatory or policy differences between jumbo and conforming loans after 2011 are unlikely to explain the redistribution we document, because these factors would have mainly affected loans just below and just above the limit by decreasing the incentive to bunch at the limit.

Systematic differences in the demand for mortgages across counties served by large or small lenders,

[3]We measure the share of mortgage originated by lenders at the county level, because households can easily obtain mortgages in zip codes or census tracts different from the ones where they reside.

such as applicants' income, race, and local house prices, do not explain our findings. Our baseline results are robust to using linear or non-linear estimators, to controlling for proxies of the full distribution of house prices in counties, and to allowing for all our covariates to vary systematically before and after 2011. The results are similar if we control for dimensions we can observe only for a subset of loans, such as the share of foreclosed properties in the zip-code in which the loan was originated. Results are also similar if we exclude non-bank originators, and if we restrict the analysis to sand states (California, Arizona, Florida, Nevada), which reacted less than other states to unconventional monetary policy (see DiMaggio, Kermani, and Palmer, 2016).

Our primary data source, HMDA, includes information on denied mortgage applications. We consider the share of income to loan amount for denied applicants, and find that the average income per loan amount increases between \$100K and the conforming loan limit of \$417K, in counties with a higher presence of large lenders. We find the opposite above the conforming loan limit. This fourth feature of the redistribution we document mirrors the results on origination, and suggests that, since 2011, large lenders applied stricter standards than small lenders to approve medium-sized mortgages, and more lenient standards to approve large mortgages.

The fact that large and small lenders are not assigned randomly across counties does not allow us to conclude definitively that the redistribution is due to the supply side of origination, as opposed to the demand side of origination. Unobservables related to the financial crisis might have changed the distribution of lenders across counties since 2011, and might have also determined a change in mortgage demand since 2011. Ideally, we would assign large and small lenders randomly across counties every year from 2008 to 2014.

We propose an instrumental-variable approach to obtain quasi-exogenous variation in the share of large lenders over total lenders across counties. We instrument the share of large lenders active in each county-year from 2008 to 2014 with the share of large lenders active in the county in 2007, before the collapse of Lehman Brothers and the financial crisis. Strategies based on supply-side pre-existing variation to instrument for the extent of exposure to a supply-side shock have been used recently in economic research (e.g., see Mian and Sufi, 2012, Chodorow-Reich, 2014, and Agarwal et al., Forthcoming).

The rationale for this instrument is that the variation in the share of large lenders across counties before the financial crisis cannot have been determined by the wave of bankruptcies during the financial crisis, or by the fiscal and monetary policy measures implemented after the financial crisis. Because of the costs of moving branches and relocating workers, inertia occurs in the local supply structure of financial services, which suggests our instrument is relevant.

The crucial identifying assumption is that the variation in the share of large lenders in 2007 does not affect the amounts lent since 2011 through channels different from the share of large lenders since 2011. This exclusion restriction cannot be tested, and hence we propose evidence to assess the extent to which the exclusion restriction might be plausible. First, we show our outcome variable and covariates are balanced across counties, based on the shares of mortgages originated by large lenders in 2007. Also, no differential growth in loan amounts occurs before 2011. Second, we use the reduced-form specification to assess the extent to which our instrument might be associated with the outcome variable through channels different from the share of large lenders after 2007. We find the instrument explains the outcome variable in the reduced-form specification in a similar way as the endogenous variable does in the baseline results, but once both the instrument and the endogenous regressor enter the same specification, we can never reject the null that the coefficient is equal to zero for both coefficients at the same time. This result suggests that alternative channels through which our instrument is associated with the outcome variable are unlikely to be economically relevant.

The instrumental-variable results confirm our baseline analysis both in terms of statistical significance and magnitude of the effects.

Because it mainly focuses on individual loans, our analysis allows us to study the intensive margin of mortgage origination – the size of loans originated since 2011 across the size distribution – but it cannot inform us on the extensive margin – the number of loans originated since 2011 across the size distribution. In Online Appendix A.1, we run the analysis at the zip-code level and show that similar results hold at the extensive margin.

In the last part of the paper, we assess a set of potential explanations for the facts we document. First, we consider the freeze of the private-label securitization market after the financial crisis.[4] This

[4]We thank Jonathan Parker for proposing this alternative explanation.

explanation cannot drive our results, because the residential-mortgage private-label securitization market remained stagnant throughout our analysis period for banks of all sizes. Furthermore, we show that large banks were not more likely than small banks to securitize their loans privately since 2011, which is a necessary condition for the dynamics of the private-label securitization market to explain our results.

Second, we consider whether risk-management considerations might explain our results. Large lenders may have started to originate more jumbo loans to decrease the riskiness of their assets in an effort to comply with capital requirements. This explanation implies that riskier banks drive our results, which we rule out.

Third, we consider the possibility that wealth polarization deriving from the disappearance of the middle class after the financial crisis explains the results. Using data from the American Community Survey, we find that our results are not consistent with this potential explanation.

Fourth, we consider the fact that GSEs have introduced a stricter "putback" policy after the financial crisis, by which they started to force lenders to buy back loans whose origination documents were found to misrepresent borrowers' ability to repay.[5] This stricter policy created uncertainty in the effectiveness of the GSEs' guarantee, and hence might have dis-incentivized lenders to originate conforming loans. This policy could explain our results below the conforming loan limit only if misrepresenting borrowers were more concentrated in loans between \$100K and \$200K, and it cannot explain why the increase was higher for the jumbo loans well above the conforming loan limit, compared to those just above the limit.

A fifth potential explanation for our results is that the costs of originating conforming loans has increased since 2011. This increase could be driven by two changes in regulation – the approval of the *Temporary Payroll Tax Cut Continuation Act* (TCCA) and/or the approval of the *Dodd-Frank Wall Street Reform and Consumer Protection Act* (Dodd-Frank).

TCCA determined a series of increases of the annual insurance premia lenders pay to GSEs ("g-fees") starting in 2011, which had to be remitted to the US Treasury for the most part.[6] This increase

[5] We thank Anthony DeFusco for suggesting this explanation.
[6] We thank Anthony DeFusco for suggesting the increase in g-fees as a potential explanation for our findings.

determined a higher per-loan cost of originating conforming loans. This increase should have led to higher origination just above the conforming loan limit, because lenders had lower incentives to bunch loans at the limit. Moreover, the increase in g-fees should have determined a decrease in origination of similar size for all loans below the conforming loan limit. Different from these predictions, we find that the increase in origination is mainly driven by loans well above the conforming loan limit, and the decrease in origination is not homogeneous below the conforming loan limit, but is mainly driven by loans further away from the limit.

Dodd-Frank increased both the fixed costs and the per-loan costs of mortgage origination. A first set of provisions increased the fixed costs of originating mortgages of all sizes. For instance, lenders had to establish an internal training system and to provide special training to all loan officers at their branches. The costs of such training decreased the institutions' incentives to originate any mortgages. Second, Dodd-Frank imposed a thorough yet costly verification of the income customers reported at the time of the application. This procedure increased the per-loan costs of origination. Because the procedure has the same fixed cost, irrespective of the size of the loan, it decreased the per-loan returns of smaller loans more than those of larger loans.

Anecdotally, financial institutions reacted immediately after the approval of Dodd-Frank, even though several provisions were not self executing.[7] This behavior is expected, because lenders had to invest substantial resources, such as building new training infrastructures and hiring specialized employees, in order to be compliant with the new provisions at the time of execution.

Because Dodd-Frank increased both the fixed costs and the per-dollar-lent costs of originating *all* loans, it determined a change in incentives consistent with the patterns we find, that is, a substantial decrease in origination for conforming loans further away from the conforming loan limit, and an increase in origination for jumbo loans further away from the conforming loan limit.

An explanation based on higher overall and per-loan costs of origination is also consistent with

[7] The Federal Reserve (Fed), and subsequently the Consumer Financial Protection Bureau, had to produce the regulations needed to make some of the provisions executable. Banks had to be compliant to the new provisions at the execution date, and hence faced the costs of compliance before that date. Because the execution date was uncertain at the time of approval of Dodd-Frank, banks' expected costs and revenues of different types of mortgages changed immediately. In Online Appendix A.2, we show direct evidence that large banks paid these costs as early as January 2011.

the fact that large lenders changed their origination behavior more than smaller lenders. This is true for at least three reasons. First, jumbo loans cannot be sold to GSEs, and hence institutions must either keep them on their balance sheets or sell them to private counterparties, which impose worse conditions than GSEs. Second, larger banks can engage in cross-selling more than small banks, credit unions, and mortgage originators. Third, large lenders have more geographically diversified operations than small lenders, and operate in more businesses. Large lenders can therefore redirect their activities to different geographies and businesses after the increase in the costs of mortgage origination, whereas smaller banks and mortgage originators can hardly do so.

1 Related Literature

This paper contributes to three strands of the economics and finance literature. First, we fit in the upcoming literature that studies the mortgage-origination behavior of financial institutions. So far, this literature has focused on origination before the 2008-2009 financial crisis, see Mian and Sufi (2009), Guiso, Sapienza, and Zingales (2013), Agarwal et al. (2014), Palmer (2015), Mian and Sufi (2016), Adelino, Schoar, and Severino (2016), Albanesi, DeGiorgi, and Nosal (2016), and Foote, Loewenstein, and Willen (2016). Our paper focuses on lenders' mortgage origination behavior *after* the crisis. Chen, Hanson, and Stein (2017) describe the lending behavior of the 4 largest US banks towards small businesses after the crisis. Our analysis suggests well-known facts regarding the mortgage-originating behavior of lenders might have changed permanently after the crisis. For instance, the bunching of the largest lenders at the conforming loan limit, a robust fact that was used to estimate the elasticity of house prices to interest rates (DeFusco and Paciorek, 2017), might have disappeared for good.

Second, we contribute to the line of research that studies the effects of fiscal and monetary policy on households' consumption and saving choices, such as fiscal stimulus and unconventional monetary policy (e.g., see Mian and Sufi, 2012, Green et al., 2014, and Broda and Parker, 2014). DiMaggio, Kermani, and Palmer (2016) and Rodnyansky and Darmouni (2016) study the effects of unconventional monetary policy – the long-duration large-scale asset purchase programs (LSAPs) – on lending behavior in the period 2008-2013. A concern with our results is we might capture the reaction of lenders to LSAPs. But, first, we exploit the different incentives of large and small lenders since 2011. Large lenders reacted more to the changes in regulation, whereas Rodnyansky and Darmouni (2016) find small lenders reacted more to LSAPs, and in particular to QE3 and QE1. If our analysis captured the effect of unconventional monetary policy, we should find the opposite of our baseline result. We also find our results for loans above the conforming loan limit are stronger in sand states (California, Nevada, Arizona, and Florida) than in other states. Instead, in DiMaggio, Kermani, and Palmer (2016), loans originated in sand states – the states more hit by the credit freeze before QE1 – did not react to LSAPs. Moreover, if unconventional monetary policy explained large lenders' move toward jumbo loans, the effect should arise after 2013. After 2013, tapering started and the rates of conforming mortgages increased, making jumbo loans comparatively less costly. Instead, we find the right shift of the loan-size distribution started in 2011. Finally, our analysis is at the level of the new-purchase mortgages, whereas DiMaggio, Kermani, and Palmer (2016) focus on refinancing loans to study within-borrower deleveraging.

On the fiscal policy side, Agarwal et al. (Forthcoming) study the effects of the Home Affordable Modification program (HAMP), which provided monetary incentives for both lenders and borrowers to renegotiate the terms of loans at risk of default. Higher renegotiation rates may suggest the banks affected would engage in less originations, because their customers would not be in the position to sign new loans. Agarwal et al. (Forthcoming) find large banks are much less likely to engage in renegotiation after the program, so our baseline estimates are, if anything, a lower bound of the true effect of bank size on origination.[8] Because Agarwal et al. (Forthcoming) also find higher exposure to HAMP resulted in lower house price declines and lower foreclosure rates, we control for these quantities directly in our baseline or robustness specifications.

Another measure of fiscal policy that targeted the mortgage market after the crisis was the First-Time Homebuyer Credit (FTHC). Berger, Turner, and Zwick (2016) study the effect of FTHC on home sales and house prices.[9] They find FTHC accelerated home purchases from several years in the future, and its effects were concentrated in active home-sale markets, which suggests the overall

[8]Although Agarwal et al. (Forthcoming) analyze servicers, they show the institutions originating the loan service the majority of loans.

[9]For the effects fo FTHC, see also Brogaard and Roshak (2011) and Hembre (2015).

stimulative effect of the policy might not have been large. As noted above, we find the higher increase in lending by large lenders above the limit is stronger for depressed home-sale markets, which suggests the effects of the FTHC cannot explain our results.[10]

Third, the paper falls into the literature that studies the effects of financial regulation on demand- and supply-side incentives. The view that regulation is necessary to eliminate abuses against vulnerable agents dates back at least to President Woodrow Wilson (see Glaeser and Shleifer, 2003).[11] Despite its aims, regulation changes the incentives of agents in ways that might not be envisioned at the time the rules are implemented. For instance, the *Freedom of Information Act* (FOIA) was enacted to allow all US citizens, especially those with limited financial resources and social networks, to access the truth about otherwise obscure procedures and actions in the governmental sector. But Gargano, Rossi, and Wermers (2017) find sophisticated institutional investors commonly gather information through FOIA requests to make money on their trades, whose counterparties are uninformed investors.

Together with financial stability, consumer protection is the main aim of financial regulation. The need for consumer protection in finance is especially compelling in times of increasing consumer autonomy in the financial realm (Campbell et al., 2011). Some authors argue ill-designed regulation created incentives that were prodromic to the recent financial crisis (e.g., see Barth, Caprio, and Levine, 2012), whereas others blame the lack of more stringent regulation (e.g., see Stiglitz, 2009).

This debate was particularly heated during the discussion of Dodd-Frank, and the establishment of the Consumer Financial Protection Bureau (CFPB). Proponents of the CFPB highlighted the need of an agency that protected vulnerable categories from abuses in the financial realm (Warren, 2007, and Krugman, 2013). Its opponents argued the agency imposed costly and unnecessary one-size-fits-all regulation, favoring big financial institutions compared to small financial institutions, and ultimately damaging consumers. Other scholars suggest that, if the institutions that had the same powers as the CFPB were unable to defend vulnerable categories, the CFPB would also possibly be unable to do so (see Barth, Caprio, and Levine, 2015).

In this paper, we show that changes in financial regulation are the most plausible explanation for

[10]Throughout our analysis, we also control for median house prices in the counties in which the applicant resides.

[11]President Wilson thought regulation was the only shield of consumers against large corporations, because courts were unable to prosecute large corporations (see Wilson, 1913).

a regressive redistribution of mortgage since 2011. Although we emphasize this channel, the paper is agnostic about the overall welfare effects of financial regulation, and especially of Dodd-Frank.

2 Data Description

Our main source of data is the Home Mortgage Disclosure Act (HMDA) data set for the years 2007-2014, which we obtain through the CFPB's website. The data set contains the universe of mortgage applications over the sample period. For each mortgage application, it includes information regarding the characteristics of the loan, the applicant, and the lender. We collect the loan amount, the loan status (approved/rejected), the lien of the loan, the purpose of the loan (home purchase/refinancing/home improvement), the owner occupancy status, the lender identifier, the applicant's income, the race and ethnicity of the applicant, and the location of the applicant (county/census tract).

Our second source of data is Zillow, through which we obtain the time-series of house prices and the number of houses foreclosed for every county-year. Because individual zip codes may contain more than one census tract, and because individual census tracts may belong to more than one zip code, we follow Adelino, Schoar, and Severino (2016) and use the Missouri Census Data Center bridge to aggregate our individual-loan data at the zip-code level. Because the census tracts definitions vary over time, we use the two alternative bridges available from the Missouri Census Data Center for the periods 2007–2011 and 2012–2014.[12]

For all individual-loan results, the working sample uses only loan applications for home-occupied new purchases secured by a first lien. We run the loan-level analysis separately for approved and rejected loans. Our preferred definition of rejected loans includes two types of action taken in the HMDA form, that is, denied applications (code 3) and approved but not accepted applications (code 2). We use this broad definition, because the applications approved but not accepted are effectively also rejected loans. All our results on rejected loan applications are similar if we only define denied applications as rejected applications.

[12]The bridges are freely available at http://mcdc2.missouri.edu/websas/geocorr2k.html and http://mcdc.missouri.edu/websas/geocorr12.html.

Table 1 reports summary statistics for the main covariates and outcomes. Panel A refers to the sample of approved loans. Our broader sample includes 13,532,723 individual-loan applications that were approved from 2007 to 2014. The median amount lent is $186K, but the loan amounts vary widely from $65K at the 5th percentile to $518K at the 95th percentile. On average, in the counties in which applicants obtain loans, 39% of the mortgages are originated by the top 20 US lenders. Large variation exists in the share of the top 20 US lenders, ranging from 16% (5th percentile) to 62% (95th percentile). The median reported gross income of approved applicants is $71K, and the variation in income is similar to the variation in requested loan amounts. On average, 7% of the applicants are Black, 7% are Asian, and 9.5% are Latino. The median house price in the counties of approved loans is $184K. The share of foreclosed properties in the zip code in which the approved borrowers reside is only available for a subset of zip codes in our sample, and hence for a subset of individual-loan applications. It is, on average, 0.08%.

Panel B of Table 1 reports the corresponding descriptive statistics for the 5,983,994 rejected applications. The median loan amount requested by rejected applicants ($176K) is similar to the one requested by approved applicants, but this median masks a distribution with fatter tails. A similar comparison holds for the distribution of rejected applicants' income and of median house prices in the counties in which applicants are rejected. Rejections seem slightly more likely than approvals in counties served by the top 20 US lenders. Moreover, rejected applicants are more likely than approved applicants to be Black (12%) and Latino (14%), but similarly likely to be Asian (7.8%). Finally, the average share of foreclosed properties in the zip code in which the rejected borrowers reside (8.6%) is two orders of magnitude higher than the one for approved lenders. This fact compels us to verify that our baseline results are not driven by the share of foreclosed properties in the applicants' zip codes.

To analyze the extensive margin of mortgage origination, and for comparability with the rest of the literature,[13] we also report results computed at the zip-code level. Panel C of Table 1 reports summary statistics for the zip-code-level sample. The variables we show are similar to the ones in the individual-level loan analysis, but are computed as averages for each zip-code-year observation from 2007 to 2014. The median loan amount is $170K, which is in line with the loan-level sample. The

[13]e.g., see Mian and Sufi (2009) and Adelino, Schoar, and Severino (2016).

top 20 US lenders originated on average 47% of the loans at the zip-code level. The statistics for the median income and racial groups of applicants are also similar to the loan-level sample.

A measurement issue that has garnered considerable attention in the mortgage origination literature is that households' gross income is self-reported in HMDA, and hence overstated. We believe overstatement of income is not an issue for our paper and, in fact, helps us capture unobserved variation in the demand for housing. Households overstate their income to obtain a larger loan than their actual income would allow. Suppose we have two households with the same gross income, but one household head overstates her income, because the household wants a larger loan than what her income suggests. If we were controlling for true income, we would mistakenly assume the two households have the same demand for loans.

3 Mortgage Origination Behavior in the Post-crisis Period

Figure 1 depicts the change in mortgage-originating behavior by larger and smaller financial institutions in the post-crisis period. Figure 1 plots raw data, and describes facts that hold in the data before we perform any multivariate analysis.

In Panel A of Figure 1, we plot the densities of the mortgage amounts originated by the top 3 financial institutions throughout our sample period (2007-2014), based on the share of mortgages originated each year. In each graph, the long-dashed red line refers to the density of loan amounts in 2008; the short-dashed blue line refers to 2011; and the solid black line refer to 2014. Changes in originating behavior are similar for these large lenders. In 2008, all financial institutions had an incentive to originate loans below the conforming loan limit of $417K. Bunching of originated mortgages just below the value of the limit in 2008 emphasizes this incentive.[14] The distribution contracted from 2008 to 2010, and started expanding thereafter. Because of the left shift, the bunching at the conforming loan limit had started reducing between 2008 and 2010.

The novel fact we document is that, since 2011, the distribution of mortgages shifted to the right

[14] The smaller bunching at the top of the distribution is due to our winsorization of loan amounts at the 0.5% level. It does not reflect a choice on the part of the originators.

for all lenders, and especially for larger lenders. Moreover, the incentives to bunch loans below the conforming loan limit has decreased dramatically for large lenders since 2011. The incentive was even lower in 2014, when the bunching was quite limited for Wells Fargo to completely eliminated for J.P. Morgan. These facts hold for all large lenders, as can be seen in the left plot of Panel B of Figure 1. Note the top 20 lenders by size account for about 40% of all mortgage originations in the period 2007-2014. The distribution of originated loan amounts had a fat right tail in 2014, which was virtually non-existent in 2008.

The drop in bunching is not evident for smaller financial institutions and non-bank mortgage lenders (see the middle and right plots in Panel B of Figure 1), even though the distribution has shifted to the right also for them. Consistent with this fact, the density of loan sizes had a thinner right tail in 2014 than the density for the largest financial institutions. At the same time, smaller lenders and non-bank lenders also reduced the share of loans originated below the mean of the distribution, and increased the share of loans just below the conforming loan limit.

In Figure 2, we present the raw percentage change in loans originated by bank size between 2008 and 2014 to show that our results are not an artifact of the choice of lenders included in the various groups reported in Figure 1. The left panel considers loans between \$100K and \$417K. The right panel considers loans above \$417K. We group institutions in 15 equal-size groups based on total lending, and report the value-weighted change in lending for each group. To avoid including very small banks, we limit the sample to the top 1,000 lenders. The plots show larger banks have decreased their lending in the \$100K – \$417K category and have increased their lending above the conforming loan limit of \$417K.

Before moving to the multivariate analysis, we use the raw data to provide back-of-the-envelope calculations of the overall change in the loan-size distribution for all lenders. Because the total amount of mortgage lending changes from year to year, we scale the total amount of lending in 2010 and 2014, so that they are both equal to 100. We then compare the proportion of lending below and above the conforming limit in 2010 and 2014. We find that lenders reduced their lending below the conforming loan limit by 15% and they increased it by 21% above the conforming limit in 2014 compared to 2010.

4 Mortgage Origination Since 2011: Multivariate Analysis

In this section we move from describing the raw data to presenting results that control for the determinants of mortgage demand.

Figure 3 plots the value-weighted residuals from regressing percentage changes between 2010 and 2014 in the fraction of loans generated within five size categories on a set of controls computed as averages at the lender level in 2007. The controls are defined as follows: $ApplicantIncome$ is the applicant income from HMDA. *Black*, *Asian*, and *Latino* are dummy variables that equal 1 if the applicant belongs to the respective demographic group. $MedianHousePrice$ comes from Zillow and is the median price of properties in the county in which the loan was originated. Lenders within or outside the top 20 by share of activity differ in the change of loans originated below and above the conforming loan limit. Large institutions decreased the share of loans below the limit more than smaller institutions. At the same time, large institutions increased the share of loans above the limit more than smaller institutions. Lenders could barely cut loans below \$100K, because these loans are most common among demographics that are protected by the Fair Lending Act.

4.1 Baseline Specification

The results in Figure 3 suggest that observable determinants of demand cannot explain the redistribution we document. To further assess whether the redistribution is a demand or supply phenomenon, we propose a regression analysis that exploits the different originating behavior of large and small lenders since 2011. We compute the shares of mortgages originated by the top 20 lenders in the United States in each county and compare the within-county lending behavior across counties with higher or lower shares of top 20 lenders since 2011. The baseline specification is as follows:

$$\begin{aligned} Log(LoanAmount)_{i,k,t} = \alpha + \beta\ Top20\_Share_{k,t} + \gamma\ Top20\_Share_{k,t} \times Post\_2011_t \\ + X'_{i,k,t}\delta + D'_{k,t}\phi + \eta_k + \eta_t + \epsilon_{i,k,t}, \end{aligned} \tag{1}$$

where $Log(LoanAmount)_{i,k,t}$ is the log amount of the mortgage obtained by applicant i in county k in year t; $Top20\_Share_{k,t}$ is the percentage of loans generated within a county by the top 20 mortgage originators by lending activity; $Post\_2011_t$ is a dummy variable equal to zero for the years 2008-2010 and 1 thereafter (2011-2014); $X'_{i,k,t}$ is a vector of individual-specific covariates that includes *Income*, the log income of the applicant, as well as dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; $D'_{k,t}$ is a county-specific vector of covariates that includes the average number of applicants – for a given year – that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price* is the log median house price in a given county for a given year;[15] η_k and η_t denote county and year fixed effects.

The coefficients of interest are β and γ. The coefficient β captures the effect of the changes in the share of large lenders in a given county on the size of the loans in that county. The coefficient γ captures the additional effect these changes in share have since 2011, compared to before 2011.

To capture changes in origination behavior across different loan sizes, we estimate separately Equation 1 for loans in five size categories: loans between zero and \$100K, between \$100K and \$200K, between \$200K and \$417K, between \$417K and \$700K, and greater than \$700K. The results for the various groups are reported in Table 2.

In the first column (the smallest loans), we find an insignificant effect of the share of top 20 lenders on loan sizes since 2011. In the second column, we find a negative and significant coefficient of -0.022 for the loans in the \$100K-\$200K category. Economically, the coefficient implies that for a hypothetical county that has a 100% share of large lenders, the average loan reduction since 2011 equals -2.2%. Likewise, for a county that has a 50% share of large lenders, the effect equals 1.1%. Assuming the average loan in the category equals \$150K, the per-loan economic effect for a county with a 50% share equals: $-\$150K \cdot 2.2\% \cdot 50\% = -\$1,650$. Multiplying this quantity by the 3,912,441 loans generated since 2011 in this category, we obtain a total economic effect of $-\$1,650 \cdot 3,912,441 = -\$6.5$ billion dollars.[16]

The coefficient estimate $\hat{\gamma}$ for the third group is also negative – even though significant only at the 10% level – whereas the results for the fourth and fifth groups are positive, economically

[15] It is important to control for house prices, because Favara and Imbs (2015) find changes in local credit supply affect house prices.

[16] The regression estimates reported in Table 2 are based on the subset of loans for which all regressors do not contain missing values (4,463,568) rather than the 6,959,924 loans originated in the period. For these computations, however, we use the full set of loans since 2011 because we want to assess overall economic magnitudes.

large, and statistically significant. If we repeat a procedure similar to the one described above, we obtain the following economic magnitudes for the three groups: group 3= $-\$300K \cdot 1.2\% \cdot 50\% \cdot 3,371,515 = -\$6,068,727$; group 4= $\$550K \cdot 4.3\% \cdot 50\% \cdot 690,453 = \$8,164,606$ and group 5= $\$800K \cdot 24.1\% \cdot 50\% \cdot 205,147 = \$19,776,171$. Note these economic magnitudes do not represent the overall change in origination since 2011, but only the differential lending behavior of large and small banks.

These results show that, since 2011, the higher the share of large banks in a given county, the smaller the size of the loans below the conforming loan limit and the larger the size above the conforming loan limit. The effects are stronger for mortgages further away from the conforming loan limit. We observe a substantial decrease in the dollar amount of loans between from $100K to $200K, and a substantial increase in the dollar amount of loans above $700K.

In Online Appendix A.1, we run the analysis at the zip-code level and show that similar results hold at the extensive margin.

4.2 Robustness

We now proceed to assess the robustness of the baseline results to alternative specifications.

We first verify the results are qualitatively similar if we change the number of financial institutions we define large or small. According to our interpretation, the larger a bank is, the higher the redistribution of lending. Therefore, the smaller the set of large institutions we consider, the larger we expect the size of this redistribution to be. In Panel A and Panel B of Table 3, we find the results are qualitatively similar to our baseline analysis when we compare the originating behavior of the top 5 largest institutions, or the top 100 largest institutions, with the behavior of the remaining lenders. As expected, the size of the estimated coefficients decreases as the size of the top group increases.

A concern with the baseline analysis is that the linear specifications cannot keep constant the characteristics of the demand of mortgages if such characteristics have a nonlinear relationship with the outcome variable. For instance, in the baseline results, we control for the median house price in the county where the applicant resides in the year of the application. But if the originating behavior of financial institutions depends nonlinearly on the overall distribution of house prices, keeping constant the median price would not be enough. Thus, in Panel C of Table 3, we control for changes in other parts of the house-price distribution, by including bottom, middle and top tier house prices from Zillow. The results do not differ from our baseline analysis.

In Panel D of Table 3, we include the full set of interactions between our control variables and the *Post_2011* dummy. These specifications allow each of our covariates, including the variables that capture the demand of mortgages, to vary systematically since 2011. The results are similar when we allow for this full set of interactions.

To control for time-varying local demand and supply shocks, in Panel E of Table 3, we add a set of state×year fixed effects to the baseline specification and replicate the baseline results.

In Panel F, we find no differential effect in sand states (California, Nevada, Arizona, and Florida) for the drop in average loan amount for loans below the non-conforming limit. If anything, the positive effect on the amount for loans above the limit is higher in sand states than in other states. Note the specification includes the full set of interactions needed to interpret the coefficients on the triple interaction, which, because of space constraints, are the only ones we report.

We then test whether the varying concentration of lending across local markets and over time might explain our results, because our share in lending activity by Top 20 financial institutions might in fact vary systematically across more or less concentrated local lending markets. Scharfstein and Sunderam (2016) show that local mortgage lending concentration at the county level is important to determine the transmission of monetary policy measures that aim to affect borrowing through changing interest rates. We follow their definition of local mortgage lending concentration, that is, compute the market share of the top 4 financial institutions by county-year. A priori, it is not clear whether local concentration is likely to be higher in counties with a higher or lower lending activity by top 20 US lenders. In fact, there is no significant correlation between our share of top 20 US lender activity and the measure of county-level lending market concentration (-0.001, p-value>85%). In Panel G of Table 3, we re-estimate our baseline specifications including, on the right hand side, the measure of local lending market concentration and its interaction with the *Post_2011* dummy. We report the estimated coefficients for the two interactions. Consistent with the fact that the two measures seem uncorrelated in the data, our baseline results are not affected by including these additional variables.

So far, we have studied the reaction of all types of lenders since 2011. One might wonder whether banks and non-bank mortgage originators reacted differently to the change in incentives. For instance, anecdotal evidence suggests that non-bank mortgage originators proliferated after the wave of bank bankruptcies during the financial crisis. In Panel H of Table 3, we exclude all loans originated by a non-bank lender, and confirm our baseline results.

In Panel I of Table 3, we add to our baseline specification the share of properties foreclosed in the zip code of the applicant as a covariate. We do so, because Favara and Giannetti (Forthcoming) find that lenders with a higher share of outstanding mortgages on their balance sheet are less likely to foreclose. The share of foreclosures is available at the zip-code level from Zillow, but only for a subset of zip codes. Adding this control thus reduces the size of the sample. We find similar results.

4.3 Mortgage Origination Since 2011: Rejected Loans

The results reported so far consider only the loans approved. To provide further evidence on the differential behavior of lenders for small and large loans, we now present results for rejected mortgage applications. In this case, the outcome variable is the income-to-loan ratio. The idea is to have a proxy for the strictness of lending standards since 2011. Higher income-to-loan ratios among denied loans suggest lending standards are stricter, because the lenders are rejecting wealthier households for the same loan amount demanded. We estimate the following specification:

$$\begin{aligned} Log(Income/LoanAmount)_{i,k,t} = \alpha + \beta\ Top20\_Share_{k,t} + \gamma\ Top20\_Share_{k,t} \times Post\_2011_t \\ + X'_{i,k,t}\delta + D'_{k,t}\phi + \eta_k + \eta_t + \epsilon_{i,k,t}, \end{aligned} \tag{2}$$

where $Log(Income/LoanAmount)_{i,k,t}$ is the log income-to-loan ratio for the mortgage denied to applicant i in county k in year t. The rest of the variables are defined as in Equation 1.

The results are reported in Table 4. Consistent with a change in lending standards, the coefficient estimates $\hat{\gamma}$ have opposite signs, compared to the ones in Table 2. Income-to-loan ratios increased for medium-sized loan applications (columns 2 and 3), and decreased for large loans (columns 4 and 5).

The economic interpretation is as follows. The coefficient estimate $\hat{\gamma}$ in column 2 is 0.03, which implies that for a hypothetical county that has a 100% share of large lenders, the income-to-loan ratio of the denied mortgage applications is 3% higher since 2011. Applicants thus needed to have a 3% higher income-to-loan ratio for their application not to be denied. The coefficient estimates for the third, fourth, and fifth specifications equal 3%, -4.6%, and -22.9% and hence are also economically significant.

Overall, the results reported in this section show the change in behavior by large institutions has had a large economic impact on the type of loans approved and denied across the United States.

5 Identification Strategies

The fact that large and small lenders are not assigned randomly across counties hinders a causal interpretation of the baseline results. Unobservables might have changed the distribution of lenders across counties since 2011 and might have also determined a change in mortgage demand since 2011, consistent with our results. In this case, the change of the originating behavior of lenders would not be due to supply-side forces, but to demand. To disentangle the two effects, we would need to assign large and small lenders randomly across counties every year from 2008 to 2014.

Our baseline analysis would document a causal effect only if at each point in time, counties with different shares of large lenders were similar in all other respects, including unobservable characteristics. In particular, households in these counties should have the same demand for mortgages, and should react the same way to changes in the supply of mortgages.

The ideal source of exogenous variation in the share of large lenders across counties should not be affected by the financial crisis and the developments of the mortgage market after the crisis. This source of variation should also be unrelated to underlying unobserved characteristics of counties, which determine the supply and demand of mortgage credit across the income distribution. Such a source of exogenous variation would allow us to test for the causal effect of the share of large lenders on the distribution of mortgages since 2011. To get close to such an ideal source of variation, we propose two strategies. First, an instrumental-variable identification strategy. Second, a propensity-score matching design, which only compares counties that are very similar based on observable characteristics, including geographic location, but differ in the share of mortgage lending activity by large lenders.

5.1 Instrument and Two-Stage Least-Squares Specification

In our first strategy, we instrument the yearly share of large lenders in a county in the period from 2008 to 2014 with the share of large lenders in the county as of 2007. The rationale is that the financial crisis or any developments in the mortgage market after the crisis could not have determined the share of large lenders in 2007.[17] Moreover, policy changes after the crisis could not have affected the share of large lenders in 2007. This instrument is likely to be relevant, because inertia is present in the spatial penetration of bank branches. We document the relevance of the instrument below.

Figure 4 and Figure 5 describe graphically the variation in the share of large lenders in 2007. Figure 4 plots the probability density function for the share of large lenders. The variable obtains values throughout its range, between 0 and 1. For the vast majority of counties, large lenders cover between 20% and 80% of the overall mortgage activity in 2007, and the modal value is about 50%. The variation in the share of large lenders across counties is substantial.

Similar to our baseline analysis, we focus on the variation in loan amounts within counties since 2011. The systematic variation in the share of large lenders across counties does not identify the coefficients in the specifications described below. At the same time, the intensity of the treatment is higher for counties with a higher share of large lenders than for other counties. We note substantial variation in type of lenders even across similar bordering counties. Panel A of Figure 5 plots the spatial variation in the share of large lenders in 2007 across all US counties. Panel B of Figure 5 plots the corresponding spatial variation for counties in Iowa. Counties in Iowa are homogeneous in terms of observable characteristics, including their racial composition, the median house prices, and average household income. Both panels document substantial spatial variation in the share of large lenders, including across areas that are otherwise similar.

To implement our instrumental-variable strategy, we estimate a set of two-stage least-squares

[17] The first signs of distress in the US financial markets were in late 2007, and the financial crisis did not hit until October 2008.

regressions. The endogenous covariate is the interaction between the county-level share of large lenders in each year from 2008 to 2014 and the dummy variable for the years 2011-2014. The county-level share of large lenders in 2007 is absorbed by the county fixed effect, as it does not vary within counties over time. For this reason, in the analysis we do not use both the level and the interaction of the share with the dummy for the years 2011-2014. We predict directly the endogenous variable using the interaction between the county-level share of large lenders in 2007 and the dummy for the years 2011-2014 as the instrument. Specifically, we estimate the following specification:

$$\begin{aligned} Top20\_Share_{k,t} \times Post\_2011_t = {} & \alpha + \gamma \; Top20\_Share_{k,2007} \times Post\_2011_t \\ & + X'_{i,k,t}\delta + D'_{k,t}\phi + \eta_k + \eta_t + \epsilon_{i,k,t}, \end{aligned} \tag{3}$$

where $Top20\_Share_{k,t}$ is the percentage of large-institution activity in a county in year t and county k, $Top20\_Share_{k,2007}$ is the percentage in year 2007 and county k, and X and D are a set of observables at the individual and county level in year t; η_k and η_t are sets of county and year fixed effects. Note that restricting the variation within years using year fixed effects absorbs completely the variation in the level of $Top20\_Share_{k,2007}$ across counties, which is why the level of the variable does not appear in the RHS of Equation 3.

The specification in Equation 3 mirrors our baseline analysis in restricting the variation within counties with the addition of county fixed effects. Hence, the variation we exploit for identification excludes any systematic differences in the share of large lenders across counties. We only exploit the component of exposure to large lenders that has not changed within counties, and look at the differential effect of this component on origination since 2011.

In the second stage, we use the instrumented interaction on the LHS of Equation 3 as the main covariate of the following specification, which is otherwise the same as our baseline regression in Equation 1:

$$\begin{aligned} Log(LoanAmount)_{i,k,t} = {} & \alpha + \beta \; \widehat{Top20\_Share_{k,t} \times Post\_2011t} \\ & + X'_{i,k,t}\delta + D'_{k,t}\phi + \eta_k + \eta_t + \epsilon_{i,k,t}, \end{aligned} \tag{4}$$

where $Log(LoanAmount)_{i,k,t}$ is the log amount of the mortgage obtained by applicant i in county k in year t.

5.1.1 Validity of the Instrument

To assess the validity of our instrument, we need to verify its relevance and discuss the plausibility that the exclusion restriction we assume for causal interpretation holds.

In terms of relevance, the instrument needs to be correlated with the endogenous variable we want to instrument, because a weak instrument would lead to inconsistent estimates and would invalidate statistical inference in the second stage. Our analysis does not appear to be prone to a weak-instrument problem. For each column of Table 7 and Table 8, we report the first-stage F-statistic associated with our instrument. The first-stage Kleibergen-Paap F-statistics are larger than 101 across all specifications.

If we want to interpret the estimated coefficient $\hat{\beta}$ in Equation 4 causally, we need to assume an exclusion restriction. The share of large lenders in a county in 2007 should only affect the amounts lent in the county in the following years through the share of large lenders in the following years, and not through unobservable characteristics at the county level or individual-borrower level. This exclusion restriction cannot be tested directly, and hence we propose two sets of results to assess its plausibility.

First, we test whether observable characteristics that are important determinants of mortgage demand are systematically related to the share of large lenders. In Table 5, we describe the balancing of the main outcome variables and covariates for counties with different shares of large lenders. The first four columns of Table 5 report the average value of each variable for the observations in all years, split into four equal-size groups based on the share of large lenders. The first column refers to observations in the bottom quarter of share of large lenders by county, whereas the fourth column refers to observations in the top quarter of the share of large lenders by county. The fifth column reports the standard deviation for each of the listed variables, which helps in assessing the size of the differences in the averages across quantiles. The outcome variables include the average change in the mortgage amounts for loans below $100K, between $100K and $200K, between $200K and $417K, between $417K and $700K, and above $700K, between 2007 and 2010. The covariates include dummy variables for the following: whether the applicant is Black, Asian, or Latino; the share of Black, Asian, and Latino population in each county-year; the log of the median house price in each county-year; and the share of foreclosed houses in each county-year.

Both the dependent and independent variables appear to be balanced for varying shares of large lenders. The average loan amounts for the groups above $417K show virtually no difference between the bottom quarter and the top quarter of observations based on share of large lenders. The two groups between $100K and $417K also appear to be substantially balanced, because the differences across group averages are small compared to the standard deviation in the underlying variables. The loans below $100K have a higher average for the top quarter by share of large lenders, but the difference with the other groups is less than one tenth of a standard deviation in the underlying variable, and this group is the one in which we find no change in origination by share of large lenders. The covariates also appear to be balanced, with no monotonic relations between group averages and share of large lenders, except for the median house price in county-years, which increases with the share of large lenders.

Second, we report the reduced-form specification, and we run our baseline specification adding the interaction between the share of large lenders in 2007 and the dummy for the years 2011-2014. In these specifications, we add loan observations for 2007, because the instrument is measured in 2007. Panel A of Table 6 shows the reduced-form results are similar to the baseline results in Table 2. If we had substantial violations of the exclusion restriction, we should find that both the instrument and the endogenous variable have an autonomous correlation with the outcome variables once they enter in the same reduced-form specification. On the contrary, Panel B of Table 6 shows we cannot reject both null hypotheses that the coefficients associated with the two interactions are different from zero for any loan size. This result suggests that even if violations of the exclusion restriction existed, they could hardly be relevant enough to explain our baseline results.

5.1.2 Second-Stage Results

We report the results for estimating Equation 4 in Table 7. Table 7 refers to specifications using approved loans, and whose dependent variable is the average loan amount within each size group. The sign and magnitude of the coefficients are similar to the baseline OLS results. A 10% increase in the instrumented interaction between the share of large lenders in the county and the dummy for the years 2011-2014 decreases the average amount of approved mortgages between \$100K and the conforming loan limit, whereas it increases the average amount of jumbo loans.

The results are similar if we add the percent of foreclosed houses in the county by year. We do not use this specification as our main specification, because the percent of foreclosed houses is only available from Zillow for about two thirds of the counties.

Table 8 refers to the specifications using denied loans, and whose dependent variable is the share of applicant income over the loan amount requested. Even in this case, the two-stage least-square results are similar to the baseline OLS regression results.

5.2 Propensity Score Matching Procedure

For our second strategy, we build on Scharfstein and Sunderam (2016) and Imbens (2015), and design a propensity score matching procedure that uses a matched sample of counties with similar observable characteristics at each point in time, but different shares of overall mortgage lending by large lenders. We then run our analysis on the matched sample.

In a first step, we create a matched sample of counties based on observable characteristics. We create a dummy variable that equals 1 if a county is above the top quartile of the county distribution based on the share of local mortgage lending originated by the top 20 US lenders. We then compute the propensity score for being a county in the top quartile of the distribution for each year between 2008 and 2014, based on the county-level characteristics we observe. The set of observables includes the following: the share of African-American residents, the share of Asian residents, the share of Latino residents, the logarithm of the total number of residents, the median household income, the share of residents that are homeowners, and state fixed-effects, which penalize heavily the matching of counties across states. To compute the propensity score, we estimate a logit specification whose outcome variable is the dummy for being above the top quartile of the distribution.

Following Scharfstein and Sunderam (2016), we limit the sample to counties that are above the top quartile or below the bottom quartile of the distribution based on the share of local mortgage lending originated by the top 20 US lenders. This restriction drops half of our sample, but allows us to match counties so that our treated group – counties above the top quartile, and hence with a high share of top 20 lending – has a substantially higher share of top 20 lending than the control group – counties below the bottom quartile. We do verify that the treated and control distributions of the propensity scores have a common support, with a substantial overlapping portion. Treated and control counties are therefore similar in terms of observables.

We conduct a nearest-neighbor matching without replacement of the treated and control counties based on the propensity score for being above the top quartile of the distribution. We drop the matches for which the values of the propensity score within the matched couple do not fall into the common support of the treated and control group. This way, we ensure that the couples of matched counties we keep in the analysis are indeed similar on observables. To further ensure the matched counties are similar on observables, we follow Scharfstein and Sunderam (2016) and also exclude matched couples for which the difference in propensity score is higher than one quarter of the standard deviation of the propensity score in the matched sample, as well as counties which fall at the extremes of the propensity score distribution – below 0.15 and above 0.80. Our results are very similar if we do not impose these restrictions.

Armed with the matched sample of counties, we merge the status of the counties – treated or control – into the individual-level loan data. Before estimating our baseline specification in the matched sample, we further verify that the individual level loans associated with treated and control counties do not differ in terms of propensity scores of the county characteristics.[18] We perform a t-test for whether the difference of the average propensity scores for individual loans associated with treated and control counties is different from zero, and we fail to reject the null hypothesis above the 96%

[18]Before the number of individual loans differ across counties, merging the county-level treatment status into the individual-level loan data might change the average of the propensity score across loans associated with treated and control counties.

level of significance.

We then estimate the following specification on the matched sample of individual loans by ordinary least squares:

$$\begin{aligned} Log(LoanAmount)_{i,k,t} = \alpha + \beta\ Treated_{k,t} + \gamma\ Treated_{k,t} \times Post\_2011_t \\ + X'_{i,k,t}\delta + D'_{k,t}\phi + \eta_k + \eta_t + \epsilon_{i,k,t}, \end{aligned} \tag{5}$$

where $Log(LoanAmount)_{i,k,t}$ is the log amount of the mortgage obtained by applicant i in county k in year t; $Treated_{k,t}$ is the percentage of loans generated within a county by the top 20 mortgage originators by lending activity, multiplied by a dummy that equals 1 if the county is above the top quartile of the distribution by top 20 lending; $Post\_2011_t$ is a dummy variable equal to zero for the years 2008-2010 and 1 for the years 2011-2014; $X'_{i,k,t}$ is a vector of individual-specific covariates that includes *Income*, the log income of the applicant, as well as dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; $D'_{k,t}$ is a county-specific vector of covariates that includes the average number of applicants – for a given year – that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price* is the log median house price in a given county for a given year; η_k and η_t denote county and year fixed effects.

Table 9 reports the results for estimating Equation 5. We confirm our results in the baseline analysis. We observe the increase in the average amount for loans above the conforming loan limit (column (5) and column (6)), and the size of the effects is larger for the largest bracket of loan amount. We also observe the drop in average amount for loans below the conforming loan limit, i.e., between \$200K and \$470K in size (column (3)). At the same time, we fail to reject the null that the average amount of loans between \$100K and \$200K has changed differently for counties in the top quartile by penetration of large financial institutions.

6 Potential Explanations

In this section, we discuss the extent to which a set of potential explanations is consistent with our findings. The only explanation consistent with all the facts we document is that lenders changed their origination strategy as a reaction to the higher costs of mortgage origination imposed by financial regulation approved in 2010.

6.1 Changes in the Private-Label Securitization Market

During the recent financial crisis, private-label securitization came to a halt (Goodman, 2015). If banks wanted to originate jumbo loans, which cannot be sold to GSEs, they could have hardly securitized them privately. As documented in Goodman (2015, Figure 2), the aggregate amount of issuance of private-label residential mortgage-backed securities has dropped from approximately $700 billion in 2007 to approximately $60 billion in 2008. After the crisis, the private-label residential mortgage-backed securitization has remained stagnant or contracted even more in both our control and treatment periods, even if private-label securitization in other asset classes – such as credit cards, automobile loans, and student loans – started to recover after 2011. The fact that private-label residential mortgage securitization did not recover throughout the period 2008-2014 is an indication that this channel cannot explain our results.

A remaining concern is that, even though the private-label residential mortgage-backed securitization market has remained frozen since 2011, small banks might have stopped the private securitization of their loans completely after 2010, and large banks might have substituted them completely. To rule out this concern, we estimate a variation of Equation 1 in which the dependent variable is a dummy that equals 1 if the originated loan is securitized privately. We find that large banks did not securitize privately more than small banks either below or above the conforming loan limit since 2011.

6.2 Risk Management

Another potential explanation is that large banks have moved towards jumbo loans more than small banks to comply with capital requirements or to reduce the risk of their pool of assets. To rule out this alternative explanation, we propose two arguments. First, we run a test in line with the one in Figure 2. We rank the top 100 institutions in our sample based on the riskiness of their assets and match them with the list of institutions in our sample. We test whether the riskiest institutions have increased the origination of jumbo loans, and have decreased the origination of conforming loans since 2011. Our preferred measure of bank risk is the share of reserves over the total amount of non-performing loans held by the bank. We report the results in Figure 6, which also replicates Figure 2 for the subsample of institutions for which we observe our risk measure. As is apparent from the pictures, there is no relation between riskiness and the change in lending behavior, whereas our baseline result is replicated for the subsample of banks for which we observe the risk measure.

A second argument is that there is no evidence that jumbo loans are at lower risk of default than smaller loans, even if the applicants are on average wealthier. If anything, evidence based on customized CoreLogic data shows that, in the second quarter of 2010, the delinquency rate on mortgages for investment homes above $1 million was twice as high as the delinquency rate on mortgages below $1 million. Delinquency rates for large mortgages were higher than those for smaller mortgages for both owner-occupied and investment real estate.[19]

6.3 Wealth Polarization

A third potential explanation is wealth polarization. Anecdotal evidence suggests the US wealth distribution has polarized since the financial crisis. Households that belonged to the middle class before the crisis have moved toward the left or the right tail of the wealth distribution. To disentangle a supply-side interpretation from wealth polarization, we estimate the baseline specification across counties with a high or low share of middle-class households before the crisis. Wealth polarization predicts that in counties with a larger share of middle-class households before the crisis, more households moved to the right tail of the wealth distribution after the crisis, and hence the demand for large loans would be higher and the demand for middle-range loans lower.[20] To the contrary, our interpretation predicts the effect should be larger in counties with a lower share of middle-class households prior to the crisis. The rationale is that in those counties, after the crisis, large banks would have more wealthy households to whom they could start to provide lending. Consistent with our interpretation, the results are stronger for counties that had a lower share of middle-class households before the crisis.

[19] *"Biggest Defaulters on Mortgages are the Rich,"* New York Times, 2010.

[20] Polarization also increased the share of low-income households after the crisis, but our results show lending to this segment did not change since 2011.

Another concern is that middle-class households might have accumulated higher mortgage debt before the crisis. They would thus be less likely to demand mortgages after the crisis. We estimate our results in counties with a high and low share of middle-class households that had a mortgage outstanding before the crisis. Contrary to the higher mortgage debt channel, the results are stronger in counties with lower pre-crisis accumulation of mortgage debt by middle-class households.

We also repeat the exercise using proxies for the ex-ante exposure of counties to the demand-side effects of the financial crisis, such as the share of overall county income from stock dividends and interests in 2007, and the share of workforce in the public administration. We find the results do not differ systematically across any of the subgroups.

Because of space constraints, we report the results described above as well as the description of the additional data sources we use for this analysis in Table A.2 and the associated Online Appendix A.3.

6.4 Putback Policy by GSEs

Fourth, we consider the fact that GSEs have introduced a stricter "putback" policy after the financial crisis, by which they started to force lenders to buy back loans whose origination documents were found to misrepresent borrowers' ability to repay. This stricter policy created uncertainty in the effectiveness of the GSEs' guarantee, and hence might have dis-incentivized lenders to originate conforming loans. This policy could explain our results below the conforming loan limit only if misrepresenting borrowers were more concentrated in loans between \$100K and \$200K, but it cannot explain why the increase is higher for large jumbo loans well above the conforming loan limit.

6.5 Changes in Financial Regulation

A fifth potential explanation for our results is that the costs of originating conforming loans has increased since 2011. This increase could be driven by two changes in regulation – the approval of the *Temporary Payroll Tax Cut Continuation Act* (TCCA) and the approval of the *Dodd-Frank Wall Street Reform and Consumer Protection Act* (Dodd-Frank).

TCCA determined a series of increases of the annual insurance premia lenders pay to GSEs ("g-fees") starting in 2011, which had to be remitted to the US Treasury for the most part.[21] This increase determined a higher per-loan cost of origination for conforming loans. This increase should have led to higher origination just above the conforming loan limit, because lenders had lower incentives to bunch loans at the limit. Moreover, the increase in g-fees should have determined a decrease in origination of similar size for all loans below the conforming loan limit. Different from these predictions, we find that the increase in origination is mainly driven by loans well above the conforming loan limit, and the decrease in origination is not homogeneous below the conforming loan limit, but is mainly driven by loans further away from the limit.

Dodd-Frank increased both the fixed costs and the per-loan costs of mortgage origination. A first set of provisions increased the fixed costs of originating mortgages of all sizes. For instance, lenders had to establish an internal training system and to provide special training to all loan officers at their branches. The costs of such training decreased the institutions' incentives to originate any mortgages. Second, Dodd-Frank imposed a thorough yet costly verification of the income customers reported at the time of the application. This procedure also increased the per-loan originating costs. Because the procedure has the same fixed cost, irrespective of the size of the loan, it decreased the per-loan returns of smaller loans more than those of larger loans.

Anecdotally, financial institutions reacted immediately after the approval of Dodd-Frank, even though several provisions were not self executing.[22] This behavior is expected, because lenders had to invest substantial resources, such as building new training infrastructures and hiring specialized employees, in order to be compliant with the new provisions at the time of execution.[23]

Because Dodd-Frank increased both the fixed costs and the per-dollar-lent costs of originating *all*

[21]We thank Anthony DeFusco for suggesting the increase in g-fees as a potential explanation for our findings.

[22]The Federal Reserve (Fed), and subsequently the Consumer Financial Protection Bureau, had to produce the regulations needed to make some of the provisions executable. Banks had to be compliant with the new provisions at the execution date, and hence faced the costs of compliance before that date. Because the execution date was uncertain at the time of approval of Dodd-Frank, banks' expected costs and revenues of different types of mortgages changed immediately. In the Online Appendix A.2, we show direct evidence that large banks paid these costs as early as January 2011.

[23]Note we are not claiming that all provisions of Dodd-Frank affecting the costs of originating mortgages produced effects since 2011. For instance, DeFusco, Johnson, and Mondragon (2017) exploit the execution of the "Ability to Repay" rule in 2014 to show that the change in costs of origination caused lenders to ration credit to US households.

loans, it determined a change in incentives consistent with the patterns of origination we find, that is, a substantial decrease in origination for conforming loans further away from the conforming loan limit, and an increase in origination for jumbo loans further away from the conforming loan limit.

An explanation based on higher overall and per-loan costs of origination is also consistent with the fact that large lenders changed their origination behavior more than smaller lenders for at least three reasons.

First, jumbo loans cannot be sold to GSEs, and hence institutions must either keep them on their balance sheets or sell them to private counterparties, which impose worse conditions than GSEs. Larger banks have a comparative advantage in originating jumbo loans, because they can keep more of them on their balance sheets, due to their larger amount of liabilities in the form of retail customer deposits. The same is not true for non-bank mortgage originators, smaller credit unions, and local banks. Accordingly, we find that, on average, the top 20 lenders keep 66.4% of the jumbo loans they originate on their balance sheet. Lenders outside the top 20 keep 61.3% of the jumbo loans they originate on their balance sheet. After Dodd-Frank, the top 20 lenders were 44% more likely to keep the loans they originated on their balance sheets, and 45% more likely to securitize them privately. Large financial institutions could therefore be more aggressive in setting lower rates for jumbo loans. Consistently, starting in 2010, the gap between the interest rates charged on jumbo loans and those charged on smaller loans started to close. On average, rates were the same for all type of loans as of 2013, and the gap was even negative for large banks.[24]

Second, large lenders can offer a much broader set of financial services to their customers than mortgage originators, small banks, and credit unions. Such services include wealth management, brokerage accounts, and credit cards. These services are especially appealing to wealthier customers, who demand larger loans. Therefore, large financial institutions may be willing to entice new customers with lower interest rates on large mortgages, even if they only break even.[25]

Third, large lenders have more geographically diversified operations than small lenders, and operate

[24] "One indication of banks' eagerness to woo jumbo borrowers is that average interest rates on 30-year fixed-rate jumbos in 2014 dropped below those on smaller mortgages for the first time in decades" *Wall Street Journal* (2016).

[25] This argument is quite salient to the financial industry. For example, according to Keith Gumbinger, vice president at HSH, "There is a potentially significant longer time frame to offer wealthier customers additional products and services. Banks can offer investment services, other loan products or other kinds of services" (Morrison, 2013).

in more businesses, such as proprietary trading or private equity. Large lenders can therefore redirect their activities to different geographies and businesses after the increase in the costs of mortgage origination, whereas smaller banks and mortgage originators can hardly do so.

7 Conclusions

From 2011 to 2014, US financial institutions reduced mortgage lending to medium-sized loans by approximately 15%, and increased lending to large loans by 21%. This regressive redistribution of mortgage credit survives after controlling for well-known individual- and county-level observable determinants of mortgage demand, and holds at both the loan and the zip-code levels.

The redistribution we document could represent a channel that helps explain the surge in wealth inequality in the United States since the end of the financial crisis. In particular, economists are debating why middle-class households have not resumed consuming at the pre-Great Recession levels, which contributes to the slow recovery. The US administration has implemented several costly policies in an attempt to stimulate consumption by the middle class. Whether these policies had any substantial aggregate effects is unclear, but the redistribution of mortgage credit from the middle class to the wealthy goes against the aims of such policies.

We discuss a set of potential demand- and supply-side explanations for the facts we document. Among the explanations we consider, the one that seems consistent with all our facts is that the redistribution was determined by a supply-side change, that is, the reaction of lenders to the increase in the fixed and per-loan costs of originating mortgages imposed by post-crisis financial regulation, and in particular Dodd-Frank.

Our results speak to the debate about the costs and benefits of regulating economic activity. Proponents of regulation aim to help vulnerable consumers. But regulators often underestimate the fact that lenders are private organizations competing in a free market, and that they react to the incentives created by regulation based on their own objective function. In the case of Dodd-Frank, middle-class households did not obtain cheaper mortgages, but were cut out of the mortgage market altogether. Future research at the intersection of Finance, Accounting, and Law should delve deeper into the welfare implications of Dodd-Frank for consumers, lenders, and society as a whole.

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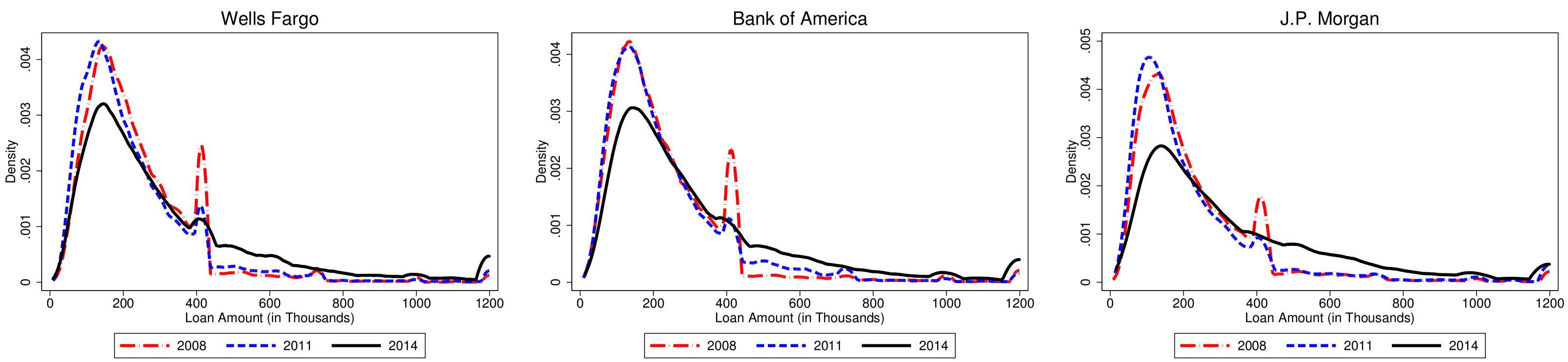
Panel A. Lending Behavior by Top Originators



Panel B. Lending Behavior for Large Institutions, Small Institutions, and Non-bank Originators

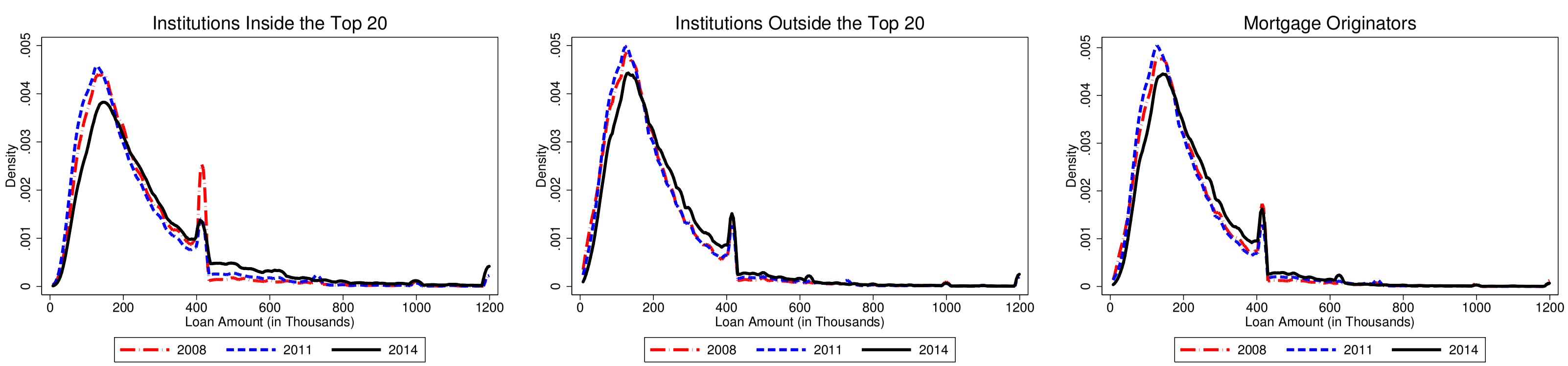


Figure 1: Panel A reports the loan size distributions for the top three institutions by mortgage lending activity over the sample: i.e. Wells Fargo, Bank of America and J.P. Morgan. Panel B reports the loan size distributions for the institutions that rank within (left plot) and outside (middle plot) the Top 20 by mortgage lending activity, as well as the loan size distributions for all the mortgage originators in our sample (right plot). Each plot reports densities for the years 2008, 2011, and 2014. Loan amounts have been winsorized at the 0.5% level.

Change in Lending by Bank size

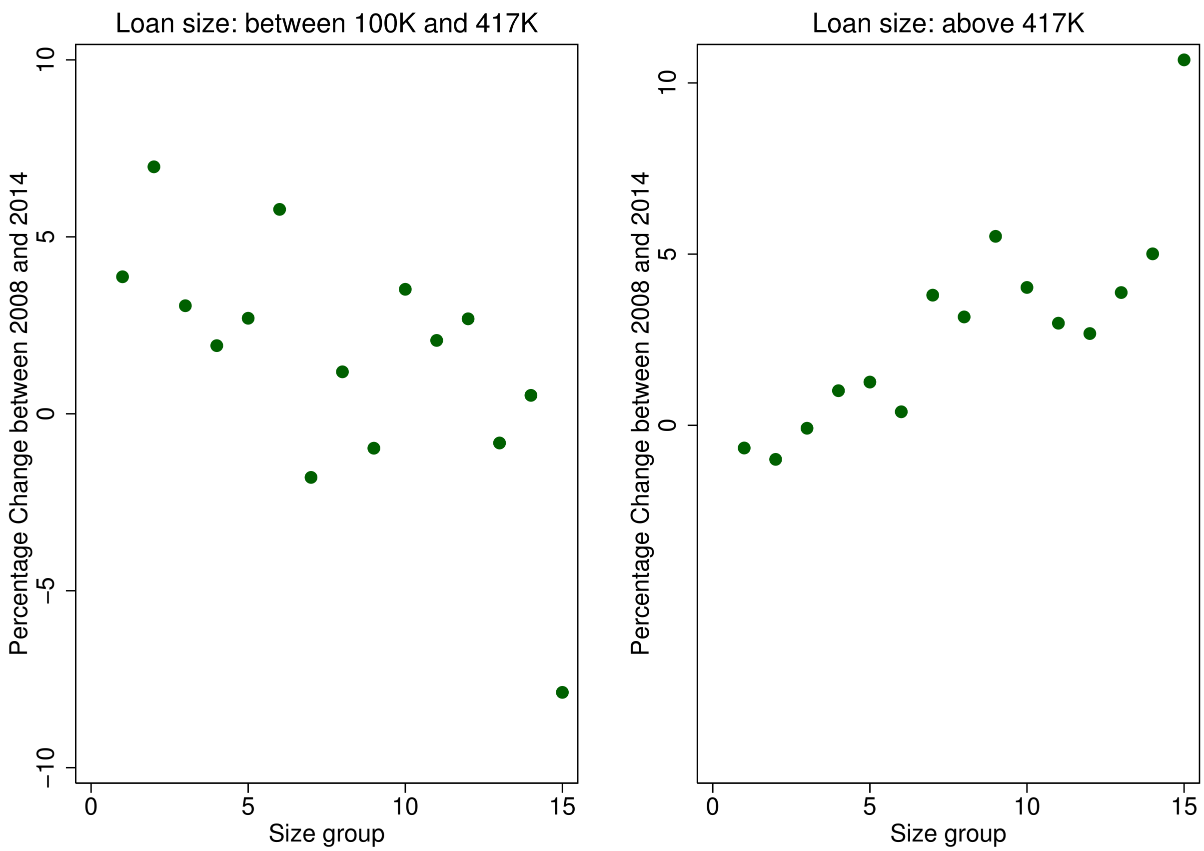


Figure 2: This figure reports the raw percentage change in loans originated by bank size between 2008 and 2014. The left panel considers loans between \$100K and \$417K. The right panel considers loans above \$417K. We group institutions in 15 equal size groups based on total lending, and report the value-weighted change in lending for each group. We limit the sample to the 1,000 top lending institutions.

Change in Lending for Institutions Inside and Outside the Top 20 by Mortgage Lending Activity - Residuals

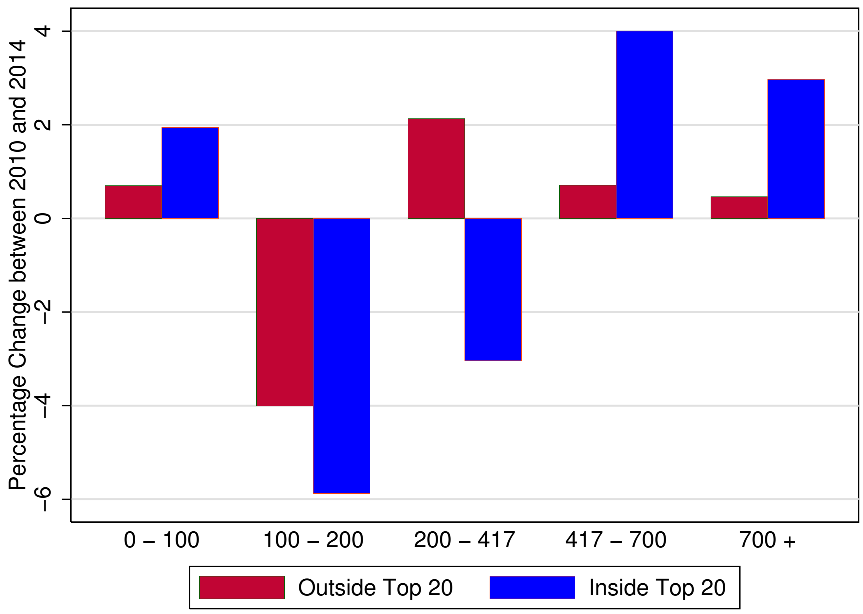


Figure 3: This figure reports value-weighted residuals from regressing percentage changes between 2010 and 2014 in the fraction of loans generated within five size categories on a set of controls computed as averages at the lender level in 2007. Weights are given by each institution's mortgage lending activity. The controls are defined as follows: $ApplicantIncome$ is the applicant income from HMDA; $Black$, $Asian$, and $Latino$ are dummy variables that equal 1 if the applicant belong to the respective demographic group; and $MedianHousePrice$ is the median price of properties in the county in which the loan was originated from Zillow. The first size category comprises loans between zero and \$100,000; the second loans between \$100,000 and \$200,000; the third loans between \$200,000 and \$417,000; the fourth loans between \$417,000 and \$700,000; and the fifth loans greater than \$700,000.

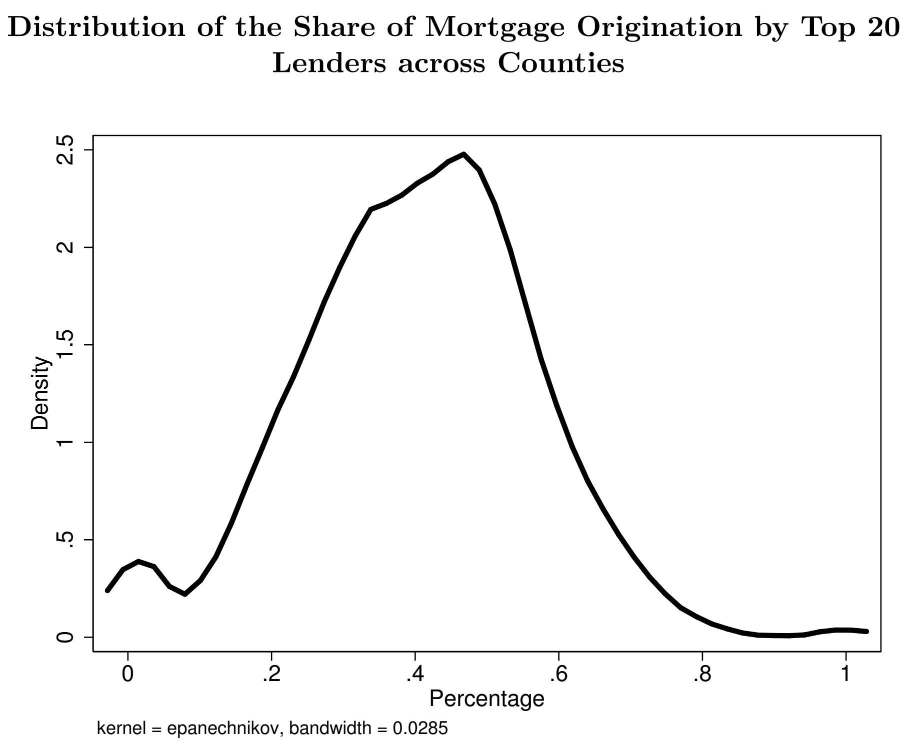
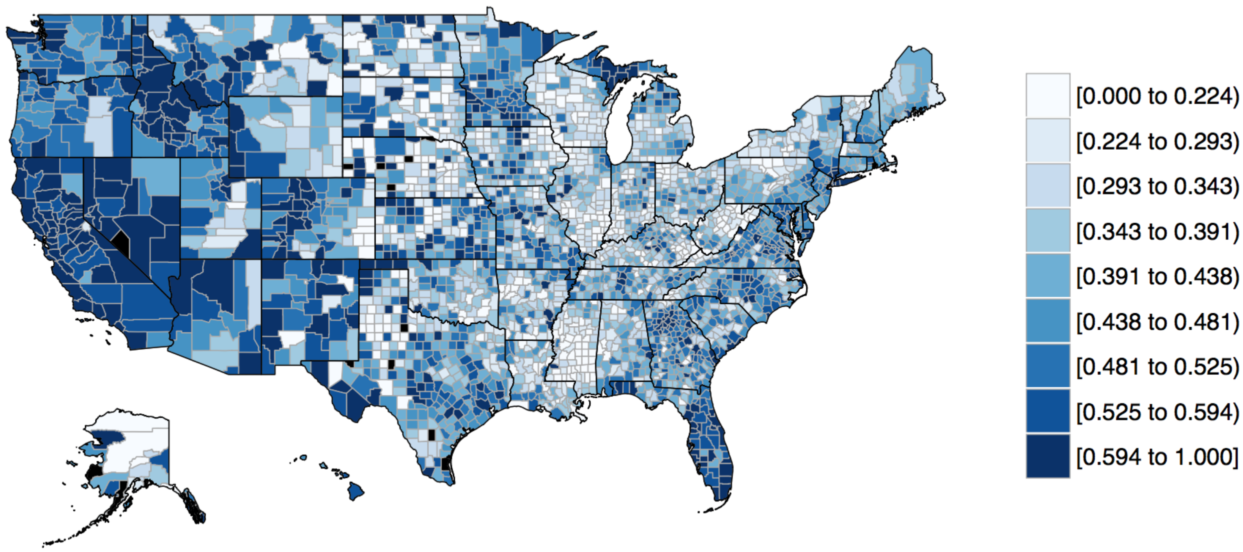


Figure 4: This figure reports kernel density estimates of the percentage of lending – across counties – generated by the Top 20 lenders for the year 2007.

Panel A. Share of Origination by Top 20 Lenders across US Counties



Panel B. Share of Origination by Top 20 Lenders across Iowan Counties

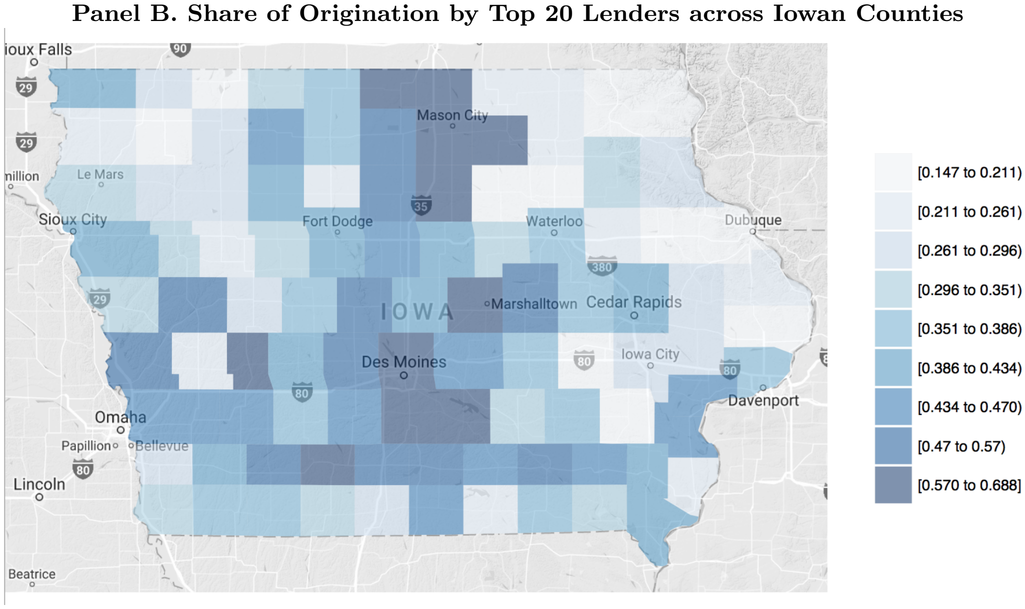
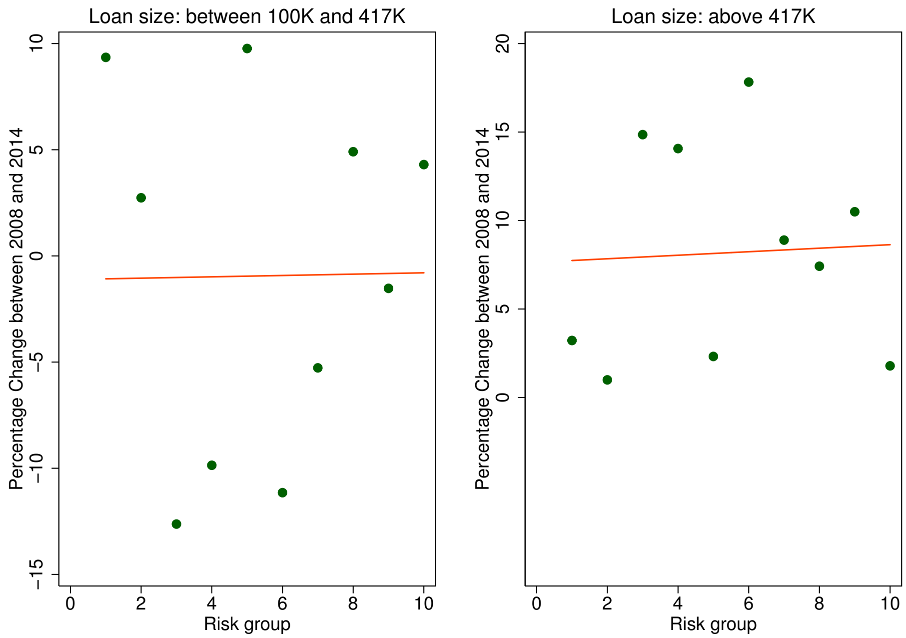


Figure 5: This figure reports choropleth maps of the percentage of lending – across counties – generated by the Top 20 lenders for the year 2007. Panel A focuses on the whole US. Panel B focuses on the state of Iowa.

Change in Lending by Bank Risk and Bank Size

Panel A. Bank Risk



Panel B. Bank Size

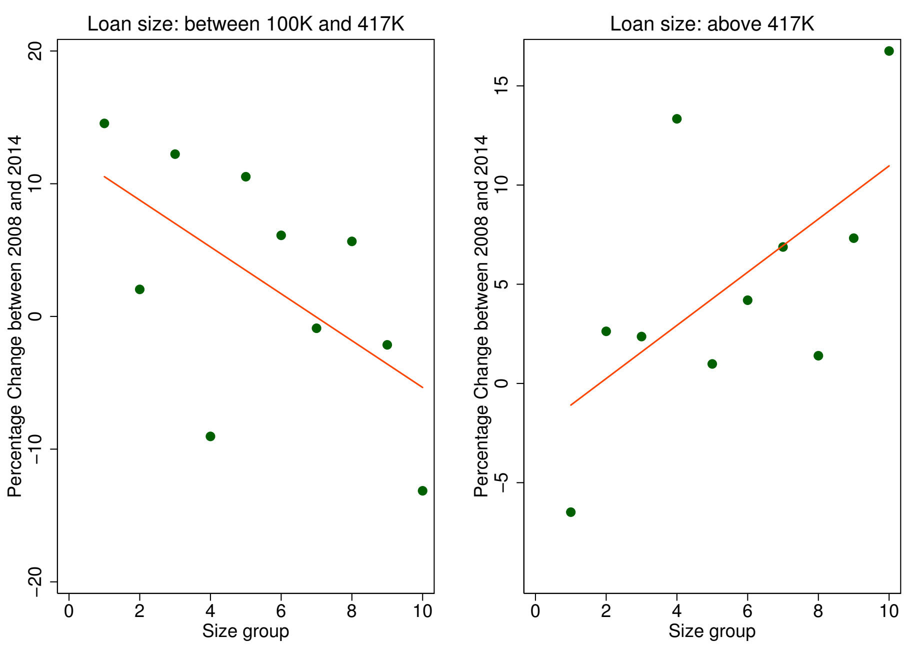


Figure 6: This figure reports in Panel A the raw percentage change in loans originated by bank risk between 2008 and 2014. The left panel considers loans between $100K and $417K. The right panel considers loans above $417K. The measure of bank risk is the share of reserves over the total amount of non-performing loans held by the bank. Because we do not observe this measure of bank risk for all the institutions in our sample, we focus on the 100 riskiest institutions and group them in 15 equal size groups based on bank risk. We report the value-weighted change in lending for each group. Panel B uses the same sample of lenders, but sorts them on size.

Table 1. Summary Statistics

Panel A. Approved Loans

	Obs.	Mean	St.dev.	5th	25th	50th	75th	95th
				Percentiles				
Loan Amount ($000)	13,532,723	228.2	162.2	65.0	124.0	186.0	285.0	518.0
Top20 Share	13,532,723	.387	.137	.161	.286	.384	.485	.616
Applicant Income ($000)	13,532,723	95.0	121.8	28.0	47.0	71.0	109.0	225.0
Black	13,532,723	.070	.255	0	0	0	0	1
Asian	13,532,723	.070	.253	0	0	0	0	1
Latino	13,532,723	.095	.293	0	0	0	0	1
Median House Price ($000)	13,532,723	219.1	199.6	101.4	132.6	184.1	264.0	459.5
Share Foreclosed (perc. points)	10,516,574	.079	.085	.004	.023	.052	.103	.268

Panel B. Rejected Loans

	Obs.	Mean	St.dev.	5th	25th	50th	75th	95th
				Percentiles				
Loan Amount ($000)	5,983,994	225.3	182.1	45.0	106.0	176.0	290.0	563.0
Top20 Share	5,983,994	.409	.143	.167	.304	.411	.519	.648
Applicant Income ($000)	5,983,994	92.8	151.8	23.0	42.0	65.0	104.0	231.0
Black	5,983,994	.121	.326	0	0	0	0	1
Asian	5,983,994	.078	.269	0	0	0	0	1
Latino	5,983,994	.141	.348	0	0	0	0	1
Median House Price ($000)	5,983,994	229.1	127.0	99.1	134.2	187.6	297.9	497.6
Share Foreclosed (perc. points)	4,475,810	8.583	9.035	.409	2.302	5.738	11.362	29.119

Panel C. Zip code-Year Level

	Obs.	Mean	St.dev.	5th	25th	50th	75th	95th
				Percentiles				
Loan Amount ($000)	15,452	208.0	128.7	88.0	127.0	169.6	245.0	458.4
Top20 Share	15,452	.472	.124	.259	.388	.477	.558	.673
Applicant Income ($000)	15,452	93.4	58.0	49.0	63.3	77.5	102.4	189.7
Avg. Black	15,452	.057	.129	0	0	.012	.048	.280
Avg. Asian	15,452	.043	.098	0	0	.009	.037	.208
Avg. Latino	15,452	.072	.138	0	.004	.026	.067	.345
Avg. Median House Price ($000)	15,452	205.5	126.0	85.1	123.4	163.3	229.5	488.0
Number Loans (000)	15,452	55.2	113.9	.030	.603	5.7	51.8	290.2

This table reports descriptive statistics for the main variables in the analysis, observed at the individual loan level (Panel A and Panel B) or at the zip code-year level (Panel C) for the period 2007-2014. *LoanAmount* and *ApplicantIncome* are the loan amount requested and applicant income – obtained from HMDA. *Top20 Share* is the share of mortgages originated by top 20 financial institutions in the zip code in which each individual mortgage is originated. *Black*, *Asian*, and *Latino* are dummy variables that equal 1 if the applicant belong to the respective demographic group. *MedianHousePrice* is the median price of properties in the county in which the loan was originated – obtained from Zillow. *ShareForeclosed* is the percentage of properties foreclosed in the zip-code in which the mortgage was originated, at the time of origination. Panel C reports the averages of the individual-loan level variables for approved loans at the zip code-year level.

Table 2. Exposure to Large Lenders and Approved Loan Amounts Since 2011

	Below \$100K	\$100K-\$200K	\$200K-\$417K	\$417K-\$700K	Above \$700K
Top20×Post_2011	-0.022	-0.022***	-0.012*	0.043***	0.241***
	(0.144)	(0.000)	(0.094)	(0.000)	(0.000)
Top20	-0.021	0.012	-0.003	-0.032**	-0.094***
	(0.199)	(0.117)	(0.762)	(0.012)	(0.009)
Log(Income)	0.098***	0.151***	0.214***	0.110***	0.159***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.011**	0.005**	0.001	0.001	-0.018***
	(0.036)	(0.033)	(0.661)	(0.765)	(0.000)
Asian	0.025***	0.009***	0.016***	-0.004	-0.009***
	(0.000)	(0.000)	(0.000)	(0.129)	(0.000)
Latino	0.011**	-0.002	-0.015***	-0.014***	-0.015***
	(0.033)	(0.426)	(0.000)	(0.000)	(0.000)
Avg-Black (county)	0.123*	-0.040	-0.049	-0.132***	0.204**
	(0.069)	(0.171)	(0.222)	(0.007)	(0.048)
Avg-Asian (county)	0.186*	-0.146***	0.011	0.112**	-0.061
	(0.086)	(0.007)	(0.849)	(0.020)	(0.366)
Avg-Latino (county)	0.266*	0.048	-0.220***	0.086	-0.144
	(0.065)	(0.374)	(0.000)	(0.455)	(0.207)
Median House Price	0.029	0.162***	0.121***	0.027**	0.137***
	(0.211)	(0.000)	(0.000)	(0.041)	(0.000)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.190	0.286

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the log loan size of the approved mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014, the percentage of loans originated by the Top 20 mortgage lenders, and a number of control variables. The control variables are: *Log(Income)*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and \$100,000; the second column reports results for loans between \$100,000 and \$200,000; the third column for loans between \$200,000 and \$417,000; the fourth column for loans between \$417,000 and \$700,000; and the fifth column for loans greater than \$700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table 3. Exposure to Large Lenders and Approved Loan Amounts Since 2011 - Robustness

Panel A. Top 5	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 5×Post_2011	0.019	-0.042***	-0.041***	0.054***	0.316***
	(0.460)	(0.000)	(0.000)	(0.000)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.190	0.288

Panel B. Top 100	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 100×Post_2011	-0.012	-0.025***	0.000	0.026**	0.218***
	(0.312)	(0.000)	(0.940)	(0.029)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.190	0.285

Panel C. Quantiles House Prices	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	-0.027*	-0.017***	-0.015**	0.031***	0.214***
	(0.065)	(0.006)	(0.045)	(0.007)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.191	0.287

Panel D. All Interactions	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	-0.021	-0.013**	-0.004	0.048***	0.095***
	(0.171)	(0.037)	(0.635)	(0.000)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.192	0.290

Panel E. Local shocks	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	0.014	-0.022***	-0.002	0.115***	0.261***
	(0.445)	(0.001)	(0.788)	(0.000)	(0.000)
State*Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.063	0.171	0.287	0.193	0.291

Panel F. Sand States	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011×Sand State	0.191***	0.001	-0.018	0.142***	0.173***
	(0.000)	(0.953)	(0.414)	(0.000)	(0.009)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922
Adjusted R^2	0.061	0.170	0.286	0.191	0.287

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Table 3. Exposure to Large Lenders and Approved Loan Amounts Since 2011 - Robustness

Panel G. Concentration Lending	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	-0.019	-0.025***	-0.018**	0.040***	0.150***
	(0.209)	(0.000)	(0.013)	(0.002)	(0.000)
Top 4 Mkt share×Post_2011	-0.015	0.032***	-0.003	0.021	0.161***
	(0.314)	(0.000)	(0.676)	(0.246)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	976,130	2,214,717	1,959,778	471,795	175,286
Adjusted R^2	0.065	0.160	0.271	0.171	0.275

Panel H. Exclude non-bank lenders	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	-0.029*	-0.033***	-0.023***	0.048***	0.255***
	(0.087)	(0.000)	(0.005)	(0.000)	(0.000)
Year and County F. E.	✓	✓	✓	✓	✓
Observations	976,130	2,214,717	1,959,778	471,795	175,286
Adjusted R^2	0.065	0.160	0.271	0.171	0.275

Panel I. Foreclosures	**Below \$100K**	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	-0.038**	-0.021***	-0.029***	0.040***	0.263***
	(0.023)	(0.006)	(0.000)	(0.000)	(0.000)
Year and County F.E.	✓	✓	✓	✓	✓
Observations	1,169,782	3,368,168	3,398,971	758,441	196,416
Adjusted R^2	0.055	0.169	0.292	0.197	0.298

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the log loan size of the approved mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014, the percentage of loans originated by the Top 20 mortgage lenders, and a number of control variables. The control variables are as in Table 2. The results are computed separately for loans in five size categories, defined as in Table 2. Standard errors are clustered at the county level. Panel A and Panel B use the share of top 5 and top 100 large lenders serving each county-year. Panel C controls for 3 quantiles of county-year house prices. Panel D allows for a full set of interactions of the controls with the *Post_2011* dummy. Panel E adds state-year fixed effects. Panel F compares the effect for counties in sand states (CA, NV, FL, AZ) and other states. Panel G adds the level and interaction of the measure of bank concentration by Scharfstein and Sunderam (2016). Panel H excludes loans originated by non-bank lenders. Panel I controls for the share of foreclosed properties in the zip code in which the loan is originated.

Table 4. Exposure to Large Lenders and Denied Loans Since 2011

	Below $100K	$100K-$200K	$200K-$417K	$417K-$700K	Above $700K
Top20×Post_2011	0.010	0.030***	0.030***	-0.046***	-0.229***
	(0.647)	(0.000)	(0.006)	(0.000)	(0.000)
Top20	0.000	-0.011	-0.016	0.009	0.093***
	(0.992)	(0.207)	(0.194)	(0.555)	(0.003)
Log(Income)	0.875***	0.892***	0.839***	0.919***	0.879***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.022***	-0.003	0.003	0.001	0.022***
	(0.000)	(0.227)	(0.364)	(0.589)	(0.001)
Asian	-0.072***	-0.010***	-0.015***	0.007**	0.015***
	(0.000)	(0.000)	(0.000)	(0.043)	(0.000)
Latino	-0.028***	0.002	0.020***	0.018***	0.012*
	(0.000)	(0.527)	(0.000)	(0.001)	(0.058)
Avg-Black (county)	-0.278***	0.088***	0.066**	-0.039	-0.072
	(0.001)	(0.002)	(0.014)	(0.266)	(0.341)
Avg-Asian (county)	-0.234*	0.085*	0.050	-0.146***	0.007
	(0.073)	(0.071)	(0.408)	(0.000)	(0.925)
Avg-Latino (county)	0.127	-0.093*	0.184***	-0.074	0.022
	(0.360)	(0.064)	(0.000)	(0.198)	(0.826)
Median House Price	0.120***	-0.141***	-0.105***	-0.042***	-0.119***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Observations	1,133,521	1,676,520	1,521,463	359,659	116,519
Adjusted R^2	0.540	0.859	0.844	0.918	0.926

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the log income-over-loan amounts of the denied mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014, the percentage of loans originated by the Top 20 mortgage lenders, and a number of control variables. The control variables are: *Log(Income)*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and $100,000; the second column reports results for loans between $100,000 and $200,000; the third column for loans between $200,000 and $417,000; the fourth column for loans between $417,000 and $700,000; and the fifth column for loans greater than $700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table 5. Validity of the Instrument: Balancing of Variables

	Quantile Large Banks in 2007				
	1	**2**	**3**	**4**	**St. Dev.**
Growth Loan Amount 2007-2010 (<$100k)	0.018	0.014	0.029	0.046	0.175
Growth Loan Amount 2007-2010 ($100k-$200k)	0.009	0.003	0.006	-0.007	0.074
Growth Loan Amount 2007-2010 ($200k-$417k)	-0.011	-0.008	-0.009	-0.025	0.096
Growth Loan Amount 2007-2010 ($417k-$700k)	-0.044	-0.039	-0.052	-0.045	0.109
Growth Loan Amount 2007-2010 (>$700k)	-0.015	-0.014	-0.006	-0.015	0.140
Avg. Black county	0.044	0.047	0.048	0.052	0.139
Avg. Asian county	0.005	0.007	0.014	0.022	0.076
Avg. Latino county	0.122	0.036	0.054	0.069	0.154
Share Foreclosed Properties	0.004	0.005	0.004	0.007	0.010
Share Middle-Class Households 2007	0.388	0.378	0.370	0.369	0.041
Share Middle-Class Households with a Mortgage 2007	0.535	0.520	0.500	0.475	0.086
Share County Income from Stock Dividends 2007	0.060	0.066	0.067	0.072	0.026
Share Workforce in Public Administration 2007	0.085	0.085	0.083	0.084	0.033

This table reports the sample mean of a set of variables within four quantiles of US counties, sorted by the share of county-level mortgage activity by large lenders. The last column reports the standard deviation of each variable to allow the assessment of the magnitude of the differences of mean point estimates across quantiles. Growth Loan Amount 2007-2010 is the sample mean county growth of the average loan amounts between 2007 and 2010. All other sample means are computed for observations throughout the sample period, that is, 2007-2014, because we need to verify the balancing of covariates both before and after 2011. Avg. Black county, Avg. Asian county, and Avg. Latino county are the mean share of Black, Asian, and Latino population in the county in the period 2007-2014. Share Foreclosed Houses is the ratio of units subject to foreclosure throughout the time period 2007-2014. Share Middle-Class Households 2007 is the share of households with an annual gross income between $25K and $75K in 2007. Share Middle-Class Households with a Mortgage 2007 is the share of households that had a mortgage outstanding in 2007 among the households with an annual gross income between $25K and $75K. Share County Income from Stock Dividends 2007 is the ratio between the gross income from stock dividends and interests over the overall gross income in 2007. Share Workforce in Public Administration 2007 is the share of households in the workforce that were employed in public administration jobs in 2007.

Table 6. Validity of the Instrument: Reduced Form Regressions

Panel A. Reduced Form

	Below \$100K	\$100K-\$200K	\$200K-\$417K	\$417K-\$700K	Above \$700K
Top20 in 2007×Post_2011	0.049** (0.011)	-0.025*** (0.001)	-0.027*** (0.007)	0.056*** (0.000)	0.242*** (0.000)
Controls	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
County fixed effects	✓	✓	✓	✓	✓
Observations	2,031,971	5,277,768	4,902,513	1,035,592	284,879
Adjusted R^2	0.061	0.170	0.286	0.190	0.287

Panel B. Reduced Form with Endogenous Regressor

	Below \$100K	\$100K-\$200K	\$200K-\$417K	\$417K-\$700K	Above \$700K
Top20×Post_2011	-0.028 (0.125)	0.001 (0.890)	0.004 (0.691)	0.088*** (0.000)	0.092 (0.120)
Top20 in 2007×Post_2011	0.070*** (0.001)	-0.026*** (0.004)	-0.030*** (0.008)	-0.022 (0.258)	0.157*** (0.004)
Controls	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Observations	2,031,971	5,277,768	4,902,513	1,035,592	284,879
Adjusted R^2	0.058	0.171	0.287	0.177	0.285

Panel A of this table reports regression coefficient estimates, and associated $p-$values, for the regression of the log loan amounts of the applied mortgages on the percentage of loans originated by the Top 20 mortgage lenders in 2007 interacted with a dummy that equals 1 for the years 2011-2014 and a number of control variables (reduced form specification). Panel B adds the interaction between the percentage of loans originated by the Top 20 mortgage lenders each year interacted with a dummy that equals 1 for the years 2011-2014 as a control variable. The control variables are: *Income*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and \$100,000; the second column reports results for loans between \$100,000 and \$200,000; the third column for loans between \$200,000 and \$417,000; the fourth column for loans between \$417,000 and \$700,000; and the fifth column for loans greater than \$700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table 7. Instrumental-Variable Results: Approved Loan Amounts - Large vs. Small Institutions

	Below $100K	**$100K-$200K**	**$200K-$417K**	**$417K-$700K**	**Above $700K**
Top 20×Post_2011	0.008	-0.044***	-0.026**	0.036***	0.338***
	(0.749)	(0.000)	(0.041)	(0.002)	(0.000)
Log(Income)	0.098***	0.151***	0.214***	0.110***	0.159***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.011**	0.005**	0.001	0.001	-0.018***
	(0.036)	(0.033)	(0.660)	(0.766)	(0.000)
Asian	0.025***	0.009***	0.016***	-0.004	-0.009***
	(0.000)	(0.000)	(0.000)	(0.129)	(0.000)
Latino	0.011**	-0.002	-0.015***	-0.014***	-0.015***
	(0.032)	(0.432)	(0.000)	(0.000)	(0.000)
Avg. Black county	0.143**	-0.055*	-0.054	-0.127**	0.207*
	(0.036)	(0.061)	(0.186)	(0.012)	(0.088)
Avg. Asian county	0.175*	-0.145***	0.009	0.113**	-0.048
	(0.099)	(0.006)	(0.870)	(0.013)	(0.625)
Avg. Latino county	0.261*	0.052	-0.222***	0.074	-0.163
	(0.070)	(0.330)	(0.000)	(0.526)	(0.174)
Log(Median House Price)	0.030	0.161***	0.121***	0.031**	0.149***
	(0.198)	(0.000)	(0.000)	(0.017)	(0.000)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Kleibergen-Paap F-Statistic	475.7	453.0	417.9	149.5	101.6
Observations	1,729,513	4,463,568	4,094,783	845,591	222,922

This table reports second-stage regression coefficient estimates, and associated $p-$values, for the regression of the log loan amounts of the applied mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014 and a number of control variables. The interaction is the endogenous variable, which is instrumented in the first stage with the percentage of loans originated by the Top 20 mortgage lenders in 2007 with a dummy that equals 1 for the years 2011-2014. The level of the percentage of loans originated is not included, because the variation in its instrument is absorbed by the county fixed effects. The control variables are: *Log(Income)*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and $100,000; the second column reports results for loans between $100,000 and $200,000; the third column for loans between $200,000 and $417,000; the fourth column for loans between $417,000 and $700,000; and the fifth column for loans greater than $700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table 8. Instrumental-Variable Results: Denied Applicants' Income over Loan Amount - Large vs. Small Institutions

	Below \$100K	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Top 20×Post_2011	0.041 (0.038)	0.040*** (0.013)	0.051*** (0.018)	-0.045*** (0.016)	-0.327*** (0.037)
Log(Income)	0.875*** (0.004)	0.892*** (0.001)	0.839*** (0.002)	0.919*** (0.003)	0.879*** (0.004)
Black	-0.022*** (0.006)	-0.003 (0.002)	0.003 (0.003)	0.002 (0.003)	0.022*** (0.007)
Asian	-0.072*** (0.006)	-0.010*** (0.001)	-0.015*** (0.003)	0.007** (0.003)	0.016*** (0.003)
Latino	-0.028*** (0.006)	0.002 (0.003)	0.020*** (0.002)	0.018*** (0.005)	0.013** (0.006)
Avg. Black county	-0.273*** (0.083)	0.092*** (0.028)	0.072** (0.029)	-0.039 (0.035)	-0.057 (0.083)
Avg. Asian county	-0.247* (0.132)	0.082* (0.046)	0.045 (0.058)	-0.146*** (0.039)	0.031 (0.080)
Avg. Latino county	0.133 (0.137)	-0.094* (0.051)	0.182*** (0.052)	-0.070 (0.057)	0.085 (0.101)
Log(Median House Price)	0.121*** (0.033)	-0.140*** (0.010)	-0.104*** (0.010)	-0.043*** (0.012)	-0.129*** (0.027)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Kleibergen-Paap F-Statistic	496.9	420.8	341.2	145.9	114.39
Observations	1,133,521	1,676,520	1,521,463	359,659	116,519

This table reports second-stage regression coefficient estimates, and associated $p-$values, for the regression of the log income-over-loan amounts of the denied mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014 and a number of control variables. The interaction is the endogenous variable, which is instrumented in the first stage with the percentage of loans originated by the Top 20 mortgage lenders in 2007 with a dummy that equals 1 for the years 2011-2014. The level of the percentage of loans originated is not included, because the variation in its instrument is absorbed by the county fixed effects. The control variables are: *Log(Income)*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and \$100,000; the second column reports results for loans between \$100,000 and \$200,000; the third column for loans between \$200,000 and \$417,000; the fourth column for loans between \$417,000 and \$700,000; and the fifth column for loans greater than \$700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table 9. Matched Counties Top/Bottom Quartile by Lending Activity Top 20 Institutions

	Below \$100K	**\$100K-\$200K**	**\$200K-\$417K**	**\$417K-\$700K**	**Above \$700K**
Treated×Post_2011	-0.055*	0.007	-0.106***	0.121***	0.168**
	(0.059)	(0.641)	(0.004)	(0.000)	(0.028)
Treated	0.042	-0.031**	0.096***	-0.042	-0.262***
	(0.175)	(0.035)	(0.004)	(0.314)	(0.000)
Log(Applicant Income)	0.115***	0.158***	0.196***	0.073***	0.131***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.006	-0.003	-0.008	0.001	-0.026**
	(0.497)	(0.717)	(0.436)	(0.893)	(0.049)
Asian	0.014*	0.009**	0.017**	-0.003	0.003
	(0.056)	(0.042)	(0.042)	(0.541)	(0.518)
Latino	0.003	-0.007**	-0.023**	-0.009**	-0.015
	(0.871)	(0.036)	(0.042)	(0.015)	(0.215)
Avg. Black county	0.318	0.087	0.036	0.442**	-0.295
	(0.127)	(0.240)	(0.691)	(0.034)	(0.302)
Avg. Asian county	-0.330	-0.603**	-0.029	-1.235***	1.469***
	(0.449)	(0.021)	(0.853)	(0.003)	(0.000)
Avg. Latino county	0.249	-0.084	-0.375***	-0.636***	0.335
	(0.277)	(0.425)	(0.000)	(0.005)	(0.461)
Log(Median House Price)	0.108***	0.146***	0.039*	-0.113**	0.067
	(0.001)	(0.000)	(0.068)	(0.014)	(0.366)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Observations	278,348	589,758	416,738	64,104	17,460
Adjusted R^2	0.066	0.173	0.262	0.127	0.228

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the log loan size of the approved mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 if the county is above the top quartile of the county-level distribution by large lenders origination (*Treated*), the interaction of *Treated* with a dummy that equals 1 for the years 2011-2014, and a number of control variables. The control variables are: *Log(Income)*, the log income of the applicant; dummy variables indicating whether the applicant is *Black*, *Asian*, and/or *Latino*; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The sample is limited to loans originated in counties that belong to a nearest-neighbor matched sample of counties based on demographics, which we construct following Scharfstein and Sunderam (2016) as described in Section 5.2. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and \$100,000; the second column reports results for loans between \$100,000 and \$200,000; the third column for loans between \$200,000 and \$417,000; the fourth column for loans between \$417,000 and \$700,000; and the fifth column for loans greater than \$700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Online Appendix:
Regressive Mortgage Credit Redistribution in the Post-crisis Era

Francesco D'Acunto and Alberto G. Rossi

Not for Publication

A.1 Zip-Code-Level Analysis

In this section, we run our analysis at the zip-code level instead of at the individual-loan level. We have two motivations for a zip-code-level analysis. First, using a level of aggregation allows us to study not only the amounts of the originated loans (intensive margin of lending), but also the number of loans originated within each mortgage-size group (extensive margin of lending). Lenders are likely to adapt their originating behavior along both margins, because they can control both. Second, most existing papers that study mortgage-origination behavior before and during the financial crisis run their analysis at the zip-code level. We run most of our analysis at the loan level, because this level allows us to control for a set of important characteristics of households, which are likely to be correlated with loan-level characteristics and might not be captured by aggregate averages. For instance, Adelino, Schoar, and Severino (2016) write, "it is households, and not zip codes, which take on mortgage loans." At the same time, we want to ensure our results can be easily compared to the results in earlier literature.

To run the analysis at the zip-code-level, we follow Adelino, Schoar, and Severino (2016) and Mian and Sufi (2009) and compute the zip-code level growth of loan amounts and loan counts within each loan-size group from 2010 to 2014. These variables capture the growth in the intensive margin and extensive margin of originated loans from 2011 onwards. Similarly, we compute the zip-code-level growth of the covariates we used in the individual-level analysis from 2010 to 2014. We then compare the growth of the loan amount and number of loans across zip codes based on the share of large lenders in 2007 in the counties in which the zip codes lie. We use the share of large lenders in 2007, before the financial crisis, because we use this year in the instrumental-variable analysis. The results do not change if we use the share in 2010.

A contentious point in the interpretation of mortgage-origination dynamics before the financial crisis is whether the analysis should use geographically unrestricted variation in the outcome and controls, or should only exploit variation within counties. In our case, the variation cannot be restricted at the county level, because our main covariate – the share of large lenders in counties in 2007 – does not vary at the county level. We will therefore propose our results when using unrestricted geographic variation and within-state variation.

We estimate the following specification with OLS:

$$\begin{aligned}\Delta Outcome_{2010\rightarrow 2014,z} &= \alpha + \beta\ Top\_20_{k,2007} \times Post\_2011_t \\ &\quad + \Delta X'_{2010\rightarrow 2014,z}\delta + \Delta D'_{2010\rightarrow 2014,z}\phi + \eta_s + \epsilon_{z,k},\end{aligned} \tag{A.1}$$

where $\Delta Outcome_{2010\rightarrow 2014,z}$ is the growth of the outcome variable – average loan amount or loan number – from 2010 to 2014 at the zip-code-level z, $Top\_20_{k,2007}$ is the share of mortgage activity by the top 20 national financial institutions in year 2007 and county k, ΔX and ΔD are the change in the set of observables at the individual and zip-code level; η_s is a set of state fixed effects. Note the variable $Post\_2011_{k,2007}$ does not vary over time. Restricting the variation within years using year fixed effects absorbs completely the variation in the level of $Top\_20_{k,2007}$ across counties, which is why the level of the variable does not appear in the RHS of Equation A.1.

Table A.1 reports the results for estimating Equation A.1. In all specifications, we cluster the standard errors at the county level to allow for correlation of the residuals of unknown form within counties and over time. In Panel A and Panel B, the outcome variable is the growth of the average loan amount at the zip-code level from 2010 to 2014. We omit reporting the coefficients associated with each of the control variables. In both panels, zip codes in counties with a higher share of large lenders in 2007 experience less growth in loan amounts for loans between \$100k and \$200k, and more growth in loan amounts above the non-conforming limit of \$417k. The results are similar if we restrict the variation within states, although statistical significance is more sparse.

In Panel C and Panel D of Table A.1, the outcome variable is the growth in the number of loans at the zip-code level from 2010 to 2014. In Panel C, we find zip codes in counties with a higher share of large lenders have less growth in the number of loans both below and above the conforming loan limit. This result is consistent with the fact that financial institutions cut the loans below the conforming loan limit not only along the intensive margin, but also along the extensive margin. At the same time, the number of loans just above the conforming loan limit also decreased, and because the average amount for the same group of loans increased, banks might have cut on the loans closer to the limit, compared to the loans well above the limit. When we restrict the variation within states, the results are qualitatively similar, although statistical significance is more sparse. In particular, the negative growth in the number of loans just above the conforming loan limit is only robust to allowing for variation across zip codes of any state.

Overall, the zip-code-level analysis confirms our baseline results at the individual-loan level for the intensive margin of lending. They suggest loans decreased also along the extensive margin, both below and above the conforming loan limit.

A.2 Execution and Timing of the Economic Effects of Dodd-Frank's Mortgage Provisions

President Obama signed Dodd-Frank into law on July 21, 2010. Most provisions in Title XIV (Mortgage Reform and Anti-Predatory Lending Act) were not self-executing. According to Section 1400, the provisions should have taken effect on the earliest date between the "date on which the final regulations implementing such section, or provision, take effect," or "18 months after the designated transfer date," in which the transfer date refers to the date on which the Federal Reserve was set to transfer its supervisory and regulatory powers based on the *Truth in Lending Act* to the Bureau of Consumer Financial Protection. This date was set to July 21, 2011, and hence – at the time of signing into law of Dodd-Frank – the latest possible date by which financial institutions had to comply with the provisions was January 21, 2013.

Whereas we know the last date by which financial institutions had to comply with the Dodd-Frank provisions, we do not know when each institution started to invest resources and effort towards this goal. On the one hand, it would take months or years to set up the training systems and hire the personnel the banks would have needed to comply with Dodd-Frank, especially for the larger banks. On the other hand, there was uncertainty about the actual date on which the provisions of Dodd-Frank would be executable, because the CFPB could have produced the required regulations before the final date of January 21, 2013. For these reasons, the regulation affected the fixed and marginal costs of originating loans well before it was executed. Because banks faced this increase in actual and projected costs well before 2013, their optimization implied a shift of their lending towards loans with a higher expected return, that is, larger loans.

Even though we do not observe directly the date at which all banks started their compliance process, we do know the exact timing of the compliance process for 14 servicers and the parent companies of 12 of them, which were deemed to be systemically important by the Federal Reserve System in November 2010. In April 2011, the Federal Reserve System disclosed a set of enforcement actions that imposed the immediate compliance to new standards for mortgage originations that were "substantially similar" to the provisions approved in Dodd-Frank (Braunstein, 2011). These banks, therefore, had no choice but to start complying with the new provisions immediately after April 2011. The set of 12 parent companies included the following institutions: Ally Bank/GMAC, Bank of America, Citibank, Everbank, HSBC, JPMorgan Chase & Co., MetLife, OneWest, PNC, SunTrust, U.S. Bank, and Wells Fargo (see FRB, 2011, Footnote 1).

One might wonder whether financial institutions indeed started to face the costs of preparing their compliance to the provisions in Dodd-Frank starting in 2011. The enforcement actions are key in this respect, because the confidential filings of the institutions to the Federal Reserve Bank of New York describe these costs explicitly.

On July 12, 2011, Citibank's CEO Michael Corbat stated that "Citi [...] has dedicated significant management and financial resources to these efforts and ongoing business needs. Incremental expenses associated with these actions are estimated to be in excess of $(confidential figure) for 2011." (see Citibank, 2011, Page 3).

On the same date, Bank of America stated that "We recognize the significant effort, time, and resources necessary to implement the Plan and to verify its consistency and rigor once implemented." (see BankofAmerica, 2011). On December 8, 2011, JPMorgan Chase & Co. stated that "Home Lending Compliance has been actively adding resources and upgrading talent and, in mid-2010, a dedicated position of Head of Compliance for Home Lending, reporting to a newly-created Head of Compliance Retail Financial Services, was established and the role filled." (see JPMorganChase, 2011).

On December 9, 2011, US Bancorp stated that "The Bank is fully committed to securing all necessary resources to respond to the Orders in an effective and timely manner." (see USBancorp, 2011).

On December 16, 2011, SunTrust stated that "SunTrust's consent order became effective on April 13, 2011. In anticipation of the Order, SunTrust created a formal program organization, comprised of individual working teams for each section of the Consent Order." (see SunTrust, 2011).

On December 23, 2011, Wells Fargo stated that "we have expanded our Dodd-Frank Program Office [...] Although the nominal date for this office to be operating is 1/1/2012, in fact personnel are in place and managing efforts [...]" (see WellsFargo, 2011).

All these documents provide *prima facie* evidence that financial institutions started to invest resources, and hence faced additional fixed and marginal costs to originate loans, since early 2011.

A.3 Additional Data Description and Alternative Explanations

In this section, we describe the construction of the variables that proxy for counties' exposure to wealth polarization and other demand shocks after the financial crisis.

Our source of data for information on household-level stock market participation, mortgage holding, and occupation is the American Community Survey (ACS). The ACS is a yearly survey-based repeated cross section that obtains demographic and economic information on 1% of the US population. The stratification is performed in two stages. The US Census Bureau first selects a set of counties among which they then select the individual households. All members of the selected households have to respond to the survey.

The ACS collects detailed information on household-members' age, race, ethnicity, income, work-force status and occupation, as well as a set of economic dimensions, including the overall gross income and its components based on IRS classification. These components include: (i) income from wages and salaries; (ii) income from business and farms; (iii) income from interest, dividend, and rents; (iv) income from retirement plans.

The micro-data files are available for download from the Integrated Public Use Microdata Series (IPUMS) from the following website: https://usa.ipums.org/usa-action/variables/group. We downloaded the ACS three-year cross section for the year 2005-2007, which we denote as the 2007 sample. This sample period includes the three years before the collapse of Lehman Brothers and the start of the 2008-2009 financial crisis. The raw sample includes 8,842,783 individual observations. To run our analysis, we perform a set of steps to select the population we are interested in. In the first step, we exclude observations which only report their state of residence, but not the county of residence. In a second step, we exclude all observations the ACS categorize as not applicable in terms of employment status, and for which individual income is missing. In a third step, we exclude individuals for which the total family income is missing.

We construct the county-level measures as follows: (i) *share of middle-class households in 2007*, is the share of household that have an overall gross income between \$25K and \$75K in 2007; (ii) *share of middle class households with mortgages in 2007*, is the share of middle-class households that own their primary residence and hold at least one mortgage; (iii) *exposure to the stock market in 2007*, is the ratio between the sum of income from interest, dividends, and rent over the sum of the gross income of households computed at the county level; (iv) *share of workers in the public administration*, is the share of individuals that declare to be part of the workforce, and whose occupation is in the 1-digit industry classification "9" in the US Census industry definition.

In Table A.2, we split the counties in terciles, and report estimates of our baseline specification for the bottom and the top terciles.

Table A.1. Exposure to Large Lenders and Approved Loan Amounts Since 2011
Zip-Code Level Analysis

Panel A. Loan Amount Without State Fixed Effects

	Below $100K	$100K-$200K	$200K-$417K	$417K-$700K	Above $700K
Share Activity Top 20 in 2007	-0.013	-0.032***	0.009	0.074***	0.209***
	(0.704)	(0.007)	(0.657)	(0.003)	(0.000)
State Fixed Effects	✗	✗	✗	✗	✗
Growth Controls 2007-2010	✓	✓	✓	✓	✓
Observations	7,045	7,469	7,374	4,890	2,711
Adjusted R^2	0.001	0.086	0.061	0.012	0.030

Panel B. Loan Amount With State Fixed Effects

	Below $100K	$100K-$200K	$200K-$417K	$417K-$700K	Above $700K
Share Activity Top 20 in 2007	0.026	-0.042***	-0.030	0.056	0.315***
	(0.479)	(0.001)	(0.108)	(0.169)	(0.000)
State Fixed Effects	✓	✓	✓	✓	✓
Growth Controls 2007-2010	✓	✓	✓	✓	✓
Observations	7,045	7,469	7,374	4,890	2,711
Adjusted R^2	0.010	0.096	0.086	0.040	0.090

Panel C. Number of Loans Without State Fixed Effects

	Below $100K	$100K-$200K	$200K-$417K	$417K-$700K	Above $700K
Share Activity Top 20 in 2007	0.277*	-0.853***	-1.728***	-1.454***	0.335
	(0.079)	(0.000)	(0.000)	(0.001)	(0.435)
State Fixed Effects	✗	✗	✗	✗	✗
Growth Controls 2007-2010	✓	✓	✓	✓	✓
Observations	7,045	7,469	7,374	4,890	2,711
Adjusted R^2	0.001	0.086	0.061	0.012	0.030

Panel D. Number of Loans With State Fixed Effects

	Below $100K	$100K-$200K	$200K-$417K	$417K-$700K	Above $700K
Share Activity Top 20 in 2007	0.447***	-0.157	-1.247***	0.501	0.533
	(0.001)	(0.164)	(0.000)	(0.280)	(0.358)
State Fixed Effects	✓	✓	✓	✓	✓
Growth controls 2007-2010	✓	✓	✓	✓	✓
Observations	7,045	7,469	7,374	4,890	2,711
Adjusted R^2	0.104	0.139	0.083	0.112	0.053

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the growth of average loan amount in zip codes from 2010 to 2014 (Panel A and Panel B), or the growth of the number of loans originated in zip codes from 2010 to 2014 (Panel C and Panel D), on the percentage of loans originated by the Top 20 mortgage lenders in 2007 in the counties where the zip codes lie, plus the growth of a number of control variables. The control variables, whose associated coefficients are not reported due to space constraints, are: *Income*, the log income of the applicant; the average number of applicants for a given year that are black, *Avg-Black (county)*, asian, *Avg-Asian (county)*, and/or latino, *Avg-Latino (county)*; and *Median House Price*, the log median house price in a given county for a given year. The results are computed separately for loans in five size categories. The first set of results are reported in the first column and are associated with loans between zero and $100,000; the second column reports results for loans between $100,000 and $200,000; the third column for loans between $200,000 and $417,000; the fourth column for loans between $417,000 and $700,000; and the fifth column for loans greater than $700,000. All specifications include year fixed effects and county fixed effects. Standard errors are clustered at the county level.

Table A.2. Post-Crisis Wealth Polarization and Exposure to Financial Crisis

	Below \$100K	\$100K-\$200K	\$200K-\$417K	\$417K-\$700K	Above \$700K
			Panel A. Share Middle-Class Households in 2007		
			Bottom Third Counties		
Top20×Post_2011	-0.019	-0.013	-0.044**	0.048***	0.231***
	(0.733)	(0.500)	(0.010)	(0.005)	(0.000)
			Top Third Counties		
Top20×Post_2011	-0.014	-0.018	-0.010	0.022	-0.102**
	(0.567)	(0.277)	(0.388)	(0.336)	(0.015)
			Panel B. Share Middle-Class Households with Mortgages in 2007		
			Bottom Third Counties		
Top20×Post_2011	-0.044	-0.058**	-0.034**	0.045**	0.208***
	(0.237)	(0.039)	(0.021)	(0.023)	(0.000)
			Top Third Counties		
Top20×Post_2011	-0.034	-0.013	-0.016	0.046	0.043
	(0.153)	(0.444)	(0.530)	(0.186)	(0.606)
			Panel C. Exposure to Stock Market in 2007		
			Bottom Third Counties		
Top20×Post_2011	-0.057*	-0.016	-0.003	0.004	0.205**
	(0.066)	(0.284)	(0.832)	(0.848)	(0.015)
			Top Third Counties		
Top20×Post_2011	0.038	-0.003	-0.005	0.062***	0.218***
	(0.264)	(0.846)	(0.807)	(0.001)	(0.000)
			Panel D. Share Workforce in Public Administration in 2007		
			Bottom Third Counties		
Top20×Post_2011	-0.026	-0.040*	-0.014	0.030**	0.310***
	(0.395)	(0.064)	(0.315)	(0.035)	(0.000)
			Top Third Counties		
Top20×Post_2011	-0.048	-0.025	0.022	0.005	0.142**
	(0.204)	(0.188)	(0.197)	(0.799)	(0.035)

This table reports regression coefficient estimates, and associated $p-$values, for the regression of the log loan size of the approved mortgages on the percentage of loans originated by the Top 20 mortgage lenders interacted with a dummy that equals 1 for the years 2011-2014, the percentage of loans originated by the Top 20 mortgage lenders, and a number of control variables. The control variables are as in Table 2. The results are computed separately for loans in five size categories, defined as in Table 2. Standard errors are clustered at the county level. In each Panel, results are reported separately for estimating the specification using approved loans in counties below the bottom tercile and above the top tercile of counties sorted by different variables. Panel A sorts counties based on the share of households with a gross income between \$25K and \$75K in 2007. Panel B sorts counties based on the share of households that had a mortgage outstanding in 2007 among the households with an annual gross income between \$25K and \$75K. Panel C sorts counties by the ratio between the gross income from stock dividends and interests over the overall gross income in 2007. Panel D sorts counties based on the share of households in the workforce that were employed in public administration jobs in 2007.