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Interest Rate Pass-Through: Mortgage Rates, Household Consumption, and Voluntary Deleveraging[†]

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Exploiting variation in the timing of resets of adjustable-rate mortgages (ARMs), we find that a sizable decline in mortgage payments (up to 50 percent) induces a significant increase in car purchases (up to 35 percent). This effect is attenuated by voluntary deleveraging. Borrowers with lower incomes and housing wealth have significantly higher marginal propensity to consume. Areas with a larger share of ARMs were more responsive to lower interest rates and saw a relative decline in defaults and an increase in house prices, car purchases, and employment. Household balance sheets and mortgage contract rigidity are important for monetary policy pass-through. (*JEL* D12, D14, E43, E52, G21, R31)

There has been a long-standing debate among economists regarding the effects of interest rates on the real economy (e.g., Bernanke and Gertler 1995). During the Great Recession, the Federal Reserve substantially reduced the overnight lending rate target and made large purchases of mortgage-backed securities in an attempt to stimulate household spending and support the prices of assets such as houses. This paper exploits this setting to explore how changes in interest rates impact the real economy. It establishes the effects of lower mortgage rates on adjustable-rate

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mortgages (ARMs)—induced by a decline of interest rates during an accommodative monetary policy period—on household choices such as consumption and mortgage debt repayment. In doing so, we assess the extent to which household mortgage contract rigidities affect the transmission of lower interest rates through household balance sheets onto the real economy.

Lower interest rates are generally thought to affect firms' investment and households' consumption by reducing the cost of external finance. Estimating this effect is challenging when the terms of debt contracts are rigid—as in the case of most fixed-rate mortgage contracts—since the pass-through of lower interest rates to households might be limited, hindering the ability of expansionary monetary policy to stimulate households' consumption. Moreover, isolating borrowers' consumption and saving responses to a change in interest rates is complicated because interest rates and refinancing decisions are endogenous and depend on a household's finances and credit-worthiness. For instance, households with a bad credit history may be unable to refinance; the same may apply to liquidity-constrained households, who cannot pay the closing costs of their preexisting mortgage.[1] Similarly, households living in counties where the housing market experienced a more severe crash are less likely to have enough equity to be able to refinance, muting their consumption response to the drop in interest rates. Finally, changes in interest rates over time can themselves reflect the broader economic condition of households.

To overcome this identification challenge, we exploit the automatic changes in monthly payments of borrowers with adjustable-rate mortgages (ARMs) originated between 2005 and 2007.[2] These mortgages have a fixed interest rate for the first five years, which is automatically adjusted at the end of this initial period based on prevailing interest rate indices (e.g., LIBOR or Treasury rate). These cohorts experience a sudden and substantial drop in their mortgage interest rates and scheduled mortgage payments upon reset, as the interest rate indices based on which these loans adjust reached a very low level during the 2010–2012 period. For example, ARMs originated in 2005 benefited from an average reduction of 300 basis points in the interest rate in 2010. Importantly, these large reductions in mortgage payments were unexpected at the time of loan origination, occurred regardless of the financial position or credit-worthiness of borrowers, and did not require active decisions by borrowers such as loan refinancing.[3] Importantly, during our sample period the borrowers we focus on had very limited refinancing opportunities.[4] The key to our identification strategy is the ability to exploit the timing of the interest

[1] For instance, Agarwal, Driscoll, and Laibson (2013) point out that the incentives might depend on the size of the mortgage, as they estimate the spread between the current and the refinancing interest rate that justifies refinancing at 1.1 to 1.4 percentage points for mortgages between $100,000 and $200,000. See also Campbell (2006).

[2] A similar identification strategy was first exploited by Fuster and Willen (2013) to investigate the effects of changes in monthly payments on defaults.

[3] Arguably, these interest rate shocks are much larger and more persistent than typical monetary policy innovations. That said, we believe that interest rate shocks do provide insights into understanding effects of monetary policy shocks.

[4] Residential borrowers in the United States are generally free to refinance their loans as the usage of prepayment penalties is fairly limited (except for subprime loans). We note however that a variety of frictions significantly limited the refinancing opportunities for borrowers during our sample period. In particular, many households were ineligible to qualify for traditional refinancing options due to the significant decline of their home values (and thus high loan-to-value (LTV) ratios) and deterioration of their credit-worthiness. Only the creation of the Home Affordable Refinancing Program (HARP) allowed many underwater homeowners to refinance, but this program was only available to agency-backed mortgages and was fully implemented beyond the main period of our study (see Section VIC for more discussion).

rate adjustment. Effectively, we exploit *within-borrower* variation around the interest rate adjustment. The empirical design allows us to circumvent the endogenous setting of interest rates for a given borrower. Instead, we can focus on the effect of a reduction in monthly mortgage payments on borrowers' consumption and repayment (deleveraging) behavior. We rely on a comprehensive panel dataset on US mortgage borrowers. This dataset results from a merge of borrowers' mortgage data with their monthly credit reports provided by a credit bureau, which allows us to observe monthly information on all their liabilities, including mortgage debt, auto loans, and other revolving debt.

We first document the effect of interest rate resets on monthly payments for households with five-year ARMs and show that monthly payments fell on average by about $940 (53 percent) upon reset. Payments tend to stay constant before the reset month, as well as afterward when they fall, suggesting that monthly payments significantly and persistently declined for households with such contracts. Our specification accounts for extensive borrowers' characteristics as well as county-time fixed effects, which capture any unobserved time-varying variation at the county level and allow for different trends for each origination cohort. Exploiting this sharp change in monthly payments faced by borrowers—an increase in their disposable income—we document three main findings.

First, we find a positive consumption response after the reset. Our main measure of consumption is car purchases. We identify the instances in which households purchase a car by taking out an auto loan. We show that the households that experience a drop in monthly mortgage payments increase their consumption of durables on average by about $108 per month (about a 35 percent relative increase).[5]

Second, we analyze voluntary partial repayments of mortgage debt—our proxy for the strength of the borrowers' deleveraging motive. We observe all payments made toward mortgage and other debt, e.g., equity loans and home equity line of credit. We show that households use more than 10 percent of the increase in disposable income to repay their debts more quickly. Although our first result suggests that low interest rates boost consumption, the second result indicates that this effect is attenuated by the desire to deleverage from high levels of debt accumulated during the boom years.[6]

Third, we complement these findings by documenting significant heterogeneity in borrowers' responses to rate reductions depending on their income and wealth. Specifically, the consumption of borrowers with little housing wealth (high loan-to-value ratios) is almost twice as responsive to rate reductions as those of other borrowers. These borrowers also deleverage less, leaving more resources available to consume. We also find that low-income households tend to consume significantly more and deleverage less than high-income ones in response to the rate reduction.

[5] We also employ information from store credit cards, such as purchases at chains like Best Buy or Macy's, as a measure of other forms of consumption and show that they also increased in response to the change in monthly payments.

[6] We note that some households receiving rate reductions simultaneously reduced their outstanding mortgage debt balances while at the same time increasing the amount of their auto debt. Given that the positive income stimulus is spread over time—and given the substantial cost of purchasing a new car—it is not surprising that households would use auto debt to finance their new car purchases. Notably, since households experience significant increases in disposable income after the reset, it is also reasonable that they end up using some of this additional income to voluntarily repay some of their mortgage debt.

These findings are broadly consistent with life-cycle household finance models that suggest the consumption of households with lower income and wealth tend to be more responsive to positive income shocks (e.g., Zeldes 1989; Carroll and Kimball 1996; Carroll 1997).

Our findings so far are estimated on a sample of non-agency prime borrowers with ARMs for owner-occupied residences. These borrowers are similar to average homeowners in the United States, with loan-to-value ratios less than 80 percent for the majority of our sample. Next, we extend our findings to a sample of conforming ("agency") loans—i.e., loans issued with credit guarantees from government-sponsored enterprises (GSEs)—that account for the vast majority of residential mortgages in the United States. In essence, this sample allows us to test for the broader applicability of our findings in the non-agency sample.

We find that borrowers with conforming five-year ARMs resetting around the same period as our main sample also experienced a sizable reduction in monthly mortgage payments. However, due to the differences in borrower and contract characteristics, the treatment effect is smaller: borrowers with conforming loans experienced a 23 percent reduction in payments on average (amounting to about $280 per month) compared to a 50 percent reduction in our main sample (about $940 per month).

Nevertheless, despite the relative difference in the magnitude of the treatment effect, we find broadly similar effects of rate reductions on borrower behavior in the conforming sample. In particular, among borrowers with non-agency (agency) ARMs, the dollar increase in new car spending during the first and second year of reduced payments amounts to about 8.1 percent (12.3 percent) and 13.6 percent (18.2 percent) of the additional monthly liquidity. Likewise, borrowers with non-agency (agency) ARMs allocate about 7.7 percent (7.5 percent) and 8.3 percent (6.0 percent) of the additional monthly liquidity generated by rate reductions to repay their mortgage debt during the first and second year of reduced mortgage rates. Finally, like our main sample, we also find stronger consumption responses among borrowers with lower income and wealth. Together, these findings suggest that our results are likely to be externally valid, as they generalize to the broader agency sample as well.

Throughout the analysis we account for borrower and contract characteristics as well as county-by-time fixed effects. However, it is possible that there are mortgage-specific time trends that are correlated with the household's consumption or debt repayment choices. In order to address this concern, we also consider a difference-in-differences research design that exploits variation in the timing of rate resets of ARMs originated at the same time but with different initial fixed-rate interest periods. The design is predicated on the fact that borrowers with five-year ARMs (the treatment group) have a five-year fixed rate period. In contrast, borrowers in the control group have ten-year ARMs (seven-year ARMs for conforming loans), implying that they can serve as a reasonable counterfactual for the treatment group during the five-to-ten-year period (five-to-seven-year period for conforming loans), when these loans do not reset. We find very similar results when we employ this alternative empirical strategy.

We also find that, consistent with prior studies (e.g., Tracy and Wright 2012; Fuster and Willen 2013), a reduction in mortgage payments leads to a substantial decline in mortgage default rates. In doing so, we also confirm that these effects

appear to be quite strong among more typical borrowers with conforming ARMs. Our estimates based on this sample indicate that a reduction of monthly mortgage payments by about 20 percent reduces the likelihood of mortgage default over two years by about 40 percent relative to the mean delinquency rate. These effects are particularly pronounced among borrowers with relatively high LTV ratios and limited access to credit.

Finally, in the last part of our analysis, we explore the impact of lower mortgage rates on broader economic activity by examining regional outcomes such as housing prices, aggregate durable consumption, and employment. To do so, we exploit the significant heterogeneity across zip codes in the share of mortgages that are of adjustable rate type. The fraction of adjustable-rate mortgages in a zip code is generally persistent over time and was determined prior to the large declines in interest rate indices that occurred during the recent crisis. Thus, we can trace the effects of these rate declines on economic outcomes using variation in this ex ante measure of regional exposure.

Indeed, we show that the fraction of outstanding ARMs in a zip code as of 2006 is highly predictive of interest rate pass-through during the period 2007–2012. In other words, the average mortgage rate in regions with a higher fraction of ARMs reacts more to the decline in interest rate indices relative to regions with a smaller ARM share. We take a number of steps—such as accounting for a host of observables, using a propensity score matched sample, and an instrumental variable analysis—to address the natural concern that zip codes with a larger share of adjustable-rate mortgages could be different from those that have a lower share.

Consistent with our earlier evidence, we show that regions with a higher concentration of ARMs experienced a significant decline in prevailing mortgage interest rates following a drop in major interest rate indices. These more exposed regions also saw a relative decrease in consumer debt default rates, lower rates of house price decline, increases in durable consumption (auto sales), and a relative improvement of employment growth in the nontradable sector. Overall, this evidence indicates that a reduction in mortgage rates during the Great Recession had an economically meaningful impact on foreclosures, delinquencies of nontargeted consumer debt, durable consumption, house prices, and employment (at least in the near term). This evidence highlights the importance of mortgage debt rigidity in the transmission of interest rate changes onto the real economy.

Our empirical strategy allows us to capture the local general equilibrium response to mortgage rate declines: the effects that we estimate are the sum of a direct effect among borrowers with ARMs and an indirect effect of other households in the same region benefiting from the resulting increase in local demand. However, it falls short of estimating the aggregate general-equilibrium effect, such as an economy-wide multiplier of low interest rate policy: we do not observe the response of lenders or investors holding the mortgage bonds to changes in interest rates.

Our paper contributes to the household finance literature and especially to studies that investigate the impact of interest rates on household behavior (e.g., Gross and Souleles 2002; Agarwal et al. 2017).[7] It is closely related to the literature that assesses

[7] See Campbell (2006) and Tufano (2009) for recent surveys of this literature.

household responses to income changes and government stimulus programs (e.g., Agarwal, Liu, and Souleles 2007; Parker et al. 2013; Kaplan and Violante 2014).[8] In the context of this literature, we provide an empirical assessment of the household response to a different form of stimulus: the prolonged reduction in mortgage rates due to low interest rates during the accommodative monetary policy period following the economic crisis. Our setting is unique because the size and persistence of the change in the cost of servicing mortgage debt reduces payments each month and by thousands of dollars over the term of the loan. In this regard, since in our setting the change in cash flow is much more persistent relative to other settings investigated in the literature, the theory predicts that our estimate of a marginal propensity to consume from extra liquidity could be higher relative to prior studies. Finally, by exploiting regional variation in exposure to lower mortgage rates, we also extend this literature and provide novel evidence on the possible effects of lower interest rates on local economic conditions.

Our paper is also connected to studies that explore the impact of mortgage rate resets and renegotiations on defaults and foreclosures. Previous research has confirmed that homeowners who face smaller mortgage payments are less likely to default (Tracy and Wright 2012; Fuster and Willen 2013), which reduces the extent of foreclosures. In contrast, we take advantage of mortgage performance data matched to a panel of consumer credit records to assess the impact of lower mortgage rates on consumer spending and debt repayment. Recently, Cloyne, Ferreira, and Surico (2015) show that borrowers in the United Kingdom and United States significantly adjust their spending in response to an interest rate change and provide evidence suggesting that the general equilibrium effect of monetary policy on households' income might even be quantitatively more important than the direct cash-flow effect.

The literature surrounding the recent financial crisis, most notably the extensive work of Mian and Sufi (2014), has emphasized the importance of residential housing and household leverage in understanding the scope and depth of the recession.[9] Agarwal et al. (2017) provide evidence that debt relief programs, when used with sufficient intensity, may meaningfully reduce foreclosures and delinquencies on nontargeted consumer debt and positively impact house prices. Our paper explores another approach to encourage deleveraging by reducing households' debt service obligations—namely, reducing mortgage rates during an accommodative monetary policy period. Our results, together with those of the previous work, support the view that policies designed to improve household balance sheets during economic downturns can have a sizable impact on a broad range of economic outcomes.

Finally, our paper is also related to the literature that analyzes the transmission of monetary policy onto the real economy. A sizable part of this literature has investigated the channels through which monetary policy impacts banks' lending decisions and risk-taking behavior (see Kashyap and Stein 2000). We complement

[8] Previous research has carefully examined household responses to fiscal stimulus, in the form of tax refunds (Shapiro and Slemrod 1995), rebates (Shapiro and Slemrod 2003; Johnson, Parker, and Souleles 2006; Agarwal, Liu, and Souleles 2007; Parker et al. 2013), or other transfer programs (e.g., Agarwal and Qian 2014; Hsieh 2003). See Jappelli and Pistaferri (2010) for a recent review of the literature on the consumption response to income changes and Parker (2011) for a discussion of studies on the effects of fiscal policy in recessions.

[9] See also Keys et al. (2010, 2013) and Mian and Sufi (2009).

these studies by uncovering the impact of low mortgage rates induced by low interest rate indices during accommodative monetary policy on households' consumption and saving decisions. Bernanke and Gertler (1995) show that households' expenditures on durable goods and residential investment are the components of GDP that respond most forcefully to changes in monetary policy. However, very few empirical studies have analyzed the impact of monetary policy on households' consumption behavior at a disaggregated level. The novelty of our approach is to document that the prolonged period of low interest rates boosts households' consumption through its impact on mortgage rates, but that this consumption effect may be attenuated by households' incentives to deleverage. Our findings highlight the important role of the household balance sheet channel in the transmission of interest rate shocks onto the real economy.

In doing so, our paper also highlights the important role played by frictions in the mortgage market. While most of the previous literature on monetary policy has focused on price rigidities, we highlight the importance of debt rigidities in the transmission of interest rates onto the real economy. This result connects our study to recent papers that analyze the relationship between the structure of housing finance and the monetary transmission mechanism. Calza, Monacelli, and Stracca (2013) show that the size of the effect of a monetary policy shock is significantly related to indicators of flexibility in mortgage markets. Scharfstein and Sunderam (2013) show that the strength of the housing channel of monetary policy is reduced in areas with more highly concentrated mortgage lending. We complement these papers by employing detailed micro-level data to study how households and regions differentially exposed to such frictions, due to an automatic adjustment of the interest rate on ARMs, respond to the prolonged reduction in interest rates.[10] In this regard, our empirical evidence is related to Auclert (2017), who provides a model evaluating the role of redistribution in the transmission mechanism of monetary policy to consumption—and predicts that if all US mortgages were of adjustable rates type, the effect of monetary policy shocks on consumer spending would be significantly higher.

I. Data

To conduct our borrower-level analysis, we use matched data on mortgages and credit bureau reports. Our primary mortgage sample comes from BlackBox Logic, a private company that provides a comprehensive, dynamic dataset with information on 90 percent of all privately securitized mortgages from that period, and consists of non-agency mortgage loans originated during the 2005–2007 period. Importantly, we restrict attention to prime borrowers for owner-occupied residences. Moreover, for 90 percent of this sample, the loan-to-value ratio is less than 80 percent, which further increases the representativeness of our main sample. This dataset includes information on mortgages and borrowers at origination, such as loan type, initial

[10] Our analysis is also related to a recent work by Wong (2016), who assesses the effects of demographic factors on the transmission of monetary policy to consumption driven by homeowners who refinance or enter new loans after interest rate declines. For other recent research on the heterogeneous effects of monetary policy, see, among others, Coibon et al. (2012); Sterk and Tenreyro (2016); and Aladangady (2014).

interest rate, borrower's initial credit score (FICO score), and loan amount. More importantly, this dataset provides monthly updates about the status of each mortgage, the monthly payments, the current balance, and other account-level information. Furthermore, since we know a borrowers' location, we can use zip code house price indices and the information about the current balance on the mortgage to construct a current loan-to-value ratio for each borrower.

These mortgage records are then matched with credit bureau reports from Equifax, one of the nation's largest credit bureaus.[11] Equifax provides information on households' balance sheets, specifically, monthly updates on all the loans that a borrower has, such as auto loans, mortgages, home equity lines of credit, monthly payment history, and current FICO credit score.

Using this merged data, we can precisely identify mortgages affected by automatic changes in mortgage payments due to interest rate resets. Specifically, we focus on non-agency five-year ARMs originated between 2005 and 2007, which are among the most common categories of ARMs. We restrict attention to ARMs that are interest-only for the first ten years. This allows us to cleanly identify the instances of voluntary repayment of mortgage debt by households. We only consider households for whom their mortgage is not in foreclosure nor is repaid or refinanced.[12] We also use data on another mortgage category, ten-year ARMs, which will serve as a control group to provide an additional robustness check in Section VIB.

Our data allow us to accurately measure the change in monthly mortgage payments. By observing the balance sheet information over time, we can also measure how much of the income shock will be utilized by the borrowers to voluntarily pay down their mortgage debts. We will use this measure as a proxy of voluntary deleveraging by borrowers.

Using the borrower's balance sheet data, we also construct a measure of durable spending for each borrower. In particular, as we explain in Section II, we can measure the new net consumption of cars, measured by the change in auto loan balances of borrowers. We also supplement this with another measure of consumption coming from the balance of borrowers' store credit cards (e.g., Best Buy, Macy's, etc.) to provide further evidence on households' consumption response.

To be clear, these measures likely underestimate the increase in consumption resulting from the decline in interest rates, because they do not capture any purchases made by cash, check or other means not recorded in Equifax. At the same time, we cannot observe a household's decision to save part of the reduction in the monthly payment in their checking or saving accounts. However, this only makes the significant portion of the positive income shock that we are able to account for through the credit record all the more striking.

To assess the robustness and external validity of our findings, in Sections VIA and VIB we use a similarly structured borrower-level panel dataset from a proprietary database of conforming loans securitized by a large secondary market participant. Unlike our main sample, these data consist of conforming ARMs issued with credit guarantees from the GSEs. Conforming loans account for the majority

[11] Equifax linked their credit information data to the BlackBox data using a proprietary match algorithm that merges on a variety of variables.

[12] See Section VIC for the discussion of this point.

of residential mortgages in the United States and are commonly made to borrowers with relatively high credit scores and fully documented incomes and assets. These loans must also meet the conforming loan limit, which has been $417,000 since 2006 for a single-family dwelling in a low-cost area.

Finally, in Section VII we complement our borrower-level analysis by exploiting regional variation in mortgage contract types to explore the impact of lower mortgage rates on a broader set of economic outcomes such as housing prices, durable spending, and employment. To obtain information regarding mortgage characteristics in a zip code, we collect individual loan-level information from two databases. The first source is the BlackBox database, which, as we discussed above, covers non-agency securitized mortgages in the United States. The second source is the LPS database maintained by Black Knight Financial Services, which provides similar dynamic information on agency and bank-held loans. Combining these two datasets yields almost complete coverage of mortgage loans in the United States, allowing us to compute zip-code-level characteristics for variables such as average borrower FICO credit scores, fraction of mortgages that are of ARM type, and the average mortgage interest rate.

We complement these datasets with the Equifax Credit Trends database, which contains zip-code-level consumer credit characteristics. In addition, we collect zip-code-level demographic information (e.g., median income, percentage of households with a college degree) from the Census Bureau's American Community Survey, house price indices from Zillow, and employment data from the Census Bureau's ZIP Business Patterns database.

We conclude this section by discussing the relative importance of adjustable-rate mortgages in the US residential loan market. First, we note that while fixed-rate mortgages with GSE guarantees do account for the majority of loans in the United States, adjustable-rate mortgages have played a substantial role in this market. As of 2004, more than 20 percent of outstanding residential loans in the United States were of the ARM type (based on BlackBox and LPS data). We also note that the ARM share in new mortgage originations has fluctuated substantially over time, reaching highs of 60 to 70 percent in 1994. Among loans originated in 2005, 2006, and 2007, which corresponds to our sample period, about 38 percent, 30 percent, and 16 percent, respectively, were of ARM type (based on BlackBox and LPS data). Moreover, among non-agency ARMs, loans with an interest-only feature accounted for about 39.5 percent of outstanding mortgages (based on BlackBox data). Finally, we note that the five-year ARM contract type we focus on is a commonly used product type among ARMs.

II. Summary Statistics and Main Variables

This section describes our sample and the main variable of interest. Panel A of Table 1 reports the summary statistics for the main variables. We consider both five-year and ten-year ARMs, since the latter will be used as an additional control group in Section VIB. Our main sample consists of 46,578 borrowers with non-agency five-year ARMs. These borrowers have an average origination FICO credit score of 723, and their loans have an average original mortgage balance of about $358,000, and an initial loan-to-value (LTV) ratio of 77 percent. These are "prime" borrowers with

Table 1—Summary Statistics for Borrowers with Non-Agency and Conforming ARMs

	Borrowers with five-year ARMs		Borrowers with ten-year ARMs	
	Mean	SD	Mean	SD
Panel A. Summary statistics for borrowers with non-agency ARMs				
FICO	723.3	39.4	736	39.7
Loan balance	357,949	271,600	536,342	347,622
Loan-to-value (LTV) ratio	77.11	10.01	72.82	12.05
Initial interest rate	6.44	0.76	6.14	0.52
Average monthly payment	1,921	1,471	2,700	1,819
Monthly new car spending	305.1	3,161	364.4	3,711
Fraction of borrowers buying new car per month	0.014	0.116	0.015	0.121
Voluntary mortgage debt repayment per month	52.21	400.1	88.49	619.6
Interest rate after adjustment for five-year ARMs	3.09	0.48	—	—
Monthly payment after adjustment for five-year ARMs	915.8	721.9	—	—
Number of borrowers	46,578		26,543	
	Borrowers with five-year ARMs		Borrowers with seven-year ARMs	
Panel B. Summary statistics for borrowers with conforming ARMs				
FICO	711.5	57.57	712.03	61.12
Loan balance	201,422	86,470	200,044	87,590
Loan-to-value (LTV) ratio	76.00	14.59	74.98	15.97
Initial interest rate	5.78	0.79	5.73	0.68
Average monthly payment	1,175	507	1,163	512
Monthly new car spending	166.2	2,310	172.32	2,252
Fraction of borrowers buying new car per month	0.008	0.092	0.009	0.094
Voluntary mortgage debt repayment per month	27.18	157.7	30.77	167.0
Interest rate after adjustment for five-year ARMs	3.28	0.45	—	—
Monthly payment after adjustment for five-year ARMs	902.3	408.1	—	—
Number of borrowers	71,741		21,466	

Notes: This table reports descriptive statistics for the main variables employed in the analysis. Panel A presents the main characteristics of a sample of borrowers with non-agency adjustable-rate mortgages (ARMs) having the first interest rate adjustment after five and after ten years from origination date, respectively. Panel B presents similar characteristics for a sample of borrowers with conforming ARMs having the first interest rate adjustment after five and after seven years from origination date, respectively.
Source: BlackBox Logic for borrowers with non-agency ARMs originated in 2005–2007 and a large secondary market participant for borrowers with conforming ARMs originated during the period 2005–2007.

LTV ratios similar to those of average borrowers in the United States, but they have a larger mortgage balance.

Most of the loans are indexed to six-month LIBOR, while the second largest group is indexed to one-year LIBOR, and a fraction are indexed to the one-year Treasury bill rate. The interest rate averages about 6.4 percent at origination and declines to about 3.1 percent after the reset, with a corresponding decrease in the average monthly payment from $1,921 to $915. The average decline in interest rates is thus about 3.3 percentage points, implying about a 50 percent reduction in monthly mortgage payments. Online Appendix A1 shows the distribution of the changes in the monthly payment at the time of the interest rate adjustment for our sample of ARMs. Moreover, considering our sample period for the post-adjustment period 2010–2012, the majority of these loans enjoy these lower interest rates for a prolonged period of time as major interest rate indices remained at low levels.

We now turn to our main measure of consumption: car purchases. The monthly information on auto debt balances allows us to identify instances of new car purchases financed with auto debt or car leases, since such transactions are accompanied by a

significant increase in a borrower's outstanding auto debt. Our measure of new net car purchases is equal to the change in the auto loan balance at the time of purchase.

Online Appendix Figure A2 illustrates how we are able to identify car purchases from auto debt data. It uses two examples drawn from our sample. We plot the balance of auto loans and the measure of new car purchases that we use in our analysis. The right panel represents the case in which a consumer bought two cars. These events correspond to clear spikes in the consumer's auto loan balance. The left panel, instead, describes the case in which the borrower has bought a car before the beginning of our sample and starts paying down the auto loan over time.

Using these measures we find that the average monthly expenditure on new cars by borrowers in our sample amounts to $305 and their monthly probability of purchasing a new car equals 1.4 percent (panel A of Table 1). Since our measure is computed using changes in auto loans, we also verify that financed car purchases are indeed an important fraction of total car sales. Online Appendix Figure A3 employs data from R. L. Polk & Company to plot financed car sales as a fraction of total new car sales, and shows that leveraged purchases account for about 80 percent of total car sales. Moreover, financed sales and total sales follow similar trends over time. This reassures us that, even if not fully comprehensive, our measure covers a substantial portion of car purchases.

The information on mortgage balance information over time allows us to compute the voluntary repayment of mortgage debt by borrowers. We use this measure as an empirical proxy for the extent of active consumer deleveraging. Panel A of Table 1 shows that borrowers with five-year non-agency ARMs allocate about $52 per month to voluntary repayment of their debt.

We compare these borrower characteristics with a sample of more than 26,000 borrowers with non-agency ten-year ARMs—another common category of non-agency ARMs in our data with longer than a five-year fixed rate period. This group serves as a control group in Section VIB when we make comparisons of mortgage borrowers who originated in the same month but face different ex post exposure to low interest rates. The main difference is that ten-year ARMs tend to be larger, with an average mortgage size of about $536,000 and a monthly payment of about $2,700. We also observe somewhat higher monthly car spending and voluntary debt repayment among borrowers with ten-year ARMs.

We next investigate whether the borrowers in our main sample differ from households holding other types of mortgages in any significant way. Online Appendix Table A4 provides information on the characteristics of almost 20 million mortgages originated between 2005 and 2008 (from LPS data), focusing on two main subgroups: borrowers with fixed-rate mortgages and those with adjustable-rate mortgages. Comparing data from panel A of Table 1 and online Appendix Table A4 we can see that most of the borrower and loan characteristics in our sample are fairly similar to the market average. The only important difference between these different types of mortgage holders is the average size of the loan, with our borrowers carrying substantially larger loan balances than the market average.

To assess the robustness and external validity of our findings in Section VIA, we use a sample of 71,741 borrowers with conforming five-year ARMs originated during the 2005–2007 period. Panel B of Table 1 shows the main summary statistics for this sample. These borrowers have an average origination FICO credit score

of 711, an average origination mortgage balance of $201,422, and an initial LTV ratio of 76 percent. Notably, on observable characteristics, this sample is broadly representative of typical US mortgage borrowers with mortgage balances close to population averages (see Keys et al. 2013). Moreover, the distribution of observable risk characteristics in this sample is quite similar to the overall distribution of these characteristics among borrowers with conforming loans (with ARMs and FRMs).

The conforming five-year ARMs face an average interest rate of about 5.78 percent at origination, which declines to about 3.28 percent after the first reset, with a corresponding decrease in the average monthly payment from $1,175 to $902. On average, this decline implies a 23 percent reduction in monthly mortgage payments. Moreover, these borrowers enjoy lower interest rates for a prolonged period of time (see Figure 2).

We note that the monthly average level of new car spending and voluntary debt repayment among borrowers with conforming five-year ARMs, about $166 and $27 respectively, is lower compared to borrowers with non-agency loans. This is not surprising since borrowers with conforming loans buy substantially less expensive homes, face lower mortgage balances, and are therefore likely to be less wealthy compared to our sample of non-agency borrowers with a significant share of large "jumbo" loans.

Finally, we compare the characteristics of borrowers with conforming five-year ARMs to a sample of 21,466 borrowers with conforming seven-year ARMs originated during the 2005–2007 period, which is another common product category among conforming ARMs with longer than a five-year fixed-rate period. We will use these loans as a control group in our difference-in-differences setting in Section VIB. Panel B of Table 1 shows that the observable characteristics, including mortgage balances, car spending, and debt repayment rates, are remarkably similar between borrowers with conforming five-year and seven-year ARMs. Moreover, we find that the outcome variables of these borrowers exhibit similar trends during the "pre-reset" period including their updated FICO credit scores (see online Appendix Figure A5).

III. Research Design

The key feature exploited in our study is that "hybrid" ARM contracts entail a fixed interest rate for the first five years, and an automatic adjustment of the interest rate five years after the origination. No matter the local house prices or economic conditions, households with such contracts see their mortgage rate reset to a much lower level during our sample period. As a consequence, these households experience a significant reduction in their monthly payment. A crucial factor in this identification strategy is that the monthly payment reduction is a feature of the contract and not an endogenous choice of the borrower.[13] We note that a similar identification strategy was first exploited by Fuster and Willen (2013) to investigate the effects of changes in monthly payments on mortgage defaults.

[13] Although in principle households could refinance their mortgages prior to the rate reset, we note that less credit-worthy and underwater borrowers had very limited refinancing opportunities during our sample period (see Section VIC).

At the borrower level, our identification strategy is designed to exploit the predetermined timing of the automatic reset for these ARMs, and the fact that it represents a positive income shock for households holding these mortgages during a period of significant decline of interest rates. Specifically, we compare the relevant outcomes of borrowers holding five-year ARMs who already experienced an interest rate reset with the outcomes of borrowers with the same mortgage contract, but who have not yet experienced the change in their interest rate. The key assumption behind this strategy is that households whose mortgage is reset in May 2010 are basically comparable to households that experience their reset, in, say, December 2010.

This identification strategy has several advantages. First, by restricting attention to households holding the same contract, we avoid picking up any difference in preferences that could drive the choice of an ARM rather than a fixed-rate mortgage. Second, the rich panel structure of our data allows us to account for household and time fixed effects in all of our specifications. In addition, we are able to account for individual credit and mortgage characteristics that absorb potential heterogeneity correlated with consumption and debt payment behavior of borrowers. Third, our research design is not subject to the endogeneity of the interest rate itself, as the timing of the adjustment for each household is independent of macroeconomic events.

Finally, we are able to account for county-time fixed effects and cohort-time fixed effects, where cohort is defined as the year of origination. The first set of fixed effects allow for heterogeneous trends in different regions. This assures us that our results are not driven by other factors, like local economic activity, that may be correlated with changes in monthly interest rates. The second set captures unobserved variation across cohorts that might affect households' responses to interest rate resets. For instance, lending standards in 2006 might have been weaker than those in 2005, as house prices rose. These fixed effects account for any differences in monthly payments across cohorts that could be impacted by the interest rate adjustment. These controls also account for differences in characteristics correlated with borrowers' consumption behavior, such as credit-worthiness and expectations about future income growth.

Formally, our main specification takes the following form:

$$Y_{i,t} = \alpha_i + \sum_{\theta=-1}^{2} \beta_\theta \mathbf{1}_{i,t\in\theta} + \Gamma X_{i,t} + \varepsilon_{i,t}, \tag{1}$$

where i denotes the households, t the month, θ the year since the interest rate adjustment, and $\mathbf{1}_{i,t\in\theta}$ is a dummy variable that takes the value of 1 if a given month occurs in the year θ since the interest rate adjustment. The main outcome variables $Y_{i,t}$ analyzed in the next section are the *within-borrower* monthly changes in the consumption of durables, as proxied by the purchase of a car, and partial mortgage prepayments as a measure of voluntary deleveraging. As further evidence, we also consider purchases made with credit cards and the repayment of home equity and home equity lines of credit. The main coefficients of interest are the β_θ that capture, for instance, the consumption response to the change in the interest payment one year before, one year after, and two years after the adjustment of the interest rate. Finally, α_i is the borrower fixed effect and $X_{i,t}$ is a vector of borrower's characteristics

designed to capture any residual individual heterogeneity. This includes the borrower's FICO score, as a proxy for his financial constraints or credit-worthiness, zip-code-level house prices to capture local economic conditions, and county-month fixed effects.[14] In some specifications, we also interact $\mathbf{1}_{i,t\in\theta}$ with indicators for different types of borrowers to analyze heterogeneity in the response to the decline in monthly mortgage payments. We double cluster the standard errors at both the borrower and at the month level.

To analyze the dynamics of the borrower response to changes in mortgage payments we also estimate a version of specification (1) where instead of "year" time dummies we include a set of quarterly fixed effects for the loan's life. This allows us to estimate the change in the relevant outcomes during the four quarters preceding the interest rate reset as well as during each of the quarters following the rate reset, with observations from the period preceding the rate reset serving as the excluded category.

One potential concern with the empirical strategy above is that there might be a mortgage-specific trend that could affect our results. In particular, since the timing of the reset is collinear with the time dummies, we cannot control directly for the age of the mortgage, which might be correlated with the household's consumption or debt repayment choices. In order to address this concern, in Section VIB we consider a difference-in-differences research design that exploits variation in the timing of rate resets of adjustable-rate mortgages originated at the same time but with different initial fixed-rate interest periods.

The difference-in-differences design is predicated on the fact that borrowers with five-year ARMs (the treatment group) have a five-year fixed rate period, while borrowers with a commonly observed longer maturity ARMs (the control group) have a longer fixed interest rate period (ten years for non-agency and seven years for conforming loans). After the fixed period concludes, these loans periodically reset thereafter (e.g., once a year) based on the relevant index to which they are benchmarked. Thus, after the five-year period the five-year ARMs reset to a new interest rate, while the longer maturity ARMs remain in their fixed-rate period (until year ten or year seven). A key identification assumption behind this empirical strategy is that the relevant outcomes of five-year and longer maturity ARM borrowers would follow similar patterns (up to a constant difference) around the five-year ARMs scheduled reset dates in the absence of a meaningful interest rate change.

More formally, to analyze the household-level response to rate resets, we run difference-in-differences regressions of the form

$$(2)\qquad Y_{i,t} = \alpha_i + \gamma_{5Y-ARM}\mathbf{1}_{i\in 5Y-ARM} + \sum_{\theta=-4}^{5}\left[\gamma_\theta \mathbf{1}_{i,t\in\theta} + \delta_\theta(\mathbf{1}_{i\in 5Y-ARM} \times \mathbf{1}_{i,t\in\theta})\right] + \Gamma X_{i,t} + \varepsilon_{i,t}.$$

In the specification above, the dependent variable, Y_{it} is the within-borrower monthly change in the relevant outcome variable and $\mathbf{1}_{i\in 5Y-ARM}$ is a dummy that takes a value of 1 if the loan of borrower i is of five-year ARM type and is 0 otherwise. The

[14] We obtain similar results when instead of controlling for the county-month fixed effect we control for the cohort-time fixed effect or allow for different trends depending on the size of the initial monthly payment.

variable γ_{5Y-ARM} captures any baseline difference between outcomes of borrowers with five-year and longer maturity ARM contracts (ten-year ARM for non-agency and seven-year ARM for conforming loans). Further, γ_θ with $\theta = \{-4, \ldots, 4\}$ captures the vintage effect of having a mortgage which is outstanding in any of the four quarters preceding and the four quarters following the five-year loan age mark, while γ_5 captures the effect of having a mortgage which is outstanding during the second year after the reset. The term δ_θ is our key coefficient of interest, indicating the difference-in-differences effect between the treatment and control group in any of the four quarters preceding and the four quarters following the first interest rate adjustment, and during the second year after the first reset date. Finally, X_i is a vector that includes borrower- and loan-level characteristics, such as the initial rate and FICO score, as well as month of origination fixed effects and state fixed effects. The panel structure of our data allows us to exploit within-borrower variation before and after the rate resets. For robustness, we also interact the product dummy and loan age dummies with other controls to allow for more dynamic flexibility.

IV. Main Results

We first investigate the effect of automatic interest rate resets on monthly mortgage payments. We then analyze the effect of mortgage payment reductions induced by rate resets on households' durable spending and debt repayment patterns. We note that throughout our analysis the main outcome variables at the borrower-level are measured at a monthly frequency.

A. *Impact of Rate Resets on Monthly Mortgage Payments*

Column 1 of Table 2 shows the regression results of scheduled monthly mortgage payments on the time dummies for the year before and one and two years after the change in the interest rate. In particular, we estimate specification (1) with one year before the reset period as the excluded category. Each coefficient captures the within-borrower average dollar change in the monthly mortgage interest payments in that year for the sample of all five-year ARMs with a ten-year interest-only payment originated between 2005 and 2007. As mentioned earlier, this specification controls for a variety of borrower, loan, and regional characteristics including the borrower's fixed effect, the borrower's FICO credit score, the CLTV ratio, and the log of the house price index in the location of the property. As we observe, borrowers experience a significant reduction in their mortgage interest payments after the reset, amounting on average to $940 per month.

Column 2 of Table 2 shows the corresponding results from a similar specification to the one in column 1. The difference here is that we normalize the monthly mortgage payment by the size of the initial payment. While there is no economically significant reduction before the interest rate adjustment, there is a reduction in the monthly payment of about 53 percent after the adjustment. Moreover, this effect persists for the next two years. As before, the estimated magnitude and statistical significance of these payment changes are robust to a rich variety of controls.

Panel A of Figure 1 investigates the timing of these effects by plotting the results from an event study around the time of the interest rate reset (time zero). It illustrates

Table 2—Impact of Rate Reductions on Monthly Mortgage Payments, New Car Spending, and Voluntary Debt Repayment

	Mortgage payment (1)	Mortgage payment/initial payment (2)	New car spending (3)	New car spending/initial payment (4)	Probability of new car purchase (5)	Voluntary debt repayment (6)	Debt Repayment/ initial payment (7)
One year before	−11.50	−0.0036	23.36	0.006	0.144	1.92	0.000
	(5.65)	(0.0008)	(10.01)	(0.0075)	(0.0339)	(1.74)	(0.001)
One year after	−943.4	−0.528	77.09	0.043	0.345	68.25	0.041
	(34.79)	(0.0058)	(12.92)	(0.0106)	(0.0440)	(3.776)	(0.0023)
Two years after	−937.6	−0.529	108.2	0.072	0.478	74.92	0.044
	(27.12)	(0.0065)	(18.92)	(0.0165)	(0.0851)	(6.307)	(0.004)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,222,930	2,222,930	2,223,476	2,223,476	2,223,476	2,037,981	2,037,981
R^2	0.704	0.737	0.001	0.001	0.001	0.006	0.004

Notes: This table presents the estimates from the OLS regressions that examine the relationship between the timing relative to the first interest rate reset occurring five years after the loan's origination and a range of outcomes in a sample of borrowers with non-agency five-year ARMs. The main independent variables are dummies identifying different time periods before and after the reset date. One year before identifies the 12 months before up to 1 month before the interest rate adjustment. One year after includes the month of the adjustment up to 11 months after. Two years after includes 12 months after the adjustment up to 23 months after. Column 1 examines the impact of mortgage interest rate resets on the scheduled monthly mortgage payment in dollars, while column 2 conducts similar analysis with scheduled monthly mortgage payments scaled by the initial monthly mortgage payment as the dependent variable. Column 3 examines the impact of mortgage interest rate resets on the monthly net dollar amount of new car purchases (financed with debt), while column 4 conducts similar analysis with net new car purchases scaled by the initial monthly mortgage payment as the dependent variable. Column 5 examines the relationship between the timing of mortgage interest rate resets and a dummy variable that takes the value of 1 in a month in which the borrower buys a new car and is 0 otherwise (reported in percentage terms). Column 6 examines the impact of mortgage interest rate resets on the voluntary monthly dollar repayment of mortgage debt, while column 7 conducts similar analysis with the voluntary repayment of mortgage debt scaled by the initial mortgage payment as the dependent variable. Other controls include a variety of borrower, mortgage, and regional characteristics including borrower FICO credit score, LTV ratio, zip-code-level house price controls, and fixed effects as discussed in the text. Standard errors (in parentheses) are clustered at the borrower and at the month level.

the estimated average monthly change in mortgage payments for our main sample of non-agency ARMs, normalized to zero in the pre-period. Monthly mortgage payments are constant for the period before the rate reset and drop significantly at the moment of the event. Consistent with our results from Table 2, the magnitude of the drop is substantial, nearly a $1,000 on average, or about one-half the monthly payment (panel B of Figure 1). This figure also highlights an important feature of our setting. The reduction in the payment is not temporary but lasts for the entire post-period. Although these ARMs usually reset their interest rate every year after the initial fixed-rate period, the low interest rate regime that was set in December 2008 remained in place through the end of our period of observation.

In sum, the automatic reset of mortgage interest rates constituted a significant positive disposable income shock for households with ARMs during this period. In contrast to the literature on households' consumption response to income shocks, which focuses on one-time payments, such as tax rebates, we can explore the effects of a relatively persistent income shock of thousands of dollars per year.

B. *Consumption Response*

We next turn to data on monthly auto loan balances to capture the response of car purchases to a reduction in the monthly mortgage payment. As described in

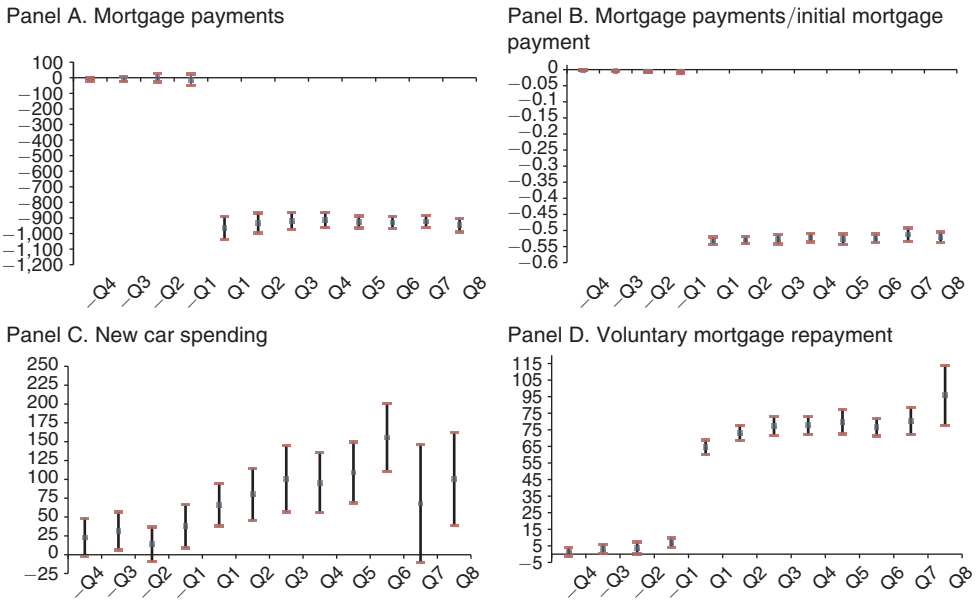

Figure 1. Monthly Mortgage Payments, New Car Spending, and Voluntary Debt Repayment

Notes: This figure shows the OLS estimates for a set of quarterly time fixed effects along with 95 percent confidence intervals that allow us to assess the change in the relevant outcomes of borrowers with non-agency five-year ARMs during the four quarters preceding the interest rate reset as well as during each of the eight quarters following the rate reset. The dependent variables are the monthly mortgage payment in dollars (panel A), the mortgage payment scaled by the initial mortgage payment (panel B), new car spending in dollars (panel C), and voluntary repayment of mortgage debt in dollars (panel D).

Section II, we can use discrete changes in the auto loan balance to identify the instances in which households purchased a car using auto financing (including car leases).

Column 3 of Table 2 shows the regression results of monthly interest payments on the time dummies for the year before and one and two years after the change in the interest rate controlling for several potential confounding effects. Borrowers substantially increase their monthly new car spending, amounting to $77 on average during the first year of lower mortgage payments and an increase of $108 during the second year. Panel C of Figure 2 explores the dynamic pattern of these effects by conducting an event study analysis around the month of the interest rate reset (time zero). Borrowers slightly increase their car consumption starting one quarter before the interest rate reset. This suggests that some households may have anticipated the mortgage payment reduction and altered their car purchasing before the reset date.[15] Importantly, however, the effect is largely present in the period after the reset and substantially increases in the subsequent quarters to an average of more than $75 per month one year after the interest rate adjustment, reaching a high of about $150 six quarters after the reset. Column 4 of Table 2 shows the corresponding results normalized by the size of the initial mortgage payment. Comparing the estimates in column 2 that capture the size of the reduction as a fraction of the initial payment

[15] Lenders are required to disclose information about future interest rate adjustments, usually at least 60 days before it occurs.

to those in column 4 is useful. It suggests that the average monthly increase in car consumption during the first year after the rate reset amounts to about 8.1 percent of the monthly liquidity generated from the rate reduction. This effect becomes larger in the second year of rate reduction, as the average increase in new car consumption during this period amounts to about 13.6 percent of the monthly liquidity generated from the rate reduction.

Finally, in column 5 of Table 2 we estimate a linear probability model to determine how the likelihood of purchasing a car is affected by the change in the monthly payment. This provides us with an estimate of households' responsiveness on the extensive margin. It shows that this probability increases by 0.35 percent a month in the first quarter after the adjustment, rising to about 0.5 percent two years after. This is an economically significant effect, given that the average monthly probability of buying a car is about 1.4 percent. Hence, households increase their car purchases by 25 to 35 percent in response to this reduction in mortgage payments.

Interestingly, these effects show a different reaction of durable consumption from the response to the stimulus identified in Mian and Sufi (2012), who estimate the impact of the 2009 Cash for Clunkers program on short- and medium-run auto purchases. They find that almost all of the additional purchases under the program were "pulled forward" from the near future. In contrast, we find that the reduction of the monthly payment significantly increased durable spending of affected borrowers, and that this effect persists over time. The difference in responses across the two studies is likely due to fundamentally different features of the underlying income shock. Unlike Mian and Sufi (2012), who examine the effects of a one-time tax subsidy, we consider a sequence of larger positive disposable income shocks that persisted over a number of years.

C. *Voluntary Mortgage Debt Repayment*

Next, we analyze the effect of rate reduction on borrowers' mortgage debt repayment. To estimate this effect, we track the changes in the debt balance for the households affected by the automatic interest rate reset. This measure allows us to assess whether additional liquidity is being used to accelerate mortgage debt repayment. A caveat is in order. While we are able to explore changes to mortgage debt, we are not able to capture other forms of saving, such as retirement accounts or savings deposits. Nevertheless, given the collapse in house prices in the aftermath of the crisis, and the high LTV ratios for the majority of the mortgages in our sample, repaying their mortgage more rapidly and building equity in their homes seems an important way for households to use additional resources. We examine this motive directly in Section V, where we also explore heterogeneous responses on this margin depending on how "underwater" a given household is on their mortgage.

Column 6 of Table 2 reports the coefficients estimated using a similar regression to equation (1). The dependent variable is the monthly reduction in the mortgage balance, computed using BlackBox data. We only consider partial prepayment, because full repayment coincides with the house being sold or the mortgage being refinanced. As we can observe, controlling for a rich variety of characteristics, borrowers spend on average an additional $68 a month to repay the principal on their mortgages in the first year and about $75 in the second year after the reset.

Moreover, there is no statistically or economically meaningful pretrend before the rate reset.

Panel D of Figure 1 confirms this finding by showing the dynamics of the average monthly amount allocated to voluntary repayment of mortgage principal around the reset date. The plot shows that in the quarter after the rate reset, households allocate an additional $65 per month to a faster repayment of their mortgage debt, and this amount increases in the subsequent quarters.

Finally, in column 7 of Table 2 we estimate a specification similar to that of column 6 but normalizing the prepayment by the initial monthly mortgage payment. We find that, on average, monthly mortgage repayment amounts to about 8 percent of the additional monthly disposable income generated by rate reduction.

To put these results in broader perspective, we can compare them with the average amount allocated by households to repay their mortgages in the pre-adjustment period. Up to 2010—the first year when any of the households in our sample benefited from the interest rate adjustment—the average amount devoted to deleveraging was about $50 per month over the 2007–2010 period. This implies that after the adjustment, households more than doubled their rate of paying down mortgage debt.

V. Heterogeneous Responses across Households

The analysis thus far has focused on the average response of borrowers to mortgage payment reductions due to rate resets. However, the theoretical literature indicates that some types of households will respond more forcefully to a positive income shock. In particular, (i) the consumption of households with lower permanent income and wealth may display a larger response to a positive income shock relative to wealthier households (e.g., Zeldes 1989; Carroll and Kimball 1996; Carroll 1997) and (ii) an important source of heterogeneous responses is difficulty in accessing credit markets. We now analyze borrowers' responses to mortgage payment reduction across several empirical measures associated with income, wealth, and credit access.

We employ several empirical proxies to capture the variation in income and wealth across households. First, we use household annual income, which we measure in the pre-interest rate adjustment period.[16] Columns 1–3 of Table 3 investigate differences in payment reductions, durable spending (new car spending), and mortgage repayment across lower and higher income groups. The "High income" flag equals 1 if a household's income is larger than the median, i.e., larger than $55,000 a year. Since we need to report the interactions between the time dummies and the income group indicator, we restrict our attention to one year before, one year after, and two years after the reset date. We only report the normalized coefficients, which makes the effect easier to interpret.[17]

The estimates of the interaction terms between high income and year dummies from column 1 of Table 3 suggest that households with above-median income

[16] We use Equifax's measure of income, based on the credit characteristics of the household including reported income from various credit applications. We use the average of this measure in the period of 12 to 24 months prior to the interest rate reset. While this measure should be taken with caution, we obtain very similar results when we use the borrower's income from their mortgage file in our conforming loan sample (Section VIIA).

[17] The estimates with quarterly dummies show exactly the same pattern.

TABLE 3—HETEROGENEOUS EFFECTS

	X = High income			X = High LTV			X = High FICO		
	Mortgage payment (1)	New car spending (2)	Debt repayment (3)	Mortgage payment (4)	New car spending (5)	Debt repayment (6)	Mortgage payment (7)	New car spending (8)	Debt repayment (9)
One year before	−0.00554	0.0273	6.86e-05	0.00132	−0.00850	0.000969	−0.00547	−0.00532	0.000264
	(0.000860)	(0.00879)	(0.00172)	(0.00147)	(0.0108)	(0.00179)	(0.000941)	(0.00853)	(0.00138)
One year after	−0.543	0.0706	0.0369	−0.520	0.0303	0.0417	−0.548	0.00312	0.0247
	(0.00580)	(0.0126)	(0.00313)	(0.00549)	(0.0145)	(0.00282)	(0.00562)	(0.0139)	(0.00268)
Two years after	−0.545	0.137	0.0435	−0.525	0.00982	0.0438	−0.547	0.0255	0.0326
	(0.00650)	(0.0219)	(0.00492)	(0.00768)	(0.0254)	(0.00601)	(0.00633)	(0.0288)	(0.00525)
(One year before) × X	0.00358	−0.0405	0.00165	−0.00958	0.0339	0.000368	0.00324	0.0178	0.00125
	(0.00175)	(0.00958)	(0.00217)	(0.00198)	(0.0140)	(0.00208)	(0.00166)	(0.0113)	(0.00172)
(One year after) × X	0.0303	−0.0529	0.00967	−0.0377	0.0485	−0.00814	0.0317	0.0592	0.0263
	(0.00314)	(0.0134)	(0.00394)	(0.00408)	(0.0214)	(0.00394)	(0.00324)	(0.0172)	(0.00327)
(Two years after) × X	0.0307	−0.124	0.00183	−0.0398	0.111	−0.000578	0.0290	0.0648	0.0176
	(0.00430)	(0.0252)	(0.00637)	(0.00491)	(0.0325)	(0.00681)	(0.00424)	(0.0302)	(0.00581)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,213,514	2,214,060	2,030,119	1,838,721	1,838,982	1,666,594	2,212,528	2,213,074	2,029,232
R^2	0.737	0.001	0.004	0.683	0.001	0.003	0.737	0.001	0.004

Notes: This table reports the coefficient estimates of OLS regressions relating the monthly mortgage payment, net new car spending, and voluntary mortgage principal repayments, all scaled by the monthly initial mortgage payment, to the timing relative to the first interest rate reset occurring five years after the loan's origination in a sample of borrowers with non-agency five-year ARMs. The main independent variables are dummies identifying different time periods before and after the reset date as well as their interaction terms with variables capturing borrower heterogeneity. One year before identifies the 12 months before up to 1 month before the interest rate adjustment. One year after includes the month of the adjustment up to 11 months after. Two years after includes 12 months after the adjustment up to 23 months after. Specifications in columns 1 to 3 include an additional independent variable High income (unreported for brevity) as well as its interaction with time dummies (reported in the table). High income is a dummy equal to 1 if the household's income, before the adjustment, is greater than the median income in our sample, and is 0 otherwise. Specifications in columns 4 to 6 include additional independent variable High LTV (unreported for brevity) as well as its interaction with time dummies (reported in the table). High LTV is a dummy equal to 1 if the loan's loan-to-value ratio one year before the adjustment is greater than 120 percent, and is 0 otherwise. Specifications in columns 7 to 9 include additional independent variable High FICO (unreported for brevity) as well as its interaction with time dummies (reported in the table). High FICO is a dummy equal to 1 if the FICO one year before the adjustment is greater than 660, and is 0 otherwise. Other controls include a variety of borrower, mortgage, and regional characteristics, as well as fixed effects in Table 2 as well as the aforementioned stand-alone dummies capturing the borrower heterogeneity. Standard errors in parentheses are clustered at the borrower and at the month level.

experience very similar reductions in monthly mortgage payments after the reset compared to below-median income households. In particular, these estimates imply that borrowers with above-median income experience a 51 percent reduction in mortgage payments compared to a 54 percent reduction experienced by borrowers with below-median income levels.

Column 2 of Table 3 shows that despite experiencing a very similar relative reduction in mortgage payments, higher-income households show a much weaker response in new car spending after the interest rate reset compared to lower-income households. In particular, the estimates in column 2 of Table 3 imply that the increase in car spending among higher-income households relative to lower-income households after the first (second) year of reduced mortgage payments is about 75 percent (90 percent) lower.

Our evidence so far suggests that lower-income households may have a significantly higher marginal propensity to consume the additional income generated from

reduced mortgage payments compared to higher-income households. Consistent with this finding, lower-income households also have a significantly lower marginal propensity to deleverage (column 3) in the first year after the interest rate reset compared to higher-income households. Notably, we obtain very similar results when we stratify borrowers based on their debt payment-to-income ratios instead of income.

Another important gauge of homeowners' wealth is the loan-to-value ratio of their mortgages. Borrowers with higher LTV ratios have less housing wealth as a share of their property value and may be relatively more liquidity constrained. Columns 4–6 of Table 3 test for differences in mortgage payment reduction, car purchases, and mortgage principal prepayment across households with different LTV ratios, respectively. We stratify borrowers into different LTV ratio groups using "High LTV," an indicator variable, that equals 1 for borrowers whose current LTV ratio is larger than 120 percent.[18] These borrowers owe at least 20 percent more on their mortgage than the house is currently worth, and are thus severely "underwater." To do so, we measure the LTV ratio over the period 12 to 24 months before the interest rate adjustment. In all specifications we control for a rich variety of characteristics including household fixed effects that capture unobserved heterogeneity at the household level and allow for differential trends across cohorts and households with different loan-to-value ratios.

We find that borrowers with high LTV ratios experience a monthly disposable income gain from rate reduction that is only slightly higher than that of other borrowers (column 4 of Table 3). However, spending on cars—following the rate decrease—by these high-LTV borrowers increases more than twice as much as low-LTV households (column 5). Moreover, borrowers with higher LTV ratios tend to deleverage less (column 6 of Table 3).

This evidence suggests that less wealthy households, in terms of housing equity, have a substantially higher propensity to use additional income from rate reductions to consume and a lower propensity to deleverage compared to more wealthy households.

Finally, we explore response heterogeneity across borrowers' ability to access credit markets using variation in credit scores. In particular, to proxy for a borrower's ability to access credit, we use their FICO credit score over the period 12 to 24 months before the interest rate adjustment. We stratify the sample into different groups using the dummy "High FICO" that captures borrowers with FICO scores above 660. Columns 7–9 of Table 3 investigate differences in mortgage payment reduction, car spending, and mortgage principal repayment across this dimension.

Column 7 of Table 3 shows a very similar reduction in mortgage payments across high and low FICO groups. In contrast, column 8 shows that new car spending by borrowers with higher FICO scores displays relatively larger increases after the reset compared to those with lower FICO scores. This finding is consistent with the interpretation that borrowers with lower credit scores may face higher borrowing costs and have limited access to auto loans during this period, which may reduce their car purchases in the absence of this rate-induced liquidity. Column 9 shows that borrowers with lower credit scores also allocate a smaller proportion of their additional

[18] Note that this is not the LTV ratio at origination, but our estimate of current LTV ratio computed using zip-code-level house price indices and the current mortgage balance.

liquidity to deleveraging compared to borrowers with higher credit scores. Notably, we obtain similar results when we stratify borrowers based on the amount of available lines of credit instead of their credit scores.[19]

Taken together, these results suggest that consumption (new car spending) by borrowers with lower income and wealth is significantly more responsive to similar mortgage payment reductions when compared to households with higher income and wealth. This finding is broadly consistent with life-cycle household finance models that show that the consumption function is concave in wealth and permanent income (e.g., Zeldes 1989; Carroll and Kimball 1996). We note, however, that borrowers with higher income or lower wealth may also have different preferences (discount factors, risk aversion, etc.) that could also explain the differential responses.

VI. External Validity and Robustness Exercises

We now present further evidence that examines the broader generalizability and robustness of our main findings.

A. *Broader Applicability: Evidence from Conforming ARMs*

We now assess the external validity of our findings using data for borrowers with conforming mortgages. We focus on a sample of 71,741 five-year ARMs originated during the 2005–2007 period that were used to finance single-family, one-unit properties. We note that on observable characteristics, including mortgage balances, this sample is broadly representative of a typical US mortgage borrower (see Section II).

Panel A of Table 4 presents the estimates from specifications similar to those in Table 2 estimated on the sample of conforming mortgages. Column 1 of panel A shows that, similar to borrowers with non-agency ARMs, those with conforming ARMs experienced a significant reduction in monthly mortgage payments. In particular, there was an average reduction of about $276 per month during the first year after the reset and about $279 per month during the second year after the reset. Column 2 shows that this reduction implies a roughly 23 percent decline in monthly mortgage payments during the first and second year after the reset relative to the initial mortgage payment.

Panels A and B of Figure 2 confirm this inference by showing the timing of these effects around the reset, with time zero being the time of the interest rate reset. Monthly mortgage payments are constant for the period prior to the reset and drop significantly at the moment of the reset. Similar to the case of borrowers with non-agency ARMs, the reduction in payments is not temporary but lasts for the entire period after the reset.

Notably, while substantial, the magnitudes of these payment reductions are not as large as in the case of non-agency ARMs. Several comments regarding this observation are in order. First, due to their relatively lower credit risk, the conforming loans carry significantly lower initial interest rates compared to non-agency ARMs (5.78 percent versus 6.44 percent on average). Due to this difference, during the

[19] Our measure of available credit to borrowers and FICO credit scores are highly correlated in our sample.

Table 4—External Validity: Impact of Rate Reductions among Borrowers with Conforming ARMs

Panel A. Main effects

	Mortgage payment (1)	Mortgage payment/initial payment (2)	New car spending (3)	New car spending/initial payment (4)	Probability of new car spending (5)	Voluntary debt repayment (6)	Debt repayment/initial payment (7)
One year before	−6.37	−0.000	−0.44	0.000	0.007	1.07	0.001
	(6.28)	(0.002)	(8.14)	(0.008)	(0.028)	(0.39)	(0.000)
One year after	−276.57	−0.227	27.67	0.028	0.127	17.91	0.017
	(5.00)	(0.002)	(6.98)	(0.007)	(0.024)	(0.73)	(0.001)
Two years after	−279.87	−0.230	35.61	0.042	0.192	15.15	0.014
	(5.59)	(0.002)	(12.16)	(0.008)	(0.034)	(0.85)	(0.001)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647
R^2	0.375	0.787	0.001	0.001	0.001	0.012	0.017

Panel B. Heterogeneous effects

	X = High income			X = High LTV			X = High FICO		
	Mortgage payment (1)	New car spending (2)	Debt repayment (3)	Mortgage payment (4)	New car spending (5)	Debt repayment (6)	Mortgage payment (7)	New car spending (8)	Debt repayment (9)
One year before	−0.000	−0.001	0.001	0.002	−0.005	0.001	−0.000	−0.001	0.000
	(0.002)	(0.009)	(0.001)	(0.000)	(0.010)	(0.000)	(0.002)	(0.011)	(0.000)
One year after	−0.223	0.034	0.017	−0.228	0.027	0.018	−0.222	0.032	0.006
	(0.002)	(0.009)	(0.001)	(0.002)	(0.010)	(0.001)	(0.001)	(0.010)	(0.001)
Two years after	−0.226	0.055	0.013	−0.233	0.026	0.015	−0.220	0.037	0.002
	(0.002)	(0.009)	(0.001)	(0.002)	(0.010)	(0.001)	(0.002)	(0.014)	(0.001)
(One year before) × X	0.000	0.003	0.001	−0.002	0.014	0.000	0.001	0.005	0.000
	(0.000)	(0.017)	(0.001)	(0.001)	(0.012)	(0.000)	(0.001)	(0.015)	(0.001)
(One year after) × X	−0.009	−0.010	0.000	0.000	0.002	−0.002	−0.008	0.002	0.015
	(0.001)	(0.014)	(0.001)	(0.000)	(0.012)	(0.001)	(0.001)	(0.013)	(0.002)
(Two years after) × X	−0.008	−0.025	0.002	0.004	0.037	−0.002	−0.015	0.021	0.016
	(0.001)	(0.015)	(0.001)	(0.001)	(0.012)	(0.002)	(0.001)	(0.025)	(0.002)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647	2,636,647
R^2	0.787	0.001	0.017	0.788	0.001	0.012	0.788	0.001	0.011

Notes: Panel A of this table presents estimates from OLS regressions similar to ones displayed in Table 2 but estimated in a sample of borrowers with conforming five-year ARMs. The main independent variables are dummies identifying different time periods before and after the reset date. One year before identifies the 12 months before up to 1 month before the interest rate adjustment. One year after includes the month of the adjustment up to 11 months after. Two years after includes 12 months after the adjustment up to 23 months after. Panel B reports coefficient estimates of OLS regressions similar to ones displayed in Table 3 but estimated in a sample of borrowers with conforming five-year ARMs. High income, High LTV, and High FICO are dummy variables that take values of 1 for observations corresponding to borrowers with relatively high income, high loan-to-value ratios on their loans, and high FICO credit scores, and are 0 otherwise. Other controls include a variety of borrower, mortgage, and regional characteristics, and fixed effects similar to those in Table 2 and 3, respectively. Standard errors (in parentheses) are clustered at the borrower and at the month level.

period of low interest rates, the conforming loans experienced a relatively smaller drop in interest rates and payments after the reset compared to non-agency ARMs. Second, on average the conforming loans carry substantially lower mortgage balances than non-agency loans ($201,422 versus $357,949), which also implies a relatively smaller dollar reduction in mortgage payments after the reset. Third, unlike interest-only non-agency ARMs, the majority of conforming loans in our sample experience partly scheduled amortization of principal balance after the reset, which contributes to the less pronounced effect of reset on mortgage payments. Regardless,

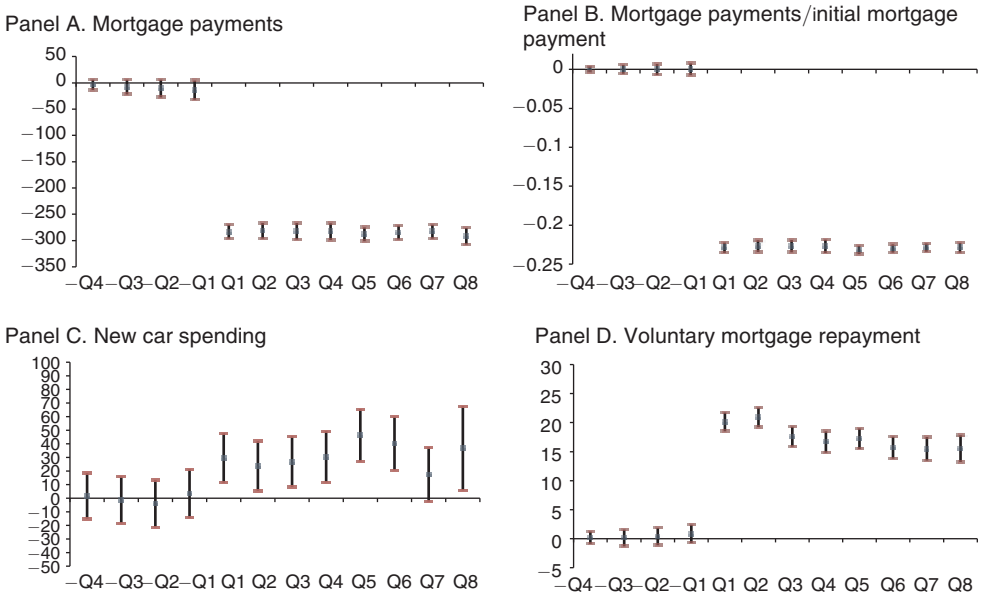

FIGURE 2. EXTERNAL VALIDITY: THE EFFECTS AMONG BORROWERS WITH CONFORMING (*Agency*) ARMS

Notes: This figure shows OLS estimates for a set of quarterly time fixed effects along with 95 percent confidence intervals that allows us to assess the change in the relevant outcomes of borrowers with conforming five-year ARMs during the four quarters preceding the first interest rate reset as well as during each of the eight quarters following the rate reset. The dependent variables are the monthly mortgage payment in dollars (panel A), the mortgage payment scaled by the initial mortgage payment (panel B), new car spending in dollars (panel C), and voluntary repayment of mortgage debt in dollars (panel D).

the reduction in payments for borrowers with conforming mortgages is significant enough to allow us to track consumption and deleveraging responses.

Columns 3–4 of panel A in Table 4 show that, consistent with our main results, borrowers with conforming loans also experience a significant increase in new auto financing after the reduction in mortgage payments. This effect, on average, amounts to a monthly increase in new car spending by $27 and $35 during the first and second years after the reset. This effect implies that, on average, borrowers' new car spending increased by between 16 percent to 21 percent relative to the pre-reset level. Panel C of Figure 2 shows the timing of these effects. There is a significant increase in new car spending among borrowers with conforming loans after the reduction in their mortgage payments, with the largest effects in the second year of reduced payments. In addition, there is a significant increase in the monthly probability of a new car purchase during the second year of payment reduction (column 5 of panel A in Table 4).

Next, we investigate the impact of rate reductions on voluntary repayments of mortgage debt for borrowers with conforming loans. The estimates in columns 6–7 of panel A show that, consistent with our prior results, borrowers with conforming mortgages experience a significant increase in voluntary mortgage debt repayment after the reduction of their mortgage rates. This effect amounts to an average monthly repayment of an additional $18 and $15 during the first and second year of lower rates, respectively.

While qualitatively similar, the results from the sample of borrowers with non-agency and agency loans are not directly comparable due to the differences in the magnitude of the reduction in mortgage payments (i.e., the treatment effect). To facilitate such a comparison, we scale the effects by the size of additional liquidity generated by mortgage rate reduction relative to initial payments. In particular, the results in Table 2 and panel A of Table 4 imply that among borrowers with non-agency (conforming) ARMs, the increase in monthly car spending during the first and second year of reduced payments amounts to about 8.1 percent (12.3 percent) and 13.6 percent (18.2 percent) of the additional monthly liquidity, respectively. Likewise, borrowers with non-agency (conforming) ARMs allocate about 7.7 percent (7.5 percent) and 8.3 percent (6.0 percent) of the additional monthly liquidity generated by rate reductions to repay their mortgage debt during the first and second year of reduced mortgage rates. Overall, this comparison points to broadly similar findings across different samples of mortgage borrowers with the same contract type.

Finally, we assess the robustness of our cross-sectional findings in the agency market based on the same empirical proxies capturing variation in income and wealth across borrowers. Panel B of Table 4 presents results from specifications similar to those in Table 3 estimated on the sample of borrowers with conforming loans.

Again, the evidence in the sample of borrowers with conforming mortgages is broadly consistent with our main findings. In particular, the estimates in columns 1–3 of panel B in Table 4 indicate that, despite experiencing a similar reduction in mortgage payments, borrowers with above-median income display a much smaller increase in new car spending after the interest rate reset relative to lower-income borrowers. In addition, the estimates in columns 4–6 suggest that less wealthy households, in terms of their housing equity, have a substantially greater (more than twice) propensity to use additional income from rate reductions on car consumption. Moreover, consistent with findings in Table 3, we also find a weaker, though insignificant, initial durable spending response among borrowers with below-median FICO credit scores (column 8 of panel B in Table 4).

B. *Difference-in-Differences Results*

In this section, we further test the validity of our main identification strategy. Since the timing of the reset is collinear with the time dummies, one potential concern with our main empirical strategy is that there could be mortgage-specific time trends that could affect our results. As discussed in Section III, in order to address this concern, we consider a difference-in-differences research design that exploits variation in the timing of rate resets of adjustable-rate mortgages originated at the *same* time but with different initial fixed-rate periods.

In particular, we exploit the fact that borrowers with five-year ARMs (the treatment group) have a five-year fixed rate period, while borrowers with ten-year ARMs (the control group) have a ten-year fixed period. After the fixed period concludes, these loans reset once a year thereafter based on the relevant index to which they are benchmarked. Thus, after the five-year period the five-year ARMs reset to a new interest rate every year, while the ten-year ARMs remain in their fixed-rate

Table 5—Difference-in-Differences Estimates Based on the Alternative Empirical Strategy

	Borrowers with non-agency ARMs			Borrowers with conforming ARMs		
	Mortgage payment (1)	New car spending (2)	Voluntary debt repayment (3)	Mortgage payment (4)	New car spending (5)	Voluntary debt repayment (6)
Four quarters before	−10.75	40.11	8.44	−4.45	−6.82	0.28
	(3.158)	(22.13)	(3.781)	(5.08)	(21.12)	(0.73)
Three quarters before	−19.75	26.13	6.935	−7.83	8.79	0.84
	(4.999)	(31.34)	(5.476)	(6.01)	(23.54)	(0.86)
Two quarters before	−23.72	48.57	12.12	−8.05	24.91	1.11
	(5.701)	(33.82)	(6.623)	(6.89)	(20.68)	(0.92)
One quarter before	−20.74	99.45	10.61	−9.32	18.87	0.37
	(7.830)	(33.01)	(8.285)	(7.22)	(23.91)	(0.97)
One quarter after	−922.3	146.7	66.26	−276.52	43.73	16.73
	(43.13)	(44.50)	(10.89)	(8.28)	(23.19)	(1.15)
Two quarters after	−848.7	162.3	75.30	−272.23	52.43	17.31
	(33.92)	(46.14)	(13.43)	(8.67)	(22.07)	(1.25)
Three quarters after	−793.6	187.3	71.74	−268.93	73.70	14.41
	(33.68)	(40.96)	(15.72)	(8.71)	(22.24)	(1.22)
Four quarters after	−750.6	186.1	67.25	−264.37	62.69	14.44
	(35.07)	(60.76)	(17.86)	(8.94)	(22.37)	(1.37)
Two years after	−713.5	137.7	62.21	−254.48	71.60	12.96
	(36.82)	(79.58)	(20.15)	(6.26)	(22.26)	(1.33)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,565,771	3,622,209	3,424,364	3,398,806	3,398,806	3,398,806
R^2	0.977	0.030	0.243	0.53	0.001	0.012

Notes: This table presents OLS estimates from the difference-in-differences specifications that examine the evolution of the monthly dollar amount of scheduled mortgage payments, new car spending, and voluntary mortgage principal repayment for borrowers with five-year ARMs (the treatment group) relative to borrowers with ARMs facing a longer initial fixed-rate period (the control group). The main independent variables are dummies identifying different quarter periods before and after the loan's age of 60 months corresponding to the first reset date of five-year ARM contracts (not reported for brevity) and the interactions of these time dummies with a dummy variable for the five-year ARM contract type (reported in the table). Columns 1–3 show the estimates for a sample of borrowers with non-agency five-year ARMs relative to a control group of borrowers with non-agency ten-year ARMs. Columns 4–6 show the corresponding estimates for a sample of borrowers with conforming five-year ARMs relative to a control group of borrowers with conforming seven-year ARMs. Other controls include a variety of borrower, mortgage, and regional characteristics, fixed effects similar to those in Table 2, as well as the stand-alone dummy capturing the five-year ARM contract type. Standard errors (in parentheses) are clustered at the borrower and at the month level.

period (until year ten). During the "pre-reset" period, the key outcome variables of borrowers in the treatment and control groups in both agency and non-agency samples exhibit similar trends in these variables including the borrower FICO score (see online Appendix A5). Observing similar trends prior to the first rate reset is comforting given that five-year ARM borrowers carry less mortgage debt and have lower FICO scores.

Columns 1–3 of Table 5 report the coefficient estimates of specification (2) relating the monthly mortgage payment, new car spending, and voluntary mortgage principal repayment to the reset five years after the origination. In contrast to the previous section's results, this sample includes both five-year and ten-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. The displayed coefficients show the estimated differential change in outcomes between borrowers with five-year ARMs (treatment group) and borrowers with ten-year ARMs (control

group) during the four quarters before, the four quarters after, and during the second year following the five-year ARM reset date.

Using this different source of identification, we find very similar results to those implied by our main empirical strategy. In particular, column 1 of Table 5 shows a differential reduction in the monthly payment of about $900 in the quarter after the interest rate reset, which is comparable to the results reported in Table 2. Column 2 analyzes the consumption decision: borrowers who experienced a reduction in their monthly payment tend to increase their consumption of cars by about $145–$185, or 15–20 percent of the monthly income shock. Similarly, column 3 shows that borrowers use about $60–$75 per month, or about 7–9 percent of their additional disposable income, to repay their mortgage faster.

Finally, columns 4–6 of Table 5 show the corresponding estimates for the sample of borrowers with conforming loans. Here we compare the outcomes of borrowers with five-year ARMs (the treatment group) to those with seven-year ARMs (the control group) who face an initial seven-year fixed rate period. Borrowers with five-year conforming ARMs experience on average a $270 differential decline in monthly mortgage payments. They use between $40 and $70 of the liquidity increase toward new car spending, and between $12 and $17 toward voluntary mortgage repayment after reset. These results are broadly consistent with the ones implied by our main empirical strategy applied to the sample of borrowers with conforming loans (panel A of Table 4 and Figure 2). Together, the estimates from the difference-in-differences empirical strategy alleviate concerns that the results from our main analysis are spuriously capturing mortgage-specific time trends.[20]

C. *Attrition*

One potential concern with our estimates is that they might be impacted due to the attrition of borrowers in our sample before or after reset. To address this issue, online Appendix A6 presents three types of loans that are present in our sample during the 2008–2012 period. In particular, we plot the number of loans that remain active throughout the period, the number of loans that are paid off over time (either because the household refinanced the mortgage or sold the house), and the number of loans liquidated (due to foreclosure, bankruptcy, or when they are real estate owned) before and after the interest rate adjustment. This figure shows that about 45 percent of the borrowers active in 2008 become delinquent or pay off their mortgage at some point in time, with the first effect dominating the second. The number of liquidated loans increases over time from almost zero at the beginning of 2008 to almost 30,000 in July 2012. The number of paid-off loans is significantly lower at about 15,000.

We can provide some insight into what drives this attrition. Online Appendix Figure A6 also shows the cumulative distribution for the current LTV for three categories of loans: active, liquidated, and paid-off. It is immediately clear that paid-off loans have significantly lower current LTV than active and defaulted mortgages.

[20] We also conducted one more robustness test related to empirical specification. In particular, we directly instrumented the change in the monthly payment with time dummies. This allows us to estimate a value interpretable as a marginal propensity to consume and deleverage upon reducing the mortgage monthly payment. Our results are qualitatively similar to those reported in the paper. Results are available upon request.

Specifically, one quarter before these loans drop out of our sample the median CLTV ratio is 78 percent, which corresponds to the vertical line in the graph. This corresponds almost exactly to the common threshold of 80 percent used by financial institutions in determining refinancing eligibility. Moreover, this LTV ratio is significantly lower than the 110 percent or 115 percent LTV ratio of active and defaulted loans, respectively. The graph for active loans shows that only about 5 percent of all active loans have current LTV ratio below 80 percent. This suggests that these households could not refinance their mortgages, and were effectively locked-in to their contract due to a lack of home equity. Similar arguments are applicable for our conforming loan sample. Notably, the one avenue where refinancing was somewhat possible during the latter part of our sample (under the HARP program) was not relevant for our main sample since non-agency loans were ineligible.[21]

Having said this, it is important to note that our analysis only considers active loans. Thus, a question remains on how our findings would change if we also considered households who defaulted or prepaid their loans. Our analysis in Section V of the heterogeneous response to the income shock provides some speculative insights into how our estimates of the marginal propensity to consume might be generalized to other households. Specifically, since low-income and more indebted households tend to consume a significantly higher fraction of the added income, borrowers who default—the large majority of those dropping out of our sample—are likely to have a higher response than the average borrower in our sample.

D. *Unexpected Rate Reduction*

The previous analysis has focused on the set of ARMs that experienced the greatest interest rate resets, because they were originated during a period of high rates and were reset when indices reached historic lows. Moreover, this reduction in rates translated into a significant reduction in monthly payments, because these were interest-only mortgages for the first ten years. Since the index rates were declining for several months before the mortgage interest rate adjustment date, households with such mortgages may have anticipated payment reductions once the economy entered in the more severe phases of the financial crisis.

While such effects, if present, would bias downward our results on impact of income shocks on consumption and deleveraging decision after the reset, we can investigate the effect of an unexpected interest rate reduction by analyzing ARMs that reset during the period January 2007 to March 2008. This covers the first time the LIBOR substantially declined. In order to increase the sample size, we gather information on all ARMs that reset during that period, not only interest-only five-year ARMs, and we focus on one single dummy for the post-adjustment period. The only difference with our main results is that, not surprisingly, there is a smaller decline in monthly mortgage payments since the interest rate indices had not yet reached their historical lows. This finding suggests that anticipatory effects related

[21] Borrowers with conforming loans could take advantage of the Home Affordable Refinancing Program (HARP) starting in 2009. However, initial restrictions on the extent of negative equity allowed (applicable until December 2011) prevented many conforming loans in our sample from refinancing.

to scheduled interest rate changes do not substantially affect our interpretation of the results from the primary sample.

E. *Alternative Consumption and Deleveraging Measures*

The richness of our data allows us to complement the previous findings by investigating the impact of lower mortgage rates on different measures of consumption and deleveraging. We focus on retail credit cards, e.g., credit cards issued by large store chains such as Best Buy and Macy's. Like our measure of car sales, this measure tracks expenditures by analyzing significant changes in the balance on these accounts. Online Appendix A7 shows the coefficient estimates of a specification relating the amount spent on retail credit cards with the interest rate reset. We find that households tend to increase their expenditures after the interest rate adjustment by almost $30. Normalizing by the size of the initial monthly payment, this increase corresponds to 3 percent of the positive income shock.

We also find evidence that households' tendency to deleverage is not restricted to their mortgage balance. In fact, the results presented so far are likely to underestimate the fraction of the positive income shock allocated by borrowers to repay their debts, because they might decide to repay other debts as well. For instance, borrowers might repay their more expensive loans, such as home equity loans. We investigate this possibility in online Appendix A7, where we restrict attention to the households who had an active home equity loan for at least one-half of the sample. We find a significant increase in repayments for this type of loan after the interest rate adjustment, with an average effect of $7 per month during the first year of rate reductions.[22]

In sum, analyzing additional measures of consumption and the repayment behavior for other forms of debt confirms the main results presented in Section IV on households' consumption and deleveraging responses to a positive disposable income shock.

F. *Impact on Delinquency*

We conclude this section by verifying that lower mortgage payments also have a significant impact on borrowers' delinquency rates. Indeed, consistent with prior studies (e.g., Tracy and Wright 2012; Fuster and Willen 2013), we find that a reduction in mortgage interest rates leads to a substantial decline in mortgage default and foreclosure rates. For example, in unreported tests, our estimates in the sample of conforming five-year ARMs indicate that the likelihood of 60+ day delinquency falls by 0.5 percentage points on average after one year and 1.8 percentage points after two years.

[22] In unreported tests, we also find that borrowers with substantial credit card balances and home equity lines of credit appear to differentially reduce their debt upon reset in both non-agency and conforming loan samples. We also observe a significant decline in revolving debt delinquency rates upon reset, which also suggests that borrowers use some of the extra liquidity to service their unsecured debt. A limitation of examining lines of revolving debt like HELOCs and credit cards is that we only observe the outstanding balance at a point in time in a month (when the "snapshot" of the credit record is taken), but not the actual spending and debt repayment rates per month. Consequently, we hesitate to draw strong conclusions from such findings and instead emphasize the mortgage debt repayment pattern, which is easier to interpret.

These estimates imply that a reduction of monthly mortgage payments by about 20 percent on average reduces the likelihood of mortgage delinquency after two years by about 40 percent relative to the mean delinquency rate among these borrowers. These effects are particularly pronounced among borrowers with relatively high LTV ratios and among less credit-worthy borrowers with limited access to credit. Finally, in results not shown, we also find that in response to mortgage rate reductions, borrowers experience a substantial reduction in delinquency rates on their other consumer debts (e.g., credit cards and auto loans) and a relative improvement in their credit scores. These findings suggest that a relative improvement in borrowers' credit-worthiness due to rate reductions can also positively contribute to car spending by facilitating access to new auto financing.

VII. Regional Evidence

In the previous sections, we used borrower-level data to limit unobserved heterogeneity and identify the effect of interest rate changes on borrowers' durable spending and mortgage debt repayment decisions. One implication of the previous findings is that the rigidity of mortgage contracts, fixed or flexible, is likely to shape the pass-through of changes in interest rates to real activity at the regional level. In particular, we would expect that a decline in interest rates may have a bigger impact on household consumption and potentially on other economic outcomes in those regions where adjustable-rate mortgages are used more intensively by households.

To shed light on this question, we now turn to regional zip-code-level data to explore the association between mortgage rate declines and regional outcome variables such as durable consumption (auto sales), house prices, and employment. It is worth noting that at the regional level we can measure car purchases directly and independently of their financing.

A. *Empirical Design*

Because the fraction of adjustable-rate mortgages in a region is relatively persistent over time and was determined prior to the period of declining rates, we can use variation in this ex ante measure of program exposure to trace out the effects of interest rate declines on different economic outcomes. We compare outcomes in regions that had a relatively higher concentration of ARMs—and therefore were also regions more likely to benefit from mortgage rate reduction—to otherwise similar regions with a relatively lower concentration of adjustable-rate loans.

To account for general trends over the recent period, we focus on the relative change in the evolution of economic outcomes during the period of rate declines in zip codes with a higher share of ARMs relative to the corresponding change in zip codes with a lower share of ARMs. Our identification assumption is that in the absence of declining interest rates, and controlling for a host of observable risk characteristics, economic outcomes in zip codes with a higher ARM share would have a similar evolution as those with a lower ARM share, up to a constant difference. This approach is similar to that used by Mian and Sufi (2012) in evaluating the effects of the Cash for Clunkers program, and by Agarwal et al. (2017), who evaluate the broader consequences of debt relief programs using regional variation

in exposure to the Home Affordable Modification Program (HAMP). As in other regional studies that use a difference-in-differences strategy, we will not be able to comment on any economy-wide effects due to declining interest rates.

The main limitation of our empirical approach at the regional level is that zip codes with a larger share of adjustable-rate mortgages could be different on observable and unobservable dimensions from those with a lower share. For example, part of the observed differences in outcomes across zip codes over time may not only reflect greater exposure to interest rate declines but also the unobservable differences in the profiles of borrowers in these regions.

We take a number of steps to address this concern. First, we focus only on zip codes that are relatively similar on key observables prior to the rate declines by matching the high- and low-exposure groups (e.g., zip codes with higher and lower share of ARMs, respectively) using a propensity score methodology and isolating the portion of the propensity score distribution with "common support." This approach employs a set of matching covariates, including zip-code-level averages of the FICO score of borrowers, interest rates, LTV ratios, and house price growth rates in the period preceding our sample period (2005–2006). We obtain a sample of 1,000 zip codes—from an initial sample of about 10,000 zip codes for which we have reliable data—each having at least 100 mortgage borrowers, equally split between high- and low-exposure groups after this matching exercise. Online Appendix A8 plots the geographical distribution of the overall sample of zip codes. Second, in our analysis we control for many other characteristics of these zip codes to account for any remaining observable differences. Finally, we note that our analysis allows for differences in the evolution in outcomes across zip codes with higher and lower shares of ARMs that are not due to interest rate declines, as long as these differences are, controlling for other observables, roughly constant over time during our sample period.

We focus on the sample period from mid-2006 through the end of 2012. As shown in panel A of online Appendix Figure A10, from mid-2006 through mid-2007 there were only minimal changes in the major interest rate indices. Afterward, however, these rate indices experienced a substantial decline, reaching record low levels around mid-2009 and remaining low thereafter. As noted earlier, we expect borrowers in regions with a larger ARM share to be more exposed to declining interest rates through rate resets.

We first verify that the matched zip codes in our sample are indeed similar on observables. Panel A of Table 6 compares the characteristics of zip codes in the period preceding the decline in interest rate indices (mid-2006 to mid-2007). To facilitate the comparison of zip code characteristics we split the matched sample into high-exposure zip codes (those with above median share of ARMs) and low-exposure zip codes (those with below median share of ARMs).

As we note from panel A of Table 6, after matching the observable characteristics are quite similar across the high- and low-exposure zip code groups. The high-exposure zip codes have very similar mean mortgage rates, credit scores, mortgage LTV ratios, unemployment rates, percentages of individuals with a college degree, and percentages of married households with children. These patterns are also visible when we examine the evolution of FICO credit scores, LTV ratios, and interest rates in the high- and low-exposure regions in the pre-treatment period (see online Appendix A9), as well as the kernel densities of these variables (unreported).

TABLE 6—REGIONAL EVIDENCE

Panel A. Summary statistics for high and low exposure zip codes

	High exposure zip codes		Low exposure zip codes	
	Mean	SD	Mean	SD
FICO	714.8	23.2	716.0	18.9
LTV ratio (percent)	64.5	7.29	68.1	7.00
Interest rate (percent)	6.64	0.57	6.68	0.48
Mortgage delinquency rate (percent)	2.81	3.09	2.23	1.83
Unemployment rate (percent)	6.04	1.55	5.91	1.47
Median income ($thousands)	58.4	14.13	52.7	14.38
Percentage of individuals with college degree	31.4	10.1	29.5	9.42
Percentage of households that are married couples with children	21.9	5.13	21.6	5.13
Consumer credit score	3.37	0.41	3.35	0.35
Quarterly house price growth (percent)	1.94	2.34	1.92	2.28
Percentage of GSE loans	69.0	9.61	70.2	10.7
ARM share	35.2	7.62	17.3	4.51

Panel B. Change in mortgage rates, mortgage delinquency, house price growth, auto sales growth, employment growth, and a zip code ARM share

	Mortgage interest rate		Mortgage delinquency growth rate	House price growth rate	Auto sales growth rate	Employment growth rate
	(1)	(2)	(3)	(4)	(5)	(6)
ARM share	−0.0198	−0.0176	−0.264	0.025	0.037	0.029
	(0.0005)	(0.0008)	(0.035)	(0.008)	(0.018)	(0.013)
Zip code controls	No	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	Yes	Yes	Yes	Yes	Yes
R^2	0.56	0.75	0.69	0.49	0.28	0.11

Notes: Panel A presents average summary statistics of key variables in high exposure (above median ARM share) and low exposure (below median ARM share) zip codes. The sample consists of 1,000 zip codes as explained in Section VIIA. The key independent variable, ARM share, is the percentage of outstanding mortgages that are of ARM type in a zip code (as of mid-2006). Consumer credit score is the average consumer rating of borrowers in a zip code assigned by Equifax (see Section I for the discussion of data sources). Panel B reports OLS estimates from regressions where the dependent variable is the mean change in mortgage interest rates (column 1 and 2), the average quarterly mortgage delinquency growth rate (column 3), the house price growth rate (column 4), the auto sales growth rate (column 5), and the employment growth rate (column 6) in a zip code between the period of interest rate declines and the period preceding the rate declines. Zip code controls include average zip code mortgage LTV ratios, interest rates, house price controls, socio-economic variables capturing a profile of the zip code population, and the average credit score of households. State fixed effects are fixed effects for the state corresponding to the location of the zip code. The estimates are expressed in percentage terms; standard errors are in parentheses.

However, relative to low-exposure zip codes, high-exposure zip codes have a larger mortgage delinquency rate (2.8 percent versus 2.2 percent).

Next, we verify that borrowers residing in zip codes with a higher ARM share are indeed more exposed to interest rate declines. As panel A of Table 6 indicates, despite the relative balance on observables, there remains significant variation in the fraction of loans that are ARMs in our matched sample. The low-exposure zip codes have a mean ARM-share percentage of 17.3 percent, compared with a mean ARM share of 35.2 percent in the high-exposure zip codes.

We next analyze the average mortgage rates in the high- and low-exposure zip codes. We observe little difference in average mortgage interest rates prior to the major decline of interest rate indices, but a significant relative decline of average mortgage rates in high-exposure zip codes after the decline of the overall level of

interest rate indices (see online Appendix Figure A10). This relative decline of mortgage rates occurs progressively (with visible differences emerging after 2008:I) as ARMs reset to lower rates at various calendar dates, depending on their origination date and type. As lower interest rates persisted, a larger fraction of ARMs reset to lower rates resulting in larger differences across high and low exposure zip codes over time.

To verify this pattern more formally, columns 1–2 of panel B in Table 6 confirm a strong association between the zip-code-level ARM share and the extent of interest rate declines in a zip code between 2007:IV and 2012:IV. The estimate in the first column of panel B implies that a 10-percentage-point absolute increase in the zip code ARM share is associated with a 17-to-20-basis-point reduction in average mortgage rates. The magnitude of this association is largely unaffected by the inclusion of a variety of control variables capturing the observable characteristics of zip codes. This estimate implies that a zip code with all ARM contracts would experience an average reduction in interest rates of about 170–200 basis points, which is reasonable given that the market interest rates to which ARMs are indexed declined by more than 400 basis points during the treatment period. As we discussed above, ARMs reset only periodically and caps and floors of different ARMs—all pooled together at the regional level—may limit the extent of rate fluctuations. Thus, we expect only a partial (and not instantaneous) pass-through of the interest rate reduction to borrowers with ARMs at the regional level.

B. *Mortgage Delinquencies*, *House Prices*, *Durable Consumption*, *Employment*, *and Zip Code ARM Share*

We now turn to the association of these interest rate declines with regional economic measures. First we verify that, consistent with our loan-level results, zip codes with a larger share of ARMs experience a relative decline in delinquencies and foreclosures. Using our matched sample, we estimate a regression with the change in the zip code quarterly mortgage delinquency growth rate between the period of interest rate declines (2007:III through 2012:IV) and the period preceding the rate declines (2006:II through 2007:II) as the dependent variable. Column 3 of panel B in Table 6 shows that, consistent with our loan-level analysis in Section VIF, zip codes with a larger ARM share experienced a relative decline in mortgage delinquency growth rates during the period of declining interest rates.[23]

Next, we examine the change in the house price growth in regions classified on the basis of their exposure to interest rate declines. The estimates in panel B of Table 6 show that zip codes with a larger share of ARMs experienced a significant relative increase in house price growth. A 10-percentage-point increase in the ARM share, which is associated with about a 20-basis-point average reduction in zip code mortgage rates (column 1), is associated with about a 0.25-percentage-point increase in quarterly house price growth (column 4).[24]

[23] In unreported results, we also find similar results when we consider the foreclosure rate: zip codes with a larger share of ARMs saw a relative decline in the foreclosure growth rate during the period of interest rate declines.

[24] It is, of course, possible that part of this house price effect reflects a change in the composition of transacted properties due to the relatively lower intensity of foreclosure sales in the high-exposure zip codes. To assess the

Next, we investigate the association between mortgage rate declines and durable consumption. Unlike in our borrower-level data, where we had to infer auto purchases from auto debt financing patterns, we can measure auto purchases directly at the zip code level.[25] We find that zip codes with more ARMs experienced a relative increase in auto sales growth rates compared to zip codes with fewer ARMs after the decline in interest rates. In particular, the estimates in column 5 of panel B in Table 6 imply that a 10-percentage-point increase in the ARM share is associated with a 0.37-percentage-point increase in quarterly auto sales growth. This result is consistent with our micro-level findings, and confirms on a regional level that lower mortgage rates had a significant impact on durable purchases.

Finally, we turn our attention to employment. For this purpose, we analyze the association between employment growth rates and zip code ARM share in our sample period. Column 6 of panel B shows that zip codes with a higher ARM share experienced relatively higher employment growth in the period of rate declines relative to the prior period: a 10 percent increase in the zip code ARM share is associated with 0.29 percentage point increase in employment growth rate. This association means that the zip codes with a higher share of ARMs experienced relatively faster employment recovery in the 2009–2012 period.[26]

Overall, our findings at the regional level corroborate those of Mian and Sufi (2014), who present evidence that adverse shocks to household balance sheets can account for a large fraction of the decline in US employment from 2007 to 2009. Consistent with this view, our results suggest that a relative improvement in household balance sheets due to mortgage rate declines had a positive impact on local employment growth, at least in the near term.

C. *Instrumental Variable Analysis and County-Level Analysis*

We verify the robustness of our regional analysis in the broader sample of zip codes—not just the matched subset of zip codes—by instrumenting for the region's ARM share with the percentage of house transactions in each zip code in the period 1998–2002 that had a price below 1.25 times the conforming loan limit.[27] The idea behind this strategy is that regions with a relatively higher share of home purchases that can be financed with conforming loans and 20 percent down payment—i.e., with price below 1.25 times the conforming loan limit—will have a smaller share of outstanding ARMs since these loans are relatively less popular among GSEs (see

robustness of our results to this concern, in results not shown we repeated this exercise using the CoreLogic house price index that excludes distressed transactions. Our inferences remain unchanged.

[25] The data on auto purchases are from R. L. Polk and Company (see Mian, Rao, and Sufi 2013).

[26] We also investigate which sectors can account for the relatively higher growth rate of employment in high-exposure zip codes. To classify industries as either nontradable or tradable industries, we closely follow Mian and Sufi (2014). Specifically, retail- and restaurant-related industries are taken as nontradable, while industries that appear in the global trade category are considered tradable. We find that high-exposure zip codes experienced a significant relative increase in employment growth in the nontradable sector during the period of rate declines. In contrast, we observe no relative significant change in the growth of the tradable sector between high- and low-exposure zip codes. This finding is reassuring, as we should not expect to find a significant association between relative employment growth in the tradable sector, which reflects broader economic conditions, and the local (zip-code-level) differential improvement in household balance sheets.

[27] We restrict our attention to zip codes that had at least 250 housing transactions in the 1998–2002 period in the DataQuick database and for which we have all other relevant data.

Fuster and Vickery 2015). The exclusion restriction behind this strategy is that the incidence of such transactions in a region in the past, which was predetermined by GSE policies, is unlikely to meaningfully impact the subsequent evolution of regional outcomes directly during our sample period.

Indeed, as online Appendix A11 shows, the first stage is strong, economically significant, and in the expected direction: a 1 percentage point increase in the zip code share of home purchases that could be financed with conforming loans is associated with about a 0.23 percentage point decline in the ARM share. In the second stage, we find patterns consistent with our earlier results. In particular, we find that a 10 percentage point absolute increase in the zip code ARM share is associated with about a 0.3 percentage point relative decrease in the mortgage delinquency growth rate, 0.08 percentage point increase in the house price growth rate, 0.029 percentage point increase in the auto sales growth, and 0.008 percentage point increase in the employment growth rate, though the latter effect is statistically insignificant.

Finally, we verify that our key findings also hold when we conduct our analysis at the county instead of zip code level. In particular, we find that a 1 standard deviation increase in the fraction of ARMs in a county is associated with a 2.5–3 percent increase in annual car sales in that county. Overall, our findings suggest that a decline in interest rates is associated with improvements in several economic outcomes in regions where adjustable-rate mortgages are used more intensively by households.

VIII. Discussion and Concluding Remarks

We exploit the automatic interest rate adjustment for borrowers with ARMs to study their consumption and debt repayment response to a decline in mortgage rates following the period of highly expansionary monetary policy in aftermath of the crisis. We find that a significant decline in mortgage rates had a substantial impact on household spending, debt repayment, and local regional activity. Borrowers who experienced a large reduction in their debt service increased their new car spending from 20 to 40 percent compared to the period immediately before the adjustment. Households used up to 15 percent of their increase in income to repay their existing debt faster, which attenuates their consumption response. Finally, regions more exposed to mortgage rate declines saw relative declines in delinquency and foreclosure rates, relatively faster recovery in house prices, increased durable (auto) consumption, and increased employment growth, with responses concentrated in the nontradable sector.

There exist important differences across households. Borrowers with lower income and wealth are significantly more responsive to mortgage payment reductions compared to more endowed and wealthy households. These findings are broadly consistent with life-cycle household finance models showing that the consumption function is concave in wealth and permanent income. They also suggest that, to the extent that borrowers' marginal propensity to consume is greater than that of lenders, the decline in mortgage rates could result in a positive overall effect on aggregate demand.

One limitation of our consumption results is that we only observe the durable spending response based on the new consumption of cars. To obtain a more comprehensive measure of consumption response, we impute the overall consumption response from car spending by doing a computation similar in the spirit to the one

in Blundell, Pistaferri, and Preston (2008). In particular, we assess how auto sales growth and total consumption growth respond to local shocks and use these elasticities to scale the response of total consumption to auto consumption for our shock.

Toward this end, we follow Di Maggio and Kermani (2016) and use state-level consumption data provided by the Bureau of Economic Analysis (BEA) to estimate the sensitivity of auto sales growth and total consumption growth to state-level Bartik shocks (see online Appendix A12). We find that the auto sales growth rate is more than three times as responsive to Bartik shocks as the total consumption growth rate (0.70 versus 2.3, respectively). Moreover, in the BEA data, auto sales account for about 4.5 percent of overall household consumption. Using these estimates, we can assess the total consumption response to our shock under the assumption that (i) the consumption response to our shock has a similar pattern to the consumption response to Bartik shocks and (ii) that households have roughly similar characteristics in both settings.

Under these assumptions we find that a $940 decrease in monthly mortgage payments per borrower that is associated with a roughly $110 increase in monthly car spending which would result in a $744 increase in total household consumption.[28] This implies that households consume about 0.8 of the monthly disposable income generated by the rate reduction.[29]

Taken together, our results support the view that policies aimed at reducing mortgage rates can have a meaningful impact on macroeconomic conditions by improving household balance sheets. This evidence is consistent with Agarwal et al. (2017), who find that mortgage modification programs, when used with sufficient intensity, may improve a range of economic outcomes. More broadly, these findings support Mian and Sufi (2009, 2014) and Mian, Rao, and Sufi (2013), who emphasize the importance of household debt in understanding the scope and depth of the recession.

Our results have a number of implications for the pass-through of monetary policy to the real economy and mortgage market design. First, our findings illustrate that the rigidity of mortgage contracts—fixed or adjustable rate—can importantly affect the pass-through of interest rates and monetary policy shocks to the real economy. In this regard, our findings point to the potential role of adjustable-rate contracts as automatic stabilizers, especially if the relevant interest rate indices to which these loans are indexed are expected to decline in recessions and rise during economic expansions.[30]

[28] As per our estimates

$$\Delta(\textit{Total Consumption}) = \frac{\textit{Total Consumption}}{\textit{Car Sales}} \times \frac{\beta_{\textit{TotCons}}}{\beta_{\textit{CarSale}}} \times \Delta(\textit{Car Sales})$$
$$= \frac{1}{0.045} \times \frac{0.7}{2.3} \times \$110 = \$744.$$

[29] We can compare our estimates to the literature on refinancing and the percentage "cashed-out." In particular, we note that Mian and Sufi (2011) estimate that average households extracted about $0.25 to $0.30 for every $1 increase in home equity. They also provide some evidence that most of this liquidity was spent on real outlays (i.e., consumption or home improvement). Our estimates imply a larger total consumption response from reduced mortgage payments (about $0.80 from every $1 of reduced payments). This difference could reflect the fact that in our setting all treated borrowers receive an automatic reduction in payments, while in the context of equity extraction borrowers need to engage in costly refinancing of their loans, which is also subject to credit-worthiness limits and other frictions.

[30] See Piskorski and Tchistyi (2010), who highlight the benefits of ARMs in reducing deadweight costs of foreclosures for less credit-worthy borrowers in an optimal dynamic contracting framework with stochastic interest rates.

Second, by automatically reducing mortgage rates when market rates are low, ARMs can circumvent a variety of frictions that limit pass-through of lower rates to borrowers. In particular, rate resets effectively allow borrowers to refinance their loans regardless of the extent of their housing equity or credit-worthiness. Moreover, they can help reduce frictions due to limited competition in the loan refinancing market (see Scharfstein and Sunderam 2013). In addition, such rate reductions can achieve similar results as mortgage modification programs while circumventing the barriers to loan renegotiation due to securitization (Piskorski, Seru, and Vig 2010), lender concerns regarding borrowers' strategic behavior (Mayer et al. 2014), and limited organizational ability of servicers to provide debt relief to a large number of borrowers in a crisis (Agarwal et al. 2017).[31] Furthermore, existing research provides evidence of significant inertia and inattention in mortgage refinancing decisions by borrowers (e.g., Andersen et al. 2015). As ARM contracts do not require the active participation of borrowers in the process of rate reduction, they can help alleviate the effects of such factors.

In conclusion, it is important to reiterate a few limitations of our findings. First, we focus on the effect of low interest rate policies on household balance sheets that operate through the reduction of mortgage rates on outstanding loans. This ignores other channels such as refinancing fixed rate mortgages or declines in credit card interest rates. Second, due to the nature of our empirical design, we are not able to comment on any economy-wide effects introduced by interest rate declines. Third, since we do not measure the propensity to spend of mortgage lenders (or MBS investors), we cannot directly assess the impact of ARM resets on aggregate demand, even if we kept all else constant. Finally, we cannot address the broader welfare implications of lowering mortgage rates through low interest rate policies. Doing so would require a proper assessment of the overall value of such polices, including their potential long-term costs and distributional consequences.

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