Understanding Excess Repayment

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Abstract

Twenty-two percent of U.S. households partially prepay their mortgage each month, a practice known as curtailment in the mortgage industry. For mortgages with interest rates below the risk-free rate, curtailment has negative net present value. We show that interest rate increases in 2022 led to $1.2 billion in curtailment losses from January 2022-February 2023, due to the rising share of mortgages with a negative rate spread. Curtailment is more frequent among households with less credit card debt or those who have no credit card at all and is correlated to the availability of disposable income. Interest rate increases reduce curtailment for adjustable-rate but not fixed-rate mortgages. Our findings suggest that curtailment is explained by both an aversion to debt and by household responses to changes in current cash flows.

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1 Introduction

Every month, more than one in five U.S. mortgage borrowers voluntarily pay more than their required minimum mortgage payment. While such prepayments can be financially sensible if the mortgage interest rate is high, they become puzzling, and financially costly, when borrowers continue to prepay mortgages with interest rates that are locked significantly below prevailing safe investment returns. Such borrowers would achieve greater wealth and pay off their mortgage faster by investing their additional funds elsewhere instead of accelerating repayment. We calculate that this widespread practice led households to incur roughly $1.2 billion in losses from January 2022 through February 2023 alone.

This paper documents two closely related puzzles. First, despite the clear negative net present value (NPV) of prepaying mortgages with interest rates below the prevailing risk-free rate, we show that a substantial share of borrowers persistently engage in such costly excess repayment (also known as *curtailment*). Second, we find limited responsiveness of prepayment behavior to interest rate changes: even when the financial disadvantage of prepayment grows due to rising market rates, few households adjust their repayment decisions. We explain these puzzles by introducing a model of debt aversion, demonstrating that an inherent dislike of holding debt can rationalize households' seemingly suboptimal financial choices, even in the face of significant monetary losses.

To understand why prepayment generates losses when the mortgage rate is below the risk-free rate, consider how mortgage repayments work. Excess repayments reduce the size of the final scheduled payment. If the mortgage rate exceeds the rate borrowers could earn from safe, alternative investments (such as Treasury bonds), excess repayment generates a positive net present value (NPV). However, when mortgage rates are fixed below prevailing market rates, as has happened since 2022, households would earn a higher return investing their funds and earning a market return. In other words, by increasing the opportunity cost of capital, higher discount rates reduce the present value of prepaying the distant final payment.

Our main results are illustrated in Figure 1. Rising interest rates in 2022 raised the share

of mortgages with a negative interest rate spread, but did not decrease curtailment of these mortgages. Panel A shows that 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 to well above the median mortgage rate. Quarterly mortgage curtailment is shown in Panel B. Prior to 2022, curtailment was widespread but since rates were low, nearly all mortgages being curtailed had a positive rate spread. After interest rates rose, an increasing number of mortgages being curtailed had a negative rate spread, with the share reaching nearly half by mid-2022. Yet there was no decrease in the overall share of mortgages being curtailed.

The net costs to households of negative-spread curtailment are substantial. The average monthly curtailment payment among households with a negative rate spread is $619, which has an average NPV of -$80 under our preferred assumptions. 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). 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 22% to 54%, and the the aggregate NPV of negative-spread curtailment varies from $430 million to over $2.4 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. Whereas refinancing "mistakes" are more common among households with lower credit scores, households with higher credit scores are more prone to negative-NPV curtailment.

Our reduced-form evidence reveals several patterns that contradict classical portfolio optimization. First, we find that borrowers' decisions to make excess mortgage repayments are highly persistent over time even as interest rates change. Households that pay more than the minimum in one month are significantly more likely to do so in subsequent months, and a substantial share of the variation in this behavior is explained by fixed, individual-specific factors.

Second, our tests indicate that these excess repayments are more closely linked to the immediate cash flows available to households than to changes in the net present value (NPV) of prepayment. This distinction parallels findings by Ganong and Noel (2020a) and Ganong and Noel (2020b) regarding mortgage modification and default behaviors. 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 provide further direct evidence by altering household disposable income without changing repayment net present value. Additional support comes from comparing locked-in ARMs versus non-locked ARMs and exploiting exogenous income changes from the Alaska Permanent Fund. Collectively, this evidence consistently indicates that households respond sharply to current cash flows but negligibly to the net present value implications of their mortgage prepayment decisions.

A third line of evidence comes from the timing of prepayments. We document a pronounced spike in excess repayments as the remaining mortgage balance declines. This pattern aligns with laboratory findings on debt attitudes (Meissner and Albrecht, 2022), suggesting that individuals derive extra psychological satisfaction from reducing their debt to near zero.

These findings underscore the insufficiency of the standard NPV approach in explaining observed mortgage prepayment behavior. In response, we introduce a model of debt aversion that supplements conventional portfolio-maximization frameworks. Our model posits that households incur a "burden" from carrying debt—an effect that leads them to allocate available disposable income toward reducing mortgage balances, even at substantial financial cost. This interpretation is bolstered by our observation that prepayment is 25–40% more likely among households without credit cards, and that a $10,000 increase in a household's credit card balance decreases the likelihood of excess repayment by roughly 5%. Existing research has found evidence for debt aversion in several consumer credit markets, including student loans

(Gopalan et al., 2021; Eckel et al., 2007), credit cards (Prelec and Loewenstein, 1998) and mortgages (Amromin et al., 2007). The setting of mortgage repayment is the most high-stakes and widespread test of debt aversion that we are aware of.

Our framework nests the conventional NPV approach but allows for the possibility that fixed, individual-specific preferences dominate marginal rate changes. Our estimates imply that some borrowers exhibit very strong debt aversion while others do not. As a consequence, even though the marginal financial benefit of prepayment may vary with interest rate changes, only a few borrowers adjust their behavior in response; thus the overall elasticity of curtailment with respect to rate spreads is small. The model can reconcile the observed insensitivity to interest rate changes with the fact that some households are extremely debt averse. Moreover, counterfactual experiments within the structural framework demonstrate that eliminating the debt-aversion channel — by, for instance, setting the individual fixed effect for borrowers with positive debt aversion to zero — would dramatically lower the observed curtailment rates. This exercise underscores that it is primarily the persistence of these fixed preferences that renders households unresponsive to changes in market rates.

The disconnect between borrowing behavior and expected asset returns is closely linked to other findings in household finance. A 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 assets 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 component.

Mortgage repayment decisions offer an ideal setting to test household portfolio theory and attentiveness. Changes in bond yields or CD rates alter the opportunity cost of paying down debt without affecting mortgage cash flows, providing a unique opportunity to isolate household sensitivity to economic incentives. We show, however, that households are remarkably unresponsive to changes in these opportunity costs. 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.

Our findings align with theories emphasizing sluggish responses to economic shocks, such as rational inattention or information stickiness (Sims, 2003; Reis, 2006). Yet benchmark models of debt aversion, which emphasize fixed psychological costs of holding debt (e.g., Gopalan et al. 2021), can explain why households make excess repayments in general, but cannot account for why households ignore shifts in opportunity costs. Even if households bear a fixed psychological burden from debt, at least some borrowers should be sensitive at the margin—reducing repayments when external returns improve. A rational inattention framework predicts that households with greater financial resources or sophistication would respond more strongly, since adjusting repayment decisions would be most valuable to them. However, we find precisely the opposite pattern: high-income, high-credit-score households—those most likely to be financially sophisticated—actually repay their mortgages at even higher rates. To reconcile this puzzling fact, our model introduces dispersion in debt aversion across households. Greater dispersion reduces the fraction of households at the margin between repayment and investment, muting the aggregate response to changes in interest rates and explaining the widespread financial inertia we observe.

Our findings on curtailment relate to existing work on financial decision-making for mortgages.[1] Economists have long studied the "mistakes" households make when shopping for a

[1]A handful of papers that have studied curtailment previously. Using self-reported data from the Survey of Consumer Finances (SCF), Amromin et al. (2007) and Adelman et al. (2010) explore the implications of mortgage curtailment payments. Amromin et al. (2007) focus on the role of taxes.[2] Adelman et al. (2010) examine factors that predict whether a mortgage borrower makes curtailment payments using the SCF. They find that propensity to

mortgage (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).

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). Inattentiveness and financial literacy possibly explain some financial "mistakes" (DellaVigna, 2009; Shah et al., 2015). 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.

save and liquidity risk have some predictive power while return on alternative investment opportunities does not. Both Amromin et al. (2007) and Adelman et al. (2010) examine mortgage data from the 1990s and early 2000s. Our findings span a period with a very different interest rate environment. Previous research did not show the effects we demonstrate of interest rates on curtailment. It also did not document curtailment of mortgages with a negative interest rate spread primarily because it is a relatively new phenomenon. McCollum et al. (2015) studies mortgage curtailment using a sample of loan-level data from a mortgage data provider. Examining data from 2001 to 2011, they find substantial growth in the percentage of U.S. mortgage borrowers who make curtailment payments. We build upon this existing work by extending the time series into this new period of a negative interest rate spread and, to the best of our knowledge, make the first calculation of its net present value. In addition, we can measure partial mortgage prepayment in consumer credit record data which allows us to examine this phenomenon in relation to other financial behaviors.

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* month's 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 month's payment by an amount γ that can be derived from the mortgage amortization formula. Specifically, letting P be the remaining principal,[3]

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

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 household's opportunity cost of capital (e.g., the after-tax yield on an alternative investment). The net

[3] 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 produces a strictly positive NPV because the mortgage's 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.[4]

Despite the clear-cut implications of the standard net present value (NPV) framework, a striking share of households voluntarily curtail their mortgage even when the mortgage interest rate r_m lies well below their available investment rate r. In theory, such behavior destroys value: rather than paying down a low-rate loan, a household could allocate the same funds to an outside asset earning a higher return and pay off their mortgage more quickly. Yet as we will document in Section 4, households systematically prepay during times of high disposable income and continue to do so even when $r - r_m$ increases substantially. These deviations cannot be explained by a preference for liquidity, since that would lead households to hold liquid assets instead of prepaying their mortgage. Further, Section 5 shows that curtailment behavior is persistent over time for each household, and that within-household, mortgage repayment barely respond to interest rate changes but do respond to exogenous changes in cash

[4]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.

flows. These deviations cannot be explained by simple inattention or lack of information alone: indeed, one might expect that with more disposable income and fewer liquidity constraints, households would be *less* inclined to prepay if investment opportunities are superior.

A natural way to explain this behavior is through the lens of debt aversion. 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). 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.

The theory of debt aversion proposed by Meissner and Albrecht (2022) proposes a model of debt aversion with declining marginal costs of indebtedness, so households benefit more from paying off debt when they are close to paying it off entirely. 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.

Thus, beyond showing a persistent and household-specific desire to eliminate debt, 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.

Theories of *search costs* provide an alternative psychological foundation for the mechanisms we uncover. 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. A search-costs framework would predict that 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. The structural model in Section 6 allows for both search-related and debt-aversion motives to study counterfactual repayment behavior.

3 Data and Measurement

3.1 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 - February 2023. 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 resepct to earnings equal to 0.[5] 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.[6] 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.[7] The main disadvantage

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

[6] 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.

[7] 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

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 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 excess payments that go toward principal and excess payments that reduce a future month's payment. The former is the focus of this paper – curtailment – but the latter is what we call "clerical" curtailment.[8] "Clerical" curtailment does not reduce the remaining principal of the loan, nor does it shorten its effective maturity. 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 vs. the loan performance data. Figure 11 highlights the discrepancies: the benchmark curtailment flag from the Fannie Mae data (solid red) and from the credit panel (solid blue) are quite different. But when we consider "clerical" curtailers in the Fannie Mae data (dashed red line), the two series look similar.

3.2 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 - February 2023). Panel B contains only January 2022 - February 2023. 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 $210,000 and $170,000, respectively. However, among negative rate spread loans in recent years, the differences are much larger: about $277,000 and $241,000 vs $292,000 and $265,000. Both types face similar marginal

borrower has a credit card, we only consider active credit card accounts.

[8]This is not an issue in the Fannie Mae data. See Appendix B for more details on how we flag curtailment.

tax rates on income (about 12%) and and mortgage interest (also about 12%). Curtailers, on average, have slightly lower mortgage interest rates (4.0%) than non-curtailers (4.15%). They also have shorter terms, on average. Naturally, curtailing households have shorter effective remaining terms; across all years curtailing households on average have 218 months until maturity, whereas non-curtailing households have 258 months remaining. This pattern is similar in the January 2022-February 2023 subsample: curtailers have 232 months until the loan is paid in full, whereas non-curtailers have about 270 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.

4 Quantifying Curtailment Losses

4.1 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 household's 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 like-

lihood 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 3 are histograms of "excess" repayment amounts, for the years 2011-2021 and 2022-2023. 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 2022-2023, 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 in 2011-2021 are about 22% and 14%, respectively; for 2022-2023, the mean and median shares are about 23% and 15%, respectively.

Panels (e) and (f) of Figure 3 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. However, from 2022 to 2023, the distribution of NPVs shifts inward, with nearly half of the observed payments ultimately carrying a negative NPV. In fact, during the last months of this time period, a majority of payments were negative-NPV due to increases in the risk-free rate.

How large are the aggregate curtailment losses? Figure 4 shows the time series of negative-NPV curtailments from January 2022 to February 2023. 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 third quarter of 2022, the total cost to was about $150mn per month for GSE-insured mortgages. This implies an average monthly cost of about $90 per borrower.

Another way of calculating the costs of negative-NPV curtailment is to ask how many basis points these borrower are "giving up" by curtailing their mortgage instead of investing in T-Bills. Panel A of Figure 5 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-February 2023, this increased from about -0.2 percentage points to nearly -1 percentage point. 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.

4.2 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.[9] The first row shows our bench-

[9]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.

mark 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, 35% of mortgages have a negative rate spread from January 2022-February 2023, curtailment losses are about $1.2bn for GSE-insured mortgages, and the average NPV of payments on negative-spread loans nearly -$80.

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 35% to 22%, and the total investment losses fall by about 75%, from $1.2bn to only $430mn. 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 mortgages with a positive or a negative rate spread.

In the third row, we show how the results change when we ignore taxes altogether. On the one hand, the tax deductability of mortgage interest lowers the effective interest rate on mortgages, reducing the benefits of curtailment (Amromin et al., 2007). On the other hand, the taxability of mortgage interest reduces the benefits to other sorts of investments. The second channel matters much more than the first, since ever since the passage of the Tax Cuts and Jobs Act in 2017 most households do not itemize their taxes so the mortgage interest deduction is irrelevant. When we ignore both channels, the estimates are close to our benchmark ones.

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 54% and the average loss rises to $103. 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.

4.3 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-February 2023. Overall, 35% of borrowers have a negative rate spread at some point, and among these, 23% choose to curtail their mortgage. Conditional on curtailing, the average extra payment is $619, with an average net present value (NPV) of about -$80. This implies that, on average, these borrowers are losing over 12% 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 (48% 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.

5 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.

5.1 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 more consistent with the theory that borrowers face an aversion to debt, rather than an alternative hypothesis that borrowers use curtailment as a commitment device for savings. If curtailment were primarily a way to self-impose savings discipline, we might expect it to be more common among those with revolving credit card balances, since these individuals might struggle with liquidity constraints and thus value the precommitment aspect of mortgage prepayment. Instead, we see the opposite pattern: households that systematically avoid other forms of debt, such as credit cards, are also more likely to curtail their mortgage, suggesting that debt aversion plays a key role. This result aligns with survey evidence in Amromin et al. (2007) and Adelman et al. (2010), which documents strong preferences among certain households for reducing their total debt burden rather than optimizing intertemporal savings.

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 household's 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, such as a strong psychological aversion to debt, play a more important role in curtailment behavior than traditional portfolio-based optimization.

5.2 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:

$$CURTAIL_{it} = \beta \text{ Spread}_{it} + \Omega X_{it} + \varepsilon_{it}, \tag{4}$$

where Spread_{it} measures the difference between the mortgage's 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% *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. 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.

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

5.3 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 6 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).[10] 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

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

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:

$$\text{Curtailment}_{it} = \alpha + \beta \cdot ARM_i \times \text{LIBOR}_t + \gamma X_{it} + \delta_i + \varepsilon_{it}, \tag{5}$$

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 borrower's 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:

$$\text{Curtailment}_{i,t+k} - \text{Curtailment}_{i,t-1} = \beta_k(\text{Rate}_{i,t} - \text{Rate}_{i,t-1}) + \gamma X_{it} + \varepsilon_{it}, \quad k = -2, 0, 1, 2, \quad (6)$$

where the independent variable of interest, $\text{Rate}_{i,t} - \text{Rate}_{i,t-1}$, captures the change in borrower i's mortgage rate upon reset. We include fixed effects and restrict the sample to ARMs with rate adjustments occurring at least quarterly. Figure 7 plots the estimated β_k coefficients, showing that a 1 pp increase in the borrower's 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 mortgage's relative NPV.

5.4 Paying Off Debt Entirely

Holding zero debt may be a psychologically distinct reference point for some individuals, whose payoff from "completing" the mortgage may dominate the potential "costs" of curtailing. If this theory holds, we should expect to see a large jump in likelihood of curtailing near the end of the loan. Figure 8 demonstrates exactly this; it plots the probability of curtailing against the remaining principal balance.[11] Curtailment occurs at more or less the same rate for all balance

[11] 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 3 and Appendix B for

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.footnoteFu (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. This evidence is consistent with the model of Meissner and Albrecht (2022), which features a nonlinear psychological benefit to reducing the amount of debt.

5.5 Sophistication and Attentiveness

A natural explanation for the low elasticity of mortgage curtailment with respect to interest rates is that borrowers are unsophisticated or inattentive. If households do not actively monitor their mortgage's relative profitability, their curtailment decisions may fail to respond to changes in the financial environment. To investigate this hypothesis directly, we focus on groups likely to exhibit greater sophistication or attentiveness in their mortgage management decisions.

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:

$$\text{Rate}_i = \alpha + \beta_1 \text{CreditScore}_i + \beta_2 \text{LTV}_i + \gamma_{q(i)} + \varepsilon_i, \tag{7}$$

more details.

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 9 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 and allow us to reject the null hypothesis of zero responsiveness, their economic magnitude is notably small—close to zero in absolute terms. Even among borrowers identified as sophisticated or attentive, 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.

Thus, the limited responsiveness to interest rate spreads we document reflects fundamental household preferences and constraints rather than cognitive frictions or informational gaps. 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 rational attentiveness.

6 Model and Counterfactuals

We consider the decision of a household that receives income F_{it} and may use it for prepayment or to make outside investments. Section 2 shows that in a conventional portfolio-maximization problem, the per-dollar value of curtailment depends on the interest rate spread, $r - r_m$. We extend the portfolio maximization problem by also allowing utility to depend on a psychological cost associated with holding debt, i.e., debt aversion. Given the importance of disposable income for prepayment, we also include a variable measuring income available for prepayment. The goal of these factors is to match the patterns shown in the previous section. Specifically, (1) A high level of negative-NPV curtailment and a low sensitivity to interest rates; (2) household-level persistence in curtailment; and (3) curtailment that is higher when the mortgage balance is smaller. Then, we use the structure of the model to calculate counterfactual levels of negative-NPV curtailment when we "turn off" each of the frictions.

We model debt aversion as having two components. First, a time-invariant household-specific parameter γ_i that captures a household's persistent preference to reduce debt. When we estimate the model, we treat γ_i as normally-distributed random effects whose distribution we estimate.[12] Allowing debt aversion to vary by household generates a distribution of repayment probabilities that are persistent within-household over time. Second, motivated by lab evidence in Meissner and Albrecht (2022) as well as our own reduced-form findings, we also include a term that varies with the remaining balance RB_{it}, $\theta(RB_{it})$. For our empirical application, we use the log of the remaining balance and its square, although the results are similar with alternative specifications.

Given the importance of cash flows for mortgage prepayment, an important feature of our model is a control for current disposable income. Beyond that, disposable income could matter if household fixed costs are important for finding outside investments. When households have more cash on hand, the fixed cost is diluted, making alternative investments relatively more

[12] Treating them as random effects avoids the incidental parameters problem in nonlinear models and is consistent with the estimation strategy in Train (2009).

attractive. Therefore, we model the role of income using the inverse of income.

Households receive random utility shocks ε_{it} so the relative benefit of curtailment is given by

$$U_{it} = \delta(r - r_m)_{it} + \frac{\beta}{F_{it}} + \theta(RB_{it}) + \gamma_i + \epsilon_{it}, \tag{8}$$

where $\varepsilon_{it} \sim N(0, \sigma^2)$. Curtailing is preferred to investing when $U_{it} > 0$ so the probability that borrower i curtails in month t is

$$\Pr\Big(\text{Curtail}_{it} = 1\Big) = \Phi\Big(U_{it}/\sigma\Big), \tag{9}$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function. The Probit model is only identified up to a scaling factor, so following standard practice, we normalize σ to 1. We can divide by $\hat{\delta}$ to interpret the coefficients in units of the interest rate spread and thus allow $\sigma^2 \equiv Var(\varepsilon_{it})$ to be a free parameter.

Complicating our estimation, F_{it} is only observed for households that curtail. When households do not curtail, we do not know how much money they had available to curtail but instead invested in something else. To overcome this issue, we impute cash flows as described in Section 3. Our imputation procedure introduces measurement error that depends on the dependent variable. We follow the approach of Train (2009) and Wooldridge (2010) by employing a control function method within our probit specification. Our instrumental variable is aggregate disposable income growth, which is plausibly exogenous during the sample period (starting in January 2017, so subsequent COVID-related income shocks provide clear variation).

We estimate five specifications to assess how the results depend on the sample and to determine the robustness of the findings. First, our benchmark specification uses our preferred measure of the interest rate spread and uses households with a negative rate spread starting in 2017. Second, we use the MBS spread instead of the spread relative to Treasurys. Third, we allow the coefficient on $\frac{1}{F_{it}}$ to vary by household and treat it as a random effect. Fourth, we

limit the sample to households that prepaid at least half the time in the period before the main regression sample begins. This specification eliminates households who rarely have enough disposable income available for curtailment.[13] Fifth, we remove households that make exactly the same mortgage prepayment over the previous 12 months.[14] Since these households could be using an "auto-pay" feature to set their mortgage payments, interest rate changes might be less salient for them.

We then perform several counterfactual experiments to assess the relative importance of the modeled channels. In one exercise we "turn off" debt aversion by setting $\gamma_i = 0$ for borrowers with $\hat{\gamma}_i > 0$, i.e., those who are estimated to be debt averse; in another, we set the cash-flow term's coefficient β to zero to eliminate its influence; and finally we set the completion function $\theta(RB_{it})$ to zero. In each case, we re-calculate the predicted curtailment rates and the implied aggregate costs, and we report these counterfactuals in a summary table.

Table 8 reports the coefficient estimates and variance components from our five specifications, along with counterfactual predictions under alternative scenarios. These estimates are obtained from subsamples of borrowers with negative interest rate spreads only. Across the baseline (Specification 1), the MBS spread (Specification 2), the model with a random cash coefficient (Specification 3), the frequent prepayer subsample (Specification 4), and the non-autopayer subsample (Specification 5), several patterns emerge.

First, the "Benchmark Spread" coefficient is positive and statistically significant in the Baseline, Frequent Prepayer, and No-Autopay specifications. Its magnitude is small (around 0.044–0.056), which is consistent with our finding that changes in the interest rate spread have little impact on curtailment behavior. This muted sensitivity can be explained by the high variance in the individual intercepts. In our model, the intercept γ_i captures each household's fixed, time-invariant propensity to prepay—that is, their degree of debt aversion. The estimated variance of γ_i is very large (ranging from approximately 3.63 to 8.46 across specifications). Such high

[13] Including households who rarely have disposable income available could make it more difficult to test for the role of search costs, since increases in cash might not benefit them. Therefore we drop these households in this specification.

[14] As shown in Figure 14, this affects only a small minority of households.

heterogeneity indicates that, for many borrowers, the psychological cost of carrying debt overwhelms the marginal financial benefit implied by the interest rate spread. In other words, the high variance in γ_i helps explain why even substantial changes in the marginal rate do not induce marked shifts in curtailment behavior.

Second, we estimate mostly negative coefficients on $\frac{1}{\text{Cash}}$. In the Baseline, Frequent Prepayer, and Non-Autopayer specifications, this coefficient is strongly negative (ranging from -18.243 to -23.473), indicating that, consistent with our earlier results, a higher level of available cash (i.e., a lower value of 1/Cash) is associated with a lower marginal propensity to prepay. In Specification 3, however, where we allow for random heterogeneity in the cash coefficient, the mean estimate is positive (14.179); this difference suggests that there is substantial heterogeneity in the search-cost (or cash-flow) effect. While there is little evidence that search costs matter on average, they may matter for some share of households.

Turning to the counterfactual predictions (displayed in the lower portion of Table 8), we note three key findings. First, the baseline predicted probability of prepayment is approximately 22–23% for most specifications, but it is noticeably higher (27%) in the No-Autopay subsample, reinforcing the idea that curtailment is an active choice by households. Second, when we "turn off" the debt-aversion channel by setting $\gamma_i = 0$ for borrowers with positive estimated $\hat{\gamma}_i$ (i.e., effectively removing the fixed effect for those identified as debt averse), the mean predicted probability of curtailment falls dramatically—to as low as 2.8% in the Baseline and Frequent Prepayer specifications and to 8.2% in the No-Autopay subsample. Finally, "turning off" the cash channel (by subtracting the contribution of β/Cash) leads to moderate changes in the predicted probabilities, with the effect varying somewhat across specifications. Together, these counterfactuals indicate that the persistent, individual-specific preference to eliminate debt (as captured by γ_i) is the dominant force driving curtailment decisions. Although the cash channel is statistically significant, its impact on aggregate behavior is secondary relative to the large, fixed component of debt aversion.

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 $1.2bn per month 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. Using a probit model, we explore how would change if they had different underlying preferences. The strongest effect on curtailment comes from changing households' fixed desire to prepay their mortgage.

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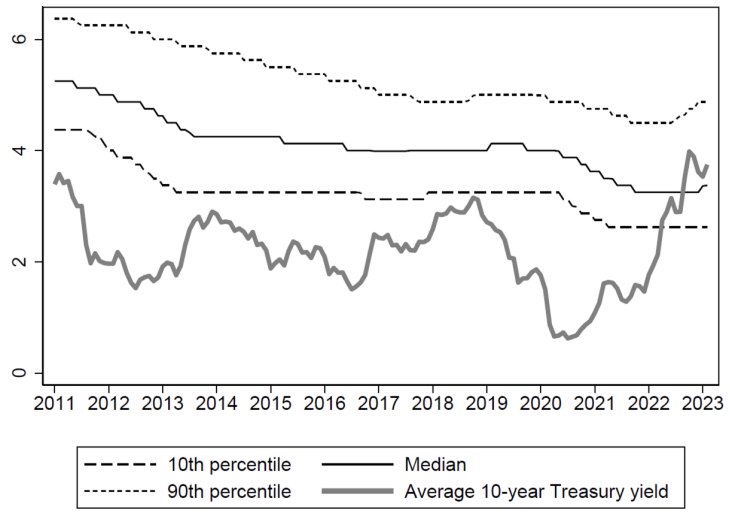
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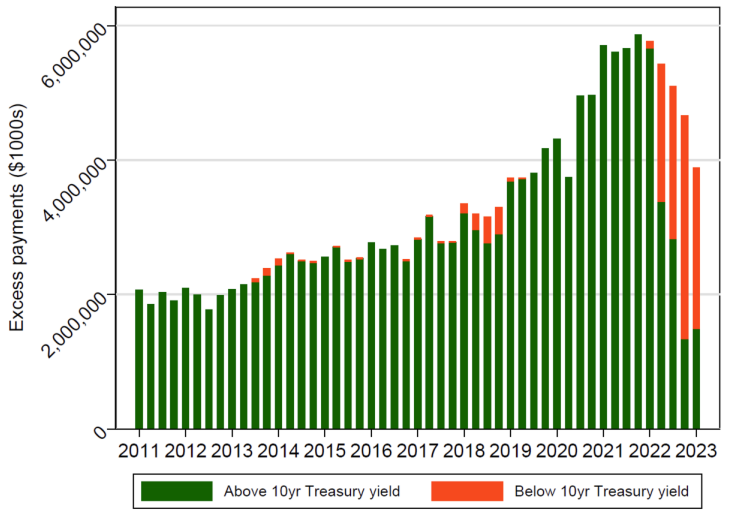
8 Figures and Tables

Figure 1

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



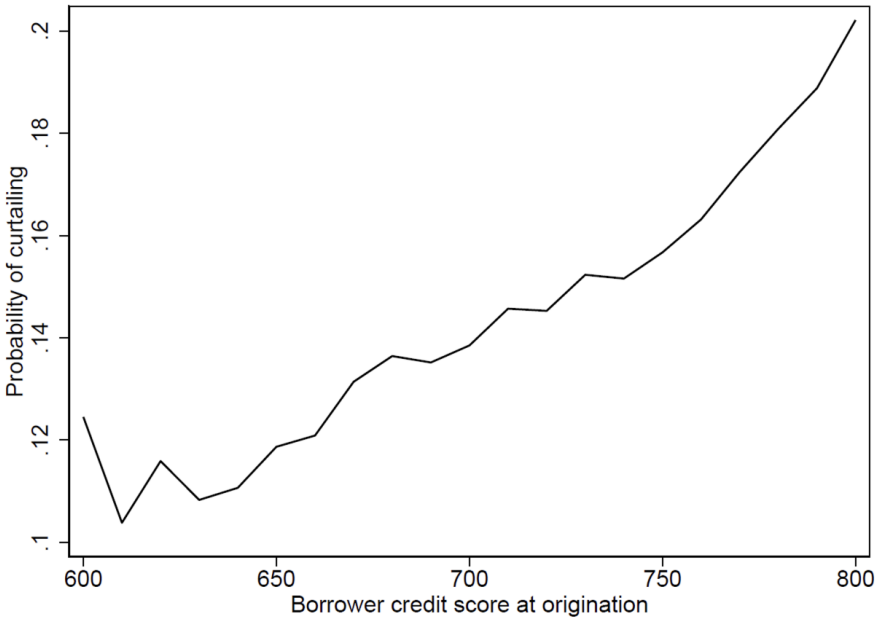
(b) Total excess payments by loans with above/below 10yr Treasury yields



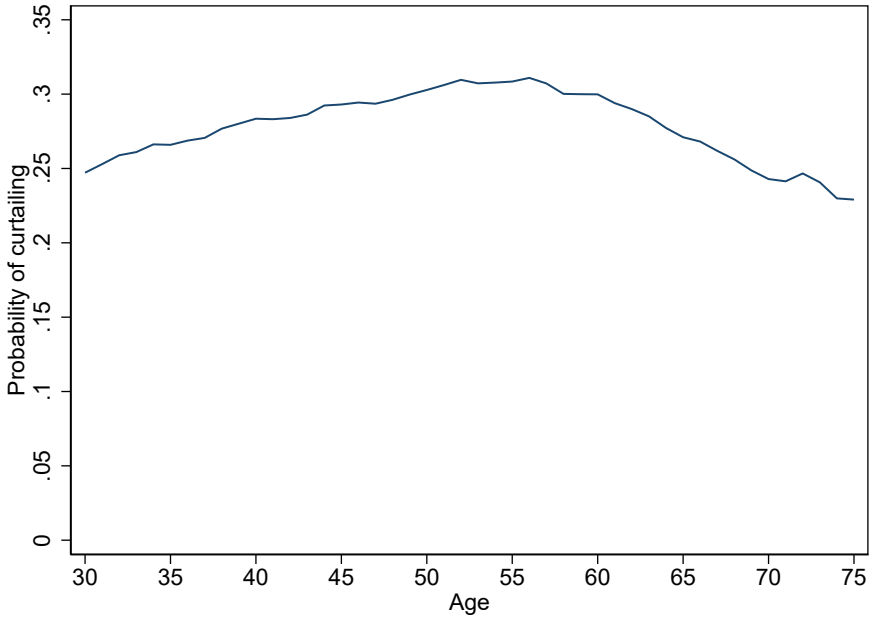
Note: Panel (a) shows the 10th, 50th, and 90th percentile for outstanding mortgage interest rates in black/dashed lines and average 10-year Treasury yield in gray, by month. Panel (b) shows total volume of curtailment amounts each quarter. The green represents the share of curtailment volume from loans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



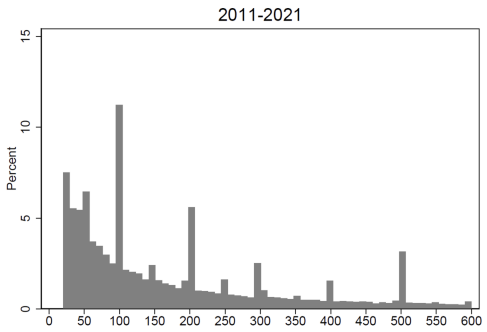
(b) Probability of curtailment by borrower age



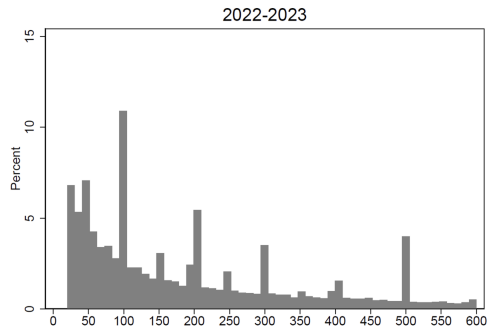
Note: Panel (a) shows the likelihood a household curtails its mortgage by credit score of borrower at origination. Credit scores are rounded to the nearest 10. Panel (b) shows the likelihood a household curtails its mortgage by age of borrower. Authors' calculations based on Fannie Mae Single-Family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

Figure 3: Distribution of excess payments

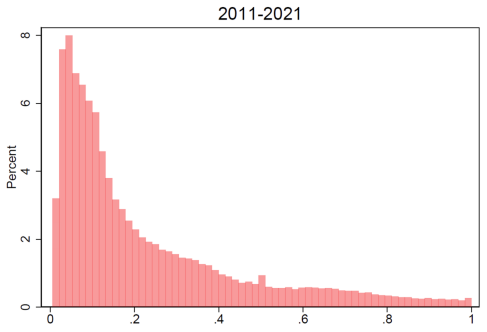
(a) Notional value 2011-2021



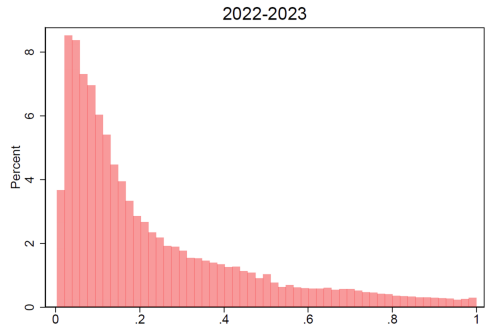
(b) Notional value 2022-2023



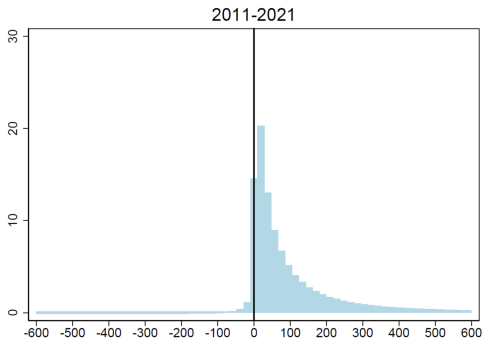
(c) Notional value scaled by monthly payment 2011-2021



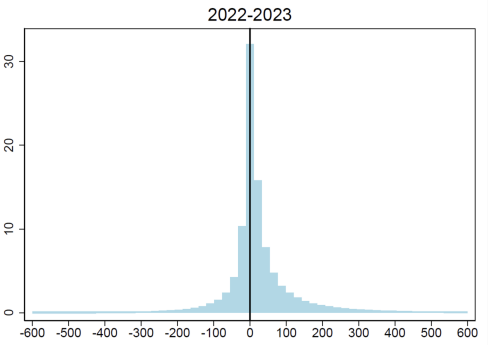
(d) Notional value scaled by monthly payment 2012-2023



(e) NPV 2011-2021



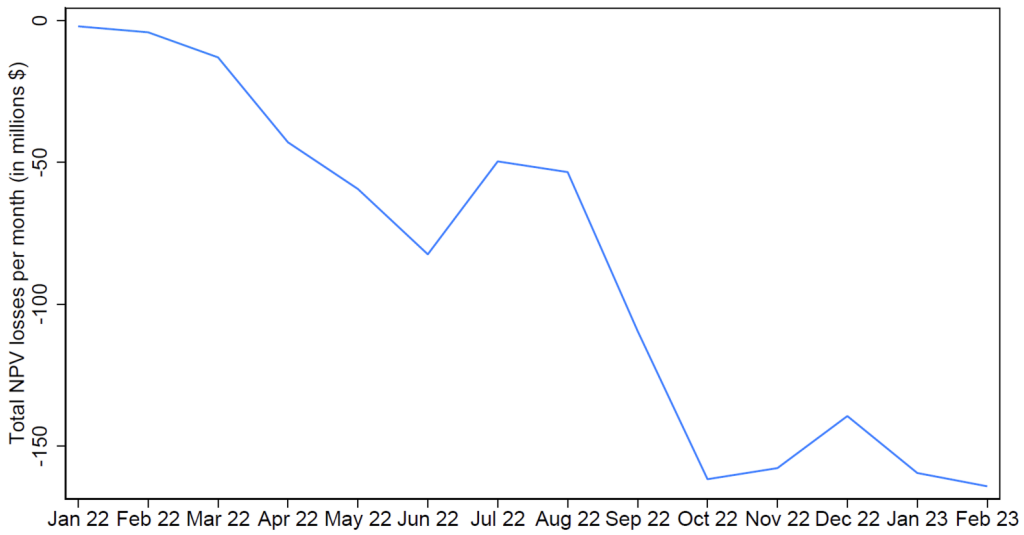
(f) NPV 2022-2023



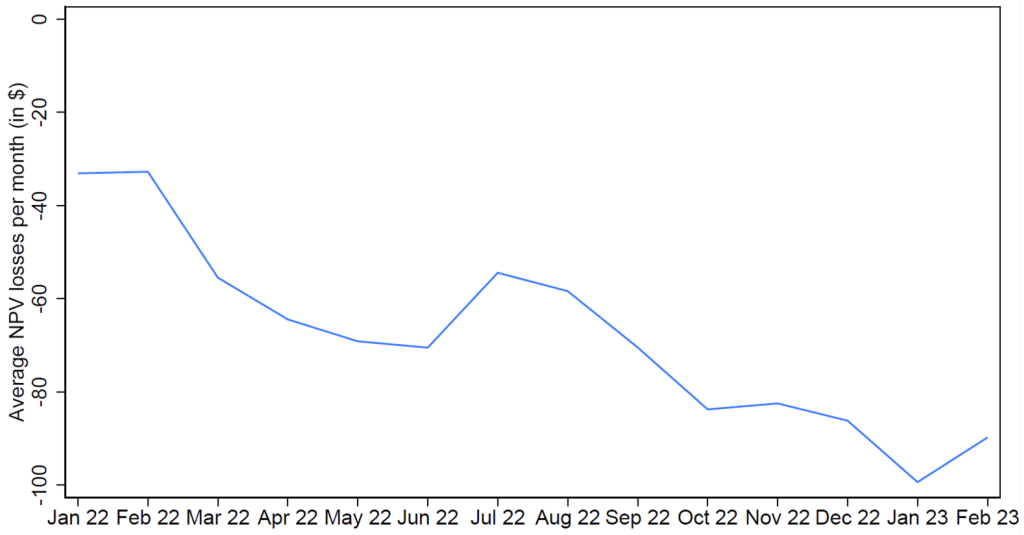
Note: The top row shows the distribution of curtailment amounts in dollar values. The middle row shows the distribution of curtailment amounts but scaled by the monthly payment. The bottom row shows the NPV of these payments. The left column is 2011-2021, and the right column is 2022-2023. NPV is calculated using tax-adjusted mortgage interest rates and tax-adjusted maturity-matched Treasury yields.

Figure 4: NPV of excess payments for negative interest rate spread mortgages

(a) Total NPV losses



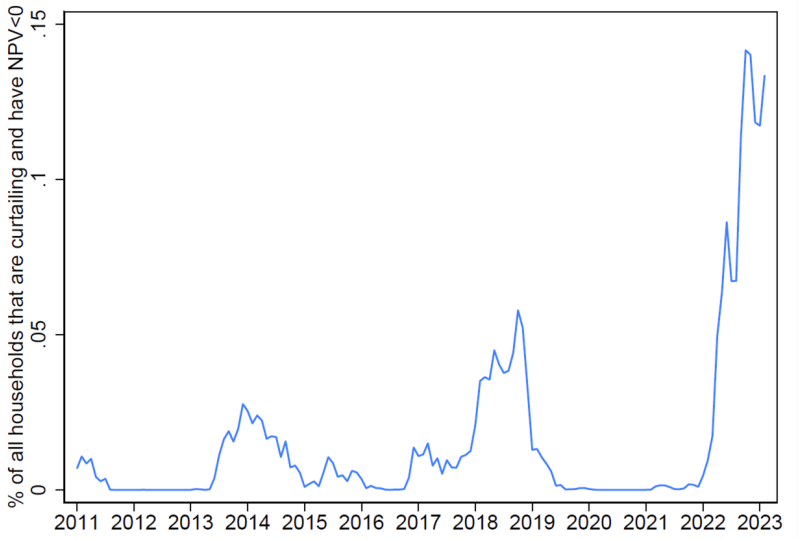
(b) Average NPV losses



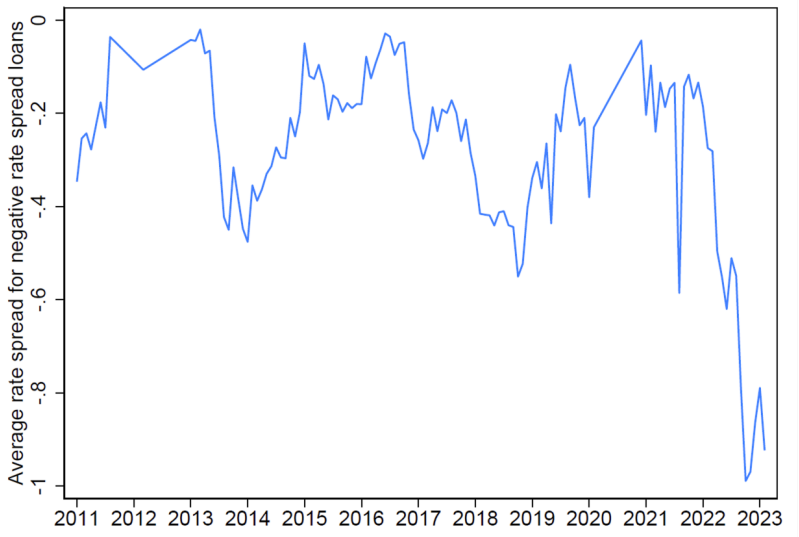
Note: Panel (a) shows the total NPV losses by per month for loans with negative interest rate spreads. Panel (b) shows the average NPV of these payments. NPV is calculated using tax-adjusted mortgage interest rates and tax-adjusted maturity-matched Treasury yields

Figure 5

(a) Share of negative rate spread households curtailing



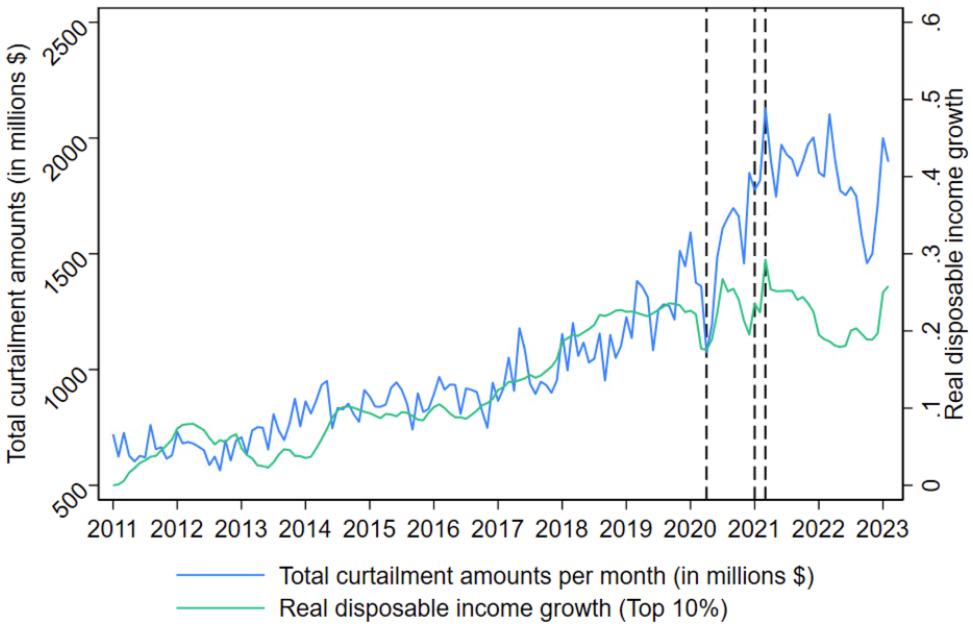
(b) Average negative rate spread (weighted by curtailment amount)



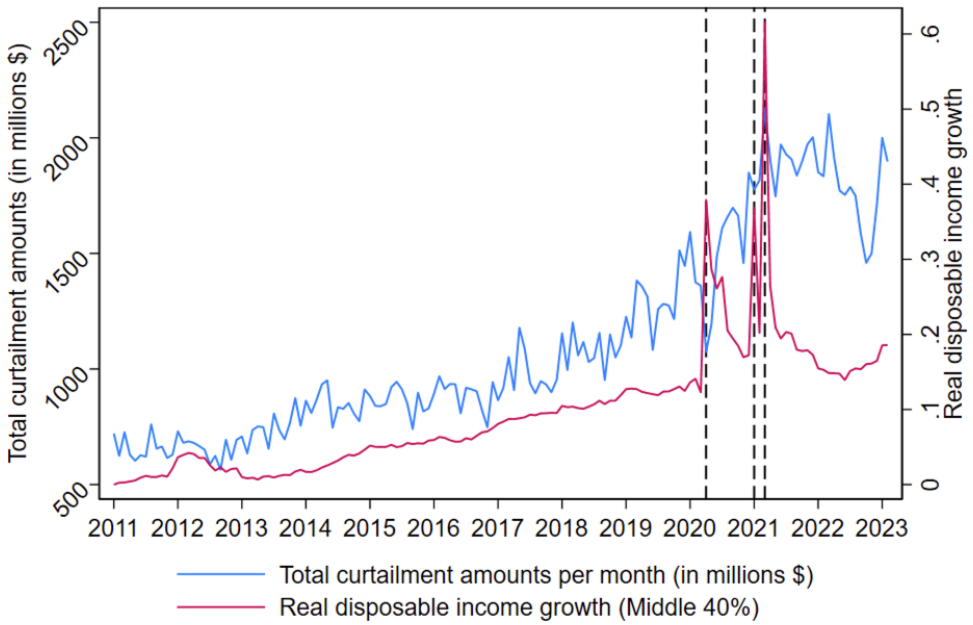
Note: Panel (a) shows share of all households that have a negative interest rate and who curtail by month. Panel (b) shows the average interest rate spread for negative rate spread loans, weighted by the dollar value of curtailment.

Figure 6: Total curtailment amounts and real disposable income growth

(a) Top 10%

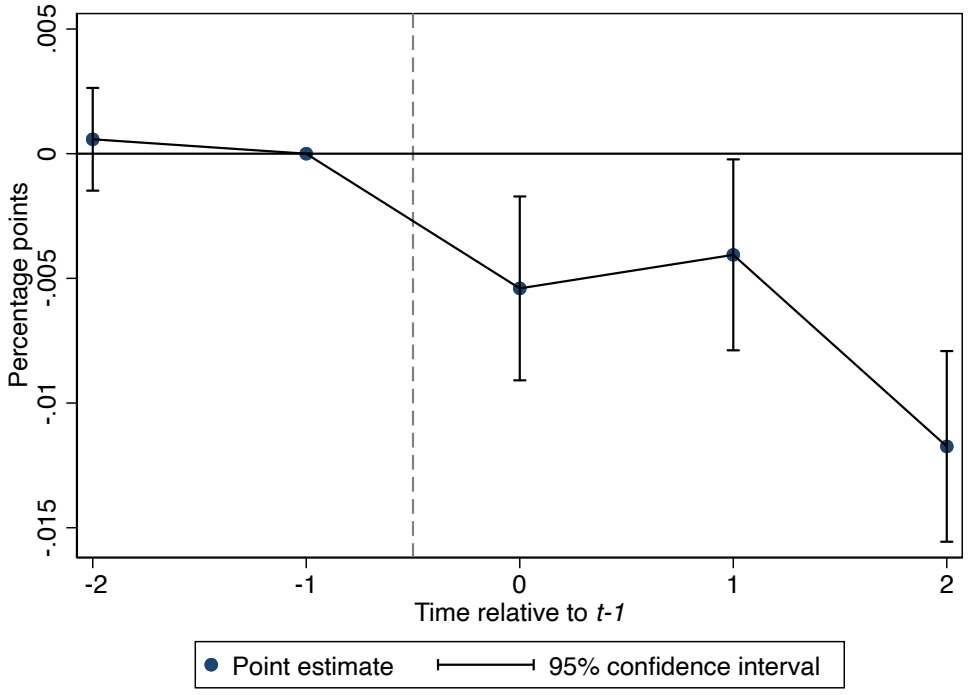


(b) Middle 40%



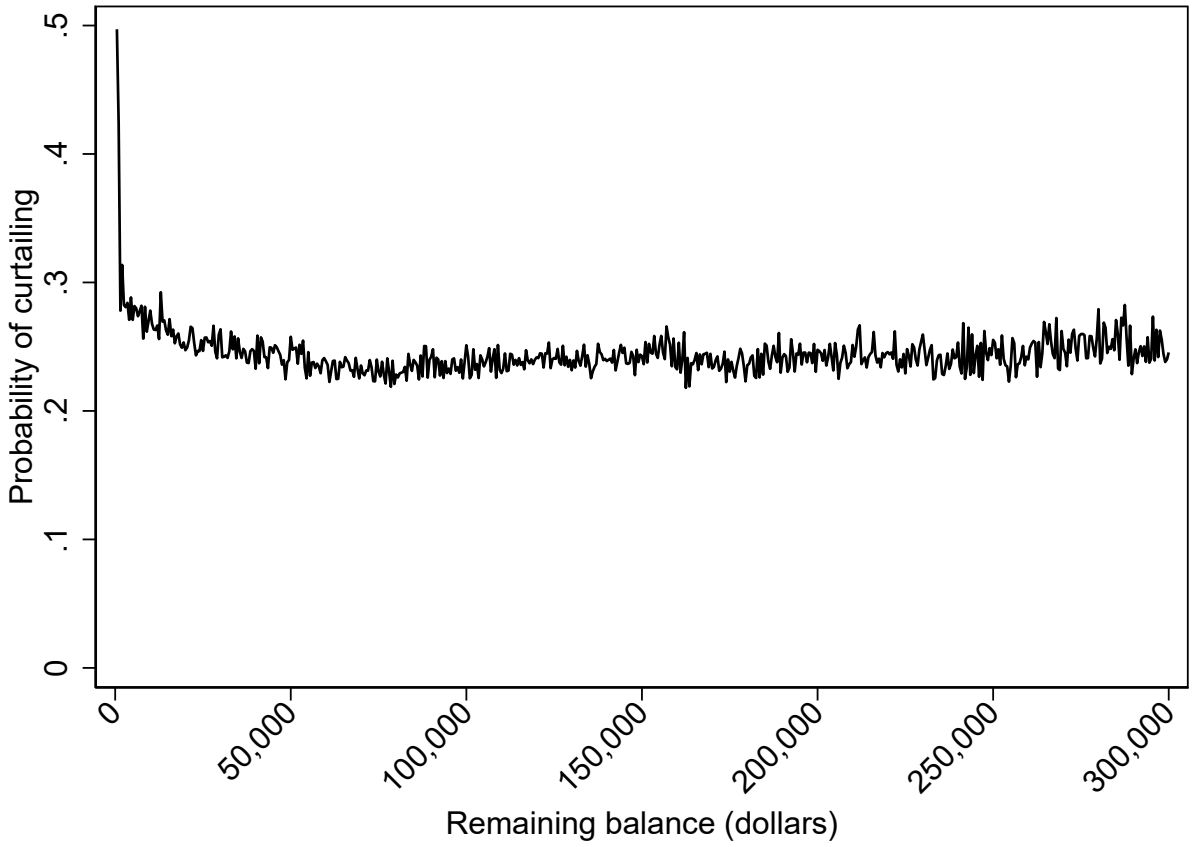
Note: Green and red lines are real disposable income growth for the top 10% and middle 40% of the income distribution, respectively. Blue line is total curtailment amounts per month in millions of dollars. Dashed lines represent months when COVID stimulus checks were sent (April 2020, January 2021, March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

Figure 7: Probability of curtailment around interest rate changes



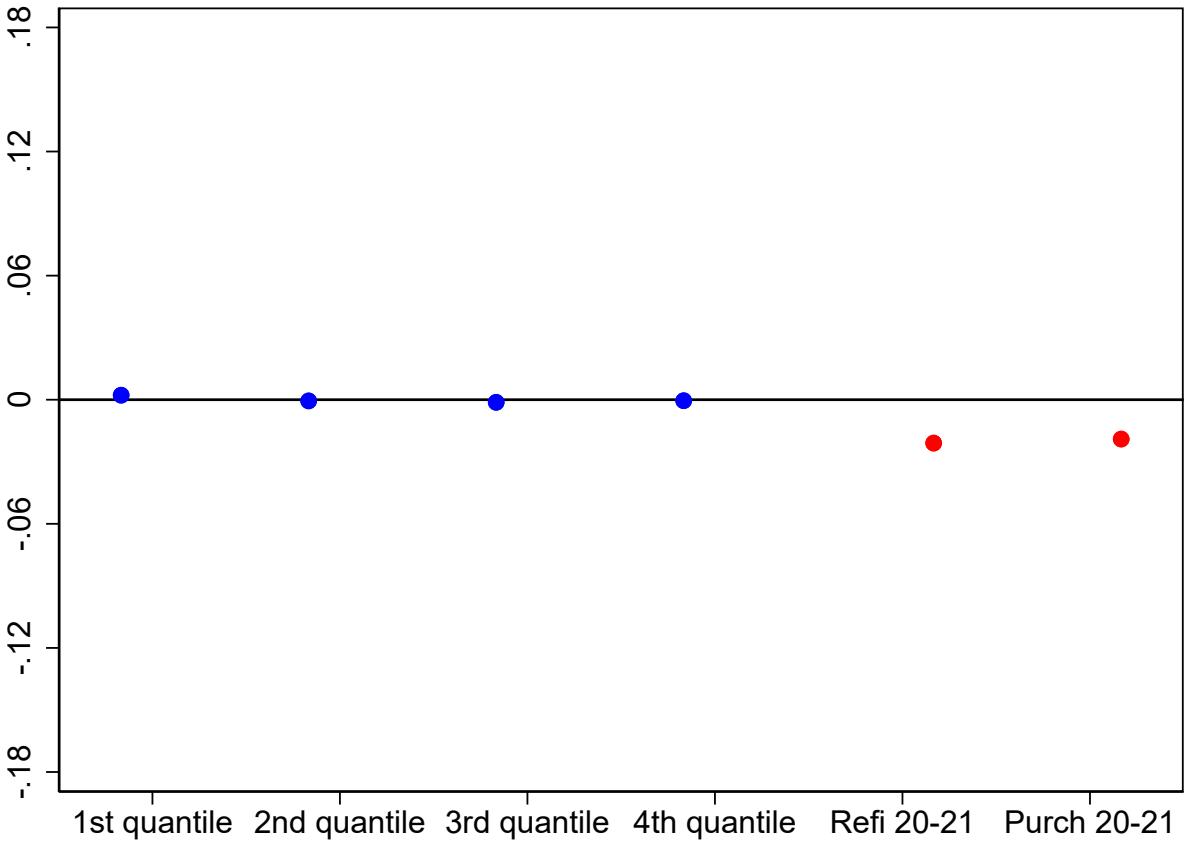
Note: Figure plots estimates of β_k's from equation (6). Authors' calcuations based on Freddie Mac Single-Family Non-Standard dataset.

Figure 8: 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 9: Effects of interest rate spreads on curtailment by measures of attentiveness



Note: Each dot represents the coefficient on $Spread$ from Equation (4) using different samples. The blue dots correspond to samples by quartiles of residuals from Equation (7). The red dots correspond to the 2020-2021 purchase and refinance samples. Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset.

Table 1: Summary statistics

Panel A: All loans (Jan 2011 - Feb 2023)	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	212,863	122,632	210,275	122,632	213,441	123,231
Current balance	170,331	127,066	173,524	119,408	169,618	128,704
Term (in months)	304	84	290	89	307	82
Months to legal maturity	259	96	241	98	263	95
Effective months to maturity	251	100	218	104	258	98
Mortgage interest rate	4.12	0.96	4.00	0.95	4.15	0.96
Marginal tax rate (federal + state)	11.67	14.35	11.71	14.29	11.65	14.37
Marginal tax rate (mortgage interest)	11.83	12.76	11.50	12.70	11.91	12.78
Loan-month obs.	14,540,283		2,655,771		11,884,512	
Panel B: All loans (Jan 2022 - Feb 2023)	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	248,375	137,986	244,701	134,452	249,367	138,908
Current balance	219,644	140,695	207,576	138,012	222,902	141,234
Term (in months)	311	79	297	85	315	76
Months to legal maturity	270	92	253	96	275	91
Effective months to maturity	262	98	232	103	270	94
Mortgage interest rate	3.46	0.85	3.35	0.78	3.49	0.86
Marginal tax rate (federal + state)	12.06	13.83	12.11	13.80	12.05	13.84
Marginal tax rate (mortgage interest)	8.69	12.68	8.57	12.49	8.73	12.73
Loan-month obs.	1,895,796		402,965		1,492,831	
Panel C: Negative rate spread loans (Jan 2022 - Feb 2023)	All		Curtailers		Non-Curtailers	
	Mean	SD	Mean	SD	Mean	SD
Origination balance	288,678	148,672	277,201	143,653	292,117	149,973
Current balance	259,509	151,171	240,960	146,543	265,068	152,091
Term (in months)	295	86	278	90	300	84
Months to legal maturity	265	95	247	98	270	94
Effective months to maturity	259	99	233	103	267	96
Mortgage interest rate	2.98	0.52	2.93	0.51	2.99	0.53
Marginal tax rate (federal + state)	5.82	12.32	6.13	12.35	5.73	12.31
Marginal tax rate (mortgage interest)	14.33	14.55	13.64	14.16	14.54	14.66
Loan-month obs.	668,766		154,212		514,554	

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	.35	-1,228.44	-79.66
No available retirement account	.22	-430.45	-43.82
No tax adjustments	.40	-1,594.79	-90.37
MBS	.54	-2,413.82	-103.46

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 - Feb 2023. See text for more details.

Table 3: Curtailment heterogeneity

	$\mathbb{P}$(Spread<0)	$\mathbb{P}$(Curtail|Spread<0)	Notional value	NPV
All	0.35	0.23	$619.08	-$79.66
Subgroup				
High Credit Score	0.39	0.25	$684.41	-$87.61
Low Credit Score	0.31	0.20	$516.47	-$67.16
First-time Home-buyer	0.29	0.19	$620.86	-$99.37
Not First-time Home-buyer	0.37	0.24	$618.86	-$77.18
Urban	0.36	0.23	$629.08	-$81.99
Rural	0.28	0.24	$518.38	-$56.22
Originated 2020-2021	0.48	0.23	$579.45	-$81.60

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 - Feb 2023.

Table 4: Predicting curtailment

	(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	-0.010***	0.000	-0.008***	-0.009***	-0.000	-0.005***	-0.007***	0.000	-0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Loan FEs		✓			✓			✓	
Month FEs			✓			✓			✓
$\bar{Y}$	0.183	0.183	0.183	0.204	0.204	0.204	0.162	0.162	0.162

Note: Table reports estimates from the linear probability model in Equation (4). "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. $N = 14,540,283$ in Columns (1) and (3). $N = 14,538,375$ in Column (2). $N = 7,210,180$ in Columns (4) and (7). $N = 7,209,426$ in Column (5). $N = 7,330,103$ in Columns (7) and (9). $N = 7,328,949$ in Column (8). Authors' calculations based on the Fannie Mae Single-Family Loan Performance dataset, FRED, 2010-2022 Surveys of Consumer Finances and NBER Taxsim. 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***	-0.010***	-0.009***	0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	12,492,531	12,492,531	12,492,531	12,492,531	12,492,531
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.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Probit Model Estimates and Counterfactual Predictions

	Baseline	MBS Spread	Random Cash	Prepayer	No Autopay
Coefficient Estimates					
1/Cash	−18.243***	−10.995***	14.179**	−18.243***	−23.473***
	(4.340)	(2.966)	(4.606)	(4.340)	(4.694)
Benchmark Spread	0.044***	–	0.005	0.044***	0.056***
	(0.009)		(0.010)	(0.009)	(0.010)
Control Function	−33.810***	−40.101***	−69.856***	−33.810***	−15.918***
	(4.341)	(2.967)	(4.610)	(4.341)	(4.697)
Log(Remaining Balance)	2.381***	2.510***	2.395***	2.381***	2.198***
	(0.090)	(0.066)	(0.102)	(0.090)	(0.094)
Log(Remaining Balance)^2	−0.111***	−0.113***	−0.110***	−0.111***	−0.104***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
MBS Spread	–	0.014*	–	–	–
		(0.006)			
Constant	−14.279***	−15.481***	−15.057***	−14.279***	−12.323***
	(0.526)	(0.381)	(0.592)	(0.526)	(0.553)
Variance Components					
Variance of Intercept (γ_i)	6.168***	4.433***	8.459***	6.168***	3.631***
	(0.085)	(0.039)	(0.091)	(0.085)	(0.040)
Variance of Cash Channel Coefficient	–	–	2343.243***	–	–
			(42.075)		
Covariance (Cash Channel, Intercept)	–	–	−102.134***	–	–
			(1.591)		
Observations					
Observations	962,292	1,697,985	962,292	962,292	622,496
Counterfactual Predictions (Mean Predicted Probability)					
Baseline	0.2228	0.2195	0.2235	0.2228	0.2697
No Positive γ_i's	0.0282	0.0451	0.0473	0.0282	0.0820
No Cash Channel	0.2474	0.2339	0.2093	0.2474	0.3138

Note: The upper block of this table reports coefficient estimates (with standard errors in parentheses) and variance components from a modified structural model of mortgage curtailment. Five specifications are compared: (1) Baseline using the benchmark spread; (2) using a MBS spread; (3) with a random coefficient on the cash–availability variable; (4) for borrowers with frequent prepayment history (prior to January 2017); and (5) excluding autopayers. The lower block shows counterfactual predictions for the mean probability of curtailment under four scenarios: the baseline prediction, predictions with borrower fixed effects removed, with the cash channel effect removed, and with the completion effect removed. These estimates help gauge the relative importance of the behavioral channels driving mortgage curtailment.

Appendix

A Data Dictionary

Figure 10: 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

B Detailed Data Construction

B.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.

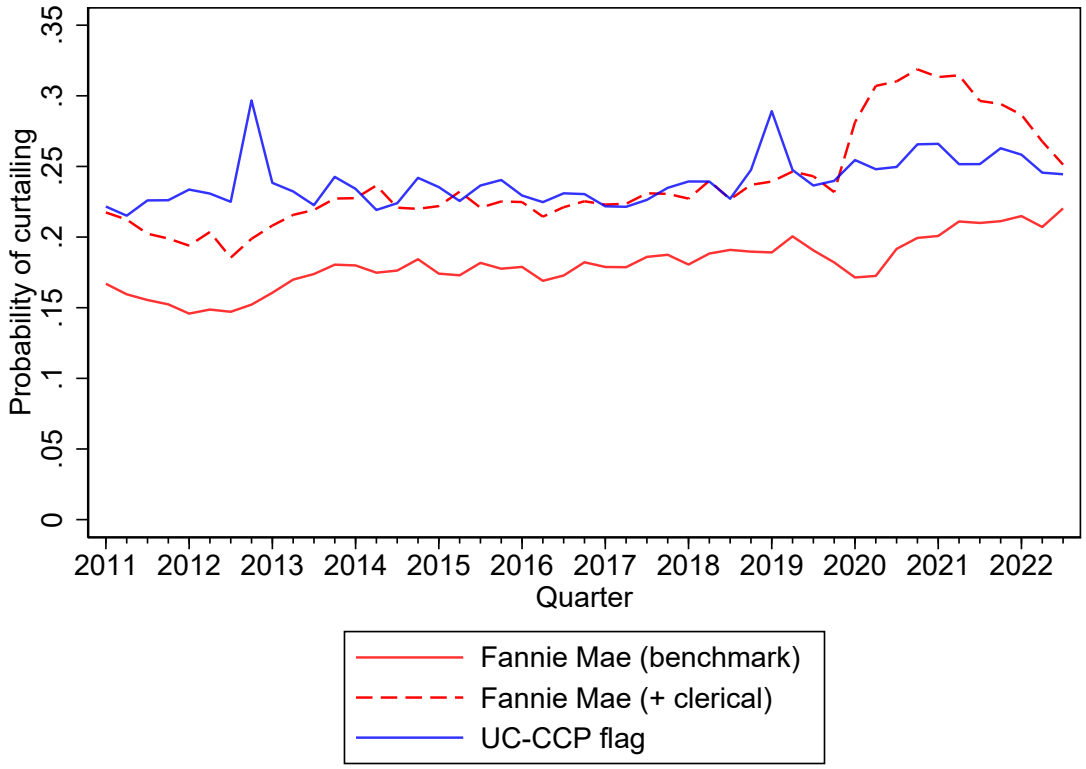
We observe the outstanding principal balance each month for a given mortgage. Using the outstanding balance in month t, we calculate the *expected* outstanding balance in month $t+1$ by subtracting the principal share of the monthly payment from the outstanding balance in month t, i.e. $\mathbb{E}[B_{t+1}] = B_t - (1-r_m) * MP$. 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 the household did not originally curtail, but rather earmarked the extra cash to go to a future payment.

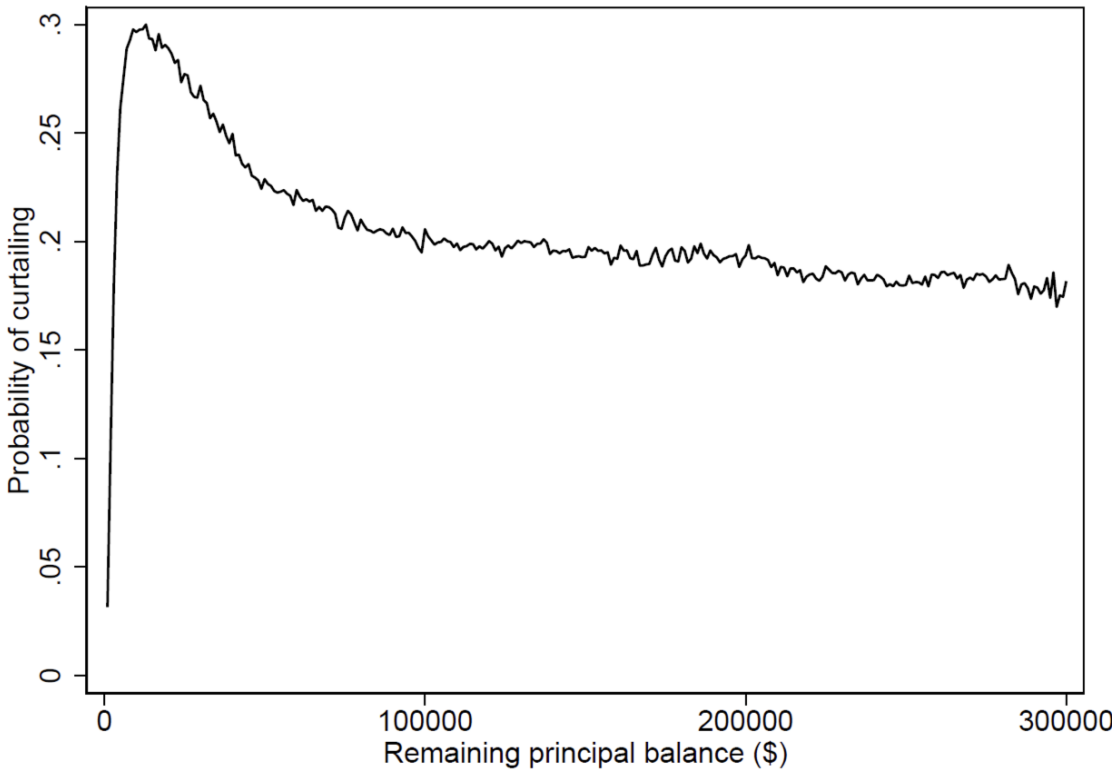
Lastly, we do not consider any excess payment in the final month of a loan's term to be a curtailment. This is because we are unable to distinguish if the loan has matured or refinanced. Because of this, the estimated probability of curtailing for very small (less than $2,000 or so) remaining balances is mechanically attenuated. This is not because households are less likely to curtail with small remaining balances – the exact opposite is true (see Figure 8). Figure 12 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.

Figure 11: 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 12: 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.

B.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[15], 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.

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

C 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).[16] All Alaska residents that have lived in 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.

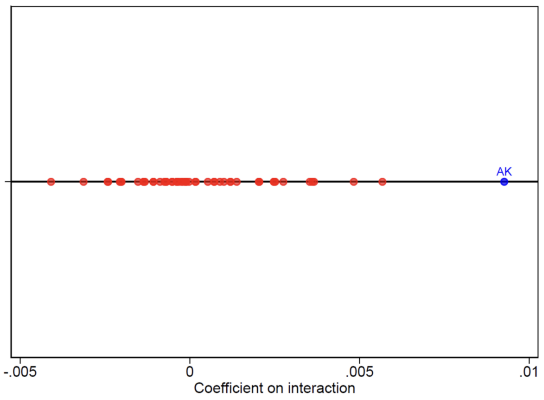
Estimates are reported in Table 9. The dependent variable in Column 1 is a curtailment indicator. The estimates imply that Alaska residents are nearly a full percentage point more likely to curtail in October on average. Relative to the sample mean, this is a roughly 5% increased likelihood. In Columns 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

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

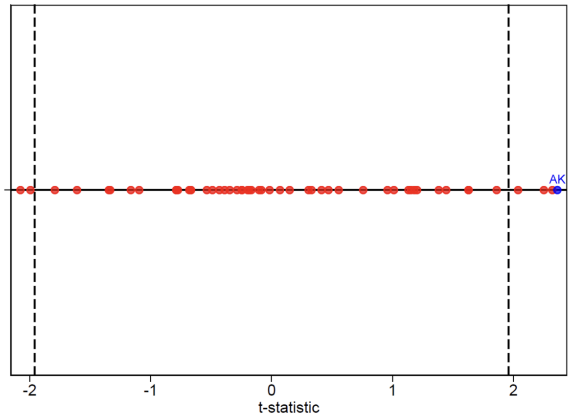
residents are using the excess income to repay their mortgages.

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 13, 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.

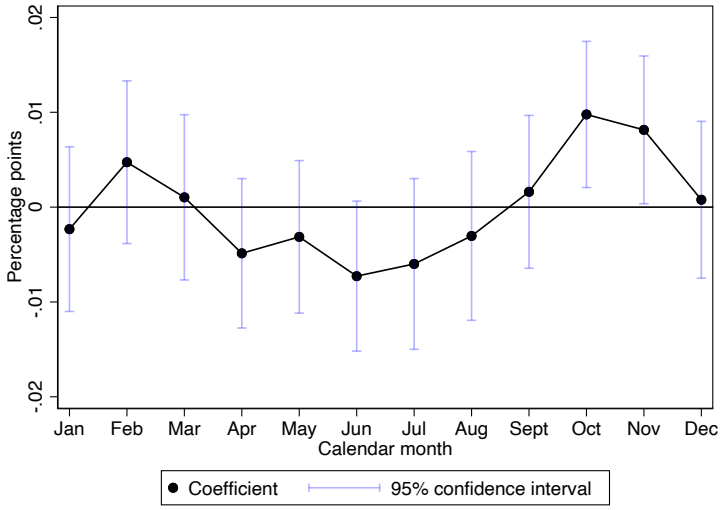
Figure 13: Effects of Alaska Permanent Fund Dividend on Curtailment



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



(b) Distribution of t-statistics



(c) Coefficients on interaction term by calendar month

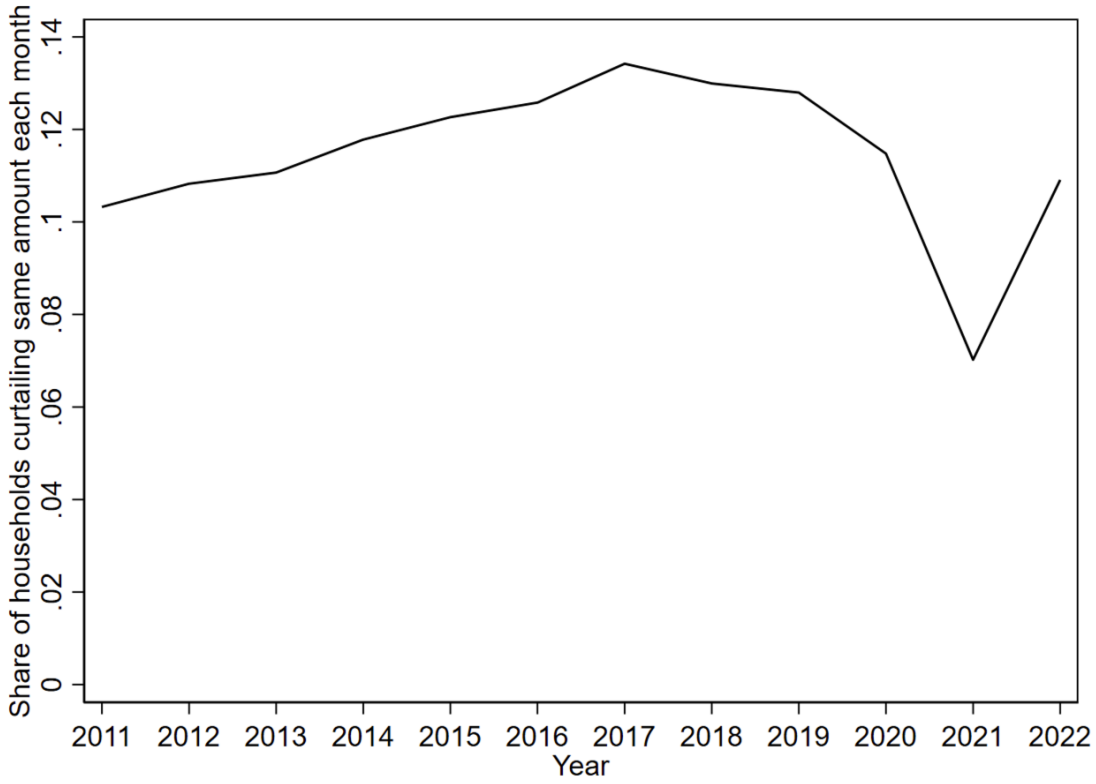
Note: Panel (a) plots the point estimates from (11) from all 51 regressions. Panel (b) plots the associated t-statistics. Panel (c) again plots the point estimates from (11) but for all 12 calendar months; coefficients are black and 95% confidence intervals are light blue. Authors' calculations based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

Table 9: 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 14: 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.