

Regulating Household Leverage

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This article 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% of the affected market completely and reduced leverage for another 20% of remaining borrowers. This reduction in quantities is much greater than would be implied by plausible demand elasticities and indicates that lenders responded to the policy not only by raising prices but also by exiting the regulated portion of the market. Heterogeneity in the quantity response across lenders suggests that agency costs may have been one particularly important market friction contributing to the large overall effect as the fall in lending was substantially larger among lenders relying on third-parties to originate loans. 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.

Key words: Household leverage, Financial regulation, Macroprudential policy, Mortgage markets

JEL Codes: G18, D14, D18, E60, R30

1. INTRODUCTION

Household leverage played a central role during the global financial crisis of 2007–9. 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 (Mian and Sufi, 2011; Eggertsson and Krugman, 2012; 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* focus and sought to decrease the likelihood of future crises by curtailing risky lending practices and preventing households from becoming highly levered again.

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While there is a large empirical literature examining the effects of many of the *ex post* policies aimed at restructuring household debt (Agarwal *et al.*, 2012, 2015a; Mayer *et al.*, 2014; Ganong and Noel, 2018), 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.*, 2017) and the growing theoretical literature suggesting that these policies may help to avoid inefficient aggregate losses that can arise when highly levered households are faced with adverse economic shocks (Farhi and Werning, 2016; Korinek and Simsek, 2016; Dávila and Korinek, 2017).

Though theory suggests that policies restricting household leverage can substantially improve financial stability, the real-world implementation of these policies is fraught with challenges. Regulators not only need to decide what kind of leverage to target [*e.g.*, loan-to-value (LTV) or debt-to-income (DTI) ratios], but must also balance any benefits to financial stability against the costs of curtailing potentially productive risk-taking. In grappling with these trade-offs, policymakers in different countries have come down on very different ends of the spectrum. Some countries have instituted outright bans or quotas on specific product types; others have merely increased the regulatory burden on risky lending in an effort to encourage lenders to internalize the costs of “excess” leverage. Both the incidence and efficacy of these policy choices depend crucially on how they end up affecting prices, quantities, and loan performance in targeted credit markets, all of which may depend on the particular institutional and market structures in place.

This article aims to advance our understanding of these trade-offs in the context of a central U.S. policy targeting household leverage in the mortgage market. The policy we study, the Ability-to-Repay and Qualified Mortgage (ATR/QM) Rule, operates as an implicit tax on lenders who originate loans with high DTI ratios. It was implemented by the Consumer Financial Protection Bureau (CFPB) in 2014 under the Dodd–Frank Act and was part of the broader U.S. policy response to the financial crisis. By studying how the market responds to this regulation, our article sheds new light on the impact and efficacy of policies that seek to regulate household leverage by imposing loan-level costs on lenders who extend potentially risky loans.

We focus our analysis on the effects of the regulation along three dimensions: prices, quantities, and loan performance. Studying the effect of the regulation on lender pricing is informative about the extent to which costs that are statutorily imposed on lenders end up being economically born by borrowers in the form of higher interest rates. While the results we document are specific to the ATR/QM rule, many other *ex ante* restrictions on household leverage, including macroprudential policies like risk-weighted capital requirements, operate in a similar fashion by penalizing lenders for issuing loans with certain risky characteristics. Our results on quantities are similarly informative about the extent to which policies that impose small costs on lenders may nonetheless lead to relatively large changes in both the distribution of leverage and overall credit availability. Finally, by studying how these shifts in the distribution of leverage are correlated with default risk, our results contribute to the debate over whether policies that specifically target reductions in the DTI ratio are able to significantly reduce individual default probabilities.

Our empirical analysis makes use of a large loan-level dataset and exploits two unique features of the policy change to measure its effects. The first is a sharp regulatory cut-off. Broadly speaking, the policy itself is not a direct tax on high-DTI mortgages. Instead, it merely mandates that creditors cannot extend *any* mortgage without first properly documenting and verifying that the borrower will be able to repay the loan. Failing to meet this new “ability-to-repay” (ATR) requirement exposes lenders to significant legal liabilities. However, to simplify compliance with this requirement, the CFPB carved out a class of lower-risk “qualified mortgages” (QM) that automatically satisfy the ATR rule and therefore shield lenders from liability. Among other

conditions, this class of mortgages is required to have a back-end DTI ratio no greater than 43%.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 economic shocks. We use this sharp cut-off as an empirical tool for distinguishing between parts of the market that are and are not affected by the regulation.

Second, in addition to establishing this cut-off, the CFPB also 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 exempted from the 43% DTI requirement. In practice, this means that it was primarily jumbo mortgages with a DTI greater than 43% 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 cut-off. This represents an increase in the cost of credit of roughly 2.5–3% relative to the average interest rate among high-DTI jumbo loans in the pre-period. Assuming borrowers refinance into a QM loan after 5 years, this premium works out to an additional \$1,700–2,600 in interest payments for the average affected loan in our sample. Interestingly, the premium we estimate is nearly identical to the CFPB's own estimates of the effect that the policy would have on lenders' costs of origination (Consumer Financial Protection Bureau, 2013). Thus, although the policy is statutorily imposed on lenders, it appears as if a large portion of the economic burden ends up being born by borrowers in the form of higher interest rates.

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%. Third, we also exploit the exemption for GSE-eligible loans by estimating a triple-difference model that includes conforming loans as an additional control group. 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

1. The back-end 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.

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

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

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% 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% cut-off.

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” behaviour 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 it.

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 ([Chetty *et al.*, 2011](#); [Kleven and Waseem, 2013](#)). 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% of the high-DTI jumbo market in the year that it was implemented and that an additional 20% of high-DTI jumbo loans were shifted from above to below the 43% threshold. These lost and shifted loans constitute 2 and 2.7% 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 \$600 million is a fairly small quantity, this reflects the fact that conforming loans are currently exempt from the DTI requirement. However, that exemption 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 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 that lenders charge for non-QM loans (DeFusco and Paciorek, 2017). We view this as clear evidence that much of the quantity response was instead driven by contractions on the supply side of the market. While the price effect we find was generally in line with what the CFPB had anticipated, this large contraction in supply was somewhat less expected and provides a cautionary note for policymakers seeking to regulate household leverage in other contexts.⁵

After estimating the overall quantity response, we also explore variation across types of lenders to better understand why this response may have been so large. We focus on one specific market friction that may have contributed to our results. In particular, we hypothesize that the ATR/QM rule, by penalizing low-quality documentation on high-DTI loans, exacerbated pre-existing agency conflicts between mortgage originators—who are responsible for collecting borrower documentation—and mortgage investors—who directly bear the costs of improper documentation. If present, these additional agency costs may have been large enough to render non-QM lending unprofitable at some lenders while still allowing other lenders who operate with a more integrated business model to continue lending at only slightly higher rates. To the extent that borrowers cannot perfectly substitute between these two types of lenders, this could generate a large aggregate decline in non-QM lending while at the same time only leading to a moderate increase in interest rates for borrowers who continue to receive loans from the lenders who stay.

While our data do not allow us to identify individual lenders, they do record both the origination channel (*e.g.* retail-versus-broker) and an indicator for whether the loan is currently being held on portfolio. Using this information, we document two additional facts about the composition of non-QM lending that are consistent with the hypothesis outlined above. First, we show that the share of jumbo mortgages issued by lenders who rely on third-parties like brokers or correspondents to collect supporting documentation fell dramatically in the high-DTI portion of the market subsequent to the policy change. Second, we show that there was a similar relative decline in the share of high-DTI jumbo loans issued by non-portfolio lenders compared to portfolio lenders after the ATR/QM rule took effect. Together, these results suggest that much of the large quantity response we find was driven by lenders who either do not directly originate their loans or who do not intend to hold the loans they originate on their balance sheet. Though other mechanisms may certainly be at play, these differential responses by lender type indicate that frictions in financial

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).

5. In its prospective cost–benefit analysis of the ATR/QM rule, the CFPB stated that “the Bureau believes that the ability to repay requirements and the accompanying potential litigation costs will create, at most, relatively small price increases for mortgage loans. These small price increases, in turn, are not likely to result in the denial of credit to more than a relatively small number of borrowers [...]” (Consumer Financial Protection Bureau, 2013).

intermediation and agency costs in particular are important factors to consider in the design of policies that seek to regulate household leverage.

Having documented the effect of the regulation on prices and quantities, we next turn to analysing its potential effects on loan performance. This analysis is important as one main goal of the policy was to reduce liquidity-driven mortgage defaults. 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 non-parametrically 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% cut-off 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, which implies that a full implementation of the policy could have potentially 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 5-year default rate by only 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% after 5 years, we view these performance improvements as relatively small. The policy may have been able to induce larger improvements in performance had the DTI threshold been set lower; however, our estimates of the effects on prices and quantities suggest the resulting impact on the availability of mortgage credit could be relatively large.

These results suggest that even though policies that marginally restrict borrowers' DTI can significantly affect market prices and quantities, restricting DTI may be a relatively ineffective way to improve individual default risk in comparison to alternative measures of household leverage such as the LTV ratio. The primary benefits to restrictions on DTI may therefore be found in how they affect other important outcomes, such as house prices or the resiliency of household demand to other shocks. We view this as an important area for future research.

Our article 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 (Chodorow-Reich *et al.*, 2012; Mian and Sufi, 2012; Berger *et al.*, 2016), large extensions to unemployment benefits (Rothstein, 2011; Hagedorn *et al.*, 2013; Chodorow-Reich and Karabarbounis, 2016), unconventional monetary policy (Krishnamurthy and Vissing-Jorgensen, 2011; Williams, 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, 2018). 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.* \(2016\)](#) focus on the several years leading up to the final ATR/QM rule and document that uncertainty over where the DTI threshold would be led some lenders to reduce their high-DTI lending. Our article differs in that we study lenders' response to the actual policy that was enacted rather than their uncertainty over what that policy would be.⁶ Similarly, [D'Acunto and Rossi \(2017\)](#) show that mortgage lending to lower income households (as proxied by the size of the loan) declined during the period of time immediately following the passage of the Dodd–Frank Act in 2010. They argue that this decline in the number of relatively smaller mortgages could be driven by the increased fixed costs of complying with the new regulation. Our focus on the period of time surrounding the actual policy change in 2014 as well as our use of a quasi-experimental research design allows us to directly isolate the effect of the ATR/QM rule on mortgage lending separately from both the other mortgage-related provisions of the Dodd–Frank Act and potentially confounding macroeconomic trends. [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.⁷

The remainder of this article proceeds as follows: in Section 2, we provide details on the institutional background surrounding the ATR/QM rule. Section 3 describes our data and sample selection criteria. In Section 4, 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 5 presents the results and research design we use to study the effect of the policy on the quantity of mortgage credit. Section 6 provides estimates of the potential effects of the policy on mortgage performance. Section 7 concludes.

2. INSTITUTIONAL BACKGROUND

In response to the 2007–8 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.⁸ The CFPB was charged with implementing this “ability-to-repay” (ATR) rule, which took effect 10 January 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

6. The fact that lenders were uncertain over where the DTI threshold would fall, as [Gissler *et al.* \(2016\)](#) document, also helps to rule out concerns that lenders were able to precisely target their behaviour in anticipation of the policy change.

7. [Baker \(2018\)](#), for example, shows that highly indebted households cut consumption significantly more in response to negative income shocks relative to households with relatively little debt.

8. The rule is similar to an earlier rule enacted by the Federal Reserve in 2008, effective since 2009, that required lenders to verify ATR on “higher-priced,” typically sub-prime loans.

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.⁹ 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.¹⁰ These loans provide a legal “safe harbour” to the lender in the event of any legal action brought by the borrower. In effect, the QM category provides lenders with an 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 DTI ratio greater than 43% nor can they feature negative amortization, interest-only payments, balloon payments, terms exceeding 30 years, or points and fees greater than 3% of the total loan size (with some exemptions). If the interest rate on a QM loan is lower than the prime rate cut-off established by the CFPB then the loan qualifies for a legal “safe harbour” 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.¹¹

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.¹² The exempt loans must not have risky loan features (such as interest-only options), but they may have DTIs greater than 43%. 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 3 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

9. 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 DTI ratio using all debt obligations listed above relative to gross monthly income, and (8) credit history.

10. The QM category should not be confused with another important category of loans in Dodd–Frank, the Qualified Residential Mortgage (QRM), 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.

11. If the loan is higher-priced, then this safe harbour 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. Due to the relatively strict lending environment during our sample period these “high-priced” loans, which are typically reserved for sub-prime borrowers, were very uncommon. For example, there are only 120 high-priced jumbo loans in our final sample, which contains more than 140,000 jumbo loans in total.

12. 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](#)).

no matter how much time has elapsed. Importantly, liability for damages under ATR/QM is not limited to just the entity that originates the mortgage; it also extends to any assignees, including secondary market investors who purchase mortgages either in full or through mortgage backed securities. As we will discuss in detail below, this assignee liability exacerbates the pre-existing agency conflict between mortgage originators and mortgage investors and may make non-QM mortgages less appealing to lenders who operate with non-integrated business models that rely heavily on third parties for due diligence.

In the event of legal action being brought by a borrower, the lender (or its assignee) 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 harbour. Barring fraud or an inability to prove that required criteria were verified (*e.g.* 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 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 3-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.¹³ 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% of the mortgage market and non-conventional loans insured by federal agencies add another 36% (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% applies mainly to the jumbo loan market, which accounted for roughly 5% of the total market in 2014. The CFPB estimated that between 1997 and 2003 about 70% of all loans would have received QM status based solely on the features of the loan and only 8% would not have satisfied the ATR rule in any way. The CFPB also estimated that almost 100% of loans in 2011 would have satisfied the ATR rule in some way, again without assuming any temporary exemptions, although only 76% of these mortgages would have received the QM safe harbour (Consumer Financial Protection Bureau, 2013). However, alternative estimates have suggested that as little as 52% of the market will qualify for QM status after the agency exemption expires.¹⁴ 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.

13. 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.

14. http://www.corelogic.com/downloadable-docs/MarketPulse_2013-February.pdf.

3. DATA

3.1. Data sources

Our main source of data is the CoreLogic Loan-Level Market Analytics (LLMA) database. This database contains detailed loan origination and performance information for roughly 60% 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 include coverage of both the agency and non-agency markets as well as the prime and sub-prime sectors going back to 1999.¹⁵

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.

3.2. 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 4 years of pre-treatment data and 2 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.¹⁶ We also drop a small number (less than 1%) of loans with DTI ratios greater than 50%, as many of these loans appear to be outliers. These restrictions leave us with a sample of roughly 1.2 million loans.¹⁷

Descriptive statistics for this sample are presented in the first column of Table 1. The average loan in our sample is for roughly \$265,000 at an interest rate of 4.3% and goes to a borrower with a FICO score of 755, LTV of 80%, and a back-end DTI of approximately 33%. In much of our analysis, we will distinguish between jumbo and conforming loans. The second and third columns of Table 1 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.

15. This dataset is not to be confused with the CoreLogic LoanPerformance Asset-Backed Securities database (LP), which is sourced primarily from sub-prime mortgages that were used to collateralize private-label mortgage-backed securities.

16. 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.

17. 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 to the data vendor. However, in Supplementary Appendix A, we show this issue should not affect our results since the incidence of missing DTIs does not change meaningfully around the time of the policy change and is roughly constant both across the jumbo and conforming markets and along any of the other borrower characteristics that we observe.

TABLE 1
Loan-level descriptive statistics

	Full sample			DTI $\in (36, 50]$		
	All loans	Conforming	Jumbo	All loans	Conforming	Jumbo
Loan amount (\$1,000's)	264.58 (189.75)	212.91 (101.26)	641.53 (249.59)	267.37 (187.31)	215.96 (101.23)	637.06 (240.84)
FICO score	756.12 (43.27)	754.50 (44.44)	767.91 (31.09)	751.09 (44.14)	749.03 (45.18)	765.93 (32.06)
Loan-to-value	80.34 (13.89)	80.81 (14.26)	76.96 (10.24)	80.51 (13.95)	81.06 (14.32)	76.55 (10.04)
Back-end debt-to-income	33.39 (9.04)	33.36 (9.10)	33.58 (8.61)	41.80 (3.51)	41.85 (3.52)	41.44 (3.39)
Interest rate	4.29 (0.56)	4.31 (0.56)	4.19 (0.54)	4.34 (0.54)	4.36 (0.54)	4.24 (0.54)
Number of observations	1,195,895	1,051,730	144,165	513,939	451,191	62,748

Notes:—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% 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 3 for further details on data sources and sample construction.

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%. Specifically, we restrict attention to loans with DTI ratios between 36 and 50%. This restriction includes all loans in the sample with DTIs greater than 43% and has the added advantage of dropping loans with DTIs less than 36%, 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 1 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.

4. THE EFFECT OF ATR/QM ON THE PRICE OF CREDIT

The ATR rule and QM designation together essentially operate as an implicit tax on lenders who issue mortgages with risky product characteristics. In particular, if a borrower who receives a non-QM loan files a legal claim for damages in the event of default or foreclosure, then the lender must defend that the non-QM loan satisfied the ATR rule in court. Even if this defence is successful it will involve legal fees. In contrast, lenders issuing loans that meet the QM definition do not face these expected costs as such loans are automatically presumed to be compliant with the ATR rule. As with any tax, lenders may choose to pass along some of the additional expected costs to borrowers by charging an interest rate premium on non-QM loans. In this section, we measure this pass-through using two alternative identification strategies which leverage different aspects of the way that ATR/QM was designed.

4.1. Research design

4.1.1. 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% 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%. Specifically, we estimate regressions of the following form:

$$r_{it} = \alpha + \delta_t + X_i' \gamma + \beta_0 \cdot \mathbb{1}[\text{DTI}_i > 43] + \beta_1 \cdot \mathbb{1}[\text{DTI}_i > 43] \times \text{Post}_t + \epsilon_{it}, \quad (4.1)$$

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 $\mathbb{1}[\text{DTI}_i > 43]$ is a non-QM “treatment” indicator that takes the value one if the back-end DTI ratio on loan i is greater than 43%. 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 (4.1) 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%. 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:

$$r_{it} = \alpha + \delta_t + X_i' \gamma + \sum_{d=36}^{50} \left[\beta_0^d \cdot \mathbb{1}[\text{DTI}_i = d] + \beta_1^d \cdot \mathbb{1}[\text{DTI}_i = d] \times \text{Post}_t \right] + \epsilon_{it}, \quad (4.2)$$

where $\mathbb{1}[\text{DTI}_i = d]$ is an indicator for whether the back-end DTI ratio on loan i rounded up to the nearest integer is exactly equal to d , and all other variables are as defined in (4.1). 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%. If the change in interest rates for high-DTI loans are 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 $d > 43$.

4.1.2. 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% DTI limit to satisfy the QM standards, changes in interest rates for high-versus-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 (4.1). 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%:

$$\begin{aligned} r_{ist} = & \alpha + \delta_{st} + X_i' \gamma + \beta_0 \cdot \mathbb{1}[\text{DTI}_i > 43] + \beta_1 \cdot \mathbb{1}[\text{DTI}_i > 43] \times \text{Post}_t \\ & + \beta_2 \cdot \text{Jumbo}_i + \beta_3 \cdot \text{Jumbo}_i \times \mathbb{1}[\text{DTI}_i > 43] \\ & + \beta_4 \cdot \text{Jumbo}_i \times \mathbb{1}[\text{DTI}_i > 43] \times \text{Post}_t + \epsilon_{ist}. \end{aligned} \quad (4.3)$$

In this specification, r_{ist} is the interest rate on loan i originated in month t in market segment $s \in \{\text{Jumbo}, \text{Conforming}\}$, 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.

4.2. Results

4.2.1. Graphical evidence. As a starting point for the empirical analysis, Figure 1 plots mean interest rates by origination month separately for jumbo loans with DTIs above 43% (circles) and those with DTIs less than or equal to 43% (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 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%. To illustrate this, Figure 2 plots detrended mean interest rates by DTI separately for jumbo loans originated before (triangles) and after (circles) the implementation of ATR.¹⁸ For loans originated prior to ATR, the relationship between interest rates and DTI is

18. 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 1% 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% bin includes all DTIs greater than 42% and less than or equal to 43%.

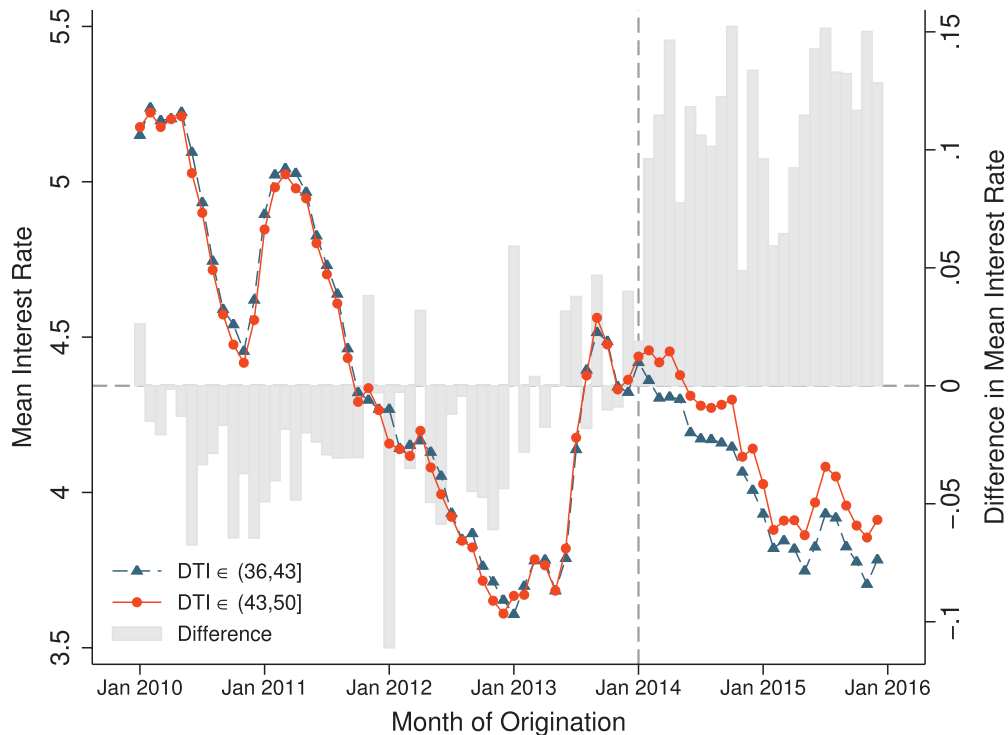


FIGURE 1

Mean interest rates by origination month for high-versus-low DTI jumbo loans

Notes:—This figure plots mean interest rates by origination month separately for high-DTI jumbo loans (circles) and low-DTI jumbo loans (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 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% described in Section 3.

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% threshold. Together, we take the results presented in Figures 1 and 2 as convincing evidence in favour of the parallel trends assumption underlying the difference-in-differences research design.

4.2.2. Regression results. Table 2 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 (4.1). In the first column, we report estimates from a baseline specification that includes only the non-QM dummy ($\text{DTI} > 43$), the interaction of that dummy with the Post indicator, and a full set of origination month fixed effects.¹⁹ 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,

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

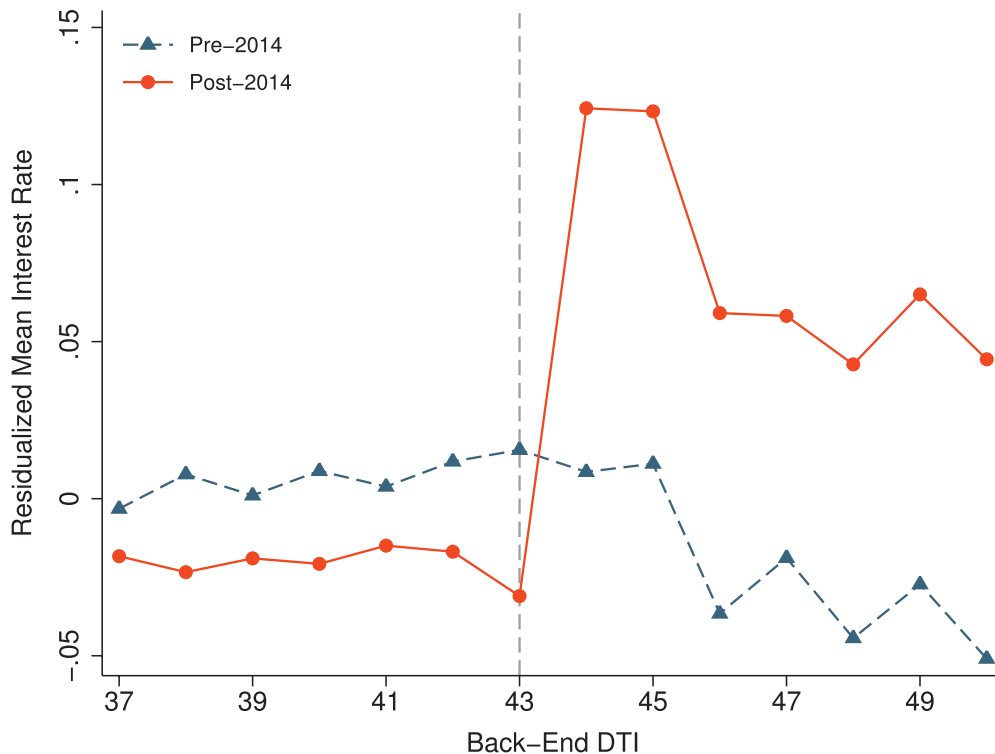


FIGURE 2

Detrended mean interest rates by DTI for jumbo loans originated pre- and post-ATR/QM

Notes:—This figure plots detrended mean interest rates by DTI separately for jumbo loans originated before (triangles) and after (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 1% 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%. DTI bins are created by rounding up to the nearest integer so that the 43% bin includes all DTIs greater than 42% and less than or equal to 43%. Detrended means are calculated using the sample of all jumbo loans with DTIs between 36% and 50% described in Section 3.

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% 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

TABLE 2
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*** (0.005)	-0.017*** (0.004)	-0.004 (0.004)	-0.009** (0.004)	0.003 (0.003)	0.003 (0.003)	0.019*** (0.002)	0.015*** (0.002)
DTI > 43 × Post	0.131*** (0.007)	0.141*** (0.008)	0.113*** (0.007)	0.115*** (0.007)	0.008*** (0.003)	0.008*** (0.003)	0.006*** (0.002)	0.008*** (0.002)
DTI > 43 × Jumbo					-0.021*** (0.006)	-0.022*** (0.006)	-0.032*** (0.005)	-0.030*** (0.005)
DTI > 43 × Jumbo × Post					0.124*** (0.008)	0.128*** (0.008)	0.108*** (0.007)	0.103*** (0.007)
Month (× Jumbo) FEs	X	X	X	X	X	X	X	X
County FEs		X	X	X		X	X	X
FICO × LTV FEs			X	X			X	X
Property Type FEs			X	X			X	X
FICO × LTV × Post FEs				X				X
Property Type × Post FEs				X				X
Implied %Δ	2.9%	3.2%	2.5%	2.6%	2.8%	2.9%	2.4%	2.3%
R-Squared	0.70	0.72	0.75	0.76	0.64	0.65	0.71	0.71
Number of observations	62,748	62,748	62,748	62,748	513,939	513,939	513,939	513,939

Notes:—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%. 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%. 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), the pairwise interaction between the two, and the property type (single family, condominium, townhouse, planned unit development). Columns 4 and 8 further interact the FICO, LTV, and property-type fixed effects with the Post dummy. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

composition of borrower types along the DTI distribution. We address this possibility in the third column, which controls flexibly for borrower and property type by including property-type fixed effects as well as a full set of 20-point FICO score bins, 5-point LTV bins, and the pairwise interaction between the two.²⁰ Doing so decreases the coefficient estimate only modestly to eleven basis points, which is statistically indistinguishable from the baseline estimate in column one. In this specification, the coefficient on the DTI > 43 dummy also falls to zero and is statistically insignificant, which indicates that the small pre-policy discount for high-DTI loans present in columns 1 and 2 and in the raw averages shown in Figure 1 reflects differences in observable borrower characteristics. Finally, in column 4 we further interact these property- and borrower-type fixed effects with the Post indicator. In this specification, we are not only controlling for changes in borrower composition but also for any changes in the way that lenders price non-DTI

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

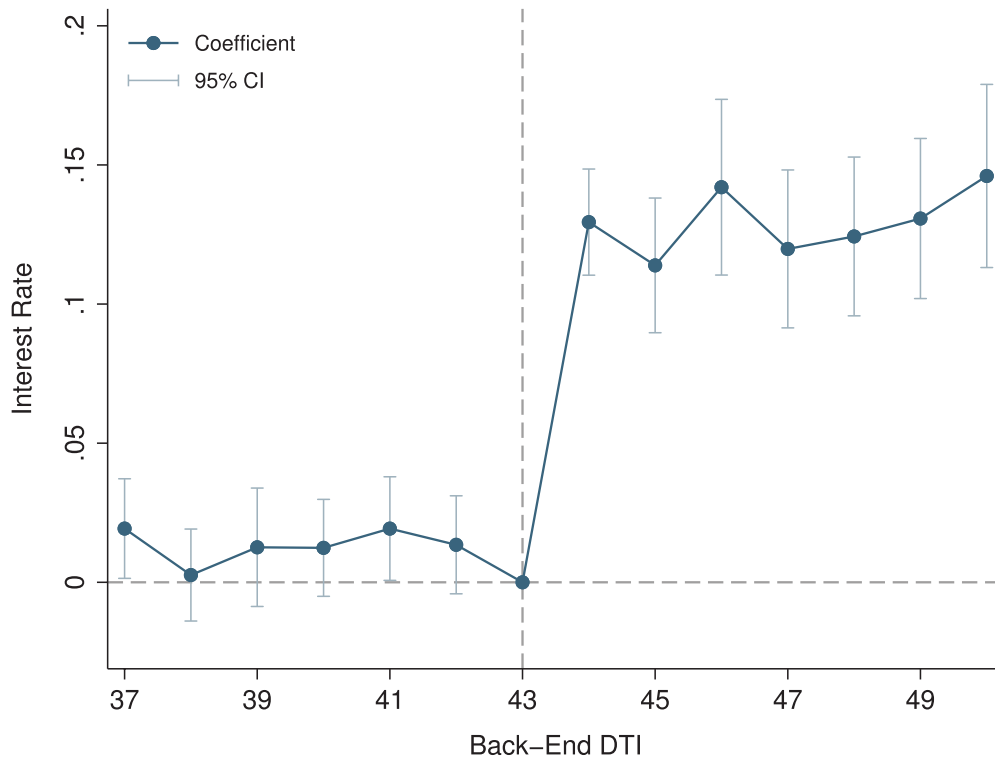


FIGURE 3

Flexible difference-in-differences estimates of the effect of non-QM status on interest rates

Notes:—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%. The DTI dummies were created by rounding up to the nearest integer so that the 43% bin includes all DTIs greater than 42 and less than or equal to 43%. 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% confidence intervals are based on standard errors that were clustered at the county level.

related borrower risk subsequent to the change in policy. The coefficient estimate remains stable at roughly twelve basis points and is statistically significant at the 1% level.

To show that these estimates are being driven directly by the change in regulation and not by overall changes to the way that lenders are pricing the underlying risk associated with DTI, Figure 3 plots coefficient estimates from the more flexible difference-in-differences specification that allows the effect to vary non-parametrically in the borrower's DTI. To generate this figure, we estimate a version of equation (4.2) that includes all of the same controls that were included in the fourth column of Table 2 and plot the resulting coefficient estimates and 95% 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%. Moreover, the premium charged for non-QM loans does not depend on the borrower's DTI;

all borrowers with DTIs greater than 43% 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% where it is easier for borrowers to get below the threshold by lowering their DTI.²¹

Finally, in the last four columns of Table 2, 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%. 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 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% relative to the pre-ATR mean.

As a rough way to put these estimates into context, we can ask how this increase in the cost of credit for borrowers compares to the expected costs of litigation for lenders issuing a non-QM loan. This type of comparison will give a sense for how much of the additional costs generated by the regulation are borne by borrowers. Of course, any calculation of this sort will depend crucially on assumptions about the probability of default, the likelihood that a borrower brings a suit conditional on default, the damages owed to the borrower if she were to win the suit, and the probability that the court rules in the favour of the borrower. Since our data do not allow us to directly estimate these quantities, we instead rely on estimates from the CFPB which, in its final rule, performed a similar calculation using a range of different assumptions taken from input provided to the agency by both industry representatives and consumer advocacy groups. Depending on the scenario, the CFPB estimated that the expected cost of issuing a loan that does not meet the QM definition would increase by roughly 12–40 basis points of the initial loan value. When amortized over the typical loan life, this would imply an increase in the interest rate of roughly 3–10 basis points if lenders were able to pass all of the additional costs on to borrowers.²² Our estimates are at the upper end of this range, which suggests that lenders are indeed passing on a substantial portion of the incremental costs directly to borrowers.

To get an alternative sense of the magnitude of this effect we can also 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% and our sample was

21. While there is some evidence of this pattern in the raw average interest rates plotted in Figure 2, Figure 3 clearly shows that this pattern disappears once we control for observable borrower characteristics. In Supplementary Appendix B.1, we show that the differences between these two figures are likely being driven by a very slight decline in FICO scores at high-DTIs subsequent to the policy change.

22. In its final ruling the CFPB stated that “estimated costs for non-QM loans (loans made under the ATR standard without any presumption of compliance) are estimated to increase by approximately twelve basis points [or three 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).

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.

5. THE EFFECT OF ATR/QM ON THE QUANTITY OF CREDIT

In addition to increasing the cost of credit, the ATR rule and QM 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 by 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% 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 4, we plot the distribution of DTIs among new jumbo mortgage originations separately for 2013 (triangles) and 2014 (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%. In contrast, after ATR was enacted in 2014, the distribution features a sharp drop at exactly 43%. Relative to the pre-period, the 2014 distribution also exhibits a significant amount of bunching to the left of 43% 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.

5.1. *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.

5.1.1. 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.²³ 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

23. 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.

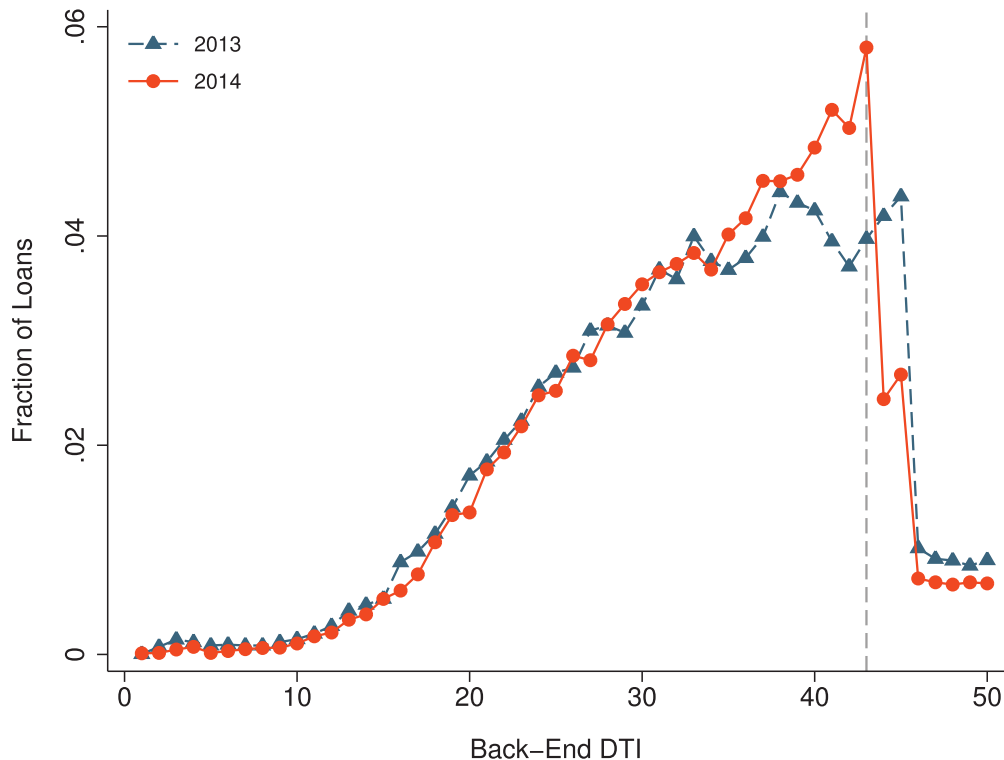


FIGURE 4

DTI distribution among jumbo mortgages

Notes:—This figure plots the distribution of DTI among jumbo mortgages separately for loans originated in 2013 (triangles) and 2014 (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%. DTI bins are created by rounding up to the nearest integer so that the 43% bin includes all DTIs greater than 42% and less than or equal to 43%. Total originations and shares by DTI were calculated using all jumbo loans contained in analysis sample described in Section 3.

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 4, this assumption is clearly violated in our context; the DTI distribution features a large discontinuity at 45% even during the pre-period. This discontinuity arises because many lenders impose their own internal maximum DTI thresholds of 45%, which leads to a large drop in the number of loans with DTIs beyond this limit.²⁴

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

24. The 45% threshold exists because of a requirement that Fannie Mae was imposing on conforming loans during our sample period. This requirement was built into Fannie’s automated underwriting software and would automatically deny most loans with DTIs greater than 45%. While there is no legal requirement for lenders making *jumbo* loans to comply with this requirement, it is common practice for many banks to adopt GSE standards even for non-GSE loans, either by explicitly processing loans through the GSE software as an initial screening device or by simply using GSE rules in manual underwriting. Interestingly, Freddie Mac did not impose this requirement as stringently during this period, which could explain why we also see a fair number of loans above the 45% threshold.

in each DTI-bin d in the post-ATR period had the ATR rule not been in effect. We denote this counterfactual number of loans as $\hat{n}_{jd}^{\text{post}}$. 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:*

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

This assumption is motivated by the fact that the conforming market was exempt from the 43% 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 Supplementary Appendix B 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 behaviour in the jumbo market near and above the QM-threshold.

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

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

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 DTI ratio \bar{d} below which the total volume of jumbo loans $N_{j\bar{d}}^{\text{post}}$ 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. $\hat{n}_{jd}^{\text{post}}$ and n_{cd}^{post}). However, when we divide each of these bin counts by the corresponding total level of activity to the left of \bar{d} in the relevant market, the ratios (e.g. $\hat{n}_{jd}^{\text{post}}/N_{j\bar{d}}^{\text{post}}$ and $n_{cd}^{\text{post}}/N_{c\bar{d}}^{\text{post}}$) 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:*

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

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. We validate this assumption below in two ways. First, we provide direct graphical evidence showing that the trends in normalized loan counts across the jumbo and conforming markets are nearly identical prior to ATR/QM, and that they only begin to diverge after the policy change in DTI bins near the 43% threshold. Second, we conduct a series of placebo tests showing that the implied counterfactual post-period DTI distribution can accurately replicate the true empirical distribution in years for which there was no policy change. Together, these two tests provide strong evidence in support of Assumption 3.

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

$$\hat{n}_{jd}^{\text{post}} = \hat{\pi}_{jd}^{\text{post}} \times N_{jd}^{\text{post}}. \quad (5.4)$$

Equation (5.4) intuitively expresses the counterfactual as a product of two terms: a measure of the observed overall level of activity in the jumbo market (N_{jd}^{post}) and the predicted allocation of that activity across the DTI distribution ($\hat{\pi}_{jd}^{\text{post}}$). 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 \bar{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.

5.1.2. 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

$$\hat{B} = \left| \sum_{d=\bar{d}}^{43} (\hat{n}_{jd}^{\text{post}} - n_{jd}^{\text{post}}) \right|. \quad (5.5)$$

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

$$\hat{M} = \sum_{d=44}^{50} (\hat{n}_{jd}^{\text{post}} - n_{jd}^{\text{post}}). \quad (5.6)$$

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 is 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 $\hat{B}/\hat{N}_{44+}^{\text{post}}$ and the extensive margin effect as $(\hat{M} - \hat{B})/\hat{N}_{44+}^{\text{post}}$, where $\hat{N}_{44+}^{\text{post}} = \sum_{d=44}^{50} \hat{n}_{jd}^{\text{post}}$. 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 (5.4)–(5.6) there are two free parameters we must choose: the lower limit of the bunching region (\bar{d}), and the time periods over which to measure the pre- and post-ATR distributions. For our main analysis, we set $\bar{d} = 38$. This choice is motivated by the evidence in Figure 4, 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 \bar{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.

5.1.3. Validating the counterfactual. Before presenting our main results, we first provide evidence validating our method for estimating the counterfactual. To do so, we proceed in two steps. First, we directly assess the validity of the parallel trends assumption (Assumption 3). Second, having validated that assumption, we then perform a series of placebo tests which verify that our approach to estimating the counterfactual is able to produce accurate and unbiased estimates of the true DTI distribution in years when there is no policy change.

In Figure 5, we plot the count of new originations by DTI, month of origination, and market segment (jumbo or conforming). For each month, DTI bin, and market segment, we normalize these loan counts by dividing by the corresponding total volume of originations in the same month and market segment with DTIs less than or equal to $\bar{d} = 38$. These normalized bin counts are the monthly equivalents of the annual ratios that we use to build up our counterfactual (n_{jd}^t/N_{jd}^t and n_{cd}^t/N_{cd}^t for $t \in \{\text{pre}, \text{post}\}$). If Assumption 3 holds, then the trend in the normalized number of loans originated in the jumbo market should track the trend in the conforming market for all months leading up to the policy change and only begin to diverge afterwards. To the extent that there is any post-policy divergence, it should be most apparent at DTIs near the threshold. This is precisely what the figure shows. Each panel reports the trends for a separate 1% DTI bin over the 4-year window bracketing the implementation of ATR. The ratios are nearly identical for jumbo and conforming loans in every month leading up to the policy change, and there is a sharp divergence for DTIs near the threshold starting in immediately the month that the policy goes into effect. The direction of the change in trends is consistent with the bunching behaviour observed in Figure 4. At DTIs at and just below the threshold, the trend for jumbo loans jumps relative to

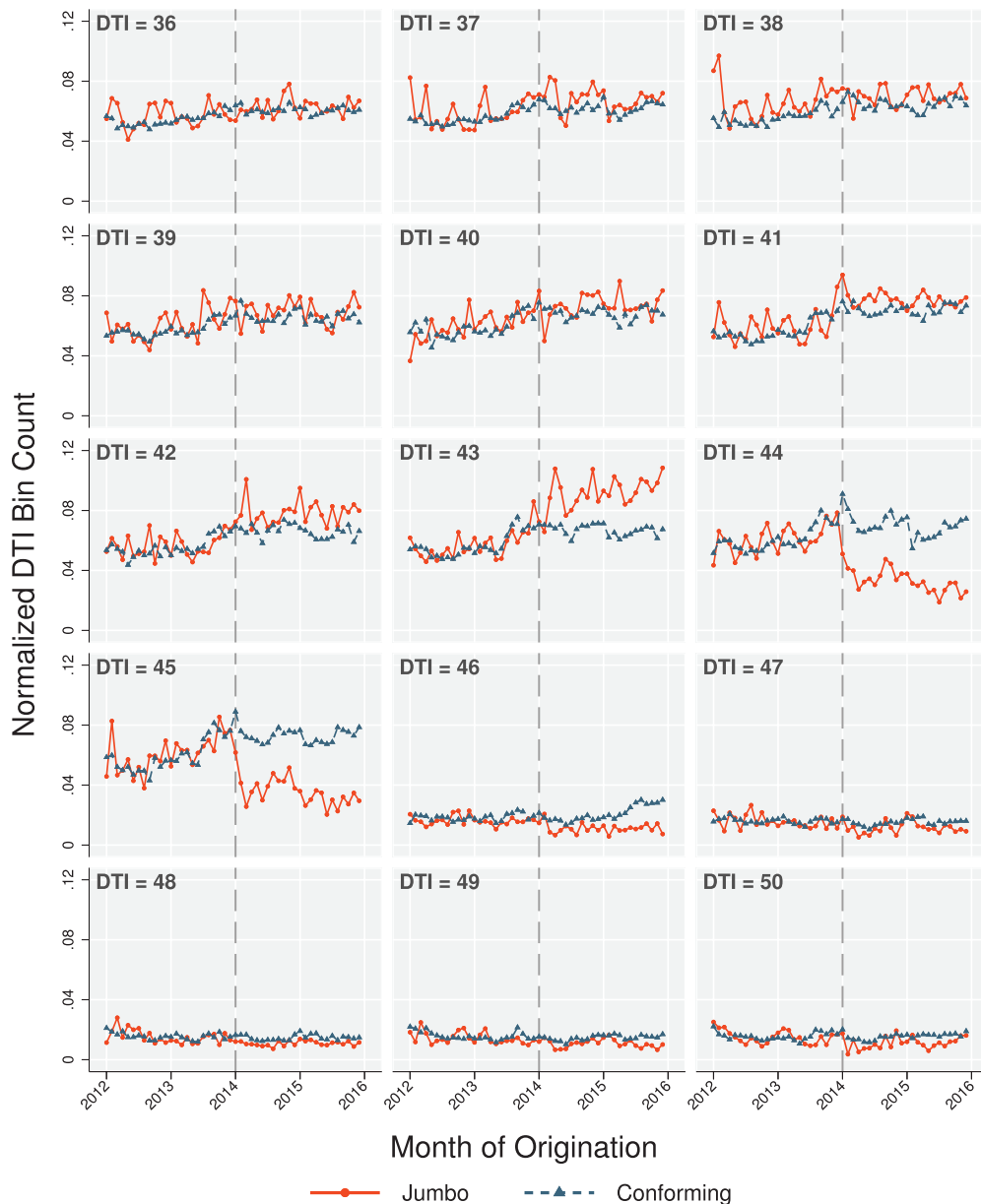


FIGURE 5

Normalized number of loans by DTI, market segment, and month of origination

Notes:—This figure plots monthly counts of new loan originations separately by DTI and across market segments (jumbo and conforming). As described in Section 5.1, loan counts are normalized within market segment and month by dividing by the total number of loans originated in the same segment and month with DTIs less than or equal to $\bar{d} = 38$. The vertically dashed grey line marks the month that the Ability-to-Repay Rule and Qualified Mortgage Standards went into effect (January 2014).

that of conforming loans, and at DTIs just above the threshold it falls. Aggregating across DTI bins, it also appears as if the total fall in originations at DTIs above the threshold is larger than the increase below it, which is consistent with an extensive margin quantity response. Together, these patterns provide strong evidence in support of the parallel trends required by Assumption 3.

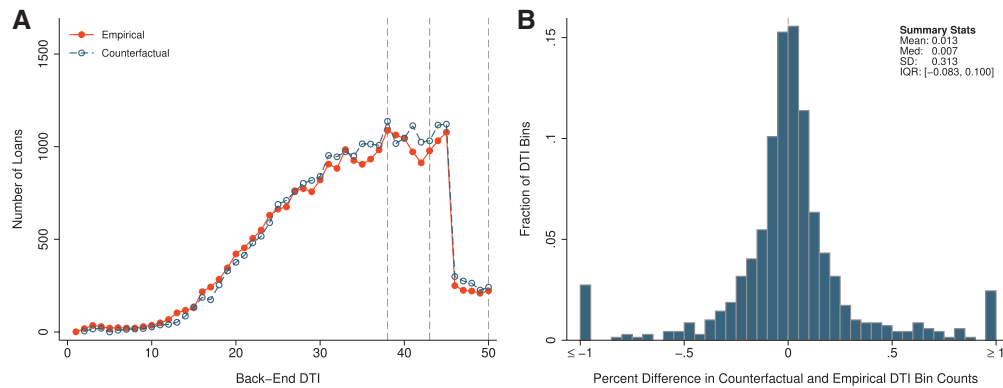


FIGURE 6

Comparison of the empirical and counterfactual jumbo DTI distributions in placebo policy years

Notes:—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 circles) and counterfactual distribution (hollow circles), treating 2013 as the placebo year. The counterfactual distribution was generated as described in Section 5.1 using 2012 as the pre-period. The vertically dashed grey lines mark the lower limit of the bunching region ($\bar{d}=38$), 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% bin includes all DTIs greater than 42% and less than or equal to 43%. Panel B summarizes the difference between the empirical and counterfactual distributions across all placebo policy years, 2000–13. 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%) for visual clarity.

Our second approach for validating the method we use to estimate the counterfactual is to show that it is able to generate a DTI 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.²⁵ 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 $\bar{d}=38$ 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 6 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%. 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 centred 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% of each other and the median difference is less than 1%. We take this as compelling evidence that the approach we use

25. The LLMA data coverage extends back to 1999; however, at least 1 year of pre-data is needed to construct the counterfactual distribution, which limits the set of possible pre-ATR/QM placebo years to 2000–13.

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.

5.2. Results

Having validated our method of generating the counterfactual DTI distribution, we now turn to using that method to estimate the effects of the ATR/QM rule on credit quantities. We begin by studying the overall effect of the regulation along both the intensive and extensive margins. As was previewed already in Figure 4, our estimates reveal that the regulation had a very significant effect on both of these margins. This leads us to further investigate two possible channels through which the quantify effect may have arisen: product substitution and agency costs. While we find little evidence that the quantity response is driven by borrower substitution into lower-cost mortgage contracts, heterogeneity in this response across lenders suggests that agency frictions between various participants in the mortgage origination chain may have contributed to the large overall effect.

5.2.1. Intensive and extensive margin quantity effects. As a starting point for this analysis, in Figure 7, we plot both the observed DTI distribution and the counterfactual for loans originated in 2014, the first year that ATR/QM was in effect. The solid 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 1% bin indicated on the x -axis. The dashed connected line plots the counterfactual, estimated as described in Section 5.1. The vertical dashed lines mark the lower limit of the bunching region ($\bar{d} = 38$), the QM-threshold of 43%, and the maximum DTI.

The empirical distribution exhibits a sharp discontinuity at the QM-threshold; moving from a DTI of 43–44% leads to a more than 50% 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 4, 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% of the loans that would have otherwise had a DTI above 43% 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% of the counterfactual number of loans in that region. Thus, we estimate that approximately 15% of all jumbo loans that would have otherwise had a DTI above 43% were eliminated due to extensive margin responses.

The first column of Table 3 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 1% level. The second through third columns of the table report analogous estimates under varying assumptions for the lower limit of the bunching region \bar{d} . We consider values of \bar{d} ranging from 30% to 40%.²⁶ In all cases, the estimated responses are of roughly the same order of magnitude and, across specifications, bracket our preferred

26. Changing the lower limit of the bunching region can affect the results in two ways: (1) by increasing or decreasing the range over which the difference between the counterfactual and empirical distributions is summed to the left of the 43% threshold and (2) by altering the calculation of the counterfactual distribution itself. Table 3 reports the combined effect of these two channels. In Supplementary Appendix B.3, we report the effect of changing the counterfactual while holding constant the range of “integration” over which the bunching estimates are calculated.

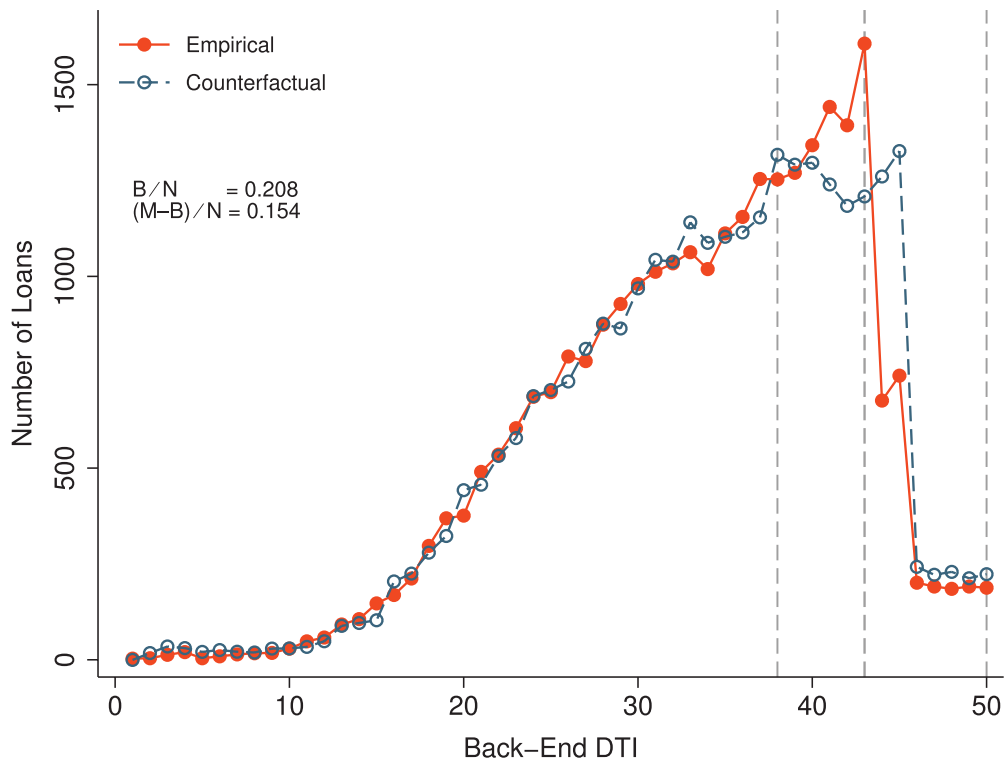


FIGURE 7

Bunching, missing mass, and the effect of ATR/QM on the quantity of credit

Notes:—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 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% bin includes all DTIs greater than 42% and less than or equal to 43%. The dashed connected line plots the counterfactual, which was estimated as described in Section 5.1 using 2013 as the pre-period. The vertically dashed grey lines mark the lower limit of the bunching region ($\bar{d}=38$), 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 5.1.

estimate. Individually, the 95% 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% and the extensive margin response ranges from 9% to 18%. All of these estimates are significant at the 1% level, with the exception of the extensive margin response when $\bar{d}=35$ which has a p -value of 0.107. Reassuringly, there is also no systematic relationship between the magnitude of the estimated effect and the level of \bar{d} . Together, the evidence presented in Table 3 provides confidence that our results are not being driven by the assumptions we make on the lower limit of the bunching region.

5.2.2. 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% of all jumbo loans in 2014 that would have

TABLE 3
Intensive and extensive margin effects of ATR/QM on the quantity of credit

	Preferred	Alternative specifications		
	(1)	(2)	(3)	(4)
	$\bar{d}=38$	$\bar{d}=30$	$\bar{d}=35$	$\bar{d}=40$
$\hat{B}/\hat{N}_{44+}^{\text{post}}$	0.208*** (0.033)	0.188*** (0.062)	0.269*** (0.048)	0.228*** (0.032)
$(\hat{M}-\hat{B})/\hat{N}_{44+}^{\text{post}}$	0.154*** (0.041)	0.180*** (0.069)	0.090 (0.056)	0.133*** (0.039)
Bootstrap replications	100	100	100	100
Number of observations	418,105	418,105	418,105	418,105

Notes:—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% 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 5.1. Column one reports our preferred estimates, which set the lower limit of the bunching region to $\bar{d}=38$. Columns 2–4 report analogous estimates from alternative specifications which set this limit to 30, 35, and 40%, 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.

otherwise had a DTI above 43% were eliminated as a result of the policy.²⁷ These lost loans constitute 2% 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 volume 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 2014.²⁸

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 4. While we have shown that the quantity of non-QM lending fell substantially in response to the ATR/QM rule, it is not clear if this quantity response is driven by contractions in supply or demand. In particular, one possibility is that the fall in quantities simply reflects the demand-side response to the non-QM interest rate premium that we documented in Section 4. To gauge the plausibility of this explanation, we can compare the magnitude of the quantity response we estimate to the reduction in lending volume that would be implied by combining the price response with external estimates of the interest rate elasticity of mortgage demand. If the actual quantity response is larger than this implied response, then it is likely that the fall in quantities was not purely demand-driven.

27. Some of these loans may have disappeared from the jumbo market as a result of substitution into the conforming market. However, we show in Supplementary Appendix B.2 that this form of substitution is likely to have been small for the typical borrower in our sample.

28. 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 5% (\$28.2 billion) of that total.

The most relevant estimates of the interest rate elasticity of mortgage demand for our context come from [DeFusco and Paciorek \(2017\)](#), who study bunching at the conforming loan limit to identify how changes in interest rates affect the intensive margin demand for loan size among jumbo borrowers. Because of the similarities in the institutional context and market segment that they study, these estimates are likely to be portable to our setting. [DeFusco and Paciorek \(2017\)](#) find that a one percentage point increase in interest rates leads to a 2–3% reduction in loan size for borrowers near the conforming loan limit. If we were to extrapolate these estimates to our context, they would imply that the 10–15 basis point increase in interest rates for non-QM loans should lead borrowers who are responding along the intensive margin to reduce their loan sizes by at most 0.2–0.45%. Yet, for the average high-DTI borrower in our sample, the reduction in loan size required to obtain a DTI below the 43% threshold is significantly larger.

For example, the average jumbo borrower above the QM-threshold in 2013 had a DTI of 45%, a loan size of \$622,000, and an interest rate of 4.08%. If we assume that this mortgage was the only debt the household carried, this would imply a monthly payment of \$2,998 and a monthly income of \$6,663. At that income, the borrower would need to reduce the monthly payment to \$2,865 to get below the 43% cut-off, which would require lowering the loan amount to \$594,356, or by roughly 4.4%. This is almost ten times larger than the amount implied by the demand elasticity estimated in [DeFusco and Paciorek \(2017\)](#). If the borrower carried other non-mortgage debt, the required reduction in loan size to obtain a back-end DTI of 43% would be even larger. Moreover, this calculation completely ignores the extensive margin response to the ATR/QM rule, which would be difficult to generate at any plausible demand elasticity given only a 10–15 basis point increase in interest rates. We view this as fairly strong evidence that the reductions in quantity we observe primarily reflect a supply-side response from lenders unwilling to originate non-QM loans.

5.2.3. Product substitution. Thus far, our discussion of the quantity effect has assumed that all loans missing from above the DTI threshold in 2014 that cannot be accounted for by excess mass below the threshold were eliminated from the market entirely. However, because we focus only on fixed-rate mortgages (FRMs), it is possible that these missing loans did not truly disappear from the market but are instead simply missing from our sample. In particular, one potential way for high-DTI borrowers to get below the threshold would be to switch from FRMs to adjustable-rate mortgages (ARMs), which typically feature lower initial interest payments and therefore lower DTIs at origination.²⁹ In our sample, these borrowers would be counted as missing, when in reality they are bunching below the QM-threshold through the choice of an alternative contract. This would lead us to overestimate the extensive margin effect of the policy and underestimate the intensive margin effect.

To investigate this possibility, in Figure 8 we expand our sample to also include ARMs and plot the share of loans in each 1% DTI bin that are ARMs separately for jumbo and conforming loans originated before and after the implementation of ATR. The results in Figure 8A show that the ARM share does indeed increase differentially among jumbo loans with DTIs just below the

29. Under the ATR/QM rule, the interest rate used to calculate the monthly payment that sets the DTI on an ARM is the “fully indexed” rate. This rate is determined by adding the fixed margin specified in the loan contract to the level of the index rate used to adjust the mortgage payments at the time of origination. In January 2014, the average margin on new 5/1 ARM contracts was 2.74%, and the 1-year London Interbank Offered Rate (LIBOR), which is the most common reference rate used to set ARM payments, stood at approximately 58 basis points. This means that the average fully indexed rate in the month that ATR/QM went into effect was roughly 3.32%, which is about one percentage point lower than the average rate for new FRMs originated that month. These figures are based on the authors’ calculations using data from the Freddie Mac Primary Mortgage Market Survey (PMMS) and ICE Benchmark Administration Limited (IBA).

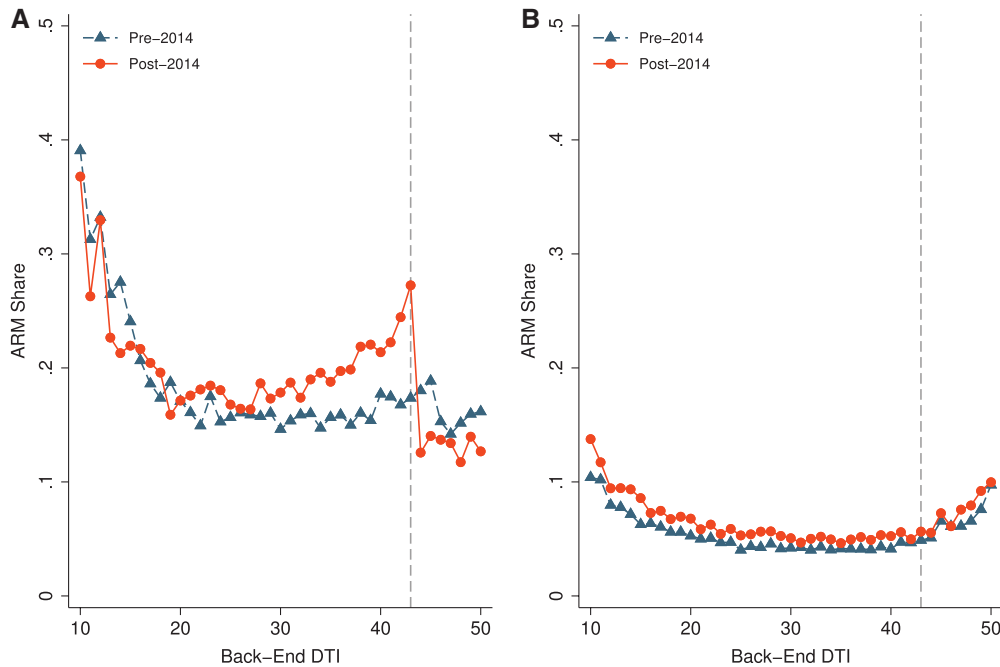


FIGURE 8

ARM share by DTI and market segment for loans originated pre- and post-ATR/QM

Notes:—This figure plots the share of mortgages with adjustable rates originated before (triangles) and after (circles) the implementation of ATR/QM separately by DTI and market segment. Panel A plots the ARM shares for jumbo mortgages only and panel B plots the analogous shares for conforming mortgages. The vertically dashed grey line marks the QM-threshold of 43%. ARM shares are calculated within DTI bins that are created by rounding up to the nearest integer so that the 43% bin includes all DTIs greater than 42% and less than or equal to 43%. The pre-2014 period includes all loans originated prior to January 2014 and the post-2014 period includes all loans originated during or after that month.

43% threshold after the implementation of the policy. This relative increase in the ARM share is not present among conforming loans (Figure 8B) and is consistent with the idea that some otherwise high-DTI fixed-rate jumbo borrowers are switching to adjustable-rate loans as a means for decreasing their DTIs. However, this evidence is not conclusive proof of product substitution. The relative increase in the ARM share at DTIs just below the QM-threshold could arise as a result of a differential intensive margin response among ARM borrowers even in the absence of any FRM borrowers choosing to switch to ARMs.

In Table 4, we explore the role of product substitution directly by reporting bunching estimates separately by product type.³⁰ For reference, column 1 repeats our preferred estimates using FRMs only. In column 2, we include both FRMs and ARMs and re-estimate the intensive and extensive margin effects in the pooled sample. Pooling the sample in this way allows for unrestricted product substitution since FRM borrowers who switch to ARMs to lower their DTIs will be counted as part of the intensive margin bunching response. Similarly, because this sample includes all loan types, the extensive margin response will provide a measure of the true share of high-DTI loans of any type that were eliminated as a result of the policy. The results in the pooled sample continue to indicate large quantity responses. On the intensive margin, we estimate that roughly one-third of all loans that would have otherwise had a DTI above 43% were shifted from above to below

30. The associated plots showing the full empirical and counterfactual DTI distributions are reported in Supplementary Appendix Figure A.5.

TABLE 4
Effects of ATR/QM on the quantity of credit by product type

	FRMs Only	FRMs and ARMs Combined	ARMs Only
	(1)	(2)	(3)
$\hat{B}/\hat{N}_{44+}^{\text{post}}$	0.208*** (0.033)	0.333*** (0.032)	0.665*** (0.123)
$(\hat{M} - \hat{B})/\hat{N}_{44+}^{\text{post}}$	0.154*** (0.041)	0.101** (0.039)	-0.056 (0.146)
Bootstrap replications	100	100	100
Number of observations	418,105	454,360	36,255

Notes:—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 across mortgage product types. Estimates are reported separately for FRMs (column 1), ARMs (column 3) and the pooled sample of fixed- and ARMs (column 2). 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 of the indicated type in the counterfactual no-policy distribution that were shifted from a DTI above the QM-threshold of 43% 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 of the indicated type that were eliminated as a result of the policy. Intensive and extensive margin effects were calculated using the bunching procedure described in Section 5.1 applied separately in each sample of loans. The lower limit of the bunching region is set to $\bar{d}=38$ in all three samples. All specifications use 2013 as the pre-period and 2014 as the post-period. The sample therefore includes all jumbo loans of the indicated type 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.

the threshold. This shift includes both the borrowers who decrease their DTIs while holding product type constant and those who reduce their DTI by switching from FRMs to ARMs. The extensive margin response reported in the second row is also large; it indicates that 10% of all jumbo loans that would have otherwise had a DTI above 43% were eliminated from the market entirely in 2014. While this number is smaller than the 15% extensive margin effect estimated in the FRM-only sample, the two estimates are not statistically distinguishable from one another and the implied *number* of loans lost due to extensive margin responses is larger in the pooled sample.

Finally, in column 3 of Table 4 we also report bunching estimates from the ARM-only sample. This sample is significantly smaller than the FRM sample so the results are somewhat noisier. However, the point estimates are informative about the potential degree of FRM to ARM substitution. In particular, the small *negative* extensive margin response reported in the second row implies that there are more ARM loans bunching below that 43% threshold than can be accounted for by the number of missing ARMs above the threshold. If we assume that all of this excess bunching can be attributed to high-DTI fixed-rate borrowers switching to ARMs, then we can place an upper bound on the fraction of FRMs that are mistakenly classified as missing in our main analysis. Specifically, let $\hat{M}_{\text{FRM}} - \hat{B}_{\text{FRM}}$ denote the estimated number of missing FRM loans implied by our main extensive margin results reported in column 1. Similarly, let $\hat{B}_{\text{ARM}} - \hat{M}_{\text{ARM}}$ denote the excess number of ARM loans bunching below the limit. If we assume that all of this excess bunching is a result of FRM to ARM substitution, then this would imply that at most $100 \times (\hat{B}_{\text{ARM}} - \hat{M}_{\text{ARM}}) / (\hat{M}_{\text{FRM}} - \hat{B}_{\text{FRM}})$ percent of the missing FRM loans in our main analysis are misclassified. Plugging the relevant numbers into this ratio yields an upper bound of 12.2%, which implies that at least 87.8% of the FRM loans we classify as missing were truly eliminated from the market. Alternatively, if we scale our preferred estimate of the extensive margin FRM response down by 12.2%, this would imply that roughly 13.5% of all

fixed-rate jumbo loans that would have otherwise had a DTI above 43% were eliminated from the market in 2014, which is not much different from the 15% baseline effect reported above. While these results suggest that the degree of product substitution induced by the policy change was likely small, this substitution is nonetheless a potentially important unintended consequence of the ATR/QM rule.

5.2.4. Evidence on lender heterogeneity. Our baseline estimates indicate that the ATR/QM rule led to a small increase in price and large reduction quantities in the high-DTI jumbo segment of the mortgage market. While the price effect we find was generally in line with what the CFPB had anticipated, the large quantity response was somewhat less anticipated. Indeed, its prospective cost-benefit analysis of the ATR/QM rule, the CFPB stated that “the Bureau believes that the ability to repay requirements and the accompanying potential litigation costs will create, at most, relatively small price increases for mortgage loans. These small price increases, in turn, are not likely to result in the denial of credit to more than a relatively small number of borrowers [...]” (Consumer Financial Protection Bureau, 2013). In this section, we explore variation in the quantity response across types of lenders to better understand why it may have been so large. Because our data do not contain lender-level characteristics or identifiers, we will not be able to provide a full accounting of this issue. However, some insight can still be gleaned at the loan level by considering the channel through which a mortgage is originated as well as the type of secondary market investor who ultimately ends up holding the loan.

Focusing first on the origination channel, the main distinction we draw is between loans processed through “third-party channels” (e.g. brokers and correspondent lenders) and those processed through the more traditional “retail channel,” in which the same entity that takes the borrower’s application and collects any supporting documentation is also responsible for setting the underwriting criteria and approving the loan. This distinction is important because liability for damages under ATR/QM depends crucially on the level and quality of documentation collected by the mortgage originator at the time the loan is approved. In particular, under the “General ATR Option” even loans with DTIs greater than 43% could be deemed compliant with the ATR rule if the lender can prove that they correctly documented the borrower’s income and arrived at a reasonable, good faith determination that the borrower would be able to repay. Given that proper documentation is costly, it may not be possible for lenders to fully contract with third-party originators in a way that incentivizes them to exert the effort needed to ensure compliance under the General ATR Option. As a result of this agency problem, lenders who rely on third-party originators may find it prohibitively costly to comply under the General ATR Option and may therefore be less likely to extend non-QM, high-DTI jumbo loans subsequent to the policy change.³¹ In contrast, lenders who operate with a more integrated business model and are able to fully internalize the benefits of proper documentation will be less affected and may therefore be able to continue lending at only slightly higher rates.³²

31. Evidence on lenders’ profit margins suggests that this heterogeneity in expected costs may not even need to be all that large to render non-QM lending unprofitable at some lenders. For example, in its Quarterly Mortgage Bankers Performance Report the Mortgage Bankers Association indicated that per-loan profits among independent mortgage companies, bank subsidiaries, and other non-depository institutions averaged roughly 32 basis points of the initial loan balance in the fourth quarter of 2014 (Mortgage Bankers Association, 2015). This margin is well within the range of expected per-loan costs that the CFPB estimated would be created by the ATR/QM rule (Consumer Financial Protection Bureau, 2013).

32. For example, Angel Oak, one of the largest non-QM lenders and securitizers active in the market today cites this vertically integrated model as a key contributor to its success, noting that “[t]he way Angel Oak does it...is a fully integrated model where 100% of the assets we securitize come from our originator. We think this is a competitive advantage because

To investigate this possibility, in Figure 9A we plot the share of jumbo loans originated through third-party channels separately by DTI in the year before and after the implementation of ATR/QM. For the sake of comparison, we normalize these shares within year relative to the third-party share in the 43% DTI bin. In 2013, the third-party origination share was roughly constant across the 43% DTI threshold. In contrast, after ATR became effective in 2014, there is a sharp drop in the third-party share that occurs precisely at the 43% threshold. This relative shift away from third-party and toward retail originations at high DTIs subsequent to the policy change is consistent with the idea that third-party originators differentially exited the non-QM market due to the potential agency problems outlined above.

This shift away from third-party originations are quantified in the first two columns of Table 5, which report results from difference-in-differences regressions measuring the effect of the policy change on the likelihood that a loan is originated through a third-party channel. The top row reports the coefficient estimate on the high-DTI “treatment” dummy, which measures the baseline difference in third-party shares between high- and low-DTI jumbo loans. The bottom row reports the estimated effect of the policy change, which is measured by the coefficient estimate on the interaction between the high-DTI dummy and an indicator for whether the loan was originated in a month following the implementation of ATR/QM. In the first column, we control only for the month of origination. In the second, we add a detailed set of loan-level controls.³³ In both cases, the coefficient on the interaction term indicates that the third-party origination share fell by roughly 30 percentage points for high-DTI loans subsequent to the policy change. These results are consistent with the unconditional evidence in Figure 9 and indicate that agency conflicts between third-party originators and mortgage lenders may have contributed to the large quantity response we observe.

Even in cases where income documentation and loan approval decisions are carried out by the same entity, differences between who originates the mortgage and who the ultimate investor is could also lead to information asymmetries and agency costs that lower the appeal of non-QM lending. Liability for damages under ATR/QM is not limited to just the entity that originates the mortgage; it also extends to any assignees, including secondary market investors who purchase mortgages either in full or through mortgage backed securities. The same agency conflict that is present between mortgage lenders and third-party originators may also exist between potential secondary market investors and originators of any type. If the originator cannot credibly commit to properly documenting the loan, secondary market investors may be less willing to purchase non-QM loans since they cannot be certain how much additional compliance risk they are taking on when doing so.³⁴ This issue is less of a concern, however, for portfolio lenders, who are both the originator and ultimate investor in the loan.

when investors perform due diligence on the sponsor, and therefore the originators, it’s easy for us to say ‘Well, we have only one originator and it’s very easy for us to check the reps and warrants, and to make sure they are following the legal and regulatory guidelines.’ (Angel Oak Capital Advisors, 2018)

33. The set of controls is the same as in our analysis of the interest rate effect in Section 4 and includes fixed effects for county, FICO score (20-point bins), LTV (5-point bins), and property type. The FICO and LTV fixed effects are fully interacted both with each other and the Post dummy. The property-type fixed effects are also fully interacted with the Post dummy.

34. During the lead-up to the final ATR/QM rule-making, the Securities Industry and Financial Markets Association (SIFMA) expressed exactly this concern when it stated before the U.S. House Committee on Financial Services that “[i]n our view, the vast majority of future mortgage lending will be loans that are QMs. Loans that are not QMs will carry with them liability for purchasers of the loans, so-called assignee liability. Due to this liability and supervisory, reputational, and other concerns, we do not expect significant origination of non-QM loans... History has shown that loans that carry significant or uncertain liability are made with a significant pricing premium or not made at all. We believe that lenders in secondary markets would respond to the liability risk through very restrictive

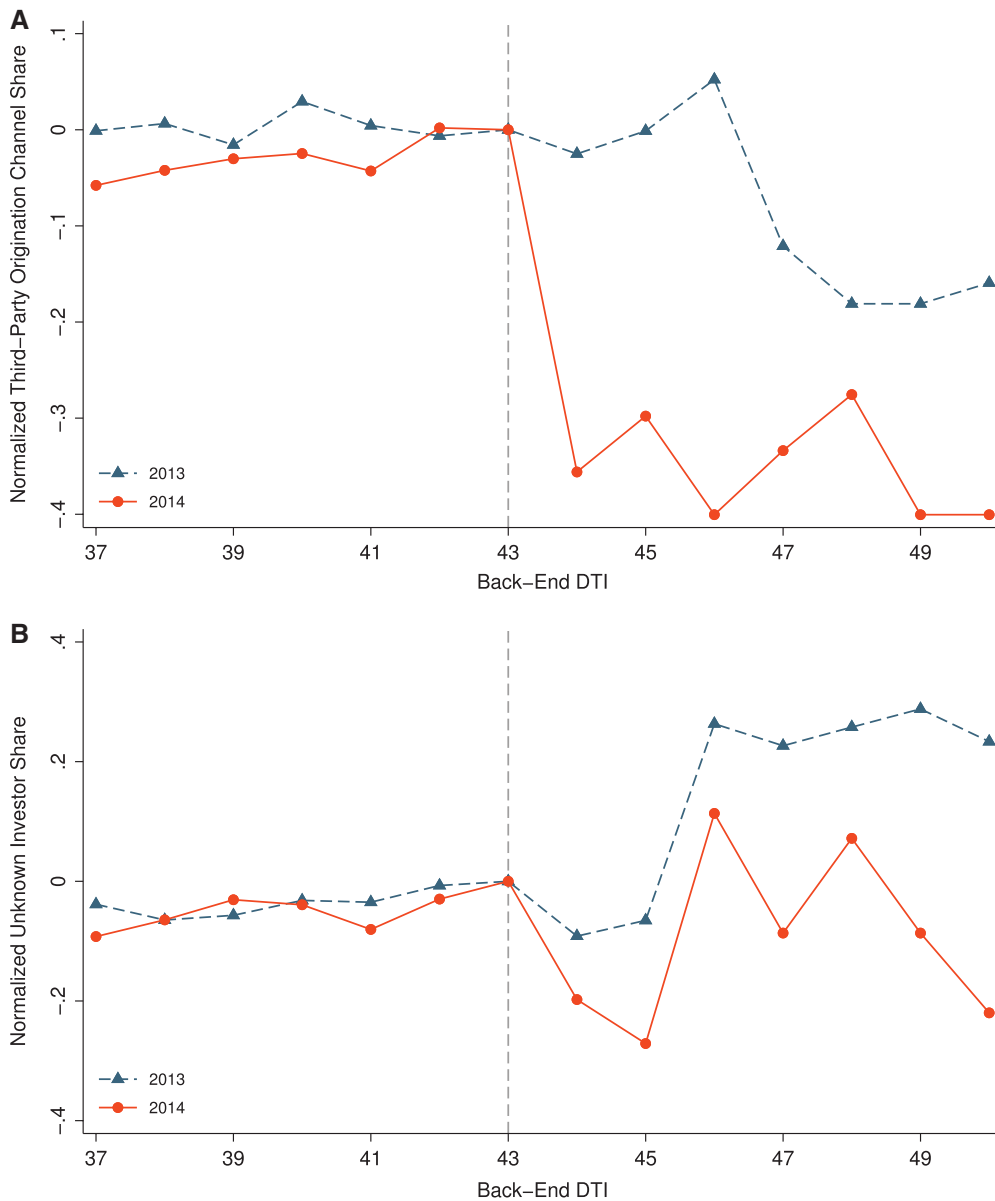


FIGURE 9

Third-party channel and unknown investor shares by DTI and origination year

Notes:—This figure plots the share of jumbo loans originated through third-party channels (A) or with unknown investors (B) by DTI and origination year. Shares are normalized within year relative to the 43% DTI bin so that each dot can be interpreted as the difference between the third-party or unknown investor share in the indicated DTI bin and the corresponding share among loans originated in the same year with DTI equal to 43%. Third-party originations include all loans originated through the correspondent, broker, or wholesale channels. Unknown investors include only instances in which the data explicitly indicates that the investor was unknown rather than being a portfolio investor (*i.e.* loans with missing investor status are not included). Investor status is measured in the third month after origination to avoid misclassifying loans that are temporarily held in portfolio before being sold to unknown, non-portfolio investors. DTI bins are created by rounding up to the nearest integer so that the 43% bin includes all DTIs greater than 42% and less than or equal to 43%.

TABLE 5
The effect of non-qualified mortgage status on origination channel and investor type

	Third-party channel		Unknown investor	
	(1)	(2)	(3)	(4)
DTI > 43	-0.009 (0.009)	-0.006 (0.009)	0.012 (0.012)	-0.000 (0.010)
DTI > 43 × Post	-0.311*** (0.023)	-0.298*** (0.022)	-0.045 (0.036)	-0.051* (0.029)
Month FEs	X	X	X	X
County FEs		X		X
FICO × LTV FEs		X		X
Property type FEs		X		X
FICO × LTV × Post FEs		X		X
Property type × Post FEs		X		X
Number of observations	22,685	22,685	22,685	22,685

Notes:—This table reports difference-in-differences estimates of the effect of non-Qualified Mortgage status on the likelihood that a loan is originated through a third-party channel (columns 1–2) or held by an unknown investor (columns 3–4). Each column reports a separate regression estimated at the loan level in the sample of jumbo loans with DTIs between 36 and 50% with non-missing origination channel and investor status. Third-party originations include all loans originated through the correspondent, broker, or wholesale channels. Unknown investors include only instances in which the data explicitly indicates that the investor was unknown rather than being a portfolio investor (*i.e.* loans with missing investor status are not included). Investor status is measured in the third month after origination to avoid misclassifying loans that are temporarily held in portfolio before being sold to unknown, non-portfolio investors. 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 1 and 3 include fixed effects for the month of origination. Columns 2 and 4 add fixed effects for the county the property is located in, the borrower’s FICO score (20-point bins), LTV (5-point bins), and property type (single family, condominium, townhouse, planned unit development). The FICO and LTV fixed effects are fully interacted both with each other and the Post dummy. The property-type fixed effects are also interacted with the Post dummy. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

While our data do not contain lender identifiers, they do indicate whether a loan is being held in portfolio by the original lender or whether the current investor is “unknown,” which would include loans held by secondary market investors in private securitization pools. In Figure 9B, we plot the share of loans with such unknown investors by DTI before and after the policy change. As before, we normalize these shares within origination year relative to the share in the 43% DTI bin. To avoid misclassifying loans that are only temporarily held in portfolio before being sold to unknown non-portfolio investors, we measure investor status in the third month after origination. At DTIs below the 43% threshold, the relationship between DTI and the unknown investor share is very similar before and after the policy change. At higher DTIs, however, the unknown investor share is significantly lower after the policy change relative to before. This pattern is consistent with the idea that mortgage originators who rely more heavily on secondary market investors differentially exited the non-QM market relative to portfolio lenders after the policy change. Columns 3 and 4 of Table 5 measure this shift away from unknown investors using the same difference-in-differences framework used to measure the change in the likelihood of third-party

underwriting guidelines, significant pricing premiums or both. These actions will result in less available credit to creditworthy borrowers, borrowers who would have otherwise received it had the boundaries of QM been drawn more broadly” (U.S. Congress House Committee on Financial Services, 2012). A similar sentiment was echoed by the American Bankers Association (ABA) in its public comment on the CFPB’s assessment of the rule in 2017 when it stated that “[i]t is worthy of note that...all ATR penalties generally transfer to assignees, which creates legal doubts for investors as well. In this sense, potential costs of court litigation and eventual settlements must form part of the Bureau’s assessment of this rule. For instance, unknown non-QM loan litigation risk has been a primary factor in the failure of investors to support a re-emergence of private label secondary markets” (American Bankers Association, 2017).

origination. While the size of the effect is not as statistically significant, the point estimates imply that the ATR/QM rule led to a reduction in the unknown investor share of roughly 5 percentage points.

Together we view these results as evidence that, by exacerbating pre-existing agency conflicts between various participants in the mortgage origination chain, the ATR/QM rule may have led high-DTI lending to become unprofitable for some types of lenders while still allowing other lenders who operate with a more integrated business model to continue lending at only slightly higher rates. In Supplementary Appendix C, we lay out a simple theoretical framework to show that if the share of lenders affected by these agency costs is large and if borrowers cannot perfectly substitute to less affected lenders, then this type of heterogeneous effect could generate a large aggregate decline in non-QM lending while at the same time only leading to a moderate increase in interest rates for borrowers who continue to receive loans from the lenders who stay.³⁵ While comprehensive data on the share of lenders for which such agency conflicts may be important is hard to come by, aggregate statistics indicate that a substantial fraction of the jumbo market could potentially be affected. For example, nearly 35% of the jumbo loans in our sample were originated through third-party channels and roughly 40% are reported with an “unknown” investor. These statistics are roughly in line with numbers from the more nationally comprehensive HMDA data, which indicate that approximately 20% of jumbo loans made between 2013 and 2014 were sold to an entity other than the originating lender or one of its affiliates.³⁶ Though other mechanisms may certainly be at play, the differential responses by lender type that we have documented indicate that frictions in financial intermediation and agency costs in particular are important factors to consider in the design of policies that seek to regulate household leverage by imposing loan-level costs on lenders.

6. 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 originated. In this section, we turn to analysing the potential effects of the policy on loan performance. This analysis is important as one main goal of the policy was to reduce liquidity driven mortgage defaults. While this was not the only goal of the policy, its effectiveness along this dimension depends crucially on the relationship between DTI and default risk.³⁷ 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 10 plots non-parametric estimates of the historical association between DTI and default for mortgages originated during the run-up to the financial crisis (2005–8).³⁸ We define a loan as having defaulted if the borrower was ever

35. In Supplementary Appendix C, we also discuss evidence indicating that frictions to borrower substitution across lenders in the mortgage market may be large. Importantly, the theoretical mechanism we propose does not require that borrowers be completely unable to switch lenders. Instead, it merely requires that the lag between being rejected at one lender and re-applying to another be large enough to lower the annual flow of new originations.

36. HMDA data only reports a loan as sold if the transaction occurs within the same calendar year that the borrower filed her application. Given that there is often a lag between origination and sale on the secondary market, many loans in HMDA will be recorded as retained when they are in fact sold several months later. This may explain some of the discrepancy between our unknown investor share and the share of loans reported as sold in HMDA.

37. While our focus is on the DTI restriction in this article, 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.

38. 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

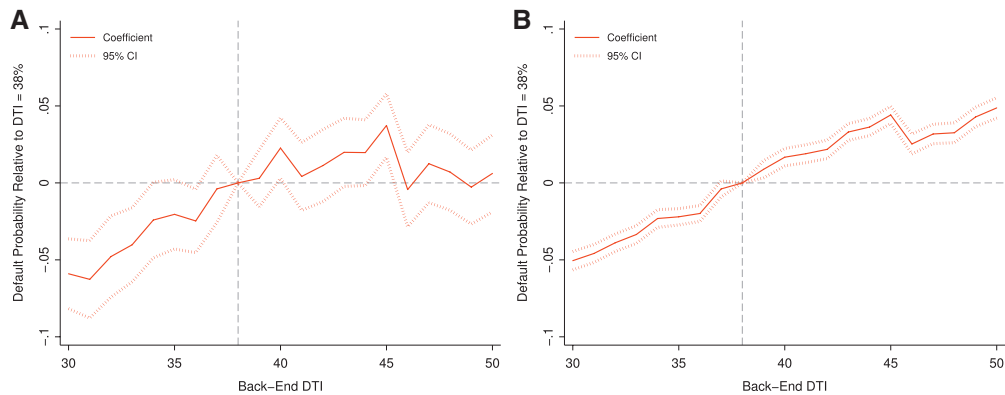


FIGURE 10

Relationship between DTI and 5-year default probability (2005–8)

Notes:—This figure plots the empirical relationship between DTI at origination and the probability of default for loans originated during 2005–8. 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 5 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% bin includes all DTIs greater than 37% and less than or equal to 38%. 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% confidence intervals are based on standard errors that were clustered at the county level.

more than 90 days delinquent or if the property was repossessed by the lender (foreclosure or REO) within 5 years of the origination date. Figure 10A plots the relationship for jumbo loans only and Figure 10B 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 ($\text{DTI} \geq 38$). 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 Figure 10B, 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 non-linear default model on data from 2005 to 2008 and find a small positive relationship between DTI and default.³⁹

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

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).

39. Importantly, this result does not imply that all measures of indebtedness relative to income are uninformative of default. For example, Mian and Sufi (2009) show that the ratio of the number of new mortgage originations in a zip code relative to aggregate zip-code level income was an important correlate of zip-code level default rates during the 2000s housing cycle.

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.

6.1. 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% cut-off 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 5: high-DTI ($\text{DTI} > 43$), medium-DTI ($\text{DTI} \in (38, 43]$), and low-DTI ($\text{DTI} \leq 38$). 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 cut-off.

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 :

$$d_{it}^h = \alpha_c + \delta_t + \beta_L \cdot \mathbb{1}[\text{DTI}_i \leq 38] + \beta_H \cdot \mathbb{1}[\text{DTI}_i > 43] + X_i' \gamma + \epsilon_{it}. \quad (6.7)$$

We consider default rates defined over 1- to 5-year horizons and estimate (6.7) 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.

6.2. 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 5. In particular, we are interested in estimating

$$\Delta \text{DefaultRate} = \sum_i \theta_i (\hat{\delta}_i - \delta_i),$$

where θ_i is the default probability for loans in DTI bin $i \in \{L, M, H\}$, δ_i denotes the observed share of loans in each bin, and $\hat{\delta}_i$ denotes the counterfactual share of loans in each bin under the assumption that the policy was in effect at the time.⁴⁰ If the policy lowers default rates, then this

40. 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.

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

$$\Delta \text{DefaultRate} = (\beta_H - \beta_L)(\hat{\delta}_H - \delta_H) - \beta_L(\hat{\delta}_M - \delta_M). \quad (6.8)$$

Equation (6.8) 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 shift in the aggregate distribution of loans from just above the 43% cut-off ($\hat{\delta}_H - \delta_H$) 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 5. 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 high-DTI jumbo loans that were not made) and α the intensive margin response (the fraction of high-DTI jumbo loans that were shifted to lower DTIs), then the observed and counterfactual number of loans in each bin (N_i and \hat{N}_i) can be related to each other as follows:

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

Noting that $\delta_i = N_i / \sum_i N_i$, these relationships can be used to express the effect of the policy on the aggregate share of loans in each bin

$$\begin{aligned} \hat{\delta}_M - \delta_M &= \frac{\gamma \delta_H}{1 - \gamma \delta_H} \delta_M + \frac{\alpha \delta_H}{1 - \gamma \delta_H} \\ \hat{\delta}_H - \delta_H &= \frac{\gamma \delta_H^2}{1 - \gamma \delta_H} - \frac{(\alpha + \gamma) \delta_H}{1 - \gamma \delta_H}. \end{aligned}$$

Finally, substituting these expressions back into (6.8) yields an expression for the change in the aggregate default rate that depends only on observable quantities:

$$\Delta \text{DefaultRate} = -\frac{\delta_H}{1 - \gamma \delta_H} \left\{ \gamma (\beta_H \delta_M + (\beta_H - \beta_L) \delta_L) + \alpha \beta_H \right\}. \quad (6.9)$$

Equation (6.9) illustrates how the counterfactual change in the aggregate default rate depends on three things: (1) the relative default probabilities for high- and low-DTI loans, (2) the baseline share of loans in each DTI bin, and (3) the share of loans shifted or lost from the high-DTI region due to intensive and extensive margin quantity effects. The extensive margin response (γ) leads to a large reduction in default risk when the default rates in the middle- and low-DTI bins are much lower than the default rate in the high-DTI bin (*i.e.* β_H and $\beta_H - \beta_L$ are large). The intensive margin response (α) reduces the default rate as long as the default rate in the high-DTI region is higher than the default rate in the middle-DTI region ($\beta_H > 0$). Overall, the default effect

depends crucially on the relative default probabilities. When β_H and $\beta_H - \beta_L$ are low relative to the average default probability, the policy cannot generate a large reduction in default risk, regardless of the share of loans in the affected region (δ_H).

6.3. Results

Table 6 reports our estimates of relative 5-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 10, 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). This relatively weak relationship between DTI and default leads to only a small and somewhat imprecisely estimated implied reduction in the aggregate default rate, which is reported in the third row.⁴¹ We calculate this implied reduction in the aggregate default rate according to equation (6.9) using as inputs the quantity estimates from column 1 of Table 3 and the pre-policy high- and medium-DTI bin shares reported in the bottom rows of Panel A. The estimate in column 1 of Panel A implies that even though high-DTI loans constituted 22% of all originations in 2005, eliminating 15% ($\gamma = 0.154$) of these loans and shifting an additional 20% ($\alpha = 0.208$) of them to the medium-DTI region would have only reduced the default rate by a little under one tenth of one percentage point.

In contrast to the jumbo market, 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 (jumbo and conforming). 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 in column 4 of Panel B suggests that the policy would have only reduced the aggregate 5-year default rate by 0.4 percentage points had it been in place at the time those loans were originated. To put this into perspective, the overall average 5-year default rate for the 2008 cohort was approximately 34%.⁴²

Figure 11 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 labour market continued to deteriorate, strengthening the relationship between ATR and default (Foote *et al.*, 2010). However, in all cases, the improvement in the default rate even after 5 years is minimal relative to the overall average default rates experienced during this time. Even though the number of loans shifted or lost as a result of the policy constitute between 5% and 10% of the total market, the improvement in loan performance associated with shifting a loan across the 43% threshold is simply not large enough to generate meaningful changes in the aggregate default rate. While further reductions in default might be possible if the policy reduced the

41. Standard errors for the aggregate default rate estimates are calculating using the delta method and assuming that the covariance between default probability and quantity adjustment estimates is zero.

42. 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 5 years to 11%.

TABLE 6
Estimates of the effect of DTI on the 5-year probability of default

	(1)	(2)	(3)	(4)
	2005	2006	2007	2008
Panel A: Jumbo loans only				
DTI ≤ 38	-0.0303*** (0.0060)	-0.0555*** (0.0105)	-0.0910*** (0.0086)	0.0025 (0.0381)
DTI > 43	0.0034 (0.0062)	-0.0112 (0.0081)	0.0002 (0.0082)	0.0779** (0.0365)
Implied aggregate effect	-0.0009* (0.0004)	-0.0002 (0.0007)	-0.0020** (0.0009)	-0.0053** (0.0022)
High-DTI bin share	0.22	0.25	0.27	0.20
Medium-DTI bin share	0.22	0.24	0.23	0.19
Number of observations	31,529	18,646	17,155	1,186
Panel B: All loans				
DTI ≤ 38	-0.0330*** (0.0018)	-0.0508*** (0.0026)	-0.0689*** (0.0028)	-0.0706*** (0.0038)
DTI > 43	0.0062*** (0.0022)	0.0083*** (0.0025)	0.0228*** (0.0029)	0.0320*** (0.0044)
Implied aggregate effect	-0.0010*** (0.0002)	-0.0017*** (0.0003)	-0.0035*** (0.0005)	-0.0040*** (0.0006)
High-DTI bin share	0.19	0.23	0.26	0.23
Medium-DTI bin share	0.20	0.22	0.22	0.19
Number of observations	353,392	330,550	295,674	91,493

Notes:—This table reports estimates of 5-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. These estimates are calculated as described in Section 6 using the relative default probabilities in the first two rows of each panel, the quantity estimates from column 1 of Table 3, and the pre-policy DTI-bin shares reported in the bottom rows of each panel as inputs. 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 5 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.

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 Supplementary Appendix B 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. This leads us to conclude that the DTI limit, even in its fullest

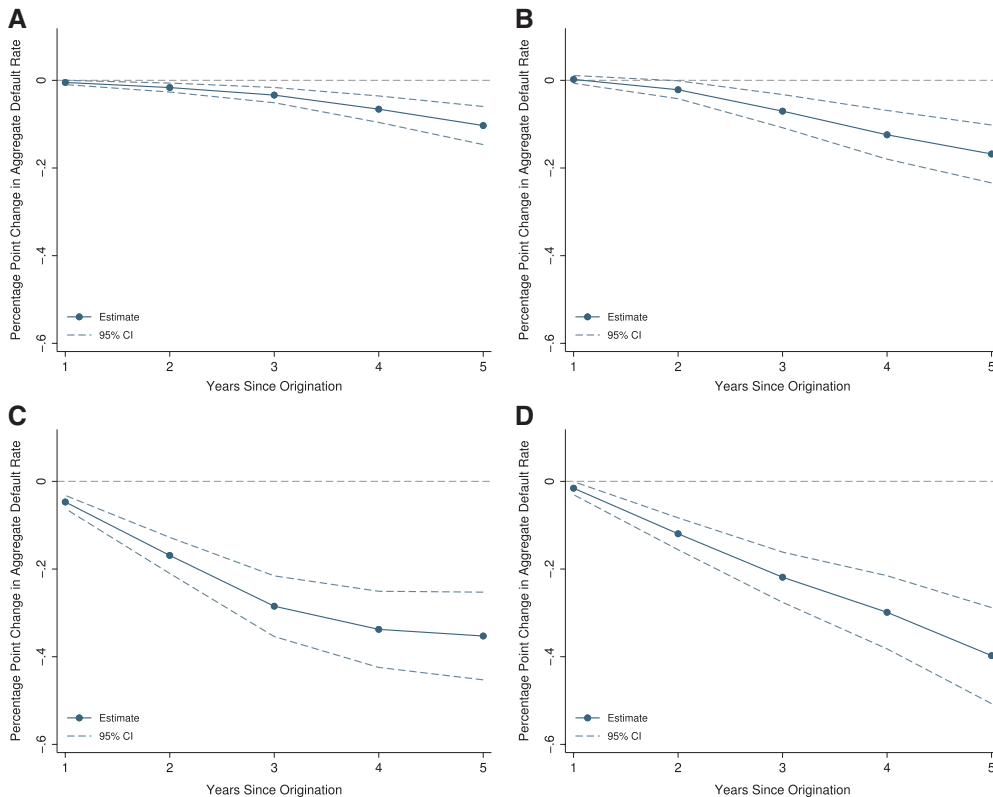


FIGURE 11

Estimated effect of ATR/QM on aggregate default rates by year of origination

Notes:—This figure plots the estimated counterfactual effect of the ATR/QM rule on the aggregate default rate for loans originated in 2005–8 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 1- to 5-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 6 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% 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.

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 rule, 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 individual default risk. Moreover, it is possible that the restrictions on DTI achieved by a policy like the ATR/QM rule may improve financial stability through channels other than the direct reduction in individual default risk associated with lowering a borrower's DTI. For example, a full analysis of the effects of DTI restrictions on financial stability would also incorporate the potentially large effects that restricting DTI may have on housing demand and therefore house price fluctuations.

7. 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 article, 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% of the affected market segment disappeared entirely and 20% of affected loans experienced a reduction in leverage. We interpret this as evidence that lenders responded to the policy not only by raising prices but also by exiting the regulated portion of the market entirely. This fall in lending was substantially larger among lenders and mortgage investors who rely on third-parties to ensure compliance with the regulation, suggesting that frictions in financial intermediation and agency costs in particular are important factors to consider in the design of policies that seek to regulate household leverage. Finally, while the policy was able to achieve large changes in the distribution of DTI, we estimate that this would have caused only a minimal reduction in the aggregate default rate.

While a full welfare analysis is beyond the scope of this article, our results highlight several questions that will be critical to answer before such an analysis can be conducted in the future. In particular, the welfare implications of policy-induced reductions in household leverage depend in large part on the reasons for why households would demand high-leverage in the first place. If households who choose to carry high levels of debt relative to their current incomes are doing so simply to smooth expected increases in future income, then restricting leverage could be welfare decreasing at the individual level. However, if the demand for debt is driven in part by inadequate financial literacy or various behavioural biases and cognitive limitations, as [Campbell *et al.* \(2011\)](#) argue, then there may be a welfare improving role for policies like the ATR/QM rule. Similarly, if choices over household leverage in the mortgage market are driven in part by house price beliefs, as [Bailey *et al.* \(2017\)](#) show, then the welfare implications of policies that limit mortgage leverage may depend on the extent to which such beliefs are based on fundamental versus behavioural factors. Finally, regardless of whether policies like the ATR/QM rule are directly beneficial to the individual households whose leverage they curtail, the aggregate welfare consequences of *ex ante* restrictions on household leverage depend crucially on how such policies affect macroeconomic outcomes like house prices and consumption. While our results on default shed some light on this issue, future work measuring the effects of this type of policy at the aggregate level is certainly needed.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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