Debt Aversion and Household Borrowing: Evidence from Mortgage Repayment

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Abstract

We study how changes in investment opportunities affect households' decision to prepay their mortgage debt. We find that a large of share of households prepay each month even when they would earn a higher return investing at the risk-free rate, causing losses in net present value terms and contradicting basic portfolio theory. We calculate $4.6 billion in prepayment losses from January 2022-November 2024, due to a high share of mortgages with a rate below the risk-free rate. Prepayment is persistent, widespread, and cannot be explained by inattention or inertia. Instead, evidence points to debt aversion: Households prepay more close to mortgage completion and if they have a high credit score and no credit card debt. Exogenous cash flow shocks also increase prepayment.

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1 Introduction

Consider a household that has extra money each month after paying their living expenses, including a mortgage. They could put the money into a 401(k) or other retirement plan. Or, they could put a bit of extra cash towards their mortgage, helping them pay it off faster and reducing the total interest they will pay. To decide what to do, they might turn to popular personal finance writer Dave Ramsey. Ramsey's best-selling *Total Money Makeover* is structured around seven "financial baby steps", one of which is to *pay off your home early*. Throughout his book, Ramsey argues for paying off debts, including the mortgage, as fast as possible in order to become debt free. Why? Not merely to save money: For Ramsey, ridding oneself of debt is not just a sound investment — it is a moral imperative that akin to achieving freedom from slavery.[1]

Dave Ramsey's view of debt is different from what standard portfolio theory suggests. Benchmark models of savings and investment, which underlie theories of monetary transmission and lifecycle savings, assume that households allocate assets to maximize the risk-adjusted return on their total portfolios, including debt and savings. Because households care about the risk and return of their portfolio, strictly-dominated investments will receive zero portfolio weight. Contra Dave Ramsey, no special consideration should be given to whether cash is spent for a new investment or to pay off debt per se as long as the risk-adjusted return is maximized.[2] In particular, households should not voluntarily use extra cash to pay off debt when higher-return investments are available for the same level of risk.

This paper tests whether households maximize the return on their portfolios across assets and liabilities, as theory assumes. To do so, we study how households adjust their mortgage repayment behavior when outside investment opportunities change. Every month, more than one in five U.S. borrowers with a conforming mortgage voluntarily pay more than their re-

[1] On the *Dave Ramsey Show*, he states that "the borrower is slave to the lender, voluntarily signing up to give our lives away. Well, this show is about breaking free." See: https://www.youtube.com/watch?app=desktop&v=3qZ9CMbtDQA&t=7m30s

[2] This is holding the liquidity and risk of different investment options the same, of course.

quired minimum mortgage payment, a practice known as *curtailment*. When mortgage rates are higher than investments with the same risk, portfolio theory predicts that instead of investing, households should either pay off their mortgage or refinance it. When investment returns are higher than the interest rate on debt, households should invest instead of prepaying. Prepayment is both less liquid than investing at the risk-free rate, and has higher risk, so it is strictly dominated when risk-free returns are higher than the household's mortgage rate.

We show that i) a large share of households — 22% under benchmark assumptions — prepay their mortgage even though they would earn a higher return investing risk-free and ii) the likelihood of prepaying a given fixed-rate mortgage did not change even after risk-free yields rose in 2022. We estimate the present value of losses from this behavior between January 2022 and November 2024 amount to $4.6 billion. Figure 1 illustrates. Before 2022, the 10-year Treasury bill rate was below the 10th percentile of rates among GSE-insured mortgages. Monetary tightening in 2022 raised risk-free rates so that half of borrowers had a mortgage rate below the risk-free rate. After interest rates rose, an increasing number of mortgages being curtailed had a negative rate spread, yet there was no decrease in the overall share of mortgages being curtailed. The average monthly curtailment payment among households with a negative rate spread is $556, which had an average NPV of -$87 under our preferred assumptions. Instead of responding to rate changes, the same households predictably prepaid their mortgage regardless of interest rates.

We consider three non-mutually-exclusive theories for why households prepay their debt when better investments are available. First, households might be averse to debt a la Dave Ramsey (Martinez and Shi, 2021; Meissner and Altmann, 2022). Second, households might be inattentive to interest rate changes or have adjustment costs (Andersen et al., 2020; Berger et al., 2024). Third, they might use their mortgage as a short-term cash flow management tool while neglecting the present value of their choices (Ganong and Noel, 2020a).

We present evidence in favor of debt aversion. Experimental evidence shows that households face declining marginal costs of debt—in other words, they gain a lot of utility from

making their final debt payments as compared to making earlier ones (Martinez and Shi, 2021; Meissner and Altmann, 2022). In line with this theory, the share of households prepaying their debt rises from 22% to nearly 50% as they approach their final mortgage payment. The rise in final period prepayment is not predicted by standard portfolio choice models or by theories of inattentiveness, and is hard to reconcile with omitted variables. Also in line with debt aversion, prepayment is more common for households with little or no credit card debt. In contrast, there is little evidence for inattentive or sluggish rate responsiveness: prepayment is equally common for households who are attentive on other dimensions, results hold after excluding borrowers making the same payment each month, and there is no quantifiable response to lagged rates.

Short-term cash flows also affect mortgage prepayment. To isolate the role of cash flows from that of interest rates, we compare adjustable-rate mortgages (ARMs) with fixed-rate mortgages (FRMs). While changes in the risk-free rate minimally affect the present value of prepayments for ARMs, the resulting higher monthly obligations significantly reduce ARM borrowers' excess repayments relative to FRM borrowers. Predictable ARM rate resets, and comparisons of lock-in to non-locked ARMs, provide further direct evidence by altering household disposable income without changing repayment net present value. We also show evidence on the role of cash flows from COVID stimulus checks and from exogenous cash flow fluctuations coming from the Alaska Permanent Fund.

What do our results mean for economic theory? We highlight two implications of debt aversion, beyond the obvious modifications to models of portfolio choice. First, debt aversion can cause a "sluggish" response to macroeconomic variables (Sims, 2003; Reis, 2006). Dispersion in households' debt aversion could mean that few households are at the margin between repayment and investment, muting the aggregate response to changes in interest rates. Second, if households are averse to borrowing, then they could act liquidity constrained even if they in principle have borrowing capacity available (Thaler, 1990).

The findings rely on a new methodology to quantify the costs of strictly-dominated mort-

gage prepayment that takes into account the tax implications of debt and investment. We measure prepayment using publicly-available from Fannie Mae and replicate our findings using data from a credit registry. The calculated costs vary somewhat depending on assumptions relating to the applicable marginal tax rates, which we estimate with the help of NBER TAXSIM and data from the Survey of Consumer Finances (SCF). Depending on assumptions about households' best alternative use of funds, the share of households with negative rate spreads varies from 39% to 69%, and the aggregate NPV of negative-spread curtailment varies from $2.2 billion to over $8.4 billion. Households with a higher credit score and more mortgage experience curtail their mortgages more often in general. They save more money when they have positive rates spreads, but they are also more likely to engage in negative-NPV curtailment, including after 2022.[3] Whereas refinancing "mistakes" are more common among households with lower credit scores, households with higher credit scores are more prone to negative-NPV curtailment.

Although the SCF tells us average marginal tax rates by borrower, we do not observe actual or perceived tax rates at the individual level (for example, we do not know how well households understand tax-deferred retirement accounts). Therefore, an important test of our theory comes by studying within-borrower behavior over time in order to hold borrower characteristics constant. Studying just changes in interest rates for the same person over time, we estimate an elasticity of prepayment with respect to rates of approximately zero. When rates change, some households should be on the margin of prepaying (even if we cannot measure precisely which ones), yet we find that almost none do. The lack of prepayment response to rate changes is the mirror of the finding that household borrowing does not change much when rates change, either (Anenberg et al., 2023).

Mortgage repayment decisions offer an ideal setting to test household portfolio theory and attentiveness. Studying how interest rates alter debt and savings decisions is challenging without knowing households' perceived rates of returns and constraints for all their assets and

[3]When the rate spread is positive, they might do even better by refinancing their mortgage. This is a different sort of financial mistake than what we focus on here.

liabilities. This is less of a concern in our setting because changes in bond yields or CD rates alter the opportunity cost of paying down debt without the returns for a fixed-rate mortgage, allowing us to isolate household sensitivity to economic incentives while holding mortgage characteristics constant. Furthermore, since mortgages constitute the largest component of household debt, and excess repayments affect roughly 35% of mortgages each year, understanding this apparent insensitivity is important.

The disconnect between borrowing behavior and expected asset returns is closely linked to other findings in household finance. Beyond our paper, existing research has found evidence for debt aversion in several consumer credit markets, including student loans (Gopalan et al., 2021; Eckel et al., 2007) and credit cards (Prelec and Loewenstein, 1998). The setting of mortgage repayment is the most high-stakes and widespread test of debt aversion that we are aware of.[4] Another preeminent puzzle in the literature is that many households have high-interest debt despite having assets that could be used to pay it off, known as the "co-holding puzzle" (Gross and Souleles, 2002). This literature has highlighted the lack of fungibility between debt and credit (Telyukova and Wright, 2008) and insurance against credit limit volatility (Fulford, 2015) as explanations for why households simultaneously hold high interest debt and low interest assets. However, these existing explanations cannot explain why households would pay down low-interest debt instead of holding higher-interest savings. Other studies have shown that consumers do not optimize mutual funds with respect to returns (Choi et al., 2009) nor do they optimize debt repayments with respect to interest rates (Gathergood et al., 2019). Few have studied how consumers optimize across assets and liabilities; rather, they have confined themselves to studying household behavior within a particular asset class or portfolio

[4] A handful of papers that have studied curtailment previously, using self-reported Survey of Consumer Finances (SCF) data. Amromin et al. (2007) argue that many 1990s borrowers could earn more by sending excess payments to tax-deferred accounts, a tax arbitrage reduced by the Tax Cuts and Jobs Act of 2017 (McClelland et al., 2022). Adelman et al. (2010) show curtailment in the same era is linked to saving propensity and liquidity risk. Using loan-level records for 2001-2011, McCollum et al. (2015) document a continued rise in curtailment. None of these studies study the recent regime in which risk-free yields often exceed fixed-rate mortgage coupons. Compared to these papers, we extend the time series through 2024, provide (to our knowledge) the first NPV estimate of negative-spread curtailment, and exploit credit-registry data to connect partial prepayment to broader household financial behavior.

component.

Our findings also contribute to the literature on financial "mistakes". Economists have long studied mistakes when shopping for mortgages (Woodward and Hall, 2012; Bhutta et al., 2020a) and the failure to refinance when mortgage rates are low (Keys et al., 2016; Gerardi et al., 2023). The monthly costs of curtailing negative rate spread mortgages is of a similar order of magnitude as these mistakes highlighted in existing research. Failure to shop for mortgages costs an estimated $100 per month (Alexandrov and Saunders, 2023). and failure to refinance costs an estimated $160 for the median household (Keys et al., 2016). Thus, the costs of "excess" curtailment are smaller than the costs of failing to refinance but are of a similar order of magnitude (Bhutta et al., 2020b).[5] Inattentiveness and financial literacy possibly explain some financial "mistakes" households make (DellaVigna, 2009; Shah et al., 2015). Different from these papers, we model prepayment as reflecting preferences to avoid debt, rather than an error made by unsophisticated borrowers.

Similar to work on tax salience (Chetty et al., 2009), the lack of salience of investment returns to mortgage borrowers has implications for how monetary policy passes through to consumer borrowing decisions. Many financial mistakes are caused by a failure to act—like a failure to search, to refinance, to save, or to invest. Prepayment is different from other mistakes because it requires action; without taking action, households would just make the default mortgage payment. By studying prepayment we learn about households' underlying preferences, abstracting away from present bias or inattention. Whereas other sorts of behaviors can plausibly be changed using "nudges" that change default behavior, changing prepayment would require more direct intervention.

[5]More broadly, many researchers have identified other financial markets where households appear to make sub-optimal choices such as in investment decisions (Calvet et al., 2007), credit card borrowing (Gathergood et al., 2019; Bertaut et al., 2009) and health insurance plan choices (Ericson and Sydnor, 2017).

2 Mortgage Valuation and the Cost of Curtailment

A conventional fixed-rate mortgage with a monthly interest rate r_m and a term of N months can be viewed as a stream of N fixed payments. Let c denote the equal monthly payment. The principal value P of this mortgage is then the present value of all future payments:

$$P = \sum_{t=1}^{N} \frac{c}{(1+r_m)^t}. \tag{1}$$

Households, however, have the option to *curtail* the loan—that is, make a payment larger than c in a given month. Unlike a standard extra payment on credit card debt, a curtailment in a mortgage reduces only the *final* months payment (rather than all future payments). As we will show, this feature has important implications for the net present value (NPV) of the decision to curtail.

Consider a curtailment of amount Δ made when there are N payments remaining. The principal balance of the loan falls by Δ, thereby reducing the final months payment by an amount γ that can be derived from the mortgage amortization formula. Specifically, letting P be the remaining principal,[6]

$$P - \Delta = \sum_{t=1}^{N-1} \frac{c}{(1+r_m)^t} + \frac{c-\gamma}{(1+r_m)^N}. \tag{2}$$

Solving for γ implies that

$$\gamma = \Delta(1+r_m)^N.$$

Thus, a partial prepayment now lowers the terminal payment by γ, but the earlier monthly payments c remain unchanged.

To evaluate the financial cost or benefit of such curtailment, denote by r the households opportunity cost of capital (e.g., the after-tax yield on an alternative investment). The net

[6] That is, P in (1) is the outstanding balance at the time of curtailment.

present value of a curtailment of size Δ is then

$$\text{NPV} = \Delta \frac{(1+r_m)^N}{(1+r)^N} - \Delta. \tag{3}$$

Whenever $r_m < r$, this NPV becomes negative because the discounted value of reducing future payments at rate r_m is below the return that could be earned at rate r. In other words, if $r_m - r < 0$, a household strictly prefers to invest Δ at the higher outside rate r rather than to pay down its mortgage. Conversely, if $r_m > r$, prepaying (or refinancing) produces a positive NPV because the mortgages effective rate of return exceeds r.

Naturally, r_m may exceed r if, for example, long-term market rates have fallen below the contractual mortgage rate since origination. In that case, refinancing often yields even larger benefits for the borrower, as refinancing would lower many or all future payments rather than merely the final one. Our analysis focuses on the case $r_m < r$, in which a curtailment is unambiguously suboptimal from a purely financial perspective. We thus abstract from the option-like nature of refinancing.[7]

In most cases, we think of prepayment as a relatively illiquid investment as compared to risk-free assets, making it an even worse investment when mortgage rates are above risk-free yields. Extracting equity from a house might require paying fees and a higher interest rate, on top of time and hassle costs. One caveat to this is that prepaying borrowers have the possibility of *recasting* their mortgage, i.e., extend the maturity after prepaying, and benefit from lower monthly payment. Recasting would potentially increase later liquidity. While there is not systematic evidence on the prevalence of recasting, industry and anecdotal evidence suggests that it is extremely rare because of high requirements by lenders (Lerner, 2021; Brown, 2023).

[7] See, for instance, Agarwal et al. (2013) for a derivation of optimal refinancing rules and Keys et al. (2016) for empirical evidence on refinancing decisions.

3 Stylized Framework and Theoretical Mechanisms

This section presents a stylized framework that shows how theories of portfolio choice can account for the mortgage-repayment patterns we observe. We begin from a frictionless baseline and add (non-mutually-exclusive) variations that generate motives to prepay. Each theory maps to a cluster of empirical tests that we explore in subsequent sections.

3.1 A frictionless benchmark

Consider the simplest static portfolio problem, abstracting from intertemporal optimization. A household receives a liquid cash flow F and allocates it between i) an outside asset that earns an after-tax, risk-adjusted return r, and ii) an extra principal payment on a fixed-rate mortgage that earns an after-tax return r_m.

Because both assets are risk-free to the borrower, optimization yields a corner solution:

If $r_m > r$, the household devotes *all* of F to pre-payment;

if $r_m < r$, it devotes *none* of F.

Formally, let $x \in [0,1]$ be the share of F allocated to pre-payment. Expected wealth next period is

$$W(x) \;=\; xF\,(1+r_m) \;+\; (1-x)F\,(1+r). \tag{2.1}$$

Maximizing $W(x)$ implies $x = 1$ when $r_m > r$ and $x = 0$ otherwise. The benchmark therefore predicts (i) no "dominated" pre-payment, (ii) an aggregate pre-payment share that jumps discontinuously when the spread $r_m - r$ crosses zero.

The benchmark also implies that *no* household should prepay its mortgage while a higher after-tax return is available at the risk-free rate. Allowing for a liquidity premium on the risk-free asset only strengthens this conclusion, since the outside asset is more liquid than the mortgage. Finally, we abstract from refinancing: when the risk-free rate exceeds the mortgage rate, refinancing is generally unprofitable.

3.2 Three extensions that rationalize repayment

We consider three mechanisms that can rationalize pre-payment even when it is not NPV-maximizing.

Search and attention. Theories of or attention costs provide one psychological foundation for our findings. The financial or psychological costs of search could prevent households from finding an alternative use of funds unless the benefits of doing so are very high. This theory is plausible because the amount of cash used for curtailment each month is typically small, less than $100.

Households may face costs of monitoring or acting on interest rates. A tractable representation is a fixed adjustment cost $\kappa > 0$: the household pre-pays only if the perceived gain exceeds that cost,

$$(r_m - r)\, F \;\geq\; \kappa. \tag{2.2}$$

Inequality (2.2) creates an *inaction band*: small rate changes leave behavior unchanged; sufficiently large changes trigger lumpy adjustment. Further, when a household receives an increase in cash available for investing, they should be less likely to curtail, since the benefits of finding higher-return uses of funds increases. Andersen et al. (2020) document such behavior for mortgage refinancing among middle-aged and richer households.

A closely related micro-foundation is *rational inattention*: households choose information precision at a cost (Reis, 2006). Optimal precision delivers a posterior belief $\mathbb{E}[r]$ that smooths rate shocks over time. Random attention, as in Berger et al. (2024), generates sluggish responses akin to a Calvo-style process (Calvo, 1983). The key prediction is that households respond to both current and past rates. Empirically, Andersen et al. (2020) find that low-income and older households often fail to refinance regardless of prevailing rates, suggestive of random attention rather than fixed costs.

Debt aversion. Search- and attention-based stories imply that households *would* change behavior if they knew about outside opportunities and could act costlessly. *Debt aversion* offers a different explanation, rooted in preferences. Experimental evidence shows that consumers may overvalue debt reduction per se, even at the cost of other higher-yielding opportunities (Loewenstein and Prelec, 1992; Prelec and Loewenstein, 1998; Meissner, 2016; Duffy and Orland, 2020; Meissner and Albrecht, 2022; Martinez and Shi, 2021). Beyond lab experiments, evidence for debt aversion has mostly come either from field experiments (e.g., Field 2009; Caetano et al. 2019) or observational studies of student loans (e.g., Gopalan et al. 2021; Eckel et al. 2007) where the evidence has been mixed. No previous research has documented the costs of debt aversion in a setting as widespread or costly as ours.

We model a psychological cost of carrying mortgage debt D. Let x be the share of the cash flow F devoted to pre-payment (so the new balance is $D - xF$). The household maximizes next-period wealth net of this cost:

$$V(x) = W(x) - u(D - xF), \tag{4}$$

where the cost function satisfies $u' > 0$, $u'' < 0$. The first-order condition is

$$r_m - r = -u'(D - x^\star F), \tag{5}$$

so pre-payment can occur with a negative return spread whenever the spread is compensated by the marginal utility of reducing debt. An additively separable "debt-utility" term of this sort arises naturally from the mental-accounting framework of Thaler (1990), who argues that debt aversion can account for high apparent household liquidity constraints. Similar formulations appear in Prelec and Loewenstein (1998).

Martinez and Shi (2021) provide direct experimental evidence that households are more willing to sacrificing high returns in order to pay off debt when their debt amounts are lower. This feature of debt aversion is captured by the concave shape for $u(D)$. Therefore, one way to

test for debt aversion is to show that repayment increases when households are close to paying off their mortgage entirely. In our setting, we expect curtailment to be higher for households with a low remaining mortgage balance. An advantage of this test is that a spike in prepayment close to completion is not predicted by other models of financial behavior, including the standard frictionless model. The "bonus" that households get when they finish paying off their debt aligns with anecdotal evidence from mortgage market observers. For example, Fabozzi (2016) writes that

> Curtailments, as well as full payoffs, tend to increase late in the life of a pool as mortgagors grow older and wish to eliminate mortgage debt prior to retirement.

Another test for debt aversion comes from studying household behavior beyond mortgages. Since we think of debt preferences as being stable across debt types, evidence for debt aversion comes from other financial behaviors, such as a tendency to pay off unsecured debt among prepaying households.

Finally, although debt aversion does not directly model a slow or lagged response to interest rates, it can generate low aggregate responses if many households are inframarginal to debt repayment. Heterogeneity across household preferences can mean that few households respond to interest rate changes because return differences are small compared to their preferences for debt repayment.

Liquidity and cash flows. In mortgage-modification settings, Ganong and Noel (2020b) show that changes in mortgage NPV have little effect on behavior, whereas changes in short-run cash flow have large effects. By analogy, households that ignore NPV may still respond to cash-flow shocks. We therefore study both predictable and unpredictable cash-flow shocks—rate resets on adjustable-rate mortgages are especially useful because they offer a natural comparison to fixed-rate loans and can be either anticipated or unanticipated.

4 Data and Measurement

4.1 Data Sources

Mortgage Data Our main source of data is the Fannie Mae Single-Family Loan Performance data set. The data set includes monthly loan-level performance history for 30-year or less, fully amortizing, single-family fixed-rate conforming mortgages. This includes both purchase and refinance loans. Only mortgages originated on or after January 1, 1999, or acquired by Fannie Mae on or after January 1, 2000 are included. We exclude loans that were ever delinquent and we limit the sample to January 2011 - November 2024. We take a one percent random sample of loans for computational convenience.

The Fannie Mae file includes variables measuring the principal balance, interest rate, maturity at origination, and a recalculated maturity that takes into account past curtailment. We use the mortgage amortization formula to calculate payments in excess of the required monthly payment. Principal balances are rounded to the nearest $1,000, so the formula sometimes predicts curtailment that is actually due to rounding error. To avoid overestimating curtailment, we ignore curtailment amounts less than $20, which is greater than the maximum measurement error. We verify the curtailment calculation in two ways: (1) by showing that the change in remaining maturity matches what we expect based on the curtailment we calculate and (2) by simulating curtailment in a placebo dataset and showing that our procedure backs out curtailment correctly.

We study adjustable-rate mortgages using the Freddie Mac Single-Family Non-Standard Dataset, which has a similar structure to the Fannie Mae Single-Family Loan Performance data set but has ARMs.

Tax Rates We use the 2010-2019 Surveys of Consumer Finances (SCF) and the NBER TAXSIM program to calculate marginal tax rates. We limit the SCF data to single-family fixed-rate mortgages and to respondents in the SCF using the current year, the mortgage origination

year and the mortgage principal amount. Since the mortgage data does not include the state of residence, we estimate state marginal tax rates by randomly assigning mortgages to each state proportional to the state population. We assume that households with a tax-deferred retirement plan available have a marginal tax rate with respect to earnings equal to 0.[8] Then we calculate the post-tax mortgage interest rates (r_m in (3)) and post-tax maturity-matched Treasury securities (r in (3)).

Discount Rates Our baseline results measure discount rates using maturity-matched rates on Treasury Bills.[9] Section 4.3 and Table 2 show results using other assumptions. We also use pre-tax mortgage interest rates for r_m, and pre-tax Treasury securities and mortgage-backed securities for r. Lastly, we also relax our assumptions regarding the availability of a tax-deferred retirement account.

Consumer credit records Other credit variables come from the University of California Consumer Credit Panel (UC-CCP). This dataset consists of loan-level credit records from 2% of the U.S. population for each quarter from 2004 to 2023. We restrict our sample to mortgage borrowers with active mortgages and information on their actual payment amount and their scheduled payment amount. Both scheduled and actual payments are available so we do not need to use the amortization formula to back out curtailment. We can also observe borrowers' credit scores each quarter and balances on other financial products.[10] The main disadvantage of using credit records is that the data do not include interest rates, so we cannot use this data to calculate the NPV of mortgage curtailment even as we know it occurs. However, by linking

[8] As of 2024, about 14% of households contribute the maximum amount to their tax-deferred retirement accounts each year (Vanguard, 2024).

[9] For mortgages with five years or more remaining maturity, we use a (post-tax) 10-year Treasury bill as the risk-free rate. For households with a mortgage maturity date between five and two years, we use a 5-year Treasury bill; between one and two years, we use a 2-year Treasury bill; and between zero and one years, we use a 1-year Treasury bill.

[10] Similar to Amromin et al. (2020) and Adelman et al. (2010), we use whether a borrower has a credit card and their credit card balances to predict mortgage curtailment payments. In contrast to this previous work, we use administrative data on credit card balances instead of self-reported values. In order to calculate whether a borrower has a credit card, we only consider active credit card accounts.

curtailment data to information about other sorts of financial behavior, credit records allow us to study behavior in a variety of products at once.

The second disadvantage of the credit registry data is the inability to distinguish between i) true curtailment that reduces the remaining mortgage maturity and ii) early payments without a reduction in maturity (e.g., when households make the next month's payment before its due date). We remove the latter category of payments, which we call "clerical" curtailment, from the analysis sample. The loan details provided in the Fannie Mae data make this possible (see Appendix B for details), but this is not possible in the UC-CCP data. Consequently, some excess payments flagged in the UC-CCP data will not be true curtailments; i.e., we calculate that households are more likely to curtail using the credit registry data as compared to the Fannie Mae data. Figure A2 highlights the discrepancies: the benchmark curtailment flag from the Fannie Mae data (solid red) and from the credit panel (solid blue) are different. But when we add back clerical curtailers in the Fannie Mae data (dashed red line), the two series look similar.

4.2 Summary statistics

Table 1 provides summary statistics of key variables from the Fannie Mae dataset at the loan-month level. We partition the table into all borrowers, curtailers, and non-curtailers. Panel A contains the entire sample period (January 2011 - November 2024). Panel B contains only January 2022 - November 2024. Panel C is the same time frame as Panel B, but we restrict the sample to mortgages with negative interest rate spreads. The table shows that across multiple dimensions, curtailers and non-curtailers are quite similar. For example, both types have origination balances and remaining balances equal to about $220,000 and $180,000, respectively. However, among negative rate spread loans in recent years, the differences are much larger: about $268,000 and $222,000 vs $281,000 and $246,000. Both types face similar marginal tax rates on income (about 12%) and mortgage interest (also about 12%). Curtailers, on average, have slightly lower mortgage interest rates (3.93%) than non-curtailers (4.10%). They

also have shorter terms, on average. Naturally, curtailing households have shorter effective remaining terms; across all years curtailing households on average have 219 months until maturity, whereas non-curtailing households have 259 months remaining. This pattern is similar in the January 2022-November 2024 subsample: curtailers have 228 months until the loan is paid in full, whereas non-curtailers have about 265 months.

Panel A of Figure 2 shows how the monthly probability of monthly mortgage curtailment varies by borrower credit score since 2011. The share of mortgages curtailed during this time is about 18%, lower than the share since 2022, which is around 21%. The share curtailed each month varies by credit score, with just over 10% curtailed each month for borrowers with credit scores in the 600s, to above 20% for borrowers with a credit score around 800.

The share of borrowers curtailing also varies by age, shown in Panel B of Figure 2. We calculate curtailment probabilities by age using credit score data, which show a somewhat higher curtailment share than Fannie Mae data. The share of borrowers curtailing follows an inverse-U shaped pattern with the likelihood rising from ages 30-55 and falling again from 55-75. The probability of retirement increases sharply at age 65, but there is no evidence that curtailment changes sharply at that age, suggesting that retirement-related incentives do not play much of a role for explaining curtailment behavior.

Lastly, we show the distribution of interest rate spreads in the data in Panel A Figure 3. The distribution in blue represents observations from 2011-2021; the mass is almost entirely positive, as outstanding mortgage rates were well above the risk-free rate. In red, we plot the distribution of spreads for 2022-2024; more than half of observations are negative. As an illustrative exercise, we show the hypothetical NPV of a $1 curtailment for a mortgage with interest rate of 4% and 100 months remaining. We keep r_m and Δ fixed, and show how the NPV changes when r goes from .01 to 8, or when the spread goes from about -4 to 4 points. Mechanically, for negative interest rate spreads, the net present value is negative. The slope of the line of best fit is about .08, indicating that for every point decrease in the spread, households lose nearly 10% of their investments.

5 Quantifying Curtailment Losses

5.1 Total costs

We calculate the total losses coming from curtailment of negative rate spread mortgages. For each mortgage and each month, we plug in the curtailment amount, mortgage interest rate and households opportunity cost of capital into Equation (3). For r_m, we use the after-tax mortgage interest rate. For r, we use the after-tax risk-free rate, taking into account the likelihood that the household can save in a tax-deferred retirement account. (Later, we will show how our estimates change under different assumptions about the tax advantages of mortgage interest rates or the taxability of investment returns.) We calculate the NPV of mortgage curtailments for each mortgage and each month. Then we calculate the total cost of negative-NPV curtailment each month.

We begin by illustrating how the net present value (NPV) of these curtailment payments compares with their nominal (or notional) amounts. The upper two panels of Figure 4 are histograms of "excess" repayment amounts, for the years 2011-2021 and 2022-2024. We right-truncate the figures at $600. Notably, mortgage repayment has a very long right tail so the average "excess" repayment is substantially larger than the median. A consequence of this is that aggregate repayment amounts are mostly driven by the largest repayers. Comparing the two time periods shows that they are broadly similar: Most excess repayments are for small amounts, and there is substantial bunching around round numbers, especially $100 and $200. The round number bunching is not a result of rounding in the data, since it is relative to the expected mortgage prepayment, which is unlikely to be a round number. Similarly, panels C and D show the distribution of curtailment volume scaled by the loan's monthly payment for 2011-2021 and 2022-2024, respectively. We right-truncate these figures at 1, which constitutes about 90-95% of curtailments. Again, the pattern of curtailment amounts relative to the monthly payment is similar in both time periods. The mean and median curtailment amounts as a share of the monthly payment are about 23% and 14%, respectively, for both 2011-2021

and 2022-2024.

Panels E and F of Figure 4 show the NPV of mortgage repayments over the same two years. From 2011-2021, nearly all excess payments were positive-NPV since most mortgages had a positive rate spread.[11] However, from 2022 to 2024, the distribution of NPVs shifts inward, with over half of the observed payments ultimately carrying a negative NPV.

How large are the aggregate curtailment losses? Figure 5 shows the time series of negative-NPV curtailments from January 2022 to November 2024. Panel A shows the aggregate amount of negative-NPV curtailment in the data; Panel B shows the average NPV of those payments. There was almost no negative-NPV curtailment prior to February 22, when interest rates began to rise. As per Figure 1, a long period of declining interest rates meant that for the previous 10 years nearly all mortgages had a positive spread relative to long-term treasury rates. The rapid increase in interest rates in 2022 meant that, for the first time, a substantial share of mortgages carried a negative rate spread. By the fourth quarter of 2023, the total cost was about $250mn per month for GSE-insured mortgages. This implies an average monthly cost of about $115 per borrower.

Another way of calculating the costs of negative-NPV curtailment is to ask how many basis points these borrowers are "giving up" by curtailing their mortgage instead of investing in T-Bills. Panel A of Figure 6 shows the share of negative rate spread households curtailing over time, while Panel B shows the average rate spread for negative rate-spread loans, weighted by the dollar value of curtailment. From January 2022-November 2024, this increased from about -0.2 percentage points to nearly -1.25 percentage points. At the same time, the share of all households curtailing their mortgage and having a negative rate spread rose to about 15%. On average, these borrowers forgo nearly a full percentage point of return.

[11] Of course, the NPV from refinancing could have been *even* higher.

5.2 Alternative assumptions

Our calculations so far have relied on assumptions about tax benefits of mortgage interest and the appropriate opportunity cost of capital for each household. We would like to understand how important these assumptions are for our results, and how the costs and benefits of mortgage curtailment would be different under alternate assumptions.

Table 2 shows how different assumptions affect the results.[12] The first row shows our benchmark assumptions: that investment returns are taxed at the marginal income tax rate except for households with an eligible retirement account, that mortgage interest is tax-deductible for households that can itemize, and that the risk-free rate is the appropriate opportunity cost of capital. Under these assumptions, 52% of mortgages have a negative rate spread from January 2022-November 2024, curtailment losses are about $4.6bn for GSE-insured mortgages, and the average NPV of payments on negative-spread loans nearly -$87.

Next, we explore the importance of tax-free retirement accounts. The availability of tax-free retirement accounts reduces the effective tax rate on investment returns. Removing the possibility for households to invest on a tax-free basis lowers the effective return to households' investments. To show how this affects our results, the second row of Table 2 assumes that the opportunity cost of capital is the $r_f \times (1-\tau)$, where r_f is the risk-free rate and τ is households' marginal tax rate. This is different from our benchmark case where we assume that investment returns are tax free only for households with a retirement account available. The estimates show that the taxability of investment returns affect the results substantially: The share of mortgages with a negative rate spread falls from 52% to 39%, and the total investment losses fall by about 55%, from $4.6bn to only $2.2bn. The reason for the change is that the marginal tax rate for curtailers is an average of around 12%, reducing the return for a hypothetical 10-year T-Bill from 4% to 3.5%. Since the 10-year T-bill rate is close to the interest rate on the median mortgage at this time, a change of one percent can substantially change the share of

[12] Column (1) shows the share of loan-month observations with a negative rate spread. Column (2) shows the aggregate NPV losses (in millions of dollars) for curtailing negative rate spread loans. Column (3) shows the conditional average NPV loss per curtailment for these loans.

mortgages with a positive or a negative rate spread.

In the third row, we show how the results change when we ignore taxes altogether. Naturally, ignoring the taxability of investments increases the effective return, which yields in larger NPV losses. When we ignore taxes, we estimate nearly $6.3 billion dollars lost due to curtailment.

Finally, another important assumption is that households use the maturity-matched risk-free rate to discount future mortgage payments. Although we think this is a reasonable assumption, mortgage payments should be discounted at a somewhat higher rate since they are not in fact risk-free. To explore the role of repayment risk premia, we recalculate negative-NPV curtailment discounting payments using the contemporaneous yield on maturity-matched mortgage backed securities. When we do this, the benefits of curtailment are much larger: The share of mortgages with a negative rate spread rises to 69% and the average loss rises to $120. The NPV of negative curtailment doubles. The reason is that households' opportunity cost of capital is higher, so future mortgage payments are discounted at a higher rate. Arguably, the higher discount rate illustrated here is more realistic, as it includes a nontrivial risk premium, and thus may offer an upper bound on the magnitude of curtailment losses.

5.3 Heterogeneity

Our analysis so far has shown that a substantial share of borrowers curtail their mortgages even when they have a negative rate spread. This section explores what types of borrowers are most likely to do so, both to see if negative-NPV curtailment amplifies existing disparities and to glean further clues about the behavioral forces at play. In particular, we ask whether households who appear more financially sophisticated (e.g., higher credit score, more experience) are less prone to negative-spread curtailment, and how curtailment varies over the lifecycle.

Table 3 explores several types of heterogeneity in negative-NPV curtailment. The first row shows summary statistics for the full population of mortgage borrowers from January 2022-November 2024. Overall, 52% of borrowers have a negative rate spread at some point, and

among these, 22% choose to curtail their mortgage. Conditional on curtailing, the average extra payment is $556, with an average net present value (NPV) of about -$87. This implies that, on average, these borrowers are losing over 15% of their payments relative to investing at the risk-free rate. Although such behavior is financially costly, it aligns with the notion that certain borrowers have a strong intrinsic aversion to debt.

Next, we partition borrowers by above- and below-median credit score to see if putatively more sophisticated households are less likely to curtail when the rate spread is negative. The results indicate the opposite. Borrowers with high credit scores are more likely to hold a negative spread (due in part to securing lower rates at origination) *and* are also more likely to curtail conditional on having a negative spread. Moreover, they tend to make larger extra payments. In effect, the costs of negative-NPV curtailment are weighted disproportionately toward higher-credit-score individuals.

Other salient subgroups reveal a similar picture. First-time home buyers, for example, turn out to be *less* likely than other borrowers to engage in negative-spread curtailment, suggesting that greater market experience does not necessarily inhibit costly prepayment behavior. Urban borrowers, meanwhile, are somewhat more likely to have negative spreads than rural ones, and make larger extra payments once they decide to curtail. Lastly, consider those who originated their mortgages in 2020–2021, a period of historically low rates. They are more likely to have a negative rate spread (70% in Table 3), but are no more likely to curtail once on a negative-spread mortgage. Nor do they make meaningfully bigger extra payments than the overall sample. This finding implies that even homeowners who have refinanced "optimally" at some point are still subject to the same psychological pull toward debt elimination, echoing our broader conclusion that refinancing mistakes and negative-spread curtailment are distinct phenomena.

Overall, the patterns in Table 3 suggest that negative-NPV curtailment is not confined to financially inexperienced or liquidity-constrained groups. Instead, it appears to reflect a cross-cutting preference for accelerated debt payoff. The next sections leverage both cross-sectional

and time-series evidence to show how the behavior changes following exogenous changes in income and interest rates.

6 Mechanisms

We now turn to a series of reduced-form tests aimed at quantifying the importance of traditional portfolio-maximization considerations for curtailment relative to alternative explanations. While standard net present value (NPV) arguments suggest borrowers should be highly sensitive to changes in the mortgage interest rate relative to their outside investment opportunities, we find that fixed household characteristics and short-run cash flows seem more important. Specifically, we show that (a) individual fixed effects capture a large share of curtailment decisions, pointing to time-invariant household characteristics; (b) curtailment is virtually unresponsive to interest rate changes; (c) exogenous changes in disposable income or monthly payments, such as those come from ARM resets, are strong predictors of prepayment; and (d) that borrowers are more likely to prepay when they are close to paying off their mortgage entirely, in line with theories of debt aversion. The results change little when we focus on borrowers who originated their mortgage when interest rates were low or when we eliminate borrowers who make the same payments each month.

6.1 Fixed Effects and Borrower Heterogeneity

We begin in Table 4 with regression specifications that demonstrate the importance of individual characteristics for explaining mortgage curtailment. Columns (1) and (2) decompose the variation in curtailment into individual fixed effects (δ_i) and time fixed effects (γ_t). Column (1) shows that individual fixed effects alone explain about 25% of the variation in curtailment (an R^2 of 0.255), suggesting that stable, time-invariant preferences play a major role in whether households overpay their mortgages. Adding time fixed effects (Column 2) raises the R^2 to 0.376, underscoring that while aggregate conditions matter, persistent household traits

matter more.

In Columns (3) and (4) of Table 4, we explore possible reasons why borrowers make mortgage curtailment payments. Column (3) adds variables measuring households' credit score, total mortgage balance, the presence of a credit card, and total credit card balance. These variables are particularly useful for testing competing explanations for curtailment. We find that higher credit scores are associated with a greater propensity to make curtailment payments. The estimated coefficient on credit score is 0.0477, so a 100-point increase in the credit score raises the likelihood of curtailment by 4.77 percentage points.

At the same time, credit card ownership and larger credit card balances predict a lower propensity to curtail. Households without a credit card are over 5 percentage points more likely to make extra mortgage payments, even conditional on other covariates. We interpret these findings as consistent with a preference to have low debt across different categories of borrowing. They could also be consistent with paying off high-interest credit card debt before paying off low-interest mortgage debt.

Finally, in Column (4), we include both individual fixed effects and individual covariates, allowing us to examine within-borrower variation over time. Perhaps surprisingly, many of the coefficients increase after including individual fixed effects. The coefficient on the credit score rises from 0.0477 to 0.114, and the coefficient on the credit card indicator increases in absolute magnitude from -5.187 to -8.665. This means that when a households credit score improves, they become more likely to curtail their mortgage; and when they acquire a credit card, they become less likely to curtail. These patterns are consistent with the idea that mortgage prepayment behavior reflects broader financial habits: borrowers who improve their financial position-either by increasing their credit score or reducing reliance on revolving debt-also tend to allocate more funds toward curtailment.

Another way to calculate the importance of fixed household characteristics is to consider the persistence of curtailment over time. Conditional on curtailing in month t, the probability of curtailing in month $t+1$ is above 80%. Conditional on not curtailing in month t, the

probability of curtailing in $t+1$ is below 5%. So overall, curtailment is highly persistent.

Our results show that the propensity to curtail changes little for each household over time, even when the benefits of curtailment shift due to changing interest rates. If households were fully cognizant of the NPV implications of prepaying, we might expect some of them to adjust their behavior dynamically as r_m and r fluctuate. Instead, we observe a persistent pattern: some borrowers consistently overpay their mortgages, while others rarely do. This suggests that individualized preferences or constraints play a more important role in curtailment behavior than traditional portfolio-based optimization.

6.2 Do Interest Rate Changes Matter?

The preceding evidence suggests persistent household heterogeneity in curtailment, potentially reflecting a fixed cost to holding debt. A separate question is whether *time-series* variation in mortgage spreads actually influences behavior. Standard models of cost-benefit calculations predict that if the outside return r rises (so that $r_m < r$ becomes even more pronounced), some marginal borrowers should stop making extra payments. Yet Figure 1 indicates little correlation between aggregate curtailment and interest rate changes.

To verify this formally, we estimate the linear probability model:

$$CURTAIL_{it} = \beta \text{ Spread}_{it} + \Omega X_{it} + \varepsilon_{it}, \tag{6}$$

where Spread_{it} measures the difference between the mortgages rate and a comparable risk-free rate. Results are shown in Table 5. Column (1) estimates a bivariate relationship and finds that each percentage point *higher* spread is associated with a 1 point (5% relative to the mean) *lower* likelihood of curtailment. Hence, curtailment is more widespread precisely where it is *less* profitable in strict NPV terms. In Column (2), we add loan fixed effects so that variation in risk-free rates over time identifies the effect. The coefficient is effectively zero, indicating that changes in the outside rate r have no discernible impact on curtailment. The exact point

estimate is just over .3 points, three times smaller than when we ignore loan fixed effects. Finally, Column (3) shows a cross-sectional relationship in line with Column (1). Households with *lower* mortgage rates (and thus a lower spread) actually curtail more—consistent with the notion that many of these borrowers are debt-averse and not primarily guided by market spreads.

We perform two robustness tests. First, to test if borrowers respond to both contemporaneous and *past* interest rates, we re-estimate (6), iteratively adding annual lags in $Spread$ up to four years. Results are shown in Table A2. Across all models, we find a precise null effect of historical spreads on a borrower's propensity to curtail. Secondly, to test for potential nonlinearities, we estimate a binscatter à la Cattaneo et al. (2024). A nonlinear and positive relationship would be evidence for switching costs or search costs in line with some theories of inattention as in Andersen et al. (2020). Results are shown in Figure A6. In Panel A, we use observations from the full sample period, Jan 2011 - Nov 2024. In Panel B, we focus only on 2022-2024, when opportunities costs are high and many households held a negative rate spread. Across all spreads, we find little if any sensitivity of variation in interest rate on household curtailment decisions. Further, the effect is effectively constant across spreads.

In short, while a classical financial perspective would predict a strong link between interest rate movements and curtailment, we see little sign of it.

6.3 The Role of Cash Flows

If households do not respond to interest rate changes, what *does* explain the time-series variation in curtailment? Building on Ganong and Noel (2020a) and related work, we hypothesize that disposable *cash flow*, rather than the present value of future payments, drives extra mortgage repayment. We test this hypothesis in two main ways: examining income shocks over time and comparing responses of adjustable-rate versus fixed-rate mortgage borrowers.

Income Shocks and Aggregate Patterns

We start by relating curtailment to disposable income. Figure 7 plots total monthly curtailment alongside real disposable income growth for high-income (top 10%) and middle-income (middle 40%) households using data from Blanchet et al. (2022).[13] Among the top 10%, curtailment closely tracks income gains and shows abrupt jumps following COVID-19 relief payments (see the vertical dashed lines), suggesting that liquidity surges fuel extra prepayment. By contrast, the middle 40% see a rise in disposable income in April 2020 but do not exhibit a commensurate spike in curtailment volume.

Additional corroboration comes from quasi-exogenous income shocks such as the annual Alaska Permanent Fund Dividend. In Appendix B, we show that each year in October, Alaskan households experience a jump in lump-sum cash, which is followed by a noticeable uptick in mortgage curtailment. This is consistent with a cash-flow channel: households who receive windfall income use at least part of it to pay down their mortgages early, regardless of the NPV.

Adjustable-Rate Mortgages as a Cash-Flow Test

We then turn to adjustable-rate mortgages (ARMs) for a more direct test of cash-flow versus NPV logic. As ARM rates fluctuate with market benchmarks, an increase in the risk-free rate r causes *both* a higher discount rate and a higher monthly payment. By contrast, for a fixed-rate mortgage (FRM), only the discount rate changes. If households weighed future cash flows in a strictly present-value sense, we might expect curtailment to be more sensitive for FRMs. If, instead, households respond primarily to current out-of-pocket payments ("cash-flow behavior"), ARMs should display more sensitivity to interest rate rises because their monthly obligations increase.

We test this by estimating a panel regression that interacts an ARM indicator with a benchmark interest rate, using the following specification:

[13] Data is downloaded from https://realtimeinequality.org/.

$$\text{Curtailment}_{it} = \alpha + \beta \cdot ARM_i \times \text{LIBOR}_t + \gamma X_{it} + \delta_i + \varepsilon_{it}, \tag{7}$$

where Curtailment_{it} is an indicator for whether borrower i makes an excess mortgage payment in month t, LIBOR_t is the 12-month LIBOR rate, and δ_i represents borrower fixed effects. X_{it} includes controls such as credit score and loan balance. Table 6 reports the results. Column (1) confirms that a 1 pp increase in LIBOR raises ARM borrowers own mortgage rate by about 0.644 pp on average. Based on equation (10) and the sample average of P, N, and r_m, we estimate that the typical ARM borrowers monthly payment rises by approximately $52 per 1 pp increase in LIBOR.

Column (2) shows that this rate hike reduces the likelihood of curtailment for ARM borrowers by about 1.9 pp, whereas FRM borrowers-whose nominal payments remain fixed-are unaffected. In Columns (3)-(5), we examine curtailment amounts, finding that small-dollar prepayments ($20-$500) decline most, consistent with a liquidity mechanism: as required payments rise, discretionary overpayments become less frequent.

To further study this relationship, we compare ARMs locked into introductory rates with those already floating. Table 7 shows that once the lock-in period ends, LIBOR fluctuations pass through fully to mortgage payments, generating a stronger negative effect on curtailment. This suggests that the relationship between rising interest rates and lower curtailment operates through the cash-flow channel rather than a shift in the perceived NPV of curtailment.

We confirm this pattern using an event-study approach around ARM rate resets. Specifically, we estimate the following local projection:

$$\text{Curtailment}_{i,t+k} - \text{Curtailment}_{i,t-1} = \beta_k(\text{Rate}_{i,t} - \text{Rate}_{i,t-1}) + \gamma X_{it} + \varepsilon_{it}, \quad k = -2, 0, 1, 2, \tag{8}$$

where the independent variable of interest, $\text{Rate}_{i,t} - \text{Rate}_{i,t-1}$, captures the change in borrower is mortgage rate upon reset. We include fixed effects and restrict the sample to ARMs with rate

adjustments occurring at least quarterly. Figure 8 plots the estimated β_k coefficients, showing that a 1 pp increase in the borrowers own interest rate reduces curtailment probabilities by 0.5–1.1 pp over the following two months, relative to a baseline mean of 18%. These results reinforce the conclusion that households respond to *contemporaneous payment obligations*, rather than changes in their mortgages relative NPV.

6.4 Paying Off Debt Entirely

Lab evidence on debt aversion has found a declining marginal cost of mortgage repayment, so we should expect to see a large jump in likelihood of curtailing near the end of the loan. This empirical prediction is important because the predicted behavior is hard to rationalize with other theories of debt repayment, including benchmark portfolio choice theory or models of inattention.

Figure 9 shows how repayment varies with the remaining mortgage balance; it plots the probability of curtailing against the remaining principal balance.[14] Curtailment occurs at more or less the same rate for all large balance sizes. For small balances, however, the probability spikes drastically. For example, the probability of curtailing at (50%) $500 remaining is nearly *double* the probability of curtailing (26%) at $50,000. Even comparing a remaining balance of $500 to that of $2,000, the probability of curtailing at the former balance is 58% greater relative to the latter.[15] This evidence is consistent with the model of Meissner and Albrecht (2022) and Martinez and Shi (2021), which feature a nonlinear psychological benefit to reducing the amount of debt.

[14] This figure uses UC-CCP data. The baseline probability of curtailing is higher in this dataset compared to the Fannie Mae dataset because we are not able to remove "clerical" prepayers. See Section 4 and Appendix A for more details.

[15] Fu (1998) finds a similar pattern using data from the 1960s-1990s, where the probability of curtailing jumps near the end of the loan's term.

6.5 Sophistication, Attentiveness and Inertia

Next, we turn to the possibility that borrowers are inattentive to interest rates. To investigate this hypothesis, we focus on groups likely to exhibit greater sophistication or attentiveness in their mortgage management decisions, and ask whether they are less likely to curtail their mortgage when interest rates rise.

We consider two proxies for sophistication and attentiveness. First, we identify households that refinanced their mortgages during the historically low-interest environment of 2020. Such borrowers are likely attentive, as refinancing typically requires active decision-making and awareness of prevailing interest rates. Second, we isolate borrowers who, conditional on observables, secured especially low mortgage rates at origination. To classify borrowers according to their mortgage rates relative to observables, we run an auxiliary regression of the mortgage interest rate on credit scores, loan-to-value ratios (LTV), and origination quarter. Specifically, we estimate:

$$\text{Rate}_i = \alpha + \beta_1 \text{CreditScore}_i + \beta_2 \text{LTV}_i + \gamma_{q(i)} + \varepsilon_i, \tag{9}$$

where $\gamma_{q(i)}$ are origination-quarter fixed effects. Borrowers are then grouped into quartiles based on the residuals from this regression, with the lowest quartile representing those obtaining particularly favorable rates, indicative of greater sophistication.

For each of these two groups-2020 refinancers and low-rate borrowers-we estimate regressions of curtailment behavior on interest rate spreads, paralleling our baseline specifications. Figure 10 Panel A presents these results, plotting the estimated coefficients and corresponding 95% confidence intervals. To contextualize these estimates, we scale the y-axis by the mean amount of monthly curtailment. This normalization provides a benchmark, allowing us to interpret the magnitude of a one percentage-point change in interest rates relative to typical repayment levels.

While the estimated coefficients are statistically significant due to the large sample, their

economic magnitude is notably small—close to zero in absolute terms. Even among borrowers identified as attentive to rates for the purpose of refinancing, curtailment decisions remain essentially insensitive to interest rate differentials. These results suggest that lack of attentiveness alone cannot explain the pervasive curtailment of negative-spread mortgages observed in our sample. Instead, the evidence is consistent with our broader findings from earlier sections: curtailment behavior appears driven primarily by household-level factors such as available cash flows and a persistent aversion to debt, rather than by a failure of attention or sophistication.

We also study the role of inertia. If households rarely update their knowledge of investment opportunities, their decisions to prepay could be determined by the interest rate when their mortgage was originated, rather than the current interest rate. Such inertia could result from a rational inattention model. The estimates in Figure 10b show how curtailment depends on initial interest rates. It shows that curtailment is indeed highly persistent, consistent with such a model: The probability of curtailment in month t conditional on curtailing in months $t-1$ and $t-2$ is about 80%. However, the probability of curtailment is nearly unrelated to the interest rates at mortgage origination. Even though curtailment is persistent over time, it is not related to the household's past interest rates. See Appendix D for more details.

Next we investigate whether persistence comes from households making exactly the same payment each month. We think that an important difference between curtailment and other financial behaviors is that curtailment requires an active choice for many households — prepaying is not typically a "default". A caveat to this claim is that households might pay the same amount every month, either due to habit or because of an "autopay" feature. Table A3 replicates the results in Table 5, removing households who make exactly the same payment each month. The results are very similar, if anything slightly negative; we cannot reject that the estimates are the same across both tables.

The estimates in these tables suggest that low responsiveness to rates is the result of time-invariant characteristics rather than a lagged response to rates or inattentiveness to market conditions. This distinction underscores the importance of incorporating behavioral and liquidity

considerations into models of household mortgage management, moving beyond traditional frameworks predicated solely on financial sophistication or attention.

7 Conclusion

What determines household decisions surrounding mortgage prepayment? A large literature studies households' decision to refinance their mortgage, but the decision to curtail a mortgage through partial prepayment is less understood. We show that between one-fifth and one-quarter of mortgages are curtailed each month, and that this share is not affected by changes in interest rates. Monetary policy tightening in 2022 caused risk-free rates to rise above the median rate on outstanding mortgages, resulting in a large share of mortgages with a negative interest rate spread being curtailed each month. This is the first paper to identify the phenomenon of curtailment of negative-rate spread mortgages. It also provides the first estimates of its costs to U.S. households. The total costs are about $4.6bn from GSE mortgages alone, which represents a pure transfer from households to mortgage investors.

We provide evidence that while households do not respond to interest rate changes, they do respond to changes in cash flows. We show this both using aggregate evidence and using exogenous changes in ARM rates which change cash flows available to households without changing investment opportunities. We also show that curtailment behavior is persistent over time, pointing us towards time-invariant psychological explanations. In line with persistent debt aversion, households become much more likely to prepay their mortgage when they are close to fully paying it off.

Our findings have important implications for economic theory. First, intertemporal consumption and savings models assume that households save and borrow to maximize expected lifetime utility given constraints (Attanasio and Weber, 2010). A preference against holding debt would lead to

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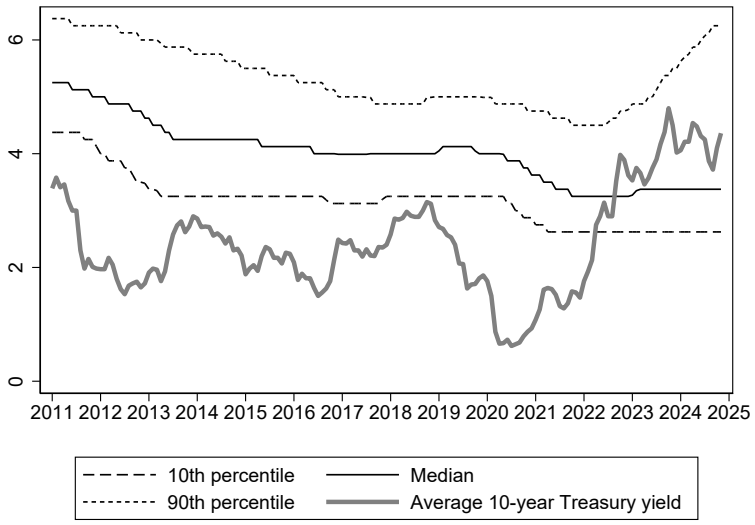
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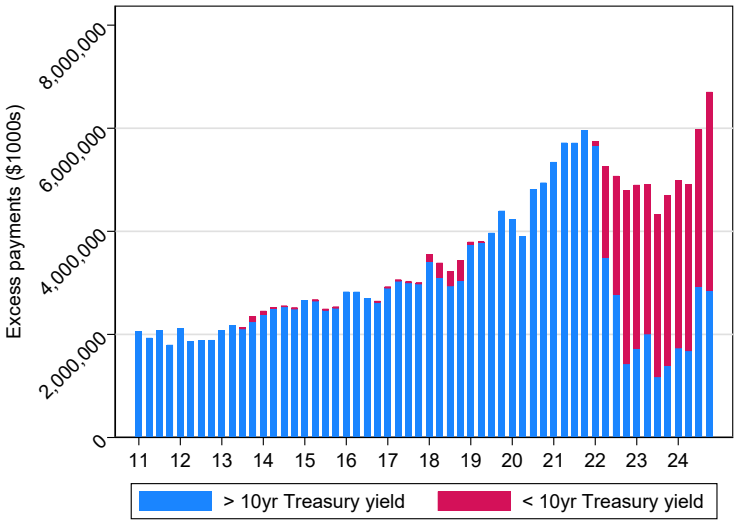
8 Figures and Tables

Figure 1

(a) Rates on outstanding mortgages and average 10-year Treasury yield



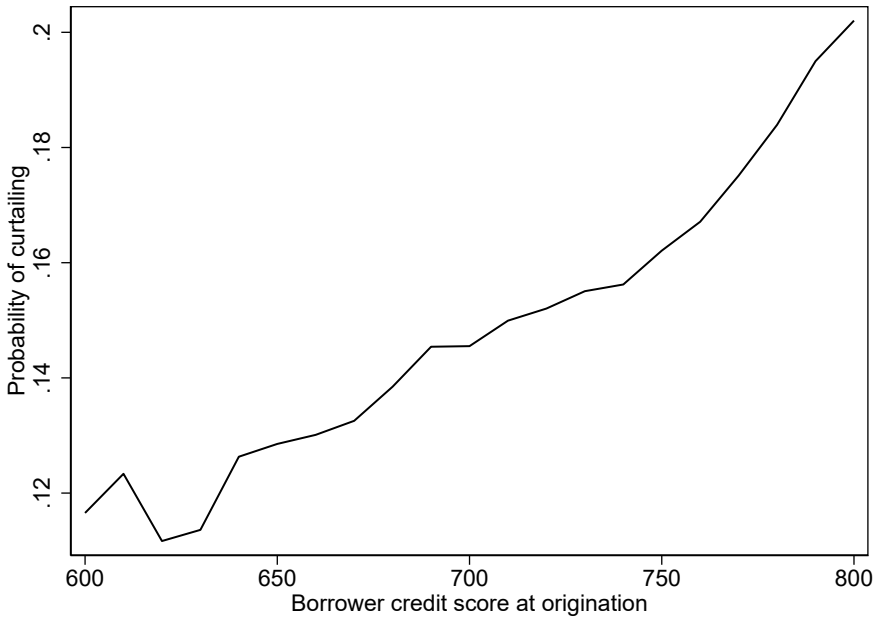
(b) Total excess payments by loans with above/below 10yr Treasury yields



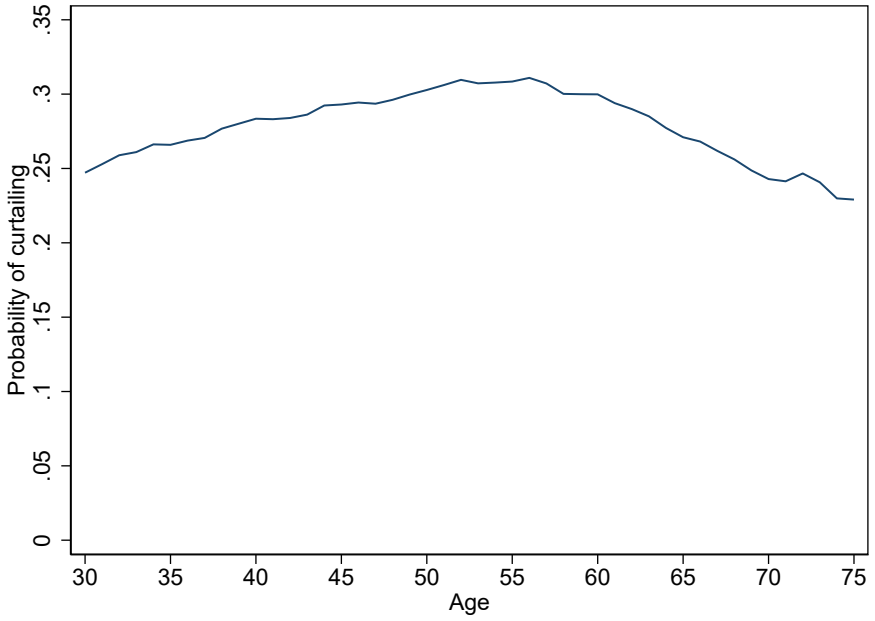
Note: Panel (a) shows the 10th, 50th, and 90th percentile for outstanding mortgage interest rates in black/dashed lines and average 10-year Treasury yield in gray, by month. Panel (b) shows total volume of curtailment amounts each quarter. The blue represents the share of curtailment volume from loans with mortgage interest rates above the prevailing 10-year Treasury yield, and the red shows the share from loans with below the 10-year Treasury yield. Interest rate data come from Fannie Mae and Treasury data come from FRED.

Figure 2: Probability of curtailment by borrower characteristics

(a) Probability of curtailment by borrower credit score



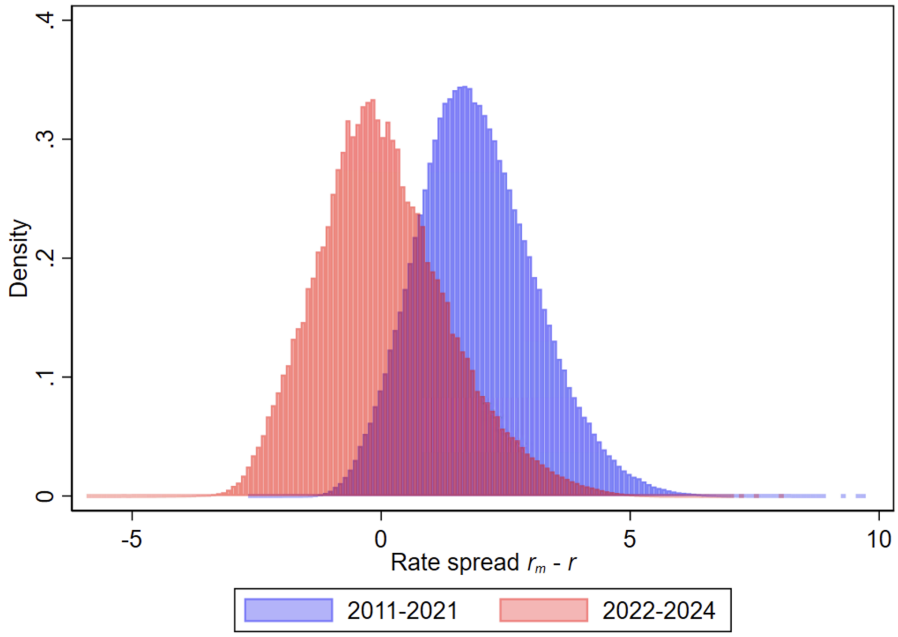
(b) Probability of curtailment by borrower age



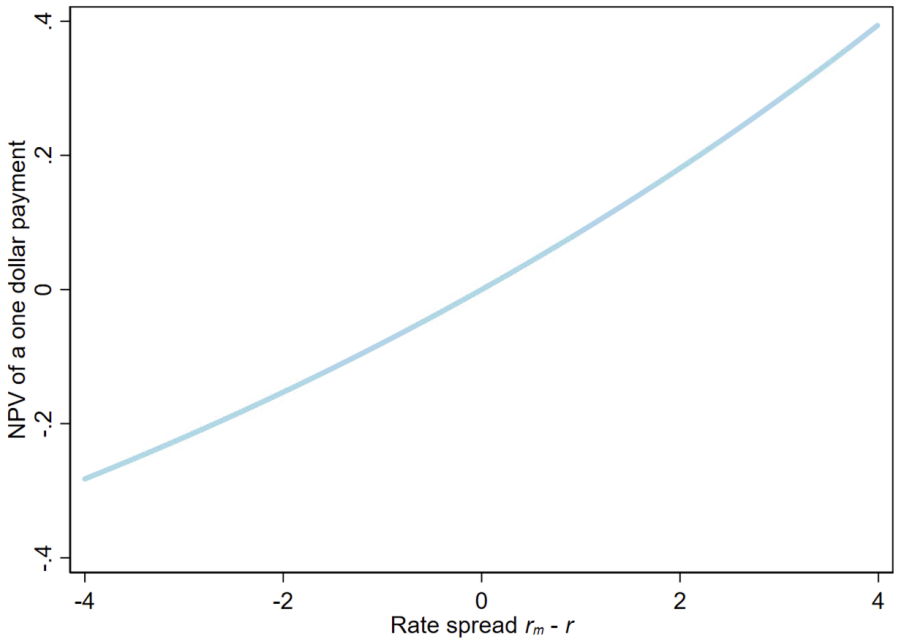
Note: Panel (a) shows the likelihood a household curtails its mortgage by credit score of borrower at origination. Credit scores are rounded to the nearest 10. Panel (b) shows the likelihood a household curtails its mortgage by age of borrower. Authors' calculations based on Fannie Mae Single-Family Loan Performance dataset and the UC-CCP.

Figure 3: Interest rate spreads and net present value of curtailments

(a) Distribution of interest rate spreads



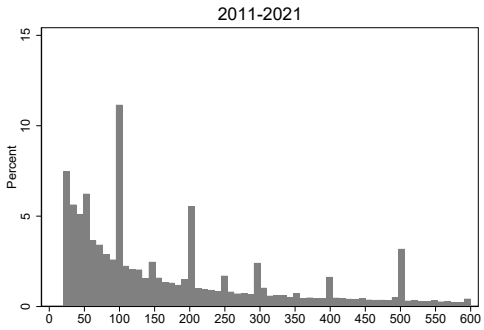
(b) Hypothetical NPV of a $1 curtailment



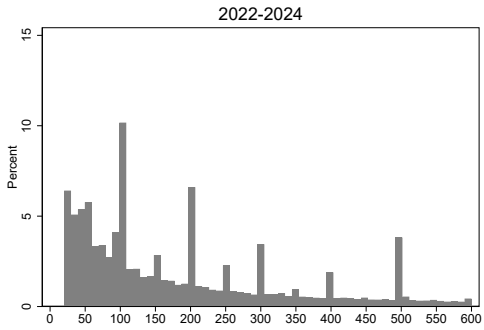
Note: Panel (a) shows the distribution of interest rate spreads for 2011-2021 (blue) and 2022-2024 (red). Panel (b) shows the hypothetical NPV of a $1 curtailment with 100 months remaining against different values of r. Authors' calculations based on Fannie Mae Single-Family Loan Performance dataset.

Figure 4: Distribution of excess payments

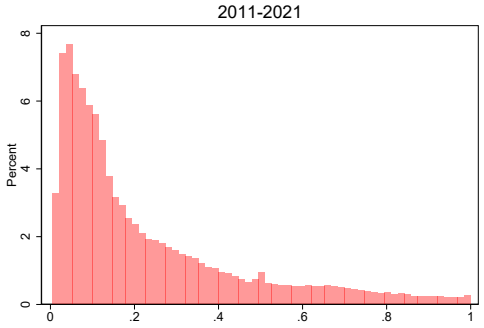
(a) Notional value 2011-2021



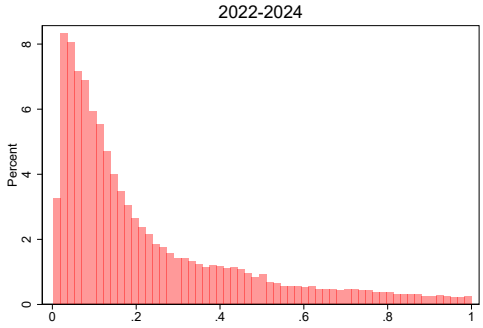
(b) Notional value 2022-2024



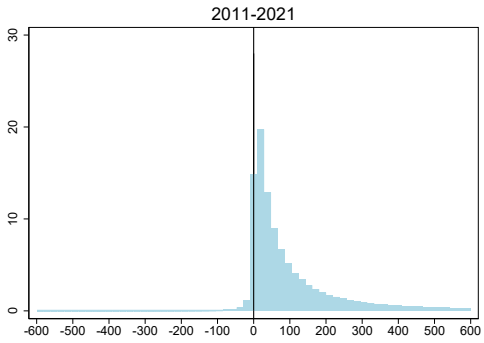
(c) Notional value scaled by monthly payment 2011-2021



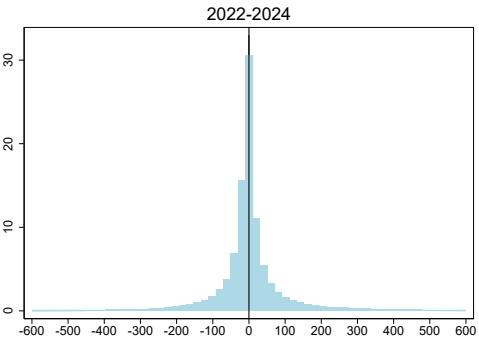
(d) Notional value scaled by monthly payment 2012-2024



(e) NPV 2011-2021



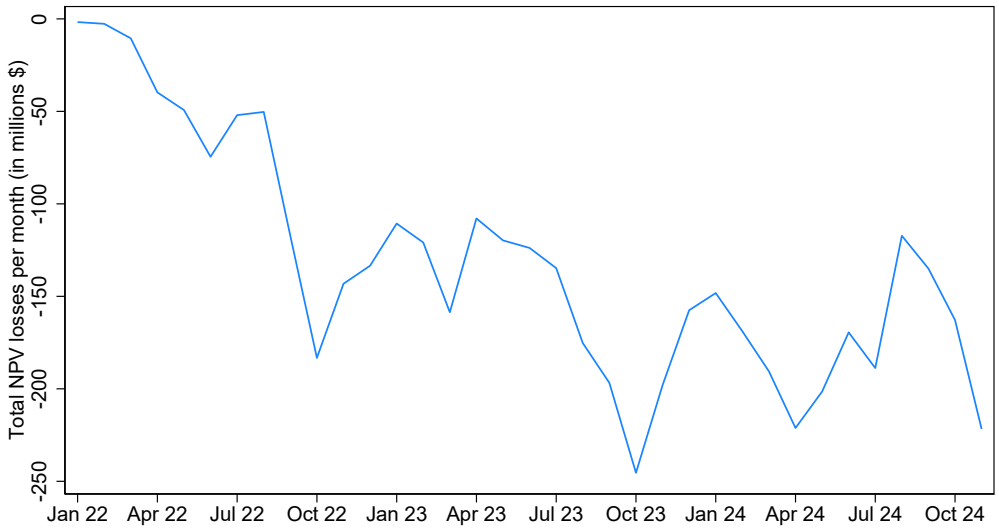
(f) NPV 2022-2024



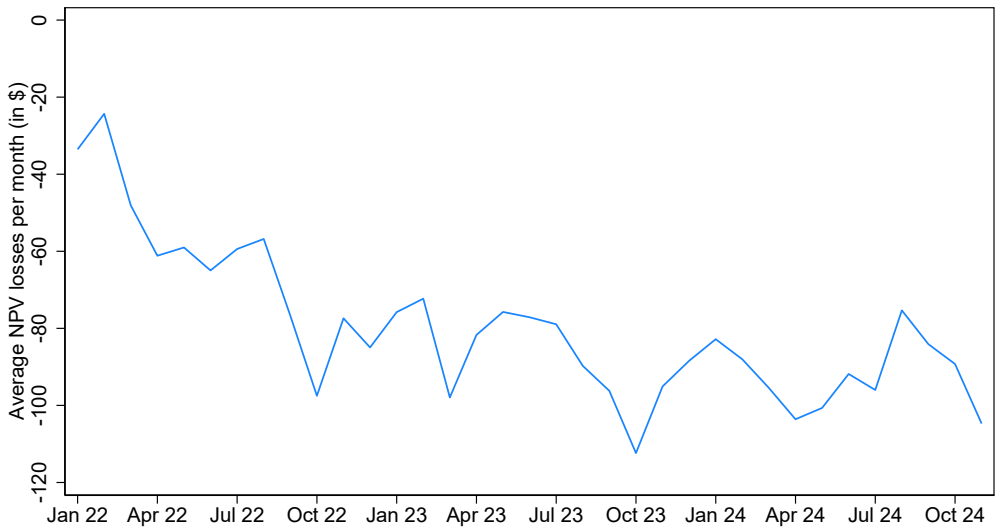
Note: The top row shows the distribution of curtailment amounts in dollar values. The middle row shows the distribution of curtailment amounts but scaled by the monthly payment. The bottom row shows the NPV of these payments. The left column is 2011-2021, and the right column is 2022-2024. NPV is calculated using tax-adjusted mortgage interest rates and tax-adjusted maturity-matched Treasury yields.

Figure 5: NPV of excess payments for negative interest rate spread mortgages

(a) Total NPV losses



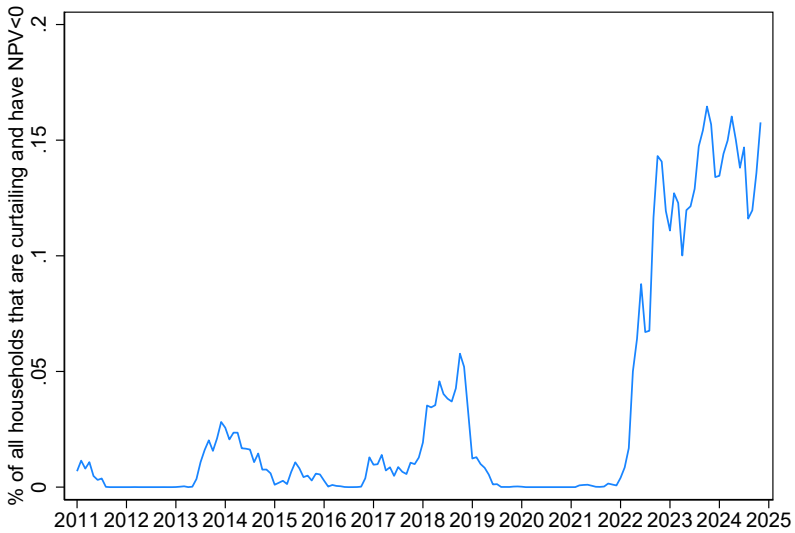
(b) Average NPV losses



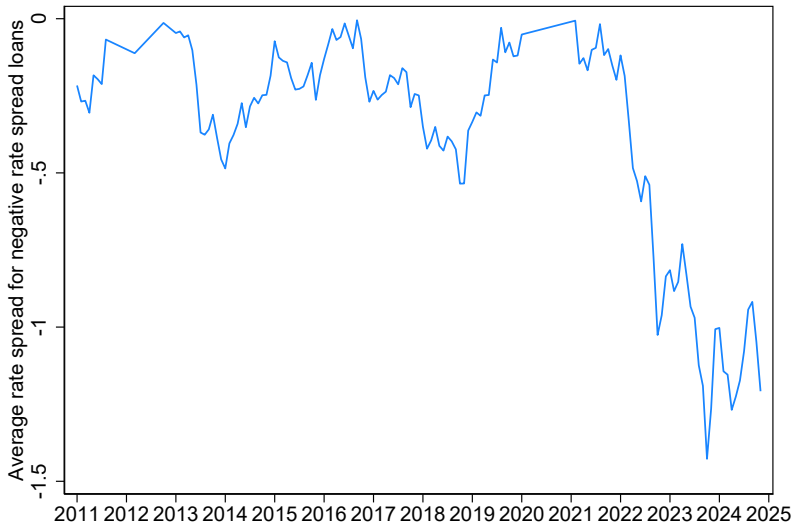
Note: Panel (a) shows the total NPV losses by per month for loans with negative interest rate spreads. Panel (b) shows the average NPV of these payments. NPV is calculated using tax-adjusted mortgage interest rates and tax-adjusted maturity-matched Treasury yields

Figure 6

(a) Share of negative rate spread households curtailing



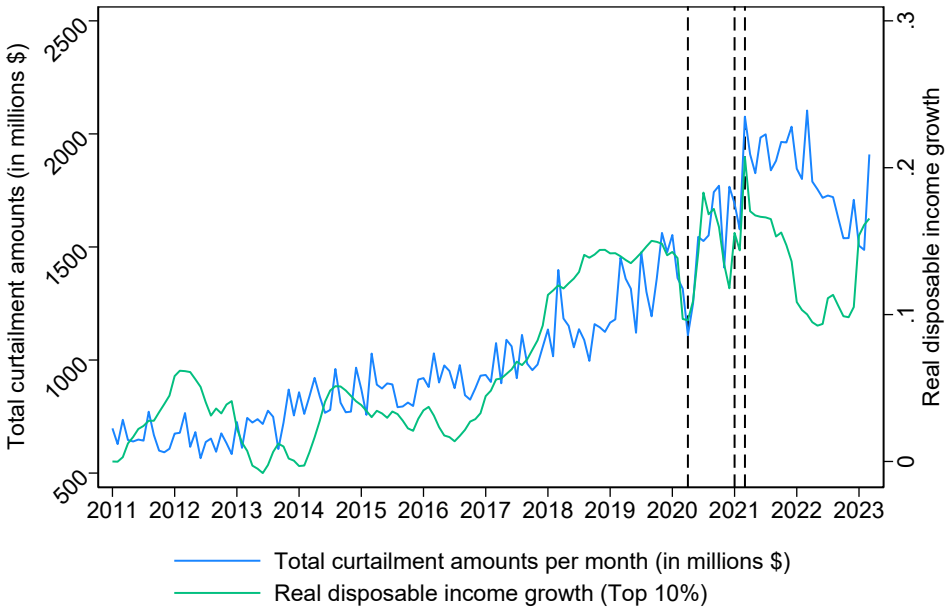
(b) Average negative rate spread (weighted by curtailment amount)



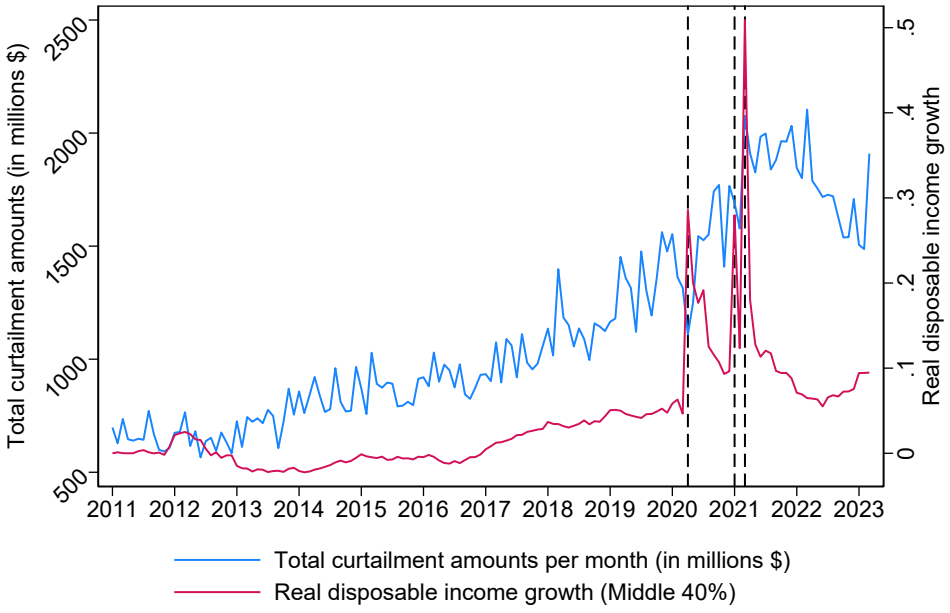
Note: Panel (a) shows share of all households that have a negative interest rate and who curtail by month. Panel (b) shows the average interest rate spread for negative rate spread loans, weighted by the dollar value of curtailment.

Figure 7: Total curtailment amounts and real disposable income growth

(a) Top 10%

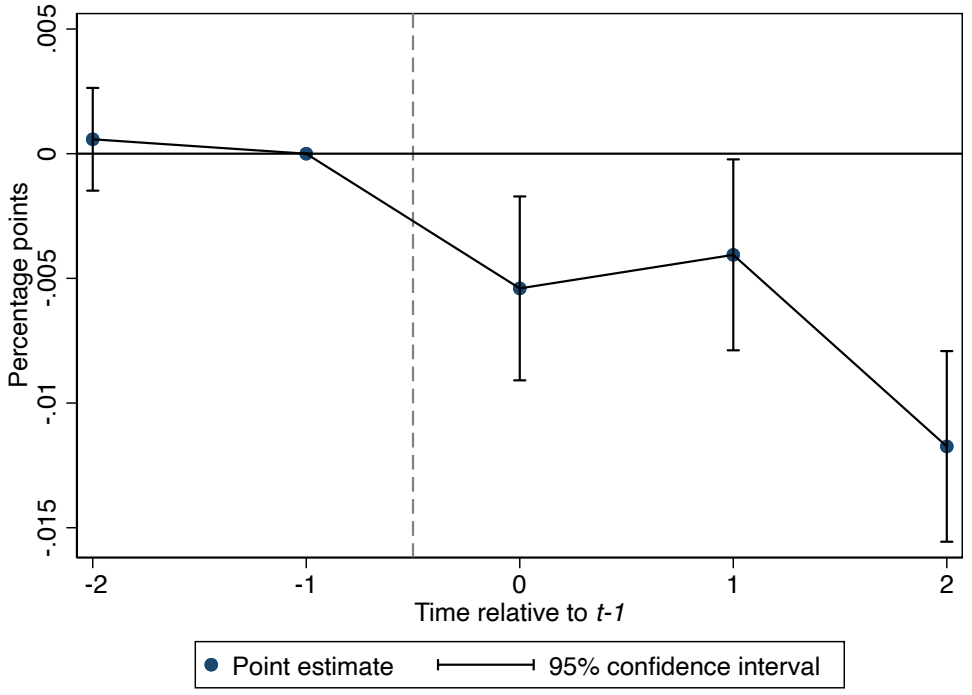


(b) Middle 40%



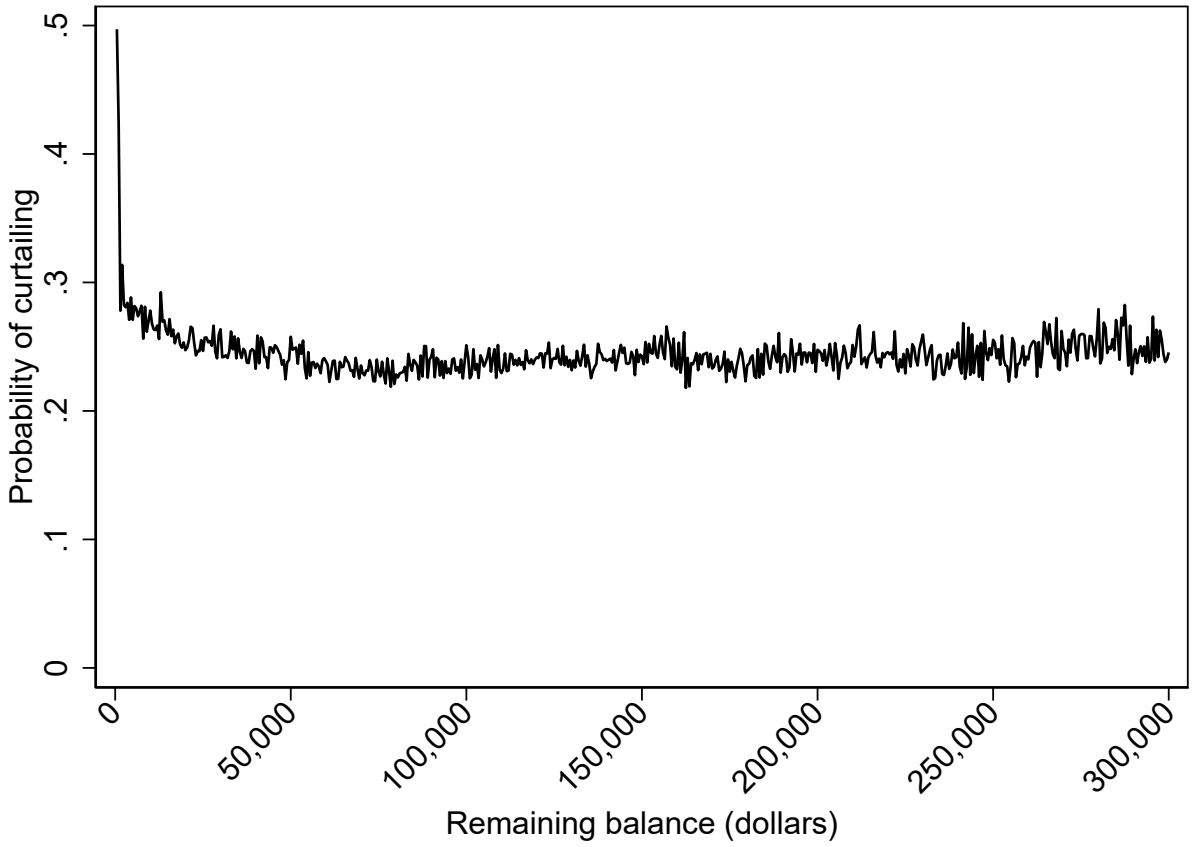
Note: Green and red lines are real disposable income growth for the top 10% and middle 40% of the income distribution, respectively. Blue line is total curtailment amounts per month in millions of dollars. Dashed lines represent months when COVID stimulus checks were sent (April 2020, January 2021, March 2021). Income data come from Realtime Inequality.

Figure 8: Probability of curtailment around interest rate changes



Note: Figure plots estimates of β_k's from equation (8). Authors' calculations based on Freddie Mac Single-Family Non-Standard dataset.

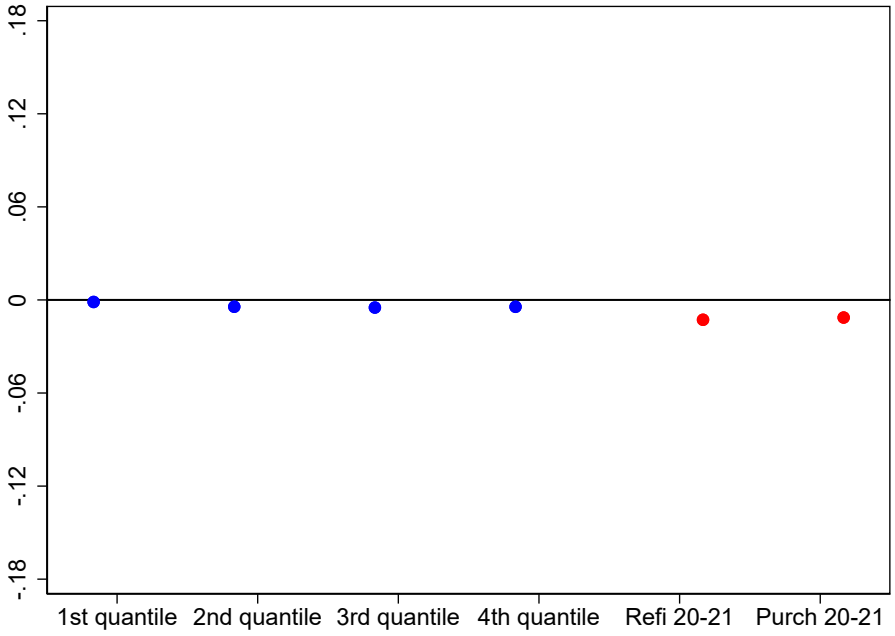
Figure 9: Probability of curtailing by remaining principal balance



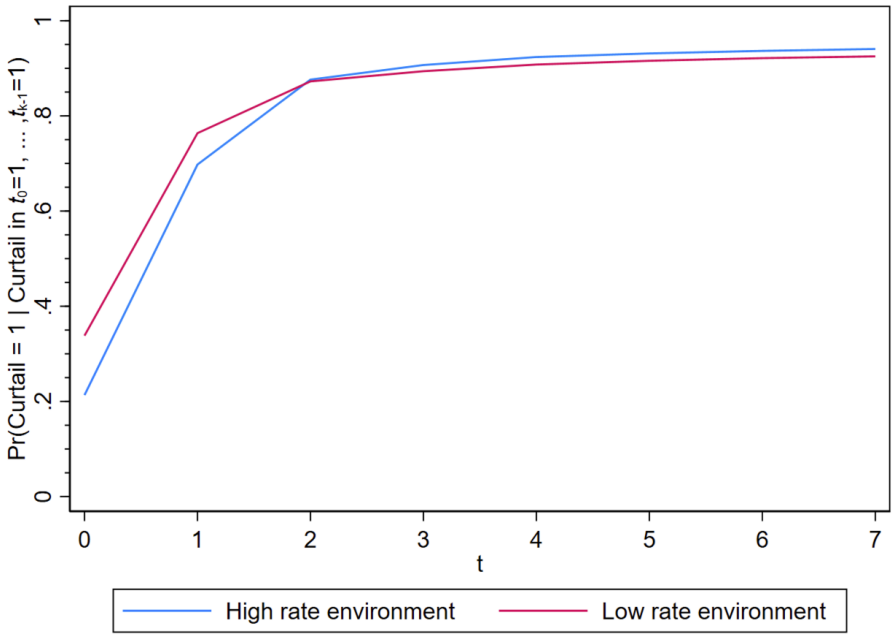
Note: Figure shows the likelihood of a loan curtailing by remaining principal balance. Balances are rounded to the nearest $500 and capped at $300,000. Authors' calculations based on the University of California Consumer Credit Panel (UC-CCP).

Figure 10: Sophistication, attentiveness and inertia

(a) Effects of interest rate spreads on curtailment by measures of attentiveness



(b) Conditional continuation probabilities by timing of first curtailment



Note: In Panel A, each dot represents the coefficient on $Spread$ from Equation (6) using different samples. The blue dots correspond to samples by quartiles of residuals from Equation (9). The red dots correspond to the 2020-2021 purchase and refinance samples. Panel B shows the probability of curtailing at time $t+k$ month conditional on curtailing in the k previous months. The blue line represents loans that first curtailed in a high-rate environment (March 2022-November 2024). The red line represents loans that first curtailed in a low-rate environment (Jan 2011-February 2022).Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset.

Table 1: Summary statistics

Panel A: All loans (Jan 2011 -Nov 2024)						
	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	220,101	127,033	217,338	124,026	220,740	127,710
Current balance	178,191	130,929	178,730	123,144	178,067	132,665
Term (in months)	306	83	293	88	309	95
Months to legal maturity	260	95	243	97	264	95
Effective months to maturity	251	100	219	103	259	98
Mortgage interest rate	4.07	1.01	3.93	0.99	4.10	1.02
Marginal tax rate (federal + state)	11.70	14.17	11.74	14.10	11.69	14.18
Marginal tax rate (mortgage interest)	11.21	12.49	10.79	12.42	11.30	12.51
Loan-month obs.	17,166,690		3,227,051		13,939,639	
Panel B: All loans (Jan 2022 - Nov 2024)						
	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	254,261	141,381	248,120	136,828	255,907	142,532
Current balance	220,539	142,181	205,486	137,893	224,572	143,039
Term (in months)	314	77	301	83	318	75
Months to legal maturity	266	91	251	94	271	90
Effective months to maturity	257	96	228	101	265	94
Mortgage interest rate	3.62	1.08	3.49	0.99	3.66	1.10
Marginal tax rate (federal + state)	11.99	13.65	12.01	13.62	11.98	13.65
Marginal tax rate (mortgage interest)	8.62	12.19	8.39	12.06	8.68	12.23
Loan-month obs.	4,621,147		976,476		3,644,671	
Panel C: Negative rate spread loans (Jan 2022 - Nov 2024)						
	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	278,811	145,307	268,048	139,589	281,903	146,762
Current balance	241,020	145,572	222,232	139,814	246,416	146,740
Term (in months)	301	83	287	88	305	82
Months to legal maturity	258	94	242	96	263	92
Effective months to maturity	251	98	224	101	258	95
Mortgage interest rate	3.09	0.60	3.04	0.59	3.10	0.60
Marginal tax rate (federal + state)	7.91	12.85	8.11	12.84	7.85	12.85
Marginal tax rate (mortgage interest)	11.89	13.27	11.33	13.09	12.06	13.31
Loan-month obs.	2,391,713		533,679		1,858,034	

Note: Authors' calculations based Fannie Mae Single-Family Loan Performance, 2010-2019 Surveys of Consumer Finances, and NBER Taxsim. Each cell is a loan-month observation. Marginal tax rate with respect to taxpayer earnings are set to 0 for households with an available retirement account.

Table 2: Results under different assumptions

	Share with spread<0	Total NPV (in millions $)	Average NPV (in $)
Benchmark results	.52	-4,632.35	-86.80
No available retirement account	.39	-2,217.65	-53.60
No tax adjustments	.59	-6,263.63	-102.25
MBS	.69	-8,411.53	-119.55

Note: Column (1) reports the share of loans with a negative interest rate spread for a given set of assumptions about taxes and opportunity costs. Column (2) reports the aggregate NPV losses in millions of dollars for these loans. Column (3) reports the average NPV loss per payment for these loans, conditional on paying in excess of the monthly minimum. The first row is the benchmark results. In the second row, we assume households do not have a tax-deferred retirement account available. Row three shows results when we do no tax adjustments. Fourth row uses post-tax mortgage interest rates but mortgage-backed securities as the discount rate. Estimates from Jan 2022 - Nov 2024. See text for more details.

Table 3: Curtailment heterogeneity

	$\mathbb{P}$(Spread<0)	$\mathbb{P}$(Curtail|Spread<0)	Notional value	NPV
All	0.52	0.22	$555.98	-$86.80
Subgroup				
High Credit Score	0.56	0.24	$617.70	-$95.59
Low Credit Score	0.47	0.20	$464.53	-$73.78
First-time Home-buyer	0.42	0.19	$581.59	-$103.26
Not First-time Home-buyer	0.54	0.23	$552.36	-$84.48
Urban	0.52	0.22	$565.54	-$88.77
Rural	0.47	0.23	$457.12	-$66.43
Originated 2020-2021	0.70	0.22	$519.26	-$92.57

Note: Column (1) reports the likelihood of having a negative interest rate spread. Column (2) reports the probability of curtailing conditional on having a negative interest rate spread. Column (3) reports the average notional value of curtailment for negative rate spread loans. Column (4) represents the NPV of these payments. Each row reports estimates for a specific subgroup. “High" and “low" credit score loans are those with above- and below- median credit scores (773), respectively. Urban is defined by residing in an MSA. Estimates from Jan 2022 - Nov 2024.

Table 4: Predicting curtailment from credit behavior

	(1)	(2)	(3)	(4)
Credit score			0.0477***	0.114***
			(0.000482)	(0.000787)
Total mortgage balance (thousands)			0.00308***	0.00406***
			(0.000141)	(0.000255)
Has a credit card			−5.187***	−8.665***
			(0.119)	(0.143)
Total credit card balance (thousands)			−0.0785***	−0.0971***
			(0.00381)	(0.00490)
Individual FEs	✓	✓		✓
Time FEs		✓		
R^2	0.255	0.376	0.0107	0.272

Note: Each column includes 1.1 million borrower-quarter observations. Columns (1) and (2) include no covariates. The dependent variable is an indicator equal to 100 if a household engages in curtailment, so the coefficients can be interpreted as a percentage point increase in the likelihood of curtailment. Mean of the dependent variable is 21.2. Authors' calculations based on the University of California Consumer Credit Panel (UC-CCP). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Effects of interest rate spread on the probability of curtailing

	All			High Credit Score			Low Credit Score		
Spread	-0.01***	-0.00***	-0.01***	-0.01***	-0.00***	-0.01***	-0.01***	0.00***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Loan FEs		✓			✓			✓	
Month FEs			✓			✓			✓
$\bar{Y}$	0.19	0.19	0.19	0.21	0.21	0.21	0.17	0.17	0.17

Note: Table reports estimates from the linear probability model in Equation (6). "High" and "low" credit score loans are those with above- and below- median credit scores (773), respectively. $\bar{Y}$ represents the baseline probability of curtailing for each group. Interest rate spread is calculated using post-tax mortgage interest rate and post-tax maturity-matched Treasury yields. $N = 17,166,690$ in Columns (1) and (3). $N = 17,164,564$ in Column (2). $N = 8,571,442$ in Columns (4) and (6). $N = 8,570,528$ in Column (5). $N = 8,595,248$ in Columns (7) and (9). $N = 8,549,036$ in Column (8). Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset, FRED, 2010-2019 Surveys of Consumer Finances and NBER Taxsim. Estimates are from Jan 2011 - Nov 2024. Standard errors (in parentheses) are clustered at the loan-level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: ARMs vs FRMs

	(1) Mortgage Rate	(2) $\mathbb{P}$(Curtail)	(3) 20-100	(4) 100-500	(5) >=500
$\mathbb{1}(ARM)\times$ LIBOR	0.644***	-0.019***	-0.009***	-0.009***	-0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	12,439,344	12,439,344	12,439,344	12,439,344	12,439,344
Loan FE	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Mean of outcome	4.26	0.18	0.07	0.08	0.04
R^2	0.97	0.52	0.45	0.46	0.38

Note: LIBOR is the twelve-month LIBOR rate. $\mathbb{1}$(ARM) is an indicator equal to 1 if the loan is an adjustable-rate mortgage and 0 if it is a fixed-rate mortgage. The dependent variable in Column 1 is the loan's interest rate. The dependent variable in Column 2 is an indicator equal to 1 if the loan curtails in that month. Columns 3-5 are indicators equal to 1 if the curtailment amount is in $(20, 100)$, $[100, 500)$, or ≥ 500 dollars, respectively. Standard errors (in parentheses) are clustered at the loan-level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Effects of interest rate variation: Flexible rate vs “locked-in" loans

	(1) Mortgage Rate	(2) $\mathbb{P}$(Curtail)	(3) 20-100	(4) 100-500	(5) $\geq$500
$\mathbb{1}$(Flexible rate)	-2.064***	0.058***	0.019***	0.031***	0.007***
	(0.015)	(0.004)	(0.003)	(0.003)	(0.002)
LIBOR $\times\mathbb{1}$(Flexible rate)	0.759***	-0.031***	-0.015***	-0.012***	-0.003**
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	3,079,361	3,079,361	3,079,361	3,079,361	3,079,361
Loan FE	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Mean of outcome	3.51	0.18	0.06	0.07	0.06
R^2	0.84	0.57	0.42	0.48	0.50

Note: LIBOR is the twelve-month LIBOR rate. $\mathbb{1}$(Flexible rate) is an indicator equal to 1 if the loan is not in its introductory rate period. The dependent variable in Column 1 is the loan's interest rate. The dependent variable in Column 2 is an indicator equal to 1 if the loan curtails in that month. Columns 3-5 are indicators equal to 1 if the curtailment amount is in $(20, 100)$, $[100, 500)$, or ≥ 500 dollars, respectively. Standard errors (in parentheses) are clustered at the loan level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix

A Detailed Data Construction

A.1 Identifying early payments

Our main sample is the Fannie Mae Single-Family Loan Performance dataset, a monthly panel of conventional loans purchased by Fannie Mae. We observe the mortgage's interest rate, term, balance at origination (rounded to the nearest thousand) and current unpaid principal balance (the exact dollar amount). We do not observe a loan's monthly minimum payment in the data, but we are able to calculate an estimated payment using the monthly interest rate, the original loan principal, and the term of the loan. Specifically, for a given principal P, term (in months) N, and monthly interest rate r_m, we calculate monthly payment MP as

$$MP = \frac{P \times r_m}{1-(1+r_m)^{-N}} \tag{10}$$

The share of each monthly payment that goes toward interest in a given month is equal to the monthly interest rate r_m times the remaining unpaid principal; the share that is applied toward principal is simply the monthly payment minus that share.

We observe the outstanding principal balance each month for a given mortgage. Using the outstanding balance in month t, we calculate the *expected* outstanding balance in month $t+1$ by subtracting the principal share of the monthly payment from the outstanding balance in month t, i.e. $\mathbb{E}[B_{t+1}] = B_t - (1-r_m) \cdot MP$. If the difference between the observed balance and the expected balance is negative, the household has paid in excess of its monthly minimum. We define curtailment to be excess payments greater than \$20 to account for measurement error.

It is not always the case that a household paying in excess of its monthly minimum is curtailment. Some households pay extra, but do not specify that the excess cash should be put

toward the remaining principal. Instead, the excess payment is used a future payment. This behavior does not reduce the overall interest burden nor does it shorten the effective maturity date; we do not consider this curtailment, and so we remove the flag for these observations. To do so, we start by flagging all households who pay in excess of $20 or more in a given month. Then, we remove the flag if we observe that the household "underpaid" by a near-symmetric amount in the next three months. Consider a month in which we observe a household with a $500 monthly payment, $450 of which goes toward principal. Suppose this households is flagged as paying an excess of $50. In the following month, we observe that the household has underpaid by $50, meaning the difference between expected and actual remaining principal is -50. This means the household did not originally curtail, but rather earmarked the extra cash to go to a future payment.

Lastly, we do not consider any excess payment in the final month of a loan's term to be a curtailment. This is because we are unable to distinguish if the loan has matured or refinanced. Because of this, the estimated probability of curtailing for very small (less than $2,000 or so) remaining balances is mechanically attenuated. This is not because households are less likely to curtail with small remaining balances – the exact opposite is true (see Figure 9). Figure A3 shows the probability of curtailing against remaining balance using the Fannie Mae data. We see the exact pattern as in the credit registry data, but for very small balances, the estimated probabilities are artificially low.

A.2 Tax data

We use the 2010-2019 Surveys of Consumer Finances (SCF) and the NBER TAXSIM program to calculate marginal tax rates, using the approach established by Gale et al. (2022). Our approach is as follows.

First, since the public use SCF does not include respondents' geographic information, we randomly assign households to states. Specifically, we make the probability of being assigned to a state proportional to each state's population; for example, since California's population

is about 1/9 of the US population as a whole, each household has a 1/9 probability of being assigned to California.

Second, we restrict the set of SCF respondents to closely resemble the households we observe in the Fannie Mae data set. That is, we remove ARMs, mortgages originated before 1999, etc. Using this subset, we calculate the federal, state, and mortgage interest marginal tax rates for each respondent in the SCF. Using TAXSIM, we then calculate the effective federal and state marginal tax rates with respect to taxpayer earnings for each respondent in the SCF. We also calculate the implicit marginal tax rate with respect to mortgage interest by adding $100 to each households' mortgage interest expense; the difference between the baseline tax liability and the new tax liability after adding the $100 is the implicit marginal tax rate on mortgage interest.

Lastly, since the SCF is a triennial survey, we apply the results for each year to the next two subsequent years. That is, the rates calculated for 2010 will be the same in 2011 and 2012. Tax rates calculated using the 2019 SCF are used for 2019-2023. Within each state-year-origination balance bin[16], we randomly match SCF respondents' tax rates to loans in the Fannie Mae dataset.

For our benchmark assumptions, we assign federal and state marginal tax rates to 0 for respondents that indicate they have some sort of retirement account.

B Alaska Permanent Dividend Fund

To empirically test if curtailment is driven by positive income shocks, we leverage eligibility and timing in dividend payouts from the Alaska Permanent Fund. Established in 1976, the fund pays yearly dividends to eligible Alaska residents. The dividend's nominal value reached a low of $331 in 1984, but has consistently surpassed $1,000 since 1996, reaching its highest point of $3,284 in 2022 (Jones and Marinescu, 2022).[17] All Alaska residents that have lived in

[16]Mortgage origination balances are rounded to the nearest $250,000 and capped at $1 million.

[17]https://pfd.alaska.gov/Division-Info/summary-of-dividend-applications-payments

the state for at least a full calendar year who intend to remain residents indefinitely are eligible for the dividend. We cannot observe if the loans in Alaska are held by eligible residents, but we use the state identifier as a proxy for eligibility. In other words, if a loan is coded as being in Alaska, we consider this household to be eligible, and households in all other states are ineligible. Dividends are distributed in October of each year. If curtailment is linked to income shocks, then we should see an increase in curtailment for Alaska households in October. To test this, we estimate the following linear probability model:

$$Y_{it} = \beta AK_i \times OCT_t + \alpha_i + \gamma_t + \varepsilon_{it} \tag{11}$$

where AK_i is an indicator equal to 1 if loan i is in Alaska, and OCT_t is an indicator equal to 1 if the calendar month is October. We include loan and month fixed effects (α_i and γ_t, respectively). Depending on the specification, Y is either a curtailment indicator or an indicator for curtailment amounts.

Estimates are reported in Table A1. The dependent variable in Column 1 is a curtailment indicator. The estimates imply that Alaska residents are nearly a full percentage point more likely to curtail in October on average. Relative to the sample mean, this is a roughly 5% increased likelihood. In Columns 2-4, we use indicators for curtailment amount as the dependent variable ($20-100, $100-500, and $\geq$ $500). While the coefficients are not statistically different from 0 at the 5% level of significance, the positive sign on each coefficient suggests that Alaska residents are using the excess income to repay their mortgages.

As a placebo test, we simulate eligibility and re-estimate (11) 50 times using indicators for all other states (including DC). We plot the distribution of the 51 $\widehat{\beta}$'s in Figure A4, Panel A and the associated t-statistics in Panel B. In Panel A, the blue dot is the coefficient from the regression using the Alaska indicator, and the red dots are the coefficients from all other states. The coefficient on the interaction using the Alaska indicator is by far the largest, nearly a full percentage point. The coefficients from the simulations are largely centered around 0. In Panel

B, we see that the estimated coefficient for Alaska is also the most statistically different from 0. Again, the blue dot depicts Alaska, and the red dots depict all other states. The dashed vertical lines represent -1.96 and 1.96, the critical values for 5% significance. With a 95% confidence level and 51 simulations, we expect to see about 3 false positives; we find 5 other states with statistically significant $\widehat{\beta}$'s. Alaska's t-statistic is also the largest. We also simulate eligibility timing and re-estimate (11) 11 times, using all other calendar month indicators, and plot the distribution of the 12 $\widehat{\beta}$'s in Panel C. The figure shows that October is associated with the largest increase in the likelihood of curtailment, with November being the second. These are the only two statistically significant coefficients, as well. In sum, both the main analysis and placebo tests show that the dividend does indeed induce curtailment, suggesting that households curtail based on available cash flows.

C Inertia

One possibility is that households begin curtailing in a low interest rate environment when curtailment was beneficial; when rates rise, they fail to reoptimize because curtailing was a sound investment previously, so inattention leads them to believe curtailing remains financially wise. This theory is motivated by the fact that the probability of a household curtailing in month t given curtailing in month $t-1$ is about 80%. To study this, we identify households whose first curtailment occurred in a low-rate environment (January 2011 - February 2022) and those whose first curtailment occurred in a high-rate environment (March 2022 - November 2024). We remove likely "autopayers" – loans that pay the exact same amount each month for 12 consecutive months – to capture *active* decisions to curtail. We then calculate the conditional probability of curtailing at month $t+k$ given the household has curtailed in the previous k periods for each household type. In other words, we first calculate the unconditional probability of curtailing, then the probability of curtailing conditional on doing so last month, then the probability of curtailing conditional on doing so the last two months, and so on.

Results are shown in Figure 10b. The blue line represents loans that first curtailed in a high rate environment, and the red line represents loans that first curtailed in a low rate environment. While the unconditional probabilities differ substantially ($\sim$ 20% vs $\sim$35%) across the two groups, by just two consecutive months later, both groups' conditional probabilities hover asymptotically at about 90%. These results show that households do exhibit persistence in their decisions to partially prepay, but this persistence does not correlate with the interest rate environment when they first began curtailing.

Additional Figures and Tables

Figure A1: Data dictionary

Fannie Mae variables

Name	Description
Mortgage interest rate (r_m)	Pre-tax mortgage interest rate
Origination principal balance	Principal balance of the loan at origination (rounded to the nearest $1000)
Current principal balance	Remaining principal balance in month *t*
Term	Length (in months) of the loan
Months to legal maturity	Number of months remaining until the loan is due to be paid in full as defined by the contract
Months to maturity	Number of months remaining until the outstanding principal balance of the mortgage amortizes to a zero balance after considering early payments

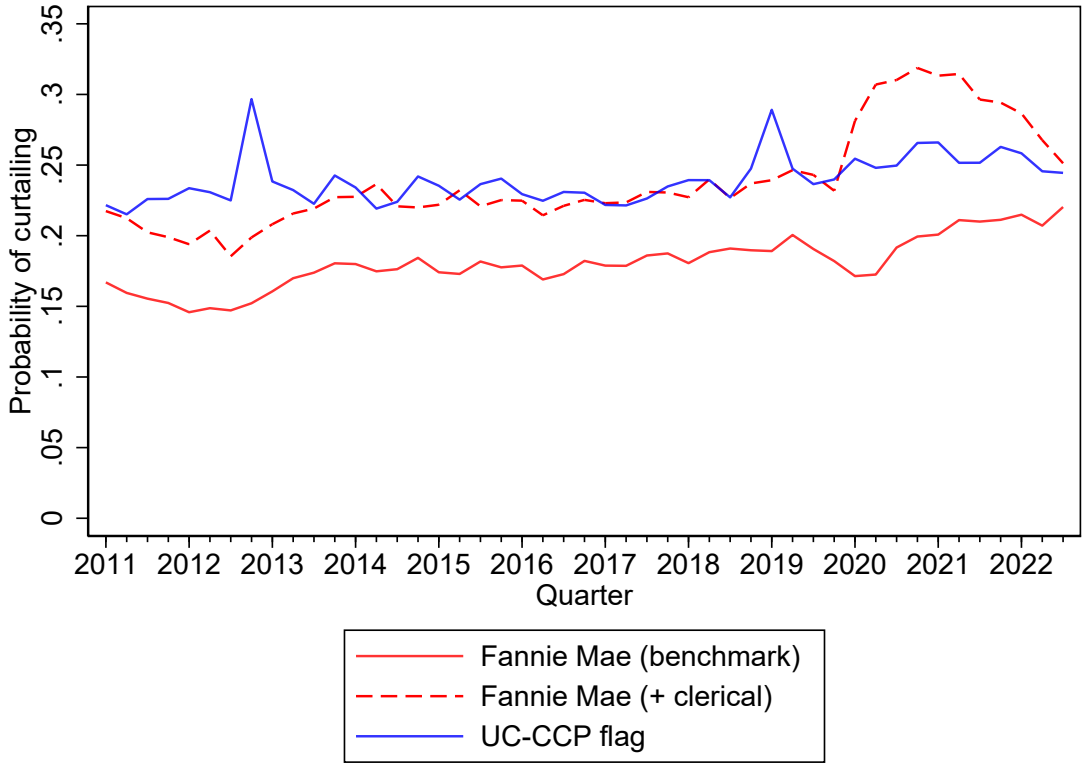
TAXSIM variables

Name	Description
Federal marginal tax rate	The estimated federal marginal tax rate on taxpayer earnings
State marginal tax rate	The estimated state marginal tax rate on taxpayer earnings

Constructed variables

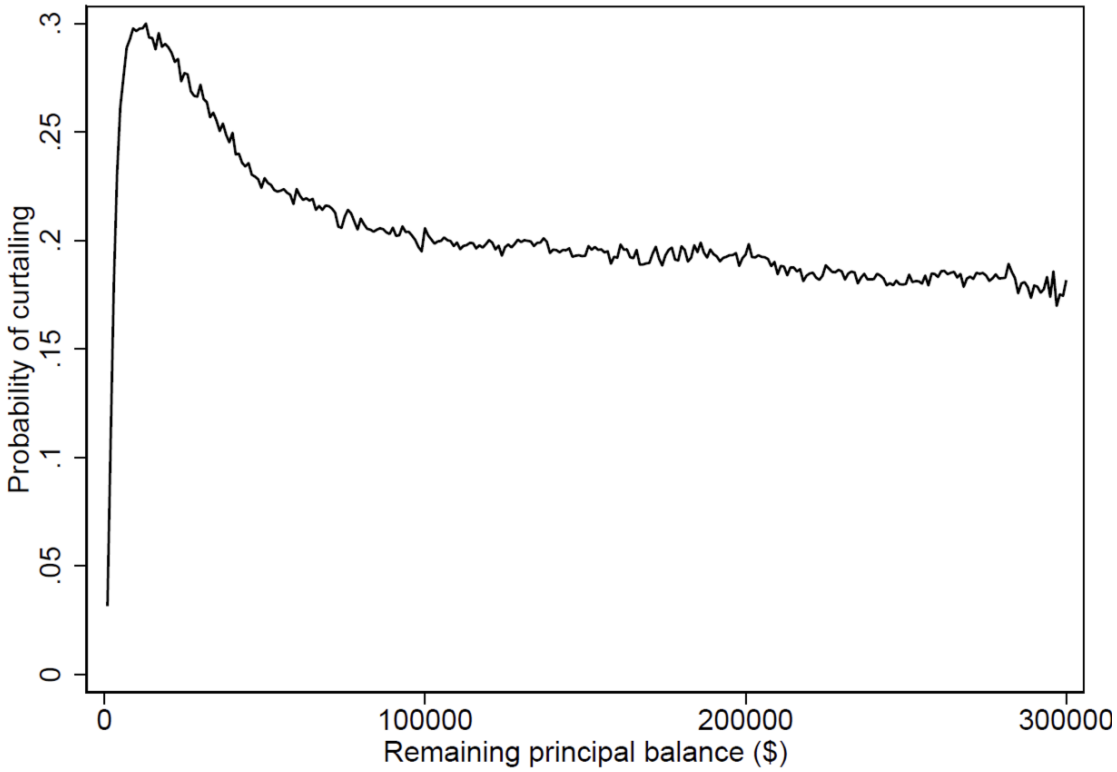
Name	Description
Monthly payment (*MP*)	The estimated monthly payment of the mortgage
Principal share of MP	The amount of the estimated monthly payment that is applied toward remaining principal each month
Expected principal balance	How much of the outstanding principal balance should remain in month *t* if the household pays exactly the monthly payment in month *t-1*
Curtailment amount	Expected principal balance - current (observed) principal balance
Curtailment flag	Indicator equal to 1 if the curtailment amount > $20

Figure A2: Credit Registry vs. Loan Performance Data



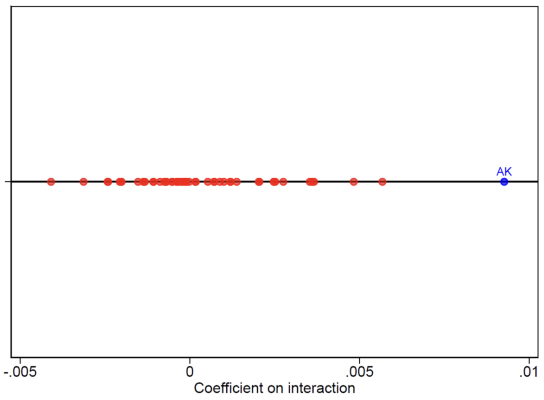
Note: Figure plots the probability of curtailing by quarter from 2011Q3 - 2023Q1. The red lines represent Fannie Mae Single-Family Loan Performance data. The solid red line uses the benchmark curtailment flag and the dashed red line uses the benchmark flag + clerical curtailment. The blue line represents the UC-CCP data.

Figure A3: Probability of curtailing by remaining balance

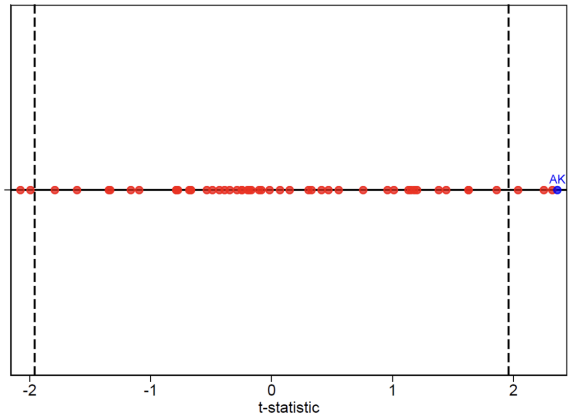


Note: Figure plots the probability of curtailing by remaining principal balance rounded to the nearest $1,000. Authors' calculations based on the Fannie Mae Single Family Loan Performance dataset.

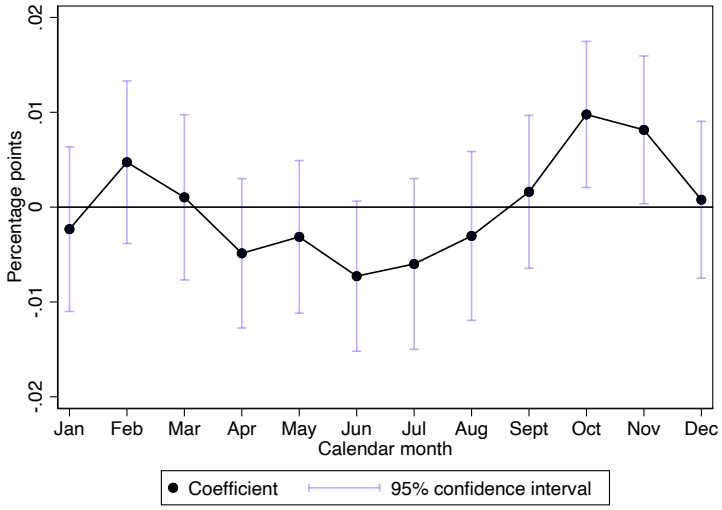
Figure A4: Effects of Alaska Permanent Fund Dividend on Curtailment



(a) Distribution of estimated coefficients on interaction term for all states



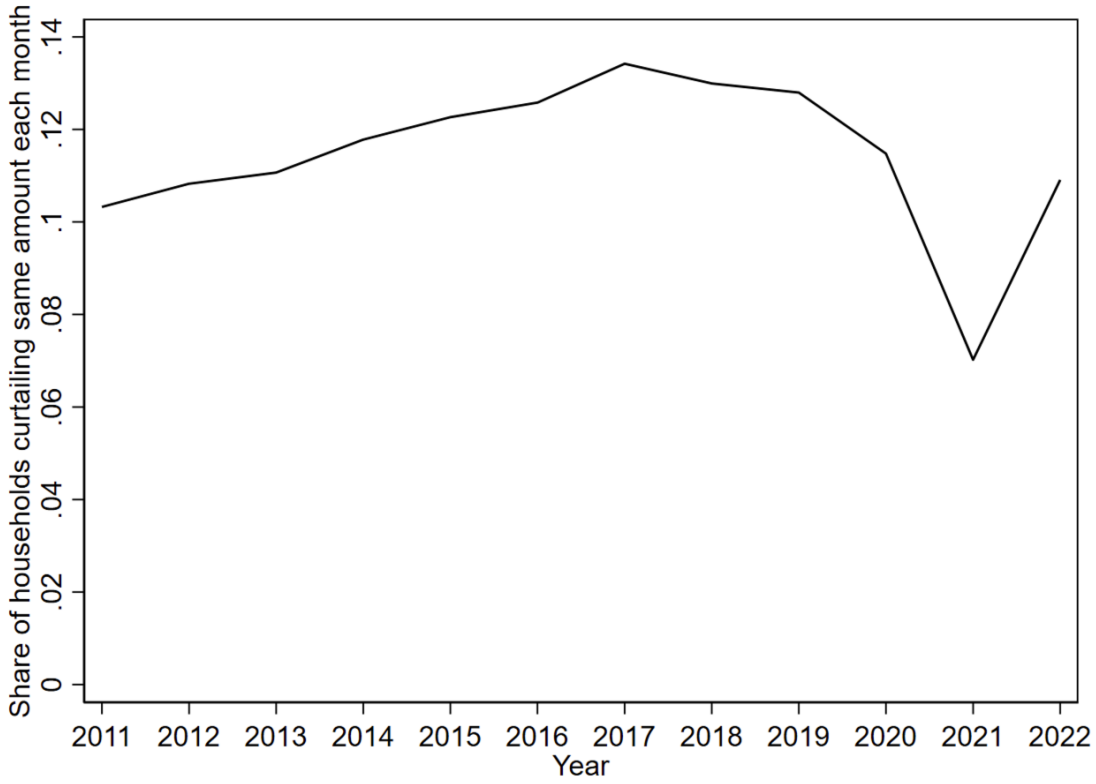
(b) Distribution of t-statistics



(c) Coefficients on interaction term by calendar month

Note: Panel (a) plots the point estimates from (11) from all 51 regressions. Panel (b) plots the associated t-statistics. Panel (c) again plots the point estimates from (11) but for all 12 calendar months; coefficients are black and 95% confidence intervals are light blue. Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset.

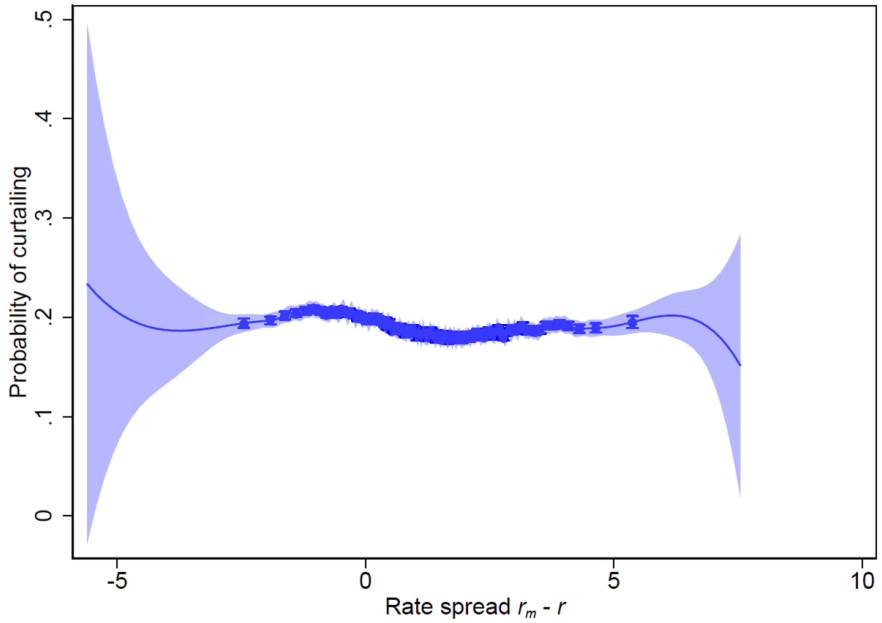
Figure A5: Share of households curtailing same amount each month



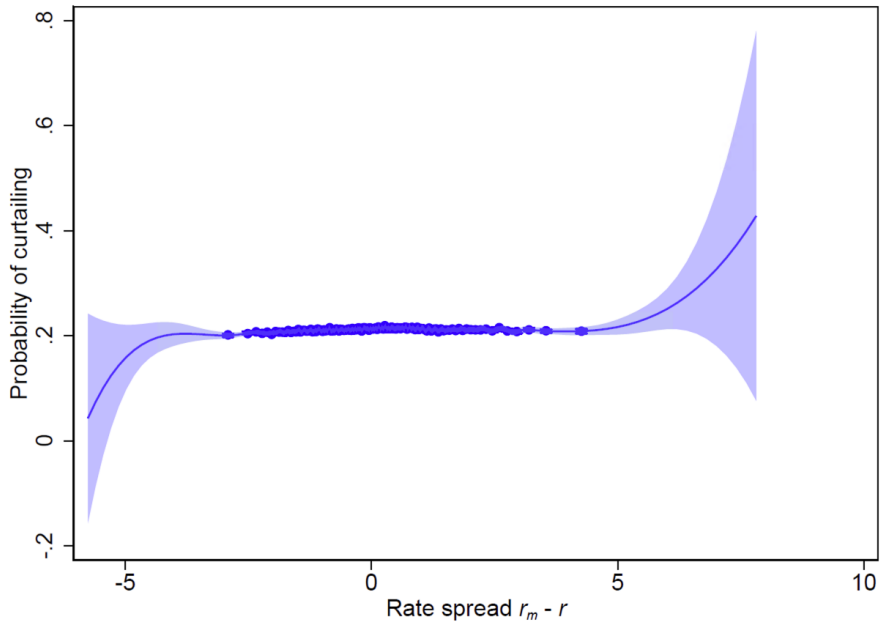
Note: Figure shows the share of households curtailing the exact amount each month by calendar year. Curtailment amounts are rounded to the nearest $5. Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset.

Figure A6: Effects of interest rate spreads on curtailment: binscatter approach

(a) 2011-2024



(b) 2022-2024



Note: Both panels show binscatter estimates of Equation 6 controlling for loan fixed effects using Cattaneo et al. (2024) estimator. Panel A uses observations from 2011-2024, and Panel B only uses observations from 2022-2024. In both specifications, we estimate a third-degree polynomial with three smoothness constraints. Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset.

Table A1: Effects of the Alaska Permanent Fund Dividends on Curtailment

	$\mathbb{1}(Curtail)$	20-100	100-500	≥ 500
$AK_i \times OCT_t$	.009*	0.001	0.006	0.003
	(0.004)	(0.003)	(0.003)	(0.002)
Observations	22,002,642	22,002,642	22,002,642	22,002,642
Loan FEs	✓	✓	✓	✓
Month FEs	✓	✓	✓	✓
Mean of outcome	.17	0.061	0.08	0.03
R^2	.47	.41	.41	.34

Note: This table reports estimates from equation (11). The dependent variable in Column 1 is a curtailment indicator. The dependent variables in Columns 3-5 are indicators for curtailment amount in $(20, 100)$, $[100, 500)$, or ≥ 500 dollars, respectively. Standard errors (reported in parentheses) are clustered at the loan level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: Effects of interest rate spread on the probability of curtailing

	(1)	(2)	(3)	(4)	(5)
Spread	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
1-year lag		-0.00***	-0.00***	-0.00***	-0.00***
		(0.00)	(0.00)	(0.00)	(0.00)
2-year lag			0.00***	0.00***	0.00***
			(0.00)	(0.00)	(0.00)
3-year lag				0.00***	0.00***
				(0.00)	(0.00)
4-year lag					0.00***
					(0.00)
Loan FEs	✓	✓	✓	✓	✓
Number of lags	0	1	2	3	4
Loan-month obs	17,614,564	13,044,558	9,637,158	6,914,454	4,889,247

Note: Table reports estimates from the linear probability model in Equation (6). Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset, FRED, 2010-2019 Surveys of Consumer Finances and NBER Taxsim. Estimates are from Jan 2011 - Nov 2024. Standard errors (in parentheses) are clustered at the loan-level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Effects of interest rate spread on the probability of curtailing: No autopayers

	All			High Credit Score			Low Credit Score		
Spread	-0.01***	-0.01***	-0.02***	-0.01***	-0.01***	-0.02***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Loan FEs		✓			✓			✓	
Month FEs			✓			✓			✓
$\bar{Y}$	0.28	0.28	0.28	0.30	0.30	0.21	0.26	0.26	0.26

Note: Table reports estimates from the linear probability model in Equation (6). "High" and "low" credit score loans are those with above- and below- median credit scores (773), respectively. $\bar{Y}$ represents the baseline probability of curtailing for each group. Interest rate spread is calculated using post-tax mortgage interest rate and post-tax maturity-matched Treasury yields. $N = 8,567,137$ in Columns (1) and (3). $N = 8,497,184$ in Column (2). $N = 4,309,527$ in Columns (4) and (6). $N = 4,278,426$ in Column (5). $N = 4,257,610$ in Columns (7) and (9). $N = 4,218,698$ in Column (8). Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset, FRED, 2010-2019 Surveys of Consumer Finances and NBER Taxsim. Estimates are from Jan 2011 - Nov 2024. Standard errors (in parentheses) are clustered at the loan-level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$