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REGULATING HOUSEHOLD LEVERAGE

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Abstract

This paper studies how credit markets respond to policy constraints on household leverage. Exploiting a sharp policy-induced discontinuity in the cost of originating certain high-leverage mortgages, we study how the Dodd-Frank "Ability-to-Repay" rule affected the price and availability of credit in the U.S. mortgage market. Our estimates show that the policy had only moderate effects on prices, increasing interest rates on affected loans by 10-15 basis points. The effect on quantities, however, was significantly larger; we estimate that the policy eliminated 15 percent of the affected market completely and reduced leverage for another 20 percent of remaining borrowers. This reduction in quantities is much greater than would be implied by plausible demand elasticities and suggests that lenders responded to the policy primarily by rationing credit. Finally, while the policy succeeded in reducing leverage, our estimates suggest this effect would have only slightly reduced aggregate default rates during the housing crisis.

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I Introduction

Household leverage played a central role during the global financial crisis of 2007–2009. In the U.S., large increases in household debt both facilitated the run-up in house prices that eventually led to the crisis and contributed to the drop in consumer spending that slowed the recovery from the Great Recession (Eggertsson and Krugman, 2012; Mian and Sufi, 2011; Mian et al., 2013; Mian and Sufi, 2014). As a result, the U.S. policy response to the crisis included many measures directly targeting household leverage. Some of these measures were *ex post*, intended to mitigate the immediate fallout from the crisis by restructuring existing debt contracts or providing households with temporary debt payment relief. Other policies had a more *ex ante*, macroprudential focus and sought to decrease the likelihood of future crises by curtailing risky lending practices and preventing households from becoming highly leveraged.

While there is a large empirical literature that has examined the effects of many of the *ex post* policies aimed at restructuring household debt (Agarwal et al., 2012; Mayer et al., 2014; Agarwal et al., 2015a; Ganong and Noel, 2017), there has been relatively little empirical work evaluating *ex ante* policies that look to regulate household leverage going forward. This is despite both the increasing global adoption of such policies (Cerutti et al., 2015), and the growing theoretical literature suggesting that these policies may help to avoid inefficient aggregate losses from financial shocks that occur when households are highly leveraged (Bianchi and Mendoza, 2010; Farhi and Werning, 2013; Jeanne and Korinek, 2013; Korinek and Simsek, 2016).

In this paper, we estimate the impact of a central U.S. policy targeting household leverage in the mortgage market. As part of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010, Congress mandated that lenders evaluate a borrower's "ability-to-repay" (ATR) when originating a mortgage. In January 2014, the Consumer Financial Protection Bureau (CFPB) implemented this requirement, which made lenders liable for damages to borrowers if they originated a mortgage that violated the ATR rule. To simplify compliance, Congress created a class of mortgages called "qualified mortgages" (QM) that automatically satisfy the ATR rule and protect lenders from legal liability. Among other conditions, this class of mortgages is required to have a debt-to-income ratio (DTI) no greater than 43 percent.[1] By reducing the fraction of income dedicated to servicing a mortgage, this requirement is intended to both reduce liquidity-driven defaults and limit the extent to which households may need to cut consumption when facing potential default. We exploit the sharp timing and discontinuous nature of this policy to study its effects on the price, quantity, and performance of mortgage credit in order to evaluate how effec-

[1]DTI refers to the ratio of monthly debt payments to income. The numerator is calculated as the total monthly payments for the loan being originated as well as all other obligations, including alimony, child support, non-mortgage debts, and any other mortgage-related expenses such as property taxes and condominium fees. The denominator, monthly income, is gross and calculated as any regular payment to the consumer that has been documented.

tively this regulation reduces household leverage and its potential implications for the stability of the mortgage market.

Our empirical analysis is aided by the fact that the CFPB temporarily exempted large portions of the mortgage market from the rule. In particular, all loans eligible to be purchased by the Government Sponsored Enterprises (GSEs), Fannie Mae and Freddie Mac, are currently exempt from the 43 percent DTI requirement. In practice, this means that it was primarily jumbo mortgages with a DTI greater than 43 percent that lost legal protection.[2] Thus we are able to identify the effects of the policy not only by comparing outcomes for high- versus low-DTI loans, but also by comparing jumbo loans to conforming (non-jumbo) loans.[3]

To identify the effect of the policy on the price of credit, we use a difference-in-differences research design that compares the change in interest rates for jumbo loans with DTIs above and below the QM-threshold before and after the policy was implemented. Our baseline estimates imply that lenders charge a premium of 10–15 basis points per year to originate loans above the DTI cutoff. This represents an increase in the cost of credit of roughly 2.5–3 percent relative to the average interest rate among high-DTI jumbo loans in the pre-period and is nearly identical to the CFPB's own prediction of the effect that the policy would have on lenders' costs of origination (Consumer Financial Protection Bureau, 2013). Assuming borrowers do not refinance, this premium works out to an additional \$13,000–20,000 in interest over the full life of the loan for the average affected loan in our sample. If we assume that borrowers refinance into a QM loan after five years, the expected interest premium ranges between \$1,700–2,600.

The key identification assumption underlying this research design is that changes in interest rates for jumbo loans with DTIs above and below the QM-threshold would have evolved in parallel in the absence of the policy. We provide three pieces of evidence in support of this assumption. First, we show direct graphical evidence that the raw average interest rates for high- and low-DTI jumbo loans moved together prior to the implementation of ATR/QM and only began to diverge afterwards. Second, we estimate a flexible version of the basic difference-in-differences specification that allows the effect to vary freely with the borrower's DTI and reveals that the increase in interest rates for high-DTI jumbo loans is driven almost entirely by a level shift in rates that occurs at a DTI of *exactly* 43 percent. Third, we also estimate a triple-difference model that includes conforming loans as an additional control group. Because only jumbo loans are required to meet the 43 percent DTI requirement, differential changes in interest rates among high- and low-DTI conforming loans can be used as a proxy for the counterfactual changes that may have occurred

[2] Jumbo loans are mortgages larger than the conforming loan limits that determine eligibility for purchase by Fannie Mae and Feddie Mac.

[3] While there are other reasons that a loan may not be GSE-eligible, in this paper we focus on loan size as the primary determinant of eligibility and will thus use the terms "conforming" and "non-jumbo" interchangeably throughout.

in the jumbo market in the absence of the policy. Estimates from this triple difference specification are nearly identical to the baseline difference-in-differences results. Together, these three tests provide strong evidence that our results are measuring the direct effect of the ATR/QM regulation.

We argue that the increase in interest rates for high-DTI jumbo loans primarily reflects the pass-through to borrowers of lenders' increased origination costs due to the ATR/QM rule. However, an alternative interpretation is that our results reflect borrower selection. If some borrowers are induced by the interest rate premium to either forgo getting a loan or to reduce the size of their loans to get their DTI below the QM threshold, then part of the post-policy interest rate differential between high- and low-DTI loans may reflect differences in the composition of borrowers across DTIs. We rule this concern out in two ways. First, we leverage the richness of our loan-level data to flexibly control for the complete set of observables that are typically used by lenders to price mortgages. Estimates from these specifications are no different from the baseline results, suggesting that our results cannot be explained by changes in the observable price-relevant characteristics of borrowers. Second, we also show that the shape of the relationship between DTI and the estimated interest rate premium strongly suggests that interest rates are not responding to selection on unobservables. If the interest rate premium for non-QM loans were driven by borrower selection, then we would expect that premium to be higher at DTIs that are just above 43 percent as it is easier for borrowers in that region of the distribution to get below the QM threshold. However, when we allow the effect of the policy to vary non-parametrically in the borrower's DTI, the estimated premium is nearly uniform across all DTIs above the 43 percent cutoff.

While these results suggest that the interest rate premium is not driven by borrower selection, this does not mean that the allocation of credit across the DTI distribution was unaffected by the policy. Some borrowers may indeed have chosen to respond to the policy either on the intensive margin by lowering their DTIs or on the extensive margin by forgoing a mortgage altogether. Similarly, in addition to increasing the price that they charge for non-QM loans, some lenders may have responded to the policy by choosing to originate fewer non-QM loans or exiting the non-QM market entirely. Thus, both the number and the size of mortgages could fall as a result of the policy.

We measure these effects of ATR/QM on the quantity of mortgage credit by comparing the actual post-policy distribution of loans across DTIs to a counterfactual distribution that assumes that there was no change in policy. Our approach is motivated by the large literature in public finance studying "bunching" behavior in the presence of non-linear budget constraints (see Kleven (2016) for a review). Intuitively, the intensive margin effect of the policy on the allocation of credit across the DTI distribution can be measured by the number of loans bunching at and just below the QM threshold. Similarly, the extensive margin effect of the policy on the total number of loans can be measured by taking the difference between the number of missing loans above the threshold and the number that were shifted to just below the threshold.

Measuring these quantities requires that we have an accurate estimate of the counterfactual DTI distribution. While the existing literature has developed standard approaches for estimating this type of counterfactual from a single cross-section of data, those approaches are typically not well-suited for measuring extensive margin responses and often require the assumption that the counterfactual distribution is smooth (Kleven and Waseem, 2013; Chetty et al., 2011). Given our explicit interest in the extensive margin effects of the policy and institutional features of the mortgage market that lead to non-continuous DTI distributions, the existing approaches are not ideal. We therefore develop a new approach to estimating the counterfactual that leverages both the time-series dimension of our data as well as the fact that the conforming market was exempt from the regulation. We construct the post-ATR/QM counterfactual jumbo DTI distribution by adjusting the pre-period jumbo distribution based on observed changes to the distribution in the unaffected conforming market. We validate the assumptions underlying this approach by showing that it is able to generate accurate and unbiased estimates of empirical DTI distributions in placebo years for which there was no policy change.

Using this approach, we estimate that the policy eliminated 15 percent of the high-DTI jumbo market in the year that it was implemented and that an additional 20 percent of high-DTI jumbo loans were shifted from above to below the 43 percent threshold. These lost and shifted loans constitute 2 and 2.7 percent of the $28.2 billion jumbo market in 2014, respectively. Our estimate of the extensive margin effect therefore suggests that the policy reduced the total amount of mortgage credit by at least $600 million in the year it was implemented. While this quantity is fairly small, this reflects the fact that the policy currently only affects a small fraction of the overall market. However, the exemption for conforming loans is set to expire in 2021 or when the GSEs exit conservatorship, at which time the policy will affect a much larger portion of the mortgage market. A naive extrapolation of our estimate to the entire $600 billion home purchase mortgage market, both jumbo and conforming, would imply a reduction of roughly $12 billion in total mortgage originations.[4] Our analysis, therefore, not only serves to provide some of the first empirical evidence on the impacts of an important *ex ante* regulation of household leverage in the U.S. mortgage market, but may also be directly informative about near-term anticipated policy changes.

Recent estimates of the elasticity of mortgage demand with respect to interest rates suggest

[4] These estimates of the total dollar volume of new purchase mortgage originations are based on aggregate statistics calculated using the nationally representative Home Mortgage Disclosure Act (HMDA) data and reported by Bhutta et al. (2015).

that our observed quantity response is an order of magnitude larger than the demand-side response that would be expected given the 10–15 basis point premium lenders charge for non-QM loans (Best et al., 2015; DeFusco and Paciorek, 2017). We view this as clear evidence that, instead of pricing the regulatory risk of originating a non-QM loan, many lenders responded to the policy by rationing credit and dropping out of the non-QM market entirely. While this effect may dissipate over time as lenders monitor the non-QM market, our results highlight a potentially important supply-side response to regulations that seek to reduce household leverage by imposing additional costs on lenders. The substantial rationing response by lenders suggests that regulatory risk-aversion, nominal rigidities, or informational frictions may critically affect the implementation of similar macroprudential policies targeting household leverage in other contexts.

These results show that the ATR/QM rule had a significant economic impact on the credit market by increasing the price and reducing the quantity of high-DTI mortgages, which was a primary aim of policy. However, an additional goal of the policy was to reduce liquidity-driven mortgage defaults, thereby improving the stability of the mortgage market and the financial sector. To shed some light on how well the policy achieves this objective, we turn to data on historical mortgage performance during the housing crisis. Specifically, we ask whether the shifts in the DTI distribution caused by the policy would have significantly affected the aggregate default rate among cohorts of loans originated during the run-up to the financial crisis.

For the policy to have any first-order effect on aggregate default rates, it is necessary for high-DTI loans to actually have worse performance than low-DTI loans. To check this, we nonparametrically estimate the relationship between DTI and default probability in a sample of loans originated between 2005 and 2008. While higher DTIs are generally associated with increased default probabilities, we find little evidence that jumbo loans in the region above the 43 percent cutoff perform worse than those just below it. This suggests that the current implementation of the policy would not have generated meaningful performance improvements had it been in effect during the run-up to the crisis. However, when we expand the sample to include all mortgages, we do find significant differences in performance between high- and low-DTI loans. Therefore, it is possible that a full implementation of the policy would have led to lower aggregate default rates during this period. Holding the historical relationship between DTI and default constant and extrapolating our estimate of the effect of the policy on the DTI distribution to the entire market, we estimate that the policy would have reduced the five-year default rate by about 0.2 percentage points for loans originated in 2007 and 2008, with smaller effects for loans originated in 2005 and 2006. Given that the 2007 cohort of loans experienced default rates as high as 24 percent after five years, we view these performance improvements as relatively small. While the policy might be able to induce larger improvements in performance if the DTI threshold were set lower, our estimates of the effects on prices and quantities suggest the resulting impact on the availability of mortgage credit might be relatively large. These calculations require us to extrapolate our quantity estimates out of sample and also ignore other potentially important features of the policy that may have had larger effects on performance, such as the exclusion of negatively amortizing loans from QM-status. Notwithstanding this limitation, our results suggest that even though policies restricting borrower DTI can significantly affect market prices and quantities, DTI itself may be a relatively ineffective way to improve mortgage market performance.

Our paper contributes to a large literature evaluating the effects of various policy responses to the financial crisis and Great Recession. Many papers in this literature have focused on *ex post* policies that were primarily aimed at the immediate problems generated by the crisis. Such policies were numerous and included direct fiscal stimulus (Mian and Sufi, 2012; Chodorow-Reich et al., 2012; Berger et al., 2016), large extensions to unemployment benefits (Rothstein, 2011; Hagedorn et al., 2013; Chodorow-Reich and Karabarbounis, 2016), unconventional monetary policy (Williams, 2011; Krishnamurthy and Vissing-Jorgensen, 2011; Di Maggio et al., 2016), and significant efforts to shore up household balance sheets through debt restructuring and mortgage payment relief (Agarwal et al., 2012; Eberly and Krishnamurthy, 2014; Mayer et al., 2014; Agarwal et al., 2015a; Ganong and Noel, 2017). The policy we study differs critically in that its primary focus is on the *ex ante* prevention of a future crisis by limiting household leverage.

Bhutta and Ringo (2015) also provide some early evidence on the effects of the ATR/QM rule using confidential HMDA data. They use alternative sources of identification and generally estimate a smaller response than we do. However, they do not provide estimates of the price response and their data prevents them from being able to evaluate the effect of the DTI threshold as we do here. In related work, Gissler et al. (2016) and D'Acunto and Rossi (2017) provide evidence that lenders may have also changed their origination behavior in anticipation of the regulatory change in the years immediately before the implementation of the ATR/QM rule but do not study the direct effects of the regulation itself. Johnson (2016) provides evidence on the effects of the verified DTI requirement on self-employed borrowers and entrepreneurship.

A major part of the justification for the ATR/QM rule was to prevent lenders from making loans that they cannot reasonably expect borrowers to be able to repay. As such, our analysis is also related to the literature on the broader regulation of consumer financial products and consumer protection in household finance (Campbell et al., 2011; Posner and Weyl, 2013; Jambulapati and Stavins, 2014; Agarwal et al., 2015b). An important distinction is that the DTI restriction we study also has the potential benefit of making mortgage performance and household consumption more robust to income shocks, which may lead to benefits at the macroeconomic level as well.

The remainder of this paper proceeds as follows. In Section II we provide details on the institutional background surrounding the ATR/QM rule. Section III describes our data and sample selection criteria. In Section IV, we discuss the research design we use to identify the effects of the policy on the cost of credit and present our primary results on interest rates. Section V presents the results and research design we use to study the effect of the policy on the quantity of mortgage credit. Section VI provides estimates of the potential effects of the policy on mortgage performance. Section VII concludes.

II Institutional Background

In response to the 2007–2008 financial crisis, Congress passed the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010. This is broad legislation directed at both reducing systemic risk and preventing predatory lending.

In line with these goals, the Dodd-Frank Act requires mortgage lenders to verify that borrowers will be able to afford all scheduled payments before extending most types of closed-end residential mortgage loans.[5] The CFPB was charged with implementing this "ability-to-repay" (ATR) rule, which took effect January 10, 2014. The final language of the ATR rule requires that lenders make a "reasonable, good faith" determination when originating a mortgage that the borrower has a "reasonable" ability to repay the loan (Consumer Financial Protection Bureau, 2013). There are a number of ways in which lenders may comply with the ATR rule. The "General ATR Option" requires lenders to verify and consider eight factors in their underwriting process and do so using "reasonably reliable" records from third parties.[6] So long as these criteria are satisfied, lenders may originate loans with any features. Loans with balloon payments, negative amortization, or interest-only options may be in compliance with the ATR rule so long as the lender has made the requisite effort to establish the borrower's ability to repay.

In addition to establishing the ATR rule, the Dodd-Frank Act created the "qualified mortgage" (QM) category of lower-risk loans which are automatically presumed to comply with ATR requirements.[7] These loans provide a legal "safe harbor" to the loan originator in the event of any legal action brought by the borrower. In effect, the QM category provides lenders with an

[5] The rule is similar to an earlier rule enacted by the Federal Reserve in 2008, effective since 2009, that required lenders to verify ability-to-repay on "higher-priced," typically subprime loans.

[6] Specifically, the lender must verify (1) current and reasonable expectations of future income necessary for loan repayment, (2) employment status if applicable, (3) monthly mortgage payment on the loan, (4) monthly payments on any simultaneous loans, (5) monthly payments for taxes, insurance, and other "certain" costs related to the property, (6) other debts and obligations (e.g. alimony), (7) monthly debt-to-income ratio using all debt obligations listed above relative to gross monthly income, (8) credit history.

[7] The QM category should not be confused with another important category of loans in Dodd-Frank, the Qualified Residential Mortgage, which applies to risk-retention rules. While the definition of the loans is identical, the regulations and subsequent costs are otherwise distinct and the QRM requirements were not made binding until well after the QM rule came into effect.

alternative, transparent means of satisfying the ATR rule.

QM loans must satisfy the same broad verification criteria required of any ATR-compliant loan but must also have certain low-risk product features. QM loans cannot have a total debt-to-income (DTI) ratio greater than 43 percent nor can they feature negative amortization, interest-only payments, balloon payments, terms exceeding 30 years, or points and fees greater than three percent of the total loan size (with some exemptions). If the interest rate on a QM loan is lower than the prime rate cutoff established by the CFPB then the loan qualifies for a legal "safe harbor" from the ATR rule. This means that any legal action by a borrower alleging violation of the ATR rule would fail once the QM status of the loan is verified.[8]

Due to concerns that these policies would cause a contraction in the supply of mortgage credit, the CFPB established temporary exemptions to standard ATR compliance. These exemptions provide additional categories of loans that are automatically considered QM loans, even though they might not satisfy the QM definition above. Quantitatively, the largest exemption is for loans eligible to be purchased by the GSEs (Fannie Mae and Freddie Mac) or loans eligible to be insured by other government agencies.[9] The exempt loans must not have risky loan features (such as interest-only options), but they may have DTIs greater than 43 percent. This exemption is set to expire in 2021, though early expiration would be triggered for conforming loans if the GSEs exit conservatorship.

Lenders who originate loans not in compliance with the ATR rule are liable for legal damages to the borrower. A borrower may sue a lender for statutory damages within three years of a violation of the ATR rule, which is understood to be the moment of the loan's consummation. If a lender brings a foreclosure action against a borrower, the borrower may always assert a violation of the ATR rule no matter how much time has elapsed. In the event of legal action being brought by a borrower, the lender must establish in court that the underwriting process satisfied the ATR rule, where the legal burden placed on the lender is expected to depend critically on whether or not the loan has the QM safe-harbor. Barring fraud or an inability to prove that required criteria were verified (for example, not providing documentation of the borrower's income or performing DTI calculations incorrectly), the borrower will not be able to claim that a QM loan violates the ATR rule. In contrast, a lender would first have to prove that a non-QM loan followed the eight underwriting criteria outlined by the ATR rule, but this would not necessarily preclude a violation of the ATR rule itself. Instead the lender would have a "rebuttable presumption" of

[8]If the loan is higher-priced then this safe harbor is weakened and the lender only has a "rebuttable presumption" of compliance. That is, even if the loan is a QM, it may still be found in violation of the ATR rule and the lender could be liable for damages.

[9]These agencies are the Federal Housing Administration, the Department of Veterans Affairs, The U.S. Department of Agriculture, and the Rural Housing Service. The CFPB created additional permanent and temporary exemptions for certain types of loans made by small lenders, for refinances from non-standard to standard mortgages, and for lenders primarily serving low-income communities (Consumer Financial Protection Bureau, 2013).

compliance, which would still allow the borrower to claim and argue that some feature of the lender's underwriting violated the ATR rule.

The exact cost associated with violating the ATR rule is unclear since no suits have yet been brought and the penalty would likely vary with the specific violation and context. In its statement of the final rule the CFPB provided estimates of the expected cost to the lender if a lawsuit on a non-QM loan were filed. If a borrower filed within the three-year window the CFPB estimated the damages awarded to the borrower would average almost $30,000 while a suit brought as a result of a foreclosure attempt would result in damages of over $50,000. In addition, the lender would be responsible for its own and the borrower's legal costs. These estimates are all conditional on legal action being brought, so the expected cost of making a non-QM loan would weight these costs by the probability a borrower actually brings legal action, which the CFPB views as low.[10] Taken together, the CFPB estimated that the total expected liability costs generated by the regulation would work out to an increase of roughly 3–10 basis points on the rate (Consumer Financial Protection Bureau, 2013).

Currently, the size of the market affected by these costs is relatively small; however, it may expand significantly when the exemptions expire. Conventional conforming loans make up about 59 percent of the mortgage market and non-conventional loans insured by federal agencies add another 36 percent (Bhutta et al., 2015). Under the temporary exemptions, both of these categories of loans automatically receive QM status if they avoid risky features. This means that the DTI limit of 43 percent applies mainly to the jumbo loan market, which accounted for roughly 5 percent of the total market in 2014. The CFPB estimated that between 1997 and 2003 about 70 percent of all loans would have received QM status based solely on the features of the loan and only 8 percent would not have satisfied the ATR rule in any way. The CFPB also estimated that almost 100 percent of loans in 2011 would have satisfied the ATR rule in some way, again without assuming any temporary exemptions, although only 76 percent of these mortgages would have received the QM safe harbor (Consumer Financial Protection Bureau, 2013). However, alternative estimates have suggested that as little as 52 percent of the market will qualify for QM status after the agency exemption expires.[11] Therefore, it is important to quantify the effects of the regulation in its current limited implementation as this may be directly informative about near-term anticipated changes to the policy that will affect much a larger portion of the market.

[10] The CFPB is also able to bring enforcement actions against lenders with systematic or egregious violations of the ATR rule, but these are difficult to quantify and are likely to be rare.

[11] http://www.corelogic.com/downloadable-docs/MarketPulse_2013-February.pdf

III Data

III.A Data Sources

Our main source of data is the CoreLogic Loan-Level Market Analytics database (LLMA). This database contains detailed loan origination and performance information for roughly 60 percent of all first mortgages originated in the U.S. and is provided to CoreLogic by a network of contributors that includes the majority of the top U.S. mortgage servicers. The LLMA data includes coverage of both the agency and non-agency markets as well as the prime and subprime sectors going back to 1999.[12]

The dataset has two main components. The first is a static file that contains loan-level information recorded at the time of origination, including borrower characteristics (e.g. FICO, DTI, occupancy status), loan characteristics (e.g. loan amount, interest rate, LTV), and property characteristics (e.g. ZIP code, property type). The second component of the data is a dynamic file that records updated monthly performance information over the life of the loan such as the outstanding balance and delinquency status. We use the originations file for our analysis of prices and quantities and the performance file to estimate the relationship between DTI at origination and subsequent loan performance.

III.B Sample Construction and Descriptive Statistics

We restrict attention to a set of relatively homogeneous mortgages that were originated between January 2010 and December 2015, choosing these endpoints to avoid the recession and incorporate the change in policy. This provides us with four years of pre-treatment data and two years of post-treatment data. Our full analysis sample includes all first-lien, conventional (non-FHA), 30-year, fixed-rate, purchase mortgages originated during this period for which CoreLogic reports a non-missing FICO, LTV, DTI, interest rate, appraisal amount and geographic identifier.[13] We also drop a small number (less than 1 percent) of loans with DTI ratios greater than 50 percent, as many of these loans appear to be outliers. These restrictions leave us with a sample of roughly 1.2 million loans.[14]

[12] This data set is not to be confused with the CoreLogic LoanPerformance Asset-Backed Securities database (LP), which is sourced primarily from subprime mortgages that were used to collateralize private-label mortgage-backed securities.

[13] Specifically, we drop all loans for which either the ZIP code is missing or the recorded ZIP code could not be matched to a county FIPS code using the HUD-USPS ZIP code to county crosswalk file for the first quarter of 2016.

[14] As is common with most mortgage performance data, restricting attention to loans with non-missing DTIs substantially reduces the sample size since many servicers do not report DTI. However, Appendix Figure A.1.1 shows that the incidence of missing DTIs does not change discretely around the implementation of ATR/QM and is roughly similar for conforming and jumbo loans throughout our sample period.

Descriptive statistics for this sample are presented in the first column of Table I. The average loan in our sample is for roughly $265,000 at an interest rate of 4.3 percent and goes to a borrower with a FICO score of 755, LTV of 80 percent, and a back-end DTI of approximately 33 percent. In much of our analysis, we will distinguish between jumbo and conforming loans. The second and third columns of Table I report descriptive statistics separately for these two categories. Jumbo loans are significantly larger than conforming loans and are taken out by borrowers with higher credit scores and who make larger down payments. The unconditional mean interest rate on jumbo loans is also lower than that of conforming loans, likely reflecting the lower average LTV and higher-quality borrower pool for jumbo loans.

To ensure relative comparability between QM and non-QM loans in our analysis of the effect of ATR/QM on interest rates, we focus on a sub-sample of loans with back-end DTI ratios in a symmetric window around the QM-threshold of 43 percent. Specifically, we restrict attention to loans with DTI ratios between 36 and 50 percent. This restriction includes all loans in the sample with DTIs greater than 43 percent and has the added advantage of dropping loans with DTIs less than 36 percent, which is a common rule-of-thumb threshold used by lenders to distinguish between high- and low-DTI loans. The last three columns of Table I report descriptive statistics for this sub-sample which are analogous to those reported in columns 1–3 for the full sample. Other than the mechanically higher DTI, the characteristics of these loans are nearly identical to those in the full sample.

IV The Effect of ATR/QM on the Price of Credit

There are two channels through which the ATR/QM rules may have affected the cost of non-QM mortgages for borrowers. First, if the borrower files a legal claim for damages in the event of foreclosure the lender must defend that the non-QM loan satisfied the ATR rule in court. Even if this defense is unsuccessful it will involve legal fees. To compensate for this additional risk, lenders may charge a premium on non-QM loans. Second, some lenders may have responded to the ATR rule by simply exiting the non-QM market entirely. There is substantial uncertainty about the number of ATR-based claims that borrowers will file or how various courts will interpret the statutes, and lenders report that they might be unwilling to risk operating in this space.[15] This exit may have left some borrowers unable to find a lender willing to originate a non-QM loan or it may have reduced competitiveness in the non-QM market, allowing lenders to charge higher markups. In both cases, we would expect interest rates for loans that do not meet the QM standards to increase following the implementation of ATR. In this section, we test this

[15]For an example of the mortgage industry reaction see http://www.nationalmortgagenews.com/dailybriefing/Safe-Harbor-QM-Loans-May-Not-Protect-Banks-1038649-1.html

hypothesis using two alternative identification strategies which leverage different aspects of the way that ATR/QM was designed.

IV.A Research Design

Difference-in-Differences

Our primary approach to estimating the effect of ATR/QM on interest rates uses a difference-in-differences research design that compares interest rates for non-QM loans relative to similar QM loans before and after the implementation of ATR. We focus on the 43 percent DTI threshold that applies to jumbo loans and compare interest rates for high-DTI (non-QM) jumbo loans to low-DTI (QM) jumbo loans before and after the ATR rule takes effect. The key identifying assumption is that in the absence of the ATR rule the trends in interest rates for high-DTI jumbo loans and low-DTI jumbo loans would have evolved in parallel. Below, we provide direct evidence in support of this assumption by showing that interest rates for high- and low-DTI jumbo loans moved in near lockstep in the months prior to ATR implementation and only began to diverge afterwards.

Our baseline specification is a simple difference-in-differences regression estimated at the loan-level using the sample of jumbo loans with DTIs between 36 and 50 percent. Specifically, we estimate regressions of the following form:

$$r_{it} = \alpha + \delta_t + X_i'\gamma + \beta_0 \cdot \mathbb{1}[DTI_i > 43] + \beta_1 \cdot \mathbb{1}[DTI_i > 43] \times Post_t + \epsilon_{it}, \tag{1}$$

where r_{it} is the interest rate on loan i originated in month t, δ_t are month of origination fixed effects, X_i is a set of loan, borrower, and property characteristics, and ϵ_{it} is an error term assumed to be conditionally uncorrelated with unobserved determinants of r_{it}. The dummy variable $\mathbb{1}[DTI_i > 43]$ is a non-QM "treatment" indicator that takes the value one if the back-end debt-to-income ratio on loan i is greater than 43 percent. Similarly, the dummy variable $Post_t$ takes the value one if origination month t falls on or after January 2014 (the month that ATR went into effect).

The coefficient of interest is β_1, which measures the differential change in interest rates for non-QM loans relative to QM loans following the implementation of ATR, holding constant individual loan, borrower, and property characteristics as well as aggregate differences in interest rates over time. To account for serial correlation and region-specific random shocks, we cluster standard errors at the county level in all specifications.

A potential concern with this specification is that the estimate of β_1 may just be picking up an overall divergence in interest rates between high- and low-DTI jumbo loans that has nothing to do with the implementation of ATR but nonetheless only begins later in the sample period. One way to address this concern is to estimate a version of (1) that allows the effect to vary flexibly in the borrower's DTI. If the interest rate differential estimated by β_1 truly reflects a causal effect of non-QM status on the cost of credit, then we should expect this effect to manifest itself as a level shift in interest rates for jumbo loans with DTIs at *exactly* 43 percent. If, instead, lenders were simply changing the way in which they priced the underlying risk related to DTI, then we would expect this premium to vary somewhat smoothly with DTI. To see whether this is indeed the case, we report estimates from the following specification:

$$r_{it} = \alpha + \delta_t + X_i'\gamma + \sum_{d=36}^{50} \Big[\beta_0^d \cdot \mathbb{1}[DTI_i = d] + \beta_1^d \cdot \mathbb{1}[DTI_i = d] \times Post_t \Big] + \epsilon_{it}, \tag{2}$$

where $\mathbb{1}[DTI_i = d]$ is an indicator for whether the back-end debt-to-income ratio on loan i rounded up to the nearest integer is exactly equal to d, and all other variables are as defined in (1). In this specification, we omit the dummy for DTI-bin $d = 43$, so that the coefficients β_1^d estimate the bin d-specific change in interest rates following the implementation of ATR relative to the change in rates for loans with DTIs of 43 percent. If the change in interest rates for high-DTI loans is truly a result of their non-QM status, then we should expect to find $\beta_1^d = 0$ for $d < 43$, and $\beta_1^d > 0$ for $d > 43$.

Triple Difference

As a final test that our results are not being driven by unobserved and time-varying heterogeneity in interest rates across the DTI distribution, we also present estimates that are based on a triple-difference strategy that uses conforming loans as an additional control group. Because only jumbo loans are required to meet the 43 percent DTI limit to satisfy the QM standards, changes in interest rates for high-vs-low-DTI conforming loans serve as a useful counterfactual for changes in interest rates across the DTI distribution that may have occurred even in the absence of ATR. By including conforming loans in the sample and differencing out their corresponding change in interest rates for high- relative to low-DTI borrowers, we are able to relax the identifying assumption underlying the main difference-in-differences specification in (1). Specifically, the triple-difference strategy only requires us to assume that the change in interest rates for high-DTI relative to low-DTI loans would have been the same for both jumbo and conforming loans in the absence of ATR.

To implement this triple difference strategy, we estimate a series of regressions of the following form using the full sample of loans with DTIs between 36 and 50 percent:

$$\begin{aligned} r_{ist} = \alpha + \delta_{st} + X_i'\gamma + \beta_0 \cdot \mathbb{1}[DTI_i > 43] + \beta_1 \cdot \mathbb{1}[DTI_i > 43] \times Post_t \\ + \beta_2 \cdot Jumbo_i + \beta_3 \cdot Jumbo_i \times \mathbb{1}[DTI_i > 43] \\ + \beta_4 \cdot Jumbo_i \times \mathbb{1}[DTI_i > 43] \times Post_t + \epsilon_{ist}. \end{aligned} \tag{3}$$

In this specification, r_{ist} is the interest rate on loan i originated in month t in market segment $s \in \{Jumbo, Conforming\}$, δ_{st} are market segment by month fixed effects, and $Jumbo_i$ is an indicator for whether loan i is a jumbo loan. The coefficient of interest is β_4, which measures the differential change in interest rates for high-DTI relative to low-DTI loans in the jumbo market relative to the conforming market following the implementation of ATR.

IV.B Results

Graphical Evidence

As a starting point for the empirical analysis, Figure I plots mean interest rates by origination month separately for jumbo loans with DTIs above 43 percent (orange circles) and those with DTIs less than or equal to 43 percent (blue triangles). Each dot in the figure represents the raw average interest rate for loans originated in the indicated month and is measured on the left axis. The vertically dashed grey line in January 2014 marks the month that ATR went into effect.

Consistent with the parallel trends assumption, interest rates for high-DTI and low-DTI jumbo loans move together prior to the implementation of ATR and only begin to diverge afterwards. This can be seen most clearly by looking at the grey bars, which plot the month-by-month difference in mean interest rates between high- and low-DTI loans, measured on the right axis. Before January 2014, the average interest rate for a high-DTI jumbo loan is typically within a five basis point range above or below the corresponding average interest rate for a low-DTI loan. However, in the month that ATR goes into effect average rates for high-DTI loans shift upward by roughly 10–15 basis points relative to low-DTI loans.

This relative shift in interest rates for high-DTI loans occurs at a DTI that is *exactly* equal to the QM threshold of 43 percent. To illustrate this, Figure II plots detrended mean interest rates by DTI separately for jumbo loans originated before (blue triangles) and after (orange circles) the implementation of ATR.[16] For loans originated prior to ATR, the relationship between interest

[16]To create this figure, we regress the interest rate on a set of origination month dummies and then average the residuals of this regression within each one percent DTI bin separately for loans originated before and after January 2014. Each dot in the figure plots the mean of the residuals from this regression for the corresponding DTI bin and time period. DTI bins are created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent.

rates and DTI is roughly flat. In contrast, for loans originated in the post-ATR period, there is a sharp jump in interest rates of roughly 15 basis points as the DTI crosses the 43 percent threshold. Together, we take the results presented in Figure I and Figure II as convincing evidence in favor of the parallel trends assumption underlying the difference-in-differences research design.

Regression Results

Table II presents our main estimates of the effect of non-QM status on interest rates. The first four columns report estimates from the basic difference-in-differences specification given by equation (1). In the first column, we report estimates from a baseline specification that includes only the non-QM dummy ($DTI > 43$), the interaction of that dummy with with the $Post$ indicator, and a full set of origination month fixed effects.[17] The coefficient of interest is reported in the second row and implies that non-QM loans have an interest rate premium of roughly 13 basis points. This estimate is highly statistically significant and is an order of magnitude larger than the difference in interest rates that existed between high- and low-DTI loans prior to the implementation of ATR as can be seen from the coefficient estimate on the non-QM dummy reported in the first row. In the top row of the bottom panel of the table, we also report the implied percentage increase in interest rates relative to the pre-period mean interest rate among high-DTI loans. A 13 basis point increase represents a roughly 3 percent increase in the cost of credit for non-QM borrowers.

In columns 2–4, we add a series of controls that increasingly restrict the nature of the variation being used to identify the premium charged for non-QM loans. In the second column we include a full set of county fixed effects so that the effect of non-QM status on interest rates is identified by comparing within county changes in rates for high-versus-low-DTI loans before and after the implementation of ATR. This controls for the fact that high-DTI borrowers are likely to be located in expensive regions of the country that may have different overall average interest rates. The resulting estimate of the effect of non-QM status on interest rates is statistically indistinguishable from the baseline estimate reported in the first column. If anything, the estimate reported in the second row of column two implies a slightly larger non-QM premium of roughly 14 basis points.

While these results suggest that lenders charge a premium for non-QM loans, it is also possible that the higher interest rates for high-DTI loans partially reflect borrower selection or differential lender screening following the implementation of ATR. This type of selection would mean that the observed difference in interest rates between high- and low-DTI loans will also reflect the changing composition of borrower types along the DTI distribution. We address this possibility in the third column, which controls flexibly for borrower type by including a full set of 20-point

[17]The $Post$ main effect is not reported in this table because it is absorbed by the origination month fixed effects.

FICO score bins, 5-point LTV bins, and the pairwise interaction between the two. Doing so decreases the coefficient estimate only modestly to 12 basis points, which is statistically indistinguishable from the baseline estimate in column one. Similarly, the coefficient remains unchanged in column four, when we also include a set of property-type fixed effects to remove differences in interest rates that lenders may charge based on the type of home being financed.[18]

To show that these estimates are not being driven by changes to the way that lenders are pricing the underlying risk associated with DTI, which would presumably vary even across high-DTI loans, Figure III plots coefficient estimates from the more flexible difference-in-differences specification given by equation (2) that allows the effect to vary non-parametrically in the borrower's DTI. We estimate a version of equation (2) that includes all of the same controls that were included in the fourth column of Table II and plot the resulting coefficient estimates and 95 percent confidence intervals for the interaction terms between each DTI bin and the $Post$ dummy. We normalize the coefficient for DTI-bin $d = 43$ to zero so that all coefficients can be interpreted as the change in interest rates for a given DTI bin following the implementation of ATR relative to the corresponding change in rates for loans with DTIs just under the QM threshold. As the figure makes clear, the increase in interest rates for high-DTI loans is driven entirely by a level shift in rates that occurs at exactly 43 percent. Moreover, the premium charged for non-QM loans does not depend on the borrower's DTI; all borrowers with DTIs greater than 43 percent are charged a premium of roughly 10–13 basis points. The fact that the premium is roughly constant across high-DTIs provides further assurance that the results are unlikely to be driven by borrower selection. If the increase in interest rates for high-DTI loans were driven by selection we would expect that increase to be higher at DTIs just above 43 percent where it is easier for borrowers to get below the threshold by lowering their DTI.

Finally, in the last four columns of Table II, we report estimates from the triple-difference strategy that uses conforming loans as an additional control group. In these regressions, we expand the sample to include all loans with DTIs between 36 and 50 percent. We identify the effect of non-QM status on interest rates by comparing the change in rates for high- versus low-DTI loans in the jumbo market following the implementation of ATR relative to the corresponding change in the conforming market. Thus, we are allowing for the possibility that QM status is irrelevant and lenders were simply pricing in an unrelated change in the risk of all high-DTI loans. Across the columns, the controls are introduced in the same order as in columns 1–4, with the exception that the month of origination fixed effects are also interacted with the $Jumbo$ dummy in the triple difference specifications. The coefficient of interest is the triple interaction term reported in the fourth row. In all cases, the estimated effect is statistically indistinguishable from

[18]The property-type fixed effects distinguish between four different types of homes: single family, condominium, townhouse, and planned unit development.

and of roughly the same magnitude as the corresponding difference-in-differences estimate. This leads us to conclude that non-QM loans are associated with an interest rate premium on the order of 10–15 basis points, which represents an increase in the cost of credit for these borrowers of roughly 2.5–3 percent relative to the pre-ATR mean. Interestingly, these estimates are nearly identical to the CFPB's own forecasts of how the regulation would affect lenders' costs of origination, suggesting that much of the additional costs are being passed along to borrowers in the form of higher rates.[19]

To get an alternative sense of the magnitude of this effect we calculate the dollar amount of the additional interest paid assuming the borrower does not refinance or default. The average jumbo loan in our sample is about $640,000 with an APR of 4.19 percent and our sample was restricted to 30-year loans. For a loan with these characteristics, the estimated premiums of 10–15 basis points imply the borrower will pay an additional $13,000–20,000 in interest over the life of the loan (not discounted to present value). If, instead, we assume the borrower refinances into a QM loan after 5 years, then the total increase in interest paid would work out to $1,700–2,600 over the life of the loan, which we view as relatively small.

V The Effect of ATR/QM on the Quantity of Credit

In addition to increasing the cost of credit, the Ability-to-Repay Rule and Qualified Mortgage Standards may have also affected the quantity of mortgage debt issued. On the supply side, lenders need not have responded to the ATR rule simply by changing the price that they charge for non-QM loans. Instead, some may have responded on the quantity margin by choosing to originate fewer non-QM loans or exiting the non-QM market entirely. On the demand side, as the price of non-QM loans increased and accessibility fell, some borrowers who would have otherwise taken a loan at a DTI above 43 percent may have responded either on the intensive margin by taking out a smaller loan or on the extensive margin by forgoing their home purchase.

A simple examination of the raw data suggests that the law did indeed have an effect on the allocation of credit across the DTI distribution. In Figure IV, we plot the distribution of DTIs among new jumbo mortgage originations separately for 2013 (blue triangles) and 2014 (orange circles). We group borrowers' DTIs into one-percent bins and plot the share of jumbo loans falling into each of these bins by year. In 2013, this share remains roughly constant as the DTI crosses the QM threshold of 43 percent. In contrast, after ATR was enacted in 2014, the distribution

[19] In its final ruling the CFPB stated that "estimated costs for non-qualified mortgage loans (loans made under the ability-to-repay standard without any presumption of compliance) are estimated to increase by approximately twelve basis points (or 3 basis points (0.03 percentage points) on the rate); under very conservative estimates, this figure could be as high as forty basis points (or ten basis points (0.01 percentage points) on the rate). Depending on the competitive conditions in the relevant product and geographic markets, some of this increase will be passed on to borrowers and the rest will be absorbed by lenders" (Consumer Financial Protection Bureau, 2013).

features a sharp drop at exactly 43 percent. Relative to the pre-period, the 2014 distribution also exhibits a significant amount of bunching to the left of 43 percent and missing mass to the right. In this section, we use these features of the post-ATR distribution—bunching and missing mass—to decompose the quantity response into its intensive and extensive margin components.

V.A Research Design

We measure the intensive and extensive margin quantity response to ATR by comparing the amount of missing mass to the right of the QM threshold to the amount of bunching at and to the left of it. Intuitively, the number of borrowers who are shifted along the intensive margin to lower DTIs should be equal to the number of loans bunching at the QM threshold. Similarly, the number of borrowers who disappear from the market entirely as a result of ATR—the extensive margin response—should be equal to the total number of missing loans to the right of the threshold minus the number that were shifted to the left of it.

Constructing the Counterfactual post-ATR DTI Distribution

To accurately estimate the amount of bunching and missing mass in the observed DTI distribution, we first need an estimate of the counterfactual distribution that would have prevailed in the absence of ATR. A large literature in public finance has developed approaches for obtaining this type of counterfactual estimate.[20] The standard approach involves fitting a high-order polynomial to the observed distribution while excluding the data in a region immediately surrounding the threshold and then extrapolating this polynomial through the omitted region (Chetty et al., 2011; Kleven and Waseem, 2013). This approach, however, is not well-suited for our context because it is based on the assumption that the counterfactual distribution is smooth at all values of the "running variable" (DTI in our case). As was shown in Figure IV, this assumption is clearly violated in our context; the DTI distribution features a large discontinuity at 45 percent even during the pre-period. This discontinuity arises because many lenders impose their own internal maximum DTI thresholds of 45 percent, which leads to a large drop in the number of loans with DTIs beyond this limit.[21]

To address this issue, we develop and validate an alternative approach to estimating the counterfactual that leverages both the time-series dimension of our data as well as the fact that the conforming market was exempt from the regulation and should therefore be unaffected by it. Our goal is to estimate the counterfactual number of jumbo loans that would have been originated in each DTI-bin d in the post-ATR period had the ATR rule not been in effect. We denote

[20] See Kleven (2016) for a comprehensive review of this literature as well as DeFusco and Paciorek (2017) and Best et al. (2015) for applications of these methods to the mortgage market.

[21] This limit also corresponds to the 45 percent DTI limit broadly imposed by the GSE's in the conforming market.

this counterfactual number of loans as $\hat{n}_{jd}^{post}$. We estimate the counterfactual distribution using information on both the actual number of jumbo loans originated in the pre- and post-ATR periods (n_{jd}^{pre} and n_{jd}^{post}) as well as the corresponding number of loans originated in the conforming market (n_{cd}^{pre} and n_{cd}^{post}).

The idea behind our approach is to construct the counterfactual post-ATR jumbo distribution from the observed pre-period jumbo distribution plus an adjustment that is based on the observed changes in the conforming market distribution. We make three assumptions that allow us to perform this exercise.

Assumption 1. *The conforming market is unaffected by the policy:*

$$\hat{n}_{cd}^{post} = n_{cd}^{post}.$$

This assumption is motivated by the fact that the conforming market was exempt from the 43 percent DTI limit. It states that the counterfactual number of conforming loans originated in each DTI bin in the post-ATR period is equal to the observed number of loans in each bin. As in our triple difference analysis above, this assumption is what will allow us to use observed changes in the conforming market to proxy for the counterfactual changes in the jumbo market that would have occurred even in the absence of ATR. It is important to note that this assumption implicitly requires that none of the borrowers leaving the jumbo market at high DTIs as a result of ATR/QM are substituting into the conforming market. We validate this assumption in Appendix A.1 by showing that there was no relative post-policy increase in the degree of "bunching" at the conforming limit among high-DTI loans, which is what would be expected if high-DTI jumbo borrowers were selecting into the conforming market as a result of ATR/QM.

Our second assumption is that the policy only affects behavior in the jumbo market near and above the QM threshold.

Assumption 2. *There exists a maximum DTI-bin* $\overline{d} < 43$ *such that the total volume of jumbo loans with DTIs less than or equal to* $\overline{d}$ *is unaffected by the policy:*

$$\sum_{d=0}^{\overline{d}} \hat{n}_{jd}^{post} = \sum_{d=0}^{\overline{d}} n_{jd}^{post} \triangleq N_{j\overline{d}}^{post}.$$

The intuition for this assumption is straightforward: imposing a maximum DTI limit should only shift loans from above the limit to just below it. Any borrower who would have optimally chosen to take out a loan with a DTI less than the QM-threshold in the absence of the policy is still able to do so. Similarly, any borrower who chooses to lower their DTI from above to below the QM-threshold in response to the policy is unlikely to choose a DTI that is significantly below that threshold. As a result, there must be some maximum debt-to-income ratio $\overline{d}$ below which the total volume of jumbo loans $N_{j\overline{d}}^{post}$ will be unaffected.

Assumption 2 provides a convenient and policy-invariant normalization that allows us to translate between the DTI distribution in the jumbo and conforming markets. Because the conforming market is significantly larger than the jumbo market, it is not informative to directly compare the number of loans in a given DTI bin across markets (e.g. $\hat{n}_{jd}^{post}$ and n_{cd}^{post}). However, when we divide each of these bin counts by the corresponding total level of activity to the left of $\overline{d}$ in the relevant market, the *ratios* (e.g. $\hat{n}_{jd}^{post}/N_{j\overline{d}}^{post}$ and $n_{cd}^{post}/N_{c\overline{d}}^{post}$) will be directly comparable.

Our third assumption relates the predicted counterfactual change in these ratios in the jumbo market to the observed change in the conforming market.

Assumption 3. *Parallel trends:*

$$\frac{\hat{n}_{jd}^{post}}{N_{j\overline{d}}^{post}} = \frac{n_{jd}^{pre}}{N_{j\overline{d}}^{pre}} + \left(\frac{n_{cd}^{post}}{N_{c\overline{d}}^{post}} - \frac{n_{cd}^{pre}}{N_{c\overline{d}}^{pre}}\right) \triangleq \hat{\pi}_{jd}^{post}.$$

In words, this assumption states the change in the (normalized) number of jumbo loans in a given DTI bin between the pre- and post-ATR periods would have been the same as the corresponding change in the conforming market in the absence of the policy.

Assumption 3 is directly analogous to the assumption underlying our triple difference analysis of the interest rate effect. However, it is somewhat more restrictive since we require it to hold for each DTI bin, not just on average for DTIs above the QM threshold. In the results section below we provide direct and compelling evidence in support of this assumption by showing that the implied counterfactual post-period DTI distribution provides an accurate and unbiased estimate of the empirical distribution in placebo years for which there was no policy change.

Given Assumptions 1–3, we are able to construct an estimate of the counterfactual post-ATR jumbo DTI distribution that depends only on policy-invariant functions of the observed pre- and post-period distributions. Specifically, our estimate of the counterfactual is given by

$$\hat{n}_{jd}^{post} = \hat{\pi}_{jd}^{post} \times N_{j\overline{d}}^{post}. \tag{4}$$

Equation (4) intuitively expresses the counterfactual as a product of two terms: a measure of the observed overall level of activity in the jumbo market ($N_{j\overline{d}}^{post}$) and the predicted allocation of that activity across the DTI distribution ($\hat{\pi}_{jd}^{post}$). By Assumption 2, the relevant measure of the overall level of activity in the jumbo market is unaffected by the policy since it only depends on DTIs below the threshold $\overline{d}$. Similarly, by Assumptions 1 and 3, the predicted allocation of that activity across the DTI distribution is also unaffected by the policy; it depends only on the pre-period jumbo distribution, which is policy-invariant by definition, and the change in the distribution in the conforming market, which is policy-invariant by assumption.

Bunching, Missing Mass, and the Effect of ATR/QM on the Quantity of Credit

With this counterfactual in hand, we are now able to measure both the intensive and extensive margin effects of the policy on the quantity of mortgage credit issued by comparing the observed post-ATR distribution to the counterfactual. On the intensive margin, the number of borrowers shifted to lower DTIs by the policy is simply equal to the number of loans bunching at and just below the QM threshold. We measure this as the sum of the difference between the counterfactual and empirical distributions over the region to which borrowers are assumed to be shifted

$$\hat{B} = \left| \sum_{d=\overline{d}}^{43} \left(\hat{n}_{jd}^{post} - n_{jd}^{post} \right) \right|. \tag{5}$$

Similarly, the total amount of missing mass to the right of the threshold is given by

$$\hat{M} = \sum_{d=44}^{50} \left(\hat{n}_{jd}^{post} - n_{jd}^{post} \right). \tag{6}$$

Some of these borrowers are missing from the right of the threshold because they were shifted to the left of it—in which case they would show up in $\hat{B}$. The remainder are missing because they have disappeared from the market entirely due to extensive margin responses. The total number of loans lost due to extensive margin responses is therefore given by the difference $\hat{M} - \hat{B}$.

To facilitate the interpretation of the results, we report the intensive and extensive margin effects as percentages of the total size of the potentially affected market segment. Specifically, we report the intensive margin effect as $\hat{B}/\hat{N}_{44+}^{post}$ and the extensive margin effect as $(\hat{M} - \hat{B})/\hat{N}_{44+}^{post}$, where $\hat{N}_{44+}^{post} = \sum_{d=44}^{50} \hat{n}_{jd}^{post}$. Our estimates will therefore reflect the percent of all high-DTI jumbo loans that were either shifted or lost as a result of the policy. We calculate standard errors for all estimated parameters by bootstrapping from the observed sample of mortgages, drawing 100 random samples with replacement and re-estimating the parameters at each iteration.

Finally, in order to estimate the components of equations (4)–(6) there are two free parameters we must choose: the lower limit of the bunching region ($\overline{d}$), and the time periods over which to measure the pre- and post-ATR distributions. For our main analysis, we set $\overline{d} = 38$. This choice is motivated by the evidence in Figure IV, which suggests that the pre- and post-ATR distributions were roughly similar for all DTIs less than this threshold. We also show that all of our results are robust to alternative choices for $\overline{d}$. To increase the likelihood that the parallel trends required by Assumption 3 hold, we focus on a narrow time window around the implementation of ATR, setting the pre-period equal to 2013 and the post-period to 2014.

V.B Results

Validating the Counterfactual

Before presenting our main results, we first provide evidence validating the assumptions underlying our approach to estimating the counterfactual by demonstrating that they generate a distribution that closely resembles the true distribution in years when there is no policy change. To do so, we designate each of the 13 years prior to ATR/QM for which we are able to construct a counterfactual as "placebo" years.[22] For each of these placebo years, we estimate the counterfactual jumbo DTI distribution as if ATR/QM had been passed in January of that year, using the prior year as the pre-period and setting $\overline{d} = 38$ as in our main analysis. We then compare this estimated distribution to the observed empirical distribution. If the assumptions we make to generate the counterfactual are valid, then these two distributions should be the same.

Figure V presents the results from this exercise. Panel A. plots the empirical and estimated counterfactual distributions for 2013. Reassuringly, the counterfactual does an excellent job of matching the empirical distribution including the discontinuity at a DTI of 45 percent. In Panel B., we generalize this comparison by summarizing the results from all of the year-by-year placebo tests in a single figure. To do so, we first generate counterfactual distributions for each of the remaining placebo years as was done in Panel A. for 2013. We then calculate the percent difference between the empirical and counterfactual number of loans in each DTI bin for each year from 2000 to 2013. The histogram plotted in Panel B. shows the distribution of these differences across all DTI bins and years along with its mean, median, standard deviation, and interquartile range. The distribution is centered at zero and spans a relatively narrow range. For over half of the DTI bins we consider, the counterfactual and empirical number of loans are within 10 percent of each other and the median difference is less than one percent. We take this as compelling evidence that the approach we use to construct the counterfactual distribution produces accurate and unbiased estimates. Critically, when we generate our estimates we take the statistical variation embedded in our approach into account through a bootstrap procedure.

Intensive and Extensive Margin Quantity Effects

Having validated our method of generating the counterfactual DTI distribution, we turn to our main analysis of the effects of ATR/QM on the quantity of credit. In Figure VI, we plot both the observed DTI distribution and the counterfactual for loans originated in 2014, the first year

[22]The LLMA data coverage extends back to 1999; however, at least one year of pre-data is needed to construct the counterfactual distribution, which limits the set of possible pre-ATR/QM placebo years to 2000–2013.

that ATR/QM was in effect. The solid orange connected line plots the empirical distribution. Each dot represents the number of jumbo loans originated in 2014 for which the borrower's DTI fell into the one-percent bin indicated on the x-axis. The dashed blue connected line plots the counterfactual, estimated as described in Section V.A. The vertical dashed lines mark the lower limit of the bunching region ($\overline{d} = 38$), the QM threshold of 43 percent, and the maximum DTI.

The empirical distribution exhibits a sharp discontinuity at the QM-threshold; moving from a DTI of 43 to 44 percent leads to a more than 50 percent drop in the number of loans. In contrast, the counterfactual number of loans in these two bins are roughly the same. Consistent with the evidence presented in Figure IV, there is also a significant amount of bunching to the left of the threshold. Our estimate of the intensive margin response, reported in the top left corner of the figure, suggests that roughly 20 percent of the loans that would have otherwise had a DTI above 43 percent were shifted from above to below the threshold. These borrowers, however, do not account for the entirety of the missing mass to the right of the limit. The difference between the counterfactual and empirical distribution to the right of the threshold represents roughly 35 percent of the counterfactual number of loans in that region. Thus, we estimate that approximately 15 percent of all jumbo loans that would have otherwise had a DTI above 43 percent were eliminated due to extensive margin responses.

The first column of Table III repeats these estimates along with their standard errors, calculated using the bootstrap procedure described above. Both the intensive (top row) and extensive margin (bottom row) responses are significant at the one-percent level. The second through third columns of the table report analogous estimates under varying assumptions for the lower limit of the bunching region $\overline{d}$. We consider values of $\overline{d}$ ranging from 30 to 40 percent. In all cases, the estimated responses are of roughly the same order of magnitude and, across specifications, bracket our preferred estimate. Individually, the 95 percent confidence interval for each of these estimates also includes the preferred estimated reported in column 1. Across columns, the intensive margin effect ranges from 19 to 27 percent and the extensive margin response ranges from 9 to 18 percent. All of these estimates are significant at the five-percent level or higher, with the exception of the extensive margin response when $\overline{d} = 35$ which has a p-value of 0.15. Reassuringly, there is also no systematic relationship between the magnitude of the estimated effect and the level of $\overline{d}$. Together, the evidence presented in Table III provides confidence that our results are not being driven by the assumptions we make on the lower limit of the bunching region.

Economic Magnitudes

Relative to the size of the potentially affected portion of the market, the quantity effects we estimate are quite large. However, it is useful to put these estimates into context to provide a sense of the potential dollar loss of credit induced by the regulation. Our preferred estimates imply that 15 percent of all jumbo loans in 2014 that would have otherwise had a DTI above 43 percent were eliminated as a result of the policy. These lost loans constitute 2 percent of the entire counterfactual jumbo market. When multiplied by the total volume of new jumbo purchase mortgages originated in 2014, this implies that at least $600 million in jumbo mortgage was eliminated as a result of the policy. While this is a relatively small quantity, the exemptions limiting the ATR/QM rule to the jumbo market are set to expire by 2021 at the latest. After this point, the regulation would apply to the entire mortgage market. If we extrapolate our estimate to the non-jumbo purchase market, it suggests the regulation would have reduced the quantity of mortgage credit by about $12 billion in 2014.[23]

As an alternative way of putting these estimates into context, it is also informative to compare them to the magnitude of the interest rate response estimated in Section IV. While we have shown that the ATR/QM policy increased the cost of non-QM loans and that the quantity of these loans then declined substantially, it is not clear if this quantity response is driven by contractions in supply or demand. Evidence on the elasticity of demand for mortgages, however, suggests that the increase in interest rates alone cannot explain the quantity response. For example, DeFusco and Paciorek (2017) estimate that a one percentage point increase in the interest rate for jumbo loans reduces the quantity of mortgage debt demanded by 2–3 percent. A naive extrapolation of these estimates to our setting suggests an average decline of at most 0.45 percent. Similarly, Best et al. (2015) estimate a wider range of mortgage interest rate elasticities between 0.1 and 1.4. Applied to our estimates, these elasticities would imply a reduction in mortgage demand of roughly 0.3–4.2 percent.[24] Instead, we find that 15 percent of borrowers facing the interest premium reduce quantities 100 percent and another 20 percent potentially reduce their mortgages by a smaller amount.[25] Together these effects imply a decline in mortgage demand of at least 15 percent, an effect 3.5–30 times larger than what we should expect if borrowers were responding to the interest rate effect alone. As a result, it is more likely that the reductions in quantity we observe primarily reflect a supply response from lenders unwilling to originate non-QM loans.

VI The Effect of ATR/QM on Loan Performance

Our results thus far indicate that the ATR/QM rule led to both an increase in the cost of credit for high-DTI jumbo borrowers and a reduction in the quantity of high-DTI jumbo mortgages orig-

[23] These calculations are based on data provided in Bhutta et al. (2015), who use HMDA data to calculate that the total volume of new purchase mortgage originations in 2014 was approximately $600 billion, and that jumbo mortgages accounted for roughly five percent ($28.2 billion) of that total.

[24] These figures are calculated assuming a 3 percent increase in interest rates associated with non-QM loans, which is at the upper range of the estimates reported in the bottom panel of Table II.

[25] This ignores any variation in the size of the mortgages being lost, and the reductions from declines in DTI ratios could be zero if DTI is reduced entirely through increases in income.

inated, which was a primary aim of the policy. However, an additional goal of the ATR/QM rule was to lower the rate at which borrowers defaulted on their mortgages. Therefore, the effectiveness of the policy along this dimension depends crucially on the relationship between DTI and default risk.[26] Without a positive association between DTI and the probability of default, a reduction in the number of high-DTI loans will have little effect on the aggregate default rate.

As an initial exploration of this relationship, Figure VII plots non-parametric estimates of the historical association between DTI and default for mortgages originated during the run-up to the financial crisis (2005–2008).[27] We define a loan as having defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within five years of the origination date. Panel A. plots the relationship for jumbo loans only and Panel B. pools across all loans. While the relationship between DTI and default is generally increasing at low DTIs in both samples, it is substantially weaker at high DTIs among jumbo loans. In fact, for jumbo loans, there is no statistically distinguishable relationship between default and DTI in the region of the distribution that was most affected by the policy ($DTI >= 38$). This suggests that the current implementation of the policy, which only applies to jumbo loans, would not have generated meaningful performance improvements had it been in effect during the run-up to the crisis. However, as shown in Panel B., there is a much stronger positive relationship between DTI and default in the sample of all loans. Therefore, it is possible that the policy would have reduced aggregate default rates had it been in place and extended to the entire market during this time period. This is consistent with the findings of Foote et al. (2010), who estimate a nonlinear default model on data from 2005 to 2008 and find a small positive relationship between DTI and default.

In this section, we combine our estimates of the effect of the policy on the DTI distribution with this historical relationship between DTI and default to generate counterfactual predictions for how the policy may have affected default rates during the financial crisis had it been in effect during that period. In performing this exercise, we assume our estimates of the effect of the policy on the DTI distribution can be extrapolated both across time and into the conforming market. We also assume that the historical relationship between DTI and default is policy-invariant. While these are strong assumptions, we think it is important to provide at least a rough estimate of the potential impacts of the policy on mortgage performance under an important crisis scenario.

[26] While our focus is on the DTI restriction in this paper, it is important to note that the policy may still be able to achieve reductions in default through the other restrictions on contract terms contained in the QM definition.

[27] Each panel reports the coefficient estimates from a regression of whether a loan defaulted on a series of dummy variables indicating whether the loan's DTI fell into a given one-percent bin. We omit the dummy for $DTI = 38$, which is the lower limit of the bunching region in our preferred specification for the quantity effect. The regression also includes fixed effects for the month of origination, county, and property type as well as flexible interactions between the borrower's FICO score (20-point bins) and LTV (5-point bins).

VI.A Estimating the Relationship between DTI and the Probability of Default

To convert our estimates of the effect of the policy on the DTI distribution into an aggregate default rate prediction, we first estimate the change in the individual default probability associated with shifting a borrower from a DTI above the 43 percent cutoff to just below it. To do so, we assign all loans originated between 2005 and 2008 into three DTI bins consistent with the approach used to estimate the quantity effect in Section V: high-DTI ($DTI > 43$), medium-DTI ($DTI \in (38, 43]$), and low-DTI ($DTI \leq 38$). Since the medium-DTI range corresponds to the bunching region used to identify the quantity effect, the differential default rate for high-DTI loans relative to loans in this region will provide an estimate of the effect of shifting a borrower from above to below the cutoff.

We estimate these relative default rates using a linear probability model where the dependent variable d_{it}^h is an indicator equal to one if loan i originated in month t defaults within a specified horizon h:

$$d_{it}^h = \alpha_c + \delta_t + \beta_L \cdot \mathbb{1}[DTI_i \leq 38] + \beta_H \cdot \mathbb{1}[DTI_i > 43] + X_i'\gamma + \epsilon_{it}. \tag{7}$$

We consider default rates defined over one to five year horizons and estimate (7) separately for each default horizon and origination year cohort. As above, we define a loan as having defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed within h years of the origination date. The coefficients of interest are β_L and β_H, which measure the probability of default for low- and high-DTI loans relative to loans in the medium-DTI range. To account for possible correlation between DTI and other factors associated with default risk we include fixed effects for the month of origination, county, and property type as well as flexible interactions between the borrower's FICO score (20-point bins) and LTV (5-point bins). Thus, the recovered coefficients will give us an estimate of the slope of the relationship between DTI and loan performance holding all other relevant observables fixed. Standard errors are clustered at the county level in all specifications.

VI.B Calculating the Effect on the Aggregate Default Rate

To calculate the counterfactual effect of the policy on a cohort's aggregate default rate, we combine the relative default probabilities β_L and β_H with the estimated effects of the policy on the DTI distribution presented in Section V. In particular, we are interested in estimating

$$\Delta DefaultRate = \frac{\sum_i \theta_i \hat{N}_i}{\sum_i \hat{N}_i} - \frac{\sum_i \theta_i N_i}{\sum_i N_i},$$

where θ_i is the default probability for loans in DTI bin $i \in \{L, M, H\}$, N_i is the number of loans in bin i, and $\hat{X}$ denotes the counterfactual value of a generic historical variable X under the assumption that the policy was in effect at the time.[28] If the policy lowers default rates, then this expression will be negative. Noting that $\theta_M = \theta_L - \beta_L$ and $\theta_H = \theta_L - \beta_L + \beta_H$, this expression can be re-written as

$$\Delta DefaultRate = (\beta_H - \beta_L)(\hat{\delta}_H - \delta_H) - \beta_L(\hat{\delta}_M - \delta_M), \tag{8}$$

where $\delta_i \triangleq N_i / \sum_i N_i$ denotes the share of loans in DTI bin i.

Equation (8) expresses the counterfactual change in the default rate as a function of the individual relative default probabilities for high- and low-DTI loans (β_H and β_L) and the aggregate shift in the distribution of loans from just above the 43 percent cutoff ($\hat{\delta}_H - \delta_H$) to just below it ($\hat{\delta}_M - \delta_M$). These shifts in the DTI distribution can, in turn, be expressed as a function of the intensive and extensive margin quantity effects estimated in Section V. In particular, if we maintain the assumption that the low-DTI portion of the distribution is unaffected by the policy and let γ denote the extensive margin response (the fraction of all jumbo loans that were not made) and α the intensive margin response (the fraction of all jumbo loans that were shifted to lower DTIs), then the observed and counterfactual number of loans in each bin can be related to each other as follows:

$$\begin{aligned}
\hat{N}_L &= N_L \\
\hat{N}_M &= N_M + \alpha \sum_i N_i \\
\hat{N}_H &= N_H - (\alpha + \gamma) \sum_i N_i \\
\sum_i \hat{N}_i &= (1-\gamma) \sum_i N_i .
\end{aligned}$$

Using these relationships we can express the effect of the policy on the share of loans in each bin as

$$\begin{aligned}
\hat{\delta}_M - \delta_M &= \frac{\gamma}{1-\gamma} \delta_M + \frac{\alpha}{1-\gamma} \\
\hat{\delta}_H - \delta_H &= \frac{\gamma}{1-\gamma} \delta_H - \frac{\alpha + \gamma}{1-\gamma},
\end{aligned}$$

which can be substituted back into (8) to yield an expression for the change in the aggregate de-

[28] In contrast with our earlier results, since these are historical data the observed outcome is the world without the policy, and the counterfactual is the world where the policy was implemented.

fault rate that depends only on observable quantities. In particular, this substitution allows us to calculate the change in the aggregate default rate as a function of the relative default probabilities (β_H and β_L), the extensive and intensive margin quantity effects (γ and α), and the observed share of loans in the high- and middle-DTI regions (δ_H and δ_M).[29]

VI.C Results

Aggregate Default Rate Implications of ATR/QM

Table IV reports our estimates of relative five-year default probabilities for high- and low-DTI loans and the implied change in the aggregate default rate by year of origination. Consistent with Figure VII, the first two rows of Panel A. show that high-DTI loans in the jumbo market do not exhibit worse performance than the omitted category (except for 2008). When combined with quantity adjustment estimates the implied change in the aggregate default rate (third row) is also very small and not consistently distinguishable from zero. In contrast, the first two rows of Panel B. confirm that there is a positive relationship between DTI and default in the sample that includes all loans. The strength of this effect changes considerably over time, with high-DTI loans made after 2006 performing more poorly relative to their lower-DTI counterparts, while the default rates of 2005 and 2006 cohorts do not vary as strongly with DTI. Combining these estimates with the implied changes in the DTI distribution generates consistently positive and statistically significant improvements in default rates, but the magnitudes are economically quite small. For example, the estimate for the 2008 cohort suggests that the policy would have only reduced the aggregate five-year default rate by 0.2 percentage points had it been in place at the time those loans were originated. To put this into perspective, the overall average five-year default rate for the 2008 cohort was approximately 34 percent.[30]

Figure VIII plots the these aggregate default rate effects using the sample of all loans by cohort for all default horizons. The policy would have resulted in a much larger default rate reduction for 2007 and 2008 cohorts than for 2005 and 2006 cohorts with differences becoming stronger as the horizon is extended. Differences across cohorts potentially reflect the fact that repayment problems are less likely to lead to default when the lender can be fully repaid from the sale of the property. Considering that property prices were declining from 2007 until 2012, the 2007 and 2008 cohorts are likely to have had a much higher incidence of negative equity while the labor market continued to deteriorate, strengthening the relationship between ability-to-repay

[29]Standard errors are calculating using the delta method and assuming that the covariance between default probability and quantity adjustment estimates is zero.

[30]This default rate was calculated using the sample of loans for which performance information is still available after 60 months. This means that loans prepaid prior to that time are excluded from the calculation. Including these loans in the denominator would reduce the default rate after five years to 11 percent.

and default (Foote et al., 2010). However, in all cases, the improvement in the default rate even after five years is minimal relative to the overall average default rates experienced during this time. This is not surprising considering that the number of loans shifted or lost as a result of the policy only constitutes around five percent of the total market. Furthermore, the improvement in loan performance associated with shifting a given loan across the 43 percent threshold is small relative to the aggregate default rate. While further reductions in default might be possible if the policy reduced the DTI limit further, our estimates suggest this would require substantial movements in mortgage quantities.

One potential concern with these results is that they rely on the implicit assumption that the relationship between DTI and default is policy invariant. However, it is possible that the change in policy actually changes the nature of the relationship between DTI and default. For example, if the policy causes lenders to put more work into verifying income and debt, then DTI may become a stronger predictor of default going forward. This would mean that the slope of the relationship we estimate between DTI and default is too flat, which would lead us to underestimate the effect on the aggregate default rate. We address this issue in Appendix A.1 by allowing the relationship between DTI and default to vary with loan documentation. We show that even among a sample of "full-doc" loans, for which DTI is more accurately recorded, the relationship between DTI and default is not strong enough to generate meaningful improvements in the aggregate default rate. Morever, even in an extreme scenario, where we assume that high-DTI loans are 10 percentage points more likely to default relative to the middle-DTI loans and that low-DTI loans are 20 percentage points less likely to default, we are only able to generate a reduction of 0.7 percentage points in the aggregate default rate. This leads us to conclude that the DTI limit, even in its fullest implementation, would likely have resulted in only minimal improvements in mortgage market performance had it been in effect during the run-up to the financial crisis.

A final, important limitation of this exercise is that we are unable to evaluate a number of other features of the ATR/QM rules, such as restrictions on complex products, that may have had important effects on loan performance. Our results suggest that it is those restrictions, not policies directed at DTI, that must have large effects on performance in order for the policy to meaningfully affect mortgage market stability.

VII Conclusion

In the wake of the deepest financial crisis since the Great Depression and the role played in it by household leverage, policies to limit household debt have received substantial interest and support, both in academic and policy spheres. In this paper we provide the first quantitative evaluation of a central U.S. policy, the Dodd-Frank ATR/QM rule, intended to regulate household leverage in the mortgage market. The policy operates by increasing lenders' risk of legal liability when originating high-leverage, potentially-risky mortgages. We find that lenders price this additional risk at a relatively low premium, increasing the cost to borrowers of high-leverage mortgages by roughly 10–15 basis points ($1,700–2,600 in additional interest expenses for the average mortgage in our sample). However, despite this relatively small market-priced cost of the regulation, we find that the policy had large effects on the distribution of leverage within the mortgage market. In the year following the implementation of the policy, as much as 15 percent of the affected market segment disappeared entirely and 20 percent of affected loans experienced a reduction in leverage. We interpret this as evidence that lenders responded to the policy primarily by rationing credit, which may have important implications for the design of similar policies targeting household leverage in other contexts. Finally, while the policy was able to achieve large changes in the distribution of debt-to-income, we estimate that this would have caused only a minimal reduction in the aggregate default rate, which raises doubts about the efficacy of similar restrictions on household leverage for improving financial stability.

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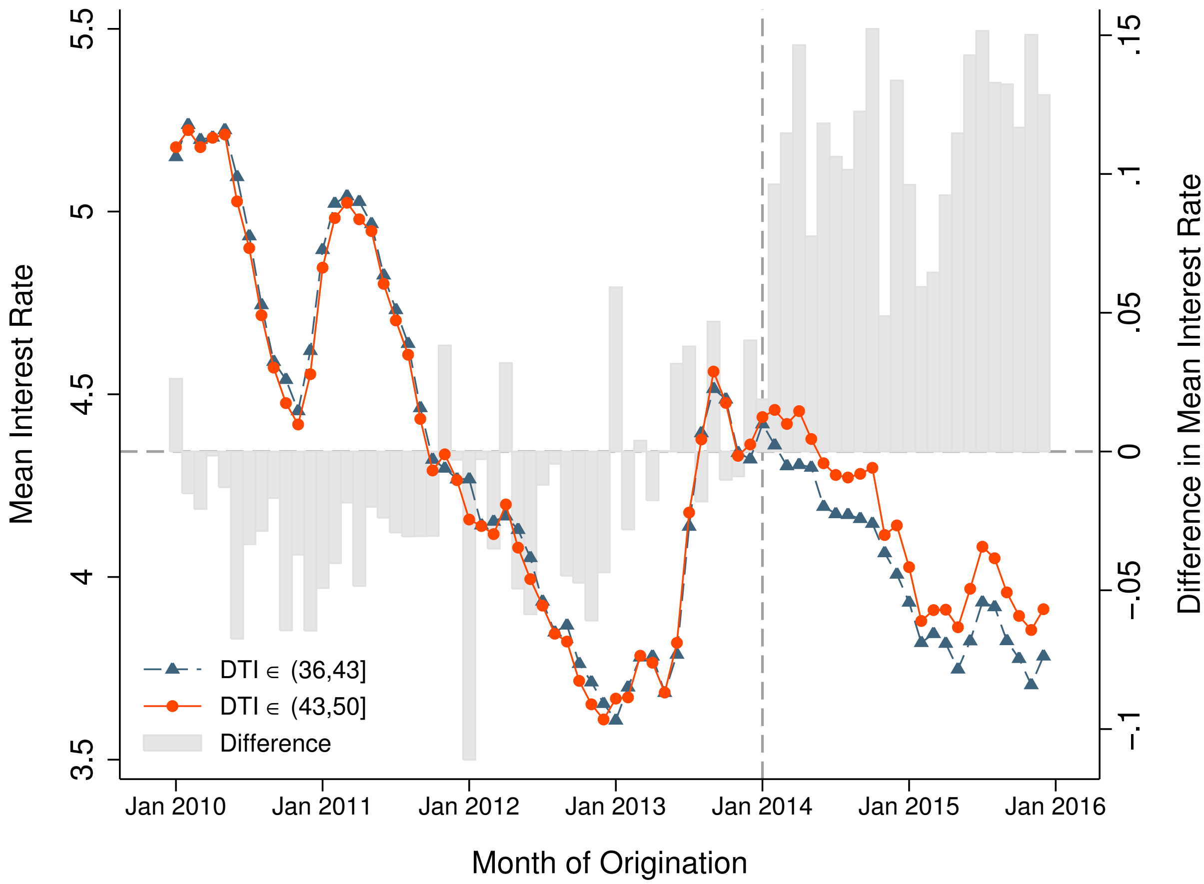


FIGURE I

Mean Interest Rates by Origination Month for High-vs-Low DTI Jumbo Loans

NOTE.—This figure plots mean interest rates by origination month separately for high-DTI jumbo loans (orange circles) and low-DTI jumbo loans (blue triangles). Each dot represents the raw average interest rate for loans originated in the indicated month, measured on the left axis. The month-by-month difference in interest rates between high- and low-DTI loans is also plotted in grey bars and measured on the right axis. The vertically dashed grey line marks the month that the Ability-to-Repay Rule and Qualified Mortgage Standards went into effect (January 2014). Means are calculated using the sample of all jumbo loans with DTIs between 36 and 50 percent described in Section III.

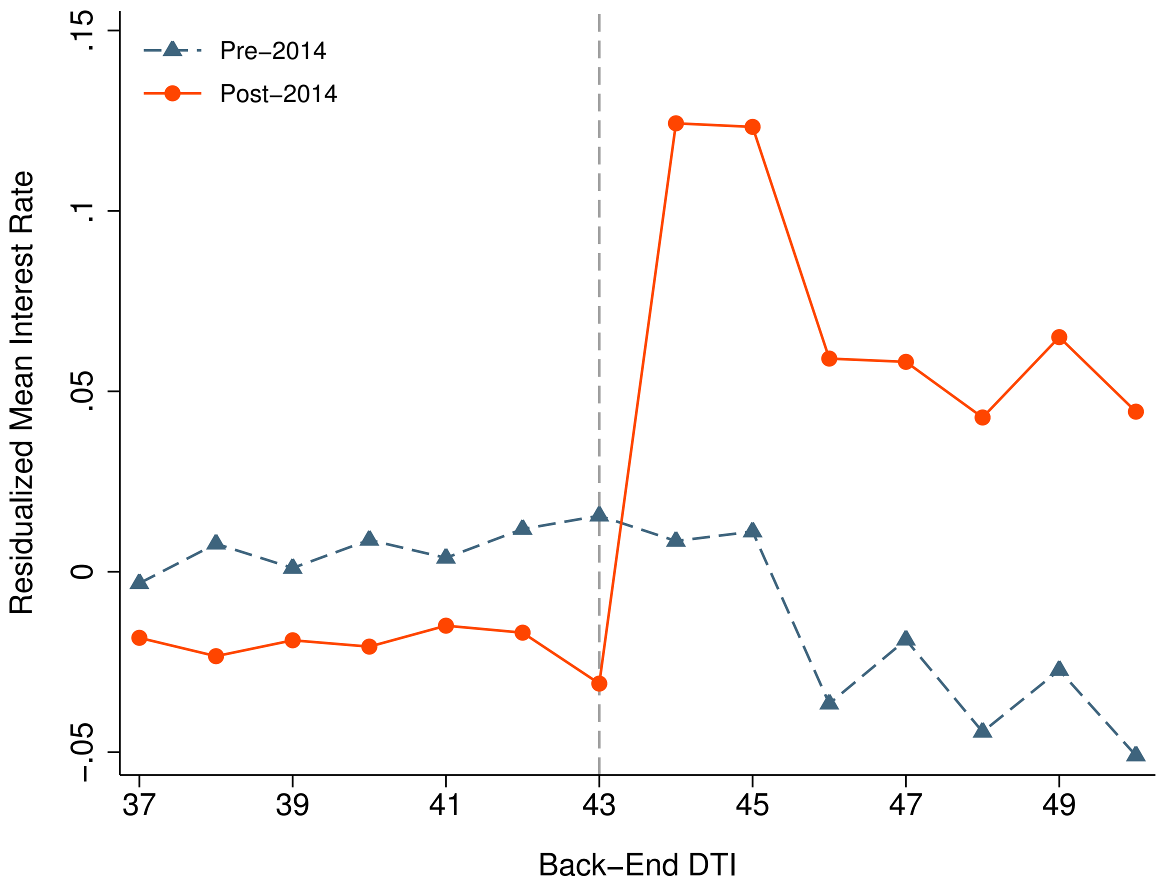


FIGURE II

Detrended Mean Interest Rates by DTI for Jumbo Loans Originated Pre- and Post-ATR/QM

NOTE.—This figure plots detrended mean interest rates by DTI separately for jumbo loans originated before (blue triangles) and after (orange circles) the implementation of ATR/QM. To create the figure, we regress the interest rate on a series on origination month dummies and then average the residuals of this regression within each one percent DTI bin separately for loans originated before and after January 2014. Each dot in the figure plots the mean of the residuals from this regression for the corresponding DTI bin and time period. The vertically dashed grey line marks the QM threshold of 43 percent. DTI bins are created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent. Detrended means are calculated using the sample of all jumbo loans with DTIs between 36 and 50 percent described in Section III.

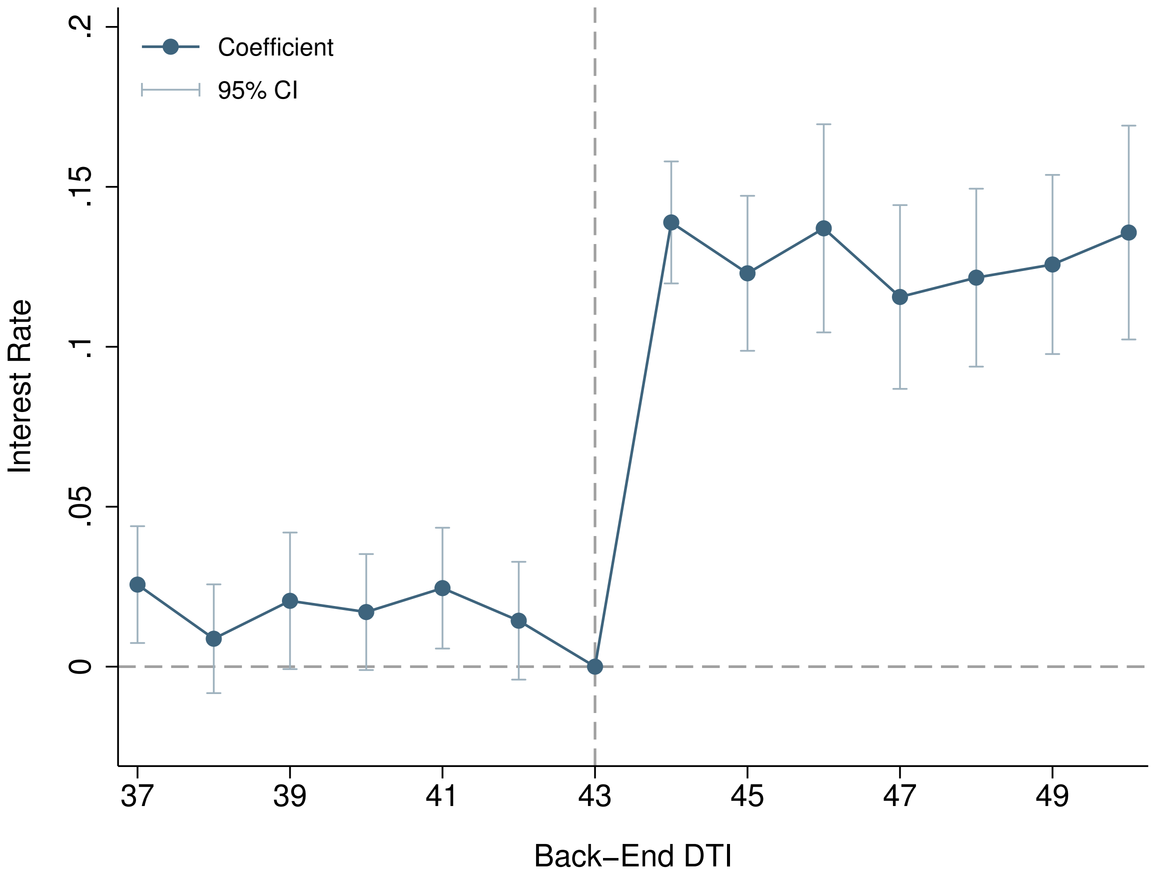


FIGURE III

Flexible Difference-in-Differences Estimates of the Effect of Non-QM Status on Interest Rates

NOTE.—This figure plots estimates of the effect of non-Qualified Mortgage status on interest rates derived from a flexible difference-in-differences specification that allows the effect to vary with the borrower's DTI. Estimates were constructed by regressing the interest rate on an indicator for whether the loan was originated after the implementation of ATR/QM and the interaction of that indicator with a series of dummies reflecting the borrower's DTI. The vertically dashed grey line marks the QM threshold of 43 percent. The DTI dummies were created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent. DTI-bin $d = 43$ is the omitted category, so that all coefficient estimates can be interpreted as the change in interest rates in a given DTI bin following the implementation of ATR relative to the corresponding change in rates for loans with DTIs just below the QM threshold. The regression also included fixed effects for the month of origination, the county the property was located in, the type of property as well as 20-point FICO score bins, 5-point LTV bins and the pairwise interaction between the two. The 95 percent confidence intervals are based on standard errors that were clustered at the county level.

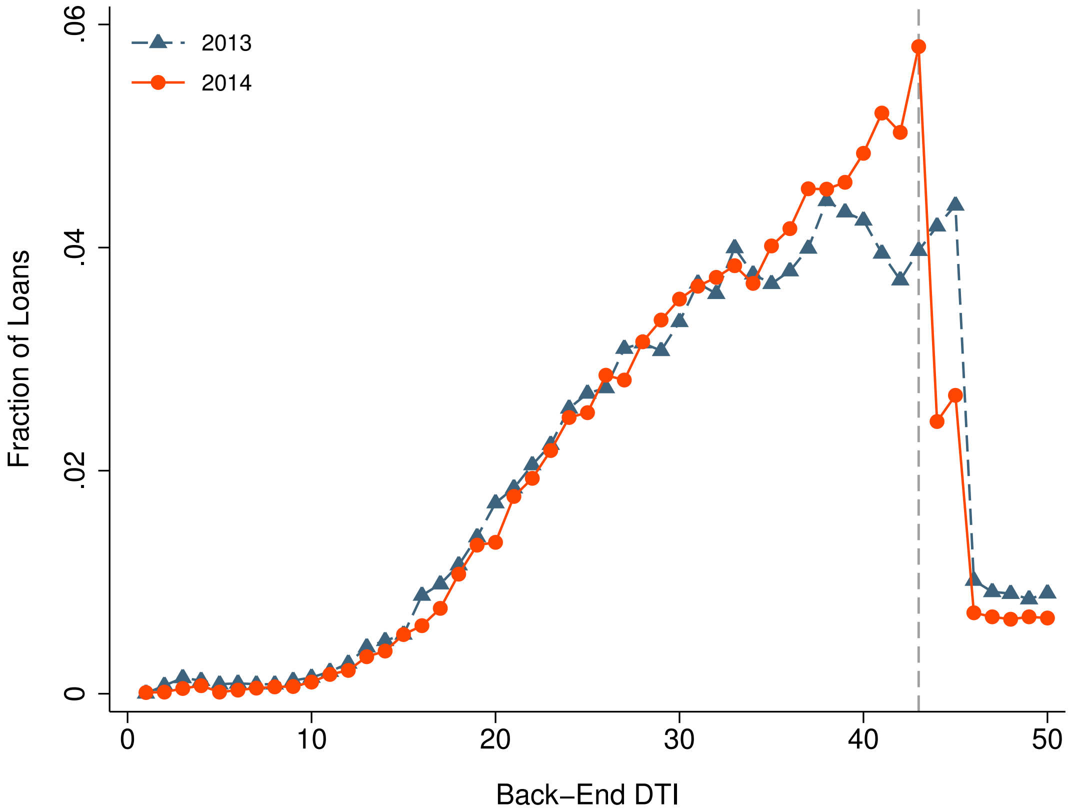
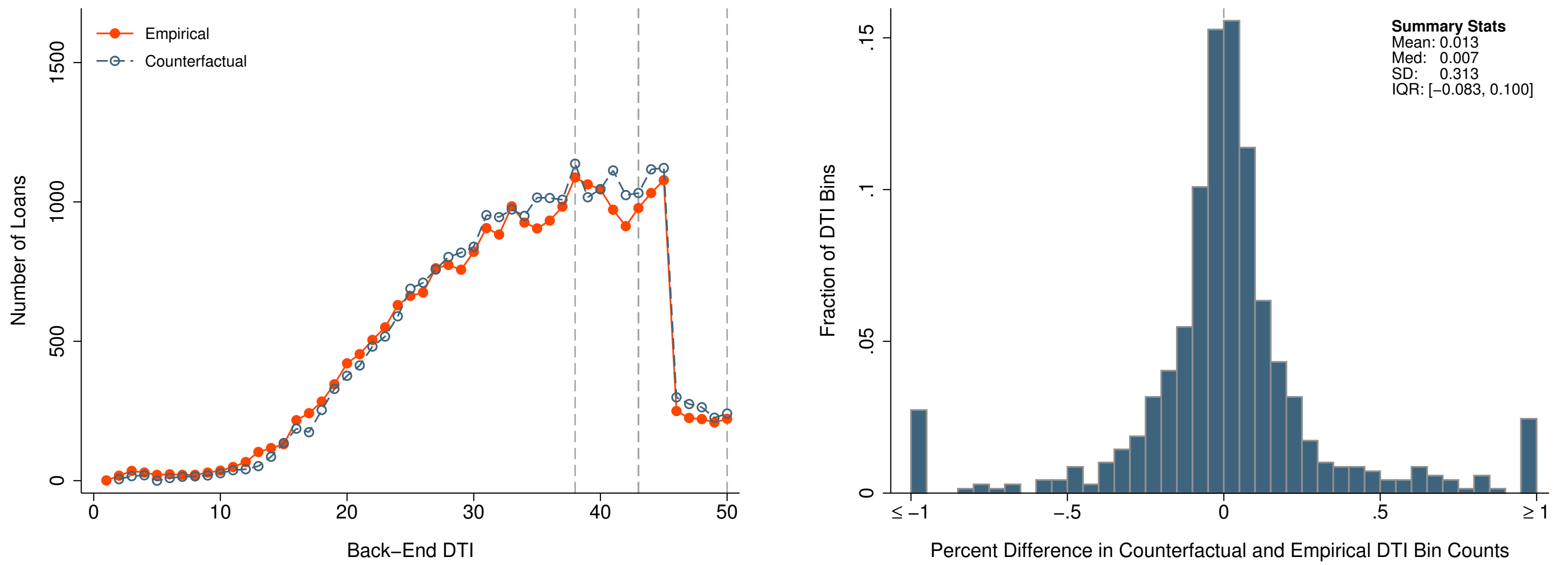


FIGURE IV
DTI Distribution among Jumbo Mortgages

NOTE.—This figure plots the distribution of DTI among jumbo mortgages separately for loans originated in 2013 (blue triangles) and 2014 (orange circles). Each dot represents the share of all mortgages originated in the indicated year for which the back-end DTI at origination fell into the one-percent bin indicated on the x-axis. The vertically dashed grey line marks the QM threshold of 43 percent. DTI bins are created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent. Total originations and shares by DTI were calculated using all jumbo loans contained in analysis sample described in Section III.



Panel A. Single Placebo Year (2013)

Panel B. All Placebo Years (2000–2013)

FIGURE V

Comparison of the Empirical and Counterfactual Jumbo DTI Distributions in Placebo Policy Years

NOTE.—This figure reports results from a comparison of the empirical and counterfactual jumbo DTI distributions for a series of placebo policy years. Panel A. plots the empirical (solid orange circles) and counterfactual distribution (hollow blue circles), treating 2013 as the placebo year. The counterfactual distribution was generated as described in Section V.A using 2012 as the pre-period. The vertically dashed grey lines mark the lower limit of the bunching region ($\overline{d} = 38$), the QM-threshold, and the maximum DTI. Each dot represents the number of mortgages for which the back-end DTI at origination fell (or is estimated to have fallen) into the one-percent bin indicated on the x-axis. DTI bins are created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent. Panel B. summarizes the difference between the empirical and counterfactual distributions across all placebo policy years, 2000–2013. For each placebo year, we generate a corresponding estimate of the counterfactual DTI distribution as in Panel A. We then calculate the percent difference between the empirical and counterfactual number of loans in each DTI bin for each year and plot the distribution of these differences across all DTI bins and years. The mean, median, standard deviation, and interquartile range of this distribution are also reported in the top right corner for reference. We use a bin width of 0.05 and winsorize the percent differences at 1 and -1 (100 and -100 percent) for visual clarity.

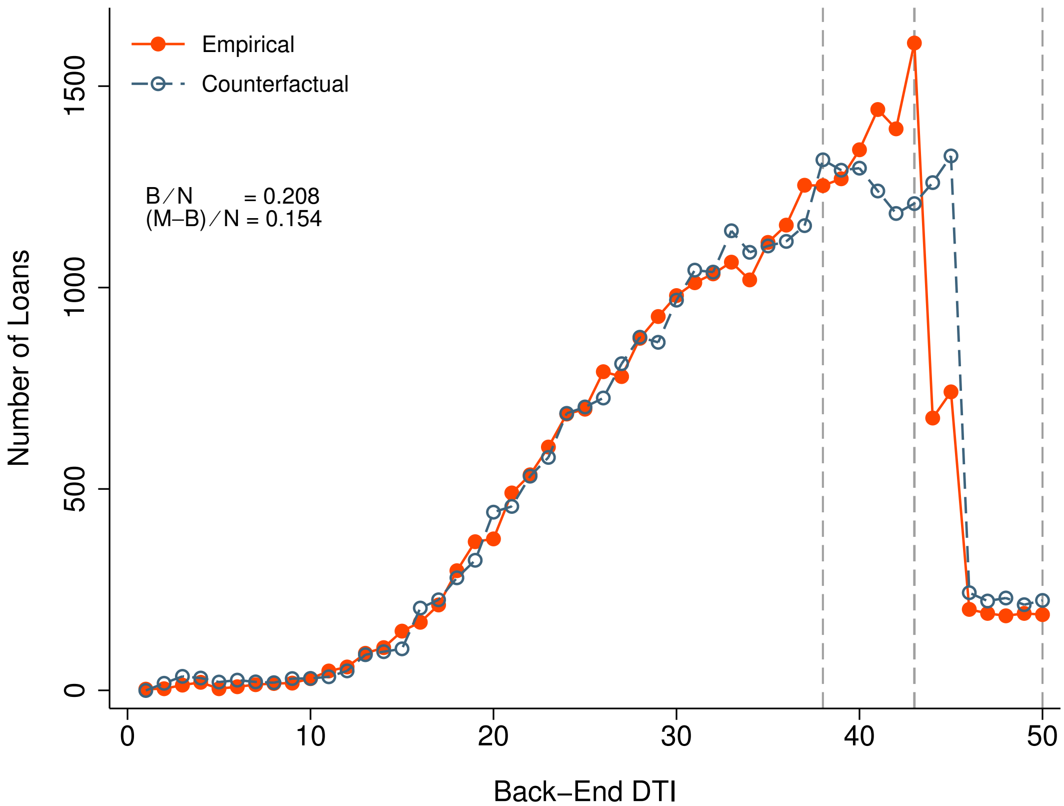
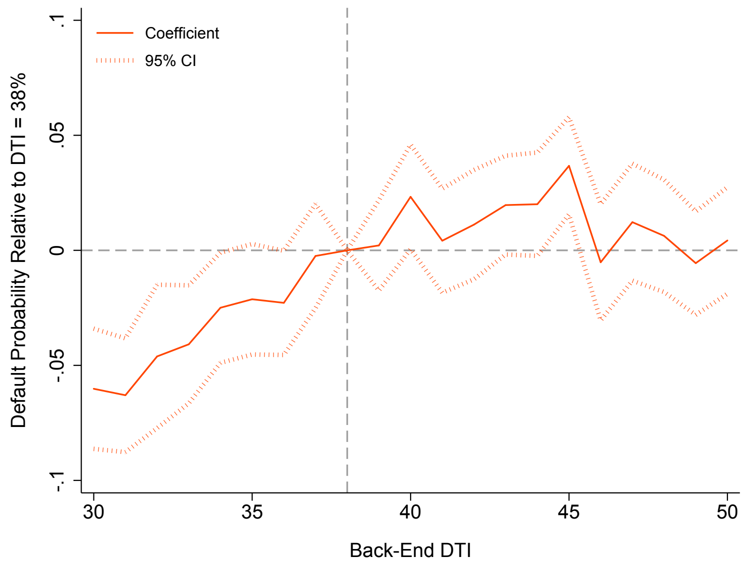


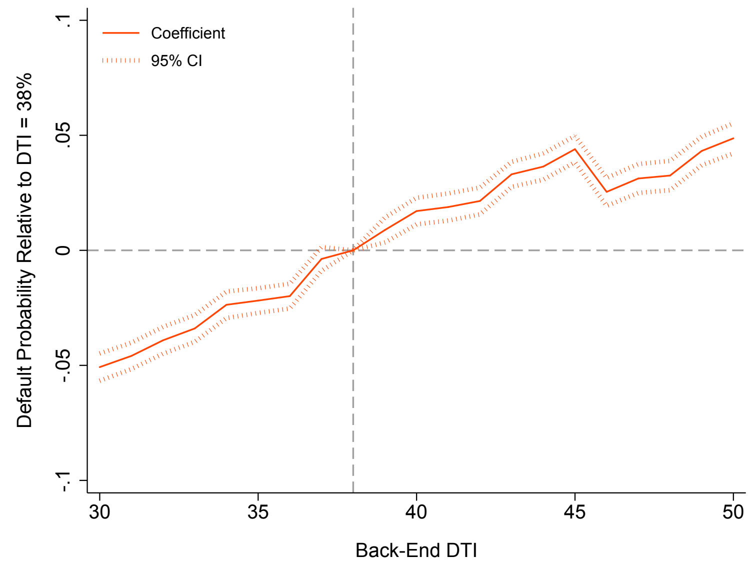
FIGURE VI

Bunching, Missing Mass, and the Effect of ATR/QM on the Quantity of Credit

NOTE.—This figure plots the empirical and counterfactual DTI distribution for jumbo mortgages originated in 2014, the first year that ATR/QM was in effect. The solid orange connected line is the empirical distribution. Each dot represents the number of loans originated in 2014 for which the borrower's DTI fell into the one-percent bin indicated on the x-axis. DTI bins are created by rounding up to the nearest integer so that the 43 percent bin includes all DTIs greater than 42 percent and less than or equal to 43 percent. The dashed blue connected line plots the counterfactual, which was estimated as described in Section V.A using 2013 as the pre-period. The vertically dashed grey lines mark the lower limit of the bunching region ($\overline{d} = 38$), the QM-threshold, and the maximum DTI. The figure also reports the implied intensive and extensive margin quantity effects (B/N and $(M-B)/N$), calculated as described in Section V.A.



Panel A. Jumbo Loans Only

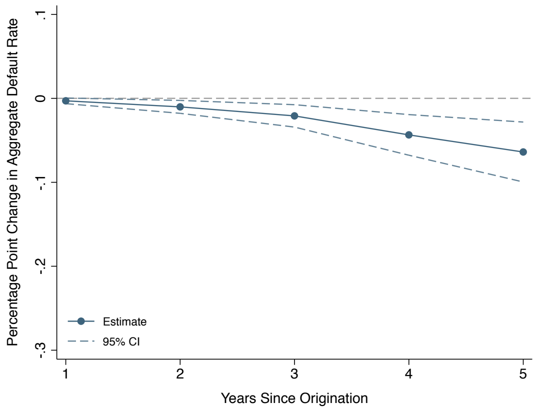


Panel B. All Loans

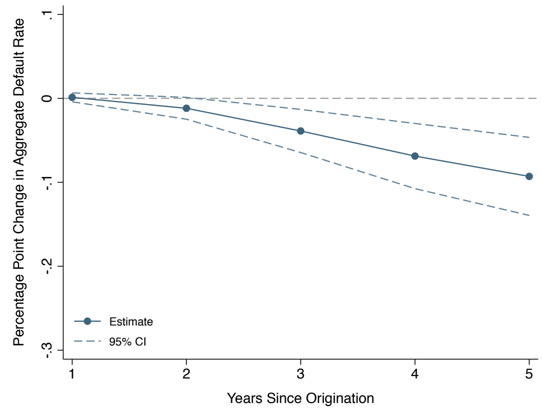
FIGURE VII

Relationship between DTI and Five-Year Default Probability (2005–2008)

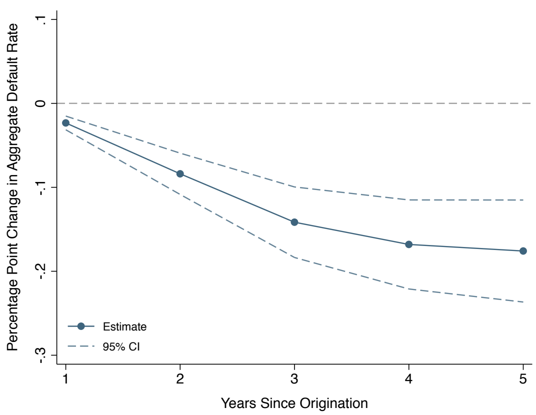
NOTE.— This figure plots the empirical relationship between DTI at origination and the probability of default for loans originated during 2005–2008. Panel A is constructed using a sample of jumbo mortgages only, whereas Panel B is based on a sample of both jumbo and conforming mortgages. Each panel reports the coefficient estimates from a regression of whether a loan defaulted on a series of dummy variables indicating whether the loan's DTI fell into a given one-percent bin. We define a loan as having defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within five years of the origination date. We omit the dummy for $DTI = 38$, which is the lower limit of the bunching region in our preferred specification for the quantity effect. DTI bins are created by rounding up to the nearest integer so that the 38 percent bin includes all DTIs greater than 37 percent and less than or equal to 38 percent. The regressions also included fixed effects for the month of origination, the county the property was located in, the type of property as well as 20-point FICO score bins, 5-point LTV bins and the pairwise interaction between the two. The 95 percent confidence intervals are based on standard errors that were clustered at the county level.



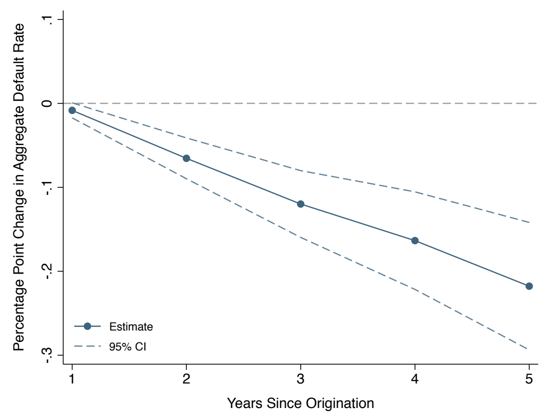
Panel A. 2005



Panel B. 2006



Panel C. 2007



Panel D. 2008

FIGURE VIII

Estimated Effect of ATR/QM on Aggregate Default Rates by Year of Origination

NOTE.— This figure plots the estimated counterfactual effect of the ATR/QM rule on the aggregate default rate for loans originated in 2005–2008 assuming that the policy was in effect and extended to the entire market during that period. Each panel reports results for a separate origination year cohort and for default rates defined over one to five year horizons. For a given horizon, we consider a loan to have defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within that horizon. Estimates were constructed as described in Section VI using information on the relative probability of default for high- and low-DTI loans, the effect of the policy on the distribution on DTIs, and the observed DTI distribution in each year. The 95 percent confidence intervals were calculated using the delta method and assuming that estimates of the effect of the policy on the DTI distribution are uncorrelated with estimates of the relative default probabilities.

TABLE I
LOAN-LEVEL DESCRIPTIVE STATISTICS

	Full Sample			DTI $\in (36, 50]$		
	All Loans	Conforming	Jumbo	All Loans	Conforming	Jumbo
Loan Amount ($1000's)	264.58	212.91	641.53	267.37	215.96	637.06
	(189.75)	(101.26)	(249.59)	(187.31)	(101.23)	(240.84)
FICO Score	756.12	754.50	767.91	751.09	749.03	765.93
	(43.27)	(44.44)	(31.09)	(44.14)	(45.18)	(32.06)
Loan-to-Value	80.34	80.81	76.96	80.51	81.06	76.55
	(13.89)	(14.26)	(10.24)	(13.95)	(14.32)	(10.04)
Back-End Debt-to-Income	33.39	33.36	33.58	41.80	41.85	41.44
	(9.04)	(9.10)	(8.61)	(3.51)	(3.52)	(3.39)
Interest Rate	4.29	4.31	4.19	4.34	4.36	4.24
	(0.56)	(0.56)	(0.54)	(0.54)	(0.54)	(0.54)
Number of Observations	1,195,895	1,051,730	144,165	513,939	451,191	62,748

NOTE.—This table presents loan-level descriptive statistics for both the full analysis sample (columns 1–3) and the restricted sample of loans with DTIs in a symmetric window around the QM-threshold of 43 percent used in the interest rate analysis (columns 4–6). The sample includes all first-lien, conventional (non-FHA), 30-year, fixed-rate, purchase mortgages originated between January 2010 and December 2015 for which CoreLogic reports a non-missing FICO, LTV, DTI, interest rate, appraisal amount and geographic identifier. All table entries represent sample means or, in parentheses, standard deviations. Summary statistics are presented pooling across all loan types (columns 1 and 4) as well as separately for conforming (columns 2 and 5) and jumbo (columns 3 and 6) loans. See Section III for further details on data sources and sample construction.

TABLE II

THE EFFECT OF NON-QUALIFIED MORTGAGE STATUS ON INTEREST RATES

	Difference-in-Differences				Triple Difference			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DTI > 43	-0.018***	-0.017***	-0.004	-0.004	0.003	0.003	0.019***	0.019***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
DTI > 43 × Post	0.131***	0.141***	0.119***	0.113***	0.008***	0.008***	0.009***	0.006***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.003)	(0.003)	(0.002)	(0.002)
DTI > 43 × Jumbo					-0.021***	-0.022***	-0.033***	-0.032***
					(0.006)	(0.006)	(0.005)	(0.005)
DTI > 43 × Jumbo × Post					0.124***	0.128***	0.110***	0.108***
					(0.008)	(0.008)	(0.007)	(0.007)
Month (× Jumbo) FEs	X	X	X	X	X	X	X	X
County FEs		X	X	X		X	X	X
FICO × LTV Bin FEs			X	X			X	X
Property Type FEs				X				X
Implied %Δ	2.9%	3.2%	2.7%	2.5%	2.8%	2.9%	2.5%	2.4%
R-Squared	0.70	0.72	0.75	0.75	0.64	0.65	0.70	0.71
Number of Observations	62,748	62,748	62,748	62,748	513,939	513,939	513,939	513,939

NOTE.—This table reports difference-in-differences and triple difference estimates of the effect of non-Qualified Mortgage status on interest rates. Each column reports a separate regression estimated at the loan level where the dependent variable is the interest rate (expressed in percentage points). Columns 1–4 report estimates from a difference-in-differences regression estimated in the sample of jumbo loans with DTIs between 36 and 50 percent. Coefficient estimates are reported for the non-QM "treatment" dummy ($DTI > 43$) as well as its interaction with an indicator for whether the loan was originated in a month following the implementation of ATR/QM ($Post$). Columns 5–8 report analogous estimates from triple difference specifications estimated in the sample of *all* loans (jumbo and conforming) with DTIs between 36 and 50 percent. In these regressions, additional coefficient estimates are reported for the interaction between the $DTI > 43$ dummy and an indicator for whether the loan is a jumbo mortgage ($Jumbo$) as well as the triple interaction between the $DTI > 43$ dummy, the $Jumbo$ dummy, and the $Post$ indicator. The first row of the bottom panel reports the percentage increase in interest rates relative to the pre-period mean implied by the corresponding coefficient estimate reported in the second (columns 1–4) and fourth (columns 5–8) rows of the table. All specifications include fixed effects for the month of origination. In the triple difference specifications, these fixed effects are further interacted with the $Jumbo$ dummy. Columns 2 and 6 add fixed effects for the county that the property is located in. Columns 3 and 7 further include a full set of fixed effects for the borrower's FICO score (20-point bins), LTV (5-point bins), and the pairwise interaction between the two. Columns 4 and 8 include additional fixed effects for the property type (single family, condominium, townhouse, planned unit development). Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE III

INTENSIVE AND EXTENSIVE MARGIN EFFECTS OF ATR/QM ON THE QUANTITY OF CREDIT

	Preferred	Alternative Specifications		
	(1)	(2)	(3)	(4)
	$\overline{d}=38$	$\overline{d}=30$	$\overline{d}=35$	$\overline{d}=40$
$\hat{B}/\hat{N}_{44+}^{post}$	0.208***	0.188**	0.269***	0.228***
	(0.043)	(0.079)	(0.056)	(0.030)
$(\hat{M}-\hat{B})/\hat{N}_{44+}^{post}$	0.154***	0.180**	0.090	0.133***
	(0.051)	(0.086)	(0.063)	(0.039)
Bootstrap Replications	100	100	100	100
Number of Observations	418,105	418,105	418,105	418,105

NOTE.—This table reports estimates of the intensive and extensive margin effects of the Ability-to-Repay Rule and Qualified Mortgage standards on the quantity of credit in the jumbo mortgage market. The top row reports the estimated intensive margin effect of the regulation on the allocation of credit across the DTI distribution. Each estimate represents the fraction of jumbo loans in the counterfactual no-policy distribution that were shifted from a DTI above the QM-threshold of 43 percent to below the threshold. The second row reports the estimated extensive margin effect of the policy on the total number of jumbo mortgages originated. Each estimate represents the fraction of the counterfactual number of jumbo loans that were eliminated as a result of the policy. Intensive and extensive margin effects were calculated using the bunching procedure described in Section V.A. Column one reports our preferred estimates, which set the lower limit of the bunching region to $\overline{d}=38$. Columns 2–4 report analogous estimates from alternative specifications which set this limit to 30, 35, and 40 percent respectively. All specifications use 2013 as the pre-period and 2014 as the post-period. The sample therefore includes all jumbo loans that were originated in either 2013 or 2014. Standard errors are reported in parentheses and are calculated by bootstrapping from the observed sample of mortgages, drawing 100 random samples with replacements and re-estimating the parameters at each iteration. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE IV

ESTIMATES OF THE EFFECT OF DTI ON THE FIVE-YEAR PROBABILITY OF DEFAULT

	(1)	(2)	(3)	(4)
	2005	2006	2007	2008
	Panel A. Jumbo Loans Only			
DTI $\leq$ 38	-0.0303***	-0.0555***	-0.0910***	0.0025
	(0.0060)	(0.0105)	(0.0086)	(0.0381)
DTI $>$ 43	0.0034	-0.0112	0.0002	0.0779**
	(0.0062)	(0.0081)	(0.0082)	(0.0365)
Implied Aggregate Effect	-0.0005*	-0.0001	-0.0010**	-0.0033**
	(0.0003)	(0.0004)	(0.0005)	(0.0014)
Number of Observations	31,529	18,646	17,155	1,186
	Panel B. All Loans			
DTI $\leq$ 38	-0.0330***	-0.0508***	-0.0689***	-0.0706***
	(0.0018)	(0.0026)	(0.0028)	(0.0038)
DTI $>$ 43	0.0062***	0.0083***	0.0228***	0.0320***
	(0.0022)	(0.0025)	(0.0029)	(0.0044)
Implied Aggregate Effect	-0.0006***	-0.0009***	-0.0018***	-0.0022***
	(0.0002)	(0.0002)	(0.0003)	(0.0004)
Number of Observations	353,392	330,550	295,674	91,493

NOTE.— This table reports estimates of five-year default probabilities for high-DTI and low-DTI loans relative to loans in the omitted category $DTI \in (38, 43]$. The relationship between DTI and default probability is estimated separately by origination year cohort and loan type. Panel A. reports results for jumbo loans only whereas Panel B. pools across all loans. The third row of each panel also reports the implied counterfactual effect of the ATR/QM rule on the aggregate default rate for a given origination year cohort and loan type estimated as described in Section VI. Estimates of the relative default probabilities are derived from a regression of whether a loan defaulted on DTI-bin dummies, fixed effects for the month of origination, the county the property was located in, the type of property as well as 20-point FICO score bins, 5-point LTV bins and the pairwise interaction between the two. We define a loan as having defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within five years of the origination date. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Regulating Household Leverage

Online Appendices

Anthony A. DeFusco   Stephanie Johnson   John Mondragon

A.1 Additional Results and Robustness Checks

A.1.A Substitution Into the Conforming Market

Our approach to estimating the counterfactual DTI distribution in the absence of the ATR/QM regulation relies on the assumption that the distribution among conforming loans was unaffected by the policy (Assumption 1). However, one way to avoid taking a non-QM loan while still maintaining a high DTI would be to substitute into the conforming market. This substitution may be optimal for borrowers with high DTIs and loan amounts that are only slightly larger than the conforming limit. If this type of substitution were prevalent, it may lead us to over-estimate the intensive margin effect of ATR/QM on the DTI distribution and under-estimate the extensive margin effect since our estimate of the counterfactual distribution would feature too many loans above the 43 percent threshold and too few below it.

To gauge the extent to which this bias may be affecting our results, we look to see if there were changes in the amount of "bunching" at the conforming loan limit among high- relative to low-DTI loans after the policy was put into effect. The easiest way for a high-DTI jumbo borrower to substitute into the conforming market would be to decrease her loan size by the minimum amount required to qualify as conforming. Therefore, if high-DTI borrowers are shifting into the conforming market we should see an increase in the amount of bunching at the conforming limit among high-DTI loans relative to low-DTI loans subsequent to the policy change.

To measure the amount of bunching at the conforming limit we follow the approach in Kleven and Waseem (2013). For a given sample of loans, we first center each loan at the conforming limit in the year that the loan was originated. A value of zero thus represents a loan size exactly equal to the conforming limit. We then group these normalized loan amounts into $5,000 bins with upper limits equal to m_j ($j = -J, \ldots, L, \ldots, 0, \ldots, U, \ldots, J$), and count the number of loans in each bin, n_j. To obtain estimates of bunching and the counterfactual loan size distribution, we define an excluded region around the conforming limit, $[m_L, m_U]$, such that $m_L < 0 < m_U$ and fit the following regression to the count of loans in each bin

$$n_j = \sum_{i=0}^{5} \beta_i (m_j)^i + \sum_{k=L}^{U} \gamma_k \mathbb{1}\left(m_k = m_j\right) + \epsilon_j. \tag{A.1.1}$$

The first term on the right hand side is a 5-th degree polynomial in loan size and the second term is a set of dummy variables for each bin in the excluded region. Our estimate of the counterfactual distribution is given by the predicted values of this regression omitting the effect of the dummies in the excluded region. That is, letting $\hat{n}_j$ denote the estimated counterfactual number of loans in bin j, we can write

$$\hat{n}_j = \sum_{i=0}^{p} \hat{\beta}_i (m_j)^i. \tag{A.1.2}$$

Bunching is then estimated as the difference between the observed and counterfactual bin counts in the excluded region at and to the left of the conforming loan limit,

$$\hat{B} = \sum_{j=L}^{0} \left(n_j - \hat{n}_j\right) = \sum_{j=L}^{0} \hat{\gamma}_j, \tag{A.1.3}$$

while the amount of missing mass due to bunching is $\hat{M} = \sum_{j>0}^{U} \left(n_j - \hat{n}_j\right) = \sum_{j>0}^{U} \hat{\gamma}_j$. We set the lower limit of the excluded region to $-\$10,000$, and choose the upper limit to minimize the difference between bunching and missing mass to the right of the conforming limit in the excluded region. Standard errors are calculated using a bootstrap procedure as in Chetty et al. (2011).[1]

Figure A.1.2 reports results from this exercise. Each panel plots the observed loan size distribution and our estimate of the counterfactual for a given sample of loans. The top row includes all loans regardless of DTI, with the columns distinguishing between loans originated before ATR/QM (2013) and loans originated afterward (2014). The second row includes only loans with DTIs in the region just below the 43 percent cutoff. We use the same DTI bins that we used to estimate the quantity effect in Section V, so that this row includes all loans with $DTI \in (38, 43]$. Similarly, the third row reports results for loans with DTIs strictly above the 43 percent cutoff. Each panel also reports an estimate of the amount of "excess mass" at the conforming limit, which we measure as the ratio of the number of extra loans bunching at the limit relative to the predicted counterfactual number of loans in that region, scaled by the width of the bin to convert to a density. For example, the excess mass of 6.79 reported in the top left panel implies that there was roughly 6.79 times more mass at the conforming limit in 2013 than would have otherwise been expected. This reflects the underlying incentive to bunch at the limit documented by DeFusco and Paciorek (2017), which results from differences in interest rates and underwriting standards that apply to jumbo loans even in the absence of ATR/QM.

Between 2013 and 2014 the overall amount of bunching decreased in all samples of loans, possibly reflecting the reduction in the interest rate spread on jumbo loans relative to conforming loans during this period. Importantly, however, this decrease was equally pronounced among high-DTI loans and loans with DTIs just below the QM threshold. Excess mass decreased by

[1] At each iteration (k) of the bootstrap loop we draw with replacement from the estimated errors, ϵ_j, in equation (A.1.1) to generate a new set of bin counts, n_j^k. We then re-estimate bunching using these new counts. Our estimate of the standard error for a given parameter is the standard deviation of the estimates across these bootstrap replications.

roughly 16.5 percent (from 6.70 to 5.60) among low-DTI loans and by 18 percent (from 6.32 to 5.18) among high-DTI loans. If high-DTI jumbo borrowers were differentially substituting into the conforming market after the policy, then we would have expected the decrease in bunching in the high-DTI market to be substantially less than that in the low-DTI market, where there is no extra incentive to bunch due to ATR/QM. If anything, we document that the decrease in bunching for high-DTI loans was slightly larger. We take this as fairly strong evidence in favor of our assumption that the DTI distribution among conforming loans was not materially affected by the policy.

A.1.B Documentation Status and the Relationship between DTI and Default

One potential concern with the performance results reported in Section VI is that they rely on the implicit assumption that the relationship between DTI and default is policy invariant. However, it is possible that the implementation of ATR/QM actually led to a change the nature of the relationship between DTI and default. For example, if the policy causes lenders to put more work into verifying income and debt, then DTI may become a stronger predictor of default going forward. This would mean that the slope of the relationship we estimate between DTI and default is too flat, which would lead us to underestimate the effect on the aggregate default rate.

To address this issue, we explore whether the relationship between DTI and default changes meaningfully with loan documentation status. To do so, we re-estimate the results reported in Panel B., column 4 of Table IV for the 2008 loan cohort separately by documentation status. These results are reported in Table A.1.1. The first column simply repeats the results from Table IV for reference. The second column restricts the analysis to the subset of loans that CoreLogic reports as having "full documentation." This sample should be reflective of the relationship between DTI and default in a scenario in which lenders are carefully verifying the borrowers income and debts. For completeness, the third column also reports results for the sample of "low documentation" loans. Comparing across columns, it is clear that our results do not depend on documentation status. While the implied reduction in the aggregate default rate is slightly larger if we use the relationship between DTI and default from the full-doc sample, the difference is statistically insignificant and economically minimal. This leads us to believe that even if ATR/QM led to an increase in the level of documentation and verification that lenders perform, our qualitative conclusion would remain the same. The relationship between DTI and default is simply not strong enough to generate meaningful improvements in the aggregate default rate given the share of loans that we estimate were affected by the policy.

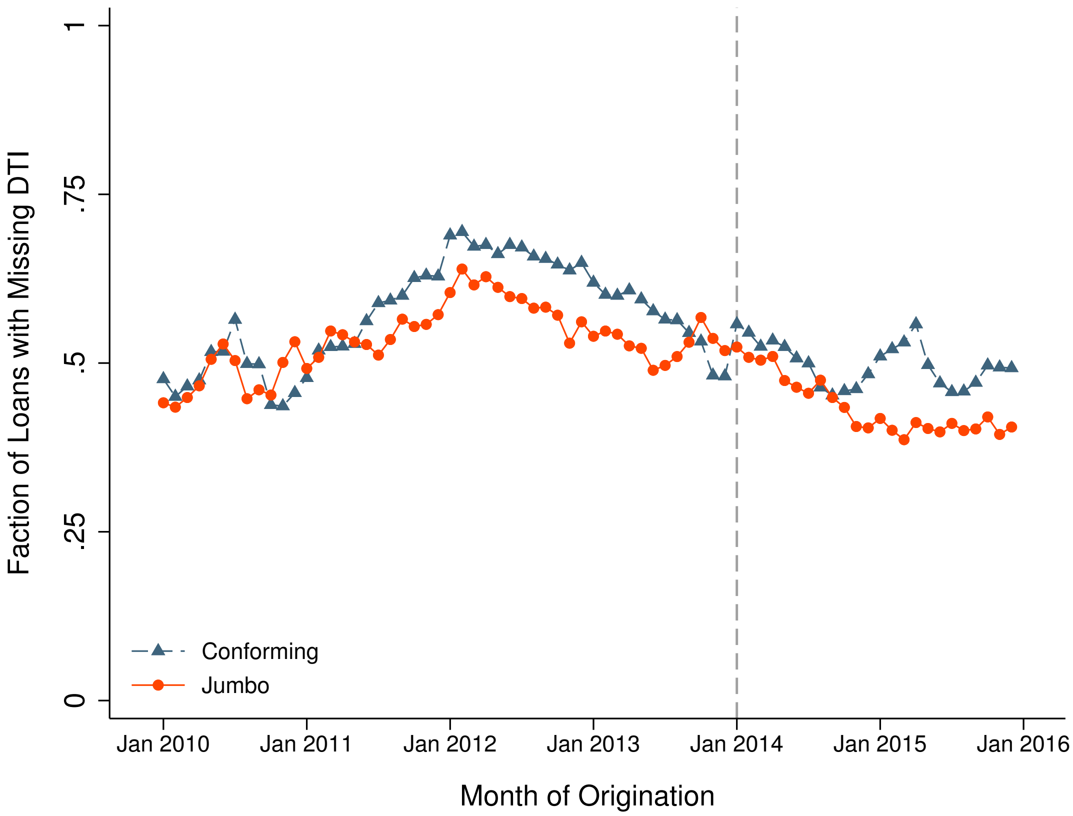


FIGURE A.1.1
Fraction of Loans with Missing DTI by Month of Origination

NOTE.—This figure plots the share of loans that are dropped from our analysis sample due to having a missing DTI. Shares are reported separately for jumbo and conforming loans and by month of origination. The vertically dashed grey lines marks the month that the Ability-to-Repay Rule and Qualified Mortgage Standards went into effect (January 2014).

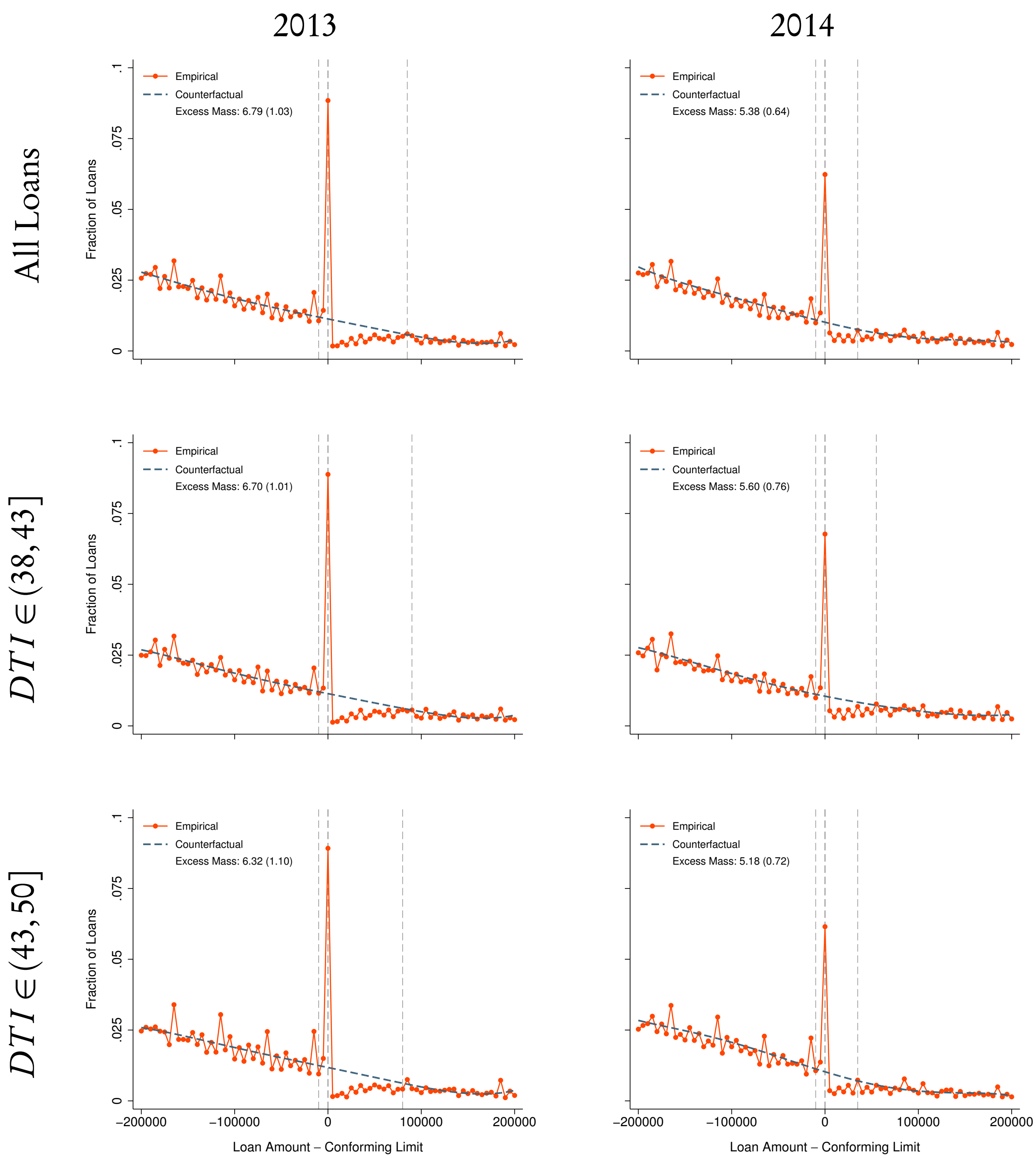


FIGURE A.1.2
Bunching at the Conforming Limit before and After ATR/QM

NOTE.—This figure plots the empirical and counterfactual density of loan size relative to the conforming limit by origination year and borrower DTI. In each panel, the connected line plot represents the fraction of loans in a given \$5,000 bin relative to the conforming limit in effect at the time of origination. The heavy dashed line is the estimated counterfactual density obtained by fitting a 5th degree polynomial to the bin counts, omitting the contribution of the bins in the region marked by the vertical dashed gray lines. The figure also reports the estimated excess mass at the conforming limit and its standard error, calculated as described in Appendix A.1.A.

TABLE A.1.1

ESTIMATES OF THE EFFECT OF DTI ON THE 2008 FIVE-YEAR PROBABILITY OF DEFAULT

	All Loans	Full Documentation	Low Documentation
	(1)	(2)	(3)
DTI $\leq$ 38	-0.0706***	-0.0709***	-0.0695***
	(0.0038)	(0.0052)	(0.0061)
DTI $>$ 43	0.0320***	0.0384***	0.0227***
	(0.0044)	(0.0056)	(0.0069)
Implied Aggregate Effect	-0.0022***	-0.0025***	-0.0018***
	(0.0004)	(0.0004)	(0.0004)
Number of Observations	91,493	58,748	30,415

NOTE.— This table reports estimates of five-year default probabilities for high-DTI and low-DTI loans relative to loans in the omitted category $DTI \in (38,43]$ for loans originated in 2008. The relationship between DTI and default probability is estimated separately by loan documentation status and includes both jumbo and conforming loans. Column 1 reports results pooling across all loans. Column 2 restricts to a sample of full documentation loans and column 3 reports results for low documentation loans only. The third row of each column also reports the implied counterfactual effect of the ATR/QM rule on the aggregate default rate estimated as described in Section VI. Estimates of the relative default probabilities are derived from a regression of whether a loan defaulted on DTI-bin dummies, fixed effects for the month of origination, the county the property was located in, the type of property as well as 20-point FICO score bins, 5-point LTV bins and the pairwise interaction between the two. We define a loan as having defaulted if the borrower was ever more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within five years of the origination date. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.