

# Liquidity vs Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession – Online Appendix

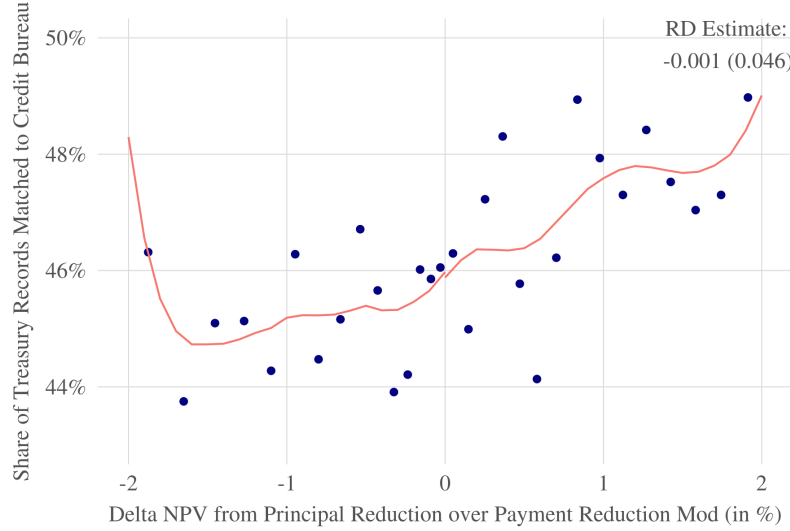
Peter Ganong and Pascal Noel

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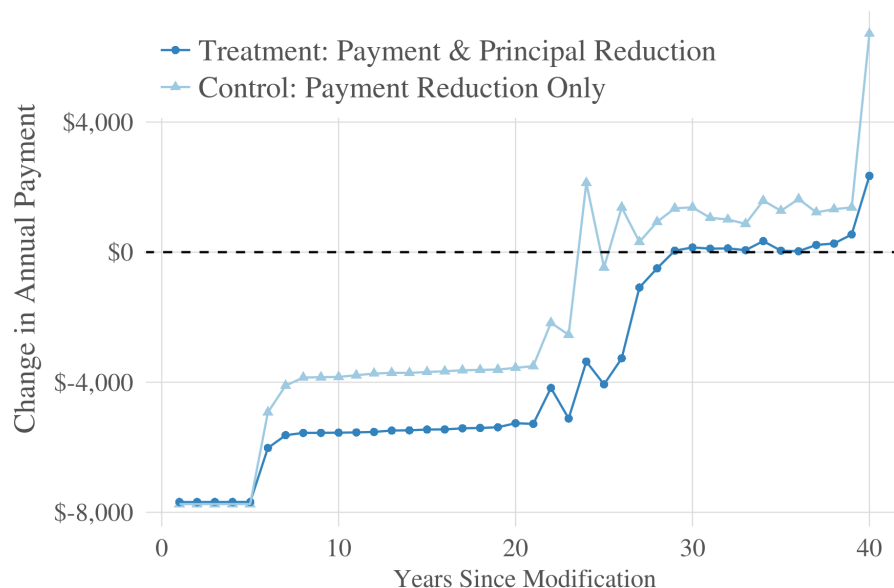
## A Appendix Figures and Tables

Figure 1: Match Rate around Principal Reduction Discontinuity



Notes: This figure plots the share of borrowers in the Treasury HAMP dataset successfully matched to their credit bureau records. The horizontal axis shows the normalized predicted gain to lenders of providing principal reduction to borrowers from equation (1). The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (2).

Figure 2: Financial Impact of Modifications with and without Principal Reduction



Notes: This figure is an unwinsorized version of Figure 1a. The plot shows the difference in average annual payments for borrowers receiving each type of modification relative to the payments borrowers owed under their unmodified mortgage contracts in the matched HAMP credit bureau dataset. The conventional HAMP waterfall includes interest rate reduction, followed by maturity extension to 40 years, followed by principal forbearance. However, some servicers offer principal forbearance prior to maturity extension, so some borrowers have large payments on the amount forbore due at the end of loan terms between 22 and 27 years. These large payments introduce variability into the average change in payment due.

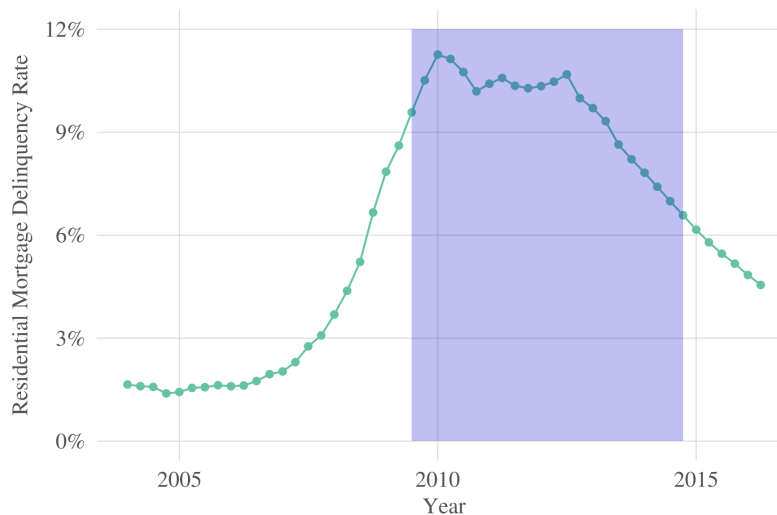
Figure 3: Modification Terms Summary

C. Interest at the rate of 2.0 % will begin to accrue on the New Principal Balance as of 1/1/2012 and the first new monthly payment on the New Principal Balance will be due on 1/15/2012. My payment schedule for the modified Loan is as follows:

Years	Interest Rate	Interest Rate Change Date	Monthly Principal and Interest Payment Amount	Estimated Monthly Escrow Payment Amount*	Total Monthly Payment*	Payment Begins On	Number of Monthly Payments
1-5	2.00%	01/01/2012	\$1,000.06	\$312.50, may adjust periodically	\$1,312.50, may adjust periodically	01/15/2012	60
6	3.00%	01/01/2013	\$1,143.71	\$312.50, May adjust periodically	\$1,455.21, May adjust periodically	01/15/2013	12
7	4.00%	01/01/2014	\$1,291.06	\$312.50, May adjust periodically	\$1,603.56, May adjust periodically	01/15/2014	12
8-35	5.00%	01/01/2015	\$1,444.00	\$312.50, May adjust periodically	\$1,756.50, May adjust periodically	01/15/2015	336

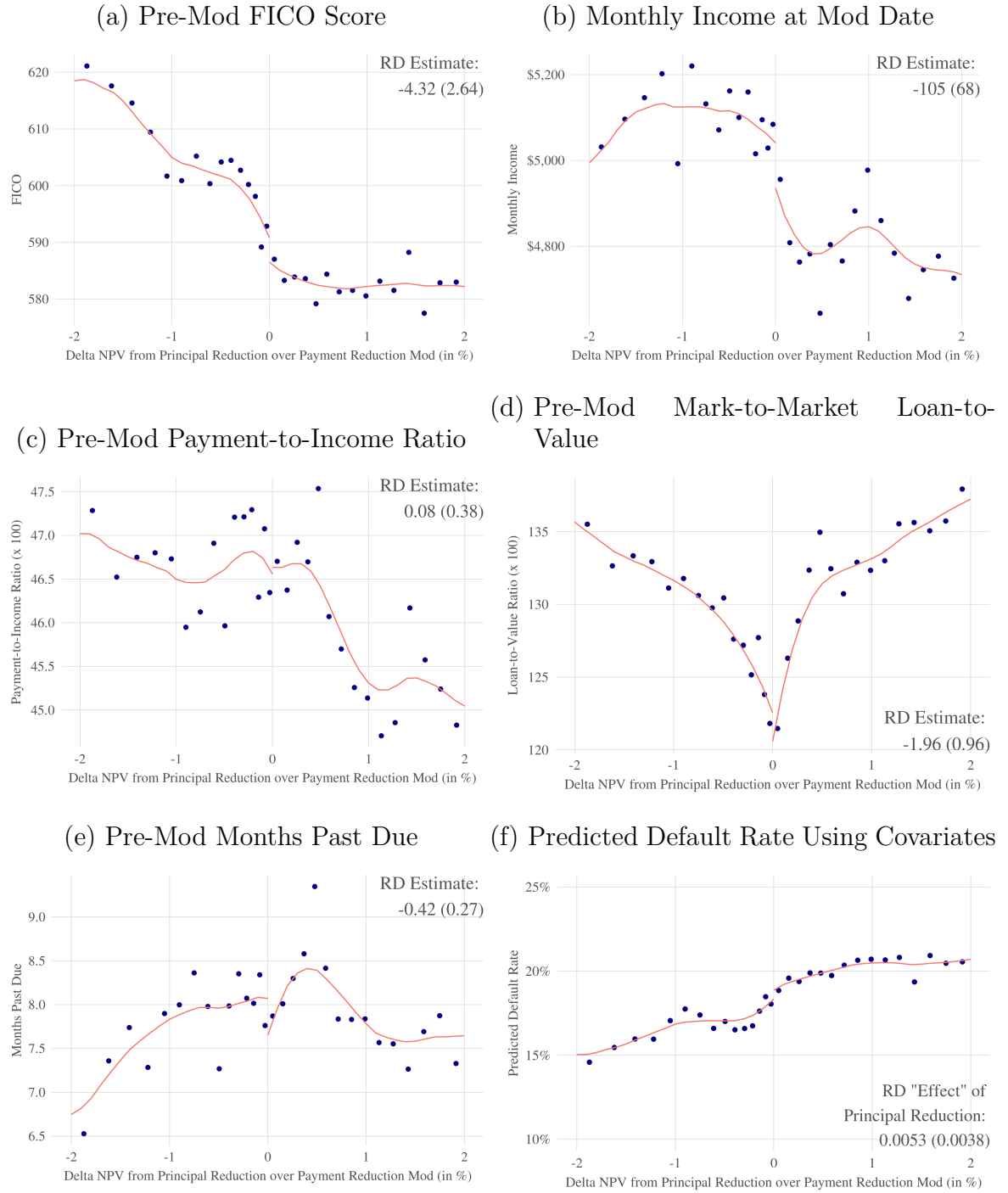
Notes: This figure shows the modified payment terms as explained to borrowers in the modification agreement, which they are required to sign. Example terms are shown for a mortgage with a post-modification principal balance of \$300,000, temporary interest rate of 2 percent, mortgage term of 35 years, and escrow payments equal to 1.5 percent of the property value (\$250,000).

Figure 4: Mortgage Delinquency over Time



Notes: This figure plots the share of U.S. residential mortgages more than 30 days delinquent as reported by the Federal Reserve Board. The shaded region denotes the period where borrowers in our principal reduction sample had their first pre-modification delinquencies.

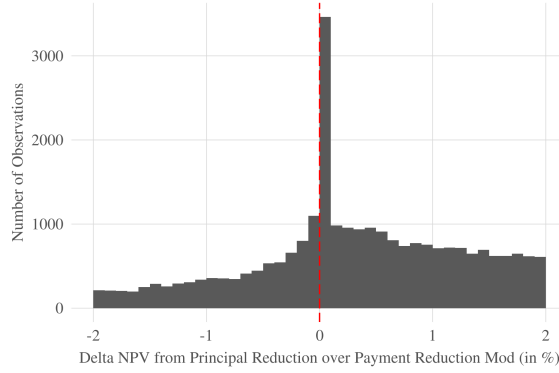
Figure 5: Pre-Modification Characteristics around Principal Reduction Discontinuity



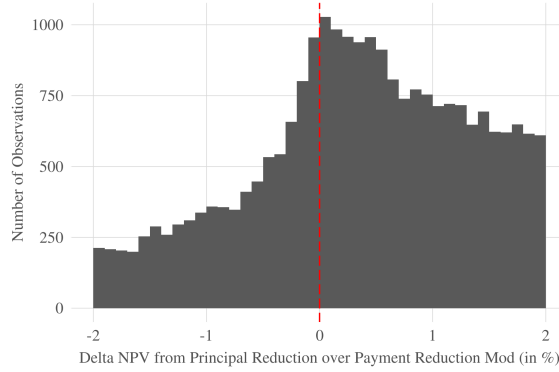
Notes: This figure shows average pre-treatment characteristics around the regression discontinuity cutoff in the matched HAMP credit bureau dataset. The horizontal axis shows the normalized predicted gain to lenders of providing principal reduction to borrowers from equation (1). The vertical axis in the first five panels shows borrower credit score, monthly income, the ratio of monthly mortgage payments to monthly income, the ratio of unpaid principal balance to the market value of the house (mark-to-market loan-to-value ratio), and the number of monthly mortgage payments the borrower is past due at application date. The final panel shows predicted default rates from a linear regression of default on the first five borrower characteristics. The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (2). See Appendix B.1.3 for details.

Figure 6: Borrower Density and Take-Up around Principal Reduction Discontinuity

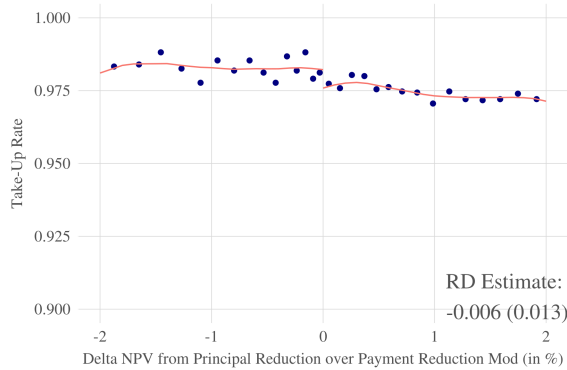
(a) Histogram of Running Variable



(b) Histogram of Running Variable Excluding Zeros

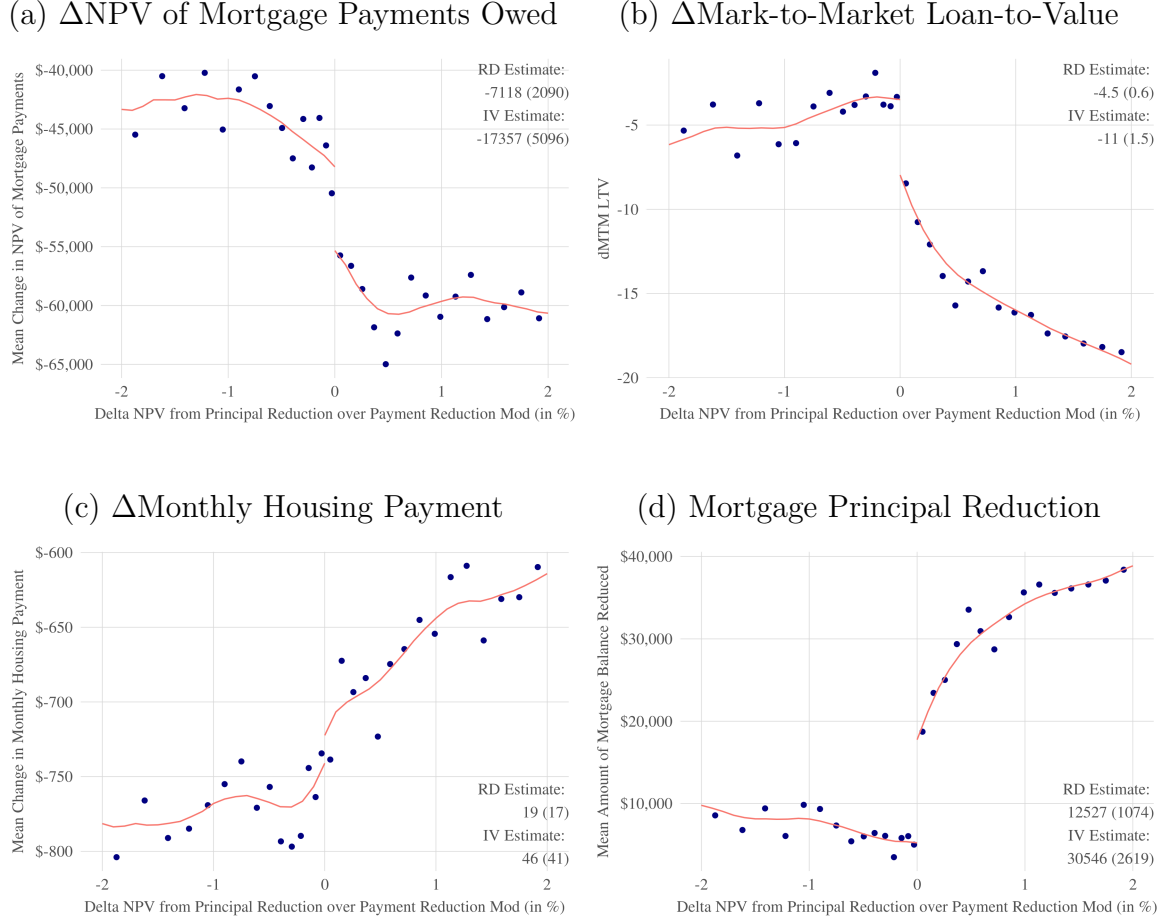


(c) Take-up Rate



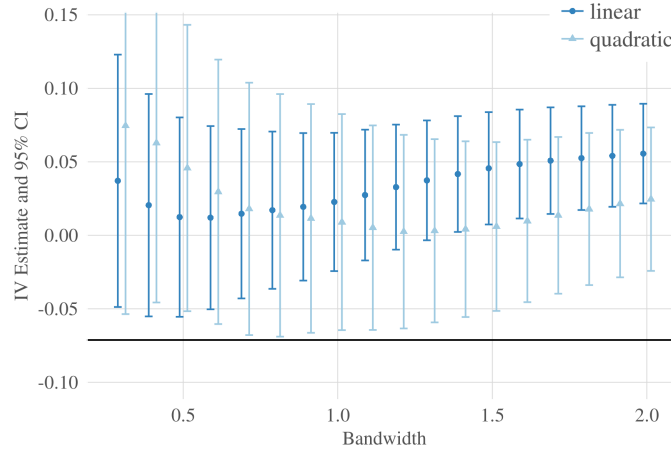
Notes: Panel (a) plots the histogram of the running variable from our regression discontinuity strategy in the matched HAMP credit bureau dataset. The horizontal axis shows the normalized predicted gain to lenders of providing principal reduction to borrowers from equation (1). HAMP program officers in the U.S. Treasury Department explain that the mass at exactly zero is due to data misreporting. Some servicers reported a single number as the calculation for both the payment reduction and principal reduction modifications, meaning that the estimated gains from principal reduction were calculated to be zero. Panel (b) plots the same histogram dropping observations exactly at zero, which is our analysis sample. Appendix B.1.3 discusses four additional arguments for why the mass at zero is unlikely to pose a challenge for the validity of the regression discontinuity research design. Panel (c) shows the take-up rate conditional on borrowers being offered a modification in the Treasury HAMP dataset.

Figure 7: Treatment Size around Principal Reduction Discontinuity



Notes: This figure shows the treatment size at the regression discontinuity cutoff in the matched HAMP credit bureau dataset. The horizontal axis shows the normalized predicted gain to lenders of providing principal reduction to borrowers from equation (1). Panel (a) shows the change in the net present value (NPV) of mortgage payments owed under the modified contract relative to the status quo mortgage contract, discounted at a 4 percent interest rate, panel (b) shows the change in the loan-to-value ratio, panel (c) shows the change in initial monthly housing payments, and panel (d) shows the average amount of principal reduction per borrower. The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (2). Construction of the IV estimate  $\hat{\tau}$  is described in Section II.B.

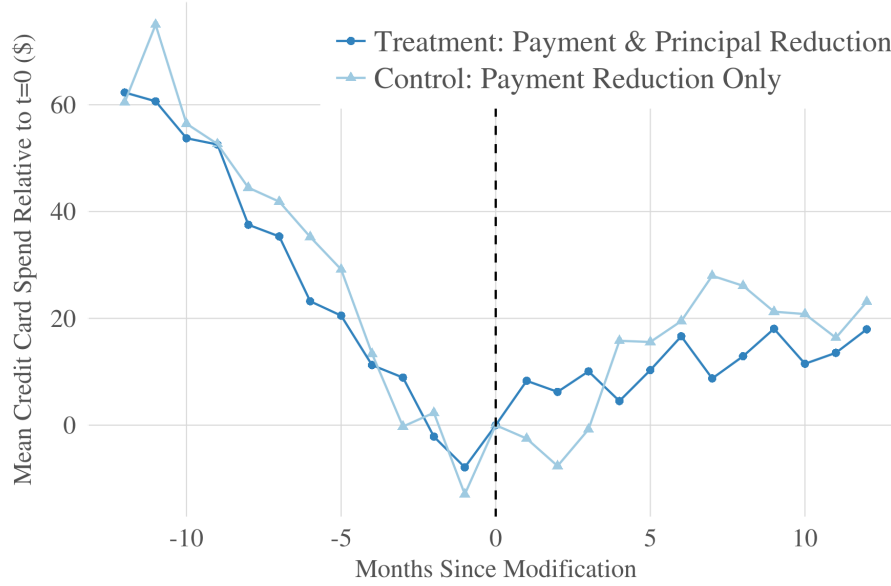
Figure 8: Regression Discontinuity Robustness to Alternative Bandwidths around Principal Reduction Discontinuity



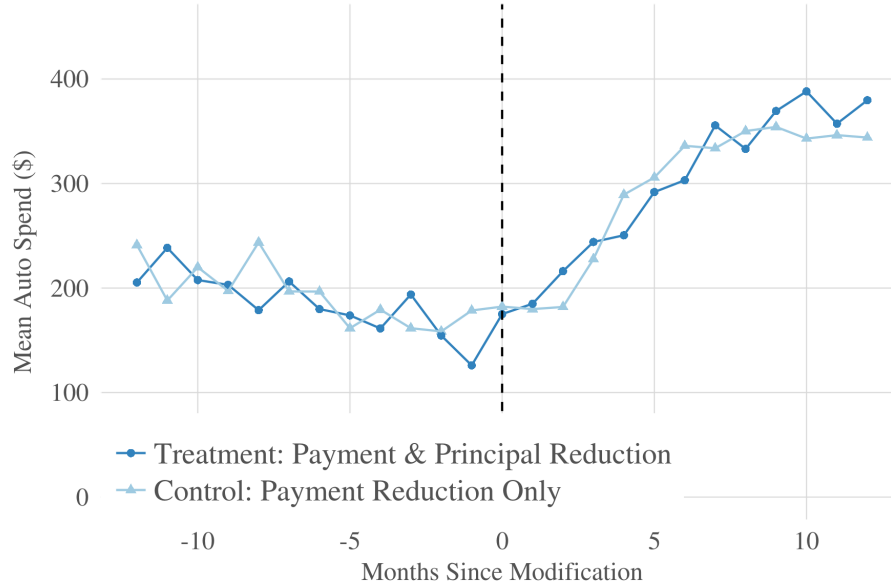
Notes: This figure shows the estimated impact of principal reduction on default under various specifications and bandwidths in the matched HAMP credit bureau dataset. Each line plots the IV estimate and associated 95 percent confidence interval from a local linear or quadratic regression on either side of the cutoff. The optimal bandwidths for the linear specification from Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014) are 0.61 and 0.63, respectively. The optimal bandwidths for a quadratic specification from Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014) are 0.80 and 0.67, respectively. The black horizontal line is the predicted impact of principal reduction on default from Treasury's redefault model.

Figure 9: Spending around Modifications with and without Principal Reduction

(a) Credit Card Spending around Modification – Normalized

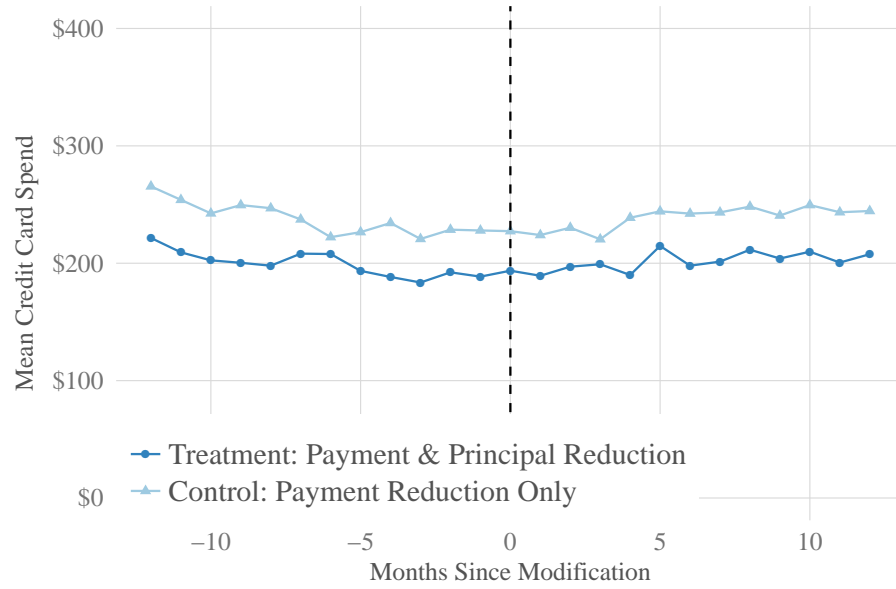


(b) Auto Spending around Modification



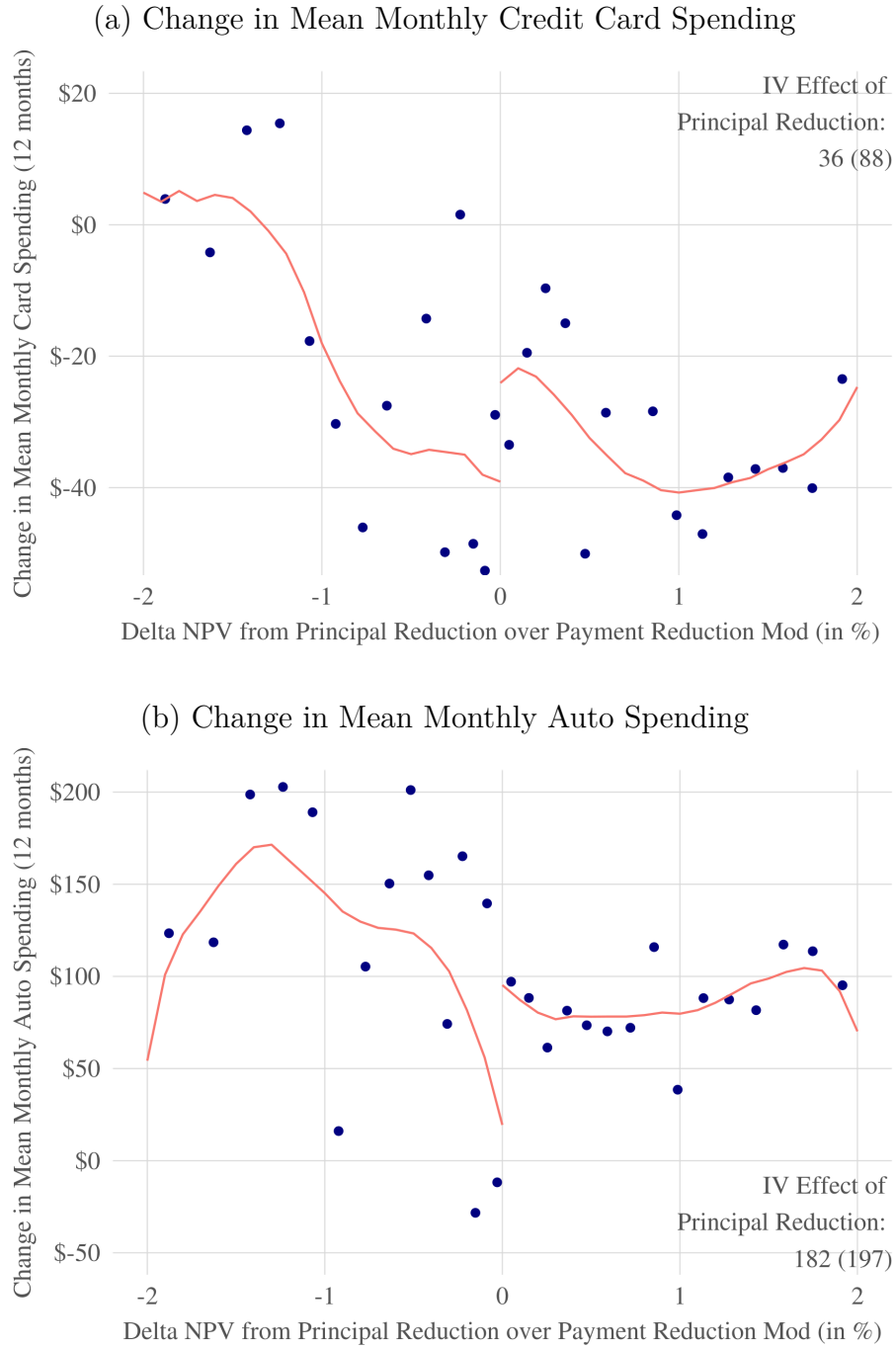
Notes: This figure shows the event study of monthly spending around modification for borrowers receiving each type of modification in the matched HAMP credit bureau dataset. The top panel plots credit card expenditure in dollars as measured from credit bureau records relative to the month of modification (discussed in Section I.A). The bottom panel shows the event study of monthly auto spending around modification. Auto spending is measured from new auto loans, as described in Section I.A. See Appendix Table 2 for sample summary statistics.

Figure 10: Spending around Modifications with and without Principal Reduction using Bank Data



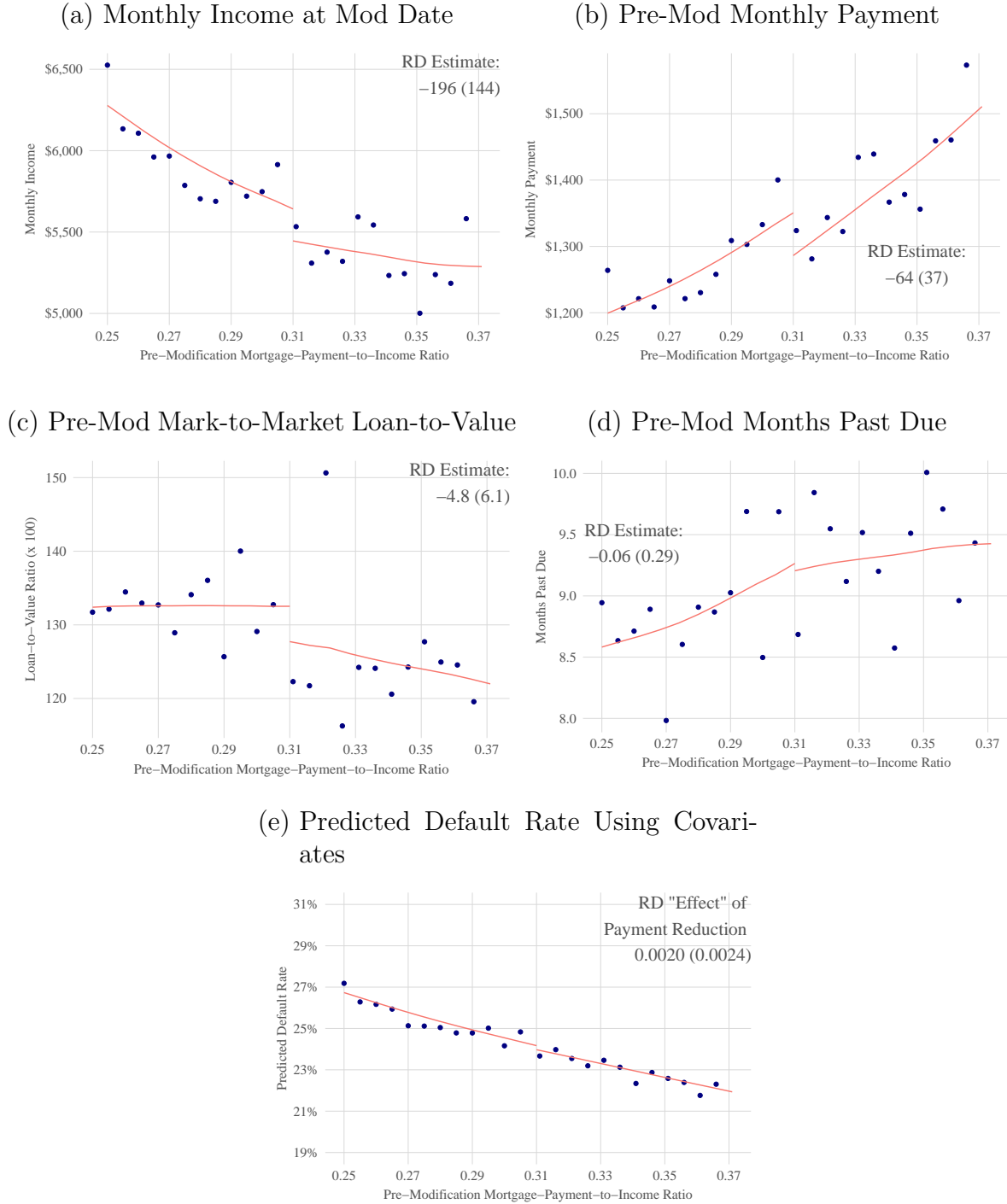
Notes: This figure shows the event study of monthly credit card expenditure around modification for borrowers receiving each type of modification in the JPMCI bank account dataset. For further details see Sections I.B and III.B.

Figure 11: Credit Card and Auto Spend around Principal Reduction Discontinuity



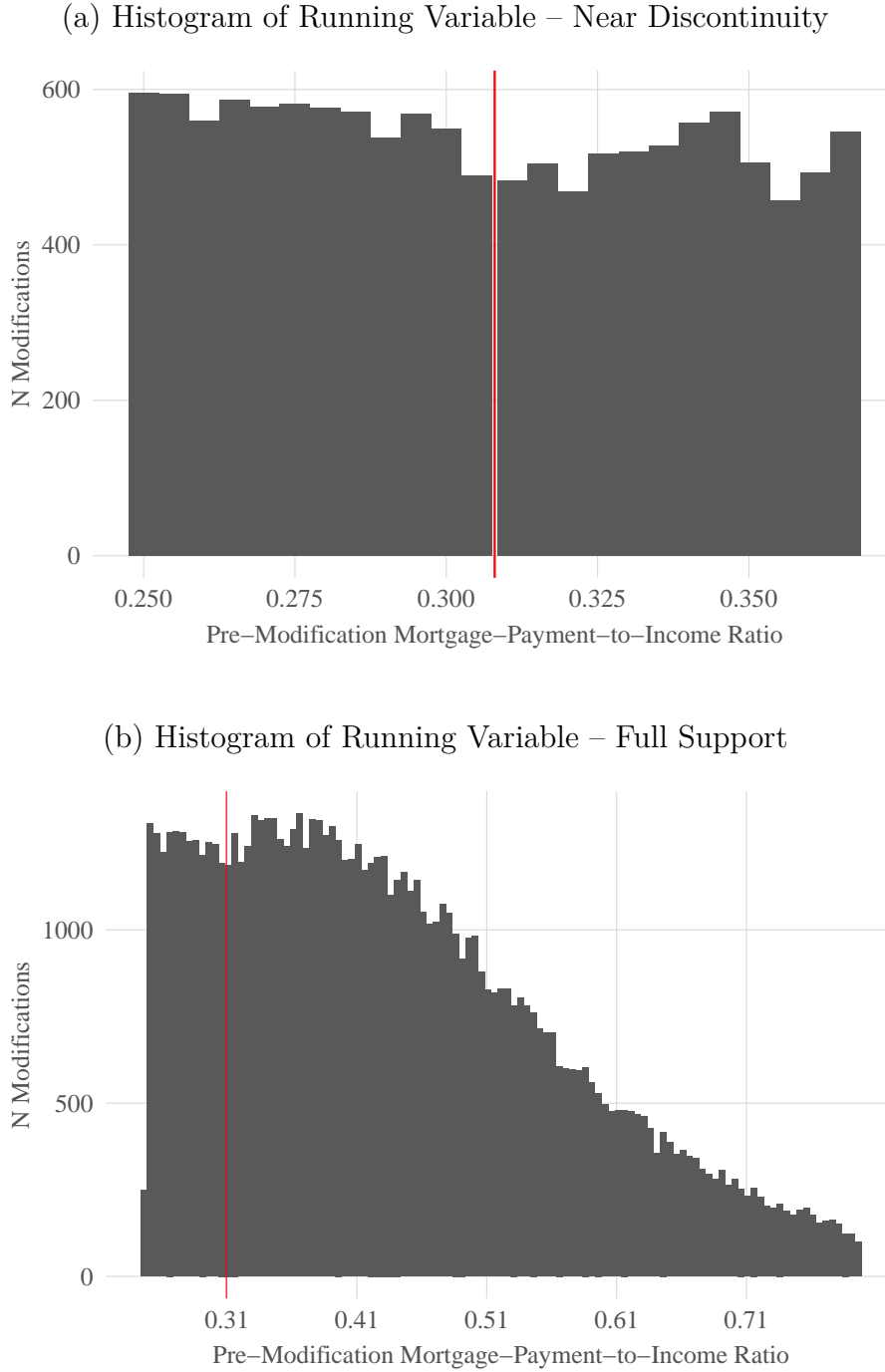
Notes: This figure shows the estimated impact of principal reduction on expenditure using the fuzzy regression discontinuity strategy in the matched HAMP credit bureau dataset. The horizontal axis shows the normalized predicted gain to lenders of providing principal reduction to borrowers from equation (1). The vertical axis on the top panel shows the average change in credit card expenditure between the 12 months before modification and the 12 months after modification. Credit card expenditure is measured from credit bureau records as discussed in Section I.A. The vertical axis in the bottom panel shows the average change in auto spending between the 12 months before modification and the 12 months after modification. Auto spending is measured from new auto loans, as described in Section I.A. The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. Construction of the IV estimate  $\hat{\tau}$  is described in Section II.B.

Figure 12: Pre-Modification Characteristics around Payment Reduction Discontinuity



Notes: This figure shows average pre-treatment characteristics around the 31 percent PTI regression discontinuity cutoff in the JPMCI bank dataset for non-GSE-backed loans. The horizontal axis shows pre-modification borrower PTI. The vertical axis in the first four panels shows monthly income, monthly payment, the ratio of unpaid principal balance to the market value of the house (mark-to-market loan-to-value ratio), and the number of months past due at modification date. The final panel shows predicted default rates from a linear regression of default on the first four borrower characteristics. The dots are conditional means for 12 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (4).

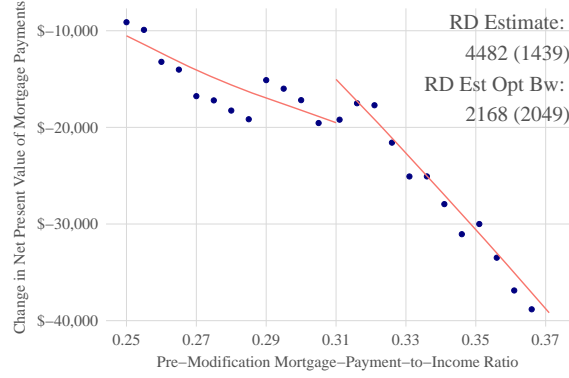
Figure 13: Borrower Density around the Payment Reduction Discontinuity



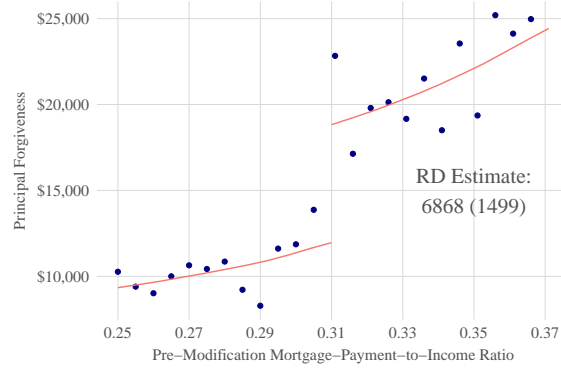
Notes: This figure plots the histogram of the running variable from our 31 percent PTI regression discontinuity strategy in the JPMCI bank dataset for non-GSE-backed loans. The horizontal axis shows pre-modification borrower PTI. The top panel shows borrowers in the main analysis sample. This sample is restricted to pre-modification PTI ratio between 25 percent and 37 percent (dropping the 241 observations between 31.0 percent and 31.1 percent), pre-modification terms 30 years or less, and fixed rate loans. This is our main analysis sample. The bottom panel shows the density for the full sample.

Figure 14: Treatment Size around Payment Reduction Discontinuity

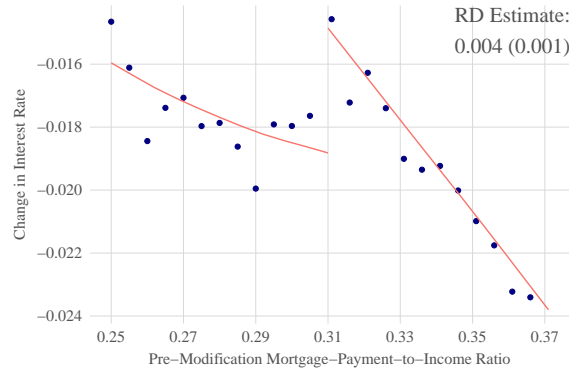
(a)  $\Delta$ Net Present Value of Payments Owed



(b) Mortgage Principal Forgiveness



(c)  $\Delta$ Interest Rate



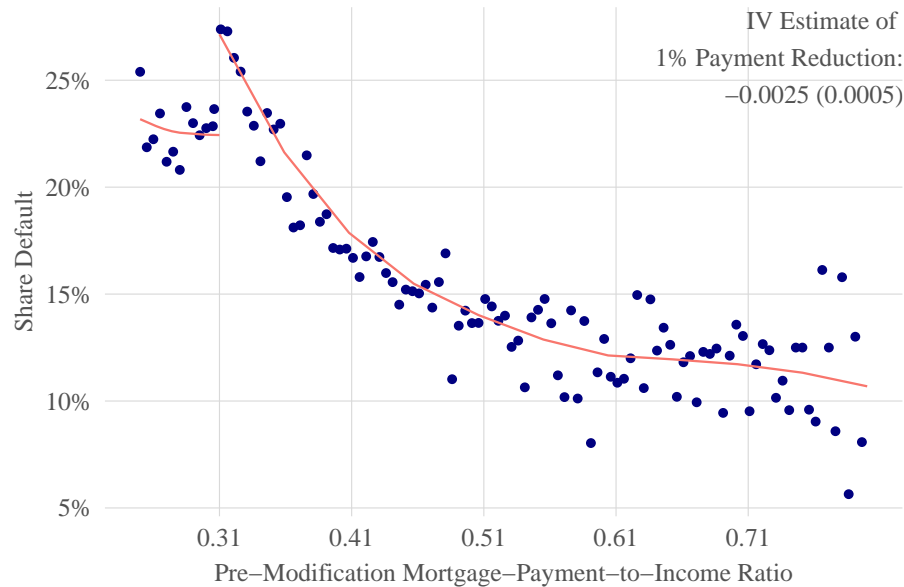
Notes: This figure describes the treatment in terms of long-term obligations around the 31 percent PTI discontinuity in the JPMCI bank dataset for non-GSE-backed loans. The dots are conditional means for 12 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (4) using the IK-optimal bandwidth for delinquency of 0.061. Panel (a) shows the change in the NPV of payments owed under the mortgage contract before and after modification. The IK-optimal bandwidth for this outcome variable is 0.039 and the label also includes a second RD estimate using this optimal bandwidth of 0.039. Panel (b) shows mortgage principal forgiveness. Panel (c) shows the change in the interest rate.

Figure 15: Effect of Payment Reduction on Default: Robustness to Broader Sample

(a) First Stage – Change in Mortgage Payment from Modification

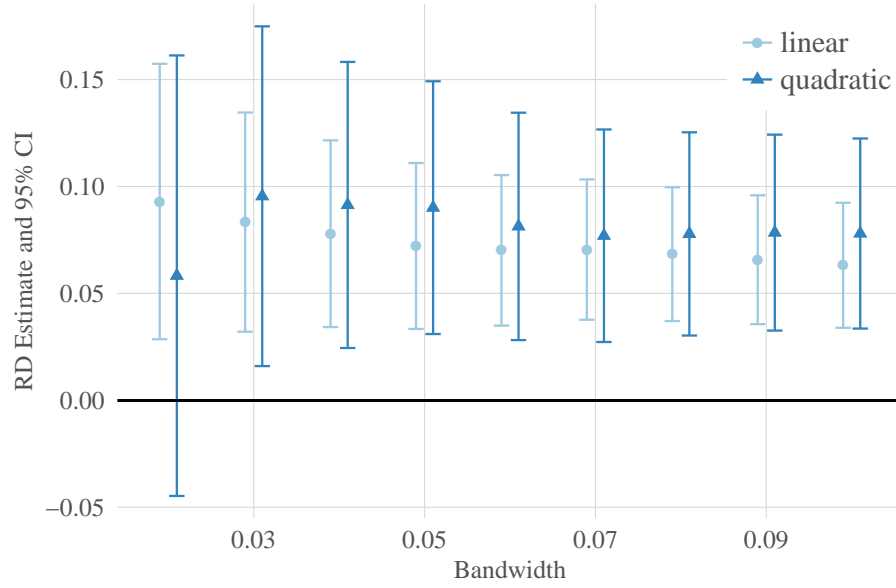


(b) Reduced Form -- Mortgage Default



Notes: This figure shows the estimated effect of payment reduction on default using the 31 percent PTI regression discontinuity in the JPMCI bank dataset for a broader sample of non-GSE-backed loans. It includes loans with pre-modification terms greater than 30 years, loans with variable interest rates, and borrowers with PTI between 31 percent and 31.1 percent, all of which are dropped in the main analysis. The top panel plots the first stage, with payment reduction on the vertical axis and borrower PTI on the horizontal axis. The dots are conditional means for equally spaced bins on each side of the cutoff. Bins are four times narrower than in Figure 5a in order to visually capture the loans between 31 percent and 31.1 percent with a separate dot. All other plot details are the same as Figure 5.

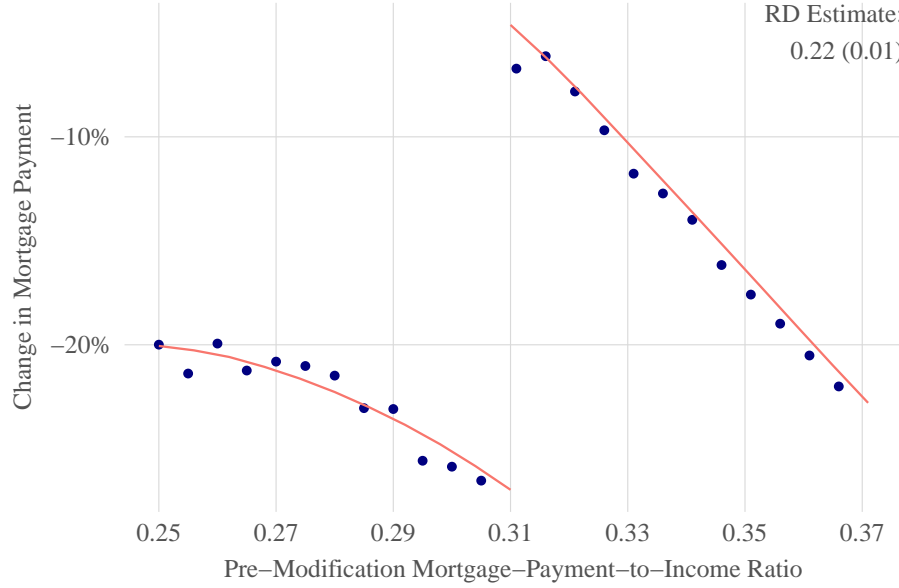
Figure 16: Effect of Payment Reduction on Default Using Alternative Bandwidths



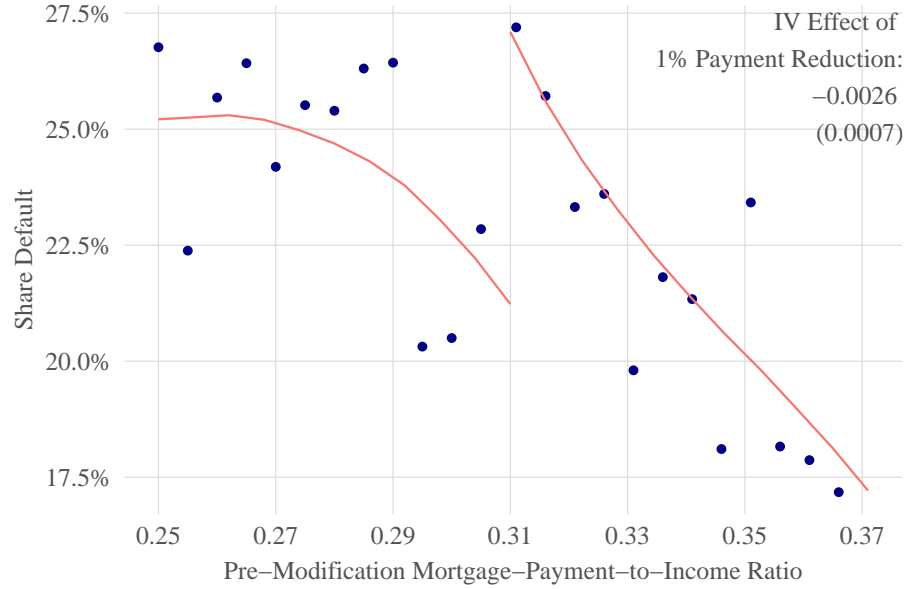
Notes: This figure plots the estimated reduced form jump in default and the associated 95 percent confidence interval at the 31 percent PTI regression discontinuity cutoff calculated using alternative bandwidths in the JPMCI bank dataset for non-GSE-backed loans. Our primary specification uses a bandwidth of 0.06. The plotting convention here differs slightly from Appendix Figure 8, which reports an IV estimate for a variety of bandwidths. Here we instead report the reduced form estimate because the estimated first stage in Figure 5a (and therefore the IV estimate reported in Figure 5b) are somewhat sensitive to bandwidth choice.

Figure 17: Effect of Payment Reduction on Default for GSE-Backed Loans

(a) First Stage -- Change in Mortgage Payment from Modification



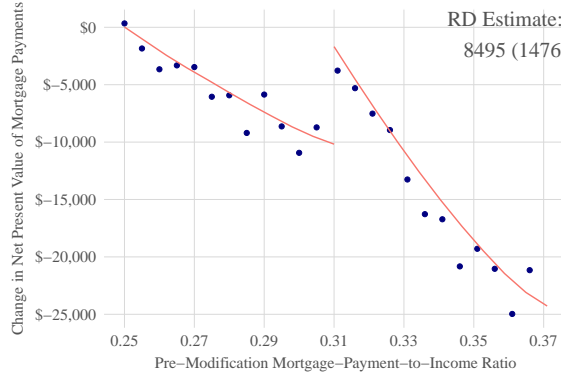
(b) Reduced Form -- Mortgage Default



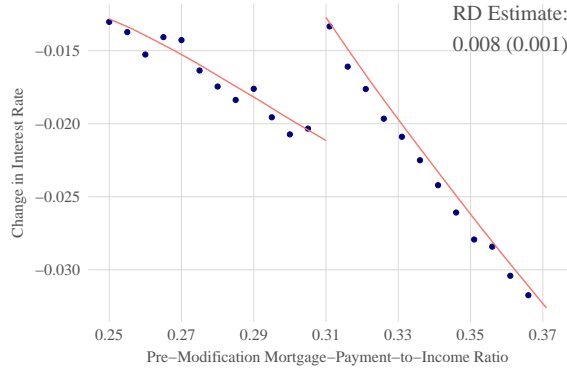
Notes: This figure evaluates the impact of payment reduction on default using a regression discontinuity at the 31 percent payment-to-income (PTI) in the JPMCI bank dataset for GSE-backed loans. The horizontal axis shows borrower PTI. The dots are conditional means for 12 equally spaced bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (4). The top panel plots mean payment reduction and the bottom panel plots the default rate on the vertical axis, which is defined as being 90 days delinquent at any point within two years of the modification date. Construction of the IV estimate  $\hat{\tau}$  is described in Section IV.B.

Figure 18: Treatment Size around Payment Reduction Discontinuity for GSE-Backed Loans

(a)  $\Delta$ Net Present Value of Payments Owed



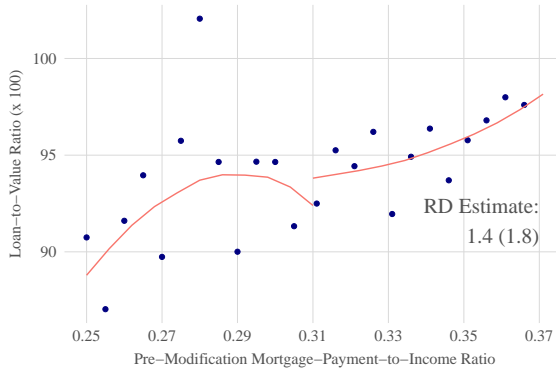
(b)  $\Delta$ Interest Rate



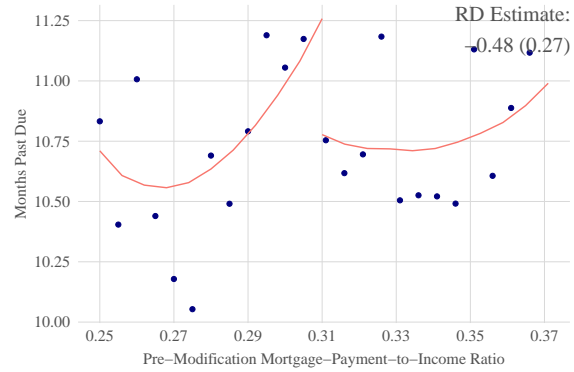
Notes: This figure describes the treatment in terms of long-term obligations around the 31 percent PTI discontinuity in the JPMCI bank dataset for GSE-backed loans. The dots are conditional means for 12 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (4). Panel (a) shows the change in the NPV of payments owed under the mortgage contract for all loans. Panel (b) shows the change in the interest rate. We do not include principal forgiveness because the GSEs did not offer mortgage principal forgiveness for either private modifications or HAMP modifications.

Figure 19: Pre-Modification Characteristics around Payment Reduction Discontinuity for GSE-Backed Loans

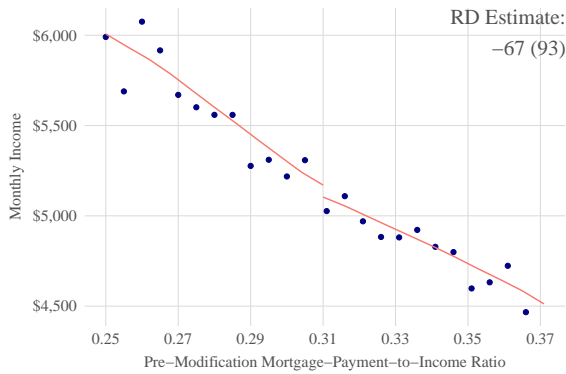
(a) Pre-Mod Mark-to-Market Loan-to-Value



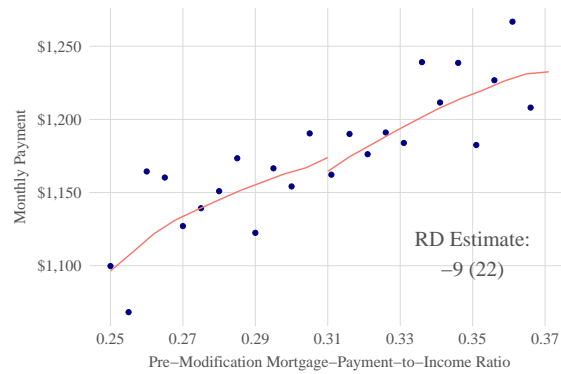
(b) Pre-Mod Months Past Due



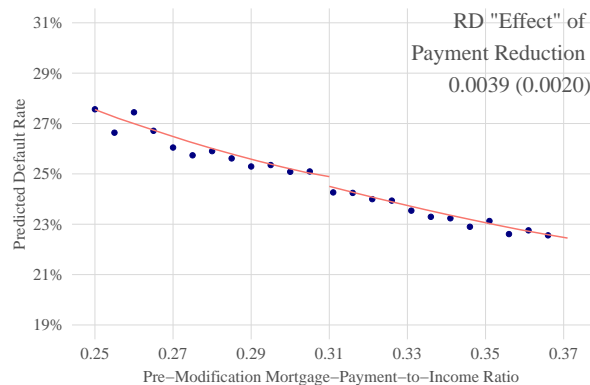
(c) Monthly Income at Mod Date



(d) Pre-Mod Monthly Payment

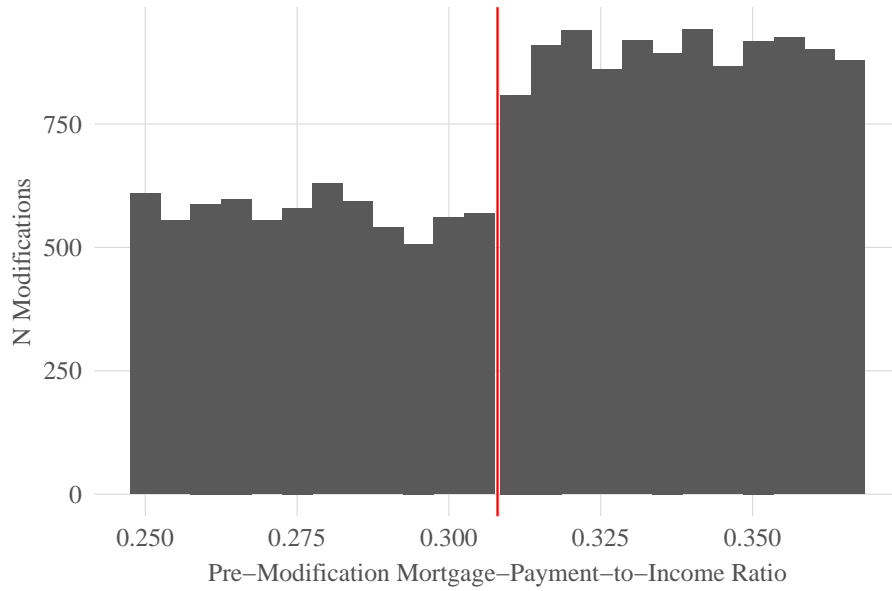


(e) Predicted Default Rate Using Covariates



Notes: This figure shows average pre-treatment characteristics around the 31% PTI regression discontinuity cutoff in the JPMCI bank dataset for GSE-backed loans. The horizontal axis shows pre-modification borrower PTI. The vertical axis in the first four panels shows the ratio of unpaid principal balance to the market value of the house (mark-to-market loan-to-value ratio), the number of months past due at modification date, monthly income, and monthly payment. The final panel shows predicted default rates from a linear regression of default on the first four borrower characteristics. The dots are conditional means for 12 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (4).

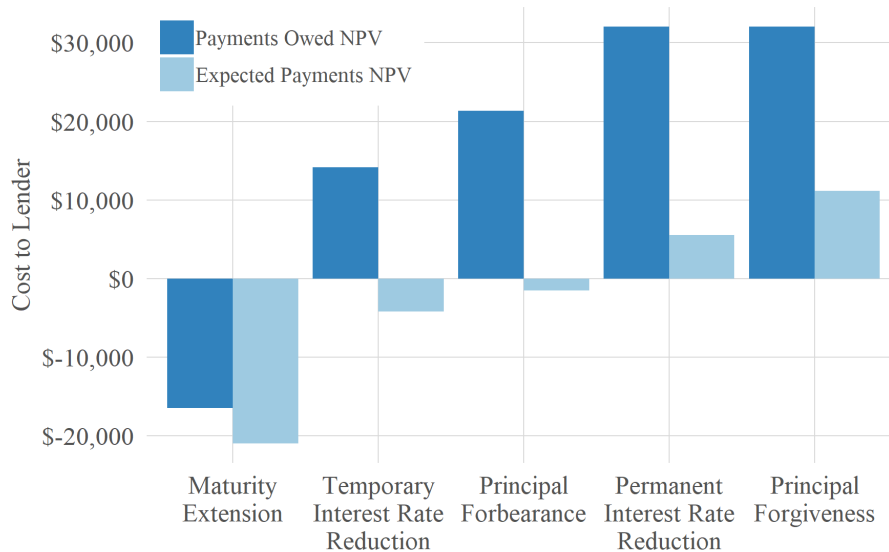
Figure 20: Borrower Density around the Payment Reduction Discontinuity for GSE-Backed Loans



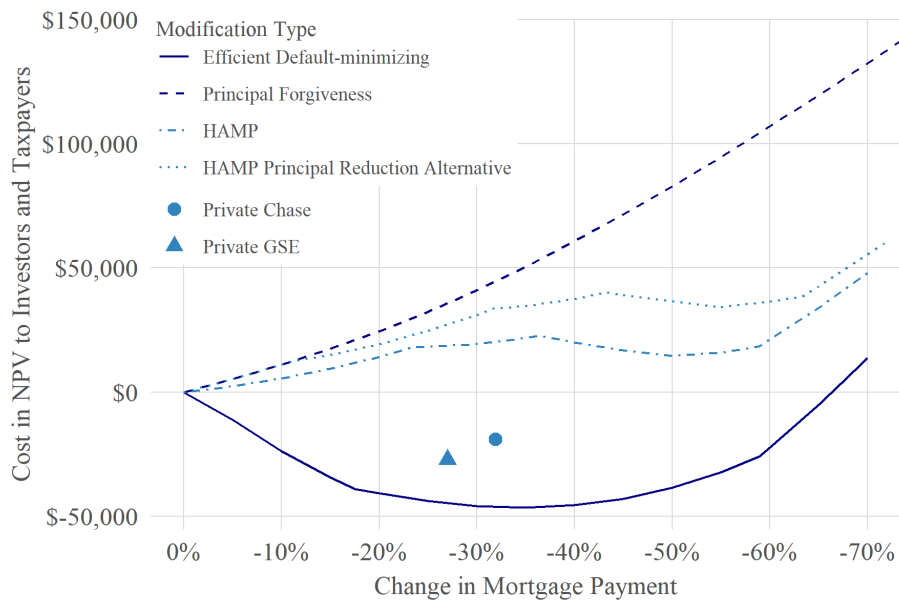
Notes: This figure plots the histogram of the running variable from our 31 percent PTI regression discontinuity strategy in the JPMCI bank dataset for GSE-backed loans. The horizontal axis shows pre-modification borrower PTI. See Section IV.C for details on why there are more borrowers to the right of 0.31.

Figure 21: Efficient Default-Minimizing Modifications

(a) Cost to Lender of Reducing Payments by 10% for Each Modification Step

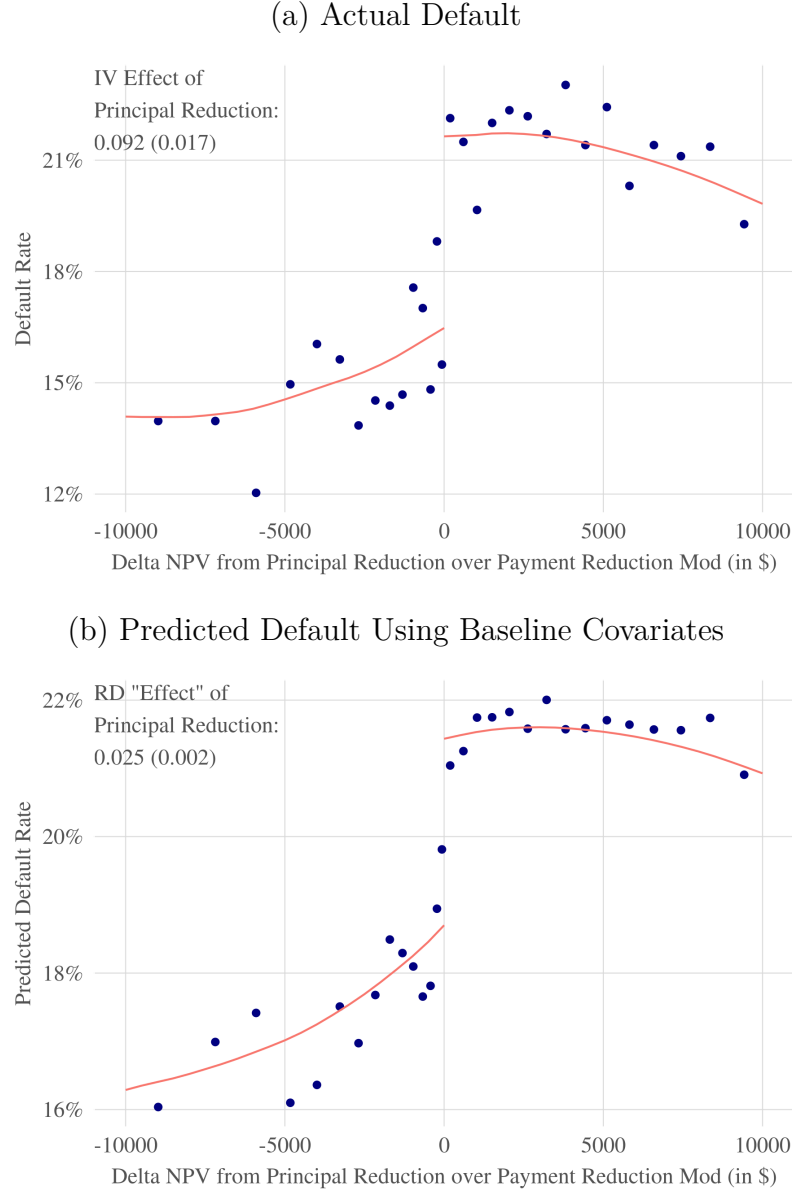


(b) Expected Payments NPV Cost of Payment Reduction for Various Sequences of Modification Steps



Notes: This figure shows the cost of different mortgage modifications for an illustrative mortgage with the average characteristics of loans at the HAMP eligibility cutoff (a 6.7 percent fixed interest rate, a 23-year term, and a mean unpaid balance of \$248,000). Panel (a) shows the cost of reducing payments by 10 percent for five different possible modification steps. The light blue bars show the change in the NPV of *payments owed* under the mortgage contract and the dark blue bars show the change in the NPV of *expected payments* to the lender incorporating the yield curve and the impact of modification on default and prepayment risk. Panel (b) shows the expected payments NPV cost of various modification strategies. The last two programs in the legend are a dot—rather than a line—because they target a specific amount of payment reduction. See Appendix C.4 for details.

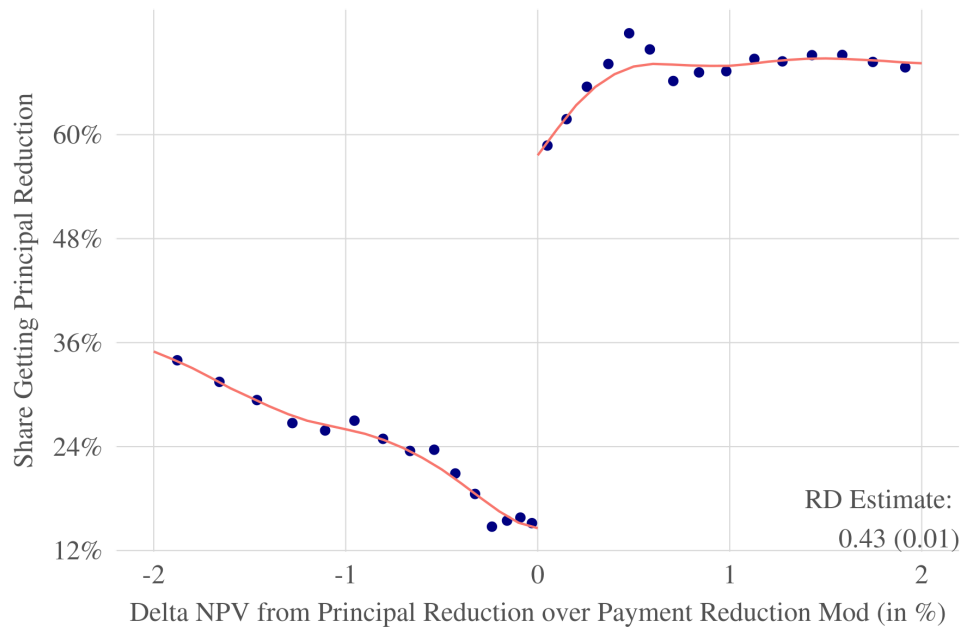
Figure 22: Actual and Predicted Mortgage Default Around Principal Reduction Discontinuity, Unnormalized Running Variable



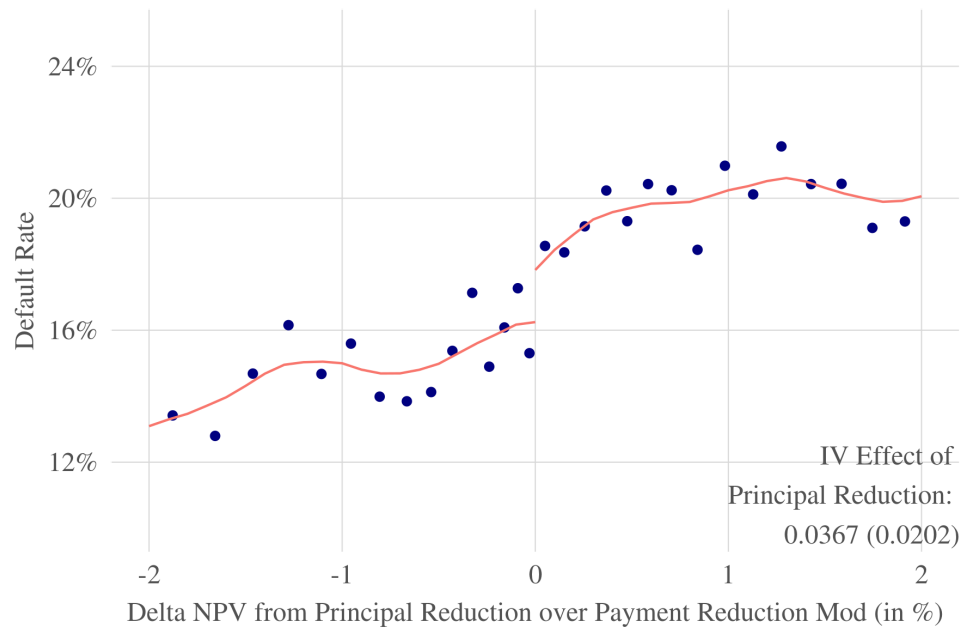
Notes: This figure shows the reduced form and predicted default rates based on pre-determined borrower covariates in the matched HAMP credit bureau dataset using an unnormalized version of the running variable. The horizontal axis shows the unnormalized predicted gain to lenders of providing principal reduction to borrowers, i.e. only the numerator from equation (1). The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. Panel (a) plots the default rate. Construction of the IV estimate  $\hat{\tau}$  is calculated as described in Section II.B, except the unnormalized running variable is used. Panel (b) plots predicted default rates from a linear regression of default on observed borrower characteristics (FICO score, monthly income, payment-to-income ratio, loan-to-value ratio, and months past due prior to modification). The RD estimate is the jump in predicted values at the cutoff, corresponding to an estimate of the numerator in equation (2). This specification fails the balance test using baseline covariates; predicted default is *higher* among the group that gets more principal reduction. Thus, this specification is uninformative about the effect of principal reduction on default.

Figure 23: Impact of Principal Reduction on Default, Unmatched Sample

(a) First Stage – Receive Principal Reduction

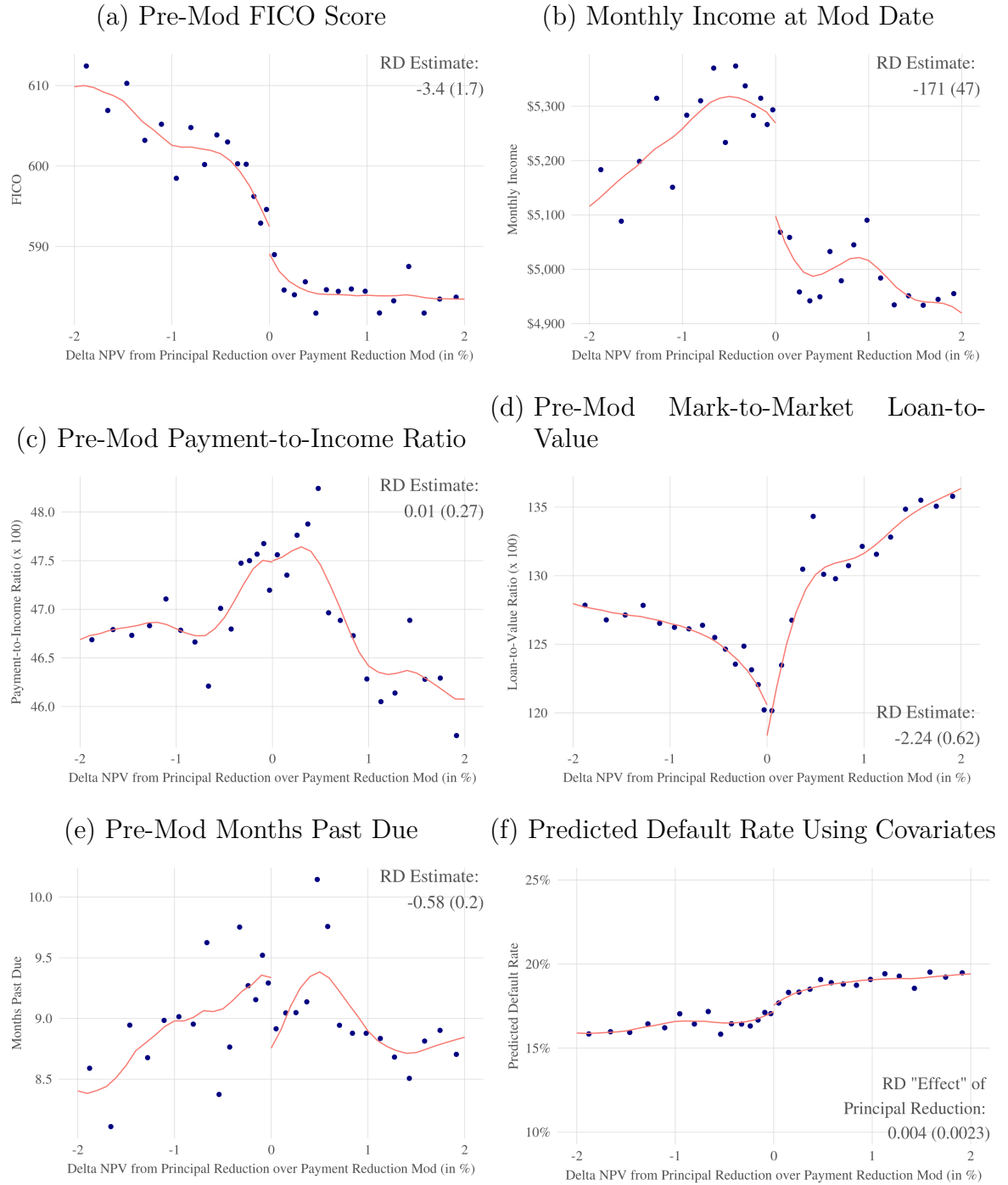


(b) Reduced Form -- Mortgage Default



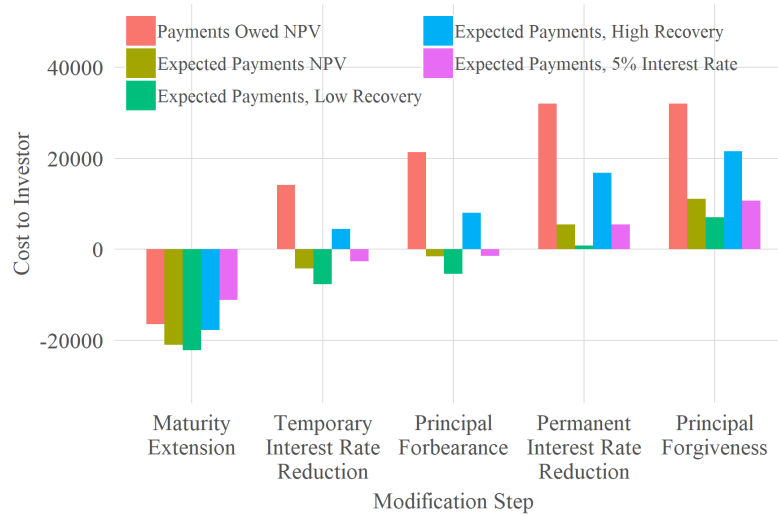
Notes: This figure replicates Figure 2a using the full HAMP public file. This file includes loans which do not have a match in the TransUnion data. See Appendix B.1.3 for details.

Figure 24: Pre-Modification Characteristics around Principal Reduction Discontinuity, Unmatched Sample



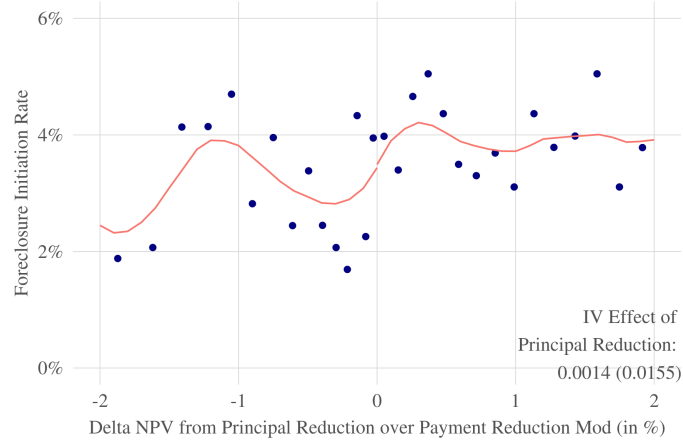
Notes: This figure replicates Appendix Figure 5 using the full HAMP public file. This file includes loans which do not have a match in the TransUnion data. See Appendix B.1.3 for details.

Figure 25: Effect of 10% Payment Reduction on NPV: Robustness



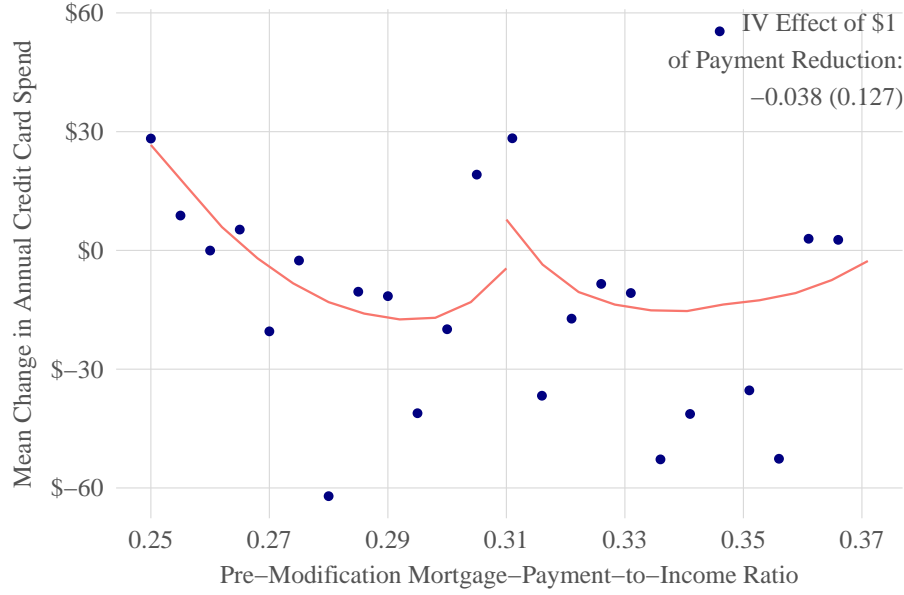
Notes: This figure shows the impact of a 10 percent payment reduction on the NPV of the loan to the investor under various assumptions. The red and yellow bars reproduce Appendix Figure 21a. The yellow bars assume a 39 percent self-cure rate on post-modification defaults and a 56 percent loss if the loan is liquidated. The green bars assume a self-cure rate of 18 percent and a liquidation loss of 61 percent. The blue bars assume a 61 percent self-cure rate and a 48 percent liquidation loss. See Appendix C.3.1 for the data sources for each of these assumption. The purple bars use the same assumptions as baseline, except a 5 percent initial interest rate.

Figure 26: Effect of Principal Reduction on Foreclosure Initiation



Notes: This figure shows the effect of principal reduction on foreclosure initiation in the matched HAMP credit bureau dataset. The foreclosure initiation rate is plotted on the vertical axis and the normalized predicted gain to lenders of providing principal reduction is on the horizontal axis. The dots are conditional means for 15 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. Construction of the IV estimate  $\hat{\tau}$  is described in Section II.B.

Figure 27: Effect of Payment Reduction on Credit Card Expenditure Using the Payment Reduction Discontinuity

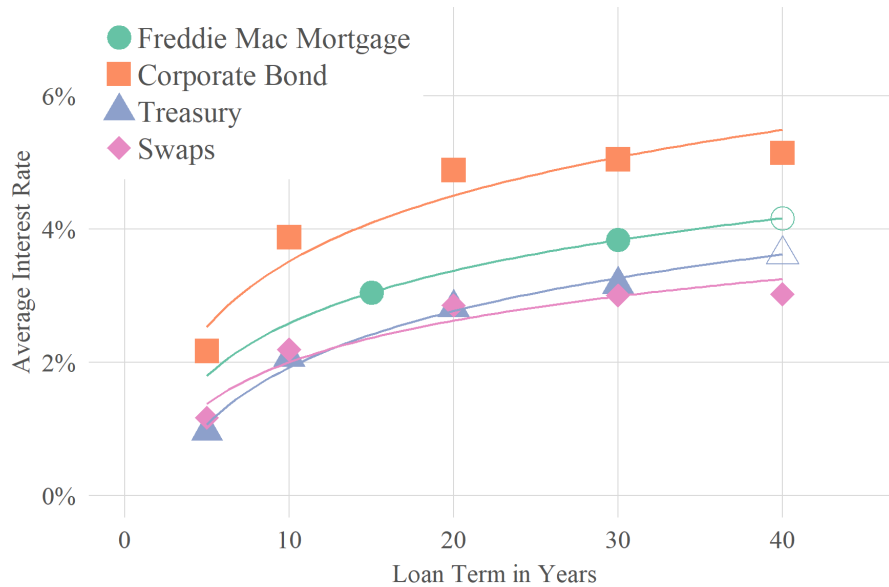


Notes: This figure shows the reduced form of the estimated impact of payment reduction on credit card expenditure using the 31 percent PTI regression discontinuity strategy in the JPMCI bank dataset. The dots are conditional means for 12 bins on each side of the cutoff. The line shows the predicted value from a local linear regression estimated separately on either side of the cutoff. Construction of the IV estimate  $\hat{\tau}$  is described in Section IV.B. This strategy is unable to detect economically meaningful changes in expenditure.

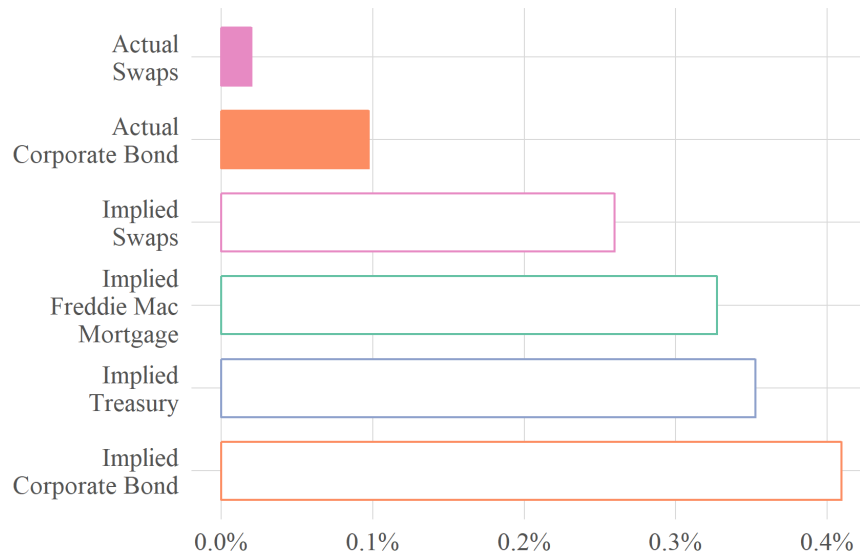


Figure 28: Projected 40-Year Mortgage Interest Rates

(a) Actual and Projected Loan Interest Rates

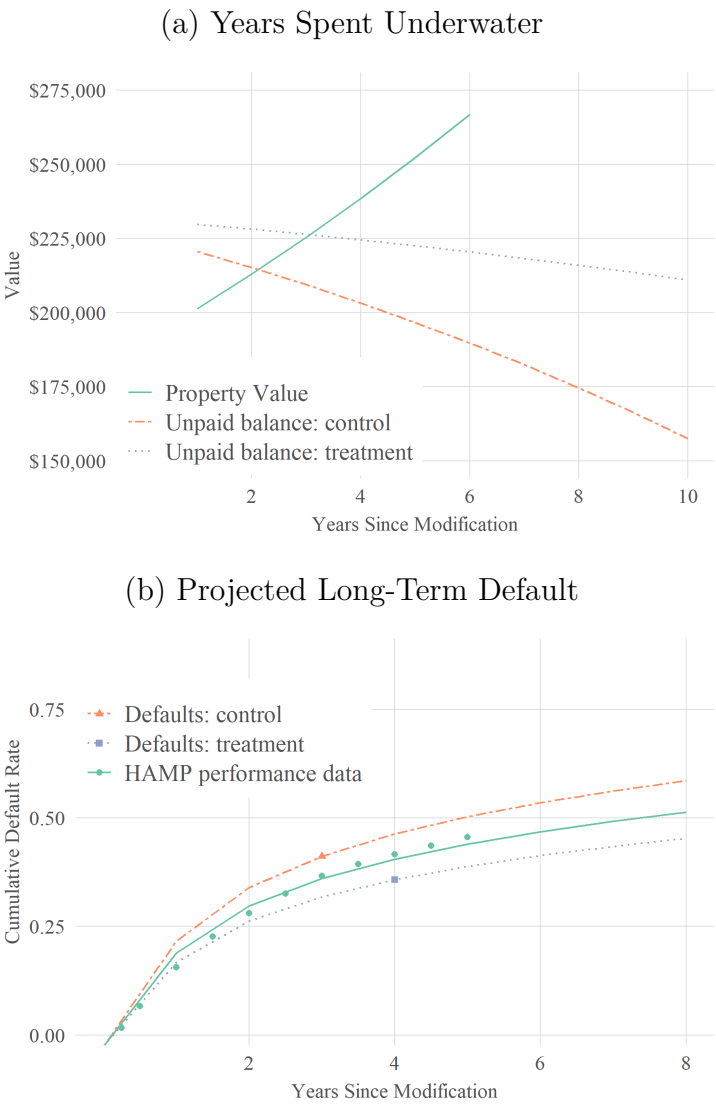


(b) Actual and Modeled Spread Between 30- and 40-year Rates



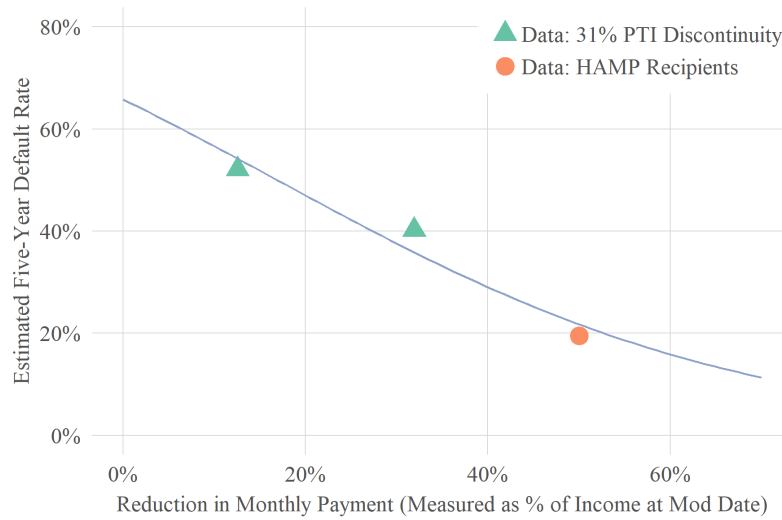
Notes: Panel (a) shows interest rates for various loan terms. Solid dots are data, lines are the best fit of  $y = \log(x)$  to the solid dots, and hollow dots are projections of 40-year interest rates. Green dots show mortgage rates from the Freddie Mac Conforming Loan Survey, red squares show corporate bond spot rates, blue triangles show Treasury note rates, and purple diamonds show fixed-for-floating interest rate swaps. Panel (b) shows estimates of the interest premium for a 40-year loan over a 30-year loan using four methodologies. It shows a premium of 10 basis points using actual corporate bond spot rates in a solid bar, a premium of 32 basis points extrapolated from shorter-term Freddie Mac mortgage rates in a hollow bar, a premium of 34 basis points extrapolated from shorter-term Treasury rates in a hollow bar, and a premium of 2 basis points using actual swap rates in a solid bar. (For reference, the panel also shows the extrapolated premium using corporate bond rates and swap rates.) See Appendix C.3.1 for calculation details and description of implied swaps and implied corporate bonds.

Figure 29: Default Risk Arising From Maturity Extension Through Additional Time Underwater



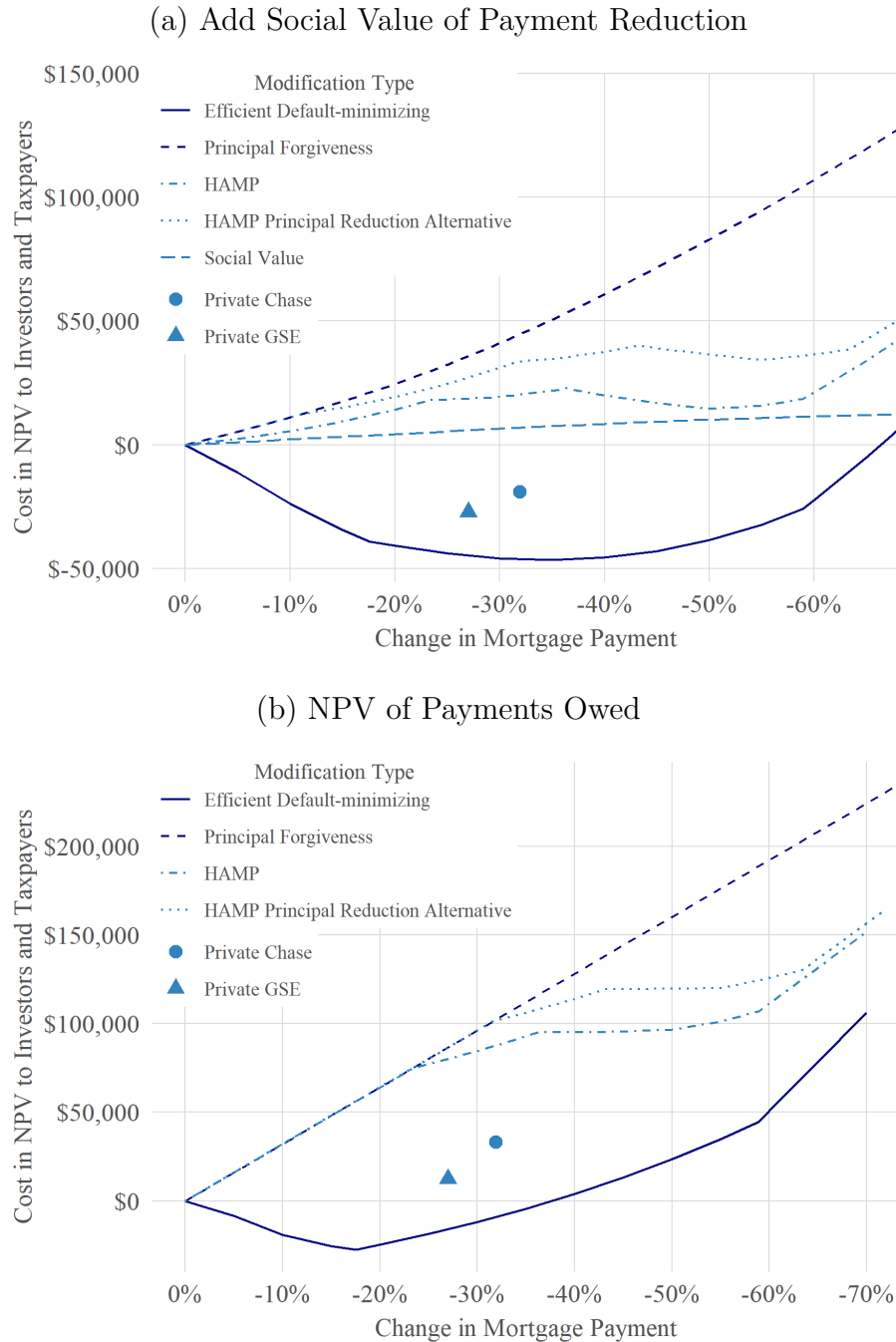
Notes: See Appendix C.3.2.

Figure 30: Amount of Payment Reduction and Default



Notes: This figure shows estimated five-year default rates for various amounts of payment reduction. The green triangles are from the two sides of the discontinuity in Figure 5b and the orange circle is borrowers with PTI of 55 percent from Appendix Figure 15. We take the two-year default rates and multiply them by 1.62, which is the ratio of five-year default rates to two-year default rates among HAMP modifications performed in 2010. The line is a best fit of a logit model to the three data points.

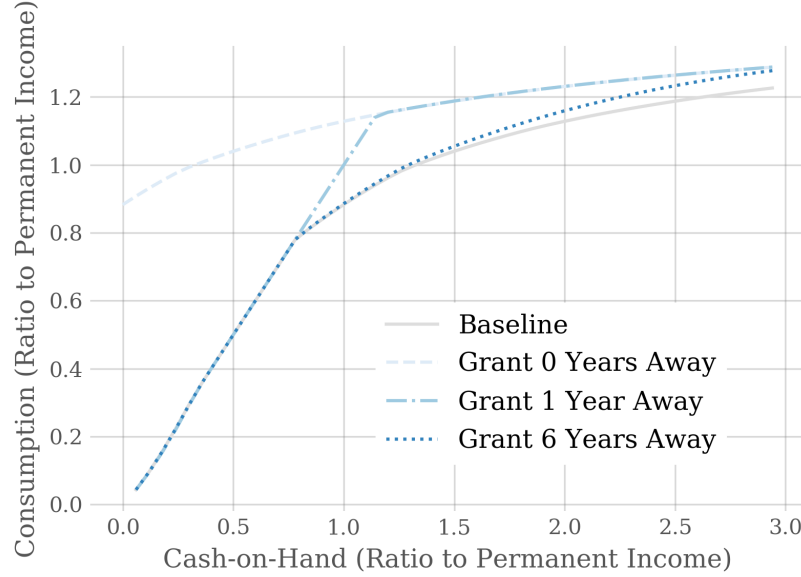
Figure 31: NPV Cost of Payment Reduction for Various Sequences of Modification Steps



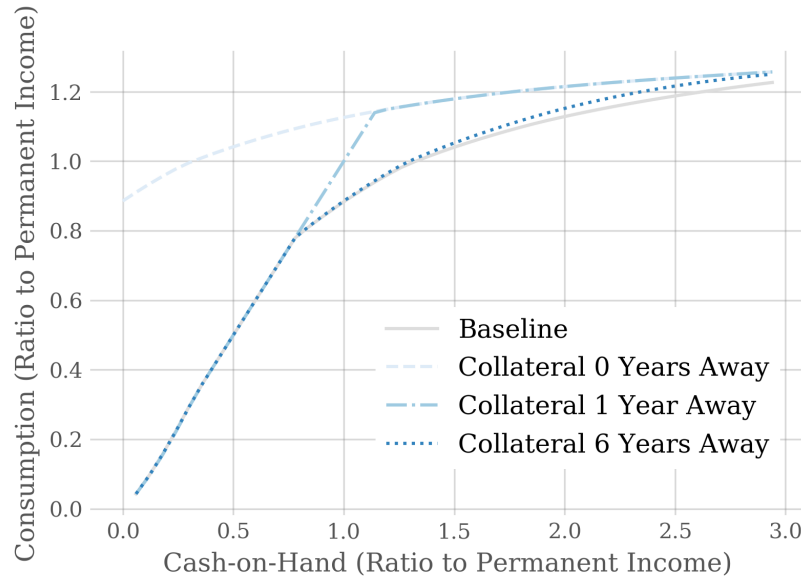
Notes: The top panel takes Figure 21b and adds a line reflecting the social value of payment reduction, assuming a \$51,000 social cost per foreclosure as estimated in U.S. Department of Housing and Urban Development (2010). The bottom panel recomputes Figure 21b using the NPV of payments owed.

Figure 32: Consumption Functions with Cash-on-Hand and Collateral Grants at Various Dates

(a) Consumption Function out of Future Cash-on-Hand

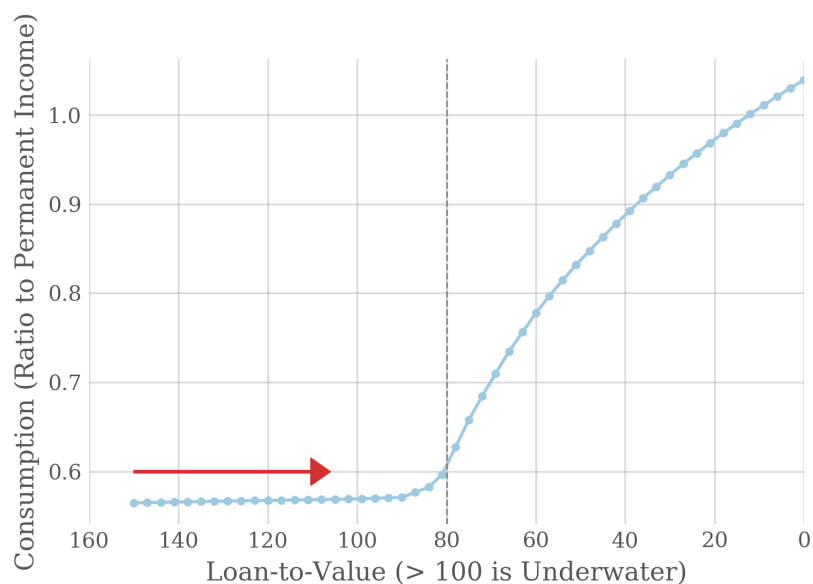


(b) Consumption Function out of Future Collateral



Notes: The top panel plots the consumption function out of cash-on-hand under various alternative scenarios from the model described in Appendix D. Both the horizontal and vertical axes are measured relative to permanent income. The baseline case considers a household with no home equity (and hence no current borrowing capacity). The lines show the consumption functions in the current period when the household is granted one year's worth of permanent income in the current period, in one year, and in six years. The bottom panel shows the equivalent consumption functions for the case when the household is granted collateral, rather than wealth, at various dates.

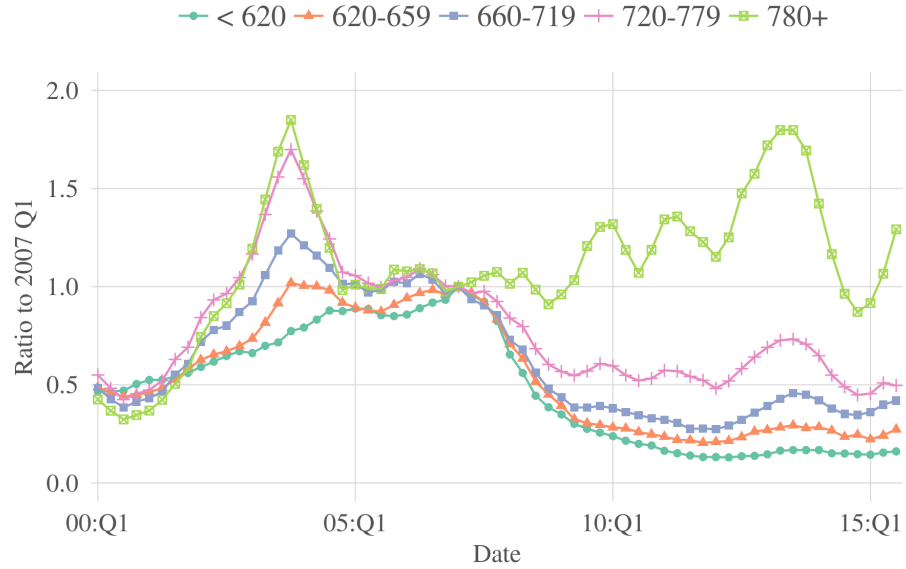
Figure 33: Consumption Function Out of Principal Forgiveness



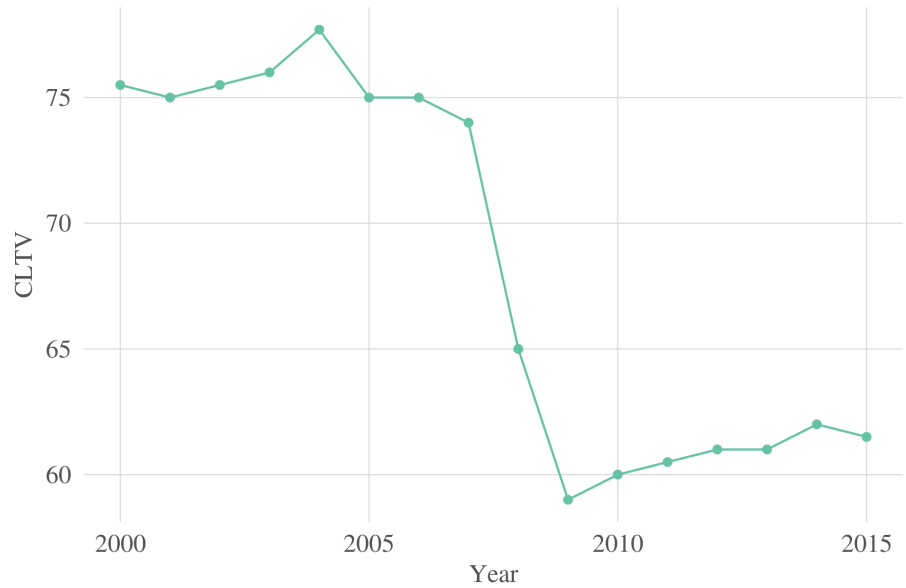
Notes: This figure plots the consumption function out of principal reduction. We begin borrowers at a 150 percent loan-to-value (LTV) ratio and give increasing amounts of principal reduction as necessary to hit a given LTV ratio. To mimic our empirical setting, mortgage payments for households who have not defaulted are fixed for five years, after which payments fall according to the new mortgage balance. The red arrow shows the treatment for the average borrower in the government program. This figure assumes a collateral constraint of 80 percent of LTV and cash-on-hand (assets + annual income) equal to 85 percent of permanent income. See Appendix D for details.

Figure 34: Mortgage Credit Availability

(a) Mortgage Originations by Credit Score



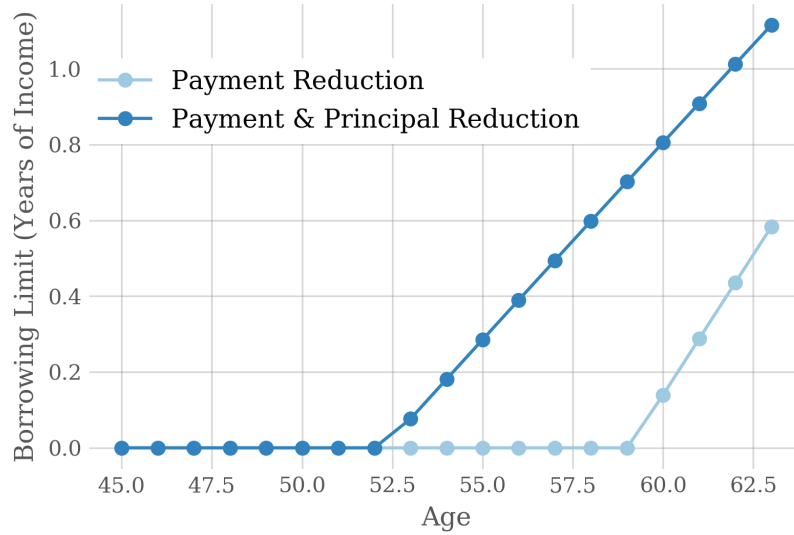
(b) Combined Loan-to-Value for New Home Equity Lines of Credit



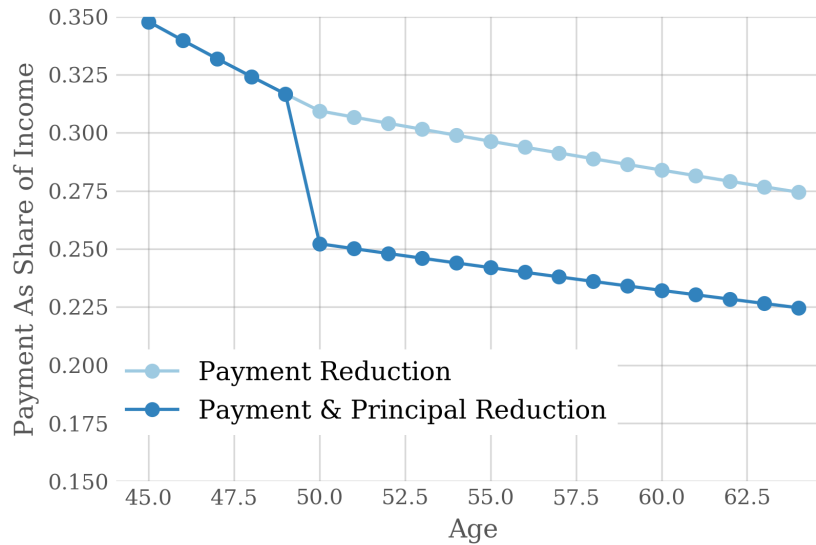
Notes: The top panel plots mortgage origination by borrower credit score from the New York Fed Consumer Credit Panel (Federal Reserve Bank of New York (2015a)). This includes first mortgages, second mortgages, and home equity installment loans. The bottom panel plots the average combined loan-to-value (CLTV) ratio for new home equity lines of credit (HELOCs). It is a reproduction of the first figure on page 4 of Corelogic (2016).

Figure 35: Effect of Modeled Principal Reduction on Borrowing Limits and Mortgage Payments

(a) Borrowing Limits



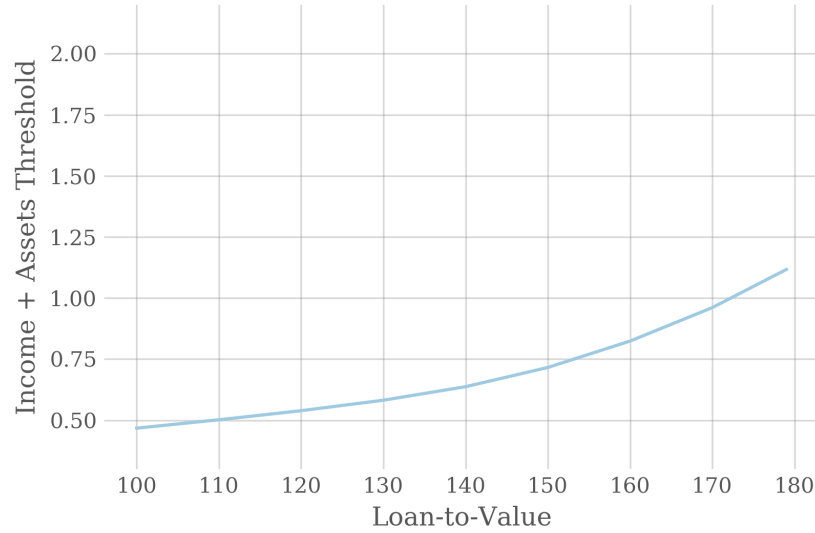
(b) Mortgage Payments



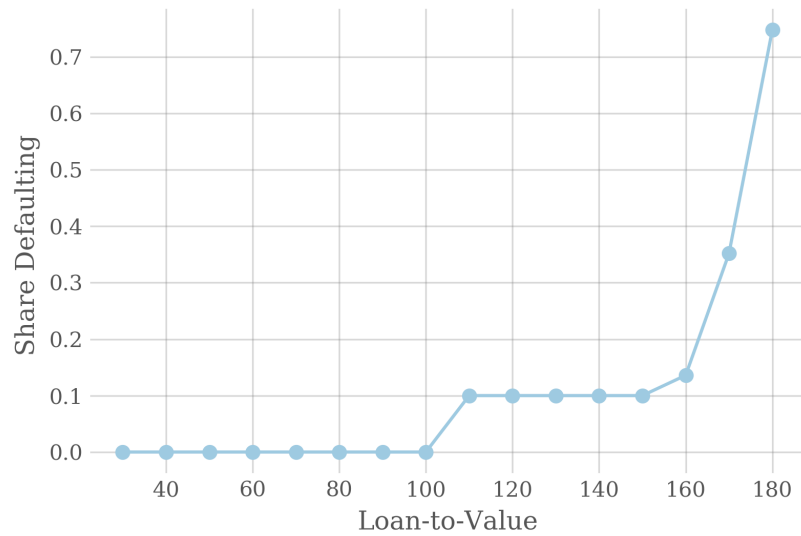
Notes: This figure shows the effect of the modeled principal reduction policy on borrowing limits and mortgage payments. We assume homeowners receive modifications at age 45. We set initial LTV equal to 150. For our treatment group, we then reduce their mortgage balance by \$70,000, bringing them to an LTV of 106 in the first year. To mimic our empirical setting, mortgage payments for households who have not defaulted are fixed for five years, after which payments fall according to the new mortgage balance.

Figure 36: Default and Mortgage Debt Levels

(a) Default if  $\text{Income} + \text{Assets} < \text{Threshold}$

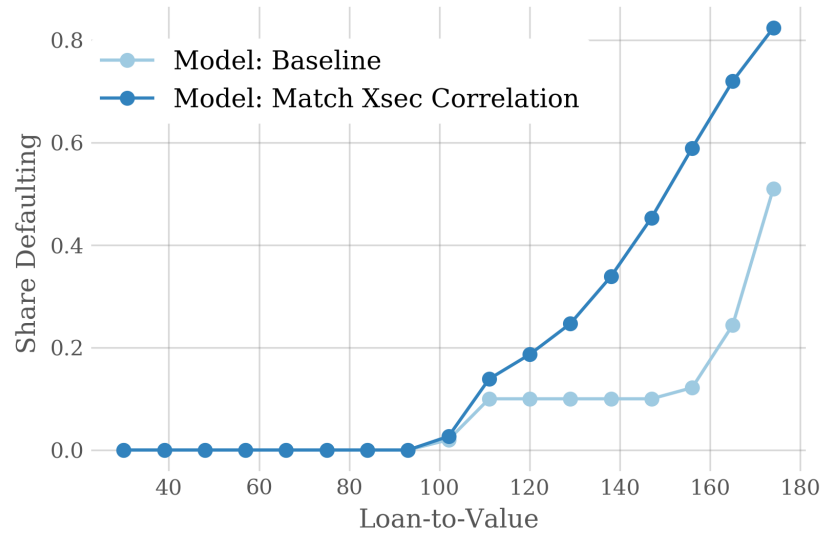


(b) Default Rate



Notes: The top panel plots the cutoff thresholds for borrower default decisions. The vertical axis is relative to permanent income. The line shows the baseline assumptions as described in equation (8). For borrowers with a given LTV ratio, the line shows the cash-on-hand (income plus assets) threshold below which borrowers decide to exercise their default option. The bottom panel plots default rates by LTV ratio. Default rates are calculated by taking the default thresholds shown in the top panel and integrating over the distribution of income shocks described in equation (9).

Figure 37: Default with Heterogeneous Utility Cost of Default



Notes: This figure plots default rates by LTV ratio in the model under alternative parameterizations. The LTV is moved according to the same policy exercise described in the notes to Appendix Figure 35a. The baseline parameterization corresponds to that in Appendix Table 5. The “Match Xsec Correlation” series assumes a distribution of default costs across the population instead of a constant default cost.

Table 1: HAMP Summary Statistics Pre- and Post-Credit Bureau Match

	Pre-Match		Post-Match		Normalized
	Mean	SD	Mean	SD	Difference
Income	54,549	24,638	51,186	23,612	-0.14
Home Value	197,080	123,940	178,250	114,753	-0.15
Loan to Value Ratio	143	44	144	46	0.00
Monthly Mortgage Payment	1,716	875	1,551	789	-0.19
Monthly Payment to Income Ratio	0.48	0.12	0.47	0.12	-0.11
Mortgage Interest Rate	0.063	0.020	0.063	0.020	-0.00
Mortgage Term Remaining (Years)	25.9	4.6	25.8	4.7	-0.01
ARM (d)	0.49	0.50	0.46	0.50	-0.05
Months Past Due	10.9	11.5	9.5	10.5	-0.12
Credit Score	584	74	582	75	-0.03
Male (d)	0.57	0.50	0.56	0.50	-0.02
Age	48.8	10.8	48.6	10.9	-0.01
Monthly Payment Reduction (\$)	737	544	641	483	-0.18
Monthly Payment Reduction (%)	42	20	41	20	-0.07
Principal Forgiveness Amount	53,046	70,413	46,025	62,173	-0.10
Received Principal Forgiveness (d)	0.59	0.49	0.59	0.49	-0.01
Post Modification LTV	134	34	135	35	0.03
Post Modification DTI	0.30	0.04	0.30	0.04	-0.05
Post Modification Default (d)	0.201	0.400	0.201	0.400	0.00
N	221,487		105,365		

Notes: This table shows characteristics for all HAMP borrowers who were underwater and evaluated for both modification types during our sample window. Our regression discontinuity and panel difference-in-differences analyses each use different subsets of the matched sample. The normalized difference in the final column is the difference in means divided by the pre-match standard deviation. All values are before modification unless otherwise noted. (d) indicates a dummy variable.

Table 2: Summary Statistics for Difference-in-Differences Analysis

	Payment Reduction		Payment and Principal Reduction	
	Mean	SD	Mean	SD
Principal Forgiveness Amount	13,257	36,479	80,460	64,675
NPV Payment Reduction	63,000	60,050	97,397	74,112
Monthly Payment Reduction (\$)	676	478	674	505
Monthly Payment Reduction (%)	38.5	18.3	41.9	21.4
Loan to Value Ratio	143	43	145	48
Post Modification LTV	148	34	122	29
Monthly Payment to Income Ratio	0.47	0.11	0.47	0.12
Post Modification DTI	0.31	0.03	0.30	0.05
Income	55,597	23,753	52,924	23,692
Credit Score	598	83	578	72
Home Value	205,275	118,748	174,994	111,218
Monthly Mortgage Payment	1,725	803	1,593	780
Mortgage Interest Rate	0.061	0.018	0.064	0.019
Mortgage Term Remaining (Years)	26.2	4.5	26.1	4.4
Male (d)	0.58	0.49	0.55	0.50
Age	48.3	11.2	48.8	10.8
N	35,485		33,457	

Notes: This table shows summary statistics for the matched HAMP credit bureau sample analyzed in the panel difference-in-differences research design discussed in Section III. The sample includes underwater borrowers who are observed in the credit bureau records one year before and after modification and report positive credit card expenditure in at least one month during this window. All variables are before-modification unless otherwise noted. (d) indicates a dummy variable.

Table 3: Impact of Principal Reduction on Credit Card Expenditure Using Bank Data

This table reports difference-in-differences estimates of the effect of principal reduction on credit card expenditure in the JPMCI bank account dataset. The coefficient of interest, *Treatment*, is the estimated change in the difference between outcomes of mortgages receiving modifications with and without principal reduction during the year after modification. All specifications include fixed effects for modification type and months since modification. Controls include pre-modification loan characteristics (LTV, principal balance), property value, and LTV at origination. The sample includes all HAMP borrowers with a mortgage and a credit card with Chase who are observed one year before and after modification. The dependent variable mean in the year before modification is reported for borrowers receiving principal reduction modifications. Standard errors, in parentheses, are clustered at the borrower level ( $n_{borrower} = 10,741$ ). See the text for additional detail on the specification, outcome measures, and sample.

<i>Credit Card Expenditure (\$/month)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (Principal Reduction x Post)	0.932 (6.969)	0.807 (7.229)	5.658 (8.921)	-0.524 (9.966)	-5.828 (10.002)	-2.227 (10.210)
MSA Fixed Effects		Yes				
Calendar Month Fixed Effects			Yes			
MSA by Calendar Month Fixed Effects				Yes	Yes	Yes
Controls					Yes	Yes
Controls x Post Interactions						Yes
Dependent Variable Mean	228.02	232.14	228.02	232.14	232.14	232.14
Observations	268,525	254,084	268,525	254,084	254,084	254,084
Adjusted R <sup>2</sup>	0.001	0.020	0.006	0.011	0.025	0.026

Table 4: Pareto Improvement at Payment Reduction Discontinuity: Robustness

Scenario	dNPV(\$)	dNPV (%)	Breakeven Discount Rate (%)
Preferred Estimate	6229	3.83	6.13
Robustness to Default Assumptions			
Low Default Reduction	2597	1.60	5.37
High Default Reduction	9861	6.07	6.77
Optimistic Recovery	-1028	-0.63	4.53
Pessimistic Recovery	9106	5.60	6.93
Robustness to Discounting Assumptions			
Flatter Yield Curve (Actual Swaps Spread)	10137	6.24	6.13
Steeper Yield Curve (Implied Treasury Spread)	5917	3.64	6.13
Discount at Treasury Rates	9838	6.05	6.13
Discount at Swap Rates	15845	9.75	6.13
Robustness to Prepayment Assumptions			
Low Prepayment	6064	3.73	5.97
High Prepayment	7014	4.31	10.0
Crosswalk to Payments Owed NPV			
Payments Owed	-2168	-1.33	3.59
Payments Owed, with Default	9053	5.57	5.70
Payments Owed, with Default and Yield Curve	5279	3.78	5.70

Notes: This table assesses the change in the Net Present Value (NPV) of expected payments to the lender of assigning a mortgage to the left-hand side of the 31 percent Payment-to-Income discontinuity instead of the right-hand side for a variety of scenarios. It also reports the percent change in the NPV and the annual discount rate a lender would need in order to be indifferent between assigning a mortgage to treatment or control. The baseline specification incorporates default risk, prepayment risk, and the yield curve. The first three panels of the table vary the assumptions about the probability of default, the recovery rate given default, the rate used to discount cash flows, and the prepayment rate. The final panel crosswalks the baseline specification to the alternative “Payments Owed NPV” discussed elsewhere in the text. See Appendix C.3.1 for details.

Table 5: Baseline Model Parameter Values

Description	Parameter	Value	Source
Life-cycle income growth	$\Gamma_s$	1.025 to 0.7	Carroll (1997)
Std. dev. income shocks	$\sigma_\delta$	0.14	Carroll (1992)
Large income shock probability	$p$	0.1	Güvenen et al. (2014)
Large income shock size	$b$	0.5	Güvenen et al. (2014)
Real interest rate	$r$	0.02	Freddie Mac
Collateral constraint	$\phi$	0.2	FHFA, Corelogic
Real house price growth	$g$	0.009	FHFA 1990-2010
Property tax rate	$\tau_p$	0.015	Himmelberg et al. (2005)
Maintenance cost	$\tau_m$	0.025	Himmelberg et al. (2005)
Utility cost of default	$\psi$	5.4	Match 10% Default
Risk aversion	$\gamma$	4	
Discount factor	$\beta$	0.96	

Notes: see Appendix D.3.6 for details.

Table 6: Housing Wealth MPC in Model and External Benchmarks

	MPC (Cents)		Source
	Model	External Benchmark	
Average	7	9	Mian, Rao, Sufi (2013)
LTV = 95	15	18	Mian, Rao, Sufi (2013)

Notes: This table shows the marginal propensity to consume out of changes in housing wealth in the model relative to the estimates in the external benchmark from Mian, Rao, and Sufi (2013) (adjusted for homeowners). The model estimates are for age 45 borrowers with different initial LTVs. We endow each agent with cash-on-hand equal to two years of permanent income, which is the median non-housing wealth for all homeowners in the 2007 SCF (2007 is chosen as the base year to mimic estimates in Mian, Rao, and Sufi (2013), which cover the 2006-2009 period). We then calculate the MPC for these agents at different LTV values. The “Average” row weights MPCs by LTV, averaging over the distribution of LTV in 2005 and the distribution in 2009. We use the distribution of LTV in the American Housing Survey reported by Carter (2012), adjusted for the well-known issue of under-reporting of LTVs in survey data using an LTV distribution from Corelogic

Table 7: MPC out of Principal Reduction in the Model

Scenario	MPC (cents)
Data	0.3
Model Parameterizations with Small Response	
Baseline Model	0.3
Low Cash-on-Hand	0.0
Age At Mod = 35	0.9
High discount rate ( $\beta = 0.9$ )	0.8
Low risk aversion ( $\gamma = 2$ )	0.9
Unused HELOCs	0.9
Model Parameterizations with Larger Response	
High Cash-on-Hand (PIH)	3.4
Collateral Constraint $\phi = 0$	4.8
Expected 5% House Price Growth	6.2
Expected 5% House Price Growth and $\phi = 0$	24.2
Alternative Policy Simulations	
Write Down to 90% LTV	1.0
Write Down to 90% LTV and $\phi = 0$	14.1

Notes: This table compares the MPC out of principal reduction in the model under alternative parameterizations to the MPC calculated in our data (discussed in Section III.B). The “Baseline Model” corresponds to the parameterization shown in Appendix Table 5 and the modeling of principal reduction policy discussed in Section D.3.4. “Low Cash-on-Hand” corresponds to initial cash-on-hand  $m_t = 0.5$  units of permanent income. The “Unused HELOCs” row corresponds to an experiment where the household is given a credit line worth \$20,000 (or 0.38 units of permanent income), and then given principal reduction. The “High Cash-on-Hand (PIH)” row corresponds to initial cash-on-hand  $m_t = 3.0$  units of permanent income. The “Expected 5% House Price Growth” row corresponds to an expected permanent annual real house price growth of 5 percent.

## B Empirical Appendix

### B.1 Effect of Principal Reduction

#### B.1.1 Sample Construction and Cleaning

**HAMP Public File Analysis** – We study permanent HAMP modifications performed between December 2010 and December 2014 for loans that originated prior to 2009 using the HAMP 1st lien file and the HAMP NPV file.

We restrict the sample to identify loans that are candidates for principal reduction. We drop loans which are owned by the GSEs because the GSEs did not allow principal reduction. To construct the running variable  $V$  we require both an estimated “payment reduction” NPV and an estimated “payment and principal reduction” NPV. Following Scharlemann and Shore (2016), we drop one servicer whose principal reduction forgiveness allocation method exhibits no discontinuity. Further, for reasons we describe in Section II.B, we require  $V \neq 0$ .

We also restrict the sample to loans that can plausibly be matched to the TransUnion credit bureau data. We require non-missing geographic location, pre-modification monthly payment that is non-missing and less than \$25,000, and positive non-missing pre-modification unpaid balance.

We then match loans between TransUnion and the public HAMP file. We require exact matches for origination year and geography (MSA when available in the HAMP file, otherwise state), and then take the closest match using normalized Euclidean distance on modification month, pre-modification monthly payment, post-modification monthly payment, and pre-modification principal balance. We keep matches with Euclidean distance less than a threshold of 0.2. Above this threshold, match quality deteriorates.

We clean the matched sample in three steps. First, we measure principal forgiveness using `ln_upb_frgv_amt` in the HAMP 1st Lien file. If it is missing there we use the value reported in the HAMP NPV file.

Second, we adjust the pre-modification mark-to-market-loan-to-value (MTMLTV) ratio in two ways. Servicers were supposed to report the value in percent (e.g. “100” for MTMLTV of 100 percent), but many instead reported the correct value divided by 100 (e.g. “1” for MTMLTV of 100 percent). To correct for this, we multiply reports less than 2 by 100. In addition, we winsorize MTMLTV at 200 percent.

Third, we also winsorize some other variables with substantial outliers: the change in MTMLTV at the 1st percentile of nonzero values, the amount of principal forgiveness at the 99th percentile of nonzero values, monthly payments (both pre-modification and post-modification) at the 99th percentile, pre-modification payment-to-income ratio at the 99th percentile, pre-modification months past due at the 95th percentile, borrower income at the 95th percentile, monthly auto spending at the 99th percentile of nonzero values, and monthly credit card spending at the 99th percentile of nonzero values.

**TransUnion Consumption Analysis** – We construct a month-to-month measure of credit card expenditure using balance and payments data as reported in TransUnion. We require that the servicer reports month to month payment amounts: we exclude any servicer that never reports payment amounts or for whom over 90 percent of the card-months are associated with payments of zero dollars. This requirement retains 83 percent of the credit cards ever observed in the sample. Among servicers that do regularly report payments, if the payment field is missing, we record this as a payment of zero for the month. We then define credit card expenditure as the balance change between two given months in addition to the payment made that month. Let  $b_t$  denote the balance at the end of month  $t$ , and  $p_t$  be the

payment made in month  $t$ . We calculate expenditure in month  $t$  as  $e_t = b_t - b_{t-1} + p_t$ . If this calculation implies negative expenditure on a card, we record the credit card expenditure as zero. We sum the expenditure over all cards associated with a customer for a total measure for that customer.

**JPMCI Consumption Analysis** — We exclude customers with more than one mortgage and customers with GSE-owned loans. Furthermore, we exclude mortgages that were not underwater prior to modification.

### B.1.2 Running Variable: Understanding Variation Arising from the Treasury NPV Model

This section explains what drives variation in the running variable  $V$  and accordingly what type of borrowers are likely to have a value of the running variable which is close to the regression discontinuity cutoff.

The Treasury NPV model is designed to value the expected cash flows to investors for a loan with various attributes. The Treasury published a 70-page document describing the model (U.S. Department of the Treasury 2015) and nearly all the model parameters are public. The model makes the simplifying assumption that if a loan defaults, it will default immediately (and be liquidated). If it doesn't default, it will pay on schedule with some probability of prepayment each period. Thus, the NPV of a loan is the weighted average of the NPV in the “pay” state and the NPV in the “default” state, with the weights given by the probability of each state, that is

$$NPV = (1 - p_{default}) \cdot NPV\{Loan\ Pays\} + p_{default} \cdot NPV\{Loan\ Defaults\}, \quad (5)$$

The equation above is the first equation in Section V of the NPV model documentation.

To allocate borrowers between the two types of HAMP modifications, this model is evaluated twice. First, it is evaluated assuming the mortgage attributes for a standard “payment reduction modification”. Second, it is evaluated assuming the mortgage attributes for a “payment and principal reduction modification.” For the discussion below, it will be useful to define some notation. The running variable  $V$  is the normalized difference between these two model evaluations,  $p_{default}^{no\ prin\ red}$  is the probability of default under a standard “payment reduction modification”, and  $\Delta p$  is the difference in default rates between the two modification types.

Relative to a standard payment reduction modification, a principal reduction modification has a benefit and a cost. The benefit is that the probability of default is lower and so puts more weight on the high return state of the world (where the loan pays). This benefit is higher when the default reduction  $\Delta p$  is greater and when the difference between the high return state of the world and the low return state of the world is greater.

The cost is that by forgiving principal it reduces  $NPV\{Loan\ Pays\}$ , the cash flows to investors in this higher return state of the world. This cost is lower when this higher return state of the world is unlikely without principal reduction (i.e., when  $1 - p_{default}^{no\ prin\ red}$  is lower). In other words, because the cost of principal reduction is only incurred if the borrower would have repaid this principal, principal reduction is less costly to the investor when this good state of the world was unlikely to occur. Intuitively, an investor who forgives principal gives up more cash flows when borrowers are expected to actually pay this principal than when they are expected to default and never pay it. As a concrete example, suppose that principal reduction always reduces default rates by 10 percentage points ( $\Delta p = 0.1$ ) and that an investor has two groups of loans, where group (a) has  $p_{default}^{no\ prin\ red} = 0.1$  and group (b) has

$p_{default}^{no\ prin\ red} = 0.5$ . In group (a), she must give up cash flows on nine non-defaulting loans to avoid one default, while in group (b), she must give up cash flows on five non-defaulting loans to avoid one default.

Thus, there are four main forces that could explain why some borrowers have high  $V$  and some have low  $V$ . First, it could be that the Treasury model assumes that principal reduction is more effective at reducing default for some borrowers than for others (heterogeneity in  $\Delta p$ ). Second, it could be that the gains from avoiding default are higher for some borrowers than for others (heterogeneity in  $NPV\{LoanDefaults\}/NPV\{LoanPays\}$ ). Third, it could be that some borrowers are assumed to have higher default rates without principal reduction (heterogeneity in  $p_{default}^{no\ prin\ red}$ ). Fourth, when a borrower is eligible for more principal reduction, the model will have a stronger opinion—positive or negative—about whether principal reduction is valuable to the investor. In Treasury’s model, the second, third, and fourth forces are more important than the first.

Before discussing how these forces affect  $V$  in detail, we begin by describing how the Treasury model predicts default rates. The model uses a logit function  $p_{default} = \exp(\ell)/(1 + \exp(\ell))$  where log-odds  $\ell = \beta X$ . The model has five  $X$  variables:

1. the borrower’s credit score,
2. the loan’s initial payment-to-income (PTI) ratio,
3. the loan’s LTV ratio,
4. the amount of short-term payment relief provided by the modification (as captured by the change in the PTI ratio),
5. and days-past-due (the number of days delinquent at the date of modification).

The model is additively separable with respect to the first four variables. It is specified separately for borrowers who are current, 30 days past due, 60 days past due, and 90+ days past due. These two facts mean that a loan’s LTV and days-past-due are sufficient statistics for predicting the impact of principal reduction in log-odds terms. *Within* each days-past-due group, the effect of principal reduction on  $\ell$  is approximately constant. This assumption is based on historical data relating default and LTV, as discussed in Holden et al. (2012). *Across* groups, however, principal reduction is assumed to be more effective for borrowers who are current or 30 days past due at the date of modification.

Empirically, we find that the expected average change in default from principal reduction ( $\Delta p$ , the first force) is approximately constant. The only variable that meaningfully affects  $\Delta p$  is days-past-due, but there is little variation in days-past-due with respect to the running variable.

In contrast, heterogeneity in  $p_{default}^{no\ prin\ red}$  is important. One example of what drives heterogeneity in  $p_{default}^{no\ prin\ red}$  is that borrowers with lower FICO scores are expected to have higher default probabilities, keeping all other characteristics constant. These borrowers will have higher values of  $V$  because principal reduction is most beneficial when  $p_{default}^{no\ prin\ red}$  is higher. This pattern is visible in Appendix Figure 4a where higher values of the running variable  $V$  are associated with lower mean FICO scores.

The third force explaining variation in  $V$  is heterogeneity in the return to the investor in the default and non-default states of the world. In the Treasury NPV model, the ratio  $NPV\{LoanDefaults\}/NPV\{LoanPays\}$  depends mostly on macro factors such as expected house price growth and the discount at which foreclosed properties are being sold. This ratio

affects  $V$  because the investor return to principal reduction is larger when the bad state that principal reduction is expected to help avoid (default) is costlier to the investor. Thus, some of the variation in  $V$  is also coming from differences across loans depending on their location and time period of modification, where high values of  $V$  are associated with loans in more distressed local markets (Holden et al. 2012).

The fourth and final force explaining variation in  $V$  is how much principal reduction a borrower is eligible for. The “payment and principal reduction” modification wrote down mortgage principal until LTV reached 115, as we discuss in Section 3.1 of the paper. When a borrower is eligible for an especially large amount of principal reduction, the model will have a relatively strong opinion about whether principal reduction is a good idea. This is because the marginal gain (or loss) from any principal reduction is magnified. However, when a borrower is eligible for a moderate amount of principal reduction, the model will be closer to indifferent. The borrowers who are eligible for a moderate amount of principal reduction are those whose pre-modification LTV is closer to the target of 115. This explains why pre-modification LTV exhibits a “V” shape with respect to the running variable as shown in Appendix Figure 5d. Nevertheless, the amount of principal reduction at the RD is still substantial. Borrowers in the treatment group received principal reduction equal to 11 points of LTV, or more than \$30,000.

### B.1.3 Robustness

**Balance Plots** – Pre-determined covariates trend smoothly through the cutoff, as shown in Appendix Figure 5. The first five panels show the distribution of pre-modification borrower credit score, monthly income, monthly mortgage payments to monthly income (payment-to-income, or PTI) ratio, LTV ratio, and months past due around the cutoff. In all cases these borrower characteristics trend smoothly. The RD estimates of the discontinuous change in these variables at the cutoff, corresponding to the numerator of equation (2), are reported on the figures. For three variables (credit score, monthly income, and PTI) the sign points to slightly worse-off borrowers to the right of the cutoff, while for two variables (LTV and months past due) the sign points to better-off borrowers to the right of the cutoff. The lack of any systematic correlation supports the validity of the design. The only covariate with a statistically significant jump at the 95 percent level is LTV, and even here the jump is not economically significant.

Lee and Lemieux (2010) note that when there are many covariates, some discontinuities will be significant by random chance. They recommend combining the multiple tests into a single test statistic. We implement a version of this by using all five pre-modification covariates to predict default, and we test whether there is a jump in this pooled predicted default measure at the cutoff. The result is shown in the last panel of Appendix Figure 5. There is no significant change in predicted default at the cutoff.

**Density** – Another relevant issue in regression discontinuity settings is the possibility that the running variable could be manipulated (McCrary 2008). The usual test is to plot a histogram of the running variable to examine whether there is an unusual increase in mass to the right of the cutoff. We show such a plot in Appendix Figure 6a. While the density is smooth on either side of the cutoff, there is a large bulge exactly at zero.

There are four reasons why we believe the bunching of borrowers at zero is not a challenge for the validity of our research design. First, program officers in charge of the dataset at the U.S. Treasury Department informed us that this bulge is a data artifact. If a borrower was ineligible for principal reduction (e.g. because her home was not underwater), servicers were not supposed to compute the NPV of a “payment and principal reduction” modification which

in our notation is  $ENPV(1, X)$ . Instead, it appears that several servicers reported their calculation for  $ENPV(0, X)$  in the  $ENPV(1, X)$  field, such that  $ENPV(0, X) = ENPV(1, X)$  which in turn implies  $V = 0$ . Consistent with this theory, we find that observations with  $V = 0$  are disproportionately likely to be above water and disproportionately likely to have “0” as the potential dollar value of principal forgiveness submitted to the NPV model.

Second, the conventional economic environment that would incentivize manipulation is not relevant here. Servicers have no economic incentive to manipulate the running variable because they receive the same compensation regardless of which modification is offered.

Third, even if servicers did have an economic incentive to manipulate, that incentive would not vary discontinuously at this cutoff: principal reduction provision is optional regardless of the outcome of the calculation.

Fourth, were servicers manipulating the running variable to zero in an attempt to rationalize principal reduction, they failed; the share of borrowers receiving principal reduction in this zero group is actually half what it is for borrowers with actual positive values of the running variable.

We were advised by U.S. Treasury staff to remove these observations as reflecting measurement error. We attribute the bunching of borrowers at zero to data mis-reporting and drop observations exactly at zero. Appendix Figure 6b shows the distribution for the resulting sample, which is our analysis sample. There is no noticeable change in density around the cutoff.

We show in Appendix Figure 6c that borrower take-up rates were high on both sides of the discontinuity. Ninety-seven percent of borrowers who are offered a modification take it up, and this trends smoothly around the cutoff. This provides further evidence against borrower manipulation to obtain one or the other modification type.

**Alternative Bandwidths** – Appendix Figure 8 tests the sensitivity of our results to the bandwidth chosen for the local linear regression. Our central estimates are constructed using the optimal bandwidth from the Imbens and Kalyanaraman (2012) procedure, which is 0.61. The optimal bandwidth recommended by the Calonico, Cattaneo, and Titiunik (2014) procedure is 0.63. The point estimate begins to rise at wider bandwidths. The rise at wider bandwidths is not surprising given the shape of the estimated conditional expectation function for default, which is particularly sloped near the cutoff. Wider bandwidths will lead to specification error when this function is particularly steep near the cutoff. A quadratic specification which can more easily mimic this slope is stable for a wider bandwidth, showing a point estimate around zero up to a bandwidth of 1.5 before rising.<sup>2</sup>

**Alternative Outcome (Foreclosure Initiation)** – Our evidence suggests that principal reduction is also ineffective at reducing foreclosures. Appendix Figure 26 shows that there is no jump in the foreclosure initiation rate at the cutoff. Due to the lengthy delay between foreclosure initiations and foreclosure completions, foreclosure completions are rarely observed in our sample period. The most optimistic point in the 95 percent confidence interval suggests foreclosure initiations were reduced at most by 3.1 percentage points. It is not surprising that the same pattern would be seen in foreclosures as in defaults. Borrowers who have defaulted and are unable to self-cure are generally unable to sell their home to avoid foreclosure while they are underwater.

**Alternative Normalization of Running Variable** – In Appendix Figure 22, we show the change in actual default as well as the change in predicted default using baseline (pre-

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<sup>2</sup>The optimal bandwidths for a quadratic specification from Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014) are 0.8 and 1.0, respectively.

determined) covariates when the running variable is specified as the change in NPV in dollars (rather than the percent change). This specification fails the balance test using baseline covariates and is thus uninformative about the effect of principal reduction on default.

**Unmatched Sample** – In principle, the regression discontinuity analysis of principal reduction can be done using the public HAMP file alone, without matching to TransUnion. Appendix Figures 23 and 24 show that our findings that default rates are the same on both sides of the RD cutoff and that predicted delinquency is similar on both sides of the cutoff continue to hold in this larger sample that includes unmatched loans.

However, there are two problems with using this larger sample. First, data quality is lower (the HAMP loans which match to TransUnion will tend to be the ones where loan characteristics are accurately measured). Second, this sample includes one large servicer that gave principal reduction to all borrowers (Scharlemann and Shore (2016) also drop this servicer). The public HAMP data do not include a servicer identifier, and so it is impossible to drop this servicer when working only with the public data. Our preferred specification in the paper uses the matched sample because it is able to address these issues.

#### **B.1.4 Representativeness of HAMP Participants Relative to Typical Delinquent Underwater Borrowers**

Our empirical analysis of the effect of principal reduction on default focuses on borrowers near the assignment cutoff for receiving principal reduction. To assess the representativeness of our analysis sample, we compare borrowers near the cutoff in the matched HAMP credit bureau file to a sample of delinquent borrowers in the Panel Study of Income Dynamics (PSID) between 2009 and 2011. Summary statistics for borrowers in both samples are shown in Table 1a. Borrowers in our sample are broadly representative of delinquent underwater borrowers during the recent crisis.

The median borrower in our sample has a higher LTV than delinquent borrowers in the PSID (121 compared to 94), but the 90th percentile LTV is similar (168 compared to 166). Since all the borrowers who are evaluated for principal reduction must be underwater, we would expect them to be concentrated in the underwater portion of the delinquent borrower distribution. The fact that borrowers in our 90th percentile are “only” at an LTV of 168, and that the median borrower is substantially less underwater, is important for interpreting our empirical results.

The PSID comparison is also helpful because it allows us to examine the liquid assets of borrowers. Delinquent borrowers in the PSID have very low levels of liquid assets. To be eligible for HAMP, borrowers had to attest that their liquid assets were less than three times their total monthly debt payments. However, the PSID data shows that this screen had little force. Even the delinquent borrower at the 90th percentile of the liquid asset distribution would have passed the HAMP screen.

### **B.2 Effect of Payment Reduction**

#### **B.2.1 Sample Construction and Robustness**

We winsorize credit card spending at the 95th percentile of positive values.

**Balance Plots** – We show the trend in pre-determined covariates through the cutoff in Appendix Figure 12. The first four panels show the borrower monthly income, pre-modification monthly payment, LTV ratio, and months past due around the cutoff. These balance plots differ in two ways from the balance plots for the discontinuity for principal reduction. First, unlike in the matched HAMP credit bureau dataset used for the investor NPV strategy, borrower credit score is not available in the JPMCI bank dataset. Second, we

cannot show balance on PTI because it is the running variable. Instead, we show balance on pre-modification monthly payment.

There is no statistically significant jump in these loan and borrower characteristics at the 95 percent confidence level. In the bottom panel we use these observable borrower characteristics to predict default and show that predicted default is also smooth at the cutoff.

**Density** – Appendix Figure 13 shows that borrower density is also smooth around the cutoff.

**Alternative Bandwidths** – Our point estimate of  $\hat{\tau}$  from equation (4) is that an extra 1 percent payment reduction reduces default rates in the two years after modification by 0.38 percentage points. Appendix Figure 16 tests the sensitivity of our results to the bandwidth chosen for the local linear regression. Our central estimates are constructed using a bandwidth of 0.06 points of PTI. We test alternative bandwidths between 0.01 and 0.1 and find that the point estimate is stable.

**Adjusting for Upper Bound of Potential Principal Forgiveness Impact** – If we take the most optimistic point in our 95 percent confidence interval for the impact of principal reduction on default from Section II.C, and scale it by the amount of relative principal increase received by borrowers just below the 31 percent PTI cutoff, we find that a principal increase of this magnitude would have led to at most a 1.3 percentage point increase in default rates. If the payment reductions had to offset this effect, this would mean that the reduced form jump in default at the cutoff would have been 8.4 percentage points without the principal increase rather than 7.1 percentage points or, alternatively, that each 1 percent reduction in payment reduced default rates by 0.44 percentage points (1.4 percent), similar to our baseline estimate of 0.38 percentage points (1.2 percent).

### B.2.2 Impact of Payment Reduction on Consumption

Our payment reduction regression discontinuity empirical strategy is under-powered for studying consumption impacts. In Appendix Figure 27, we plot the reduced form of the 31 percent PTI strategy with the change in mean credit card spending from the year before modification to the year after modification as the outcome variable. The standard error is so large that, using the same procedure for calculating an MPC as described in Section II.D, we cannot rule out an MPC above 1 or below -1.

Unlike with principal reduction, we are unable to increase the precision of our payment reduction estimates by using a difference-in-differences design. The difference in principal reduction received by borrowers with and without principal reduction remains large when we expand the sample to a wider bandwidth. In contrast, the difference in payment reduction between HAMP and Chase modifications falls when looking at a wider sample (as can be seen by looking at the edges of Figure 5a). This is because the PTI target in HAMP generates larger payment reduction for higher PTI borrowers. Hence, comparing borrowers who received HAMP and Chase modifications at a wider bandwidth results in a shrinking size in the payment reduction treatment. We therefore conclude that our data and available research designs are unsuited for credibly estimating the effect of payment reduction on consumption.

## C Net Present Value Calculations

In this section we provide more detail on our NPV calculations. The NPV calculations use borrower-level micro data on loan terms where available and otherwise use projections from the U.S. Treasury NPV model discussed in Appendix B.1.2.

Appendix C.1 discusses the basic setup, which is applicable to our analysis in Sections II.A, II.D, and V.A. Appendix C.2 pertains to our discussion of the cost per avoided foreclosure from using principal reduction in Section II.C.

Appendices C.3 and C.4 pertain to our analysis in Section V.A. Appendix C.3 provides more detail for calculating the change in the NPV of expected payments to lenders at the HAMP eligibility cutoff. Appendix C.4 provides more detail for calculating the potential gains from redesigning HAMP modifications.

### C.1 Net Present Value of Expected Payments

We use two equations to estimate the NPV of the loan. Equation (6) estimates the value of a mortgage that “cures,” meaning that the borrower repays on time or early:

$$NPV\{Loan\ Pays\}(\delta) = \sum_{i=1}^T \frac{1}{(1+\delta)^i} [(UPB_{i-1} - Prin_i)(s_{i-1} - s_i) + (Prin_i + I_i)s_{i-1}] \quad (6)$$

where  $T$  is the term of the loan,  $\delta$  is the investor’s discount rate,  $UPB_i$  is the unpaid principal payment at time  $i$ ,  $Prin_i$  is the principal payment for period  $i$ ,  $I_i$  is the interest payment for period  $i$ , and  $s_i$  is the survival probability of loan, which is constructed as  $s_i \equiv \prod_{k=1}^i (1 - Prepay_k)$  where  $Prepay_k$  is the prepayment probability in year  $k$ . The time period is annual. We observe  $UPB_i$ ,  $Prin_i$ , and  $I_i$  for loans in the treatment and control groups in both the HAMP data and the JPMCI data.

We use the Treasury NPV model to estimate annual prepayment rates. This is the same model used by servicers to calculate the expected cash flows to lenders under various HAMP modification types, which we use for identification in Section II.A, which is documented in U.S. Department of the Treasury (2015). The model uses a logit equation for predicting prepayment rates (Section V of U.S. Department of the Treasury 2015); we use the coefficients for owner-occupied homes reported in Appendix C of U.S. Department of the Treasury (2015) for borrowers that are 90+ days delinquent at modification date.<sup>3</sup>

Our second key equation incorporates default risk into our NPV estimate. We take equation (5) from Appendix B.1.2 and modify it to allow for the fact that not all defaults end up being liquidated:

$$NPV = (1 - p_{default}) * NPV\{Loan\ Pays\} + p_{default} * [P(liquidate|D) * NPV\{Liquidate\} + (1 - P(liquidate|D))NPV\{Loan\ Pays\}] \quad (7)$$

where  $p_{default}$  indicates 90-day default. We follow the Treasury NPV model in making a simplifying assumption that borrowers make a one-time decision to default or not default.

To estimate the probability that a default results in a liquidation (with an accompanying loss for the investor), we use HAMP performance data. Among HAMP modifications that are disqualified due to default, 26 percent end up in foreclosure, 14 percent end in a short sale,

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<sup>3</sup>Because we do not have access to all the covariates used in the Treasury NPV model, we need to separately estimate the intercept in the logit equation. We choose this intercept to match an annualized prepayment rate of 0.9 percent. This estimate is based on the prepayment rate of HAMP-modified mortgages in the first five years after modification.

18 percent self-cure, 33 percent get a proprietary modification, and 10 percent have delayed action, such as a borrower going through bankruptcy (U.S. Department of the Treasury 2017). Of loans whose status is fully resolved, 45 percent are foreclosed on, 24 percent end in a short sale, and 31 percent self-cure. We assume that loans which get a proprietary modification or delayed action ultimately have the same distribution of final outcomes. (Unfortunately, we do not have data on the outcomes of these proprietary modifications. Our assumption that these modifications have the same distribution of final outcomes is conservative in that it likely overstates the losses on these loans.) We explore alternative assumptions in the robustness analysis below.

Unfortunately, HAMP does not collect data on losses after disqualification so we draw on GSE performance data to estimate the NPV of loans that are “liquidated.” The GSEs report losses on loans that are liquidated via either foreclosure or short sale. Goodman and Zhu (2015) document that GSE losses are quite similar on foreclosures and short sales. We use performance data from loans liquidated in 2011 because that was the year in which the GSEs experienced the largest number of liquidations. In that year, the Fannie Mae reported losses at liquidation equal to 41 percent of the unpaid balance on the loan (Fannie Mae 2018). However, this includes reimbursements from third parties (mortgage insurers and mortgage originators) to the GSEs equal to 15 percent of the unpaid balance of the loan. Altogether, investors lost 56 percent of the unpaid balance of the loan at liquidation. We explore alternative assumptions in the robustness analysis below.

## C.2 Cost Of Preventing a Foreclosure

What is the cost to lenders and taxpayers of preventing a foreclosure via principal reduction? In this appendix, we answer this question using data from the policy discontinuity in Section 3 combined with the NPV model described above.

Our point estimates imply that principal reduction *raises* the probability of default by 1.2 percentage points. This estimate is small and statistically insignificant, with a standard error of 3.18 percentage points. However, we are unable to reject that principal reduction has *any* impact on lowering default rates. To assess whether principal reduction might be *cost-effective*, we consider the extreme case that principal reduction actually reduces default by 5.0 percentage points ( $1.2 - 1.96 \times 3.18$ ). This is the most optimistic number which is consistent with our 95 percent confidence interval.

We translate this default reduction to a foreclosure reduction using the estimates in Appendix C.1. Specifically, we assume that 45 percent of defaults end in completed foreclosures in our baseline analysis, so principal reduction reduces foreclosure completion by no more than 2.3 percentage points.

An alternative method to compute foreclosure reduction delivers a similar result. Appendix Figure 26 shows that we can rule out a reduction in foreclosure initiations of 2.9 percentage points. It is uncertain what fraction of initiations end up in foreclosures in our sample. Herkenhoff and Ohanian (2019) report that approximately half of borrowers with a foreclosure initiation end up with a completed foreclosure. Applying their estimate to our sample implies that principal reduction reduces foreclosure completion by no more than 1.5 percentage points during the window we study, even less of an impact than we consider in our baseline case.

To assess whether such a foreclosure reduction is cost-effective, we use the lender valuation model described in Appendix C.1. The modification terms correspond to those at the policy discontinuity arising from the Treasury NPV model. The borrower receives an

average of \$31,000 in principal forgiveness at the discontinuity. We implement the model for a representative standard HAMP modification that includes only payment reduction and for a HAMP Principal Reduction Alternative modification, which includes both payment and principal reduction.

Relative to a standard payment reduction modification, a principal reduction modification has a benefit and a cost. The benefit is that the probability of default is lower (at least under our optimistic assumptions) and so puts more weight on the high return state of the world (where the loan pays). This benefit is higher when the default reduction is greater and when the difference between the high return state of the world and the low return state of the world is greater. The cost is that by forgiving principal it reduces the cash flows to investors in this higher return state of the world, (i.e., for a borrower that would have paid regardless, the lender is getting less cash flow with principal reduction than without it). This cost is lower when this higher return state of the world is unlikely without principal reduction.

Even under the most optimistic assumption about the effectiveness of principal reduction at reducing defaults, the lender valuation model implies that principal reduction is not a cost-effective way to prevent foreclosures. We calculate that lenders and taxpayers would incur a cost of at least \$766,000 to prevent a foreclosure. The intuition for why this number is large is that the benefit is quite small (a 2.3 percentage point reduction in the foreclosure probability), while costs are quite large (reduced cashflows from the vast majority of borrowers who are not defaulting). Further, this estimate is a *lower bound* on the cost of preventing a foreclosure; at our actual point estimate, principal reduction does not prevent any foreclosures.

This cost can be borne by taxpayers, lenders or some combination of the two. Scharlemann and Shore (2016) find that the average government subsidy in the first cohorts of PRA was around \$20,000, with an average amount of \$74,000 of principal forgiveness. At this subsidy rate, we calculate that the \$766,000 estimate in the previous paragraph can be decomposed into a cost per foreclosure prevented of \$365,000 to taxpayers and \$402,000 to investors.

### C.3 Calculations for Pareto Improvement from Maturity Extension

Since the amount of liquidity provision appears to be more important than how that liquidity is provided, our results imply that maturity-extension-financed payment reduction may generate a Pareto improvement, leaving borrowers, lenders, and taxpayers all better off. To understand why this is true, it is useful to revisit the particular structure of mortgage modifications around the HAMP eligibility discontinuity we study in Section IV. A borrower who moves from the right-hand side of the cutoff (“control”) to the left-hand side (“treatment”) sees deeper immediate payment reductions that are offset by continued payments in the long-term. Intuitively, maturity extension is equivalent to the lender “lending” the borrower their monthly payment reductions at the mortgage interest rate, with repayments on this “new” loan beginning at the end of the original mortgage term and continuing through the end of the new loan term.

For borrowers, a maturity extension which moves funds from the future to the present might be particularly valuable in periods of acute economic distress. In our setting, all borrowers are better off in the treatment group under relatively mild assumptions about monotonicity and revealed preference. Borrowers can be divided into four potential groups: those who default under both contracts, those who switch from defaulting to paying on schedule, those who switch from paying on schedule to defaulting, and those who pay on

schedule under either contract. The first group defaults under both contracts and is therefore no worse off receiving treatment. The second group is better off because they indicate by revealed preference that the modified loan is more attractive than defaulting. If we assume that default is monotonically decreasing with the extent of payment reduction—the canonical assumption from Imbens and Angrist (1994) needed to identify a local average treatment effect—then there is no one in the third group. Finally, the fourth group is better off because their choice set is expanded; they always have the choice to ignore the maturity extension and repay the loan on the original schedule.

One question raised by the conclusion that borrowers are better off from the maturity extension we study is whether this is always the case. Are borrowers always better off from extended maturities, and if so, does this imply that the standard 30-year mortgage is too short? Unfortunately, our evidence does not inform this broader question. We only conclude that in the specific period we study, extending mortgage maturities for already-distressed borrowers appears to have benefited those borrowers. Because borrowers who are experiencing financial distress are likely to value additional liquidity, we conjecture that the benefit of maturity extension for borrowers is likely to extend to other periods of financial distress. However, our findings do not imply that longer maturities are optimal *ex-ante* (only that the option to extend in the face of financial distress may be optimal *ex-post*).

For lenders, a maturity extension will increase the NPV of total payments owed when the mortgage’s interest rate is higher than the lender’s current discount rate. In this time period the interest rates on new mortgages were a few percentage points below the average interest rate on existing mortgages. Hence, when using current interest rates as a measure of the opportunity cost of capital, extending maturities on existing mortgages would increase the NPV of payments owed to the lender. Although most of the incremental payment reduction at the cutoff we study was achieved from maturity extension, part came from interest rate reduction, which reduces the NPV of payments owed. This works against the maturity extension effect.

In our setting, we find that the NPV of the payments owed under the contract is similar in the treatment and control group. To be specific, Appendix Figure 14 shows that treatment lowers NPV by \$2,168 relative to the control group when lenders use a 4.11 percent discount rate, which is the average of the 30-year mortgage rate during our sample period. This is a modest loss, and with a standard error of \$2,049 we are unable to statistically reject that there is no change in the NPV to lenders (consistent with the criteria for a Pareto improvement). This estimate—which assumes that all borrowers repay on schedule—has three shortcomings: first, some borrowers default (and treatment reduces default); second, that some borrowers prepay their mortgage; and third, that lenders may discount cash flows after year 30 at a higher rate than short-term cash flows.

To address these limitations, we build an *expected payments* NPV model and find that under plausible assumptions lenders are better off assigning a borrower to treatment. We proceed in three steps. First, we incorporate default by using our causal estimate of the effect of treatment on default. We combine this with prior evidence on the losses incurred by lenders when borrowers default described in Appendix C. Because treatment significantly reduces default rates, this moves the lender to a gain of \$9,053. Second, the fact that the payments arrive further in the future means that we need to use a higher discount rate, which decreases the NPV. We estimate a term premium of 32 basis points between 30- and 40-year mortgages by extrapolating from observed mortgage rates (see Appendix Figure 28), which shrinks the gain to the lender to \$6,138. Finally, we incorporate realistic prepayment behavior for a final estimate of \$6,229.

The finding that lenders are better off from maturity extensions depends on two crucial assumptions. First, we assume that prevailing mortgage interest rates accurately reflect a lender’s opportunity cost of capital. But if some lenders are liquidity-constrained (as might be implied by the fall in mortgage originations during this time period), then market rates for those borrowers who can get a mortgage might not reflect a lender’s true cost of capital.<sup>4</sup> We calculate that a lender is better off from treatment as long as her discount rate is below 6.13 percent.

Second, we assume that implied mortgage spreads accurately capture the lender’s disutility from extending the mortgage term from 30 to 40 years. Because of uncertainty over this spread, we show that our results are robust to using a variety of term premium assumptions in Appendix C.3.1. However, the term premium we use in our baseline case (32 basis points) is already significantly higher than the actual 30- to 40-year spread for swaps and corporate bonds in our sample period (2 and 9 basis points, respectively). Furthermore, the flat swap yield curve during this time period implies that a lender concerned about increased portfolio duration risk from extending mortgage maturities could hedge this risk at low incremental cost.

Finally, taxpayers are also better off from more maturity extensions. The government spent substantial resources subsidizing HAMP modifications above the eligibility cutoff with small payment reductions and high default rates, whereas lenders were willing to provide borrowers below the cutoff private modifications requiring no government assistance which had large payment reductions and low default rates. This suggests that using maturity extensions as the first step in modifying mortgages could have saved substantial taxpayer subsidies.

Our findings for payment reduction—that payment reductions can be structured so as to reduce default rates while leaving all parties better off—contrast sharply with our findings for principal reduction, which was ineffective even while being costly to both lenders and taxpayers. Future private and public modification programs will have a menu of options for restructuring loans. Our findings suggest that among these options, those that maximize immediate payment reduction are likely to be most effective, and that maturity extension is a particular way to achieve large immediate payment reductions at little cost to lenders and taxpayers. We explore the broader implications of these results for modification design in more detail in Appendix C.4.

We first provide more detail on the expected payments NPV calculation as well as describe several robustness checks below.

### **C.3.1 Robustness of Expected Payments NPV at 31 Percent PTI Discontinuity**

Our choice of the discount rate  $\delta$  for future cash flows depends on the maturity of the mortgage. Recall that assignment to treatment involves an extension in the term of the mortgage and 80 percent of loans in the treatment group last 40 years after modification. Ideally, we would use the interest rate on 40-year mortgages to discount these cash flows, but unfortunately we are unaware of any publicly available data source with prices for 40-year mortgages. Instead, we estimate the price of a 40-year mortgage by using a simple functional form to extrapolate from the price of 15-year and 30-year fixed mortgages sold by Freddie Mac. The JPMCI payment reduction sample includes modifications from October 2011 through January 2014. The average 15-year rate is 3.06 percent during this period and the average 30-year rate is 3.84 percent.<sup>5</sup> We fit an equation  $r = \alpha + \beta \log(\text{term})$  to these

<sup>4</sup>Appendix Figure 34a shows the time series of mortgage originations during this period.

<sup>5</sup>This is quite similar to the average 30-year rate of 4.11 percent during the time period when modifications

data and estimate a hypothetical 40-year mortgage rate of 4.16 percent.<sup>6</sup> In the robustness analysis below, we explore the sensitivity of our estimates to alternative assumptions about the discount rate and the yield curve.

We estimate the effect of treatment on default rates using our causal estimates from the regression discontinuity design and HAMP performance data. We estimate a 90-day default rate in the two years after modification of 24.8 percent for the treatment group and 32.1 percent for the control group. Among HAMP modifications done in 2010, the default rate is 28.1 percent two years after modification and 45.6 percent five years after modification (U.S. Department of the Treasury 2017), for a ratio of 1.62. We project default rates five years after modification in our data by multiplying our estimated default rates by 1.62. This calculation assumes that payment reduction is equally effective in years three, four, and five. We project the default rate will be 38.6 percent in the treatment group and 46.2 percent in the control group. We explore alternative assumptions for the impact of treatment on the default rate in the robustness analysis below.

Our estimates imply a gain to the investor from assigning a borrower to treatment. Recall that treatment is essentially a loan to the borrower in the form of lower mortgage payments for 22 years which is offset by additional mortgage payments extending beyond the pre-modification term of the loan. The change in the NPV arising from this maturity extension treatment is \$6,229, as shown in Appendix Table 4. This is equal to a 3.8 percent increase in the NPV of the loan.

As an alternative to the NPV calculation, we also report the discount rate an investor would need to break even on providing treatment to a group of mortgages. While the prior calculation assumed that the lender discounted future mortgage cash flows at our best estimate of the market interest rate, an alternative approach allows us to be agnostic as to the lender’s discount rate. The NPV of a mortgage that cures is a function of the discount rate  $\delta$ , as shown in equation (6) and the expected NPV of all mortgages in equation (7) relies on this, so we can rewrite  $NPV$  in equation (7) as a function  $NPV(\delta)$ . Then, we can solve for the discount rate that satisfies the lender’s indifference condition such that the change in NPV from offering the treatment modification is the same as the change from offering the control modification:

$$\delta^* \text{ such that } \Delta NPV(\delta|T) = \Delta NPV(\delta|C).$$

In our baseline specification, we estimate that a lender that discounts the future annually by 6.13 percent will be indifferent between offering this modification. This implies that a lender with an annual discount rate less than 6.13 percent will be better off offering the treatment modification.

Did lenders in fact discount future cashflows at less than 6.13 percent? It appears that most did. As part of the HAMP NPV test, mortgage servicers chose a discount rate for future cashflows. Their choice set ranged from the market interest rate as a lower bound to 250 basis points above the market interest rate as an upper bound. SIGTARP (2012) report that 96 percent of servicers discounted future cashflows at the market interest rate, which

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were performed for our principal reduction sample.

<sup>6</sup>When a mortgage term lasts less than 35 years, we use the 30-year rate and when a mortgage term lasts 35 years or more, we use the 40-year rate. Our results would change very little if we instead used different discount rates for every possible mortgage maturity between 30 and 40 years. In the analysis sample, 51 percent of mortgages last exactly 40 years after modification and 40 percent last 30 years or less after modification.

was around 4 percent during this time period. Any servicer that discounted cashflows at this rate would have accepted maturity extension, which was NPV positive for any discount rate less than 6.02 percent.

We explore the robustness of our NPV and discount rate estimates to alternative assumptions on default rates, recovery rates on losses, discounting, and prepayment in Appendix Table 4. Across almost all scenarios, we find that the NPV of the loan to the investor increases from assigning a loan to treatment instead of control. First, we explore the impact of alternative assumptions about the impact of treatment on mortgage default. Using the lower and upper bounds of our 95 percent confidence interval, we estimate the change in NPV ranges from \$2,597 to \$9,861.

Second, we show that impact of treatment on NPV is sensitive to our assumptions on the recovery rate on defaulted loans, but is always positive or statistically indistinguishable from zero. Our specification with the most optimistic recovery rates assumes that *every* proprietary modification and *every* action pending self cures, meaning that there is a 61 percent self cure rate, and uses the highest possible recovery rate on GSE loans during the crisis, which was a 48 percent loss in 2009. Our specification with the most pessimistic recovery rates assumes that all proprietary modifications and action pending ends in liquidation, meaning that there is an 18 percent self-cure rate, and the lowest possible recovery rate on GSE loans, which was a 61 percent loss in 2014. Treatment in the optimistic scenario causes an NPV loss to the investor of -\$1,028, while in the pessimistic scenario it causes an increase of \$9,106. Note that -\$1,028 is indistinguishable from zero given our standard errors, and therefore the criteria for a Pareto improvement (which is that at least one party is better off and no party is worse off) is still satisfied in this scenario.

Third, we show the impact of using alternative methodologies for estimating the discount rate. Intuitively, the treatment modification defers cash flows from the present to the future and investors require a higher rate of return for deferring these cash flows. Recall that the average interest rate for a 30-year fixed rate mortgage during our sample period is 3.84 percent and in our baseline specification we estimated an additional 32 basis points for a 40-year mortgage. At one extreme, an alternative methodology which relies on a comparison of 30-year and 40-year loans is the swap rate where the yield curve is flatter and the average spread in our sample period is only 2 basis points. At the other extreme, projecting hypothetical spreads using interest rates on debt issued by the U.S. Treasury implies a steeper yield curve with an additional 34 basis points for a 40-year mortgage. Both of these projections are shown in Appendix Figure 28. This flatter yield curve implies a change in NPV of \$10,137 and the steeper yield curve implies a change in NPV of \$5,917. The figure also shows that, if anything, the log functional form overestimates the term premium at higher maturities. Forty-year maturities are actually observed for swaps and corporate bonds. For these, the “implied” spread between 30 and 40 year maturities using the log functional form assumption are much larger than the actual spreads.

It may be preferable to use the risk-free rate to discount cashflows in our model. The argument for using the risk-free rate here is that lenders offering mortgages charge a premium over the risk-free rate in order to compensate the lender for prepayment risk and default risk. However, our expected payments NPV calculation already takes into account default and prepayment risk. The average rate on 30-year Treasury notes during this time period is 3.17 percent, and we project that the rate on a 40-year note would be 3.52 percent. The average rate on fixed-for-floating swaps is 3.00 percent for 30 years and 3.02 percent for 40 years. Under these assumptions, we calculate changes in NPV of \$9,838 and \$15,845 respectively. The value is greater to the investor under this scenario because a maturity

extension delays cashflows, and switching to a lower discount rate makes cashflows far in the future more valuable.

Fourth, we show that prepayment rates have little effect on the change in NPV from treatment. At one extreme, we assume an annual prepayment rate of 0.9 percent (the observed prepayment rate after HAMP modification) for the life of the loan. At the other, we assume an annual prepayment rate 6.8 percent (the observed prepayment rate on all Fannie Mae loans 1999-2017). The change in NPV varies from \$6,064 under the low prepayment scenario to \$7,014 under the high prepayment scenario..

Finally, we crosswalk our expected payments NPV estimate to the payments owed NPV estimates reported elsewhere in the text. Recall that the investor's return from treatment is a \$6,229 gain in terms of expected payments NPV, but a loss of \$2,168 when using the payments owed NPV estimate reported in Appendix Figure 14. (To be precise, the figure shows that the investor loses \$2,168 more from treatment). This assumes that the loan is repaid on schedule (no default or prepayment) and the investor discounts cashflows at 4.11 percent annually.

### **C.3.2 Does More Time Underwater Offset the Default-reducing Benefits of Payment Reduction?**

Our empirical results show a 23 percent reduction in default rates (from 32.1 percent to 24.8 percent) at the cutoff. One possible downside of maturity extension is that it leaves borrowers underwater for longer. The default-reducing benefits of payment reduction might be offset by a longer period of being underwater during which potential shocks (such as health or job loss) could potentially push a borrower into foreclosure.

The magnitude of this offsetting effect depends on how much longer a borrower who receives maturity extension remains underwater. To quantify this, we analyze two hypothetical mortgages which match the average characteristics of borrowers on each side of the 31 percent PTI discontinuity. These borrowers have an average loan-to-value ratio of 131 percent. The right-hand side borrower receives a 70 basis point reduction in the interest rate and \$19,500 in principal forgiveness, which enables a payment reduction of 14 percent (equivalent to payment reduction on the right-hand side) for the remaining 23 years on the loan. The left-hand side borrower receives a payment reduction of 31 percent (equivalent to payment reduction on the left-hand side). This borrower receives an extension of the maturity of the loan to 40 years, a 110 basis point reduction in the interest rate, and \$14,000 in principal forgiveness. These parameterization choices are designed to approximate the modification terms at the cutoff; we could instead obtain the same amount of payment reduction on each side if we gave each borrower a 160 point basis point reduction in the interest rate and also gave maturity extension to 40 years to the borrower on the left-hand side. We assume that nominal house prices grow at 5.8 percent per year, which is the average annual growth rate of the FHFA housing price index from 2011-2016 (Federal Housing Finance Authority (2019)).

We find that the typical borrower on the left-hand side of the cutoff spends an additional year underwater. Appendix Figure 29a plots the projected value of the home and the loan's unpaid balance under each type of modification. It shows that a typical borrower on the right-hand side reaches the above water mark in the third year after modification. On the left-hand side, the borrower reaches the above water in the fourth year.<sup>7</sup> Defaults after these

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<sup>7</sup>The difference is even smaller in many high-LTV geographies who had bigger price declines during the crisis followed by sharper rebounds. For example, Las Vegas had the biggest price drop of any MSA between 2006 and 2010, and it experienced average annual nominal house price growth of 11 percent between 2011 and 2016. A borrower in Las Vegas on the left-hand side of the cutoff would have only spent an additional five months underwater compared to a borrower on the right-hand side of the cutoff.

points in time are very unlikely to result in foreclosure because the borrower would prefer to sell the house and get back her home equity.

The additional year underwater raise the risk of foreclosure modestly, but by less than the reduction in defaults generated by the additional payment reduction. To project default rates beyond the two-year horizon that we observe in the JPMCI data, we use public tabulations of the performance of HAMP loans. These tabulations include default rates of HAMP recipients from three months to five years after modification. We fit a regression model where  $default_t = \beta \log(t)$  and show the projections in Appendix Figure 29b. We project default rates for the left-hand and right-hand sides using  $default_t^{\{LHS,RHS\}} = default_{24 \text{ months}}^{\{LHS,RHS\}} \frac{default_t^{perf}}{default_{24 \text{ months}}^{perf}}$  and fit the regression model from above separately for each group. We project a three-year default rate for the right-hand side group of 41.2 percent and a four-year default rate for the left-hand side group of 35.8 percent. Defaults with significant foreclosure risk are 5.4 percentage points (about 13 percent) lower for borrowers on the left-hand side. Thus, maturity extension appears to benefit borrowers even after incorporating how the additional time underwater exacerbates foreclosure risk.

These calculations change little when we incorporate prepayment. The intuition for why prepayment is unimportant is that the prepayment rate on recently modified loans is quite low (less than 1 percent annually).

#### C.4 Efficient Default-Minimizing Modification Design

In this section, we explain how our results help to answer the question of how to efficiently design mortgage modifications for all borrowers, and we quantify the potential gains of implementing our proposed design relative to the mortgage modifications that were actually pursued. Our analysis focuses on applying the empirical lessons from the episode we study towards designing a uniform mortgage modification for all borrowers. We do not consider additional gains that could potentially be achieved by targeting specific modification offers to borrowers with particular characteristics.

**Lessons for Modification Design** – Our empirical findings that default is responsive to liquidity but not wealth suggest a simple principle for reducing mortgage defaults: the “best” modification steps are those that achieve immediate payment reduction at the lowest possible cost. The costs of payment reductions must be borne by either lenders or taxpayers. Minimizing costs per dollar of immediate liquidity provision will maximize the amount of payment reduction (and hence default reduction) that the market will find privately optimal to provide on its own and identify the most efficient use of government subsidies. Hence, we call modifications that follow this minimum-cost structure “efficient default-minimizing modifications.”

To uncover the efficient default-minimizing structure of mortgage modifications we evaluate the five modification steps that were used in various combinations in the public and private modification programs we are aware of: maturity extension, temporary five-year interest rate reduction, principal forbearance, permanent interest rate reduction, and principal forgiveness. These policies all reduce mortgage payments for at least five years but have very different costs. We calculate these costs for an illustrative mortgage with the characteristics of the average loan at the HAMP eligibility discontinuity.<sup>8</sup>

<sup>8</sup>This loan has a 6.7 percent fixed interest rate, a 23-year remaining term, an unpaid balance of \$248,000 and a loan-to-value ratio of 131 percent.

We rank modification steps by their cost-effectiveness in Appendix Figure 21a. We calculate the change in mortgage terms needed to reduce payments by 10 percent and the change in the NPV of payments owed to the lender from this modification step. We find that maturity extension is NPV-*positive* by nearly \$20,000 for the lender because the interest rate on the loan is higher than the lender’s discount rate (recall that interest rates on new mortgages fell substantially during the crisis, so the spread between the old rate on the mortgage being modified and the available return on new mortgages widened). Temporary interest rate reductions are NPV-negative, costing about \$14,000, while principal forbearance, permanent rate reduction, and principal forgiveness are even more NPV-negative, with costs between \$22,000 and \$32,000. Appendix Figure 21a also shows that the same ranking of policies continues to hold when we examine the change in the NPV of expected payments incorporating the yield curve and the impact of modifications on default and prepayment risk.<sup>9</sup> Across all modification steps we find that the costs to the lender are lower when we incorporate these features and that the ranking from least cost to highest cost is the same.<sup>10</sup> Finally, Appendix Figure 25 shows that this ranking is robust to alternative assumptions about recovery rates and interest rates.

We use this ranking to propose efficient default-minimizing modifications for a range of payment reduction targets. As we note above, by efficient we mean that we use the lowest-cost policies first. We assume there are limits to how much some mortgage terms can plausibly change. For example, maturity extension cannot possibly reduce payments below the interest payments on the unpaid balance. We restrict the set of possible modifications using the quantitative limits implemented by the HAMP, Chase, and GSE modification programs: we allow for maturity extension up to 40 years, we allow temporary interest rates to be reduced to 2 percent, and we allow permanent interest rates to be reduced to the prevailing 30-year mortgage rate. Adopting these limits makes our characterization of the potential gains from more efficiently-designed mortgage modifications conservative.

The efficient default-minimizing modification uses maturity extension, followed by temporary interest rate reduction, followed by principal forbearance. The efficient modification never uses principal forgiveness. In Appendix Figure 21b, we depict the cost of such a modification. Maturity extension can reduce payments by up to 18 percent if the loan term is extended from 26 to 40 years, payments can be reduced by an additional 41 percent by reducing the temporary interest rate to 2 percent, and forbearance can reduce payments even further.<sup>11</sup> The figure also shows the cost of principal forgiveness. Together these two lines

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<sup>9</sup>We use the same assumptions to calculate expected payments NPV calculation as in Appendix C.3. We need one new assumption, which is a function that maps payment reduction to default rates for lower levels of payment reduction. We extrapolate from our evidence on default rates and Appendix Figure 30 shows the function.

<sup>10</sup>One limitation of this ranking is that it assumes that payment reduction over a five-year horizon is the main driver of a policy’s effect on a loan’s lifetime default rate. Implicitly, we are assuming that payments in the next five years are the relevant variable for the re-default probability of a borrower who is currently in financial distress. This assumption could be wrong in either direction. On one hand, perhaps reducing payments for one year or three years can deliver much of the same reduction in defaults with even less cost. On the other hand, default could rise substantially when payments rise in year six, though empirical evidence in Scharlemann and Shore 2019 shows that this effect is quantitatively small. A payment increase of about 10 percent in year six raises the default rate in the following year from 3.8 percent to 4.6 percent. Unfortunately, our empirical evidence does not speak to this question, and we think that understanding the optimal duration of payment reductions is an important area for future research.

<sup>11</sup>Our estimates imply that a lender would break-even by offering a payment reduction of 65 percent through a maturity extension to 40 years, a temporary interest rate reduction down to 2 percent, and principal forbearance of 18 percent of the loan balance. Appendix Figure 31a shows that when we incorporate the

show the envelope of the least and most costly ways to achieve various amounts of payment reduction and hence various amounts of default reduction.

The modification policies actually used during the crisis occupy an intermediate position within the envelope of the most efficient and least efficient policies. Recall that HAMP targets a PTI ratio of 31 percent. This means that borrowers receive widely-varying amounts of payment reduction on the basis of their initial PTI ratio. As discussed in Section II.A, HAMP first reduces the permanent interest rate, then the temporary interest rate, then extends the mortgage term, and finally does principal forbearance. Appendix Figure 21b shows that by first reducing the permanent rate, HAMP has a marginal cost for small payment reductions that is very close to the cost of principal forgiveness and much larger than the efficient default-minimizing modification, which uses maturity extension first. HAMP Principal Reduction Alternative (PRA), which offered principal reduction as the first step in a mortgage modification (see Section II.A for details) has an even higher cost from payment reductions. The figure shows that HAMP PRA follows the upper envelope of the least efficient policy until a payment reduction of 11 percent is reached. After that, the path of the HAMP PRA line parallels the standard HAMP modification.

In contrast to HAMP modifications, the GSE and private modifications we analyze are much closer to the efficient frontier. Recall from Section IV.A that the GSEs and Chase offered most borrowers maturity extension, followed by permanent rate reduction, temporary rate reduction (Chase only), and then principal forbearance.<sup>12</sup> Because the GSEs and Chase had a specific payment reduction target, we depict them in Appendix Figure 21b using dots rather than the lines that we used to depict HAMP.

**Potential Gains from More Efficient Modification Design** – Our results imply that modifications in a future crisis can be redesigned to make borrowers, lenders, and taxpayers better off. To gauge the magnitude of the potential gains, we evaluate our proposed modification structure in comparison to HAMP, where the median borrower received a 38 percent payment reduction. We consider two ways to quantify the potential gains of more efficient modifications.

First, using our “efficient default-minimizing modification” structure we find that the same median payment reduction could have been provided at \$67,000 lower cost per modification to lenders and taxpayers. This is equal to 27 percent of the unpaid balance of a typical loan.<sup>13</sup> Aggregating over all 1.8 million HAMP modifications, this implies a potential unnecessary cost of \$121 billion. Taxpayers spent around \$27 billion subsidizing HAMP modifications (Government Accountability Office 2016 ). Therefore, our results imply that HAMP could have been designed with no taxpayer subsidies and a much lower cost to investors, while maintaining the same amount of payment reduction.

Alternatively, if we allocate all the gains from redesigning modifications to reducing borrower payments, the same amount of lender and taxpayer cost can be used to achieve substantially more default reduction. Again considering the median HAMP borrower, we find that it was possible to reduce payments by 72 percent (rather than the 38 percent that actually occurred) at the same cost. Extrapolating from our empirical results using

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social cost of foreclosure of \$51,000 from U.S. Department of Housing and Urban Development (2010), the break-even point for lenders and taxpayers is a payment reduction of 70 percent. Appendix Figure 31b shows that the same broad patterns hold for payments owed NPV as for expected payments NPV.

<sup>12</sup>Chase also offered principal forgiveness to some borrowers with high LTVs.

<sup>13</sup>Although our estimate of the potential value from well-designed modifications is large, it is similar to prior work by Maturana (2017) showing that private modifications raised the NPV of a loan by 16 percent of the unpaid balance.

the function mapping payment reduction to default rates shown in Appendix Figure 30, we find that this quantity of payment reduction would have cut default rates by one-third. Aggregating over all HAMP modifications, this implies that 267,000 defaults could have been avoided at no additional cost to lenders or taxpayers. Looking forward, one caveat to achieving these gains in a future modification program is the extent to which the diminishing returns to payment reduction may be more pronounced in alternative (perhaps less severe) economic environments.

Our proposed approach has benefits beyond improving outcomes for borrowers in a government program. For example, the potential default reduction would be even larger if we considered redesigning *all* the 10 million public and private modifications completed in the Great Recession. Furthermore, when incorporating the \$51,000 social cost of foreclosures estimated in U.S. Department of Housing and Urban Development (2010), the default reduction just from redesigning HAMP would have generated \$6 billion in social value. Taken together, these results suggest that the gains in a future modification program could be divided such that borrowers, lenders and taxpayers all benefit relative to what was implemented in the Great Recession.

Another advantage of the efficient default-minimizing modification is that it is likely to generate less ex-ante moral hazard. Some borrowers who did not have a liquidity problem may have defaulted on their mortgages in order to become eligible for the generous subsidies. Mayer et al. (2014) document increased defaults after Countrywide announced a generous modification program. The Home Affordable Refinance Program was specifically created to allow underwater borrowers to refinance without needing to become delinquent to get HAMP. Compared to HAMP, a modification with little change in the NPV of payments owed may not be attractive to borrowers who are current on their mortgages, and, even if it is, the resulting modifications will not be costly to lenders or taxpayers.

#### **C.4.1 Ex-ante Mortgage Design**

Our empirical results about ex-post debt restructuring also help inform the theoretical debate about optimal ex-ante mortgage design. A number of recent papers have analyzed alternative mortgage contracts with built-in features designed to assist households overcome periods of financial distress. Eberly and Krishnamurthy (2014) propose a fixed rate mortgage (FRM) with a one-time option to convert to an adjustable-rate mortgage (ARM), and Guren, Krishnamurthy, and McQuade (2018) develop an equilibrium model of the housing market to evaluate this contract. They find that the option to convert to an ARM is more effective than alternative contracts because it front-loads the payment reduction to the borrower, and does so at similar cost to the lender. Our results provide empirical evidence that contracts front-loading payment reductions to households in financial distress will be more effective at preventing defaults than an alternative contract with equal cost to the lender that spreads payment reductions throughout the mortgage term.

Similarly, we also provide empirical support for proposals specifically contemplating the option to extend mortgage maturities. Campbell, Clara, and Cocco (2018) compare an ARM with a refinance option to an ARM with the option to temporarily allow for interest-only payments with a corresponding increase in its maturity. They find that this maturity extension option outperforms a refinance option because it provides similar liquidity-provision benefits to borrowers at much lower cost to lenders. Our results show that immediate payment reduction can indeed be effective at reducing defaults even if structured with offsetting payments in the future so as to minimize costs to lenders.

## D Partial Equilibrium Life-cycle Model with Housing

We argue that the inability of underwater borrowers to monetize the wealth gains from principal reduction can explain why they are far less sensitive to housing wealth changes than borrowers in other economic conditions. In the changes examined in prior research, housing wealth gains expanded borrowers’ credit access. Mian and Sufi (2014) show that equity withdrawal through increased borrowing can account for the entire effect of housing wealth on spending between 2002 and 2006. But if homeowners face a collateral constraint rather than a “natural” borrowing limit, allowing them to monetize the present value of their minimum expected lifetime net worth, principal reductions that still leave borrowers underwater will not immediately relax this constraint.<sup>14</sup> Indeed, Defusco (2017) shows that a significant fraction of the additional borrowing arising from house price gains is due to relaxing collateral constraints. If borrowers cannot immediately monetize the wealth gained by debt forgiveness, it may not be surprising that they do not respond by increasing consumption.<sup>15</sup>

On the other hand, even if borrowing constraints are not relaxed immediately, it is possible that forward-looking agents building up a buffer of assets could respond if they believed principal forgiveness would relax their constraints in the near future. We calculate that since borrowers remained underwater and collateral constraints had tightened, it would take eight years before the average principal reduction recipient in HAMP would expect to be able to increase borrowing as a result of these principal reductions. A dynamic incomplete markets model of household optimization is useful for understanding whether such a lengthy delay can indeed explain why borrowers did not increase consumption.

We describe such a model in the remainder of this appendix. Section D.1 describes the model setup, Section D.2 describes the model’s parameterization, Section D.3 describes the model’s predictions for consumption (including an extended discussion of Figure 6 in Appendix D.3.5), and Section D.4 describes the model’s predictions for default.

### D.1 Setup

We consider a partial equilibrium life-cycle model of household consumption and default decisions. Households live for a maximum of  $T$  periods. The first  $T_y - 1$  periods correspond to working age, the subsequent periods to retirement.

Households maximize expected utility, have time-separable preferences, and discount utility at rate  $\beta$ . Per-period utility is

$$U(c_{it}, d_i) = \frac{c_{it}^{1-\gamma}}{1-\gamma} - d_i \mathbf{1}(t=0)\psi$$

where  $c_t$  is non-housing consumption,  $d_i$  is an indicator variable equal to 1 if the household defaults, and  $\psi$  is a utility cost of defaulting. This additive default cost follows the structure in Campbell and Cocco (2015), Hembre (2018), Kaplan, Mitman, and Violante (2017), and Schelkle (2018). It reflects the moral and social stigma associated with defaulting on debt

<sup>14</sup>Beraja et al. (2019) document a related channel: underwater borrowers are usually not able to refinance their mortgages. See Carroll (1992) and Aiyagari (1994) for discussions of natural borrowing limits.

<sup>15</sup>The possibility that liquidity can explain the lack of response is also consistent with prior research looking at large price declines. Mian, Rao, and Sufi (2013) and Kaplan, Mitman, and Violante (2016) both document a non-linearity in the consumption response to house price declines, with a large MPC for small declines but a decreasing MPC for large declines. Mian, Rao, and Sufi (2013) suggest that the non-linearity they document could be caused by smaller responses once borrowers become underwater. Similarly, our evidence suggests that for borrowers who start substantially underwater, gains in housing wealth do not affect their consumption.

obligations as well as moving costs. We discuss the timing of default at the end of this section.

Agents consume a fixed quantity of housing. We assume housing and non-housing consumption are separable and, since quantity is fixed, follow Campbell and Cocco (2015) who show that under these conditions it is unnecessary to include housing explicitly in household preferences.<sup>16</sup> In the first period, agents are endowed with a home with market price  $P_{i1}$  and a 30-year fixed rate mortgage with balance  $M_{i1}$  and interest rate  $r$ . We assume home prices evolve deterministically according to  $\Delta \log P = g$ , where  $g$  is a constant, though we solve the model under various home price growth expectations. As long as households stay in this home, their housing costs include their mortgage payments (given by the standard annuity formula), property taxes  $\tau_p$  that are proportional to the current market value of their home, and maintenance costs  $\tau_m$  that are proportional to the initial value of their home.<sup>17</sup> Renters pay the user cost of housing for the equivalent home. Thus, housing payments are given by

$$h_{itj} = \begin{cases} M_{i1} \frac{r(1+r)^{30}}{(1+r)^{30}-1} + \tau_p P_{it} + \tau_m P_{i1}, & j = \text{owner} \\ (r - g + \tau_p) P_{it} + \tau_m P_{i1}, & j = \text{renter} \end{cases}. \quad (8)$$

If they have not defaulted, households sell their home at retirement (i.e. at  $t = T_y$ ), enter the rental market, and use the proceeds of the home sale to supplement their income for the remainder of their life.

Households can only borrow out of positive home equity, subject to a collateral constraint. Thus, their liquid assets  $a_t$  can never fall below their borrowing limit  $\underline{a}_t$  given by

$$a_{it} \geq \underline{a}_{it} = \min \{ -[(1 - d_i)(1 - \phi)P_{it} - M_{it}], 0 \},$$

where  $(1 - \phi)$  is the fraction of a house's value that can be used as collateral.<sup>18</sup> Renters are not able to borrow.

Households face an exogenous income process. During working age, labor income is given by

$$z_{it} = \Gamma_t \theta_{it},$$

where  $\Gamma_t$  reflects deterministic life-cycle growth and  $\theta_{it}$  is an i.i.d transitory shock with  $\mathbb{E}[\theta_{it}] = 1$ . During retirement, income is given by a constant social security transfer which is captured in the  $\Gamma_t$  process. Total income, including income from home sales in the first

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<sup>16</sup>Campbell and Cocco (2015) show that these preferences are consistent with preferences over housing and non-housing consumption given by  $\frac{c_{it}^{1-\gamma}}{1-\gamma} + \lambda_i \frac{H_{it}^{1-\gamma}}{1-\gamma}$  for  $H_{it} = H_i$  fixed and where the parameter  $\lambda_i$  measures the importance of housing relative to non-housing consumption.

<sup>17</sup>The assumption that maintenance costs are proportional to initial values ensures that maintaining the same home does not become more expensive simply because market home prices rise.

<sup>18</sup>In the main parameterizations of our model house price growth is positive, such that once borrowers attain positive equity they do not risk falling back underwater. With negative home price growth, the borrowing limit is given by

$$a_{it} \geq \underline{a}_{it} = \min \{ \min \{ -[(1 - d_i)(1 - \phi)P_{it} - M_{it}], 0 \}, a_{it-1} \}$$

in order to prevent forced deleveraging of liquid assets.

period of retirement, is<sup>19</sup>

$$y_{it} = \begin{cases} \Gamma_t \theta_{it} & t < T_y \\ \Gamma_t + (1 - d_i)(P_{it} - M_{it}) & t = T_y \\ \Gamma_t & t > T_y \end{cases}.$$

Households can invest in a liquid asset earning a rate of return  $r$ . End of period assets evolve according to

$$a_{it} = (1 + r)a_{i,t-1} + y_{it} - c_{it} - h_{itj}.$$

We will often discuss our results in terms of cash-on-hand  $m_{it} = (1 + r)a_{i,t-1} + y_{it}$ .

We model default as a one-shot decision. Households begin the first period with a given mortgage, home price, and asset level. They then observe their first-period income shock, and decide whether to default or hold the house until retirement. This provides a simple way to analyze the short-term default decisions which we study empirically in Section II.C. In Section D.4 we study how changing the initial conditions by modifying a borrower's mortgage affects their default decision in the model and compare this to our empirical results.

We solve the household problem recursively using the method of endogenous gridpoints suggested in Carroll (2006). This generates optimal consumption paths and the initial default decision.

## D.2 Parameterization

The main parameter values are summarized in Appendix Table 5. We assume that each period corresponds to one year. In our baseline case we assume households start life at age 45 and live with probability 1 until retirement at age 65. Survival probability shrinks every year during retirement, and households are dead with certainty by age 91, as assumed by Cagetti (2003). We solve the model for different first-period ages from 35 to 55 to examine the effect of principal reduction at different ages.

We follow Carroll (2012) who assumes income shocks have a lognormal component as well as an additional chance of a large negative shock. The large negative shock, which we call unemployment, captures the idea that the income process has a thick left tail (Güvenen, Ozkan, and Song 2014). Formally, income shock  $\theta$  is distributed as follows:

$$\theta_{it} = \begin{cases} b & \text{with probability } p \\ \frac{\delta_{it}(1-b)p}{1-p} & \text{with probability } (1-p) \end{cases} \quad (9)$$

where  $\log \delta_{it} \sim \mathcal{N}\left(-\frac{\sigma_\delta^2}{2}, \sigma_\delta^2\right)$ ,  $p$  is the probability of unemployment, and  $b$  is the unemployment replacement rate. This ensures that  $E[\theta_{it}] = 1$ . All income risk, including unemployment, is turned off in retirement. We follow Carroll (1992) and set  $\sigma_\delta = 0.14$ .<sup>20</sup> We use data from Güvenen, Ozkan, and Song (2014) to parameterize  $b$  and  $p$ . They show that the tenth percentile shock between 2008 and 2010 was a reduction in income of 50 percent, so we set  $p$  to 0.1 and  $b$  to 0.5. This large negative shock is critical to understanding default dynamics, which we explore in more detail in Section D.4. The life-cycle growth path of permanent income  $\Gamma_t$  is from Carroll (1997).

<sup>19</sup>In all of our parameterizations borrowers have positive equity by retirement.

<sup>20</sup>Carroll (1992) allows for temporary and permanent income shocks, each with a standard deviation of 0.1. We only have one income shock, whose standard deviation we set to  $\sqrt{0.10^2 + 0.10^2} = 0.14$ .

All parameters in our model are real, so we set the interest rate  $r$  to 2 percent. This matches the average 30-year mortgage rate from the Freddie Mac Conforming Loan Survey for the period 2010-2014 (4.1 percent) minus the average expected inflation on 30-year Treasury bonds over the same period (2.1 percent). We assume a collateral constraint  $\phi$  of 0.2, such that homeowners can only borrow up to 80 percent of the value of their home. This matches the caps for cash-out refinancing from Fannie Mae and Freddie Mac, as well as evidence from Corelogic (2016) that average CLTVs on new HELOC originations fell 20 points from their peak in 2004 when CLTVs of 100 were possible. In our baseline model we set real annual house price growth  $g$  at 0.9 percent, which is the average from FHFA’s national index between 1991 and 2010, as well as the expected annual price growth from home price futures in 2011, though we test the sensitivity of our results to alternative house price growth rate paths. We follow Himmelberg, Mayer, and Sinai (2005) and set the property tax rate to 1.5 percent and the maintenance cost to 2.5 percent. These parameters generate a first-period user cost of housing of 5.1 percent, similar to the empirical estimates in Diaz and Luengo-Prado (2008) and Poterba and Sinai (2008), who find 5.3 percent and 6 percent, respectively.

We choose baseline preference values of  $\beta = 0.96$  for the discount factor and  $\gamma = 4$  for the coefficient of relative risk aversion. Our choice of a relatively high value for  $\gamma$  is not important for our consumption results, but is necessary in order to generate optimizing double-trigger behavior.<sup>21</sup>

We estimate our final parameter  $\psi$ , the utility cost of default, such that the first-period default rate in the model matches the 10 percent first-year default rate for moderately underwater borrowers in our data. Since our empirical default results focus on borrowers below 150 LTV, we allow default to rise above 10 percent for more underwater borrowers. We estimate  $\psi$  to equal 5.4 utils. To translate this into meaningful units, we calculate that this is equivalent to a 10 percent permanent income loss. This loss is in line with other estimates in the literature that uses structural models with default costs to match observed default rates. Schelkle (2018) builds a model to match the rise in default rates in the U.S. between 2002 and 2010 and estimates a default cost equal to 8 percent of permanent income. Kaplan, Mitman, and Violante (2017) calibrate a default cost to match the foreclosure rate in the late 1990s and find a cost which is equal to 4 percent of permanent income for the median household, and approximately 7 percent for mortgagors. Hembre (2018) studies default behavior for all HAMP modifications and finds that a cost equal to 70 percent of per-period consumption is necessary to explain observed default rates.

## D.3 Consumption

### D.3.1 Consumption Response to Principal Reduction in Model

A reduction in mortgage debt levels affects today’s consumption through two channels. The first is a future cash-on-hand effect. Reducing mortgage debt reduces a borrower’s housing payments over time and increases a homeowner’s expected home equity gain when they sell the house. These translate into consumption according to the homeowner’s MPC out of cash-on-hand gains at future dates. The second channel is a collateral effect. Reducing debt levels frees up home equity that raises the household’s borrowing limit over time.

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<sup>21</sup>Our choice of a high  $\gamma$  ensures that agents default when they are hit with a bad income shock but do not default under regular economic circumstances. The model exhibits this behavior because when  $\gamma$  is high, the value function for the agent paying her mortgage is much more concave than the value function for the agent who is defaulting, generating a region where default is sensitive to income. In contrast, when  $\gamma$  is low in our model, LTV is the primary determinant of default decisions, which is inconsistent with our empirical findings. We discuss this choice in more detail in Section D.4.

This change translates into consumption today according to the homeowner’s MPC out of increased collateral in future dates. We show this decomposition formally in Appendix D.3.3.

This clarifies that the key forces determining the consumption response to debt forgiveness are the timeline under which debt reductions translate into higher cash-on-hand (through lower payments or through home sale) and increased borrowing capacity, and the borrower’s MPC out of these future cash-on-hand and future collateral gains. Berger et al. (2018) show that the response to housing wealth gains achieved from increased house prices depends on current home values and the marginal propensity to consume out of wealth. Our analysis adds that the MPC out of wealth gains will depend crucially on a borrower’s initial home equity position. Underwater borrowers receiving a dollar of housing wealth are only able to monetize this gain by borrowing or selling their home once they are above water, which may be far in the future. In this case, the consumption response today will depend on a household’s MPC out of expected cash or collateral gains far in the future.

In our model, households are unresponsive to cash or collateral gains far in the future. To explain why they are unresponsive, it is helpful to divide households into three categories based on their cash-on-hand relative to permanent income. First, households with low cash-on-hand consume all their assets each period. These households are only responsive to cash or collateral they can access today. Second, households with moderate levels of cash-on-hand are building up a buffer of assets. For these households, near-term cash or collateral reduces the precautionary value of saving in the current period and increases consumption. However, cash or collateral grants several years in the future have no precautionary value and do not affect spending. Third, high cash-on-hand households consume only the annuity value of cash grants regardless of their timing. We show this visually by plotting the consumption response to future cash and future collateral gains in Appendix Figure 32.

The lack of response to future cash and collateral gains can explain why HAMP-type principal reduction failed to increase consumption. We explore this in the model by considering the consumption response to principal forgiveness for a household matching the typical HAMP borrower. By design, principal reduction in HAMP had no incremental effect on payments relative to alternative modifications until year six, and we find that it would be eight years before principal forgiveness translated into increased borrowing capacity.<sup>22</sup> Since no group of borrowers has strong consumption responses to cash and collateral gains so far in the future, we find that this can explain why HAMP did not lead to increased spending in the short term.

This can be seen visually in Appendix Figure 33, which shows the consumption function out of increasing amounts of principal reduction for borrowers starting at an LTV of 150 (the median LTV for HAMP principal reduction recipients). It shows that the consumption function out of home equity gains is S-shaped, convex in a small region below the collateral constraint, and concave above it. Borrowers are insensitive to principal reductions until such reductions bring them close to their constraint. Principal reduction for deeply underwater borrowers does not relax current constraints and has little precautionary value, hence consumption is unaffected. In Appendix Section D.3.6 we show quantitatively that the MPC out of HAMP-like principal reduction is close to zero under a variety of alternative parameterizations.

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<sup>22</sup>This eight years estimate is based on the HAMP mortgage contract, which left borrowers underwater with a median LTV ratio of 114 after modification, an assumption that homeowners can only borrow up to 80% of the value of their home in this time period, and an assumption of 1% real annual house price growth based on contemporaneous futures contracts. See Appendix D.3.4 for details supporting the second and third assumption.

Our result contrasts with debt overhang models in which forced deleveraging leads to depressed consumption. For example, in Eggertsson and Krugman (2012) and Guerrieri and Lorenzoni (2017), debt is modeled as a one-period bond. In this setting, when a credit crunch reduces the borrowing limit, borrowers who find themselves beyond the borrowing constraint are forced to immediately cut consumption in order to delever. Applying this assumption to the mortgage setting implies that when housing prices fall such that the LTV ratio becomes greater than 100, borrowers need to immediately repay their outstanding debt until they are above water. Under this hypothetical scenario, borrowers receiving principal reduction would see immediate decreases in the amount of forced repayment. Principal reduction would increase consumption by reducing debt overhang.

But in practice, as in our model, mortgages in particular are long-term loans. Nothing forces borrowers to immediately delever when they are far underwater. Modeling housing debt as a long-term contract removes a mechanical link between debt levels and consumption present in some of the prior literature, and reduces the expected effectiveness of mortgage debt reduction policies. Other recent papers to consider the effect of debt and housing wealth in settings with long-term contracts include Berger et al. (2018), Chen, Michaux, and Roussanov (2019), Kaplan, Mitman, and Violante (2017), and Justiniano, Primiceri, and Tambalotti (2015).

### D.3.2 Comparison to Boom-Era Housing MPC Distribution

Our model makes reasonable quantitative predictions about consumption out of housing wealth changes, for which prior empirical papers provide an external benchmark. We focus on replicating estimates corresponding to the pre-2009 period and use Mian, Rao, and Sufi (2013) as our external benchmark. We use our model to estimate the MPC out of housing wealth gains for age 45 borrowers with different initial LTVs. We endow each agent with cash-on-hand equal to two years of permanent income, which is the median non-housing wealth for all homeowners in the 2007 Survey of Consumer Finances (SCF).<sup>23</sup> We then calculate the MPC for these agents at different LTV values, and weight them according to the distribution of LTV in 2007 reported in Carter (2012).<sup>24</sup> Thus, heterogeneity in household leverage is a source of MPC heterogeneity as in Auclert (2019).

Appendix Table 6 reports the average MPC out of an additional dollar of housing equity for the average borrower as well as for high-leverage (but still above-water) borrowers. We find MPCs of 8 and 15 cents, respectively. These are similar to the average MPC for homeowners of 9 cents reported in Mian, Rao, and Sufi (2013), and the 18 cent MPC of homeowners living in counties with average LTV ratios above 90. In our model, high-leverage above-water borrowers have high MPCs because they have low housing wealth and are the most borrowing constrained.

### D.3.3 Sufficient Statistic Expression for Principal Reduction

To build intuition for the effect of principal reductions on consumption, we consider a simplified version of our model without a default option, in which we can develop a straightforward formula for the effect of debt levels on consumption. In this case, a homeowner's problem can be written as a function of four state variables: cash-on-hand ( $m_{it}$ ), the wealth gain from home sale at retirement ( $w_{iT_y} = P_{iT_y} - M_{iT_y}$ ), and the vectors of housing payments and collateral constraints for the rest of life ( $\vec{h}_i, \vec{a}_i$ ). We can then decompose the effect of a

<sup>23</sup>Mian, Rao, and Sufi (2013) show that wealth does not vary with LTV, so we assign this median number to all borrowers.

<sup>24</sup>Carter (2012) reports LTV distributions in 2005 and 2009, so we take the average.

change in mortgage debt level at date  $t$  in the following way:

$$\begin{aligned}
\frac{dc_{it}(m_{it}, w_{iT_y}, \vec{h}_i, \vec{a}_i)}{dM_{it}} &= \frac{\partial c_{it}}{\partial w_{iT_y}} \cdot \frac{\partial w_{iT_y}}{\partial M_{it}} + \sum_{s=t}^T \frac{\partial c_{it}}{\partial h_{is}} \cdot \frac{\partial h_{is}}{\partial M_{it}} + \sum_{s=t}^T \frac{\partial c_{it}}{\partial a_{is}} \cdot \frac{\partial a_{is}}{\partial M_{it}} \\
&= \underbrace{MPC_{t, w_{iT_y}} \frac{\partial w_{iT_y}}{\partial M_{it}} + \sum_{s=t}^T MPC_{t, h_{is}} \cdot \frac{\partial h_{is}}{\partial M_{it}}}_{\text{Future cash-on-hand effect}} + \underbrace{\sum_{s=t}^T MPC_{t, a_{is}} \cdot \frac{\partial a_{is}}{\partial M_{it}}}_{\text{Collateral effect}} \quad (10)
\end{aligned}$$

Equation (10) shows that a reduction in debt levels affects today's consumption through two channels. The first is a future cash-on-hand effect. Reducing mortgage debt increases a homeowner's expected home equity gain when they sell the house and reduces their housing payments every year. These translate into consumption according to the homeowner's marginal propensity to consume today out of wealth gains in future dates. The second channel is a collateral effect. Reducing debt levels frees up home equity that raises the household's borrowing limit over time. This change translates into consumption today, according to the homeowner's marginal propensity to consume out of increased collateral in future dates.

#### D.3.4 Difficulty of Accessing Housing Wealth During Recovery

Three pieces of evidence suggest that borrowers could expect a lengthy delay before being able to access wealth from principal reductions. First, borrowers in our sample are still underwater even after receiving principal reductions, with a median LTV ratio after modification of 114. Furthermore, these leverage ratios only account for first liens, while home equity depends on all liens on a property (i.e., the combined loan-to-value ratio, or CLTV).

Second, the time series of mortgage credit origination shows that credit constraints had tightened during the recovery. Appendix Figure 34a shows mortgage originations by borrower credit score from 2000 to 2015. This covers all mortgages, including second mortgages and home equity lines of credit (HELOCs). It shows that originations dipped sharply after 2007, and for low-credit score borrowers, originations have never recovered. Borrowers receiving HAMP principal reductions had mean FICO scores of 579, with 85 percent below 660, the cutoff for the red line in the figure. This evidence suggests that even with positive equity, the low credit-score borrowers in our sample may have been unlikely to obtain additional housing-related credit. This is further reinforced by Appendix Figure 34b, which shows the time series of average CLTV ratios for borrowers able to obtain HELOCs in a given year. The average CLTV ratio fell 20 points between 2004 and 2009, indicating a tightening of underwriting constraints. Mian and Sufi (2014) argue that tightening credit conditions could explain why the house price recovery from 2011 onward did not contribute significantly to economic activity, since in this case the borrowing channel is restricted. Our results support this hypothesis for underwater borrowers. Furthermore, Agarwal et al. (2018) show that credit expansions during the recovery were more likely to benefit higher-FICO borrowers, precisely those least likely to respond by increasing borrowing.

Third, home price expectations were depressed relative to the boom years. Home price future contracts indicated a market expectation of 1 percent real annual home price growth between 2011 and 2016 (U.S. Department of Housing and Urban Development 2016).

Appendix Figure 35 shows the evolution of borrowing limits and mortgage payments around principal reduction for the average borrower according to our model and using the assumptions described above. We consider an average household with first period income  $y_t = 0.85$  units of permanent income, based on Bernstein (2017) who finds that borrowers

receiving mortgage modifications during the recent crisis had temporarily low incomes. We set initial LTV equal to 150, the median pre-modification LTV for borrowers receiving principal reduction in our difference-in-differences analysis.<sup>25</sup> For our treatment group, we then reduce their mortgage balance by \$70,000, bringing them to an LTV of 106.<sup>26</sup> To mimic the policy implemented in HAMP we keep mortgage payments for households who have not defaulted fixed for five years.

Principal reduction translates into increased borrowing capacity and increased wealth with a considerable delay. Principal reduction eventually increases borrowing limits, but these increases do not occur for another eight years. This is because even after receiving principal reduction, borrowers remain slightly underwater. Furthermore, to be able to borrow against their home given the collateral constraint they need to get down to an LTV of 80, which takes several years under baseline price growth and mortgage principal pay-down schedules. The bottom panel shows that housing payments decrease substantially, but only starting six years in the future.

### **D.3.5 Collateral Constraints Drive a Wedge Between MPC Out of Housing Wealth and MPC out of Cash**

The inability to monetize housing wealth drives a wedge between an underwater borrower’s marginal propensity to consume out of cash and their marginal propensity to consume out of housing wealth. Figure 6 (in the paper, not the appendix) demonstrates this visually. In this figure we take low-cash-on-hand borrowers at various LTV levels and plot the MPC out of \$1 of cash or \$1 of housing wealth gained by principal reduction.<sup>27</sup> As in the empirical results in Mian, Rao, and Sufi (2013), borrowers near their collateral constraint have a high MPC out of housing wealth gains. However, borrowers far underwater are unresponsive to housing wealth changes even though they are highly responsive to cash transfers. This highlights one way that housing wealth is special. Because it can only be monetized when borrowers have positive home equity above a collateral constraint, borrowers respond less to housing wealth gains than they do to cash.

One implication of the wedge between cash and housing MPCs is that in a period where many borrowers are underwater and collateral constraints have tightened, high leverage is a bad “tag” for targeting policies that increase housing wealth, even though it is a good “tag” for targeting policies trying to provide cash to borrowers with high MPCs. Our model suggests that low-wealth, underwater borrowers would have an MPC out of cash around 30 cents. The government spent an average of \$0.30 to subsidize each dollar of principal forgiveness in HAMP, for a total government cost of \$4.6 billion. Our model suggests that if the same amount of money had been spent on direct transfers to borrowers, the partial equilibrium spending increase would have been \$1.4 billion, ten times more than even the upper bound of our estimates for the consumption response to principal forgiveness would suggest. Policies seeking to raise aggregate demand by increasing the housing wealth of leveraged borrowers will be ineffective precisely when policymakers might otherwise want to use them.

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<sup>25</sup>This corresponds to an initial home price equal to \$173,000 (or 3.25 units of permanent income) and an initial mortgage debt of \$259,000 (or 4.88 units of permanent income).

<sup>26</sup>The median LTV post-modification in our data is actually 114, because borrowers’ unpaid mortgage payments are capitalized into the new mortgage balance. We abstract from this in our model, though it would only serve to further reduce the effect of principal reduction.

<sup>27</sup>In this experiment, housing payments fall immediately when debt is reduced, unlike in Section D.3.1 where we delayed payment relief in order to mimic principal reduction in HAMP. We consider the average household from our policy experiment, so we set cash-on-hand to 0.85.

The low MPC out of housing wealth for underwater borrowers can help explain the sluggish response to house price gains during the recent recovery (Mian and Sufi 2014). The borrowers ordinarily most responsive to wealth gains may have found themselves unable to translate increased housing wealth into disposable wealth. This also points to a limitation of one of Fisher’s policy recommendations for reversing “debt deflations” (Fisher 1933). He suggests reflating asset prices. Our results suggest that this may be ineffective at increasing demand for those who are underwater unless pursued aggressively enough to bring them into positive equity.

### **D.3.6 Consumption Response to Principal Reduction Under Alternative Parameterizations**

In our model, principal reduction is ineffective under a variety of alternative parameterizations. Appendix Table 7 reports the MPC for the principal reduction policy experiment described above under various alternative assumptions. The baseline MPC is 0.3 cents per dollar of mortgage principal reduced. This is similar to our empirical results. Changing borrower age, discount rate, and risk aversion has little impact on the MPC.

Principal reduction remains ineffective even when borrowers have modest access to liquidity. To show this, we calculated the effect of principal reduction assuming households had access to an unused HELOC line worth \$20,000, which is twice the amount available to the average household with a HELOC in the 2015 New York Fed Consumer Credit Panel (Federal Reserve Bank of New York 2015*b*). The MPC for this household is still only 0.9 cents. The reason is that households that have access to liquidity are optimizing incorporating this liquid buffer. Principal reduction does not increase their buffer in the near term, so has little effect on the value of maintaining this buffer. This explains why even borrowers who are actively saving or deleveraging, and therefore not literally at their liquidity constraint, are unresponsive to principal reduction. Even when borrowers are saving for precautionary reasons, the increase in housing wealth gained from principal reduction is of little precautionary value because it cannot be monetized for several years.

Generating a large consumption response requires an alternative, unrealized economic environment (relaxed collateral constraints and optimistic home price growth) or an alternative policy of more generous writedowns. Setting the collateral constraint to zero such that homeowners can lever up to 100 LTV generates a moderate MPC of 4.8 cents. Even though borrowers remain underwater after principal reduction, allowing them to monetize wealth starting at 100 LTV would have some immediate precautionary value. Similarly, if households expected permanent real annual house price growth of 5 percent (equal to realized growth rates from 2000 to 2005), the MPC would be 6.2 cents because borrowers would expect to be able to monetize their housing wealth more quickly. Combining both of these assumptions about the economic environment generates a large MPC of 24.2 cents. However, the period when principal reduction was implemented is exactly when neither of these conditions was likely to hold. In the aftermath of the crisis, home price growth expectations were tepid and credit supply was tight.<sup>28</sup>

## **D.4 Default**

In this section, we explore the effect of principal reduction on default. We show that when defaulting imposes utility costs in the short-term, most households only default when they face a large negative income shock. This means that default is relatively insensitive to mortgage balance until borrowers are substantially underwater.

<sup>28</sup>See U.S. Department of Housing and Urban Development (2016) for house price expectations data and Corelogic (2016) for evidence of tightening collateral constraints.

#### D.4.1 The Effect of Principal Reduction on Default

In forward-looking models with a housing asset and labor income risk, default emerges from two motives: (1) an agent is so far underwater that her house is no longer a good investment and (2) default offers a way to access short-term liquidity when cash-on-hand is low. In our model, the core tradeoff underwater borrowers face when making their default decision is whether the short-term gain from reduced housing payments is worth the utility cost of defaulting and the lost resale value of the house at retirement.<sup>29</sup> Both the costs and benefits of default vary with current payment levels, current incomes, and total debt obligations. When borrowers have high current payments or low current incomes, the short-term payment relief is particularly valuable because it allows borrowers to avoid making severe cuts to consumption. Similarly, when total debt levels are high, the costs of default are low because the house is less valuable as an asset.

To show the effect of principal reduction and relate it back to our empirical results, we simulate changes in mortgage principal holding payments constant. We assume homeowners receive modifications at age 45. To match the low assets of delinquent borrowers in the PSID, we set initial assets  $a_t = 0.01$  units of permanent income. We set initial LTV equal to 119, the median pre-modification LTV for borrowers in our regression discontinuity analysis (Table 1a). We then vary the LTV, holding mortgage payments for households that have not defaulted fixed for five years, after which payments fall according to the annuity formula in equation (8) applied to the new mortgage balance.

Appendix Figure 36a shows that for a given current payment level and LTV ratio, there is a cash-on-hand level below which households will find it optimal to default. The more underwater the household, the smaller the income shock necessary to push them to default. For borrowers in our baseline scenario, the income cutoff for defaulting is both low and relatively insensitive to debt levels. In particular, below LTVs of about 150, low-asset borrowers will only default if their income is less than three-quarters of its permanent level, a shock of about two standard deviations. This means that default is most likely to occur for borrowers who are hit with “unemployment,” the large liquidity shock in our income process.

We find that default rates are insensitive to principal reduction for the typical borrower. Appendix Figure 36b plots the default rate in the first period after modification for borrowers with various amounts of principal reduction. In our baseline case, additional principal reduction is ineffective below an LTV of about 160. For such moderately underwater borrowers, the gain from defaulting is not worth the cost unless they are hit by a liquidity shock. However, far underwater borrowers have much higher default rates because they default even in the absence of liquidity shocks.

#### D.4.2 An Optimizing Double Trigger Model of Default

Borrowers in our baseline case exhibit what we call “optimizing double trigger” behavior. In the “double trigger” class of models, agents default when two conditions are triggered: (1) they are underwater and (2) face negative income shocks. In the most basic of these models, agents are not optimizing. While negative equity is necessary for default, the level of negative equity is irrelevant (see description of these models in Gerardi et al. 2018). Agents do not consider the costs and benefits of defaulting, they simply default when they are forced

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<sup>29</sup>Because we assume house prices evolve deterministically, our model does not capture the option value of mortgages. With house price uncertainty, paying a mortgage is equivalent to purchasing a call option, giving the borrower the right to “buy” future home equity gains, if realized, at the price of the unpaid balance on the mortgage. Incorporating house price uncertainty would reduce the gain from defaulting and would lead us to estimate a smaller utility cost of defaulting to match the average 10 percent default rate.

to by an income shock that leaves them without enough funds to pay their mortgage (Guren and McQuade 2018).

In our model, agents are optimizing. At moderate levels of underwaterness, it is only optimal for agents with large liquidity shocks (i.e., unemployment in our model) to default. Default is insensitive to negative equity in this region because the costs of default are high and the gains for an employed agent are low. However, beyond about 160 LTV, their optimizing behavior generates a steep causal relationship between LTV and default. These borrowers are defaulting for what is sometimes referred to as “strategic” reasons, that is they default even when their payments are affordable.

The optimizing double trigger behavior, with a small effect of LTV on default at low LTV levels followed by a steep slope at high LTV levels, is consistent with recent dynamic models of mortgage default such as Schelkle (2018) and Campbell and Cocco (2015).<sup>30</sup> Campbell and Cocco (2015) study default decisions in a calibrated model where borrowers are liquidity constrained and face labor income, house price, inflation, and interest rate risk. In their model the kink occurs at about 135 LTV. Below this level, the option value of staying in the mortgage outweighs the gains of defaulting for most borrowers. Our empirical evidence suggests that default is insensitive to LTVs even at slightly higher LTV ratios, which is consistent with adding a utility cost of default to this type of model. The result that borrowers without income shocks do not exercise their default option until substantially underwater is consistent with empirical evidence in Bhutta, Dokko, and Shan (2017), who show that the median homeowner without an income shock does not default until their LTV is greater than 174.<sup>31</sup>

In our model, the key force generating our results is that the income cutoff for defaulting is not very sensitive to the size of mortgage debt. This generates a flat, positive-default-rate region followed by a steep slope at high LTV levels. Generating this region, which is consistent with our empirical evidence, relies on three empirically plausible features of our model. First, most underwater borrowers do not default because they would incur a utility cost of default. This is supported by survey evidence in Guiso, Sapienza, and Zingales (2013), who find that about 80 percent of homeowners consider it morally wrong to default when payments are affordable. Second, agents face thick-tailed income shocks (Guvenen, Ozkan, and Song 2014).<sup>32</sup> Third, households are risk averse and default when hit with a very bad income shock. When we reduce risk aversion to  $\gamma = 2$ , default rates are either zero or high, with no flat, positive-default-rate region.<sup>33</sup>

Our empirical evidence favors models like ours over alternatives that generate smooth upward-sloping relationships between LTV and default. Kau, Keenan, and Taewon Kim (1993) and Stanton and Wallace (1998) build off of the frictionless option model that predicts a single cutoff LTV value above which all borrowers default. Because the cross-sectional relationship between LTV and default is smooth, these authors propose introducing a distribution of additional default costs, which generates a distribution of cutoff values and therefore

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<sup>30</sup>See also Li, White, and Zhu (2011).

<sup>31</sup>Similarly, Foote, Gerardi, and Willen (2008) study homeowners in Massachusetts who were underwater in the early 1990s and find that fewer than one percent eventually lost their home to foreclosure.

<sup>32</sup>If we eliminate this feature of our income process, we estimate both a smaller stigma cost in order to match an average 10 percent default rate, and we find that default is sensitive to LTVs even at low LTV levels, which is inconsistent with our empirical results.

<sup>33</sup>The short-term liquidity motive for default is most valuable when risk aversion is high. When risk aversion is low, default is largely a function of LTV. As the utility function becomes increasingly linear, the function mapping LTV to default becomes increasingly binary, approaching a rule of thumb where *no* agents default below an LTV cutoff and *all* agents default above the LTV cutoff.

a smooth relationship between LTV and default. We add a distribution of default costs in our model in Appendix Figure 37. In this model, a distribution of default costs is isomorphic to increasing the variance of temporary income shocks and so we simulate this by increasing the variance of income shocks. We show that this simulation generates a smooth relationship between LTV and default. However, our empirical results, which find that default is insensitive to LTVs for moderate amounts of underwaterness, reject this parameterization of our model.

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