The Impact of Crisis-Period Interest Rate Declines on Distressed Borrowers*

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Forthcoming, *Review of Financial Studies*

September 10, 2024

Abstract

We measure the causal impact of reductions in benchmark interest rates on the renegotiation and performance of distressed loans, using 2000s subprime mortgages as a laboratory. Subprime borrowers treated with larger benchmark rate reductions benefited from increased debt-renegotiation probabilities and lower debt-service payments. Modification rates were similar among current and delinquent borrowers but higher for real estate investors, highlighting the role of financial acumen in renegotiation. Renegotiations also reduced longer-run foreclosures, but treated borrowers who lingered in delinquency offset these benefits. Findings suggest monetary easing can spur debt renegotiation but alone may not lead to longer-run curative outcomes.

JEL Classification: E52, E58, R20, R30;

Keywords: Monetary Policy Transmission, Financial and Economic Crisis, Distressed Borrowers, Housing and Mortgage Markets

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Crisis-period economic policy often targets distressed borrowers so as to limit broad deadweight losses to the economy and mitigate adverse distributional outcomes. In the case of monetary easing, however, distressed borrowers may experience problems of debt qualification that disrupt the refinance or debt origination channels of monetary policy (Amromin et al., 2020; DeFusco and Mondragon, 2020). Even among borrowers provided immediate interest rate relief, little is known about the longer-run salutary effects of monetary interventions.

We use 2000s subprime adjustable-rate mortgages (ARMs) to provide new insights into the short- and long-run causal impacts of crisis-era reductions in benchmark interest rates on distressed borrowers. Interest rates on subprime ARMs were typically priced at LIBOR plus a basis point margin and adjusted ("reset") every 6 months during their variable rate period. Borrowers whose mortgages reset during the 2000s period of financial market distress often faced outsized payment shocks as systemic credit risks fueled high LIBOR-based mortgage rates. Borrower payment shocks can lead to mortgage defaults that create deadweight losses for borrowers, mortgage servicers, and loan investors (Posner and Zingales, 2009; Federal Reserve Board, 2012; Adelino et al., 2013; Gupta, 2019; Gabriel et al., 2021).

Mortgage contract terms required that the higher payments associated with ARM resets remain fixed for 6 months. Yet in the wake of crisis-period monetary interventions and an easing of financial conditions, benchmark rates declined quickly, such that the LIBOR values used to initially reset mortgage rates were often substantially higher than prevailing market rates. We find that declines in market LIBOR rates spurred loan modifications. The loan modifications lowered borrower interest rates and reduced the spread between mortgage rates and LIBOR, monthly payments, and the likelihood of default. We call this the debt-renegotiation channel of monetary policy.

However, untangling the causal impacts of the debt-renegotiation channel of monetary policy is complicated by a lack of natural treatment and control groups, as all borrowers are simultaneously exposed to changing interest rates. Thus, our identification strategy exploits a natural experiment that combines ex ante differences in mortgage contract terms with the aggregate volatility in 6-month LIBOR. This approach allows us to simultaneously assess both the debt-renegotiation channel of monetary policy and the quasi-random assignment of different benchmark interest rate changes to otherwise similar borrowers via the loan modification mechanism.

A key characteristic of ARM resets is that the mortgage servicer typically measured LIBOR 15–45 days before adjustment in order to notify the borrower before the new monthly payment. Thus, LIBOR is measured during the lead-up to an ARM payment change. The servicer then informs the borrower about the upcoming payment adjustment and, finally, the borrower's payment changes.

Using this mortgage feature, we construct an instrumental variable (IV) for a two-stage least squares (2SLS) research design. The IV equals the change in 6-month LIBOR between the date of first benchmark interest rate measurement (first measurement) and the date of first payment adjustment (first adjustment) for each ARM. Our assignment of different benchmark interest rate changes across borrowers thus stems from the 6-month LIBOR change between first measurement and first adjustment (henceforth, the loan-level LIBOR change).

In the first stage, we regress the loan-level probability of an interest rate modification between first measurement and first adjustment (an indicator variable) on loan-level LIBOR changes. A steeper decline in LIBOR between first measurement and first adjustment, all else equal, drives loan renegotiations that reduce mortgage obligations or mitigate payment shocks to stave off borrower default, even among borrowers not immediately distressed. This setting differs somewhat from the broader housing literature that focuses on modifications targeting previously delinquent or underwater borrowers facing default (see, e.g., Ganong and Noel, 2023). In the second stage, our 2SLS estimates capture the causal impact of LIBOR change-induced modifications on borrower-level outcomes.

Results show that reductions in benchmark interest rates during the 2000s crisis caused marked increases in debt-renegotiation probabilities. A one-percentage-point drop in 6-month LIBOR during the Great Recession increased the probability of a subprime mortgage interest rate modification by 5.57 percentage points (robust S.E. = 0.30%; t-statistic = 18.59). Given that the overall modification rate for subprime ARMs was 8.6%, an estimate of 5.57

percentage points is substantial. Moreover, benchmark interest rate declines translated into meaningful reductions in debt-service payments: LIBOR change-induced debt renegotiations lowered monthly mortgage interest rate payments by 0.294 log points (robust S.E. = 0.011; t-statistic = 27.34), or, correspondingly, by nearly $480 on average per borrower per month. This decline in mortgage payments also reduced total debt payments relative to income (back-end debt-to-income [DTI] ratios) for modified borrowers, coinciding with a broader easing of their debt-service obligations relative to income.

We also assess heterogeneity in LIBOR change-induced modification rates, borrower benefits, and investor loan losses by mortgage and housing risk proxies. Current borrowers and borrowers facing serious delinquency experienced similar modification probabilities due to a decline in LIBOR, all else equal. Yet conditional on an interest rate modification, delinquent borrowers experienced larger payment reductions over the life of the loan. For the whole group of borrowers 90 days or more delinquent, a one-percentage-point drop in LIBOR led to a $4.5k reduction in payments per borrower, via the debt-renegotiation channel, versus just $3.0k for current borrowers. Thus, the modification benefits of crisis-era interest rate declines largely flowed to distressed borrowers despite a lack of targeting by monetary authorities.

Findings also indicate that debt-renegotiation rates were substantially higher for borrowers viewed as less risky at origination and for real estate investors, all else equal. Our result for real estate investors, who are evaluated relative to owner-occupied borrowers, suggests that borrowers' investment focus and financial acumen are critical to successful renegotiation. Conditional on modification, more risky borrowers at origination and owner-occupied borrowers experienced larger payment reductions over the life of the loan. In the case of owner-occupied borrowers, higher benefits conditional on renegotiation more than offset lower modification rates so that the average owner-occupied borrower gained $3.1k from a one-percentage-point drop in LIBOR, via the modification channel, versus just $2.2k for real estate investors. For more risky borrowers, bigger payment reductions over the life of the loan did not offset lower modification probabilities, but the gains for these borrowers were not trivial. A decline in LIBOR of one-percentage-point reduced payments for the riskiest quintile of borrowers at origination by an average of \$2.2k over the life of the loan through the debt-renegotiation channel. Finally, we find that the relationship between loan-level LIBOR changes and investor loan losses varies little within risk proxies, meaning that investors did not subsidize one set of borrowers over another.

Over the medium- and longer-term, the modifications induced by LIBOR declines led to markedly lower foreclosure rates for treated borrowers: after 48 months, the probability that a borrower loses their home to an REO foreclosure or forced sale with a loss to the loan investor fell by 41.6 percentage points (robust S.E. = 6.17%, t-statistic = 6.74). However, treated borrowers lingering in delinquency offset the foreclosure mitigation benefits of benchmark rate change-induced modifications. Indeed, a loan-level regression of an indicator variable for if a loan entered REO foreclosure, was liquidated with a loss, or was 90 days or more delinquent after 48 months on loan-level LIBOR changes yields a precisely estimated coefficient near zero (estimate = 0.11%; robust S.E. = 0.35%). This finding highlights the longer-run limits of interest rate declines, in and of themselves, in alleviating borrower distress following an economic crisis.

Yet post-renegotiation mortgage performance also varied across regions depending on labor market strength. Borrowers living in regions with stronger employment growth were generally less likely to linger in a state of mortgage nonperformance than borrowers whose regions faced dimmer employment prospects. This evidence suggests that micro-level loan renegotiations may need to be paired with macro-level stabilization policies to be effective over the longer term.

A causal interpretation of our first stage regression, relating loan-level LIBOR changes to the probability of modification, requires loan-level LIBOR changes to be as good as randomly assigned across borrowers (the IV independence assumption). Consistent with the independence assumption, balance tests show that first measurement to first adjustment LIBOR differences are uncorrelated with origination and pre-first measurement borrower characteristics. Likewise, in falsification tests, LIBOR changes do not predict mortgage and credit performance outcomes one year before first measurement.

For the second stage of our 2SLS approach to be valid, we require that loan-level LIBOR changes only affect borrowers through the modification channel (the IV exclusion restriction).

In our case, a violation of the exclusion restriction would arise if LIBOR changes between first measurement and first adjustment affected borrowers through channels outside of the modification mechanism (outside of the first stage relationship). National-level changes in LIBOR likely affect borrowers' asset values, refinancing prospects, and other debt payments. Thus, to account for broader interest rate dynamics, all regressions include first adjustment date fixed effects. We hence compare borrowers within first adjustment date cohorts who face the same market-level interest rates over time. This approach leaves modifications as the only channel through which LIBOR changes between first measurement and first adjustment can affect borrower-level outcomes, consistent with the exclusion restriction. Indeed, a falsification test shows that LIBOR differences between first measurement and first adjustment are uncorrelated with changes in debt-service payments exclusive of first mortgages after controlling for first payment adjustment date fixed effects, meaning that the loan-level LIBOR change IV does not affect other borrower debt obligations.

The last threat to identification centers on the IV monotonicity (no defiers) assumption. In our setup, the monotonicity assumption maintains that LIBOR declines between first measurement and first adjustment will not decrease the probability of modification. During normal economic times not characterized by widespread default and foreclosure, this assumption may not hold. A mortgage investor in subprime PLS securities might be reluctant to modify a loan after a decline in LIBOR between first measurement and first adjustment, as a higher spread to prevailing market rates would translate into additional investor profits. However, the 2000s housing crisis witnessed substantial house price declines and high foreclosure rates among distressed, subprime ARM borrowers, with attendant deadweight losses for borrowers, servicers, and loan investors. Hence, servicers had an incentive to modify subprime ARMs to avoid the costs related to default and losses for loan investors. In several robustness checks, we examine subgroups of potential defiers and find that LIBOR declines between first measurement and first adjustment increase the probability of modification, congruent with the monotonicity assumption.

Our findings have important implications for mortgage markets and the financial economy.

For example, COVID-19-era interest rate declines were correlated with substantial loan modification and forbearance as well as lower bankruptcy rates, consistent with our findings (see Gerardi et al. (2022) and *Bloomberg* (2021, 2022)). Moreover, a widely held view contends that adjustable-rate loans, which constitute over $10 trillion of the global lending market (Alternative Reference Rates Committee, 2021), compared to fixed-rate loans, may be advantageous as adjustable-rate payments adjust downwards during a crisis (Fuster and Willen, 2017; Piskorski and Seru, 2018; Amromin et al., 2020). Our work supports this view and also suggests that modifications may alleviate adverse shocks for loans indexed to credit sensitive rates. More broadly, we find that adjustable-rate borrowers treated with lower benchmark rates are less likely to default.

On a cautionary note, however, our results also suggest that borrowers treated with benchmark rate declines may suffer from subsequent performance difficulties, limiting the crisis-era benefits of lower rates, especially for riskier borrowers facing broader adverse macroeconomic shocks. Future research may assess the performance of modified and adjustable-rate loans in the wake of the COVID-19 pandemic and the subsequent sharp rise in interest rates due to monetary tightening.

1 Data Sources

Our primary loan-level data set comprises the universe of subprime ARM loans sold into private-label securitization (PLS) from Moody's Blackbox. These data cover a multitude of loan-level mortgage origination and performance characteristics. We merge these data with the Equifax consumer credit panel to obtain, for example, the borrower's credit score and estimated debt-to-income before first payment adjustment. For our main analysis, we retain loans with an origination date between 2002M01 and 2006M12 and a first payment adjustment date (the year-month where the mortgage payment adjusts with an interest rate reset) between 2007M10 and 2009M09. Our data set also includes loans at any stage of delinquency 4 months before first adjustment. Yet it does not include loans that borrowers paid off, entered into REO foreclosure, or liquidated with a loss in the month before first adjustment.

To assess the regional impacts of aggregate interest rate declines, we also obtain ZIP code house prices from Zillow and ZIP code household income from the IRS Statistics of Income. Lastly, LIBOR interest rates are from the Federal Reserve Economic Database (FRED), and fed funds futures are from Bloomberg. Finally, we compile Bartik (1991) labor demand shocks from the Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW).

2 2000s Subprime ARMs: Benchmark Interest Rate Indices and Payment Adjustment Frequencies

Table 1 presents counts and summary statistics that describe the ARMs sold into PLS during the 2000s housing boom, where panel A focuses on subprime loans (FICO credit score ≤ 660), and panel B presents summary statistics for non-subprime ARMs (FICO credit score > 660). We tabulate summary statistics for ARMs by the benchmark interest rate index (columns 1 and 2) and the payment adjustment frequency (column 3). Each panel only shows the top-five categories in terms of loan counts.

Typically, ARM interest rates and mortgage payments adjust regularly. The new mortgage rate at adjustment equals a benchmark interest rate (e.g., LIBOR) plus an additional margin.[1] Panel A documents that most 2000s subprime ARMs were indexed to LIBOR (over 86%), with over 74% of these loans indexed to 6-month LIBOR with a mortgage payment that adjusts (e.g., because of an interest rate reset) every 6 months (panel A, row 1). Another 10% subprime ARMs with a 6-month payment adjustment frequency (panel A, row 2) also tracked LIBOR, but with the exact LIBOR term not identified in the data. Yet as the mortgage rates for these ARMs adjust every 6 months (row 2, column 3), many of these loans were also likely benchmarked to 6-month LIBOR. Thus, the subprime ARMs originated and sold into PLS during the 2000s boom followed a specific loan archetype: They were typically benchmarked to 6-month LIBOR with a mortgage interest rate that adjusted every 6 months. Our analysis below focuses on these subprime ARMs as they constituted a substantial share of distressed mortgage debt during the 2000s housing crisis.

Table 1 also examines key origination summary statistics and the share of loans that experi-

[1]Interest rate changes can also be subject to caps and floors at each payment adjustment or over the life of the loan.

enced their first payment adjustment in each monetary policy episode during the 2000s housing bust. Columns 6 and 7 of Table 1, panel A, show that subprime ARMs indexed to 6-month LIBOR with a 6-month payment adjustment frequency (rows 1 and 2) were the riskiest group of 2000s PLS ARMs in Table 1. Their FICO credit score averaged under 600 at origination with a mean LTV near 82.

Next, columns 8-11 in Table 1 show when the share of loans within each row experienced their first payment adjustment by monetary policy episode (e.g., before fed funds easing [column 8], the fed funds easing period [column 9], QE1 [column 10], and the post-QE1 period [column 11]). Thus, columns 8-11 by row sum to 100%. Nearly 30% of ARMs indexed to 6-month LIBOR with a 6-month payment adjustment frequency (row 1 of panel A) experienced their first payment adjustment before the Fed began lowering interest rates (e.g., before 2007M10). An additional 46% entered their first payment adjustment period during the traditional fed funds easing cycle (200710 to 2008M11). Thus, by the start of QE1 in November 2008, over 75% of these subprime ARMs experienced floating interest rates, meaning that monetary easing and broader interest rate declines could have a marked impact on these loans. It is the causal effects of such interest rate declines on subprime ARM borrowers, induced in part by monetary easing, that we aim to uncover below.

Finally, for comparison, Table 1, panel B, shows the same counts and summary statistics for the top-five non-subprime ARM categories by interest rate index and first payment adjustment date. A notable share of non-subprime ARMs tracked 6-month LIBOR with a 6-month payment adjustment frequency (33% from panel B, row 1, column 5). Yet non-subprime loans were substantially less concentrated in a specific interest rate index or payment adjustment frequency group. In contrast, as noted above, subprime ARMs originated during the 2000s boom typically reset every 6 months with interest rates indexed to 6-month LIBOR.

3 Institutional Details: PLS and 6-Month LIBOR ARM Modifications

This section discusses the institutional details surrounding PLS modifications and the renegotiation rates for LIBOR ARMs. In aggregate, modification rates for mortgages sold into private-label securitization (PLS mortgages) remained low throughout the 2000s housing bust (Adelino et al., 2013). The blue line in Figure 1, panel A, shows that the average modification rate for all PLS loans originated from 2002 to 2006 reached just 2.5% by 2009 and barely breached 5% by the end of 2010. In contrast, the modification rate for ARMs sold into PLS and indexed to 6-month LIBOR, the focus of this study, rose sharply with the onset of the crisis and reached nearly 20% by 2011, with cumulative modification rates for some subprime LIBOR ARM first payment adjustment cohorts exceeding 30% by 2010 (panel B).

Broadly, most private-label securitizations allow for loan renegotiation. In fact, "[t]he decision to modify mortgages...rests with the servicer, and servicers are instructed to manage loans as if for their own account and maximize the net present value of the loan" (Congressional Oversight Panel, 2009). While the pooling and servicing agreements (PSAs) that govern servicers' management of loans in private-label securities vary, Hunt (2013) finds that 60% of PSAs associated with 2006 subprime securitizations provide express authority for servicers to modify loans. Another 32% of securitization contracts are silent on the servicer's right to modify loans, but the grant of power to manage the loans is assumed to allow the servicer the right to modify loans. Only 8% of subprime securitizations ban modifications outright.

PLS securitization contracts do specify certain conditions for modification. For example, half of securitization contracts that expressly authorize modifications require permission from third parties, such as the rating agency, the trustee, or the credit issuer, to modify more than 5% of the loan pool (Hunt, 2013). However, as noted by a Congressional Oversight Panel (2009), "[w]hile restrictive PSAs present an obstacle to foreclosure mitigation efforts, it is important not to overstate their significance...Further, to date the Panel knows of no litigation against mortgage servicers for engaging in modifications that violate the terms of PSAs."

The institutional features supporting the renegotiation of distressed loans like 6-month subprime LIBOR ARMs emanate from financial incentives encouraging servicers to keep borrowers in their homes via modification rather than initiate foreclosure. First, foreclosure is more expensive than modification for the servicer (Adelino et al., 2013) and can create deadweight losses for borrowers and loan investors (Posner and Zingales, 2009; Federal Reserve Board, 2012; Gabriel et al., 2021), prompting servicers to renegotiate distressed mortgages, like subprime LIBOR ARMs, at a higher rate than the broader, better performing PLS loan population.

Differences in servicing fees also contribute to higher modification rates for LIBOR ARMs. The average annual servicing fee for a LIBOR ARM originated from 2002 to 2006 was 0.49% of principal balance, compared to just 0.32% for the average PLS mortgage. Servicers therefore could increase revenue by prioritizing modifications for LIBOR ARMs to increase their duration at the expense of lower revenue-generating loans. Indeed, Diop and Zheng (2022) find that larger servicing fees predict higher modification rates. Moreover, unpaid servicing fees have precedence over investors' rights to proceeds in a foreclosure sale (Diop and Zheng, 2022), further incentivizing servicers to avoid foreclosure, lengthen the life of the loan, and let any unpaid servicer fees accrue.

In our data, as discussed below in Section 6.4, a LIBOR change-induced modification boosted servicer fees by nearly $6700, equivalent to a move along the interquartile range for unmodified loans.

Finally, loans indexed to LIBOR are intended to reflect the contemporaneous cost of credit in the broader LIBOR market. Therefore, mortgage servicers likely viewed struggling borrowers assigned a higher LIBOR value at first measurement relative to first adjustment as prime targets for renegotiation. In these cases, an interest rate modification that lowers the mortgage rate aligns borrower interest rate expenses with loan investors' LIBOR funding costs while reducing the probability of borrower default.

4 Instrumental Variable Identification Strategy

Subprime 2000s ARMs typically contained an initial fixed-rate period followed by a subsequent interest rate adjustment. After adjustment, the new interest rate equaled a benchmark interest rate (e.g., LIBOR) plus an additional margin.[2] To allow mortgage borrowers to prepare for mortgage interest rate and payment changes, mortgage contract terms typically required servicers to measure the interest rate index several days before payment adjustment. This so-called "lookback period," often between 15 and 45 days, varied across loans and often led to different benchmark interest rates even for loans with the same benchmark interest rate index and first payment adjustment date.

We exploit this variation in lookback periods, along with the volatility of 6-month LIBOR, in our IV strategy to assign different benchmark interest rate changes to otherwise similar borrowers. First, note that, as stated above, we focus on 6-month LIBOR-indexed ARMs with a 6-month payment adjustment frequency as these loans comprise the vast majority of subprime ARMs originated during the 2000s boom (Table 1). Moreover, to ensure that borrowers are ex ante otherwise similar, we only consider interest rate resets around each loan's first payment adjustment. Thus, our assignment of different benchmark interest rates to each borrower stems from the change in 6-month LIBOR *between* the first interest rate measurement date and first payment adjustment date for each loan (the loan-level LIBOR change). Borrower-level variation in LIBOR changes arises because of (1) the timing of interest rate measurement and (2) volatility in the path of 6-month LIBOR. The volatile nature of LIBOR during the 2000s crisis (e.g., Figure 2, panel 1A) creates notable variation in the change in 6-month LIBOR between first measurement and first adjustment across borrowers.

Our first fundamental supposition, the first stage in our 2SLS strategy, is that larger benchmark interest rate declines, as measured by LIBOR changes between first measurement and first adjustment, increase debt renegotiations. A more sizable reduction in 6-month LIBOR between first measurement and first adjustment led to a wider spread between the LIBOR rate used in the ARM rate calculation and its actual market value at first adjustment. In other words, ARM rates in such cases were higher than had LIBOR been measured at first adjustment. As a large share of ARM borrowers were distressed during the 2000s housing crisis, higher ARM rates likely increased defaults and created deadweight losses for borrowers, mortgage servicers,

[2] ARMs were also often subject to rate caps and floors at each adjustment and over the life of the loan.

and loan investors (Posner and Zingales, 2009; Federal Reserve Board, 2012; Adelino et al., 2013; Gabriel et al., 2021). The high cost of such defaults likely incentivized servicers to reduce borrower interest rates towards prevailing market rates through loan modification.

For the relationship between loan-level LIBOR changes and modifications to be causal, the difference in LIBOR between first measurement and first adjustment must be as good as randomly assigned (e.g., the IV independence assumption). Gupta (2019) finds that lookback periods are uncorrelated with pretreatment borrower characteristics. Yet our identification scheme adds an additional layer of exogeneity by also relying on the aggregate volatility of 6-month LIBOR. In all regressions, we control linearly for the number of lookback days, akin to a time trend. So, the key identifying assumption is that the non-linear change in 6-month LIBOR between first interest rate measurement and first payment adjustment is independent of ex ante borrower characteristics. To assess this assumption, we use balance tests to examine the correlation between the loan-level LIBOR change and various origination and pre-first interest rate measurement borrower and ZIP-code-level variables. If the loan-level LIBOR changes are as good as randomly assigned, they should be uncorrelated with ex ante borrower-level characteristics. The results are in Table 2, which shows coefficient estimates from separate loan-level regressions of each variable in the left column on the loan-level LIBOR change. We standardize each left-hand-side variable to have zero mean and unit variance to facilitate the interpretation of the coefficients. In each regression, we control for the lookback period (in days). Controls also include first payment adjustment fixed effects so that the estimates are a (weighted) average of correlations taken within each first payment adjustment year-month. Each regression uses 350,946 loan-level observations, and robust standard errors are clustered at the three-digit ZIP code level.

Table 2 indicates that loan-level LIBOR changes are uncorrelated with ex ante borrower-level outcomes. All coefficient estimates in Table 2 are small in magnitude and not statistically significant at the 1% level. These results extend to origination characteristics, such as the FICO credit score, the combined loan-to-value ratio (CLTV), the initial interest rate, and the interest rate margin (a risk proxy for ARMs). There is also a limited correlation between loan-level

LIBOR changes and variables measured 4 months before first adjustment, including the FICO credit score, Equifax estimates of income, the imputed LTV, an indicator for if the loan was ever modified, and delinquency proxies. The final two rows of Table 2 show that loan-level LIBOR changes are also uncorrelated with ZIP-code-level income in 2006 and crisis-era pretreatment house price growth measured between 2006M01 and 2007M08. Note that the coefficient on the loan-level LIBOR change is significant at the 5% level (but not at the 1% level) when the dependent variable corresponds to an indicator variable for if the loan was ever 60 days delinquent as of 4 months before first payment adjustment. Yet this estimate is only marginally significant, and all other ex ante borrower-level characteristics are uncorrelated with loan-level LIBOR changes. Indeed, the other delinquency proxies in Table 2, including the number of days delinquent, if the loan was ever 90 days delinquent, or if the loan was ever 150 days delinquent, are uncorrelated with loan-level LIBOR changes.

Using the first measurement to first adjustment differences in 6-month LIBOR, we estimate our first-stage loan-level regression to examine the impact of loan-level LIBOR changes on loan modifications:

$$\text{Mod}_{it} = \lambda_t + \Gamma_i X_i + \pi \mathit{LIBORChange}_{it} + \eta_{it}. \quad (1)$$

The dependent variable is an indicator variable that equals one for an interest rate modification for loan i at first payment adjustment date t. We define an interest rate modification as a one-percentage-point or more decline between the ex ante expected interest rate based on the loan contract terms using 6-month LIBOR at the first measurement date and the actual ARM interest rate during the first remittance period following first adjustment. Note that this modification definition aptly captures the modifications in the broader data set, as 73% of modifications identified by Moody's Blackbox satisfy this interest rate modification definition.[3]

The candidate instrument is the first measurement to first adjustment 6-month LIBOR change for each loan, $\mathit{LIBORChange}_{it}$. We code $\mathit{LIBORChange}_{it}$ so that more negative values indicate a steeper decline in LIBOR between first measurement and first adjustment. The first-

[3]Eighty-three percent of modifications identified by Moody's Blackbox are associated with an interest rate reduction of 0.5 percentage points or more. Our results are similar but smaller in magnitude, as expected, if we use 0.5 percentage points as the threshold for an interest rate modification. See Internet Appendix A.

stage effect, π, is of interest. Its negation measures the impact of a one-percentage-point decline in LIBOR on the probability of loan modification. Hence, π captures the debt-renegotiation channel of benchmark interest rate declines and represents the transmission of aggregate benchmark interest rate reductions to subprime ARM modifications.

In Equation (1), λ_t represents first payment adjustment month fixed effects. In our 2SLS framework, these fixed effects ensure that modifications are the only channel through which interest rate changes affect borrower outcomes so that the IV exclusion restriction holds. In our case, the exclusion restriction would be violated if loan-level LIBOR changes affected borrower-level outcomes through any channel outside of the modification mechanism. The first payment adjustment date fixed effects allow us to compare borrowers who face the same macroeconomic environment by grouping them into first payment adjustment date cohorts. Hence, broader interest rate and other macroeconomic changes equally affect borrowers' refinancing prospects, financial assets, and other debts across treatment and control groups, making modifications the only channel through which loan-level LIBOR changes can affect borrower outcomes, consistent with the exclusion restriction. In this sense, our setup matches the education literature where researchers control for such "risk sets" across cohorts using a fixed effects strategy (see, e.g., Angrist et al., 2023).

To identify the average causal treatment effect for compliers (loans that receive a modification based on a steeper LIBOR decline between first measurement and first adjustment) via 2SLS using the above first-stage equation, we also require the monotonicity (e.g., no defiers) assumption. Within the context of Equation (1), the monotonicity assumption states that a larger LIBOR decline will not decrease the probability of modification. In normal economic times, this assumption may not hold as a wider spread between LIBOR at first measurement and prevailing market rates at first adjustment would correspond to higher investor profits, making servicers less inclined to modify such loans. However, during the 2000s housing crisis, subprime borrowers were broadly distressed. As foreclosure can create deadweight losses for borrowers, mortgage servicers, and loan investors (Posner and Zingales, 2009; Federal Reserve Board, 2012; Adelino et al., 2013; Gabriel et al., 2021), servicers likely had an incentive to modify these loans to avoid the fallout from default. Indeed, evidence below from binscatters shows that the first stage is monotonic. Further, in robustness checks, we examine various subgroups of potential defiers (e.g., ex ante high-quality borrowers) or remove likely defiers (e.g., current borrowers 4 months before first adjustment) and find no differences in our results. Defiers hence are unlikely in our data, and the monotonicity assumption likely holds.

X_i is a vector of borrower origination and pre-first adjustment controls with coefficient Γ_i. These controls include several origination borrower characteristics, such as the FICO credit score, the CLTV, and three-digit ZIP code fixed effects. Controls also include borrower-level variables measured 4 months before first adjustment, such as the loan balance, FICO credit score, income estimates, debt-to-income proxies, and delinquency status.[4] η_{it} is the error term.

We then model the causal effect of modifications on borrower-level outcomes:

$$\mathrm{y}_{it} = \alpha_t + \gamma_i X_i + \rho \mathrm{Mod}_{it} + \varepsilon_{it}. \tag{2}$$

α_t are the first adjustment month fixed effects, X_i is the vector of borrower-level ex ante characteristics (listed in footnote 4), and Mod_{it} is the modification indicator for loan i with first payment adjustment date t.

We consider several borrower-level outcomes for the left-hand-side variable, y_{it}, but the first variable of interest is the monthly mortgage interest rate payment. When the dependent variable is the monthly mortgage interest rate payment, the 2SLS estimate, ρ, is the local average treatment effect (LATE) given by the ratio of (1) the average effect of the loan-level LIBOR change on monthly mortgage interest payments (reduced form), and (2) the average effect of the

[4]Controls include the lookback period (in days), origination FICO credit score, origination CLTV, the origination ARM interest rate margin, the origination interest rate, the loan balance 4 months before first adjustment, and an indicator if the loan had ever been 60 days delinquent 4 months before first adjustment. Controls also include fixed effects for the three-digit ZIP code, the origination year-quarter, owner-occupied type, the purpose of the loan, the property type, the type of loan documentation at origination, the loan type, and the MBA delinquency status 4 months before first adjustment. We also include ventiles for the imputed LTV 4 months before first adjustment, the FICO credit score 4 months before first adjustment, the Equifax estimated debt-to-income ratio 4 months before first adjustment, the log of Equifax estimated income 4 months before first adjustment, the amount of funds available in HELOC loans relative to the size of the monthly mortgage payment 4 months before first adjustment, the amount funds available on credit cards relative to the size of the mortgage payment 4 months before first adjustment, the household income for each borrower's ZIP code in 2006, the log difference in ZIP code house prices from 2006M01 to 2007M08, and the county Bartik labor demand shock from 2006M01 to 2010M12. For these latter variables, where we convert numeric variables to ventiles, we also include a separate dummy in cases in which the variable is missing so that missingness does not bias our sample.

loan-level LIBOR change on the probability of modification (first stage; Equation (1)). More plainly, the 2SLS LATE estimates capture the impact of LIBOR change-induced modifications on monthly mortgage payments and are the causal chain that represents the transmission of benchmark interest rate declines to monthly mortgage payments via loan modification. This is the debt-renegotiation channel of monetary policy.

In alternative specifications, we also construct our main instrument using a leave-one-out, jackknife estimator, following Gupta and Hansman (2022). For each loan, the jackknife estimator captures the lookback period × reset month interest rate variation by computing the mean interest rate for all other loans with the same lookback period and reset date.[5] The results using this jackknife-based instrument (Internet Appendix B) are congruent with our main findings, highlighting the robustness of our identification scheme.

5 Preliminary Evidence: LIBOR Changes, Subprime ARM Mortgage Rates, and Modifications

This section provides an overview of interest rates and monetary policy shocks during the Great Recession, plots of realized LIBOR ARM mortgage rates versus those predicted from loan contract terms to highlight the breadth of interest rate modifications over our sample, and preliminary, visual evidence of our first-stage and reduced-form relationships.

First, Figure 2, panel 1A, shows the path of benchmark interest rates from the start of the subprime bust in 2007 through the early QE period. The graph plots the expected fed funds rate in 6 months as measured by fed funds futures (blue line) and 6-month LIBOR (red line).

Panel 1A shows that while the expected fed funds rate and 6-month LIBOR follow the same general downward trend, 6-month LIBOR experienced relatively large bouts of volatility, especially from late 2007 through early 2009. Because of ex ante differences in interest rate measurement dates for otherwise comparable loans, these large swings created differences in LI-

[5] The jackknife estimator for loan i is the mean interest rate for all other loans with the same lookback period ($L(i)$) and reset month ($m(i)$) at t: $z_{it} = \frac{1}{n_{L(i)\times m(i)}-1} \sum_{j=1,j\neq i}^{n_{L(i)\times m(i)}} Int_{jt}$. Int_{jt} is the interest rate for loan j at time t and $n_{L(i)\times m(i)}$ is the number of surviving loans originated lookback period $L(i)$ with reset period $m(i)$. We would like to thank Arpit Gupta for pointing us in this direction.

BOR changes between borrowers' first interest rate measurement and first payment adjustment dates. This yields the quasi-random assignment of different benchmark interest rate changes to otherwise similar borrowers. Indeed, through mid-2007, the spread between 6-month LIBOR and the expected fed funds rate was narrow as credit conditions had yet to tighten. Then, with the onset of the Great Recession and housing crisis-induced financial market distress, the expected fed funds rate fell sharply.

In contrast, 6-month LIBOR became volatile and remained elevated. The 6-month LIBOR–expected fed funds rate spread thus widened, signaling a broader deterioration in credit conditions. At the end of 2007, 6-month LIBOR fell quickly, but its spread relative to the expected fed funds rate persisted. In mid-2008, the expected fed funds rate and 6-month LIBOR increased somewhat prior to the Lehman Brothers crisis in September 2008, where LIBOR spiked. In late 2008, LIBOR fell rapidly before rising again slightly in early 2009. At that point, the expected fed funds rate had neared its zero lower bound, and LIBOR began a gradual downward trend that continued through 2010.

Panel 1B documents the impact of monetary policy shocks on 6-month LIBOR during both the conventional (red line) and QE (blue line) periods. To measure the LIBOR response to conventional monetary policy actions, we calculate the difference in 6-month LIBOR from the day before to the day after each FOMC meeting (Vissing Jorgensen and Krishnamurthy, 2011). We then cumulatively sum these changes from January 2007 to October 2008 to get the total impact of conventional monetary policy shocks on 6-month LIBOR. The red line in panel 1B indicates that conventional monetary policy shocks had a sizable effect on 6-month LIBOR, leading to a one-percentage-point decline from mid-2007 to early 2008. During the latter half of 2008, conventional monetary policy shocks were contractionary, and 6-month LIBOR increased by 60 basis points. The blue line in panel 1B similarly computes the impact of unconventional monetary policy shocks on 6-month LIBOR. QE dates are from (Greenlaw et al., 2018, (GHHW)).[6] The blue line in panel 1B shows that unconventional monetary policy

[6] Using the GHHW QE dates, we measure the impact of monetary policy shocks on 6-month LIBOR during the zero-lower bound period by cumulatively summing the difference in 6-month LIBOR from the day before to the day after each monetary policy date, as during the conventional monetary policy episode.

shocks lowered 6-month LIBOR by over 100 basis points.

Next, Figure 2, panel 2A, documents that sizable mean differences surfaced between actual 2000s subprime ARM interest rates versus those predicted by origination loan contract terms, highlighting the prevalence of interest rate modifications during the housing bust. In particular, panel 2A focuses on subprime ARMs indexed to 6-month LIBOR with a 6-month payment adjustment frequency (e.g., Table 1, row 1), whose first adjustment occurred in January 2009 (e.g., the first time that their payment adjusts ("resets") following the initial fixed-rate period). The plot shows the predicted (blue-dashed line) versus the actual (red-solid line) credit risk-adjusted mean ARM interest rate. The predicted interest rate represents the estimated mean interest rate that would have prevailed had no borrowers received a mortgage modification. The plotted credit risk-adjusted mortgage interest rates account for origination loan-level differences: They correspond to a borrower with a FICO credit score of 600, an LTV of 82, an ARM interest rate margin of 6%, and the sample mean origination balance and initial interest rate. More specifically, the credit-risk-adjusted interest rates are the estimates of year-month fixed effects from the following regression (without an intercept), estimated separately for the actual and predicted mortgage interest rates:

$$\begin{aligned} \text{Interest Rate}_{it} = \tau_t &+ \beta_1(\text{FICO}_i - 600) + \beta_2(\text{LTV}_i - 82) \\ &+ \beta_3(\text{Margin}_i - 6\%) + \beta_4(\text{OrigBal}_i - \text{OrigBal}_{\text{mean}}) \\ &+ \beta_5(\text{Initial Rate}_i - \text{Initial Rate}_{\text{mean}}) + \varepsilon_{it}. \end{aligned} \tag{3}$$

Figure 2, panel 2, reports the fixed effect estimates ($\hat{\tau}_t$) from Equation (3). Panel 2A plots the fixed effect estimates when predicted (blue-dashed) or actual (red-solid) mortgage interest rates represent the outcome variable. Panel 2B shows the output from similar regressions but where monthly predicted and actual interest rate payments represent the dependent variables. The blue-dashed line in panel 2A documents that the ex ante expected mean mortgage rate would have increased from just under 8.5% to over 9% in January 2009 with the first payment adjustment. Panel 2B shows that the corresponding increase in interest rate payments (not including principal payments) using the predicted interest rate would have averaged $140 per month (a nearly 10% increase). However, the red-solid line in panel 2A highlights how the mean realized mortgage rate fell dramatically with the start of the variable rate period. These large differences between the mean actual and predicted interest rates imply that several borrowers with a first adjustment date in January 2009 received a modification that led to an economically meaningful decline in debt-service payments (panel 2B, red line).

Disparities between predicted and actual interest rates following the initial reset extend across first adjustment date cohorts. In Figure 2, panel 3A, we plot the credit risk-adjusted mean difference in the actual and predicted subprime ARM mortgage rates for six quarters after first adjustment by first adjustment year-quarter from 2007Q4 through 2009Q3. Darker lines represent an earlier year-quarter of first adjustment. As in panel 2, the realized and predicted subprime ARM interest rates are credit risk-adjusted using the regression in Equation (3) by each first adjustment date. The graph documents considerable differences between the actual and predicted mean mortgage rates for all first payment adjustment cohorts. Once ARMs near first adjustment, the mean realized interest rate falls relative to the rate expected from loan contract terms. The differences between the realized and predicted interest rates are noteworthy, ranging from 1 to 1.6 percentage points. Therefore, several 2000s subprime borrowers received an interest rate modification during the housing crisis and experienced a decline in interest rate payments.

Figure 2, panel 3B, shows that LIBOR changes have little impact on subprime ARMs before the initial interest rate reset. Each confidence band in the figure corresponds to estimates from a separate regression of the probability of an interest rate modification on a placebo loan-level LIBOR change by placebo first payment adjustment date. To build the sample for these placebo regressions, we first compute the placebo first interest rate measurement dates by subtracting the number of lookback days in each loan contract from the placebo first payment adjustment dates plotted along the horizontal axis in the figure. The placebo loan-level LIBOR change for each regression is then the difference in realized LIBOR values between the placebo first measurement and placebo first adjustment dates. We include all loans before their actual first payment adjustment date, and controls include origination risk proxies.[7] Note that the

[7] Controls include the number of lookback days, first payment adjustment date fixed effects, and the following variables at origination: the credit score, CLTV, the ARM interest rate margin, the interest rate, and the loan

confidence bands, based on ±2.5 robust standard errors, widen for later placebo first payment adjustment dates as the loans with earlier interest rate resets fall out of the sample, reducing the sample size and increasing the standard errors.

The results in panel 3B indicate that LIBOR changes do not affect the probability of an interest rate modification before the initial ARM reset. Hence, only LIBOR changes during the reset window impact renegotiations.

Overall, Figure 2 shows that high rates of ARM renegotiations coincided with a period of declining benchmark interest rates. Yet this preliminary evidence alone cannot ascribe subprime ARM modifications to broader interest rate declines. Indeed, benchmark interest rates were falling because of widespread economic and financial market distress, induced at least in part by struggling subprime borrowers targeted for modification. Thus, macroeconomic and mortgage market performance likely contaminates any naive associations between falling benchmark interest rates and subprime interest rate modifications. Therefore, we employ our IV strategy from Section 4 to generate causal estimates of the impact of LIBOR change-induced modifications on borrower-level outcomes.

Figure 3, row 1, visualizes the first stage of our 2SLS design by plotting the mean probability of an interest rate modification over time by first payment adjustment year-quarter (panel 1A) or for our whole sample by binned loan-level LIBOR changes (panel 1B). In panel 1A, we plot the first and fourth quartiles of subprime ARMs sorted by the difference in LIBOR between first measurement and first adjustment using no control variables. The plot documents that loans treated with a larger LIBOR decline (1st quartile, red line) experienced substantially higher average modification probabilities than those treated with relatively smaller LIBOR declines (e.g., 4th quartile, green line). Moving from the fourth to first LIBOR change quartile (green line to red line) corresponds to an increase in the probability of modification of nearly 2 percentage points in 2008Q1 and over 10 percentage points in 2009Q1. Over the whole sample, the binscatter in panel 1B likewise shows that a steeper LIBOR decline between first interest rate measurement and first payment adjustment leads to a higher likelihood of interest

balance.

rate modification. This relationship is linear across loan-level LIBOR changes, not driven by outliers, robust to different groups of controls, and broadly monotonic, supporting a linear regression specification in the first stage as well as the IV monotonicity, no-defiers assumption.

Figure 3, row 2, plots reduced form estimates that emphasize the causal transmission of LIBOR declines to average mortgage payments. Importantly, the first-to-fourth quartile LIBOR differential pattern in mortgage payments over time (panel 2A) mirrors the same LIBOR differential pattern in the probability of modification (panel 1A). The binscatter in panel 2B reinforces these findings over the whole sample in both a baseline model (blue) and one that accounts for the origination credit metrics (purple) listed in the table notes. These reduced-form estimates show that subprime borrowers treated with steeper LIBOR declines experienced lower average mortgage payments.

6 Results

Figure 4, panel 1A, plots the interquartile range for the change in 6-month LIBOR between first interest rate measurement and first payment adjustment (loan-level LIBOR changes) by first adjustment year-quarter. The sample comprises subprime ARMs (FICO $\leq$ 660 at origination). Darker colors in panel 1A correspond to more subprime ARMs within a given first adjustment year-quarter. The plot in panel 1A highlights the sizable interquartile range (and thus treatment variation) in loan-level LIBOR changes across several first adjustment year-quarters. For example, the interquartile range reached approximately 1.5 percentage points for the loans with a first adjustment date in 2008Q1 or 2008Q4. Panel 1A also shows that most subprime borrowers experienced their first adjustment in 2007 and 2008, in line with the summary statistics highlighted in Table 1.

Next, Figure 4, panel 1B, plots the modification and REO foreclosure rates for baseline subprime ARMs. Baseline subprime ARMs, those least affected by the instrument, are in the top quartile of first measurement to first adjustment LIBOR changes. In panel 1B, we define the probability of an interest rate modification (blue dots) as in Equation (1) and measure REO foreclosure rates within 36 months of first payment adjustment. Confidence bands correspond to ± 2.5 robust standard errors clustered at the three-digit ZIP code level from separate regressions estimated by first adjustment year-quarter.

For the baseline subprime ARMs, panel 1B shows an interesting correlation: loans with an earlier first adjustment year-quarter were substantially less likely to receive a modification but experienced markedly higher foreclosure rates. For example, only 2.2% of loans with a first adjustment date in 2007Q4 received modifications, but 31.7% ended up in REO foreclosure after 36 months. In contrast, servicers renegotiated 20.6% of loans with a first payment adjustment date in 2009Q3, and only 17.1% of loans in this cohort ended up in REO foreclosure 3 years after first adjustment. Hence, modifications appear to be associated with lower foreclosure rates. Yet considerable changes in economic conditions over this period likely affected both modifications and foreclosures, confounding the correlations in panel 1B. Thus, to generate causal estimates that capture the impact of LIBOR change-induced debt-renegotiations on borrower-level outcomes, we employ the IV strategy outlined in Section 4.

Panel 2A of Figure 4 plots our first-stage estimates. We separately estimate Equation (1) by each first adjustment year-quarter to gauge the impact of LIBOR changes on modifications over the housing crisis and allow the coefficients on controls to vary over time. The blue dots in panel 2A represent the year-quarter coefficient estimates of π in Equation (1), where confidence bands correspond to ± 2.5 robust standard errors clustered at the three-digit ZIP code level. Note that we retain first payment adjustment month fixed effects. Thus, for each year-quarter, we interpret these estimates as the average impact of a one-percentage-point increase in LIBOR on the probability of modification within each first payment adjustment month.

Overall, the blue dots in panel 2A show that LIBOR changes had nearly no impact on modifications in 2007Q4 and 2008Q1 when renegotiations in the overall data set were rare (panel 1B). Then, for each payment adjustment year-quarter beginning in 2008Q2, a decline in 6-month LIBOR causes a statistically significant increase in the probability of an interest rate modification. The coefficient estimates increase for later first payment adjustment cohorts as the modification rate rose in the broader sample (panel 1B). Indeed, for loans with a first payment adjustment date in 2008Q2, a one-percentage-point decline in LIBOR increases the probability of modification by 5.027 percentage points (robust S.E. = 0.518%; t-statistic = 9.71; first-stage F-statistic = 94.22) compared to 10.230 percentage points for 2009Q3 (S.E. = 1.858%; t-statistic = 5.51; first-stage F-statistic = 30.36).

LIBOR declines thus incite modifications, with strong predictive power in the statistical relationship: For the entire sample, for loans with a first adjustment date ranging from 2007M10 to 2009M09, a 100-basis-point decline in LIBOR increases the probability of modification by 5.569 percentage points (see Table 3; robust S.E. = 0.300%; t-statistic = 18.59; first-stage F-statistic = 345.57). As the overall modification rate for subprime ARMs is 8.6%, this estimate of 5.57 percentage points is noteworthy.

To further gauge the economic significance of our first-stage estimates, the red dots in panel 2A plot the increase in the modification rate induced by loan-level LIBOR changes sorted by first payment adjustment year-quarter. Specifically, we multiply the first-stage coefficient estimates (blue dots; panel 2A) by the LIBOR change moving from the fourth to the first quartile (panel 1A). We then divide this product by the modification rate for the baseline ARMs in the top LIBOR change quartile (panel 1B). The result is the increase in the modification rate for each year-quarter induced by the interquartile difference in loan-level LIBOR changes.

The red dots in panel 2A show that the LIBOR changes led to an economically meaningful jump in the modification rate starting in 2008Q2. The modification rate increased 40% in 2008Q2 and over 60% in 2008Q4 (just after the Lehman Crisis when the interquartile range for loan-level LIBOR changes widened) before falling to just over 20% in 2009Q1–Q2 and then under 10% by 2009Q3. The declining trend in the red dots in panel 2A (outside of the outlier in 2008Q4) perhaps corresponds to diminishing marginal increases in the modification rate with LIBOR changes as the baseline modification rate rose (panel 1B). Yet the sizable increase in the modification rate during 2008Q4 indicates that benchmark interest rate declines can markedly affect debt-renegotiations in the wake of severe economic and financial market distress.

Next, the top graph in panel 2B lets the dependent variable in second stage regression in Equation (2) be the log difference in the monthly mortgage interest rate payment between its actual value in the first remittance period following first payment adjustment and its ex ante expected value. We calculate the predicted values using loan contract terms and the value of 6-

month LIBOR at the first interest rate measurement. The excluded instrument is the loan-level LIBOR change between first measurement and first adjustment. Confidence bands are based on ±2.5 robust standard errors clustered at the three-digit ZIP code level. We only estimate the second-stage regression for first payment adjustment year-quarters where the first-stage t-statistic is greater than 2.5.

In panel 2B, the top plot documents that LIBOR change-induced modifications caused a large and statistically significant drop in monthly mortgage interest rate payments, ranging from about 0.25 to 0.55 log points. These decreases in monthly mortgage payments have important implications for borrowers' ability to service their debt obligations. The bottom plot in panel 2B shows point estimates from 2SLS regressions where the change in interest rate payments in dollars represents the outcome variable. These results document that LIBOR change-induced modifications reduced monthly mortgage payments by about $375 per month (2009Q1) to nearly $1,200 per month (2009Q2).

Columns 1 and 2 of Table 3 summarize the 2SLS estimates from Figure 4, panel 2B, for our whole sample of subprime ARMs with a first payment adjustment between 2007M10 and 2009M09. The table also shows the corresponding first-stage, reduced-form, and OLS estimation output. The 2SLS estimates (panel A) document that LIBOR change-induced modifications lower interest rate payments at first adjustment, relative to those predicted at first measurement, by a statistically significant 0.294 log points (column 1) or $477 per borrower per month (column 2). These estimates are economically meaningful. Indeed, the 2SLS estimate from column 2 implies that LIBOR change-induced modifications reduce mortgage payments by over $5,700 on average per year.

The reduced-form estimates in panel B show the total impact of LIBOR declines on interest rate payments for all loans in our sample (regardless of whether they received a modification) at first adjustment. These estimates indicate that a 100-basis-point decline in 6-month LIBOR lowers monthly payments for all borrowers by 0.016 log points or $27 per month, corresponding to a total annual decrease in interest rate payments for all borrowers in the sample of over $100 million at first adjustment. Over the life of a loan, the ex post net present value (NPV) of the reduced payments for typical interest rate modification discounted back to first adjustment was \$51.9k relative to origination loan contract terms. Thus, a one-percentage-point LIBOR decline led to a \$2.9k (\$51.9k * 0.0557) gain for the average borrower in the data set via the interest rate modification channel.

Figure 5, panel A, plots the reduced-form relationship via a binscatter for a baseline model (blue) that only controls for first payment adjustment date fixed effects and an expanded model (purple) that also incorporates the origination credit risk metrics listed in the figure notes. The plot highlights the tight correlation between the loan-level LIBOR change IV and interest rate payments. This correlation is linear, not meaningfully affected by outliers, and similar across different sets of controls. Overall, the binscatter, combined with the estimates in Table 3 that account for a full array of mortgage and credit predictors, underscores the robustness of the reduced-form relationship that relates the loan-level LIBOR change IV to borrower interest rate payments.

Returning to Table 3, columns 1 and 2, in panel C indicate that the OLS estimates of the impact of interest rate modifications are biased downward. The OLS coefficient in column 2 is over \$180 lower than the 2SLS estimate, suggesting that servicers select loans for renegotiation that result in smaller interest rate payment reductions even after accounting for the loan balance 4 months before first payment adjustment, among other controls.

In Table 3, column 3, the outcome variable is the difference in the back-end debt-to-income (DTI) ratio from 4 months before first adjustment to 1 month after first adjustment. Back-end DTI is estimated from the consumer credit panel by Equifax. As Equifax reports back-end DTI as a unitless score, we normalize the dependent variable by the standard deviation of DTI 4 months before first adjustment. Thus, the reported coefficient in column 3 represents the change in standard deviations in the back-end DTI ratio from 4 months before first adjustment. Panel A implies that LIBOR change-induced modifications reduce back-end DTI by an economically meaningful and statistically significant 0.19 standard deviations. Figure 5, panel B, further highlights the impact of the loan-level LIBOR IV on DTI via a binscatter of the reduced form. The plot shows that LIBOR declines lower DTI ratios for models that include both a minimal set of controls (blue) and origination credit risk metrics (purple). Altogether, these findings emphasize the robust diffusion of LIBOR declines to distressed borrowers by lowering debt-service burdens via the modification channel.

Column 4 of Table 3 assesses the IV exclusion restriction via a falsification test. The exclusion restriction requires that modifications be the only channel through which loan-level LIBOR changes affect borrower outcomes, meaning that the IV should not affect other, non-mortgage borrower obligations. So, for this falsification test, we let the dependent variable be the log difference in all debt-service payments, except those from first mortgages, from 4 months before first payment adjustment to 1 month after. The reduced-form estimates in column 4 of panel B show that LIBOR differences between first measurement and first adjustment are uncorrelated with changes in debt-service payments exclusive of first mortgages. Figure 5, panel C, arrives at a similar result via a binscatter with only minimal (blue) or origination credit risk (purple) controls. The loan-level LIBOR change IV therefore does not affect other borrower debt obligations. As such, the 2SLS estimates are insignificant (panel A, column 4), consistent with the IV exclusion restriction. More broadly, the first payment date fixed effects (e.g., α_t in Equation (2)) ensure that national LIBOR changes and broader economic conditions do not affect borrower prospects outside of the modification channel.

Next, Table 3, column 5, assesses the durability of LIBOR change-induced interest rate modifications. In particular, we let the dependent variable be the difference in the ARM interest rate between the first payment adjustment and 6 months after the first payment adjustment (corresponding to the second payment adjustment).[8] We aim to determine if the interest rates between modified and unmodified loans converge following first adjustment, perhaps as servicers modify loans previously not modified between first measurement and first adjustment. The 2SLS estimate in panel A, column 5, instead indicates that modifications between first measurement and first adjustment are associated with a further 0.67-percentage-point drop in the mortgage interest rate 6 months after first adjustment. Thus, the borrower benefits of

[8] We use the last available interest rate between the first and second payment adjustments for loans that become inactive (e.g., because of a foreclosure, liquidation with a loss, or prepayment). Thus, there may be survivorship bias in this regression. We also only look at interest 6 months after first adjustment as the share of inactive loans rises over time.

LIBOR change-induced debt-renegotiations between first measurement and first adjustment, relative to the overall sample, appear to persist immediately following modification.

Finally, the dependent variable in Table 3, column 6, is an indicator that equals one if the borrower loses their home while in distress: if the loan entered REO foreclosure or liquidated with a loss relative to the market sale value of the underlying collateral (henceforth, foreclosures). Note that controls include MBA delinquency status fixed effects 4 months before first adjustment. The 2SLS estimate can thus be interpreted as the weighted average of the impact of LIBOR change-induced modifications within each MBA delinquency status bin, holding other controls constant.

The 2SLS results in column 6 indicate that renegotiations immediately affect foreclosures: A LIBOR change-induced modification reduces the probability of foreclosure by 19 percentage points 6 months after first adjustment. Hence, LIBOR change-induced debt-renegotiations provide distressed borrowers an immediate reprieve from foreclosure. Below in Section 6.3, we will further assess the dynamic impacts of modifications on foreclosures and other borrower-level outcomes.

6.1 Identification Strategy Robustness

To further assess the robustness of our IV identification strategy, Figure 6 plots falsification tests from separate regressions by outcome variable and first payment adjustment date. The left-hand-side variable in each regression is a credit performance indicator (panel A) or a mortgage performance variable (panel B) measured 1 year before first payment adjustment. The key right-hand-side variable is the LIBOR change between first measurement and first payment adjustment, analogous to our first-stage regressions in Equation (1). Controls only include origination loan characteristics, and we standardize all dependent variables to have zero mean and unit variance. The plotted bands correspond to ± 2.5 robust standard errors clustered at the three-digit ZIP code level. Generally, the results imply that first measurement to first adjustment LIBOR changes are uncorrelated with these credit and mortgage performance variables, congruent with the assumption that the LIBOR instrument is independent of ex ante borrower characteristics.

Further, Figure 7 shows confidence bands from a falsification test where we run separate regressions by year-month of the pretreatment probability of an interest rate modification on the LIBOR change between the actual first measurement and first payment adjustment dates. The sample for each regression includes all loans before their actual first payment adjustment date, and controls include the origination risk proxies listed in the figure notes. The confidence bands widen for later estimates as we exclude loans once they reach first adjustment, reducing the sample size and boosting the standard errors. Consistent with the IV independence assumption, loan-level LIBOR changes between the actual first measurement and first adjustment dates are uncorrelated with previous instances of interest rate modifications.

Also, recall that our 2SLS estimates depend on monotonicity. This assumption asserts that a decline in 6-month LIBOR between first measurement and first payment adjustment will not decrease the probability of modification. For defiers, a reduction in LIBOR *decreases* the likelihood of an interest rate modification, a violation of monotonicity. Potential defiers may arise under normal economic circumstances, as a decline in LIBOR between first measurement and first adjustment would yield higher investor profits and may reduce modification incentives. However, amid the housing crisis, fears of widespread mortgage default abounded as foreclosures can create deadweight losses for borrowers, mortgage servicers, and loan investors. Thus, the benefits of modification likely outweighed any forgone interest payments from an elevated LIBOR ARM measurement–market rate spread.

More specifically, we consider three specific subgroups of likely defiers: Loans current 4 months before first payment adjustment (Internet Appendix Table C1); non-sand state loans (Internet Appendix Table D1); and loans associated with non-subprime (FICO score $>$ 660) borrowers current 4 months before first payment adjustment whose homes were in ZIP codes that experienced positive house price growth from 2006M01 to 2007M08 (Internet Appendix Table E1). In all these cases, the first-stage coefficient on the LIBOR change between first measurement and first adjustment instrument has the expected negative sign, congruent with the monotonicity, no-defiers assumption. Hence, LIBOR declines appear only to increase modification probabilities. Likewise, removing current loans has little impact on our main estimates

(Internet Appendix F). Altogether, these results suggest that the monotonicity, no-defiers assumption holds in our data.

6.2 LIBOR Change-Induced Modifications, Modification NPVs, and Investor Losses Within Risk Proxies

Next, we examine modification probabilities, borrower benefits from modifications, and investor losses for key origination risk proxies by quintile or factor variable categories. We compute these estimates by running separate regressions by quintile or factor category for each risk metric. The analysis allows us to statistically compare modification rates and related benefits and losses among low versus high origination FICO score, ARM margin, and CLTV borrowers and by loan delinquency status, documentation type, and owner-occupancy. In Figure G1, we also show that modification probabilities do not vary across quintiles for other risk proxies, such as the imputed LTV, the loan balance, Bartik (1991) labor demand shocks, and ZIP code 2006 household income and house price growth.

Figure 8, row 1, displays the effects of loan-level LIBOR changes on modification probabilities by quintile for origination FICO score, ARM interest rate margin, and CLTV. Per the above, each estimate represents the output from a separate regression where controls match our main specification. Thus, we study how our first-stage relationship varies across key origination characteristics, holding other risk metrics constant. The blue points signify the regression estimates by quintile, and the red points correspond to differences relative to quintile 1.

Results show that a decline in loan-level LIBOR led to larger modification probabilities for the highest-quality borrowers, as proxied by FICO credit scores (panel 1A) or ARM interest rate margins (panel 1B). The difference in estimates across ARM interest rate margin quintiles is particularly stark, with a one-percentage-point drop in LIBOR increasing the modification probability by 12.34 percentage points (robust S.E. = 0.55%) for the highest-quality borrowers in quintile 1, compared to 8.47 percentage points (robust S.E. = 0.55%) for quintile 2 and less than 5 percentage points for the lowest-quality borrowers in quintiles 3, 4, and 5 (blue points). The difference in estimates between quintile 1 and quintiles 2, 3, 4, and 5 is large in magnitude and statistically significant (red points).

Recall that the ARM margin, the difference between the mortgage interest rate and LIBOR, reflects the borrower's overall risk. Hence, panel 1B suggests that servicers make modification decisions based on the ARM margin by prioritizing renegotiations for borrowers deemed less risky at origination, all else equal. Low-risk borrowers may also be more financially literate and more likely to pursue modification.

At the same time, borrowers with higher origination CLTVs (panel 1C; quintile 5), all else equal, experienced higher LIBOR change-induced modification rates. Moving from the first to the fifth CLTV quintile increases the contribution of a 1% LIBOR decline to the probability of modification from 4.65 percentage points (robust S.E. = 0.55%) to 8.40 percentage points (robust S.E. = 0.54%), a statistically significant gain of 80%. Borrowers with elevated CLTVs face higher debt-service payments. These borrowers thus may be likely targets for interest rate modifications or seek renegotiation to ease their heavy debt payment obligations.

For modified loans, Figure 8, row 2, shows the average ex post net present value (NPV) of the reduced payments for an interest rate modification, relative to origination loan contract terms, by quintile for each metric. For comparison purposes, note that an interest rate modification reduced payments by $52k for the mean borrower over the life of the loan. In comparison, the riskiest borrowers, as measured by credit score and the ARM margin, benefited most from renegotiation, with the lowest credit score borrowers gaining an average of $66k from modification (panel 2A; quintile 1) and the highest ARM margin borrowers netting $55k (panel 2B; quintile 5).

While our aim here is to examine the aggregate effects of modifications across borrowers, benefits over the life of the loan reflect the size of payment reductions and loan duration. Borrowers choosing to refinance limit their lifetime gains from modification, whereas borrowers who enter foreclosure leave the loan pool before experiencing the longer-lasting benefits of renegotiation. Nonetheless, for borrowers in the fifth ARM margin quintile (riskiest borrowers), a LIBOR changed-induced modification reduced monthly payments at first adjustment by $670 compared to unmodified borrowers in that same risk quintile. The analogous estimate for the least risky borrowers in the first quintile is just $440. Thus, risky borrowers benefited from larger payment reductions due to LIBOR change-induced modifications, contributing to their higher lifetime NPV of renegotiation.

In panel 2C, breakdowns by CLTV exhibit the opposite risk pattern as those from panel 2B, as the lowest CLTV borrowers benefited more from renegotiation, on average, congruent with results from panel 1C on modification probabilities.

Figure 8, row 3, calculates the mean NPV benefit of LIBOR change-induced modifications for all loans, regardless of renegotiation status, calculated simply as the increase in the probability of an interest rate modification due to a one-percentage-point decline in LIBOR (panel 1) times the NPV of an interest rate modification (panel 2) by quintile for each origination metric. In essence, these estimates measure the overall impact of LIBOR declines via the interest rate modification mechanism for each group of borrowers. The results indicate that the overall mean modification benefit rises slightly with credit score (panel 3A) but falls sharply as the ARM margin increases (panel 3B). This latter finding implies that LIBOR declines primarily aid the most creditworthy borrowers, measured using the ARM margin, in panel 3B, quintile 1, with an average gain for borrowers in this group of $4.5k. As lower ARM margins strongly correlate with higher mean ZIP code household incomes and house prices, this latter finding indicates that the benefits of LIBOR change-induced modifications largely flowed to wealthier neighborhoods.

More risky ARM margin borrowers (quintiles 3, 4, and 5 in panel 3B) received substantially smaller overall modification benefits. Yet the gains for these borrowers are nontrivial: a 1% decline in LIBOR netted the mean borrower in ARM margin quintile 5 $2.2k over the life of the loan (panel 3B; quintile 5). Finally, higher CLTV borrowers experienced a more considerable modification benefit, mainly because of their larger increases in modification probabilities with LIBOR declines, as seen in panel 1C.

Last, row 4 shows output from a reduced-form regression where the outcome variable is cumulative investor losses discounted back to first adjustment. Note that losses are calculated from origination contract terms and include any forgone proceeds associated with modification. Overall, the plots show that investor losses vary little with loan risk measured at origination, indicating that loan investors are not cross-subsidizing borrowers via modifications as larger benefits for less risky borrowers likely lead to higher loan proceeds and recoveries.[9] Yet servicers more frequently modified loans perceived as less risky at origination (panel 1B), perhaps indicating that servicers expected net lower losses on these loans, that they were easier to modify, or that less risky borrowers pursued modification with greater intensity.

Figure 9 repeats the foregoing analysis for origination categorical variables, including owner occupancy (column A), documentation type (column B), and delinquency status 4 months before first adjustment (column C).

Column A displays estimation output by occupancy status, where we compare owner-occupied borrowers, non-owner-occupied borrowers, and borrowers with a mortgage secured by a second home. Non-owner-occupied borrowers are typically classified as real estate investors who purchase homes to accrue rents and speculate on house price appreciation. These borrowers are seen as more sophisticated, having more real estate market experience, and are viewed as less susceptible to biases that lead to suboptimal financial decisions. Likewise, borrowers with loans secured by second homes likely have more real estate market experience than owner-occupied borrowers. Real estate investors and borrowers with second homes may have been more aware of renegotiation opportunities, more likely to pursue modification, and more adept at navigating the modification process. More sizable increases in modification probabilities for these borrowers, in response to changes in benchmark rates relative to the baseline owner-occupied borrowers, all else equal, would indicate that investment experience and financial acumen are important determinants of modification and renegotiation outcomes.

For the baseline, owner-occupied borrowers, who make up 87% of our sample, a one-percentage-point decline in LIBOR increased the probability of modification by 5.73 percentage points (robust S.E. = 0.26%), consistent with our main estimates. In comparison, real estate investors (non-owner-occupied borrowers) and borrowers with more experience (borrowers with loans secured by second homes) benefited from substantially higher modification probabilities. Because of a one-percentage-point drop in LIBOR, the likelihood of modification rose 8.97

[9] See Fisher et al. (2022) and Zhang (2023) for studies that examine cross-subsidies in the mortgage market.

percentage points (robust S.E. = 0.86%) for real estate investors and 10.90 percentage points (robust S.E. = 2.06%) for more experienced borrowers. Thus, increases in modification rates were substantially higher for investors and experienced borrowers, where these differences relative to owner-occupied borrowers are statistically significant (panel 1A, red). The results from panel 1A of Figure 9 suggest that borrower financial acumen and experience are critical drivers of the debt-renegotiation process.

Yet conditional on an interest rate modification, owner-occupied borrowers gained the most from renegotiation with an average NPV of modification exceeding $50k, compared to just $25–30k for non-owner-occupied borrowers and those with a loan secured by a second home (panel 2A). Larger total benefits (panel 2A) offset lower contributions of LIBOR declines to modification probabilities (panel 1A) for owner-occupied borrowers. Thus, a one-percentage-point fall in LIBOR netted the average owner-occupied borrower $3.1k via the modification channel versus just $2.2k for real estate investors proxied by non-owner-occupied borrowers (panel 3A). Altogether, these results imply that the benefits of interest rate declines, a blunt policy tool, flowed mainly to owner-occupied borrowers who are often the target of government mortgage renegotiation interventions, both on an average and aggregate basis, as most loans were owner-occupied. More experienced borrowers, with loans secured by a second home, enjoyed the biggest mean benefit of $3.6k, though these borrowers make up just under 2% of our sample. Finally, panel 4A shows that the relationship between loan-level LIBOR changes and investor losses varies little with occupancy status.

Figure 9, column B, compares loans by documentation type as researchers often cite poor loan documentation, related to, for example, income and employment status, as a pivotal contributor to the 2000s mortgage bust (Jiang et al., 2014). Panel 1B finds that the contribution of LIBOR declines to modification rates varies little with documentation status. Note that the regressions in Figure 9 include the controls in our main specification outside of the plotted variable. Thus, the result in panel 1B indicates that servicers did not alter modification propensities with LIBOR changes based on documentation status, after controlling for other factors, and instead likely sorted borrowers for modification using readily available metrics, such as the

ARM margin in Figure 8, panel 1B.

However, given modification, no documentation ("No Doc") and low documentation ("Low Doc") borrowers benefited more from renegotiation (panel 2B), leading these borrowers to accrue larger overall benefits from falling interest rates. Indeed, panel 3B shows that average benefits for No Doc and Low Doc borrowers ranged from \$4–4.5k, compared to just under \$3k for full documentation ("Full Doc") borrowers. Last, panel 4B documents that losses due to loan-level LIBOR changes vary little with documentation status.

Next, column C of Figure 9 provides estimates by delinquency status 4 months before first adjustment, using the classifications from the Mortgage Bankers Association (MBA). The MBA system sorts each loan into one of the following categories: Current, 30 days delinquent, 60 days delinquent, 90 days or more delinquent (90+), in foreclosure (Forc; a foreclosure start), and where the borrower is facing bankruptcy proceedings (Bankrupt). Note that 60-day delinquency, a critical cutoff, often corresponds to default in the mortgage industry.

Interestingly, panel 1C shows that the impacts of interest rate declines on renegotiation rates vary little across current and even seriously delinquent borrowers, with a one-percentage-point drop in LIBOR increasing the probability of modification by 6.25 percentage points (robust S.E. = 0.31%) for current borrowers versus 7.29 percentage points (robust S.E. = 0.92%) for borrowers 90 days or more delinquent, all else equal. The difference in these estimates is not statistically significant (red points). Yet borrowers facing foreclosure or bankruptcy experienced smaller gains in modification rates with interest rate declines, as a one-percentage-point drop in LIBOR increased the probability of modification by 4.70 percentage points (robust S.E. = 0.64%) for borrowers in foreclosure and just 2.55 percentage points (robust S.E. = 1.29%) for those in bankruptcy. The difference in these first-stage estimates, relative to the baseline current borrowers, is statistically significant with p-values of 0.029 for foreclosure and 0.006 for bankruptcy. Overall, the results in panel 1C imply that servicers do not penalize delinquent borrowers when considering modification if the borrower is not in foreclosure or bankruptcy.

As seen in panel 2C, delinquent borrowers, not in foreclosure or bankruptcy, accrue the largest renegotiation benefits, conditional on modification. Following modification, borrowers

90 days or more delinquent gained \$61k, relative to origination loan contract terms, whereas current borrowers netted just \$48k. This difference is statistically significant (red points). Borrowers in foreclosure experience similar modification benefits to current borrowers, while an interest rate modification is only worth \$36k to the average borrower in bankruptcy.

Since delinquency corresponds to more sizable gains in LIBOR change-induced modification probabilities (panel 1C) and benefits (panel 2C), distressed borrowers, not facing foreclosure or bankruptcy, profited most from interest rate declines (panel 3C) via the loan renegotiation channel. A one-percentage-point decrease in LIBOR translated into an additional \$4.4k per delinquent borrower, versus \$3.0k for current borrowers, \$2.4k for borrowers facing foreclosure, and only \$0.9k for borrowers in bankruptcy. These results are important for policymakers, indicating that distressed borrowers are the primary beneficiaries of crisis-era LIBOR change-induced modifications despite a lack of targeting by monetary authorities. Yet we also note that borrowers experiencing foreclosure or bankruptcy benefit markedly less from broader interest rate declines via the modification channel, suggesting that other interventions may need to target these borrowers specifically.

Finally, in panel 4C, loan-level LIBOR changes have little differential impact on investor losses across borrowers sorted by delinquency status.

Figure G1 examines the first stage, relating loan-level LIBOR changes with the probability of modification across quintiles for several pre-first adjustment loan characteristics. Four months before first adjustment, such features include the imputed LTV, credit score, estimated debt-to-income (DTI), estimated income, and loan balance. We also examine the effects of loan-level LIBOR changes on modifications sorted by quintiles of household income in 2006, house price levels in 2007M08, the Bartik (1991) labor demand shock from 2006M01 to 2010M12, and ZIP code income growth from 2006M01 to 2007M08. Broadly, the first-stage coefficients vary little across the quintiles for all plotted variables, indicating that these variables have a limited impact on differential modification outcomes, holding other factors constant.

6.3 The Dynamic Impacts of LIBOR Change-Induced Modifications on Borrower Outcomes

Next, we examine the dynamic impacts of LIBOR change-induced modifications on various borrower-level outcomes using the 2SLS approach outlined in Section 4. We modify Equation (2) to examine borrower-level outcomes j periods after first adjustment:

$$\mathrm{y}_{i,t+j} = \alpha_t + \gamma_i X_i + \rho \mathrm{Mod}_{it} + \varepsilon_{it}. \tag{4}$$

where $y_{i,t+j}$ is a given outcome associated with loan i, j months after first payment adjustment. We estimate Equation (4) separately for each j. The excluded instrument is the first measurement to first adjustment LIBOR change for loan i. Thus, the 2SLS effect, ρ, can be interpreted as the causal impact of a LIBOR change-induced modification on $y_{i,t+j}$.

In Figure 10, we first examine the effects of LIBOR change-induced modifications on an indicator for real estate owned (REO) foreclosures or loans liquidated with a loss relative to the market sale value of the underlying collateral (henceforth, foreclosures; panel A) as well as an indicator for prepayments (without any loss for the investor; panel B). The horizontal axis is the number of months after first adjustment, and the confidence bands correspond to ± 2.5 robust standard errors clustered at the three-digit ZIP code level. The sample includes all subprime loans (FICO $\leq$ 660) with first adjustment between 2007M10 and 2009M09. We let foreclosure or prepayment be absorbing states: Once a loan enters foreclosure or is prepaid during month $t + j$ after first adjustment, it remains in that state for all subsequent months.

The red lines in Figure 10 correspond to a base model that only includes first payment adjustment month fixed effects (where the excluded dummy corresponds to October 2007) and controls for the following demeaned variables: The lookback period (in days), the FICO credit score at origination, CLTV at origination, the initial interest rate, and the loan balance 4 months before first payment adjustment. Thus, the intercept in the left plot in panel A tracks the probability of foreclosure for a loan that did not receive a LIBOR change-induced modification with a first payment adjustment date in October 2007 and mean values for the aforementioned credit risk predictors (e.g., baseline loans). In panel A, the path of the intercept indicates that 45% of these baseline borrowers lost their homes to foreclosure 48 months after first adjustment.

The right plot in panel A shows the 2SLS estimates. LIBOR change-induced modifications markedly lower the foreclosure probabilities after first adjustment. The effects are immediate. After just 12 months, a LIBOR change-induced modification reduces the likelihood of foreclosure by 20.8 percentage points (red line; robust S.E. = 5.91%; t-statistic = 3.51). From there, the beneficial effects of LIBOR change-induced modifications escalated: after 48 months, the probability of foreclosure for loans that receive a LIBOR change-induced modification falls 36.9 percentage points (red line; robust S.E. = 7.10%; t-statistic = 5.20). Hence, LIBOR change-induced modifications have a statistically significant and economically meaningful effect on the foreclosure rate in the baseline model.

The green line in Figure 10 uses a full set of controls. The 2SLS estimates in panel A are similar with the inclusion of these controls. Yet the standard errors fall slightly, supporting the IV independence assumption as these variables have predictive power for foreclosures but are generally uncorrelated with the instrumented modification indicator. Table 4, column 1, reports the 2SLS, first-stage, reduced-form, and OLS estimates when the dependent variable is an indicator for foreclosure after 48 months (e.g., if the loan ever entered REO foreclosure or liquidated with a loss). The 2SLS estimate in Table 4, panel A equals the last point estimate of the green line (with controls) in the right plot in Figure 10, panel A. This estimate shows that LIBOR change-induced modifications lower the probability of foreclosure by a statistically significant 41.6 percentage points (t-statistic = 6.74).

The reduced-form estimates in column 1 of Table 4, panel B, document that a 100-basis-point LIBOR decline lowers the probability of foreclosure for our entire sample of subprime ARMs by 2.3 percentage points (t-statistic = 6.79), regardless of whether the loan received a modification. Thus, LIBOR declines have a statistically significant and economically meaningful impact on foreclosures for our entire sample of subprime ARMs. Figure H1, panel 1A, plots a binscatter of this reduced-form relationship and shows that it is linear and not driven by outliers.

Next, column 1 of Table 4, panel C, suggests that the OLS estimates based on Equation (4) are biased toward zero. Indeed, the OLS estimates indicate that an interest rate modification, whether or not a LIBOR change induces it, lowers the probability of foreclosure by just 2.4 percentage points. This small point estimate may result from servicers selecting modifications with smaller monthly payments (Table 3, column 2, panel C) or the convergence of ARM interest rates in OLS estimates following first adjustment (Table 3, column 5, panel C).

Figure 10, panel B, plots the dynamic impacts of LIBOR change-induced modifications on prepayments (without any loss to the investor). The left panel shows that the probability of prepayment for baseline loans leveled off after about 20 months following first payment adjustment and only reached 18% after 48 months. The 2SLS estimates in the right panel show that after 48 months, the probability of prepayment for loans with LIBOR change-induced modifications increased nearly 6 percentage points, but this estimate is not statistically significant (see also Table 4, column 2).

Figure 11 expands our proxy for borrower distress and examines the effects of LIBOR change-induced modifications. Specifically, we let the outcome variable be an indicator variable equal to one if the borrower ever lost their home to foreclosure (REO foreclosure or liquidated with a loss) or, for active loans, if the loan is seriously delinquent j months after first adjustment. Panel A panel tracks estimated effects for loans in foreclosure or that were 90 days or more delinquent, while panel B uses a 180-day delinquency threshold. First, the left plot of panel A corresponds to baseline subprime ARM loans with mean origination and pre-first adjustment mortgage characteristics and a first payment adjustment in October 2007 that did not receive a LIBOR change-induced modification. The left plot documents that 60% of the baseline subprime ARM borrowers entered foreclosure or were 90 days or more delinquent 27 months after first adjustment. This sizable point estimate highlights the broad distress faced by these borrowers in the aftermath of the 2000s housing crisis. Yet the right plot in Figure 11 shows that LIBOR change-induced modifications have nearly no combined impact on foreclosures and serious delinquencies. Indeed, after 48 months, column 3 of Table 4, panel A, shows that the effect of LIBOR change-induced modifications on the subsequent probability of foreclosure or serious delinquency is near zero. For example, the reduced-form estimate in panel B is just

0.11 percentage points (robust S.E. = 0.35%; t-statistic = 0.31). When we use 180 days as the delinquency threshold in Figure 11, panel B, and Table 4, column 4, the 2SLS estimates remain statistically insignificant, though they are slightly larger in magnitude. Hence, LIBOR change-induced modifications do not appear to have longer-run curative outcomes for subprime borrowers, on average. Instead, higher late-stage delinquency rates offset the beneficial impacts of these modifications, nullifying the positive foreclosure mitigation effects associated with the LIBOR change-induced modifications.

Columns 3 and 4 of Table 4, panel C, report the OLS estimates when the outcome variable is an indicator equal to one if the loan ever entered into foreclosure or is 90 days or more delinquent 48 months after first adjustment (column 3) or entered foreclosure or is 180 or more days delinquent (column 4). These OLS estimates indicate that modifications correlate with lower borrower distress over the longer run, perhaps as servicers select higher-quality loans for renegotiation.

In Internet Appendix I, we re-estimate our 2SLS regressions for only real estate investors proxied by non-owner-occupied borrowers. Benchmark interest rate declines led to smaller foreclosure reductions for these borrowers. Yet, like in our full sample, such interest rate reductions did not induce longer-run curative outcomes. Results estimated separately for owner-occupied borrowers match those using our main sample, as owner-occupied loans compose 87% of our data.

6.4 Welfare Effects

Next, we discuss the broad direction of the welfare effects associated with loan renegotiations. Mortgage renegotiations directly affect borrowers, investors, and servicers while indirectly affecting surrounding neighborhoods. We examine the impact of LIBOR change-induced modifications on these parties in turn and then discuss the aggregate results to understand the broad direction of welfare effects.

6.4.1 Borrowers

Borrowers benefit directly from renegotiation through an easing of their ongoing debt obligations. LIBOR change-induced interest rate modifications lower monthly borrower payments by nearly \$480 per month immediately following renegotiation (Table 3). Moreover, a revealed preference argument suggests that by remaining in their homes rather than moving, distressed borrowers benefit from renegotiation, even if they are seriously delinquent. Indeed, among severely distressed borrowers, Diamond et al. (2020) and Collinson et al. (2022) find that eviction increases housing instability, homelessness, financial distress, hospital visits, and divorce while reducing durable consumption, earnings, employment, and creditworthiness. A crucial feature of these studies is that identification relies on random judge assignment to foreclosure cases. Hence, among borrowers on the brink of eviction, those randomly assigned to REO foreclosure experience severe adverse outcomes. Together, this evidence suggests that modification, rather than foreclosure, substantially improves borrower outcomes, even if the borrower subsequently lingers in delinquency.

6.4.2 Loan Investors

Repossessing homes via foreclosure can often lead to higher loan losses for investors and lenders (Bolton and Rosenthal, 2002; Campbell et al., 2011; Gabriel et al., 2021). However, in our case, borrowers lingering in serious delinquency may offset investor gains from foreclosure avoidance.

To assess the net impact of LIBOR change-induced modifications on investors, we examine differences in cumulative investor losses by modification status discounted back to first adjustment. Investor losses are calculated relative to the loan contract and include any modification impacts, accrued missed payments, or recoveries from forced sales or foreclosure.

More specifically, for loans inactive as of June 2023, the loss equals the cumulative investor loss at loan termination or repayment minus the cumulative loss from origination up to 4 months before first adjustment, the investor loss over the treatment period. For loans active as of June 2023, we calculate investor losses as the difference in cumulative losses up to June 2023 relative to 4 months before first adjustment. Note that loans active as of June 2023 comprise just

11% of the data set. We then discount investor losses back to first adjustment using 6-month LIBOR.

As a baseline, note that 78% of unmodified loans in our sample incur losses, the median loss is $68,581, and, given any loss, the median loss is $98,082. These summary statistics exemplify the risky nature of 2000s subprime LIBOR ARMs.

Table 5 displays the results. The 2SLS results in panel A show that LIBOR change-induced modifications do not alter investor losses (column 1), the probability that investors suffer losses (columns 2, 3, 4, and 5), or the probability that the losses are substantial (columns 6, 7, and 8). Given any loss, interest rate modifications also do not affect the size of the loss (column 9). Interestingly, the OLS results in panel C imply that renegotiation correlates with lower investor losses, both on average (column 1) and across the distribution of the losses (columns 2–8), meaning that servicers choose loans for modification that lead to lower losses.

Overall, LIBOR change-induced modifications have little impact on investor losses. This result differs from the prevailing view that investors should prefer renegotiation to foreclosure, owing to the finding in our analysis that delinquencies offset the benefits from loss mitigation and indicating that servicer selection into modification plays a role in loss avoidance.

6.4.3 Mortgage Servicers

Servicers had strong financial incentives to pursue renegotiation over foreclosure. First, foreclosure is more expensive than modification for servicers (Adelino et al., 2013). Also, servicing revenue consists of a share of principal balance, distributed each month (James, 2010), and servicers have precedence over investors in foreclosure proceedings (Diop and Zheng, 2022). Thus, servicers can boost revenue by extending the life of the loan through modification and by letting unpaid servicing fees accrue. Moreover, subprime LIBOR ARMs of the 2000s had higher servicing fees than the broader PLS population on average (0.49% versus 0.32% of principal balance, annually), providing further impetus for servicers to pursue modification (Diop and Zheng, 2022). Modification rates by loan type reflect these incentives as servicers renegotiated a higher share of LIBOR ARMs relative to loans in the broader population (Figure 1).

Concerning servicer revenue, Table 6 studies the relationship between LIBOR change-induced modifications and post-first adjustment servicer fees. Overall, results indicate that modifications lift the probability that post-first adjustment servicer fees exceed $3,000 by 15.7 percentage points (panel A, column 2), compared to a baseline probability of 51% for unmodified loans, and exceed $7,500 by 23.8 percentage points (panel A, column 3), compared to a baseline probability of 25 percent. Hence, modifications stemming from loan-level LIBOR changes substantially increase the likelihood that servicer fees surpass the median and 75th percentile relative to the baseline, unmodified population. In dollar terms, column 3 documents that a LIBOR change-induced modification raises servicer fees by nearly $6700. This estimate is sizable and economically meaningful, equivalent to a move along the interquartile range of servicer fees for unmodified loans.

6.4.4 Surrounding Neighborhoods

A long literature documents the negative impacts of foreclosure on the surrounding neighborhood. Gupta (2019) shows that a foreclosure increases the likelihood of subsequent foreclosures for surrounding homes. Following foreclosure, neighborhood prices may decline due to an increased supply of homes on the market or a "disamenity effect" whereby reduced maintenance on the foreclosed property impairs surrounding home values.[10] Indeed, foreclosure prevention policies can lead to large gains in housing wealth (Gabriel et al., 2021).

Beyond the immediate economic impacts, foreclosures increase neighborhood hospital visits (Currie and Tekin, 2015) and reduce the survey-based measures of well-being for those near, but not experiencing, a foreclosure (Makridis and Ohlrogge, 2022).

Together, this overwhelming evidence indicates that foreclosures sharply reduce neighborhood welfare. A wrinkle in the interpretation of this literature, however, is our finding that borrowers linger in delinquency after modification. Delinquent borrowers may have reduced means to maintain their homes or limited incentives, as they may anticipate foreclosure at a later date. Yet delinquent borrowers have better maintenance records than lenders and servicers who take over foreclosed properties via REO foreclosure (Lambie-Hanson, 2015); further, negative price spillovers peak once properties become bank-owned (Harding et al., 2009; Ger-

[10] See Lambie-Hanson (2015) and Makridis and Ohlrogge (2022) for an overview of this literature.

ardi et al., 2015). Thus, during a crisis, neighborhoods are likely more adversely affected by foreclosures, even if the borrowers remain in delinquency.

6.4.5 Overall Welfare Effects

LIBOR change-induced modifications, rather than foreclosures, benefit borrowers, servicers, and surrounding neighborhoods. Yet any mitigation of losses for investors associated with renegotiation and foreclosure avoidance may be offset by borrowers who remain in a persistent non-performing state. That being said, since LIBOR change-induced modifications did not increase investor losses and likely led to benefits for servicers, households, and surrounding neighborhoods, these renegotiations, in aggregate, were welfare enhancing. Thus, policymakers may aim to encourage modifications for risky borrowers, even if subsequent loan nonperformance is possible.

Labor market and macroeconomic heterogeneity can also affect loan performance following a modification, where borrowers living in neighborhoods with dimmer economic prospects may suffer from subsequent nonperformance. We assess such heterogeneity in Figure 12, where we run our 2SLS regressions separately by Bartik (1991) labor demand shock quintiles.

We segment our 2SLS regressions by Bartik labor demand shocks, rather than county-level employment growth, as Bartik shocks are independent of local labor market idiosyncrasies. ARM payment shocks and modifications, the focus of our study, likely bias local, county-level employment proxies since they may affect borrowers' local consumption patterns, non-mortgage borrowing, entrepreneurial tendencies, and other foreclosure or housing externalities. Conversely, Bartik labor demand shocks weigh national, industry-level labor market growth by each county's initial industry employment shares and thus suppose that the employment growth for each county follows the national trend for that county's start-of-period industry allocation. Importantly, Baritk labor market demand shocks predict local, county-level employment growth (Albouy et al., 2019).

We calculate the Bartik shock for each county from January 2006 to December 2010 using the BLS QCEW data. We then map these county-level data to the loan-level data by ZIP code. Extending the Bartik calculation period through 2012 yields similar results.

For each Bartik quintile in Figure 12, we run our main 2SLS regression using Equation (2) (blue points). We also calculate estimates relative to quintile 1 (red points), where quintile 1 corresponds to the weakest labor demand shock group.

Overall, Figure 12 documents a U-shaped pattern in the impact of LIBOR-change-induced modifications on longer-run foreclosures and serious delinquencies across Bartik quintiles. Within Bartik quintiles 1 and 2, renegotiations due to LIBOR declines had little effect on borrower performance, suggesting that poor labor market prospects thwart the potential economic benefits of modification. Conversely, results from Bartik quintiles 3 and 4 indicate that relatively brighter employment prospects translate LIBOR change-induced modifications into substantially better loan performance. In fact, the point estimate within Bartik quintile 4 in panel B shows that a LIBOR change-induced modification lowered the probability that a borrower experienced a foreclosure or was 180 days delinquent 48 months after first adjustment by 35.8 percentage points (robust S.E. = 14.1%; t-statistic = 2.55), relative to borrowers in quintile 4 that did not receive a modification. Moving from Bartik quintile 1 to quintile 4 in panel B lowers the probability of foreclosure or longer-run delinquency following renegotiation by nearly 50 percentage points (red point in quintile 4; robust S.E. = 19.3%; t-statistic = 2.55). Our analysis thus provides evidence of substantial differences in the impact of modifications on loan performance across regions varying with labor market strength.

Results from Bartik quintile 5 in Figure 12, corresponding to regions with the highest 2006 to 2010 employment growth, indicate that LIBOR change-induced modifications matter little in regions with the most robust employment prospects.

Finally, given the potential of adverse labor market shocks to offset the benefits of mortgage modifications, policymakers may find supplemental labor or housing market insurance useful during times of economic distress. Broadly, Albouy et al. (2019) document that stronger labor market institutions, such as redistributive transfer systems, unemployment insurance, and unionization, blunt the impact of adverse economic shocks. Specific to housing markets, Hsu et al. (2018) find that unemployment insurance acts as a housing market stabilizer, preventing foreclosures by allowing distressed borrowers to continue making mortgage payments.

Combined with mortgage modifications, such policies may further ameliorate a deterioration in housing and mortgage markets.

7 Conclusion

This paper uses a 2SLS research design to examine the 2000s crisis-period short- and long-run borrower-level effects of LIBOR change-induced debt-renegotiation. Our findings show that LIBOR declines substantially increased debt-renegotiation rates. Further, LIBOR change-induced modifications reduced borrower debt-service costs by nearly $480 per month. In line with lower debt-service payments limiting defaults, results show that LIBOR change-induced modifications markedly lowered subsequent foreclosure probabilities. However, on average, these benefits were offset by treated borrowers who often lingered in serious delinquency.

Our results highlight the limits of interest rate reductions in aiding distressed borrowers during a crisis. Falling interest rates may spark debt-renegotiation and lower debt service payments. Yet more sophisticated borrowers are more likely to take advantage of renegotiation opportunities. Moreover, the recurrent nonperformance of modified distressed borrowers may ultimately reduce the benefits of interest rate declines, especially in the face of adverse macroeconomic shocks. Future research should aim to test the external validity of our estimates to other markets and time periods as the performance of distressed debt is of first-order importance for policymakers implementing crisis period response.

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Table 1: Counts and summary statistics for subprime and non-subprime ARM loans by interest rate index

							First Payment Adj. Date by Monetary Policy Episode Within Each Row (%)			
Interest Rate Index	Interest Rate Type	Payment Adj. Freq. (Months)	Num. of Loans	% of Loans By Panel	Mean Orig FICO	Mean Orig LTV	Pre-Fed Funds Easing	Fed Funds Easing	QE1	Post-QE1
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Top 5 subprime ARM categories (FICO $\leq$ 660)										
LIBOR	LIBOR 6-month	6	1,781,474	74.21	599.13	81.85	29.33	45.77	19.43	5.47
LIBOR	LIBOR Unknown	6	242,982	10.12	594.98	81.54	13.68	51.10	28.85	6.37
Unknown	Unknown	6	78,248	3.26	603.30	82.43	17.16	33.49	39.67	9.68
LIBOR	LIBOR 12-month	12	49,233	2.05	635.98	76.27	5.72	6.72	11.59	75.96
Treasury	Treasury 1 year	1	48,402	2.02	635.07	76.30	64.73	27.55	0.41	7.32
Panel B: Top 5 non-subprime ARM categories (Fico > 660)										
LIBOR	LIBOR 6-month	6	916,418	33.20	710.73	79.34	18.02	30.08	16.47	35.43
LIBOR	LIBOR 12-month	12	477,127	17.29	734.82	73.48	3.24	7.90	12.82	76.04
Treasury	Treasury 1 year	1	243,126	8.81	720.66	75.88	67.52	25.13	0.22	7.13
Treasury	Treasury 1 year	12	177,301	6.42	741.08	71.57	4.59	11.42	17.26	66.73
Other	Other	1	175,917	6.37	720.46	59.94	82.49	16.18	0.17	1.16

Notes: ARM counts and summary statistics by ARM index category by subprime (FICO $\leq$ 660) and non-subprime (FICO > 660) categories. Only loans outstanding in September 2009, at the start of the Fed's QE Easing Cycle, are included. Column 1 shows the broad interest rate index category, while column 2 prints a more specific interest rate type. Column 3 shows how often the interest rate and interest rate payments adjust for the ARM loans. Column 4 counts the number of loans by each row, while column 5 shows the percent of loans for a given ARM interest rate index type within each panel. Columns 6 and 7 tabulate mean origination FICO credit scores and loan-to-value (LTV) ratios. Finally, columns 8–11, within each row, show the percent of loans whose first payment adjustment date falls within a given monetary episode (Pre-Conventional Monetary Easing Episode (pre-2007M09; column 8), Conventional Monetary Easing Episode (2007M09-2008M11; column 9), QE1 (2008M11-2010M02; column 10), post-QE1 (post-2010M02; column 11)).

Table 2: Coefficient estimates from regressions of pretreatment borrower outcomes on the change in 6m LIBOR between first measurement and first payment adjustment

LHS Var	Estimate (1)	Std. Error (2)	t-statistic (3)	p-value (4)
Origination FICO Credit Score	0.010	0.009	1.216	0.224
Origination CLTV	−0.005	0.010	−0.507	0.612
Initial Interest Rate	0.007	0.008	0.968	0.333
ARM Interest Rate Margin	0.012	0.008	1.555	0.120
FICO Credit Score 4 Mths Before First Adj.	0.012	0.008	1.524	0.127
Equifax Income Estimate 4 Mths Before First Adj.	−0.015	0.009	−1.640	0.101
Imputed LTV 4 Mths Before First Adj.	−0.007	0.012	−0.556	0.578
Loan Balance 4 Mths Before First Adj.	−0.017	0.009	−1.896	0.058
Ever Modified 4 Mths Before First Adj.	−0.018	0.012	−1.499	0.134
Days Delinquent 4 Mths Before First Adj.	0.010	0.009	1.133	0.257
Ever 60 Days Delin 4 Mths Before First Adj.	−0.020	0.009	−2.200	0.028
Ever 90 Days Delin 4 Mths Before First Adj.	−0.005	0.009	−0.564	0.573
Ever 150 Days Delin 4 Mths Before First Adj.	−0.004	0.009	−0.433	0.665
ZIP code household income in 2006	0.012	0.008	1.443	0.149
ZIP code house price growth, 2006M01-2007M08	0.007	0.010	0.719	0.472

Notes: Left-hand-side variables are standardized to have zero mean and unit variance. The reported coefficient estimates correspond to a regression of the left-hand-side variable on the change in 6-month LIBOR between first measurement and first payment adjustment, where each regression is estimated separately by row. Controls include the number of days between first measurement and first payment adjustment as well as first payment adjustment month fixed effects. Regressions include 350,946 loan-level observations. Robust standard errors are clustered at the three-digit ZIP code level.

Table 3: Regression estimates at first payment adjustment

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. − meas.	Rate pymt $, first adj. − meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. − first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2939 (0.0108)	−477.2122 (23.3813)	−0.1936 (0.0896)	−0.0426 (0.1693)	−0.6669 (0.2201)	−0.1892 (0.0390)
First-stage coef on LIBOR diff IV	−0.056	−0.056	−0.060	−0.056	−0.056	−0.056
First-stage F-stat	345.57	345.57	386.31	349.59	345.57	345.57
B. Reduced form						
LIBOR diff between First meas & adj.	0.0164 (0.0008)	26.5779 (1.6231)	0.0116 (0.0054)	0.0024 (0.0095)	0.0371 (0.0114)	0.0105 (0.0021)
C. OLS estimates						
Modification indicator	−0.2162 (0.0015)	−293.9736 (8.1759)	−0.0496 (0.0050)	0.0370 (0.0073)	0.9629 (0.0096)	−0.0447 (0.0027)
Observations	350,440	350,440	326,434	347,679	350,440	350,440
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table 4: **Regression estimates 48 months after first adjustment**

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.4157	0.0553	−0.0198	−0.1130
	(0.0617)	(0.0351)	(0.0623)	(0.0651)
First-stage coef on LIBOR diff IV	−0.056	−0.056	−0.056	−0.056
First-stage F-stat	345.57	345.57	346.52	346.52
B. Reduced form				
LIBOR diff between first meas & adj.	0.0231	−0.0031	0.0011	0.0063
	(0.0034)	(0.0020)	(0.0035)	(0.0037)
C. OLS estimates				
Modification indicator	−0.0240	−0.0063	−0.0267	−0.0307
	(0.0029)	(0.0014)	(0.0029)	(0.0030)
Observations	350,440	350,440	350,050	350,050
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

Table 5: Investor loss regression estimates

		Probability that Investor Losses Are Greater Than							Given Loss > $0
	Losses ($)	$5k	$10k	$25k	$50k	$100k	$150k	$200k	Losses ($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 2SLS estimates									
Modification Indicator	6,270.13	0.0163	0.0084	−0.0016	−0.0480	0.0275	0.0028	0.0539	8,137.38
	(10,826.48)	(0.0617)	(0.0615)	(0.0605)	(0.0601)	(0.0559)	(0.0476)	(0.0400)	(10,715.32)
First-stage coef on LIBOR diff IV	−0.058	−0.058	−0.058	−0.058	−0.058	−0.058	−0.058	−0.058	−0.059
First-stage F-stat	361.16	361.16	361.16	361.16	361.16	361.16	361.16	361.16	297.82
Panel B: reduced Form									
LIBOR Diff Between First Meas & Adj.	−365.74	−0.0010	−0.0005	0.0001	0.0028	−0.0016	−0.0002	−0.0031	−478.12
	(628.26)	(0.0036)	(0.0036)	(0.0035)	(0.0035)	(0.0032)	(0.0028)	(0.0023)	(623.91)
Panel C: OLS estimates									
Modification Indicator	−8,383.75	−0.0076	−0.0069	−0.0073	−0.0130	−0.0233	−0.0308	−0.0265	−9,572.64
	(659.12)	(0.0029)	(0.0029)	(0.0030)	(0.0030)	(0.0028)	(0.0029)	(0.0026)	(659.61)
Observations	345,104	345,104	345,104	345,104	345,104	345,104	345,104	345,104	272,002
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Controls are listed in footnote 4.

Table 6: Post-first adjustment discounted servicer fee regression estimates

	Prob that Servicer Fees		
	>$3k	>$7.5k	Servicer Fees ($)
	(1)	(2)	(3)
Panel A: 2SLS estimates			
Modification Indicator	0.1568	0.2376	6,665.8250
	(0.0608)	(0.0562)	(985.3640)
First-stage coef on LIBOR diff IV	−0.058	−0.058	−0.058
First-stage F-stat	367.04	367.04	367.04
Panel B: Reduced form			
LIBOR Diff Between First Meas & Adj.	−0.0091	−0.0138	−387.4247
	(0.0035)	(0.0033)	(56.7394)
Panel C: OLS estimates			
Modification Indicator	0.0450	0.0342	613.3043
	(0.0028)	(0.0029)	(52.4353)
Observations	349,555	349,555	349,555
Controls	✓	✓	✓

Notes: Controls are listed in footnote 4.

Figure 1: PLS and 6-month LIBOR ARM modifications

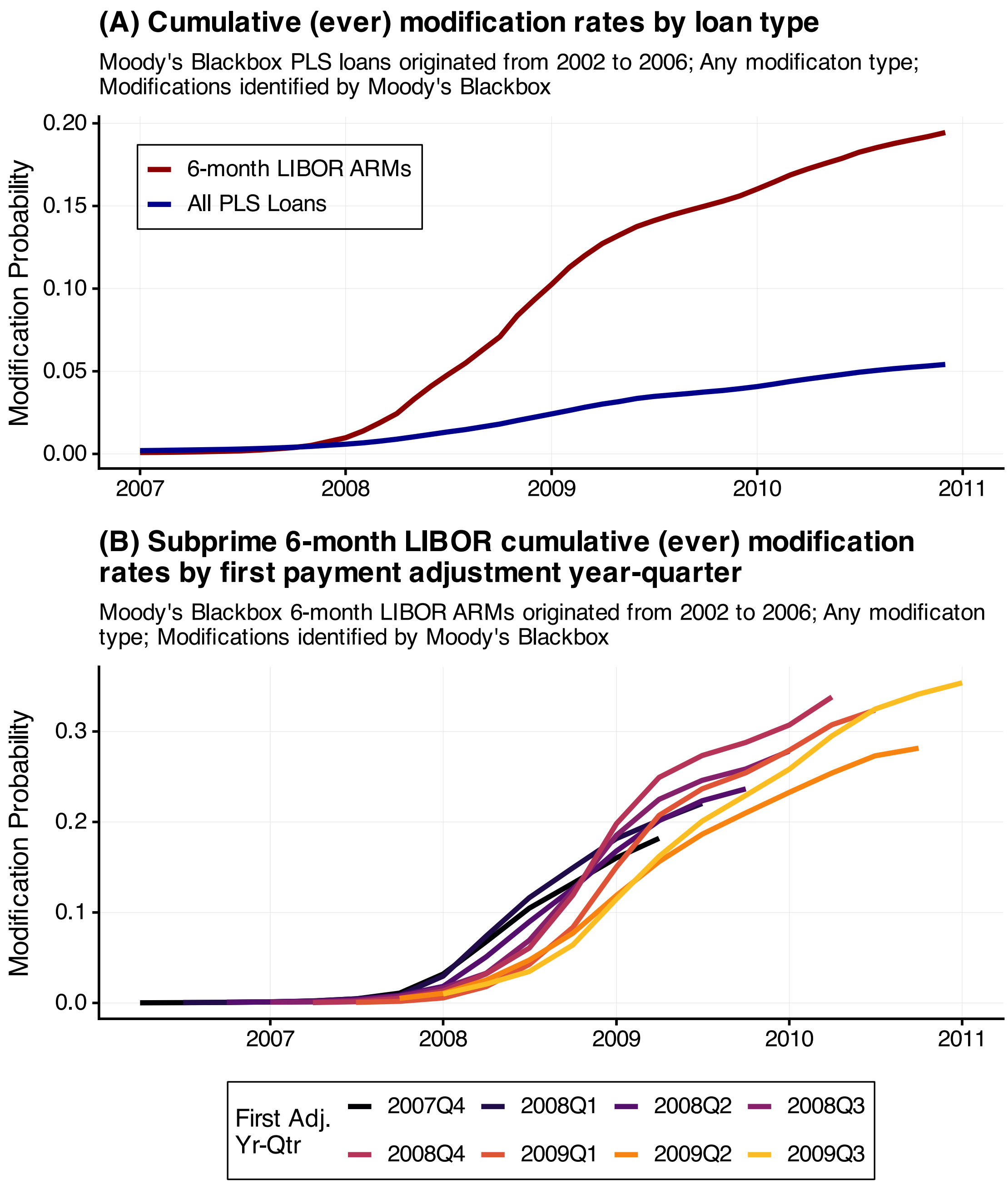


Figure 2: Monetary policy, LIBOR shocks, subprime ARM rates, and modifications

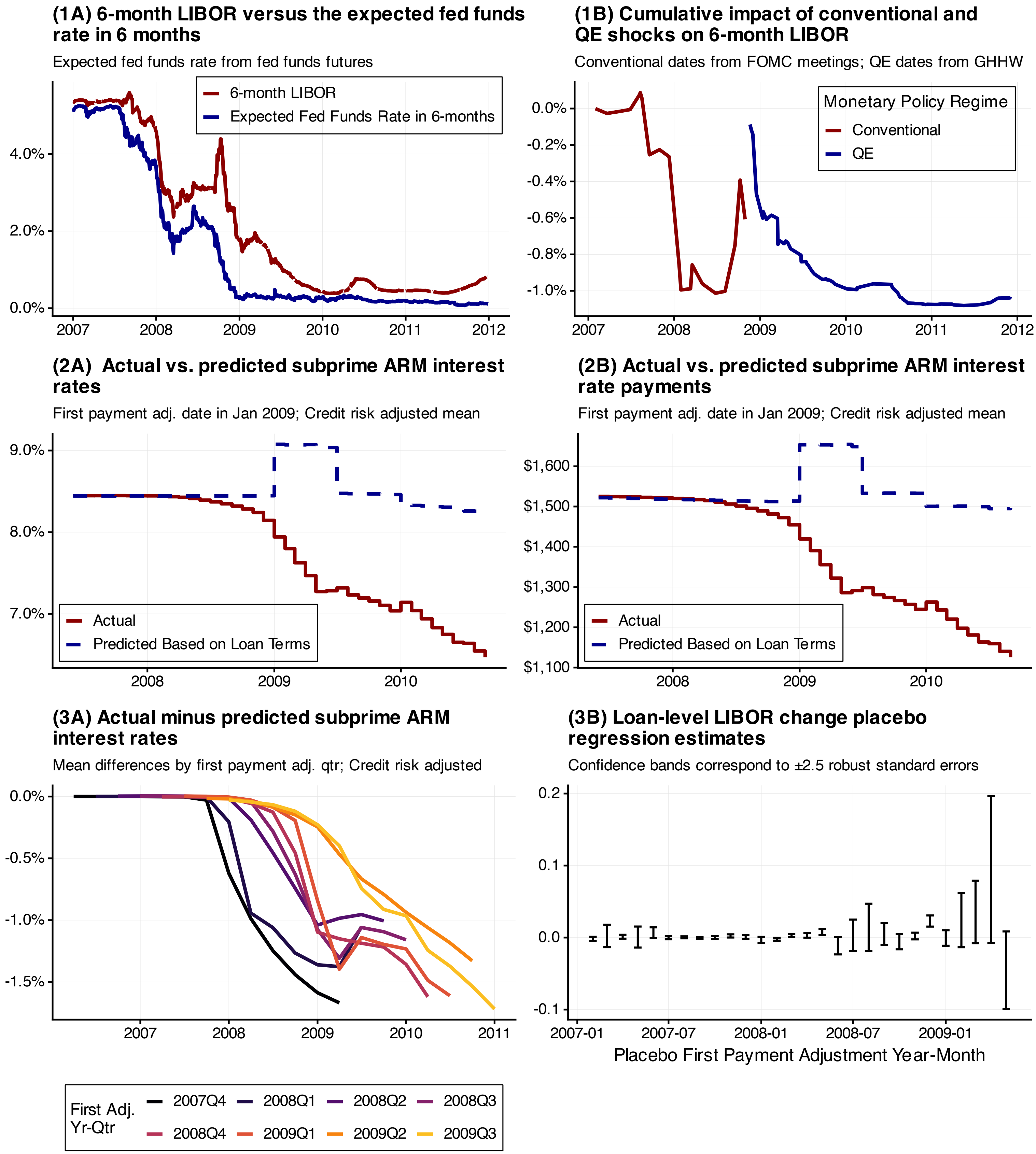
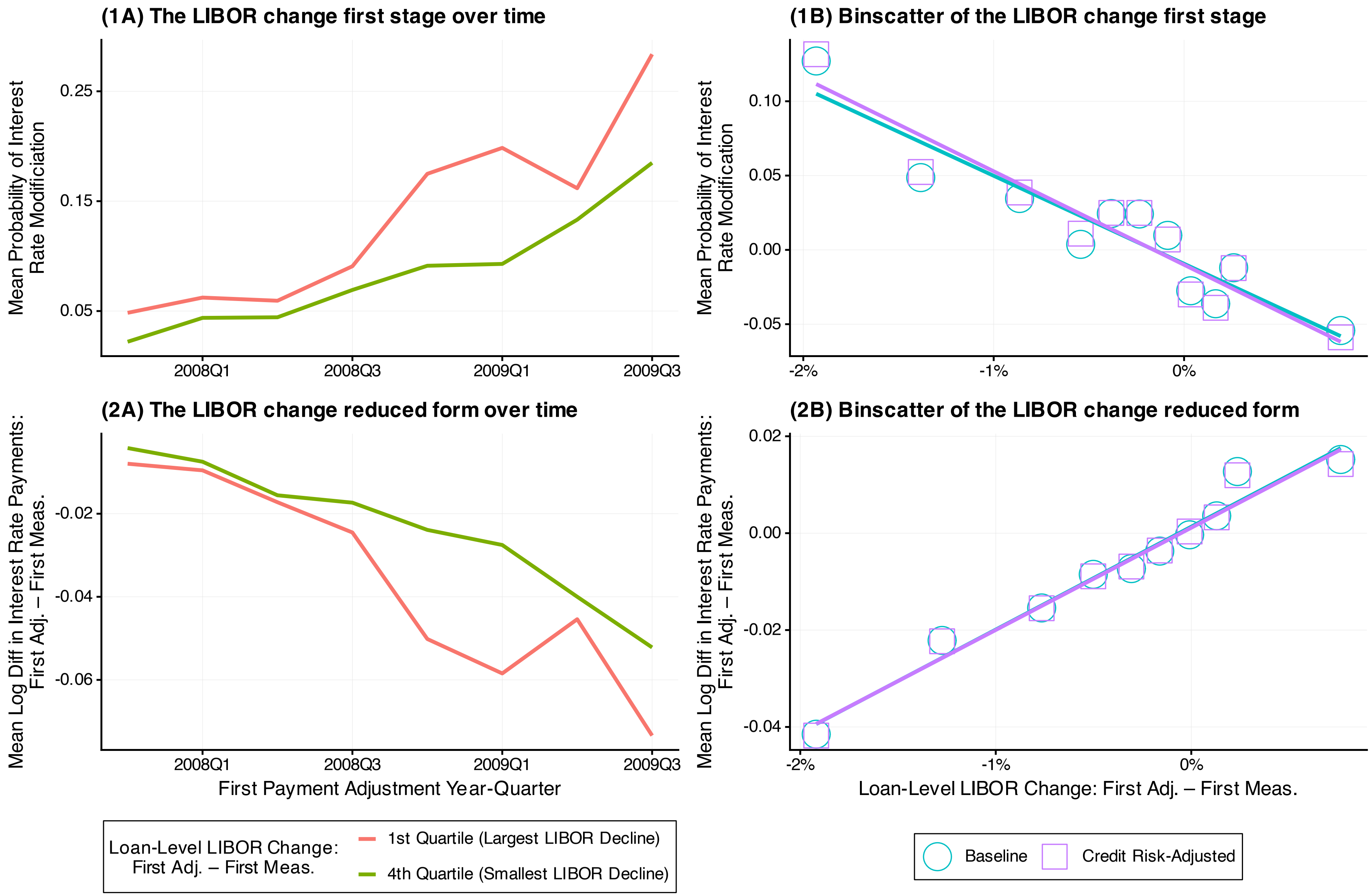
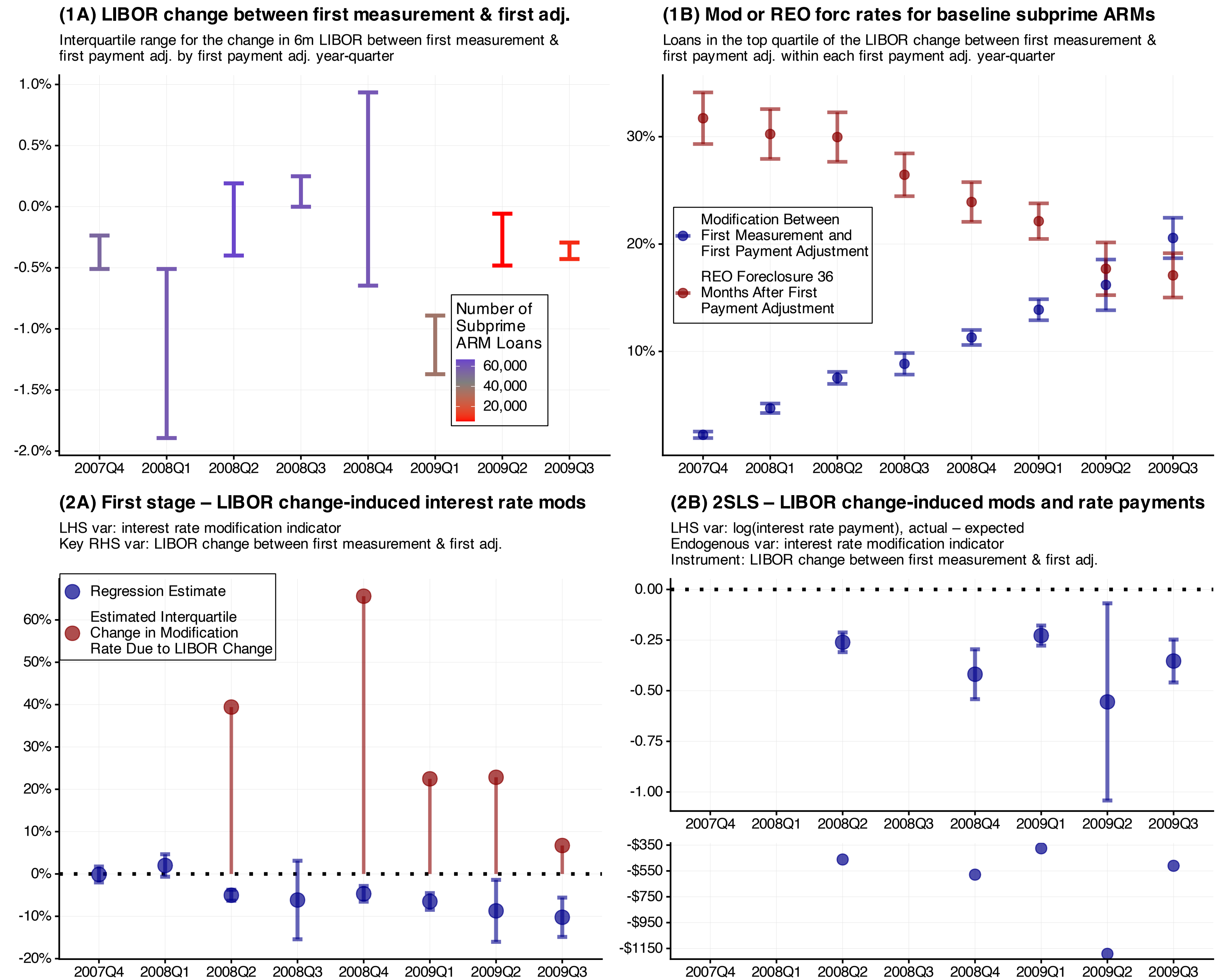


Figure 3: The LIBOR change first stage and reduced form



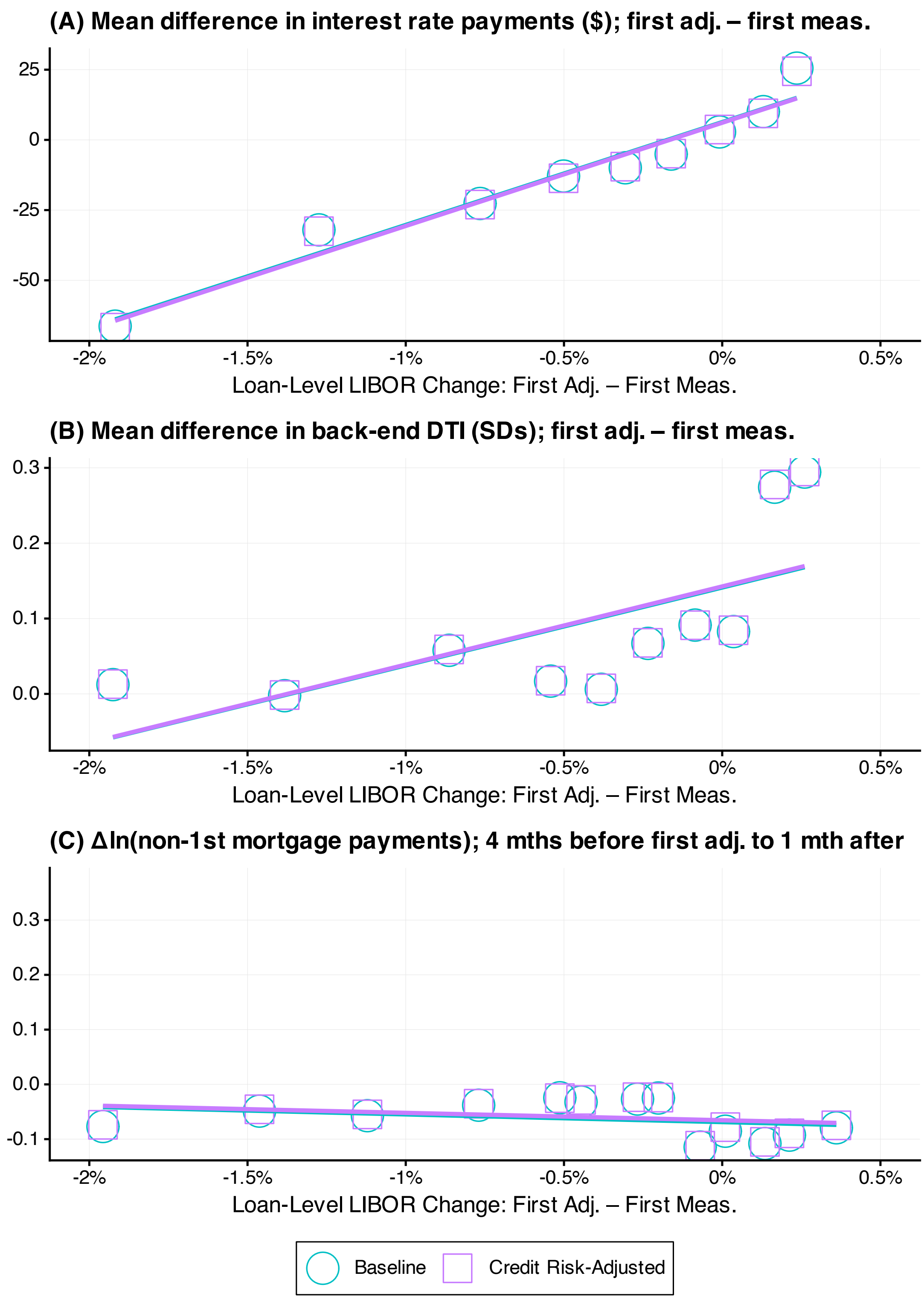
Notes: For the 1st and 4th quartiles of loan-level LIBOR changes by first payment adjustment year-quarter, panel 1A plots the mean probability of an interest rate modification (first stage), and panel 2A shows the mean log difference in interest rate payments (reduced form). The binscatters in panels 1B and 2B group loan-level LIBOR changes into bins as in Cattaneo et al. (2023). For each bin, we calculate the mean probability of an interest rate modification (first stage; panel 1B) or the mean log difference in interest rate payments between first interest rate measurement and first payment adjustment (reduced form; panel 2B). Baseline estimates in column B (blue) only control for first payment adjustment date fixed effects. The credit risk-adjusted estimates (purple) also partial out origination credit scores, combined loan-to-value ratios (CLTVs), and the ARM interest rate margin. The data consist of subprime ARMs indexed to 6-month LIBOR originated between 2002 to 2006 in line with our main sample.

Figure 4: First-stage and 2SLS estimates – LIBOR change-induced modifications



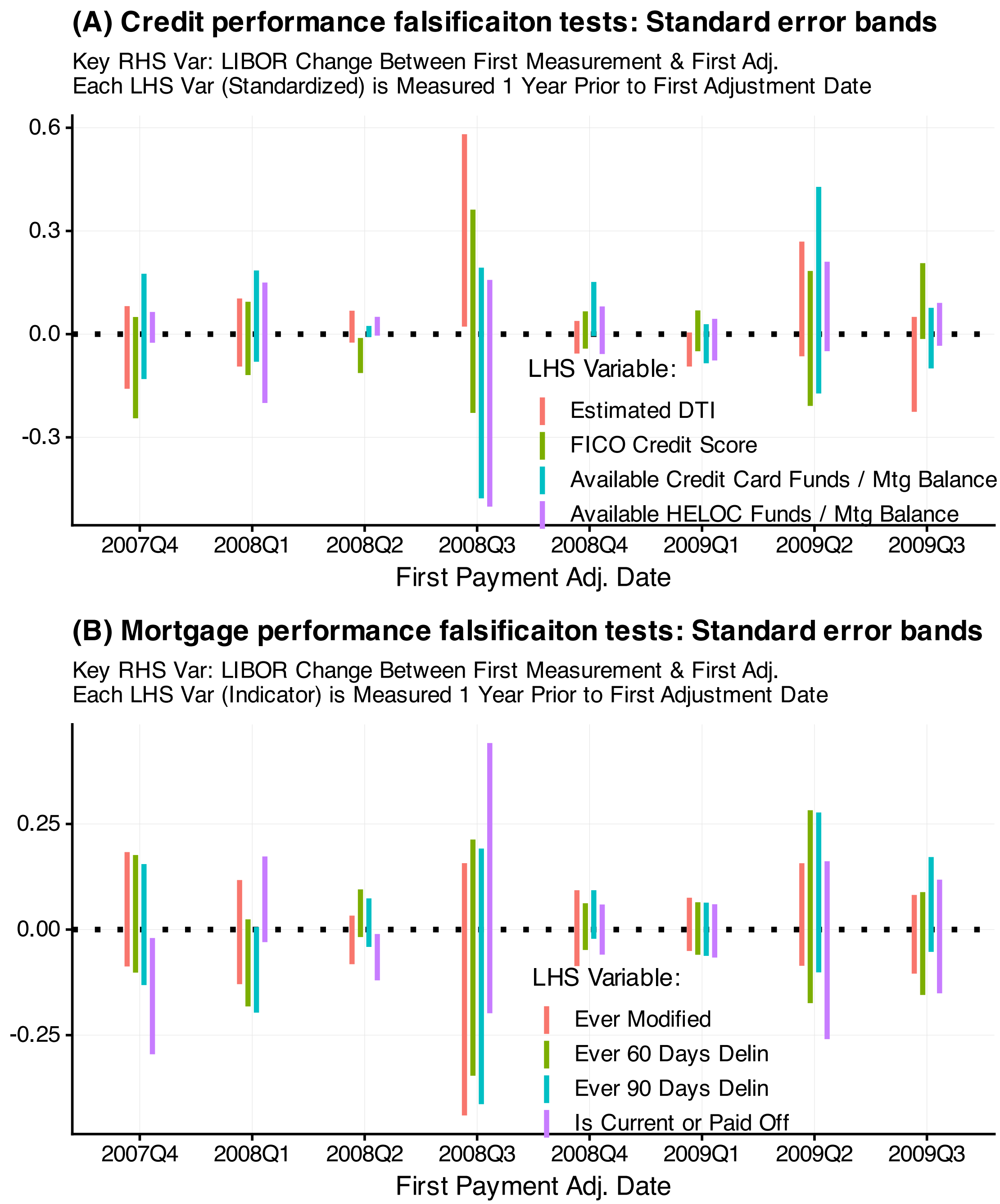
Notes: Confidence bands are based on ±2.5 robust standard errors clustered at the three-digit ZIP code level. Controls are listed in footnote 4.

Figure 5: Reduced-form binscatters at first payment adjustment



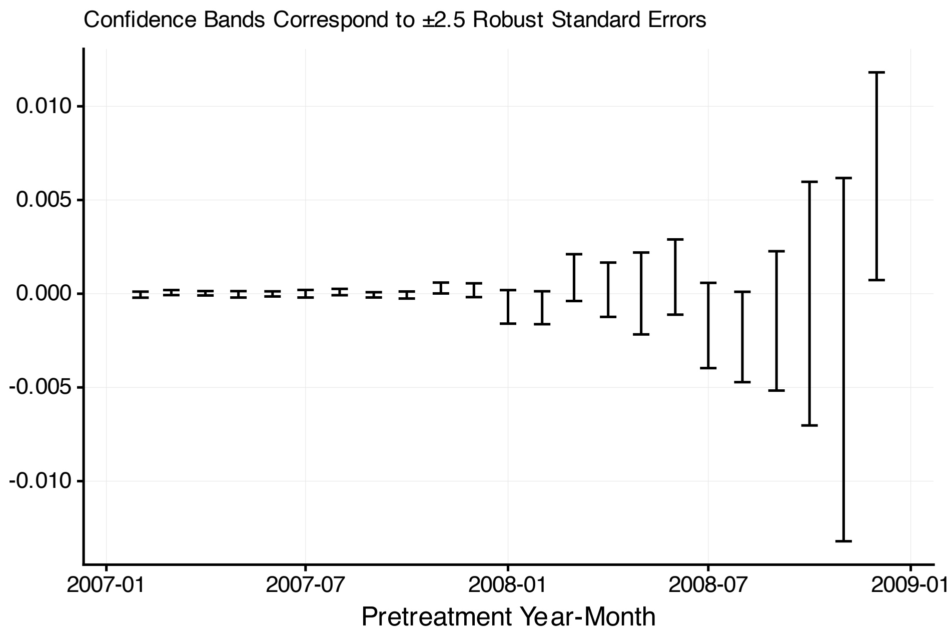
Notes: For each panel, the binscatters group loan-level LIBOR changes into bins as in Cattaneo et al. (2023). Then, within each panel, we calculate the mean of the variable plotted on the vertical axis by bin. Baseline estimates (blue) only control for first payment adjustment date fixed effects. The credit risk-adjusted estimates (purple) also partial out origination credit scores, combined loan-to-value ratios (CLTVs), and the ARM interest rate margin. The data consist of subprime ARMs indexed to 6-month LIBOR originated between 2002 to 2006 in line with our primary sample.

Figure 6: Falsification tests – the loan-Level LIBOR IV and credit or mortgage performance metrics



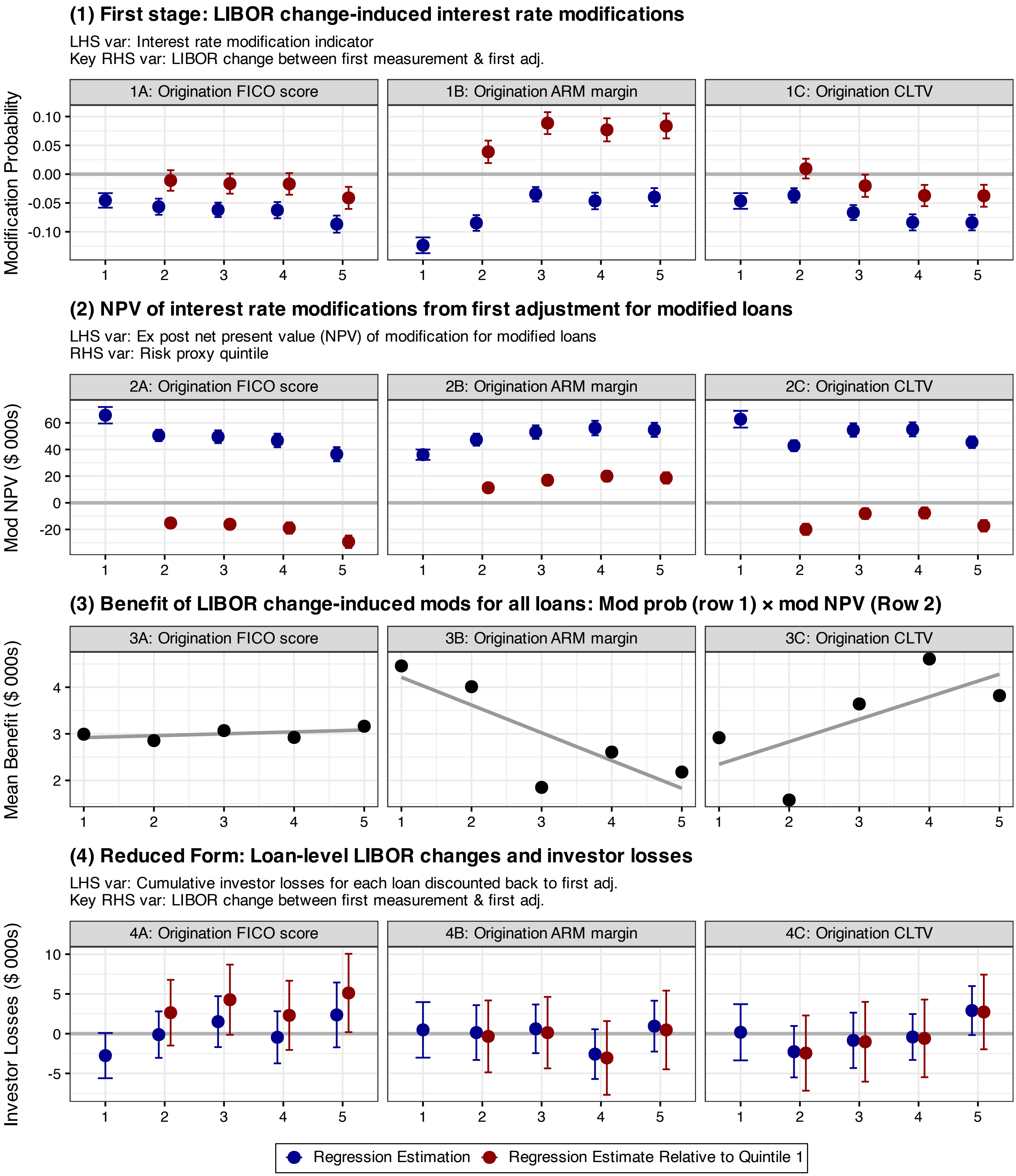
Notes: Confidence bands are based on ±2.5 robust standard errors clustered at the three-digit ZIP code level. Controls only include the origination loan characteristics listed in footnote 4.

Figure 7: Loan-level LIBOR change falsification test regression estimates



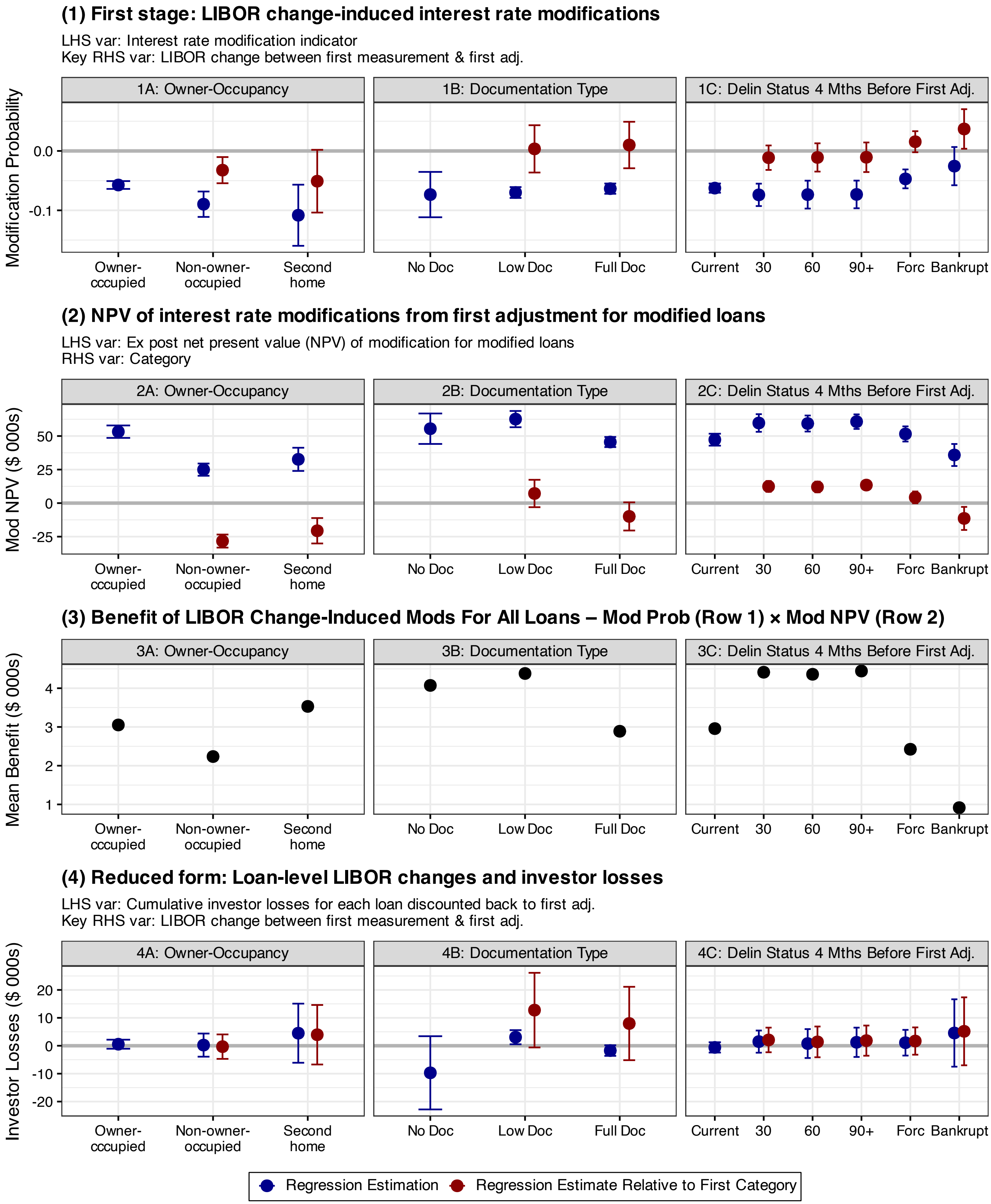
Notes: Estimates correspond to separate regressions by year-month. Controls include the number of lookback days, first payment adjustment date fixed effects, and the following variables at origination: The FICO credit score, CLTV, the ARM interest rate margin, the interest rate, and the loan balance.

Figure 8: LIBOR change-induced modification rates, the NPV of modified loans, and investor losses by origination risk proxy quintiles



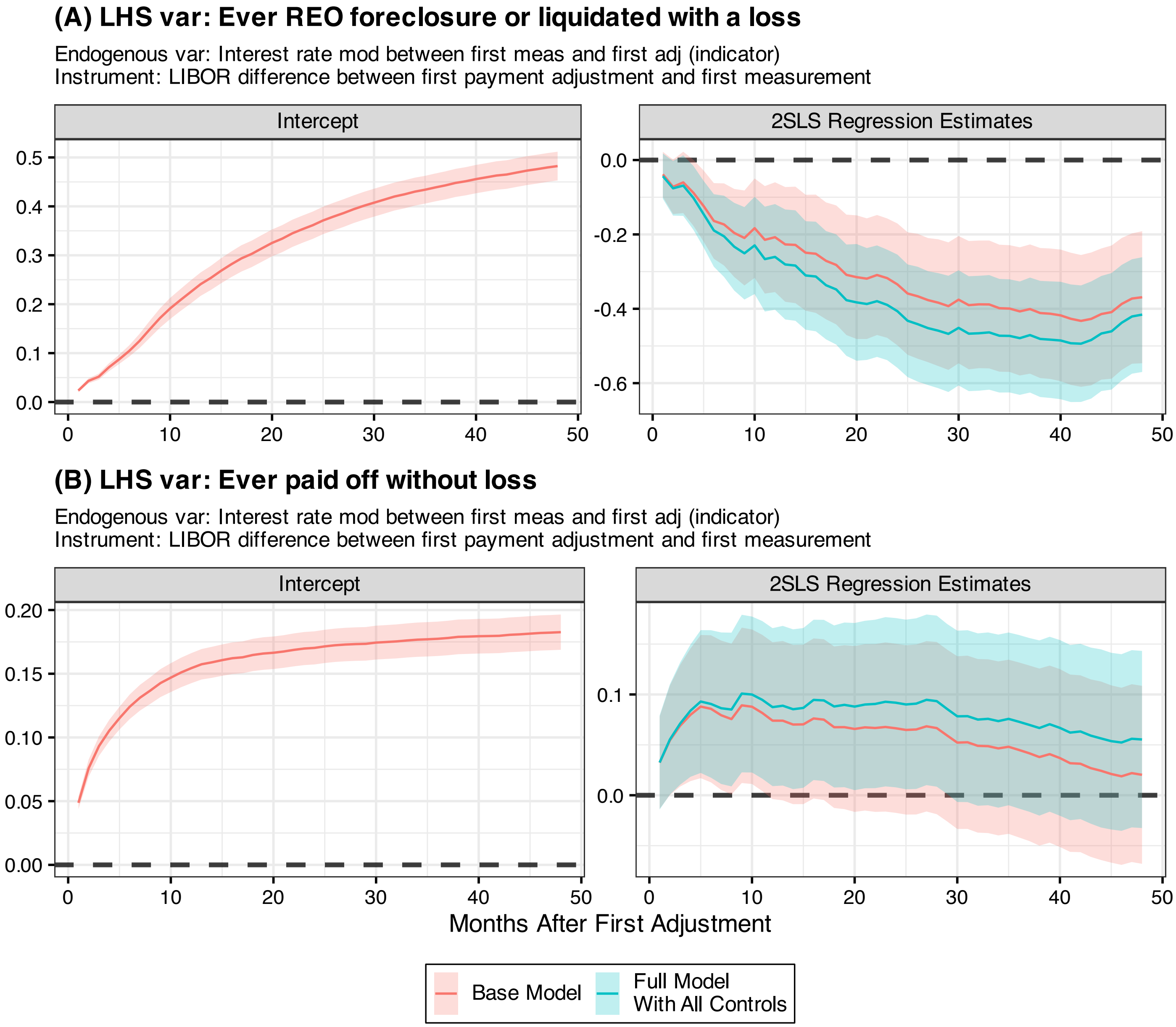
Notes: Estimates correspond to separate regressions by quintile within each panel. Controls are listed in footnote 4.

Figure 9: LIBOR change-induced modification rates and investor losses by category



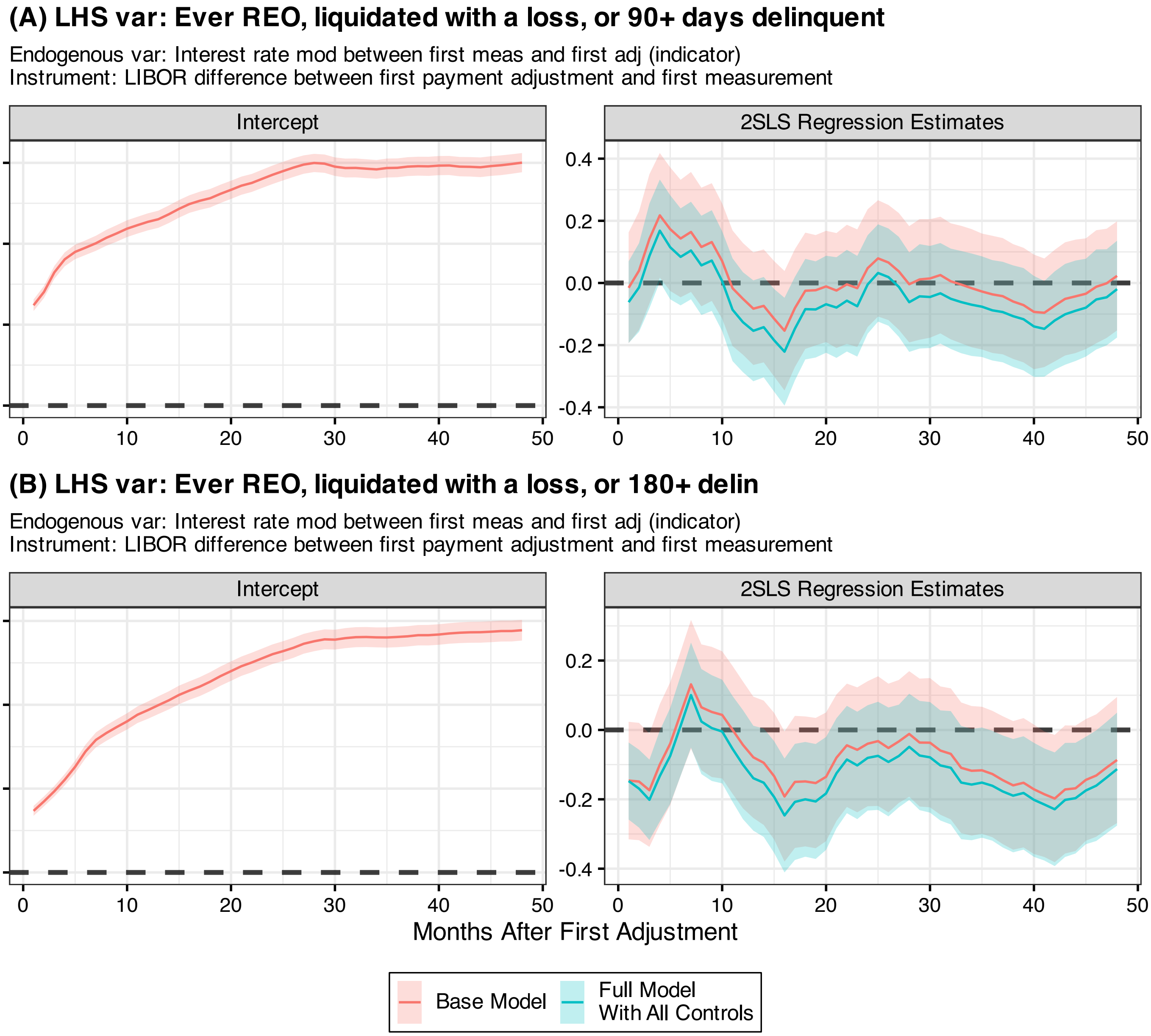
Notes: Estimates correspond to separate regressions by category within each each panel. Controls are listed in footnote 4.

Figure 10: The dynamic impact of LIBOR change-induced modifications on foreclosures and repayments



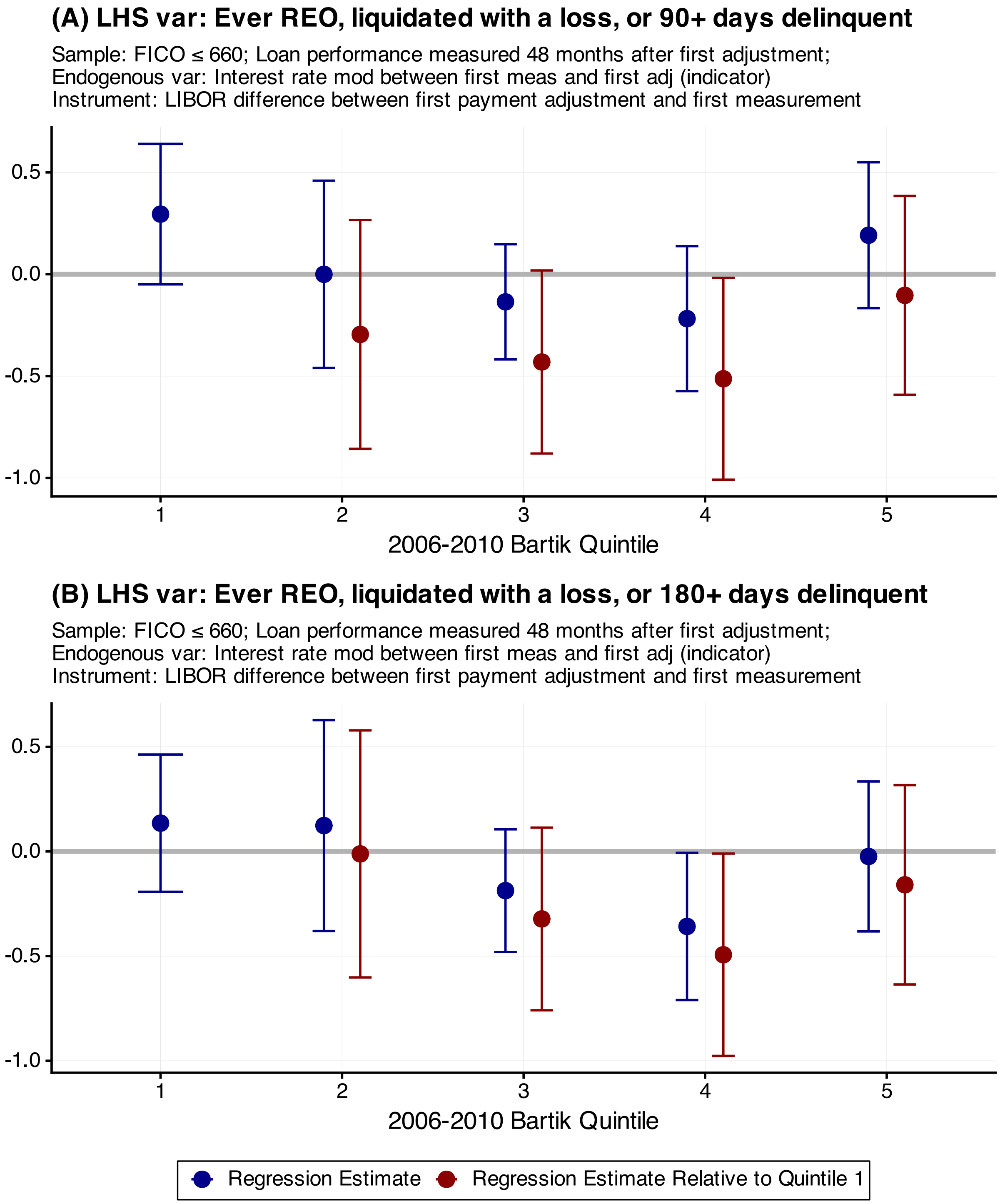
Notes: Red lines use a baseline set of controls described in Section 6.3. Green lines use a full set of controls listed in footnote 4. Confidence bands are based on ±2.5 robust standard errors clustered at the three-digit ZIP code level.

Figure 11: The dynamic impact of LIBOR change-induced modifications on foreclosures and defaults



Notes: Red lines use a baseline set of controls described in Section 6.3. Green lines use a full set of controls listed in footnote 4. Confidence bands are based on ±2.5 robust standard errors clustered at the three-digit ZIP code level.

Figure 12: 2SLS estimates by Bartik quintile – LIBOR change-induced modification performance 48 months after first adjustment



Notes: Estimates correspond to separate regressions by 2006M01 to 2010M12 Bartik quintile within each panel. Controls are listed in footnote 4, *except* for the household income for each borrower's ZIP code in 2006, the log difference in ZIP-code-level house price growth from 2006M01 to 2007M08, and the county Bartik labor demand shock from 2006M01 to 2010M12. Confidence bands correspond to ±2.5 robust standard errors clustered at the three-digit ZIP code level.

A Appendix: Using An Interest Rate Decline of 0.5 Percentage Points as the Modification Threshold

Table A1: Regression estimates at first payment adjustment – using 0.5 percentage points as the modification threshold

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. − meas.	Rate pymt \$, first adj. − meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. − first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2072	−336.3921	−0.1373	−0.0300	−0.4701	−0.1334
	(0.0079)	(17.9374)	(0.0637)	(0.1192)	(0.1510)	(0.0273)
First-stage coef on LIBOR diff IV	−0.079	−0.079	−0.085	−0.079	−0.079	−0.079
First-stage F-stat	562.16	562.16	684.98	571.10	562.16	562.16
B. Reduced form						
LIBOR diff between First meas & adj.	0.0164	26.5779	0.0116	0.0024	0.0371	0.0105
	(0.0008)	(1.6231)	(0.0054)	(0.0095)	(0.0114)	(0.0021)
C. OLS estimates						
Modification indicator	−0.1933	−265.4000	−0.0404	0.0356	0.8730	−0.0412
	(0.0013)	(7.5370)	(0.0044)	(0.0062)	(0.0092)	(0.0025)
Observations	350,440	350,440	326,434	347,679	350,440	350,440
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table A2: Regression estimates 48 months after first adjustment – using 0.5 percentage points as the modification threshold

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.2930 (0.0421)	0.0390 (0.0248)	−0.0140 (0.0439)	−0.0797 (0.0458)
First-stage coef on LIBOR diff IV	−0.079	−0.079	−0.079	−0.079
First-stage F-stat	562.16	562.16	564.51	564.51
B. Reduced form				
LIBOR diff between first meas & adj.	0.0231 (0.0034)	−0.0031 (0.0020)	0.0011 (0.0035)	0.0063 (0.0037)
C. OLS estimates				
Modification indicator	−0.0248 (0.0025)	−0.0084 (0.0013)	−0.0284 (0.0026)	−0.0327 (0.0027)
Observations	350,440	350,440	350,050	350,050
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

B Appendix: Jackknife Estimator – First Stage and 2SLS Estimates

Table B1: Regression estimates at first payment adjustment using a jackknife estimator

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. − meas.	Rate pymt $, first adj. − meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. − first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2868	−443.8301	−0.2821	−0.1077	−0.5174	−0.2165
	(0.0114)	(22.4784)	(0.0977)	(0.1726)	(0.2297)	(0.0421)
First-stage coef on LIBOR diff IV	0.055	0.055	0.059	0.055	0.055	0.055
First-stage F-stat	359.22	359.22	401.32	363.95	359.22	359.22
B. Reduced form						
LIBOR diff between First meas & adj.	−0.0156	−24.2151	−0.0168	−0.0059	−0.0282	−0.0118
	(0.0008)	(1.5486)	(0.0058)	(0.0095)	(0.0119)	(0.0023)
C. OLS estimates						
Modification indicator	−0.2162	−293.9736	−0.0496	0.0370	0.9629	−0.0447
	(0.0015)	(8.1759)	(0.0050)	(0.0073)	(0.0096)	(0.0027)
Observations	350,440	350,440	326,434	347,679	350,440	350,440
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4. The instrument is computed using a jackknife estimator to capture lookback period × reset month variation.

Table B2: Regression estimates 48 months after first adjustment using a jackknife estimator

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.5101	0.0693	−0.0192	−0.1206
	(0.0671)	(0.0388)	(0.0686)	(0.0711)
First-stage coef on LIBOR diff IV	0.055	0.055	0.055	0.055
First-stage F-stat	359.22	359.22	361.16	361.16
B. Reduced form				
LIBOR diff between first meas & adj.	−0.0278	0.0038	−0.0011	−0.0066
	(0.0035)	(0.0021)	(0.0038)	(0.0039)
C. OLS estimates				
Modification indicator	−0.0240	−0.0063	−0.0267	−0.0307
	(0.0029)	(0.0014)	(0.0029)	(0.0030)
Observations	350,440	350,440	350,050	350,050
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4. The instrument is computed using a jackknife estimator to capture lookback period × reset month variation.

C Appendix: Current Loans – First Stage and 2SLS Estimates

Table C1: Regression estimates at first payment adjustment – borrowers current 4 months before first adjustment

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. – meas.	Rate pymt $, first adj. – meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. – first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2545 (0.0119)	−428.1916 (28.9058)	−0.1443 (0.1312)	−0.1222 (0.2216)	−0.6238 (0.3095)	−0.0378 (0.0341)
First-stage coef on LIBOR diff IV	−0.050	−0.050	−0.051	−0.050	−0.050	−0.050
First-stage F-stat	171.37	171.37	172.84	170.35	171.37	171.37
B. Reduced form						
LIBOR diff between First meas & adj.	0.0127 (0.0009)	21.3971 (1.8173)	0.0074 (0.0068)	0.0061 (0.0111)	0.0312 (0.0146)	0.0019 (0.0017)
C. OLS estimates						
Modification indicator	−0.1791 (0.0014)	−245.0458 (7.2025)	−0.0702 (0.0053)	0.0210 (0.0084)	0.9140 (0.0123)	0.0011 (0.0014)
Observations	208,611	208,611	200,440	206,976	208,611	208,611
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table C2: Regression estimates 48 months after first adjustment – borrowers current 4 months before first adjustment

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.2971	0.0951	0.0654	−0.0281
	(0.0922)	(0.0676)	(0.1035)	(0.1063)
First-stage coef on LIBOR diff IV	−0.050	−0.050	−0.050	−0.050
First-stage F-stat	171.37	171.37	171.41	171.41
B. Reduced form				
LIBOR diff between first meas & adj.	0.0148	−0.0048	−0.0033	0.0014
	(0.0046)	(0.0034)	(0.0051)	(0.0053)
C. OLS estimates				
Modification indicator	0.0035	−0.0132	−0.0083	−0.0097
	(0.0039)	(0.0026)	(0.0041)	(0.0043)
Observations	208,611	208,611	208,436	208,436
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

D Appendix: Non-Sand Sates Loans – First Stage and 2SLS Estimates

Table D1: Regression estimates at first payment adjustment – non-sand states

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. − meas.	Rate pymt \$, first adj. − meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. − first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2846	−396.2339	−0.0661	−0.0616	−0.3985	−0.1469
	(0.0143)	(25.1967)	(0.1209)	(0.2349)	(0.2802)	(0.0475)
First-stage coef on LIBOR diff IV	−0.052	−0.052	−0.057	−0.052	−0.052	−0.052
First-stage F-stat	208.65	208.65	232.44	209.50	208.65	208.65
B. Reduced form						
LIBOR diff between First meas & adj.	0.0149	20.7171	0.0037	0.0032	0.0208	0.0077
	(0.0011)	(1.7637)	(0.0069)	(0.0123)	(0.0141)	(0.0025)
C. OLS estimates						
Modification indicator	−0.2088	−245.7472	−0.0547	0.0380	0.9866	−0.0332
	(0.0019)	(8.2441)	(0.0068)	(0.0103)	(0.0135)	(0.0022)
Observations	228,664	228,664	213,024	226,873	228,664	228,664
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table D2: Regression estimates 48 months after first adjustment – non-sand states

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.4093 (0.0862)	0.0117 (0.0572)	0.0169 (0.0893)	−0.0971 (0.0924)
First-stage coef on LIBOR diff IV	−0.052	−0.052	−0.052	−0.052
First-stage F-stat	208.65	208.65	209.36	209.36
B. Reduced form				
LIBOR diff between first meas & adj.	0.0214 (0.0044)	−0.0006 (0.0030)	−0.0009 (0.0047)	0.0051 (0.0048)
C. OLS estimates				
Modification indicator	−0.0244 (0.0039)	−0.0052 (0.0021)	−0.0281 (0.0040)	−0.0323 (0.0042)
Observations	228,664	228,664	228,476	228,476
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

E Appendix: Non-subprime, Current Loans in Positive Pretreatment House Price Growth ZIP Codes – First Stage and 2SLS Estimates

Table E1: Regression estimates at first payment adjustment – non-subprime, current loans, in ZIP codes with positive house price growth from 2006m01 to 2007m08

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. – meas.	Rate pymt \$, first adj. – meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. – first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2990 (0.0138)	−340.8206 (23.9260)	−0.0324 (0.1405)	−0.0443 (0.2592)	−0.3542 (0.3008)	0.0264 (0.0439)
First-stage coef on LIBOR diff IV	−0.082	−0.082	−0.086	−0.083	−0.082	−0.082
First-stage F-stat	122.39	122.39	127.90	124.35	122.39	122.39
B. Reduced form						
LIBOR diff between First meas & adj.	0.0244 (0.0019)	27.8028 (2.6137)	0.0028 (0.0121)	0.0037 (0.0216)	0.0289 (0.0235)	−0.0022 (0.0036)
C. OLS estimates						
Modification indicator	−0.2031 (0.0040)	−260.9016 (11.7412)	−0.0468 (0.0119)	0.0027 (0.0254)	0.6208 (0.0249)	−0.0011 (0.0036)
Observations	49,104	49,104	46,640	48,556	49,104	49,104
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table E2: Regression estimates 48 months after first adjustment – non-subprime, current loans, in ZIP codes with positive house price growth from 2006m01 to 2007m08

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.1847 (0.1161)	0.0216 (0.0911)	−0.2544 (0.1253)	−0.2002 (0.1192)
First-stage coef on LIBOR diff IV	−0.082	−0.082	−0.082	−0.082
First-stage F-stat	122.39	122.39	123.27	123.27
B. Reduced form				
LIBOR diff between first meas & adj.	0.0151 (0.0096)	−0.0018 (0.0074)	0.0208 (0.0105)	0.0164 (0.0100)
C. OLS estimates				
Modification indicator	−0.0177 (0.0102)	−0.0394 (0.0071)	−0.0393 (0.0104)	−0.0402 (0.0098)
Observations	49,104	49,104	49,025	49,025
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

F Appendix: Non-Current Loans – First Stage and 2SLS Estimates

Table F1: Regression estimates at first payment adjustment – borrowers not current 4 months before first adjustment

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. – meas.	Rate pymt $, first adj. – meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. – first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.3269 (0.0194)	−519.0668 (34.6399)	−0.2453 (0.1215)	−0.0257 (0.2853)	−0.8319 (0.3082)	−0.3012 (0.0739)
First-stage coef on LIBOR diff IV	−0.060	−0.060	−0.069	−0.061	−0.060	−0.060
First-stage F-stat	197.01	197.01	238.75	201.33	197.01	197.01
B. Reduced form						
LIBOR diff between First meas & adj.	0.0196 (0.0014)	31.0884 (2.3333)	0.0168 (0.0083)	0.0016 (0.0173)	0.0498 (0.0170)	0.0180 (0.0042)
C. OLS estimates						
Modification indicator	−0.2557 (0.0020)	−347.5961 (9.6456)	−0.0388 (0.0096)	0.0500 (0.0131)	1.0356 (0.0111)	−0.0843 (0.0051)
Observations	141,829	141,829	125,994	140,703	141,829	141,829
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

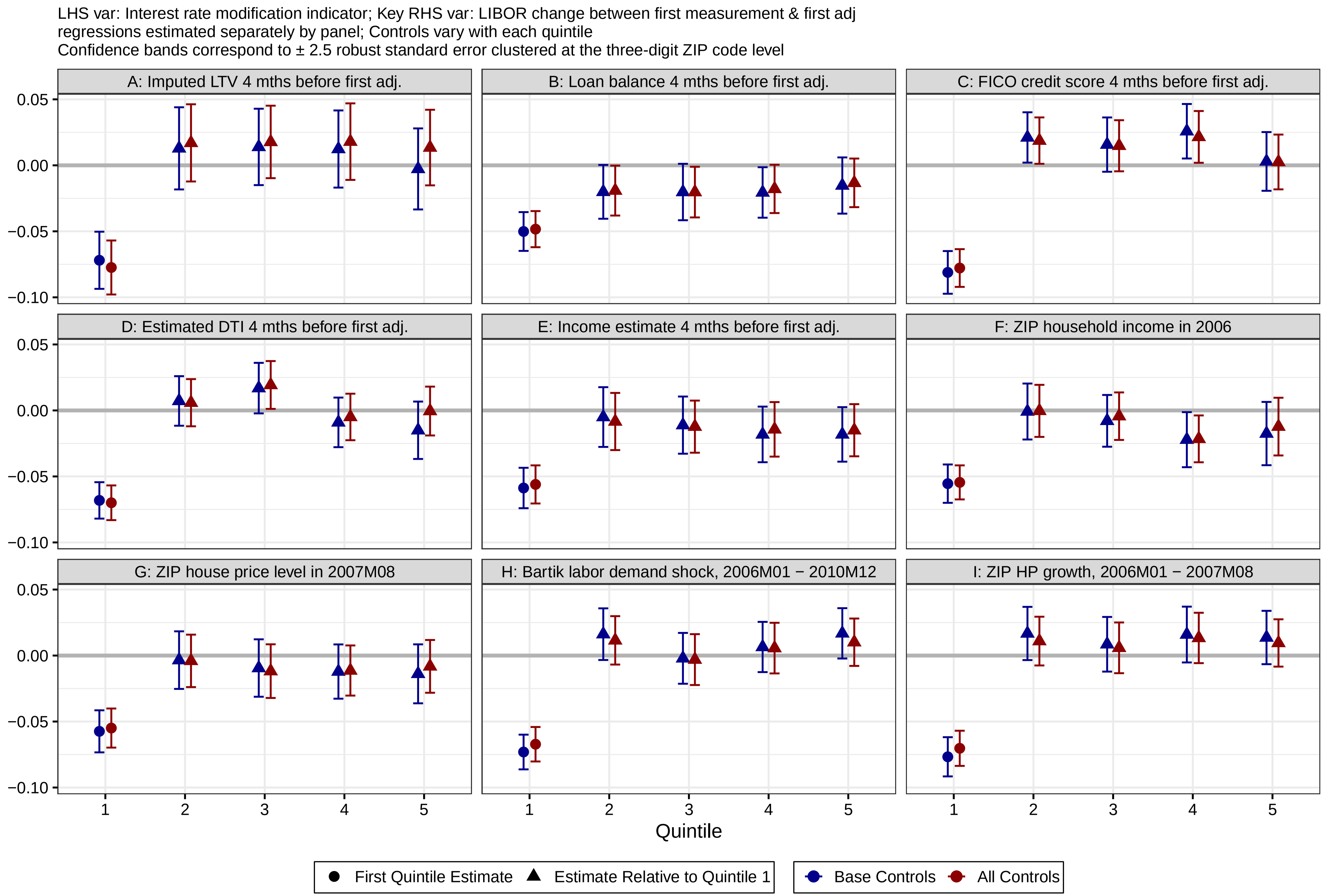
Table F2: Regression estimates 48 months after first adjustment – borrowers not current 4 months before first adjustment

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.5001	−0.0089	−0.0921	−0.1912
	(0.0945)	(0.0233)	(0.0794)	(0.0806)
First-stage coef on LIBOR diff IV	−0.060	−0.060	−0.060	−0.060
First-stage F-stat	197.01	197.01	199.15	199.15
B. Reduced form				
LIBOR diff between first meas & adj.	0.0300	0.0005	0.0055	0.0115
	(0.0052)	(0.0014)	(0.0048)	(0.0048)
C. OLS estimates				
Modification indicator	−0.0534	0.0022	−0.0525	−0.0586
	(0.0042)	(0.0011)	(0.0039)	(0.0040)
Observations	141,829	141,829	141,614	141,614
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.

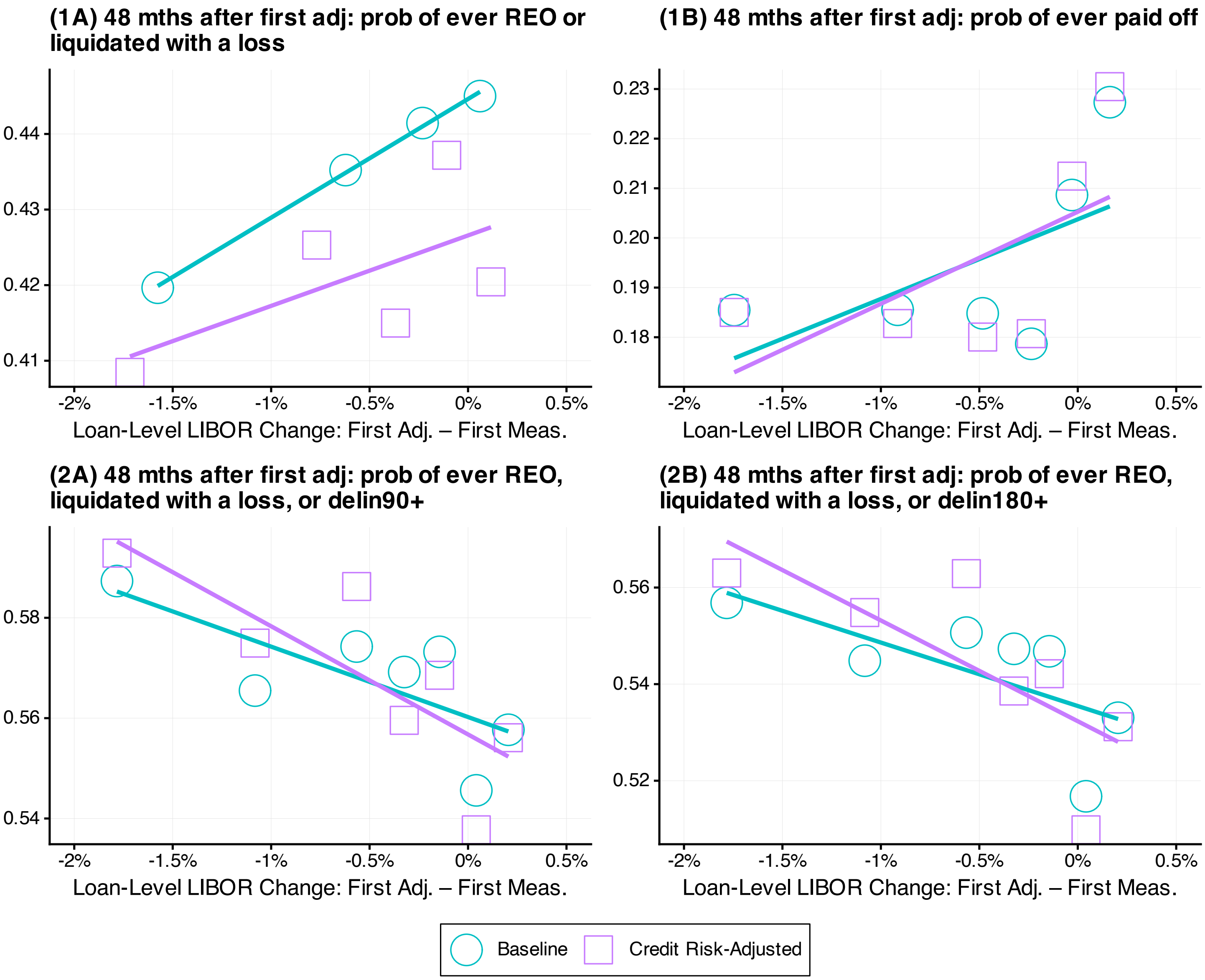
G Appendix: LIBOR Change-Induced Interest Rate Modification Probabilities by Risk Proxy Quintiles

Figure G1: LIBOR Change-Induced Interest Rate Modification Probabilities by Risk Proxy Quintiles



H Appendix: Reduced form Binscatters 48 Months After First Adjustment

Figure H1: Reduced Form Binscatters at 48 Months After First Adjustment



Notes: For each panel, the binscatters group loan-level LIBOR changes into bins as in Cattaneo et al. (2023). Then, within panel, we calculate the mean bin. Baseline estimates (blue) only control for first payment adjustment date fixed effects. The credit risk adjusted estimates (purple) also partial out origination credit scores, combined loan-to-value ratios (CLTVs), and the ARM interest rate margin. The data consist of subprime ARMs indexed to 6-month LIBOR originated between 2002 to 2006 in line with our main sample.

I Appendix: Non-Owner-Occupied Borrowers – First Stage and 2SLS Estimates

Table I1: Regression estimates at first payment adjustment for non-owner-occupied borrowers

	Dependent variable:					
	At first adj.				6-months after first adj.	
	ln(rate pymt), first adj. − meas.	Rate pymt $, first adj. − meas.	ΔDTI, back-end (SDs)	Δln(non-1st mtg pymts)	Int rate, 6m post adj. − first adj.	Ever REO or liq with loss
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimates						
Modification indicator	−0.2811 (0.0319)	−327.3711 (45.3294)	−0.3810 (0.2577)	−0.0248 (0.5865)	−1.1685 (0.6527)	0.0028 (0.1616)
First-stage coef on LIBOR diff IV	−0.067	−0.067	−0.073	−0.066	−0.067	−0.067
First-stage F-stat	39.36	39.36	45.49	38.87	39.36	39.36
B. Reduced form						
LIBOR diff between First meas & adj.	0.0187 (0.0031)	21.8023 (3.7407)	0.0279 (0.0187)	0.0016 (0.0387)	0.0778 (0.0387)	−0.0002 (0.0108)
C. OLS estimates						
Modification indicator	−0.2224 (0.0088)	−243.0920 (13.7764)	0.0124 (0.0267)	0.0312 (0.0488)	0.8263 (0.0583)	−0.0605 (0.0137)
Observations	16,451	16,451	15,406	16,224	16,451	16,451
Controls	✓	✓	✓	✓	✓	✓

Notes: Regression estimates around first payment adjustment. Controls are listed in footnote 4.

Table I2: Regression estimates 48 months after first adjustment for non-owner-occupied borrowers

	Dependent variable:			
	Ever REO or loss	Ever paid off	Ever REO or loss, or is delin90+	Ever REO or loss, or is delin180+
	(1)	(2)	(3)	(4)
A. 2SLS estimates				
Modification indicator	−0.0679	−0.3216	−0.0748	0.0141
	(0.2368)	(0.1261)	(0.2161)	(0.2261)
First-stage coef on LIBOR diff IV	−0.067	−0.067	−0.067	−0.067
First-stage F-stat	39.36	39.36	39.30	39.30
B. Reduced form				
LIBOR diff between first meas & adj.	0.0045	0.0214	0.0050	−0.0009
	(0.0157)	(0.0078)	(0.0144)	(0.0151)
C. OLS estimates				
Modification indicator	−0.0716	−0.0064	−0.0448	−0.0485
	(0.0193)	(0.0091)	(0.0200)	(0.0199)
Observations	16,451	16,451	16,417	16,417
Controls	✓	✓	✓	✓

Notes: Regression estimates 48 months after first payment adjustment. Controls are listed in footnote 4.