Monetary Policy Pass-Through: Household Consumption and Voluntary Deleveraging *

Marco Di Maggio Amir Kermani Rodney Ramcharan

November 25, 2014

Abstract

Do households benefit from expansionary monetary policy? We investigate how indebted households' consumption and saving decisions are affected by anticipated changes in monthly interest payments. We focus on borrowers with adjustable rate mortgages originated between 2005 and 2007 featuring an automatic reset of the interest rate after five years. The monthly payment due from the average borrower falls by 52 percent ($900) upon reset, resulting in an increase in disposable income totaling tens of thousands of dollars over the remaining life of the mortgage. We uncover three patterns. First, the average household increases monthly car purchases by 40 percent ($150) upon reset. Second, this expansionary effect is attenuated by the borrowers' voluntary deleveraging, as a significant fraction of the increased income is deployed to accelerate debt repayment. Third, the marginal propensity to consume is significantly higher for low income and underwater borrowers. To complement these household-level findings, we employ county-level data to provide evidence that consumption responded more to a reduction in short-term interest rates in counties with a larger fraction of adjustable rate mortgage debt. Our results shed light on the income channel of monetary policy as well as the role of debt rigidity in reducing the effectiveness of monetary policy.

*We thank Daron Acemoglu, Patrick Bolton, Luigi Guiso, Adam Guren, Tullio Jappelli, Chris Mayer, Claudio Michelacci, Emi Nakamura, Marco Pagano, Jonathan Parker, David Romer, Jón Steinsson, Suresh Sundaresan, Nancy Wallace, James Wilcox, Daniel Wolfenzon, Steve Zeldes, and seminar participants at the NBER Monetary Economics Fall meeting, Cornell University, University of Toronto, EIEF and Federico II University of Naples for helpful comments. We are grateful for financial support from the NBER Household Finance Grant. We also thank Katrina Evtimova, Jeremy Oldfather and Calvin Zhang for outstanding research assistance. All remaining errors are ours. The views in this paper do not necessarily reflect those of the Federal Reserve System. Di Maggio: Columbia Business School (email: mdimaggio@columbia.edu); Kermani: UC Berkeley (email: kermani@berkeley.edu); Ramcharan: Federal Reserve Board (email: rodney.ramcharan@frb.gov). Supplemental material can be found here.

1 Introduction

Six years after the financial crisis, many households remain debt-burdened and unemployment remains elevated despite highly expansionary monetary policy. Perhaps unsurprisingly, the effectiveness and appropriateness of monetary policy after the financial crisis is a subject of vigorous debate. This paper adds to our understanding of the impacts of monetary policy by providing household-level evidence on the effect of interest rates on household consumption and saving decisions. In doing so we assess the extent to which household deleveraging and mortgage contract rigidities dampened the effectiveness of monetary policy.

The conventional wisdom is that monetary policy affects firms' investment and households' consumption by reducing the cost of external finance. However, contractual frictions might limit the extent to which changes in monetary policy actually affect the cost of finance for households and firms. When the terms of debt contracts are rigid, as in the case of most fixed-rate mortgage contracts, changes in interest rates have little direct effect on consumption and investment decisions for already indebted households: only potential new borrowers or those able to refinance their mortgages will be affected. In fact, during recent years banks were unwilling to refinance mortgages on "underwater" homes that were worth less than the amount still owed on them.[1] This can limit the pass-through of lower interest rates to households, and therefore the ability of expansionary monetary policy to stimulate households' consumption.

If borrowers' marginal propensity to consume is greater than that of lenders, a decline in interest rates results in a positive income shock that should increase consumption, to an extent that depends on the magnitude and the persistence of its effect (i.e. the period of time in which monthly payments are likely to remain at lower levels), and boost economic activity.[2] However, this consumption response can be dampened by an increase in precautionary saving, which we call "*voluntary deleveraging*".[3] We provide micro evidence of both effects.

[1] Programs like the Home Affordable Refinance Program that removed loan-to-value requirements for the refinancing of loans insured by GSEs were implemented specifically to pass on the benefits of the new monetary policy regime to households.

[2] See, for instance, Carroll et al. (2014) for evidence supporting the hypothesis that the MPC is correlated with wealth; and Mian and Sufi (2012b) for evidence on the importance of this aggregate demand channel and its role in explaining the increase of unemployment in the U.S. during the Great Recession.

[3] A similar mechanism is proposed by Guerrieri and Lorenzoni (2011), who study the effects of a credit crunch on consumer spending and show that after an unexpected permanent tightening of consumers' borrowing capacity, the most indebted consumers tend to readjust towards lower levels of debt. Relatedly, Carroll (2009) show that if consumers are subject to transitory as well as permanent shocks, a positive shock to permanent income might

Isolating borrowers' consumption and saving responses to a change in interest rates is difficult because interest rates and refinancing decisions are endogenous and depend on a household's finances and creditworthiness. For instance, households with a bad credit history may be unable to refinance; the same may apply to liquidity-constrained households, which cannot pay the closing costs of their pre-existing mortgage.[4] Similarly, households living in counties where the housing market has 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.

To overcome this identification challenge, we exploit the anticipated changes in monthly payments of borrowers with adjustable rate mortgages (ARMs) originated between 2005 and 2007, with a fixed interest rate for the first 5 years, which is automatically adjusted at the end of this initial period. These cohorts experience a sudden and substantial drop in the interest rates they pay upon reset, regardless of their financial position or credit worthiness and without refinancing. These cohorts are of particular interest because the interest rate reduction they experienced is sizeable: the ARMs originated in 2005 benefited from an average reduction of 3 percentage points in the reference interest rate in 2010.

The key to our identification strategy is the ability to exploit the timing of the interest rate adjustment. Effectively, we compare borrowers who will experience the interest rate adjustment at different points in time. This allows us to control for the endogeneity of the refinancing decision and to focus on the effects of the cut in monthly payments on their consumption behavior. To implement this strategy, we merge households' mortgage data with their credit reports (provided by Equifax), which allows us to observe the balance of all their liabilities, such as credit cards and auto loans, and other revolving or installment debts. To investigate whether the interest rate adjustment resulted in faster deleveraging, we analyze data on the households' repayment behavior for both installment and revolving loans. Furthermore, by restricting attention to households with this type of mortgage, we limit potential concerns about the households' characteristics driving the choice between fixed-rate and adjustable-rate mortgages.[5]

temporarily boosting saving.

[4] For instance, Agarwal et al. (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. Campbell (2006) discusses these issues in greater detail.

[5] Campbell and Cocco (2003) show that unconstrained households prefer ARMs when inflation risk is large relative to real interest rate risk, while credit-constrained households might opt for them when they have low risk aversion;

We first document the effect of interest rate resets on monthly payments for households with 5-year ARMs and show that the monthly payment fell on average by $900 (52 percent) upon reset. The payments tend to stay constant before the reset month, as well as afterwards, suggesting that indeed the monthly payments featured a significant, permanent step decrease as a function of the interest rate. We control for 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 different origination cohort. Exploiting this sharp change in monthly payments, we document three main findings.

First, we find a positive consumption response to a drop in monthly payments. We measure consumption in two different ways. First, we identify the instances in which households purchase a car by applying for an auto loan, which constitutes our main measure of consumption of durable goods. Second, we employ information from bank credit cards as well as revolving store credit cards, as a measure of other forms of consumption, such as purchases at chains like Best Buy or Macy's. Using both measures, we show that the households that experience a drop in monthly mortgage payments increase their consumption of durables on average by $150 per month (or 40%), controlling for household and time fixed effects in the quarter after the change. Since the change in the mortgage payments is anticipated, we observe a slight but statistically significant increase in the quarter before the change, but the households' consumption expenses spike in the quarter after the reset and remain significantly higher for two years. This result is robust to controlling for county-time fixed effects, as well as allowing for heterogeneous trends for each origination cohort and for different mortgage sizes. In other words, a borrower with a mortgage resetting in January 2010 (after interest rates were reduced) will consume significantly more in the first quarter of 2010 than a borrower with a reset at a different point in time, say June 2010. The amount spent on durable goods, and also the probability of purchasing a new car, spikes after the reduction in the interest rate. Similar results are obtained with our second measure of consumption derived from retail credit cards.

We then turn to the analysis of voluntary deleveraging. We observe all the payments made towards mortgages and all the other debts, e.g. equity loans and home equity line of credit. We show that households use more than 10% of the increase in disposable income to repay their debts

however, they are unattractive to risk-averse credit-constrained households with a high debt-to-income ratio.

faster. This amounts to a doubling of their expenditures on debt service. Although our first result suggests that low interest rates boosted consumption, this second result suggests that this effect was attenuated by the high level of debt accumulated during the boom years and a desire to deleverage.

We complement these results by analyzing the behavior of borrowers with different loan-to-value ratios (LTV) and show that there is a significant difference in their consumption behavior. Specifically, borrowers with an LTV above 120% a year before the reset invest less in deleveraging. But interestingly high-LTV households' consumption is almost twice as much as other borrowers. This can be for two different reasons. First, because they are deleveraging less, they might have more resources available to consume. Second, as shown by Zeldes (1989a) and Aiyagari (1994), the borrowers hit by worst wealth shocks are those that react most strongly to the cash flow shock.[6] But interestingly borrowers with lower LTV are those that invest more in deleveraging. This confirms the intuition that the borrowers with low or intermediate LTV ratios, who are closest to building equity in their homes, are more likely to delever than deeply underwater homeowners. We also find that low-income households tend to consume significantly more and deleverage less than high-income ones.[7]

Finally, we investigate the implications of our findings for the aggregate economy. To estimate the effect of monetary policy on county-level aggregate consumption, we exploit the geographical variation in the presence of adjustable-rate mortgages: ARMs have been more popular in some parts of the U.S. than others due to different housing costs. Specifically, counties in California and Florida – and in general in coastal areas – have had higher levels of ARM origination. Using data from Lender Processing Services (LPS), on the fraction of ARMs originated at the county level, we construct a measure of how much each region is directly exposed to changes in monetary policy.

First, we show that the fraction of outstanding ARMs as of 2006 is highly predictive of the monetary policy interest rate pass-through in 2007-13. In other words, the average mortgage rate in regions with a higher fraction of ARMs reacts more to the decline in interest rates. Then, by looking at quarterly car sales between 2007 and 2013, we show that changes in the interest rates tend to have a disproportionately larger effect on car sales in counties with a greater fraction of

[6] It is also possible that households that experienced a more severe wealth shock during the recession have reduced more their consumption and therefore it is a longer time since they purchased a car (see Eberly (1994) and Berger and Vavra (2014)).

[7] Evidence of financing constraints at the household level has been widely documented by, among others, Zeldes (1989a), Jappelli and Pagano (1989), Campbell and Mankiw (1989), and Carroll and Dunn (1997).

ARMs. These results remain significant even when we control for debt-to-income ratio, median income, securitization activity and poverty rate all computed in 2006, as well as county and time fixed effects. These results suggest that relative debt rigidity – the responsiveness of loan contracts to interest rate changes – plays an important role in the transmission of monetary policy to the real economy. Moreover, this empirical strategy allows us to capture the local general-equilibrium response to interest rate changes, as the effects that we estimate can be seen as the sum of the direct increase in car purchases by borrowers holding ARMs, and the indirect increase by all the other households in the same area who benefit from the resulting increase in local demand. However, admittedly our methodology falls short of estimating the aggregate general-equilibrium effect, such as an economy-wide multiplier of interest rate policy; because, for instance, we do not observe the lenders' response to such changes in interest rates.

1.1 Related Literature

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 analyze the impact of monetary policy on households' consumption behavior at the disaggregated level. Moreover, households' consumption reaction to monetary policy can be driven by a valuation channel and an income channel.[8] Our first contribution is to use household-level data to fill this gap by investigating the role of the *income channel* of monetary policy in shaping households' consumption and saving behavior.

Recent papers have investigated the channels through which monetary policy impacts on banks' lending decisions and risk-taking behavior. In a seminal paper, Kashyap and Stein (2000) provide evidence on the bank lending channel of monetary transmission. More recently, Jimenez et al. (2014) show that a lower overnight interest rate induces less capitalized banks to lend to riskier firms. Jimenez et al. (2012) show that tighter monetary policy and worse economic conditions substantially reduce lending, especially by banks with lower capital or liquidity ratios. Finally, Maddaloni and Peydró (2011) find that low short-term interest rates soften standards for household and corporate loans, an effect that is amplified by monetary policy rates that remain too low for too

[8] See Doepke and Schneider (2006) for evidence regarding the valuation channel.

long.[9] We complement these studies by uncovering the impact of low interest rates on households' consumption and saving decisions.[10] The novelty of our approach is to document that the prolonged period of low interest rates boosts households' consumption both at the individual and the aggregate level, but that the effect is attenuated by the households' incentives to deleverage.[11]

Hence, our paper is related to the work that measures the size of the household consumption responses to fiscal stimulus policy or tax rebates. For instance, Souleles et al. (2006), Shapiro and Slemrod (2009) and Parker et al. (2013) focus on the episodes of 2001 and 2008, in which the U.S. Treasury scheduled payments based on the last two digits of individual Social Security numbers. They exploit this randomized timing of the receipt of payments to conclude that households spend approximately 25 percent of rebates on nondurables in the quarter that they are received, relative to the control group of households that do not receive the rebate in that same quarter. There is also evidence of interesting dynamic effects. For instance, Agarwal et al. (2007) analyze the tax rebate of 2001 and show that consumers initially saved some of it, by increasing their credit card payments and thereby paying down debt, but that their spending increased again soon afterwards. This uncovers an important liquidity mechanism, which is confirmed by our results on the heterogeneous marginal propensity to consume of borrowers facing different wealth shocks.[12] Recently, Mian and Sufi (2014) examine households' borrowing and spending behavior resulting from rising house prices from 2002 to 2006. Baker (2013) use a novel dataset to test whether consumption among households with higher levels of debt is more sensitive to a given change in income. Mian et al. (2013) estimate, instead, the elasticity of consumption with respect to net housing worth, during the 2006-2009 period, employing the unequal geographic distribution of wealth losses across the United States.

In contrast to most of the literature which focuses on anticipated shocks, Agarwal and Qian (2013) study how households respond to an unanticipated income shock. Specifically, they study

[9] Other relevant papers in this literature include Bernanke and Blinder (1988), Christiano and Eichenbaum (1992), Landier et al. (2011), Landier et al. (2013), Stein (2012) and Williamson (2012).

[10] We also contribute to the literature that investigates the redistributional implications of monetary policy for the aggregate economy, like Doepke and Schneider (2006) and Sterk and Tenreyro (2014).

[11] For other papers on the effects of unconventional monetary policy see Swanson and Williams (2012), Romer and Romer (2013), Krishnamurthy and Vissing-Jorgensen (2011), Krishnamurthy and Vissing-Jorgensen (2012), and Gagnon et al. (2011).

[12] Bertrand and Morse (2009) also use the 2008 tax rebate, studying the behavior of borrowers who use payday loans. They find that only the low-to-middle users of payday lending services (measured in terms of frequency of use in the prior year) retire debt in the pay cycles that follow the receipt of the tax rebate.

the effect of the Growth Dividend Program, which consisted in a one-time cash payout of $1.17 billion ranging from $78 to $702 to 2.5 million Singaporeans. They find that for each dollar received, consumers on average spent 80 cents during the ten months after the announcement. Jappelli and Padula (2014), instead, study the effect of a change in the severance pay for public employees in Italy, which entailed substantial losses for future generations of public employees and can be considered as an unanticipated income shock. They find that each euro reduction in severance pay reduces the average propensity to consume by 3 cents and increases the wealth-income ratio by 0.32.

Other related papers in this literature include Jappelli and Pistaferri (2010), Kaplan and Violante (2011), Hsieh (2003), Shapiro and Slemrod (2003b), Shapiro and Slemrod (2003a), Souleles (2002) and Stephens Jr (2008). Jappelli and Pistaferri (2010) develop a theoretical framework that has several predictions for consumption response to unanticipated and anticipated income shocks depending on their persistence and the degree of completeness of credit and insurance markets.[13] Specifically, they argue that consumption should not respond to anticipated but should respond to unanticipated income changes. Recently, Kaplan and Violante (2011) propose a quantitative framework, where households can hold two assets: a low-return liquid asset (e.g., cash, checking account, etc.) and a high-return illiquid asset that carries a transaction cost (e.g., housing or a retirement account). They show that this model yields consumption responses to fiscal stimulus payments that are in line with the empirical evidence. Hsieh (2003) provides evidence of consumption smoothing employing the Alaska permanent fund, while Shapiro and Slemrod (2003b) and Shapiro and Slemrod (2003a) provide further evidence on the tax rebate in 2001 and Souleles (2002) analyzes the effects of the Reagan tax cuts. Finally, Stephens Jr (2008) examines the consumption reaction to predictable increases in discretionary income, following the final payment of a vehicle loan, to investigate whether households "smooth" consumption in response to predictable changes in income.

In sum, we contribute to this literature in two ways. First, whereas the existing literature focuses on the effects of fiscal policy through tax rebates, our study highlights the role that monetary policy can play in shaping consumers' behavior through mortgage payments and housing wealth. Second, the magnitude of the average income shock per households is significantly larger than that of any

[13] For an earlier account of the major developments in this literature see Deaton (1992).

other previous study considered. In fact, our estimates do not rely on a one-time shock, but on an average effect of about $900 per month.

Finally, our paper also highlights the important role played by frictions in the mortgage market. While the previous literature on monetary policy has focused on price rigidities, we highlight the importance of debt rigidities in the transmission of monetary policy. This also connects our study to recent papers by Calza et al. (2013), Scharfstein and Sunderam (2013) and Keys et al. (2014). Calza et al. (2013) analyze the relationship between the structure of housing finance and the monetary transmission mechanism in several industrialized countries. They show that the size of the effect of a monetary policy shock is significantly related to indicators of flexibility in the mortgage markets and that residential investment is significantly more responsive to policy innovations in countries with a variable-rate mortgage structure. 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. Keys et al. (2014), instead, examine the reasons behind the failure to refinance for households in the U.S., even when this would lead to significant savings. Specifically, they compute that the median household that is holding on to a mortgage with too high an interest rate would have saved approximately $45,000 (unadjusted) over the remaining life of the loan by refinancing. Moreover, they argue that the failure to refinance was too widespread to be explained simply by more conservative underwriting standards in the wake of the crisis.[14]

We complement these papers by showing how households less subject to such frictions, due to an automatic adjustment of the interest rate, responded to the positive monetary policy shock. We are able to do so by employing an identification strategy similar to that proposed by Fuster and Willen (2013), who employ these changes to monthly mortgage payments to shed light on borrowers' default behavior and show that the reduction of the payment significantly reduces the hazard of becoming delinquent. Our findings are also related to concurrent work by Keys et al. (2014), who employ a similar identification strategy to analyze the effects of rate reductions in a sample of agency borrowers.

The remainder of the paper is organized as follows. Section 2 provides details on the data sources and summary statistics. Section 3 explains the research design and how it is made operational.

[14] Lucas et al. (2011) analyze a large-scale mortgage refinancing program that would relax current income and loan-to-value restrictions for borrowers whose mortgages are currently insured by Fannie Mae, Freddie Mac, or the Federal Housing Administration.

Section 4 describes and interprets the main results on the households' marginal propensity to consume. Section 5 presents evidence suggesting heterogeneous MPC among households facing different liquidity constraints and Section 6 discusses a number of robustness checks. Section 7 presents aggregate level evidence and Section 8 concludes.

2 Data and Summary Statistics

We take advantage of two main sources of information, one on the characteristics of the mortgages and one on households' balance sheets. Specifically, we collect data on mortgage loans originated every month from 2005 to 2013 through Blackbox Logic, a private company that provides a comprehensive, dynamic dataset with information on 21 million privately securitized Subprime, Alt-A, and Prime loans. These loans account for about 90% of all privately securitized mortgages from that period. This dataset allows us to keep track of the information on the mortgages and the borrowers at origination, such as the loan type, the initial interest rate, the initial FICO score and the amount of the loan, but more importantly it provides us with monthly updates about, for instance, the status of each mortgage, the monthly payments, the current balance and other important information. Furthermore, since we know the borrowers' location we can employ the zip-code house prices and the information about the current balance on the mortgage to construct a current loan-to-value ratio for each borrower.

These loans are then matched with credit bureau reports from Equifax. Equifax provides us with detailed households' balance sheets, specifically, the monthly information on all the loans that a borrower has, such as credit cards, auto loans, mortgages, and home equity line of credit, but also on current FICO score. The two datasets allow us to construct our main variables of interest. First, we can precisely identify the mortgages that should be affected by changes in monetary policy. Specifically, we focus on prime five-year ARMs originated between 2005 and 2007, which are among the most common categories within the ARMs. We restrict attention to ARMs that are interest-only for the first 10 years, because these loans experience the largest decline in their monthly payments when the interest rate is reset (see Fuster and Willen (2013) for further discussion on this). Finally we only consider households for whom their original mortgage is not in foreclosure nor

is repaid or refinanced.[15] We also employ the second-largest category, ten-year ARMs, as control group to provide an additional robustness check in Section 6. Second, we can accurately measure the change in the mortgage monthly payment as we observe the borrowers' mortgage payments each month. By observing this balance sheet information over time, we can also estimate how much of the income shock will be utilized by the borrower to pay down debt. Finally, we can construct a measure of consumption for each borrower. In particular, we can measure the consumption of durable goods, proxied by the change in auto loans. We also supplement this with another measure of consumption coming from the balance of the borrowers' store credit cards (e.g. Best Buy card, Macy's card, etc.) to provide further evidence on the households' consumption response.

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

Let us start by describing the main variable of interest. Figure 1 shows the distribution of the changes in the monthly payment at the time of the interest rate adjustment for our sample of ARMs. In other words, the average monthly positive disposable income shock is about one thousand dollars which, as we will show in the next section, corresponds to half the monthly payment. However, depending on the size of the loan, some borrowers' monthly payment goes down by more than three thousand dollars. We take account of this heterogeneity by analyzing the behavior of households subject to a different intensity of the treatment.

This change in the monthly payment is triggered by the automatic reduction of the mortgage rates. In our sample, mortgage rates are computed as a fixed spread over an index. Since these are all prime borrowers, the spread is relatively low with a range of 2 to 4 percentage points. Most of the loans are indexed to 6-month LIBOR, the second largest group comprised is indexed to 1-year LIBOR, and finally a fraction are indexed to the 1-year Treasury bill rate. Figure 2 shows the cumulative distribution function for the change in the mortgage rate between the origination and the date of the adjustment. The average decline in the interest rates is about 3.3 percentage points.

[15] See Section 6 for the discussion of this point.

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.

We can now turn to our main measure of consumption: car purchases.[16] Since our measure is computed using changes in auto loans, we first show that financed car purchases are indeed an important fraction of the total car sales. Figure 3 employs data from Polk to plot the financed car sales as a fraction of the total new car sales, and shows that leveraged purchases account for about 80 percent of the total car sales. Moreover, they follow very similar trends over time. This reassures us that, even if not fully comprehensive, our measure is covering a very significant portion of the market. Figure 4 is a representation of the way in which we are able to identify car purchases through two examples drawn from our sample. We plot the balance of the auto loans and the measure of new car purchase that we use in our analysis. The right panel represents the case in which a consumer has bought two cars. These events correspond to a clear spike in his auto loan balance. Our measure of car purchase is equal to the change in the auto loan balance at the time of purchase. 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 his auto loan over time. This explains why our measure stays constant at zero for the whole period, while the blue dots trace the decreasing auto loan balance.

Figure 5 complements the previous description by showing the average monthly expenditure on car purchase for the period 2006-2012 for the sample of households in our data with 5-year ARM mortgage contracts. It starts from its highest level at the beginning of our sample, at about $400 per month spent, and declines to $250-$300 during the Great Recession. The bottom graph shows, instead, the average probability of a car purchase in a month, which is between 1.3% and 2.1%. These data are useful in interpreting the magnitude of our consumption response.

Before discussing the summary statistics, we also plot the average monthly partial mortgage prepayment in Figure 6. This captures the amount allocated by the borrower to repay the mortgage. It is $40 during the pre-adjustment period 2007-2010, but then rises to $120 in the later years in our sample. This increase reflects the fact that starting in 2005, a significant fraction of these households benefited from the adjustment to the interest rate and, as we will show in the next

[16] Due to data limitations, we cannot exploit the distinction between consumption and expenditures proposed by Aguiar and Hurst (2005) and Aguiar and Hurst (2007).

section, they allocate on average an additional $70 to repay their mortgages.

Table 1.A reports the summary statistics for the main variables. We consider both 5-year and 10-year ARMs, since the latter will be used as an additional control group in Section 6. Our sample comprised prime borrowers with an average FICO score of 736, an average original mortgage balance of $357k, and an initial loan-to-value ratio of 77%. The interest rate averages 6.4% at origin and declines to 3% after the adjustment, with a corresponding decrease in the average monthly payment from $1.900 to $915. We also compute that the average monthly expenditure on new cars by these borrowers is $319 and their monthly probability of purchasing a new car 1.5%. Next we can compare these characteristics with the 10-year ARMs. The main difference is that 10-year ARMs tend to be larger, with an average mortgage size of $536k and a monthly payment of $2.700, but the borrowers' consumption and saving behavior is very similar.

To analyze the aggregate effects of changes of interest rates on the county-level consumption we use a dataset from R. L. Polk & Company (Polk) that records all new car sales in the United States.[17] Beginning in 2002, for each new car purchased in the United States, the dataset identifies whether the car was purchased by a private consumer (a retail purchase), a firm (commercial purchase), or by the government. It also gives the county, year and quarter in which the car was registered.

The lower panel of Table 1.A shows information about the key control variables that we use in Section 7 to capture county-level heterogeneity. We collected information on median income, population, household leverage, poverty rate, fraction of securitized loans and, more importantly for our analysis, the fraction of ARMs in 2006. The latter exhibits a significant variation: 17% on average, but ranging from 3% to 63%.

For the households' balance sheet information we employ data from LPS. It provides loan-level information collected from the major mortgage servicers in the US, covering about 60 percent of the mortgage market. We use these data to construct the total stock of outstanding mortgage debt in each county, disaggregating the principal balance by whether the mortgage is fixed rate or adjustable rate and combining the principal balances for adjustable and hybrid mortgages. We include both refinances as well as new mortgage originations in order to measure broadly the potential channels through which interest rate movements might affect consumption. The main advantage of this

[17] This same data has been previously used by Mian et al. (2013).

dataset over the one we use for the loan-level analysis is broader coverage, because it includes non-securitized loans and loans insured by GSEs. However, we cannot employ this dataset throughout the paper, because it does not contain credit bureau information from which we derive our measures of consumption and prepayment.

We can take advantage of the larger sample in LPS to see whether the borrowers in BlackBox differ from households holding other types of mortgages in any significant way. Table 1.B provides information first on the characteristics of almost 20 million mortgages originated between 2005 and 2008, and then on three main subgroups: borrowers with fixed-rate mortgages, those with adjustable rate mortgages and those with 5-year ARMs (which are not restricted to hybrid mortgages only). Comparing data from Table 1.A and Table 1.B we can see that in our sample the borrowers have a somewhat better FICO (736) than the average borrower (703), or borrowers with fixed-rate mortgages (705), but very similar to the sample of 5-year ARMs in LPS (721). The same is true for the interest rate at origination, the initial monthly payment and the loan-to-value ratio. The only important difference between these different types of mortgage holders is the average size of the loan. In LPS we have an average size of \$349k, compared to \$196k and \$239k for fixed-rate mortgages and the average borrower respectively. However, this is very close to what we report in Table 1.A for our sample of 5-year ARMs in BlackBox (\$357k). This evidence reassures us that the main mortgage characteristics of the borrowers who experienced the automatic adjustment of the interest rate mirror those of the more general population of households holding a mortgage in US.

3 Research Design

The monetary policy implemented in the aftermath of the crisis could have benefited existing homeowners through the possibility of refinancing at lower interest rates and so boosted aggregate consumption. But a substantial fraction of homeowners were not able to take advantage of the low interest rates due to the collapse in house prices, which resulted in spiking loan-to-value ratios and the consequent inability to refinance. This paper starts with the observation that during the period 2004-2007 an important part of the mortgages originated were adjustable rate mortgages.[18] The key feature exploited in our study is that these mortgages entail a fixed interest rate for the first

[18] By 2006 they reached over 12% of all new mortgages and close to 40% in some well-performing markets (see Krainer and Laderman (2014), Piskorski and Tchistyi (2010) and Garmaise (2013)).

5 or 10 years with an interest-only payment for the first 10 years, and an automatic adjustment of the interest rate 5 or 10 years after the origination. In other words, no matter the local house prices, these households would see their mortgage rate reset at much lower level, which would result in a significant reduction in their monthly payment. A crucial factor in our identification strategy is that the monthly payment reduction is a feature of the contract and not an endogenous choice of the borrower.[19]

At the individual level, our identification strategy is designed to exploit the timing of the change in the interest rate and the automatic reset for these ARMs as a positive income shock for households holding these mortgages.[20] The estimation methodology employed for the individual level is a version of the difference-in-differences estimator (DD). Specifically, in each month t the treatment group includes all the households holding 5-year ARMs who have their mortgages reset in month t, while the control group comprises those with the same type of mortgage, but that did not experience the change in their interest rate. In other words, we estimate the consumption response of the households who experienced a reduction in the interest payment, relative to that of households holding the *same* mortgage, but with a different reset date.

This identification strategy has several advantages. First, by restricting attention to households holding the same contract, we avoid picking up some difference in preferences that could drive the choice of an ARM rather than a fixed-rate mortgage. Second, this strategy allows us to exploit the timing of the change, which is unlikely to be correlated with the households' consumption behavior. That is, the assumption is that households whose mortgage is reset in May 2010 are basically comparable to households that experience their reset, say in December. Third, thanks to the panel nature of our data, we can control for household and time fixed effects in all our specifications, as well as a vector of characteristics that absorb potential heterogeneity correlated with their consumption and saving behavior. Moreover our research design has the advantage that it 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.

Moreover, as a further robustness check, we can include county-time fixed effects and cohort-

[19] Although households could refinance their mortgages even before the interest rate adjustment, we show in Section 6 that only loans that had current LTV below 90 percent could refinance.

[20] At the aggregate level, we can exploit the inter-county heterogeneity in exposure to these type of contracts to analyze how the prolonged period of low interest rate has affected households' consumption behavior. This will be explained in greater detail in Section 7.

time fixed effects, where cohort is defined as the year of origination.[21] These more conservative specifications correct for two potential confounding effects. First, we allow for heterogeneous trends in different regions, which assures that our results are not driven by other factors, like local economic activities, that are correlated with the changes in the monthly interest rates. For instance, households in counties with a more severe bust and economic recession might show different consumption behavior from those in counties less severely affected.

Second, we allow for heterogeneous trends by cohort of origination, which captures unobserved variation across cohorts that might affect the households' response to the interest rate reset. For instance, mortgages originated in 2007 had higher LTVs than those originated in 2005, as the house prices rose in the meantime and lending standards became laxer. This means that households which purchased a house in 2007, with a higher monthly payment, are affected differentially by the interest rate adjustment, both because of the stronger income shock and because of potential characteristics correlated with their consumption behavior, such as creditworthiness and expectations about future income growth.

Formally, our main specification is the following

$$Y_{i,t,g,\tau} = \sum_{\theta=-4}^{4} \beta_\theta 1\{\tau = \theta\} + \beta_5 1\{\tau \geq 5\} + \lambda_i + \eta_{g,t} + \Gamma X_{i,t} + \varepsilon_{i,t,\tau}, \tag{1}$$

where i denotes the households, g the county, t the month or the quarter and τ the quarter since the interest rate adjustment.[22] The main outcome variables $Y_{i,t,g,\tau}$ analyzed in the next section are the increase in consumption of durables, as proxied by the purchase of a car and partial mortgage prepayments as measure of deleveraging. As further evidence, we also consider purchases made with credit cards and the repayment of home equity and home equity line of credit. The main coefficients of interest are β_θ which capture, for instance, the consumption response to the change in the interest payment one quarter, or even four quarters before and after the adjustment of the interest rate, with β_5 capturing everything after one year from the reset. λ_i captures the households fixed effects, whereas $\eta_{g,t}$ is the county-month fixed effects. Finally, $X_{i,t}$ is a vector of borrower's characteristics designed to capture any residual individual heterogeneity not captured

[21] Since in our specifications we estimate the consumption and saving response with quarterly dummies before and after the interest rate adjustment, we cannot estimate different trends for quarters of origination. In the supplementary appendix, we also estimate a similar specification but by focusing on each cohort separately.

[22] In our specifications we also include a dummy equal to one after two years of the interest rate adjustment.

by the household fixed effect. This includes the borrower's FICO score, as proxy for his financial constraints or creditworthiness, and the zipcode-level house prices to capture the local economic conditions. Alternatively, instead of controlling for the county-month fixed effect we can control for the cohort-time fixed effect or allow for different trends depending on the size of the initial monthly payment. To analyze heterogeneity in the response to the decline in the monthly mortgage payment, we interact $1\{\tau = \theta\}$ with indicators for different types of households. We correct the standard errors to allow for arbitrary heteroskedasticity and we cluster them at the household level.

We start by quantifying the average change in the monthly payment, which will constitute our income shock. Figure 7 shows an event study analysis with time zero being the time of the interest rate reset and the x-axis being quarters before and after the adjustment. In the top graph, we plot the average monthly payment, normalized to zero in the pre-period, which stays constant for the period before the event and drops significantly at the moment of the event. The bottom graph shows the change in the monthly payment once we normalize it by the monthly payment at origination. The magnitude of the drop is very substantial, nearly a thousand dollars on average, or about half the monthly payment. This figure also highlights one important feature of our setting, namely that the reduction in the payment is not temporary, but lasts for the whole post period. This is because even though these ARMs usually reset the interest rate every year after the initial fixed-rate period, the low interest rate regime that was set in December 2008 is still in place.

Now we can present our estimation results. Table 2 shows the regression of interest payments on the time dummies for the four quarters before and after the change in the interest rate as in (1). Each coefficient captures the dollar reduction in the interest payment in that quarter for the sample of all 5-year ARMs with a 10-year interest-only payment originated between 2005 and 2007. In Column (1) we control for households and month fixed effects and show that in the quarter after the event there is a significant reduction in the interest payment of about $1045. Similar estimates are presented for the subsequent quarters. The presence of small changes in the pre-period is due to the possibility of voluntary payment or adjustments by the households. However, coefficient goes from -$47 to -$1,044, which highlights how important the change in the interest rate has been for households' balance sheets.

Column (2) confirms similar results controlling for the borrower's FICO score and the log of house prices in the county. Column (3) is a more restrictive specification, as we control for county-

month fixed effects as well as household fixed effects. But even when we capture this time-varying heterogeneity at the county level, neither the economic magnitude nor the statistical significance is affected. Our preferred specifications are those in Columns (4) and (5). Column (4) includes households fixed effects and origination cohort-time fixed effects. As is evident from the magnitude and statistical significance of our results, the estimated coefficients are not affected by potential heterogeneity across mortgages originated in different time periods.

The reduction in the monthly payment tends to go down from $920 in the first quarter after the adjustment to $720 two years afterward, but this is because that for later quarters the only borrowers on whom we have the data two years after the adjustment are those originated earlier in our sample, and these are mortgages with lower monthly payments than the later cohorts. This is taken into account in Column (5): allowing for differential trends depending on the size of the initial monthly payment, the reduction in the monthly payment is very stable at around $920 for the entire post-adjustment period. Finally, in Column (6) we run a similar specification to the one in Column (4), the only difference being that we normalize the monthly payment by the size of the original payment. Intuitively, the estimated coefficients capture the size of the reduction in percentage points. While there is no economically significant reduction in the monthly payment before the interest rate adjustment, it is reduced by 53 percent in the first quarter after the adjustment and this effect persists for the next two years.[23]

In sum, the automatic reset of the interest rates constituted a major positive disposable income shock for these households. Unlike most of the literature on households' consumption response to income shocks, which focuses on one-time payments, such as tax rebates, we have the opportunity to investigate a shock of tens of thousands dollars per year.

4 Main Results

We start by investigating the effect of the change in interest payments on the households' consumption behavior and then analyze its effect on their debt-repayment strategy.

[23] The small reduction in monthly interest payments before the adjustment in Figure 5 shows that even before the adjustment, households make some partial prepayment of their mortgages and therefore their monthly payments decline by less than 1%.

4.1 The Consumption Response

We take data on auto loans to capture the purchase of a car in response to the reduction in the monthly mortgage payment. We can track the changes in the auto loan balance to identify all the instances in which households purchased a car using financing.[24]

We start our analysis with Figure 8 which shows an event study analysis with time zero as the month of the interest rate reset and the x-axis showing the quarters before and after the event. The top graph plots the average monthly amount spent on car purchases through an increase in auto loans. It shows that households increase their car consumption starting one quarter before the interest rate reset, allocating on average $50 to it. This suggests that households were anticipating the mortgage payment reduction and began to increase their car purchasing before the reset date.[25] Interestingly, however, the effect increases in the subsequent quarters to an average of as much as $200 one year after the interest rate adjustment. The bottom graph plots the same coefficients normalized by the initial monthly payment. On average, the households that expect the reduction in the monthly payment allocate about 5% of the positive income shock to purchase a car in the quarter before the adjustment, but this effect increases significantly in magnitude in the next quarters to over 20% one year after the adjustment.[26]

Table 3 confirms the previous results controlling for several potential confounding effects. Column (1) presents the coefficients controlling for household and month fixed effects. It shows that starting one year before the change in the interest rate, households start spending about $40 more on durable goods. However, in the quarter after the reset the households more than double their consumption spending to almost $100, and to $203 two years after the interest rate adjustment. Column (2) controls for the borrower's FICO score and the log of house prices. Both the statistical and the economic magnitude of the estimates are unaffected. Intuitively, higher FICO scores

[24] In Section 6 we complement these results using a different measure of consumption, such as store and bank credit cards.

[25] An alternative explanation for the increase in consumption the quarter preceding the reset is that lenders are required to disclose information about future interest rate adjustments, at least 60 days before it occurs. Therefore, the increase in consumption before the reset can be the borrowers' behavioral response to the resolution of the uncertainty surrounding the amount of decline in the interest rate. Moreover, the regulation implemented after the crisis, such as the Truth in Lending Act, extended this term to six months. For more information see http://archive.regulationroom.org/mortgage-protection/issue-posts/for-all-borrowers-adjustable-rate-mortgages/.

[26] Note that the coefficients are normalized by the initial monthly payment, and we know from Table 2 that the payment falls 50%. Hence, we need to double our point estimate to capture the fraction of the monthly reduction in payment allocated towards car purchases.

predict higher consumption, because they capture the credit available to these borrowers.

In Column (3) we saturate the model with county-month fixed effects, showing that allowing for differential trends across different counties does not affect our estimates. This is particularly important for this estimation, because households' consumption decisions can be significantly affected by local economic conditions. For instance, households living in counties worst hit by the financial crisis, such as those that experienced the sharpest declines in house prices or employment, may have a different marginal propensity to consume than households living in less severely affected regions. In Column (4), instead, we introduce origination cohort-time fixed effects. The coefficients are robust to this factor, which shows that the differential conditions at origination, such as the different equity in the house, have no significant impact on our estimates. In fact, households start allocating an additional $52 to consumption before the interest rate adjustment, increasing to $93 in the quarter after and $150 one year after.

Column (5) shows the coefficients for the specification in which we allow for different trends for different quartile of the monthly payment at origination. This captures in a non-linear fashion the possibility that households facing different monthly payments behaved differently. For instance, households with higher monthly payments might have higher incomes and own larger houses, which might also lead them to have different consumption responses to the positive income shock. The magnitude of these effects is particularly large if compared with the data in the top plot of Figure 5. Since we are examining the consumption behavior of households with mortgages originated between 2005 and 2007, we are investigating consumption behavior in the period 2010-2012. Considering the monthly expenditure on cars for that same period, the top panel of Figure 5 shows that it averages $300 a month. Our estimates suggest that after the interest rate adjustment the households' car purchases increased by at least 30%, and as much as 55%.

In Column (6) we estimate a linear probability model to determine how the likelihood to purchase a car is affected by the change in the monthly payment, which provides us with an estimate of the households response on the extensive margin. It shows that this probability increases by 0.35% a month in the first quarter after the adjustment, rising to 0.5% two years after. This is an economically significant effect, because as shown by the bottom panel of Figure 5, the average monthly probability of buying a car in the 2010-2012 period is about 1.3%. Hence, households increase their monthly probability of purchasing a car by at about 40 percent.

Finally, Column (6) reports the estimated coefficients of monthly car purchases normalized by the size of the original monthly mortgage payment, controlling for household and mortgage cohort-time fixed effects. Households spend 10% of the income shock the first quarter after the interest rate adjustment, but this effect increases over time, reaching 20% one year later.[27]

Interestingly, these effects together show a different reaction of durable consumption from its response to the fiscal stimulus recently identified by Mian and Sufi (2012a). The latter estimate the impact of the 2009 "Cash for Clunkers" program on short and medium-run auto purchases and show that the resulting boost in aggregate demand is quite short-lived. In fact, they find that almost all of the additional purchases under the program were pulled forward from the near future. In our case, instead, the reduction of the monthly payment significantly increased aggregate demand, and we find no evidence of intertemporal substitution. This follows from the absence in the quarters before the interest rate adjustment, of any evidence that households decreased their consumption. Nor do we find that the effect is short-lived; quite the contrary, we find that it increases over time. We believe this is due to the different features of the underlying income shock. Unlike Mian and Sufi (2012a), which examines a one-time subsidy to purchase prices, we consider a shock that is much larger and that persists over a number of quarters. Hence, as we shall show in Section 5, our mechanism is likely to involve wealth and liquidity effects. Moreover, since the shock is less temporary than in Mian and Sufi (2012a), households do not change just the timing of their consumption but also its level.

More generally, we can compare our estimates with the literature on households' consumption response to income shocks. Among the most recent contributions to this literature, Parker et al. (2013) analyzes the reaction to the Economic Stimulus Act (ESA) of 2008, which consisted in a tax credit of $300 to $1200 depending on the household's size. They find that households spent about 12 to 30 percent of their stimulus payments on non-durable consumption goods and another 38 to 60 percent on vehicles, which is only slightly smaller in magnitude than the response to the

[27] Since we do not observe the households' consumption of non-durable goods (except for credit cards balances, which we use in Section 6 to corroborate our results), one possibility is that the observed increase in the car expenditures results from a substitution between non-durable and durable goods purchases. However, for this argument to hold, the timing of this substitution should be correlated with the adjustment in the mortgage interest rate (which has no direct impact on the interest rate on car loans). Second, since car purchases in our data are leveraged purchases, for each dollar of reduction in non-durable purchases the household is able to increase the car expenditure by more than 5 dollars. Therefore, even in this case the net effect on household consumption would be positive. Having said this, given the limitation of our data we cannot rule out this possibility completely.

2001 tax rebates (see for instance Johnson et al. (2009)). On the one hand, we find a smaller effect (comparing the $150 spent on vehicles out of the $900 reduction in monthly payment with the estimates provided by Parker et al. (2013)).[28] On the other hand, we find that the radically different source of the income shock means that in our case the effect lasts for up to two years, which makes the overall consumption spending significantly larger. Another significant factor in this comparison is that mortgage interest is tax-deductible, which makes the effective income shock about thirty percent lower, depending on the household's tax bracket. In the next section we provide evidence of one important reason why households might not spend the additional income on consumption: voluntary deleveraging.

4.2 Voluntary Deleveraging

The way in which monetary policy can affect households' behavior depends crucially on precautionary saving. In general, we would expect that if households are liquidity-constrained, a decrease in debt service will be associated with an increase in consumption. But the magnitude of this effect can be a function of their incentive for precautionary saving. That is, the greater the income risk, the smaller the consumption response.[29]

To estimate this effect, we record the changes in the debt balance for the households affected by the automatic interest rate reset. This allows us to track down their incentive to allocate the savings to faster repayment of their loans. To be sure, we are not able to capture other forms of saving, such as retirement accounts or savings deposits, so we necessarily underestimate the precautionary incentive. Nevertheless, we believe that given the collapse in house prices and the high loan-to-value ratios for the majority of the mortgages in our sample, repaying their mortgage more rapidly and building equity in their homes might constitute an important way for households to use the additional resources available. As we will discuss in Section 5, the incentive to build equity in their homes crucially depend on their current LTV, because for households with LTV closer to 100 percent, the option to default is less attractive than for deeply underwater households, who might then have lower incentives to deleverage.

[28] Consistent with the literature, we consider leveraged purchases, which imply that the actual cash outflow for the household can be lower than the amount spent on the new car.

[29] Consistent with this intuition, Agarwal et al. (2007) analyze the 2001 tax rebate and show that consumers initially saved some of the rebate, increasing their credit card payments and so paying down debt, but that their spending increased soon afterwards.

Figure 9 shows an event study analysis with time zero at the interest rate reset and quarters before and after the event on the x-axis. The top panel shows the average monthly amount allocated to voluntary repayment of mortgage principal. We only consider partial prepayment, because full repayment coincides with the house being sold or the mortgage being refinanced. The plot shows that, in contrast to the consumption response presented in Figure 8, even if the change in the monthly payment is anticipated, the borrower does not allocate an economically significant amount of money until the quarters following the interest rate adjustment. Specifically, households allocate on average \$60 per month to a faster repayment of their mortgage, and the amount increases in the following quarters. The bottom panel shows that this corresponds to about 11% of the positive disposable income shock.

Table 4 reports the coefficients estimated using a similar regression to (1), which allows us to supplement the findings of Figure 9 by controlling for several other factors that could distort our results. The dependent variable is the monthly reduction in the mortgage balance; it is computed from BlackBox data. Column (1) controls for household and month fixed effects and shows that borrowers spend about \$60 a month to repay the principal on their mortgage in the first quarter after the reset, but no significant pre-trend (the coefficient for the quarter before the adjustment is insignificant). Column (2) shows that the effect is robust to the FICO score and the county log of house prices. It shows that the FICO score positively predicts an increase in borrowers' voluntary deleverage. Moreover, higher housing prices that are correlated with general economic conditions tend to be correlated with faster deleveraging.

Columns (3) and (4) show the robustness of our results to the inclusion of county-month fixed effects and cohort-time fixed effects. In fact our effect is even larger, reaching almost \$80 two years after the reset. These tests further reassure us that our results are not driven by heterogeneity in county or time of origination factors that might determine the households' saving decisions. The specification in column (5) allows for different trends for households with different monthly payments. Even here, however, the statistical and economic significance of our estimates remains unaffected. Finally, in Column (6) we estimate a specification similar to that of Column (4) but normalizing the prepayment by the original monthly mortgage payment. We find that on average 10% of the income shock goes to repay the mortgage.

To evaluate these results, we can compare them with the average amount allocated by households

to repay their mortgages in the pre-adjustment period (Figure 6). Up to 2010, the first year when any of our sample households benefited from the interest rate adjustment, the average amount devoted to deleveraging was $40 over the period 2007-2010. This means that after the adjustment, households more than doubled their efforts to reduce their debt level.

4.3 Instrumental Variable Estimates

Up to now we have estimated the effect of the reduction in the mortgage interest rate comparing households that experienced the adjustment at different point in time. However, we can also directly instrument 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 reduction in the mortgage monthly payment.

Specifically, we can estimate equation (1) by two-stage least squares (2SLS) in Table 5. We instrument for the lags and leads of the monthly payments using indicator variables equal to one for the same lags and leads. In all columns we control for time and household fixed effects, and we allow for heterogeneous time trends for different origination cohorts. Columns (1) and (2) only reports the coefficient for the month of the change in the monthly payment. Columns (3) and (4) adds the results for the three months before the adjustment and the columns (5) and (6) for the three months after the adjustment. The signs are negative because we are estimating the effect of a reduction in the monthly payment.

As with the estimates in Tables 3 and 4 we observe a significant increase in car purchases and mortgage repayment after the interest rate adjustment. There is some evidence of a small increase in consumption, by about 2.5%, before the adjustment, but no similar effect is found for mortgage prepayments. By looking at the first few months around the adjustment we observe a total effect of about 12 percent for car sales and 8 percentage points for partial prepayments.

5 Heterogeneous Responses across Households

In this section we analyze the heterogeneity in households' consumption and saving decisions in response to the income shock.

The theoretical literature indicates the types of households that should respond more forcefully

to a positive income shock. For instance, Zeldes (1989a) shows that an important source of heterogeneity is the tightness of households' liquidity constraint, which can motivate the reaction to an anticipated income shock like ours. Liquidity-constrained households may be unable to increase their consumption until the income shock occurs. A number of studies, such as Zeldes (1989b), Jappelli (1990), Aiyagari (1994), Jappelli et al. (1998), Kaplan and Violante (2011) and Jappelli and Pistaferri (2014) among others, support this hypothesis.[30]

We can employ several measures to capture liquidity constraints. First, a cash-flow measure of liquidity is borrower income, which we measure in the pre-adjustment period.[31] Table 6 tests for differences in spending and saving across income groups. Since we need to report the interactions between the time dummies and the income indicator, we restrict attention to one year before, one year and two years after the reset date, and we only report the normalized coefficients, which makes the effect easier to interpret.[32] High income equals one if the households' income is larger than the median one, i.e. larger than $55,000 a year. High-income households' cash flow shock is about 5 percentage points smaller, as shown in Column (1), which could reflect the fact that these high-income households had better credit scores at origination and, therefore, their initial interest rate was slightly lower. Interestingly, Column (2) provides supporting evidence that low-income households tend to have a higher marginal propensity to consume. However, we also find that they have a significantly lower marginal propensity to deleverage (Column (3)) in the first year after the interest rate reset.[33]

Another important gauge of liquidity constraints on homeowners is their loan-to-value ratio (LTV). Table 7 tests for differences in car purchases and mortgage principal prepayment across households with different LTV. "High LTV" is an indicator variable, equal to one for current LTV larger than 120 percent.[34] We measure the LTV in the 24 months up to 12 months before the interest rate adjustment and in all specifications we control for household fixed effects, origination

[30] Given the size of the income shock, its monthly nature and its duration, we believe that the utility costs of not smoothing consumption before and after the interest rate adjustment are not likely to be small. See Caballero (1995), Parker (1999), Sims (2003) and Reis (2006) for studies about this possibility.

[31] Equifax provides a measure of income that is based on the dynamic credit characteristics of the household. We use the average of this measure in the period of 24 to 12 months prior to the interest rate reset.

[32] The estimates with quarterly dummies show exactly the same pattern and are available in the online appendix here.

[33] Recently, Krueger and Perri (2006) and Aguiar and Bils (2011), among others, analyze if consumption inequality has tracked income inequality.

[34] Note that this is not the LTV at origination, but the LTV computed by using information on the zip-code level house prices and the current mortgage balance.

cohort-time fixed effects and high- and low- LTV-time fixed effects. These 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 a high LTV experience a monthly income gain only slightly higher than the other borrowers (Column 1). This might reflect the fact that households, who purchased their houses in 2006, had the highest initial interest rate, and experienced the largest decline in the value of their houses. However, these households have a significantly higher marginal propensity to consume. In fact, high-LTV borrowers spend more than twice as much on durable goods as low-LTV households (Column 2). Next, we investigate how saving decisions are affected by LTV. We find that borrowers with higher LTV tend to deleverage less. Intuitively, borrowers who are deep underwater have little incentives to use the reduction of the monthly payment to repay their debt, because they do not expect to be able to build equity in their homes any time soon. In contrast, the households with intermediate LTV can really benefit from the reduction in the interest rate, as a smaller repayment may well get them out from underwater and enable them to build equity.[35]

Finally, we can also capture the heterogeneity in households' access to credit with their FICO score, proxying credit availability with the average borrower's FICO score over the period 24 to 12 months before the interest rate adjustment, so that post-adjustment consumption and saving decisions cannot influence this proxy. We divide the sample between borrowers with FICO scores above and below 660, denoted by the dummy "High FICO". Since we measure durables consumption with leveraged car purchases, we should expect that households with less access to the credit market will spend less on vehicles.[36] Table 8 tests this hypothesis.

In all specifications we include household fixed effects, as well as origination cohort-time fixed effects, but we also modify our baseline specification to allow for heterogeneous time trends between high- and low-score households. Column (1) investigates the reduction in the monthly payment: households with high FICO scores have a monthly payment reduction only 6% lower than those with low FICO. However, Column (2) shows that borrowers with high FICO consume 13% more than those with less access to the credit market. This is consistent with the interpretation that

[35] Consistent with this view, Gross and Souleles (2002) show that consumers whose credit card limits get increased increase their debt, and that the effect is larger for consumers near their current limit, which is consistent with binding liquidity constraints. We supplement these results by analyzing the behavior of borrowers with different loan-to-value ratios.

[36] For an analysis of the role played by credit constraints in the auto loan market see Attanasio et al. (2008).

low FICO households face higher borrowing costs and poorer access to credit, i.e. auto loans, which presumably result in lower car purchases. This is important to an understanding of how the fiscal multiplier varies when borrowers are more credit-constrained: when leveraged purchases are limited or credit standards tightened, the benefits from low interest rates can be limited for the more constrained borrowers. Column (3) also shows that there are significant differences in the deleveraging behavior of households with different FICO scores, because the more creditworthy deleverage by 30% more than the less creditworthy.

Taken together, our results on the marginal propensity to consume and deleverage in different types of household suggest the importance of liquidity constraints. The reduction in the monthly payments, as a result of the low-interest-rate environment, provided the resources for down payments on leveraged purchases of cars, which would not have been available otherwise. This corroborates the hypothesis that the fiscal stimulus we identify is likely to operate through wealth and liquidity mechanisms. Moreover, since the underlying income shock is not a one-time subsidy to purchase prices (Mian and Sufi (2012a)), or an "economic stimulus payment" (Parker et al. (2013)), households do not appear to engage in intertemporal substitution, but rather make new purchases made possible by the relaxation of their liquidity constraints.

6 Further Evidence

In this section we present further evidence corroborating our previous findings and testing their robustness.

6.1 Attrition

One potential concern with our estimates is that they might be distorted by some form of attrition. This is an important concern, especially if we consider that our sample period covers the recent crisis and that the hybrid ARMs we consider might have had an even harder time during the Great Recession than less risky mortgage types. In Table 1.B we have compared the characteristics of these ARMs and of the households holding them with those of the larger representative sample of loans in LPS, which includes both fixed-rate mortgages and a more general form of ARMs; we found no significant difference except for mortgage size. In this section, however, we want to examine

potential source of attrition within our sample.

We start by reporting in Figure 10 three types of loans that are present in our sample for the 2008-2012 period. Specifically, this plot shows the number of loans that remain active throughout the period (and we restrict our analysis to them), the number of loans that over time are paid off 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 or after the interest rate adjustment. This figure shows that about 40% 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. In fact, the number of liquidated loans increase significantly 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. BlackBox does not report information on current loan to value ratios (CLTV), but we can compute it by using information on the mortgage balance and house prices at zip code level. Figure 11 shows the cumulative distribution for the CLTV for these three categories of loans: active, liquidated and paid-off. What is immediately clear is that paid-off loans have significantly lower CLTV than active and defaulted mortgages. Specifically, one quarter before these loans drop out of our sample the median CLTV is 78%, which corresponds to the vertical line in the graph. This corresponds almost exactly to the common threshold of 80% used by financial institutions in determining the refinancing costs.[37] Moreover, this is significantly higher than the 110% or 115% CLTV of active and defaulted loans, respectively.[38] The graph for active loans shows that only about 5 percent of all active loans have current LTV below 80%. This suggests that these households could not refinance their mortgages, and benefit in this way from the low interest rates before the adjustment, because they were essentially locked-in their contract, due to the low home equity. Finally, one advantage of our data is that the loans in our sample are not eligible for HARP because privately securitized.[39]

[37] Caplin et al. (1997) report that, since the higher borrowing rate associated with higher CLTV is applied to the whole outstanding mortgage balance, not just the incremental equity removed, the additional cost for households refinancing with an initial LTV just above 80% can be as much as 20 percent. See also Hurst and Stafford (2004) for a discussion of this issue.

[38] Notice also that our measure of CLTV tends to underestimate it. Elul et al. (2010) have access to a measure of total household debt, combined LTV, and they show that for the households with a second mortgage, using only the first-mortgage LTV underestimates their total CLTV by 15 percentage points.

[39] In order to be qualified for HARP, the mortgage must have been acquired by Freddie Mac or Fannie Mae on or before May 31, 2009.

This figure suggests two observations. First, the decision to refinance a mortgage is mainly driven by the CLTV, which means that households in counties less affected by the housing bust will have access to this opportunity, while those living in the worst affected regions are unlikely to be eligible for refinancing. Second, the absence of any significant difference between the CLTVs of active and defaulted mortgages is perfectly consistent with the "double trigger" hypothesis that mortgage default depends on the joint occurrence of negative equity and a life event like job loss. Gerardi et al. (2013) show that the strongest predictor of default, in fact, is individual unemployment. Moreover, they also show that only a very small fraction of defaulters have both negative equity and enough assets to make one month's mortgage payment, which suggests that "strategic" defaults were relatively rare. Similarly, Elul et al. (2010) found that negative equity, illiquidity as measured by high credit card utilization, and unemployment shocks are all associated with higher default risk, and that the latter interacts strongly with CLTV.

In our analysis we only consider active loans, comparing the consumption and savings decisions of borrowers benefiting from the interest rate adjustment at different points in time. We do not consider the households who defaulted or prepaid their loans, as this would clearly bias our results. Our analysis in Section 5 of the heterogeneous response to this income shock provides some speculative insights into how our estimates of the marginal propensity to consume might be generalized to this sample as well. Specifically, since low-income and those credit-constrained households tend to consume a significantly higher fraction of the added income, the exclusion of borrowers who enjoyed the income shock but then defaulted (the majority of those dropping out of our sample) probably biases our results downward.

6.2 Difference-in-Differences Results

In this section, we further test the validity of our identification strategy. One potential concern with the consumption and deleveraging estimates presented in Section 4 is that there might be a mortgage-specific trend that could affect our results. In particular, since it is collinear with the time dummies, in the previous specification we could not control for the age of the mortgage, which might be correlated with the household's consumption or prepayment behavior. For instance, households might be more inclined to purchase a new car twelve months after they bought a house, or they might have a greater incentive to prepay their mortgage once they have built enough equity

in it. Then there might be heterogeneity among households with mortgages of different vintages. In order to correct for this possibility, we consider as control group the mortgages that have the interest rate reset 10 years after origination, i.e. 10-year ARMs.

This allows us to compare the behavior of the borrowers who experienced a reduction in the monthly payment with that of other borrowers who bought houses during the same period under a similar hybrid mortgage. Table 9 reports coefficient estimates of least square regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset 5 years after the origination. In contrast to the previous estimations, this sample includes both 5-year and 10-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. We report in Columns (1)-(3) the results in dollars, in Columns (4)-(6) the normalized coefficients.

Columns (1) and (4) show a reduction in the monthly payment of $900 on average, about half of the monthly payment, in the quarter after the interest rate reset, which is comparable to the results reported in Table 2. Columns (2) and (5) analyze consumption decisions: borrowers who experienced a reduction in the monthly payment tend to increase their consumption of cars by about $145-$185, or 15%-20% of the income shock, within the first year compared to the borrowers holding a similar mortgage but not experiencing the payment reduction. Finally, Columns (3) and (6) show that borrowers employ about $60 per month, or 10% of their additional disposable income, to repay their mortgage faster.

One drawback of this alternative specification is that households with different characteristics might be endogenously sorted in different contract types, 10-ARMs versus 5-ARMs. However, in all specifications we include household fixed effects which absorb any fixed variation at the individual level. More importantly, we also allow for differential trends for different loan cohort and types. That is, our estimates do not rely on the assumption that households who decided to purchase a house, for instance, in 2005 with a 10-ARM, will follow a similar trend to the households that purchase one, say in 2006 with a 5-ARM. Finally, since we also include the age of the mortgage as an additional control, these estimates reassure us that we are not capturing any time trend specific to the mortgage vintage.

6.3 Unexpected Rate Reduction and the Role of Uncertainty

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 the index was at the minimum. Moreover, this reduction in the interest rate translated in a significant reduction in the monthly payment, because these were interest-only mortgages for the first ten years. Since the index rates have been declining for several months before the mortgage interest rate adjustment date, the households with this mortgages might have expected the interest rate reduction once the economy entered in the more severe phases of the financial crisis.[40]

However, we can investigate the effect of an unexpected interest rate reduction by analyzing ARMs that reset during the period January 2007-March 2008. This covers the first time the LIBOR declined and a relatively quiet period for the US economy. In order to increase the sample size, we gather information on all ARMs that reset during that period, not only interest-only 5-year ARMs, and we focus on one single dummy for the post-adjustment period. Table 10 reports both the least squares and the instrumental variable estimates, controlling for time and household fixed effects.

Column (1) shows that on average during this period the monthly payment declines by $252. This reduction is significantly smaller than what reported in Table 2, because the reduction in the interest rate is lower and the monthly payment comprise both interest and principal repayment. Column (2) shows that $88 dollars, or equivalently 35 percent of the monthly payment reduction, is spent on repaying the mortgage faster. In Column (3) we investigate the households' consumption response and show that they increased by $128 on average, or equivalently 50 percent of the reduction in mortgage payment, their car purchases after the interest rate adjustment. Columns (4)-(7) report similar results when we instrument the monthly payment with time dummies. Moreover, in contrast to Table 3 we find no pre-trend in the households' consumption response. This is consistent with the interpretation that this interest rate reduction came more unexpected that the one considered in the previous sections.

A growing literature starting with Eggertsson and Krugman (2012) studies the effects of deleveraging on the economy, and recent theories attribute an increase in deleveraging to different factors, for instance, Guerrieri and Lorenzoni (2011) develop a model highlighting the role of an unexpected

[40] If any, this would bias our results downward, due to anticipatory changes in consumption and saving decisions.

tightening in consumers' borrowing capacity, while Alan et al. (2012) links deleveraging to an increase in income uncertainty. We can obtain few more insights into the reasons behind the observed increase in deleveraging by focusing on another period, from November 2008 up to December 2009, which is the period in which the LIBOR decreased, causing an increase in disposable income, and the volatility index (VIX) significantly decreased, capturing a significant decrease in the level of uncertainty in the economy.

Table 11 reports both the least squares and the instrumental variable regressions for all ARMs that experienced a decrease in the interest rate during this period. As before, due to data availability we focus only on one time dummy for the post rate adjustment period. There are three main results. First, to the reduction in the monthly payment of $400 (Column (1)), households respond with an increase in their mortgage prepayment by almost $50 (Columns (2) and (6)). Second, there is no evidence of an increase in their car purchases (Columns (3), (5) and (7)). More importantly, we interact the time dummy with the VIX to study the effect of an increase in uncertainty on the households' consumption and saving behavior. Columns (4) for the least square and (8) for the IV regressions highlight the importance of an increase in the uncertainty in the economy on the households' precautionary saving incentives. Specifically, in periods of higher uncertainty households significantly increase their deleveraging, with a one standard deviation increase in the VIX (which is about 30) almost doubling the households' prepayment efforts. This evidence suggests that, as proposed by the existing theoretical literature, higher uncertainty might lead to a significantly higher precautionary saving motive.

6.4 Alternative Consumption and Deleveraging Measures

The richness of our data allows us to complement the previous findings by investigating the impact of monetary policy on different measures of consumption and deleveraging. We observe the balance on all the borrower's revolving accounts and focus on retail credit cards, e.g. credit cards issued by large store chains such as Amazon and Macy's. Like our measure of car sales, this measure tracks the consumption expenditures of the households by analyzing significant changes, above $500, in the balance on these accounts. Table 12 shows the coefficient estimates of a least square regression relating the amount spent on retail credit cards with the interest rate reset. In Column (1) we control for household and time fixed effects, as well as loan cohort fixed effects, and show that

households tend to increase their expenditures after the interest rate adjustment by almost $30. Here too we uncover a similar spending pattern, with households starting to increase their consumption one quarter before the interest rate reset and keep consuming more after it. In Column (2), instead, we show that normalizing by the size of the initial monthly payment, this increase corresponds to 3% of the positive income shock. Finally, Column (3) augments the specification by allowing for heterogeneous trends depending on the size of the monthly payment. The results are still statistically significant at the 1% level, but the magnitude is smaller, with an average consumption response of about $16. We also identified a subsample of households that use their credit cards as a payment card and show in Columns (4)-(6) that their credit card balance significantly increase after the interest rate adjustment.[41] Specifically, one quarter after the reset the credit card balance increases by $123, and keeps increasing up to $275 one year after the reset.

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 be an underestimation of 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 the more expensive loans, such as equity loans and home equity lines of credit. We investigate this possibility in Panel B of Table 12, where we restrict attention to the households who had an active home equity or home equity line of credit for at least half of the sample. For brevity, we provide the two most restrictive specifications, those controlling for origination cohort-time fixed effects and monthly payment bin-time fixed effects, and the estimates normalized by the initial mortgage payment. Columns (1), (3) and (5) test for an increase in payment towards equity loans. They all show that there is indeed a significant increase in prepayment of this type of loans after the interest rate adjustment with an average effect of $15 per month in the more restrictive specification. Columns (2), (4) and (6) report the results for home equity line of credit. We find that in the quarter after the interest rate reset, borrowers allocate on average about $20 per month towards the repayment of these lines of credit.

In sum, analyzing a different measure of consumption and the repayment behavior for the case of other two types of debt, we confirm and reinforce the main results presented in Section 4 on

[41] Please see the online appendix for more details about how we identified the households who used their credit card as a payment card.

households' consumption and deleveraging response to the positive income shock.

7 Aggregate Level Evidence

In the previous sections, we have used the sub-sample of hybrid ARMs to limit unobserved heterogeneity and identify more cleanly the effect of interest rate changes on household's consumption and saving decisions. We now turn to county level data to gauge the extent to which these results might be generalized across a broader sample of households, and to better understand their local general equilibrium implications. Admittedly, our methodology cannot estimate the aggregate general-equilibrium effect, such as an economy-wide multiplier of interest rate policy, as for instance we do not observe the lenders' response to such changes in interest rates. Nevertheless, 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 county level. And in particular, we would expect that a decline in interest rates will likely have a bigger impact on household consumption in those counties that use adjustable rate mortgages more intensively. Recently, Garriga et al. (2013) develop a general equilibrium model showing that monetary policy affects decisions through the cost of new mortgage borrowing and the value of payments on outstanding debt. The transmission is found to be stronger under adjustable- than fixed-rate contracts, suggesting that mortgages are an important example of a persistent nominal rigidity.

To develop tests based on this idea, we augment our data with three additional data sources. First, we aggregate loan level data from LPS, which constitutes a nationally representative sample of mortgages with a 64% coverage of the U.S. mortgage market (Fuster and Vickery (2013)). Second, to correctly estimate the mortgage and the credit card balance at the county level, we employ data from the New York Federal Reserve Consumer Credit Panel, which collects information from a 5 percent representative sample of households in the U.S., and aggregate this information at the county level. Third, we measure consumption of durable goods through the car sales data provided by Polk. This data provider collects information on the sales of new vehicles at quarterly frequency by county.

We start by describing the main variables of interest. Figure 12 illustrates the county-level variation in adjustable-rate mortgages at the peak of the boom in 2006. It shows that the cross-

sectional variation in the fraction of ARMs is not random, as these contracts are relatively more frequent along the coast, where housing costs are generally higher. So it is possible that the variation in the fraction of ARMs could be correlated with some unobserved factors that might explain the transmission of monetary policy to the local economy. We take this possibility into account in our empirical strategy by controlling for several county characteristics. Over our sample period, there is also substantial variation in our measure of local consumption as shown by Figure 13, which plots the year-on-year quarterly change in car sales. As it is evident, the number of cars sold is high and stable in the first half of our sample, but then it sharply declines during the Great Recession to almost half of what it was before, i.e. two million cars. Finally, Figure 14 plots the six-month LIBOR from 2005 to the end of 2013 and shows a decline of about 4.5 percentage points. This confirms that the ARMs indexed to the LIBOR are able to take advantage of a significant reduction in the interest rates.

Table 13 presents our estimation results. We first provide simple correlations for the fraction of ARMs in a county in 2006 and county-level characteristics from the same period in Panel A. This guides us in understanding what the main drivers of higher ARMs origination are and which controls we need to include. Higher-income counties tend to have a higher fraction of ARM debt, which might capture the preference of households with higher incomes to purchase houses with jumbo loans featuring an adjustable interest rate. Likewise, counties with more highly leveraged households—a higher median debt to income ratio—tend to also have a higher fraction of ARM debt. Moreover, ARM debt is more common in counties with greater securitization activity, capturing the greater incentives for risk-taking by banks, and with a higher poverty rate, which might reflect the prevalence of teaser rates and negative amortization mortgages among the more credit-constrained households.

Since during our sample period interest rates were declining, if regions with higher concentration of ARMs also experience a more significant decline in consumption due to these other factors, we might find a spurious correlation between changes in interest rates and aggregate consumption. Moreover, many of these factors could also independently shape the transmission of interest rate movements to household consumption. Higher-income households may have easier credit access, for example, and be better placed to buffer any changes in the cost of credit.

Accordingly, we absorb these variables by using county fixed effects, but we also interact them

with the six-month LIBOR. Our sample period extends from 2007 to 2013, and because households can adjust at the margin to changing economic conditions, throughout we use county-level variables observed in 2006 to avoid endogenous responses when measuring the impact of interest rate movements on county-level economic outcomes.

Panel B of Table 13 shows how the average interest rate for outstanding mortgages and the average monthly payment in a county can be differentially affected by changes in the LIBOR, depending on the fraction of ARMs. In all columns we control for county level controls, such as the log of population, the debt-to-income ratio as computed in 2006, the change in securitization computed as the change in the fraction of loans privately securitized over the period 2002-2006, the poverty rate and their interactions with the six-month LIBOR. Moreover, we also include time and county fixed effects, which absorb the impact of aggregate economic shocks that might affect counties simultaneously, and time-invariant county characteristics, such as the elasticity of housing supply. Standard errors are clustered at the county level. Columns (1) and (2) investigate the effect on the average interest rate, while Columns (3) and (4) examine the average monthly payment.

We find that as the interest rate declines, counties with a higher fraction of ARMs display a more significant reduction in the average mortgage rate and in their average monthly payments, which suggests higher pass-through of changes in monetary policy and in LIBOR to these counties. The economic magnitude too is substantial, because if we consider the observed decline in interest rates, about 5 percentage points from 2007 to 2013, a one-standard-deviation increase in the fraction of ARMs translated into a 9-11 basis points decrease in the average mortgage rate by 2013 and 2-3 percentage points lower average monthly payments.[42] Columns (2) and (4) also control for state-specific time trends, which allow for heterogeneous trends across states. For instance, states with a higher concentration of ARMs might react differently to the decline in the interest rates from states with a higher fraction of fixed-rate mortgages. We find that the main coefficient of interest is stable across specifications and always significant at the one percent confidence level.

In sum, the fraction of adjustable rate mortgages is a strong predictor of pass-through of changes in monetary policy to households' mortgages rates and monthly payment. Hence, to the extent that we are able to control for the other variables that are correlated with the fraction of adjustable rate

[42] Alternatively, for each one percent decline in LIBOR rates, a county in the top decile of fraction of adjustable rate mortgages enjoys 7 bps more decline in their average interest rates than a county in the lowest decile of fraction of adjustable rate mortgages.

mortgages in a county, we can use the interaction between fraction of adjustable rate mortgages and the LIBOR rate as an instrument for the average mortgage interest rates (or monthly payments) paid by households in that county. This allows us to try to determine whether the results on consumption and deleveraging in Section 4 at the individual level could be generalized to the aggregate economy.

Building on our previous results, Table 14 presents the effect of changes in the mortgage interest rate on car sales and on credit card and mortgage balances. We present both the reduced form results (Panel A) as well as the instrument variable estimates (Panel B). Columns (1)-(3) present the results of the reduced form, while Columns (4)-(9) focus on the IV results. Column (1) relates to the effect of changes in the interest rate on quarterly car purchases. We find that a decline in interest rates like that of 2007-2013 leads to a significant consumption response in counties with a higher share of ARMs in 2006. The point estimates suggest that a one-standard-deviation increase in the fraction of ARMs is associated with about 2.5-3% increase in car sales in that county.

We also use as an alternative measure of consumption the balances of households on credit cards which can be a noisy proxy for the households' consumption. In the New York Consumer Credit Panel, the average household carries only $800 balance on its credit cards, which is suggestive that on average households use their credit cards as a payment card rather than as a borrowing tool. The result in Column (2) suggests that a one-standard-deviation increase in the fraction of ARMs is associated with about 2.5% increase in balances on credit cards. Since we can only consider the aggregate balance on credit cards, we cannot fully distinguish between durable and non-durable goods. However, this evidence corroborates the hypothesis that aggregate demand responded more forcefully in areas where households could enjoy the reduction in mortgage rates.

Finally, we confirm that the decline in interest rates is associated with a more significant deleverage in counties with more AMRs in Column (3). In fact, we find that a one-standard-deviation increase in the fraction of ARMs is associated with about 1.5% decline in mortgage balances.

Columns (4)-(9) present our IV results for the interest rate and the monthly payment. As in the individual level analysis, where we estimate the effect of the change in the mortgage rate and the resulting decline in the monthly mortgage payment on households' consumption and saving behavior, at the aggregate level, we can instrument for these variables with the interaction between the index rate (i.e. LIBOR) and the fraction of ARMs. The results are both statistically and

economically significant. For instance, a 10 percentage points decline in monthly mortgage payment is associated with an increase of 10% in car sales, a 5% higher deleveraging and about 15% higher credit card balances.

To be clear, even if the exclusion restriction is satisfied, we cannot use these elasticities to calculate the aggregate effect of changes in interest rates on households consumption and deleveraging decisions. This is because as long as the fraction of adjustable rate mortgages in a county is correlated with the fraction of lenders that operate in that county, the estimated coefficients only capture the partial equilibrium reaction of borrowers to changes in the interest rates (or monthly payments). However these results can still be used as an upper bound in estimating the effects of monetary policy on consumption and deleveraging.[43]

8 Discussion and Concluding Remarks

We have exploited the automatic interest rate adjustment for households with hybrid ARMs to study households' consumption and saving response to the highly expansionary monetary policy of the post-crisis period. Our identification strategy exploits the expected change in monthly payments for borrowers who have adjustable rate mortgages originated between 2005 and 2007 with an automatic reset after five years. Focusing only on this type of mortgage allows us to be sure that issues relating to the selection of different types of contracts, such as FRM versus ARM, cannot contaminate the estimation.

The magnitude of the positive income shock for these households is large indeed: the monthly payment falls on average by $900 at the moment of the interest rate adjustment. Potentially, this could free up important resources for these indebted and mainly underwater households. We show that households increase their car purchase spending by more than $150 per month, equivalent to a 40 percent increase compared to the period immediately before the adjustment. Their monthly credit card balances also increase substantially, by almost $200 a month within the first year after the adjustment. Moreover, there is not any sign of intertemporal substitution or reversal within two years of the adjustment.

However, we also show that households use 15% of their increase in income to repay their

[43] See Nakamura and Steinsson (2014) and the literature aiming at estimating the fiscal multiplier for a discussion of a similar point.

debts faster, almost doubling the extent of this effort. This suggests that households' consumption response was attenuated by their desire to voluntary deleverage.

There exist important differences across households. Underwater mortgages are associated with allocation of a larger part of the income gain to car purchases and with less debt repayment. Low-income households, as well, according to our point estimates exhibit a higher marginal propensity to consume and less deleveraging. Those with less access to credit due to bad credit history, instead, tend to spend less on durables and to repay their debts more slowly. These effects underscore the importance of credit-constraint in shaping households' consumption and saving decisions, and uncover the negative correlation between these two.

We can use our household-level analysis to compute the partial equilibrium effect of a decline in interest rates on aggregate consumption. There are, however, several limitations that needs to be taken into account. First, we only capture the effect of the change in interest rates on the borrowers and not on the lenders, which might lead the partial equilibrium effect to overestimate the impact of the reduction in interest rates on consumption. Moreover, the elasticities estimated at the household level only capture the direct effect of the decline in interest rates on mortgage holders, and not the indirect effect on other households. Furthermore, the marginal propensity to consume of the average mortgage holder may differ from that of the households with 5-year ARMs in our data. In spite of these concerns, our estimates show that the direct partial equilibrium effect of a 2 percent decline in average interest rates can explain up to 10 percent of the increase in annual car sales and up to 8 percent of the increase in households' deleveraging.

Finally, we also show how debt rigidity can determine the aggregate effects of monetary policy transmission, providing evidence that the effects of a decline in the interest rate differ according to the concentration of adjustable-rate mortgages in different areas: the fall in interest rates produces a significantly larger increase in consumption and deleveraging in counties with a higher fraction of ARMs. This suggests that we can analyze how much aggregate consumption would have increased if a higher fraction of mortgage in U.S. were ARMs, implying a larger pass-through of a decline in interest rates to households. Keeping in mind that by abstracting from the effect of higher pass-through on lenders' consumption, this estimate is an upperbound on the economic stimulus, we can conclude that if the fraction of ARMs in the U.S. was equal to the 90th percentile of its distribution (i.e. 14 % higher ARMs), the aggregate car sales were 4% higher and households

would have deleveraged 2% more. Relatedly, Auclert (2014) provides a model evaluating the role of redistribution in the transmission mechanism of monetary policy to consumption, which predicts that if all U.S. mortgages were adjustable-rates, the effect of changes in the Federal Funds rate on consumer spending would be significantly higher.

These results can be useful in informing the discussion of the effects that the exit from quantitative easing could have on aggregate consumption through an income channel (see Hall (2013) and Shin (2013)). Our findings also indicate that interest rate or monthly payment reductions can help to ease cash flow and liquidity constraints and should be considered as possible policy responses in times of crisis (Eberly and Krishnamurthy (2014)).

References

Agarwal, S., J. C. Driscoll, and D. I. Laibson (2013). Optimal Mortgage Refinancing: A Closed-Form Solution. *Journal of Money, Credit and Banking 45*(4), 591–622.

Agarwal, S., C. Liu, and N. S. Souleles (2007). The Reaction of Consumer Spending and Debt to Tax Rebates–Evidence from Consumer Credit Data. *Journal of Political Economy 115*(6).

Agarwal, S. and W. Qian (2013). Consumption and Debt Response to Unanticipated Income Shocks: Evidence from a Natural Experiment in Singapore. *Available at SSRN 2324914*.

Aguiar, M. and E. Hurst (2005). Consumption versus Expenditure. *Journal of Political Economy 113*(5).

Aguiar, M. and E. Hurst (2007). Life-Cycle Prices and Production. *The American Economic Review 97*(5), 1533–1559.

Aguiar, M. A. and M. Bils (2011). Has Consumption Inequality Mirrored Income Inequality? Technical report, National Bureau of Economic Research.

Aiyagari, S. R. (1994). Uninsured idiosyncratic risk and aggregate saving. *The Quarterly Journal of Economics*, 659–684.

Alan, S., T. Crossley, and H. Low (2012). Saving on a rainy day, borrowing for a rainy day.

Attanasio, O. P., P. Koujianou Goldberg, and E. Kyriazidou (2008). Credit Constraints in the Market for Consumer Durables: Evidence from Micro Data on Car Loans. *International Economic Review 49*(2), 401–436.

Auclert, A. (2014). Monetary Policy and the Redistribution Channel. *MIT mimeo*.

Baker, S. R. (2013). Debt and the consumption response to household income shocks. *Stanford University Mimeo*.

Berger, D. and J. Vavra (2014). Consumption dynamics during recessions. Technical report, National Bureau of Economic Research.

Bernanke, B. S. and A. S. Blinder (1988). Credit, Money, and Aggregate Demand. *The American Economic Review*, 435–439.

Bernanke, B. S. and M. Gertler (1995). Inside the Black Box: The Credit Channel of Monetary Policy. *The Journal of Economic Perspectives 9*(4), 27–48.

Bertrand, M. and A. Morse (2009). What Do High-Interest Borrowers Do with Their Tax Rebate? *The American Economic Review*, 418–423.

Caballero, R. J. (1995). Near-Rationality, Heterogeneity, and Aggregate Consumption. *Journal of Money, Credit and Banking*, 29–48.

Calza, A., T. Monacelli, and L. Stracca (2013). Housing finance and monetary policy. *Journal of the European Economic Association 11*(s1), 101–122.

Campbell, J. Y. (2006). Household finance. *The Journal of Finance 61*(4), 1553–1604.

Campbell, J. Y. and J. F. Cocco (2003). Household Risk Management and Optimal Mortgage Choice. *The Quarterly Journal of Economics*, 1449–1494.

Campbell, J. Y. and N. G. Mankiw (1989). Consumption, income and interest rates: Reinterpreting the time series evidence. In *NBER Macroeconomics Annual 1989, Volume 4*, pp. 185–246. MIT Press.

Caplin, A., C. Freeman, and J. Tracy (1997). Collateral Damage: Refinancing Constraints and Regional Recessions. *Journal of Money, Credit, and Banking*, 496–516.

Carroll, C. and W. Dunn (1997). Unemployment expectations, jumping (S, s) triggers, and household balance sheets. In *NBER Macroeconomics Annual 1997, Volume 12*, pp. 165–230. MIT Press.

Carroll, C. D. (2009). Precautionary saving and the marginal propensity to consume out of permanent income. *Journal of Monetary Economics 56*(6), 780–790.

Carroll, C. D., J. Slacalek, and K. Tokuoka (2014). The Distribution of wealth and the MPC: implications of new European data. *The American Economic Review 104*(5), 107–111.

Christiano, L. J. and M. Eichenbaum (1992). Liquidity Effects and the Monetary Transmission Mechanism. *The American Economic Review*, 346–353.

Deaton, A. (1992). *Understanding consumption*. Clarendon Press Oxford.

Doepke, M. and M. Schneider (2006). Inflation and the redistribution of nominal wealth. *Journal of Political Economy 114*(6), 1069–1097.

Eberly, J. and A. Krishnamurthy (2014). Efficient Credit Policies in a Housing Debt Crisis.

Eberly, J. C. (1994). Adjustment of Consumers' Durables Stocks: Evidence from Automobile Purchases. *Journal of Political Economy*, 403–436.

Eggertsson, G. B. and P. Krugman (2012). Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach*. *The Quarterly Journal of Economics 127*(3), 1469–1513.

Elul, R., N. S. Souleles, S. Chomsisengphet, D. Glennon, and R. Hunt (2010). What" Triggers" Mortgage Default? *The American Economic Review*, 490–494.

Fuster, A. and J. I. Vickery (2013). Securitization and the Fixed-Rate Mortgage. *FRB of New York Staff Report* (594).

Fuster, A. and P. S. Willen (2013). Payment size, negative equity, and mortgage default. Technical report, National Bureau of Economic Research.

Gagnon, J., M. Raskin, J. Remache, and B. P. Sack (2011). Large-Scale Asset Purchases by the Federal Reserve: Did They Work? *Economic Policy Review 17*(1), 41.

Garmaise, M. J. (2013). The attractions and perils of flexible mortgage lending. *Review of Financial Studies 26*(10), 2548–2582.

Garriga, C., F. E. Kydland, and R. Sustek (2013). Mortgages and monetary policy. Technical report, National Bureau of Economic Research.

Gerardi, K., K. F. Herkenhoff, L. E. Ohanian, and P. S. Willen (2013). Unemployment, negative equity, and strategic default.

Gross, D. B. and N. S. Souleles (2002). Do Liquidity Constraints and Interest Rates Matter for Consumer Behavior? Evidence from Credit Card Data. *The Quarterly Journal of Economics 117*(1), 149–185.

Guerrieri, V. and G. Lorenzoni (2011). Credit crises, precautionary savings, and the liquidity trap. Technical report, National Bureau of Economic Research.

Hall, R. (2013). Routes into and out of the zero lower bound. In *2013 Federal Reserve Bank of Kansas City Economic Symposium at Jackson Hole*.

Hsieh, C.-T. (2003). Do consumers react to anticipated income changes? Evidence from the Alaska permanent fund. *American Economic Review*, 397–405.

Hurst, E. and F. Stafford (2004). Home is where the equity is: mortgage refinancing and household consumption. *Journal of Money, Credit and Banking*, 985–1014.

Jappelli, T. (1990). Who is credit constrained in the US economy? *The Quarterly Journal of Economics*, 219–234.

Jappelli, T. and M. Padula (2014). The Consumption and Wealth Effects of an Unanticipated Change in Lifetime Resources. Technical report.

Jappelli, T. and M. Pagano (1989). Consumption and capital market imperfections: An international comparison. *The American Economic Review*, 1088–1105.

Jappelli, T., J.-S. Pischke, and N. S. Souleles (1998). Testing for liquidity constraints in Euler equations with complementary data sources. *Review of Economics and statistics 80*(2), 251–262.

Jappelli, T. and L. Pistaferri (2010). The Consumption Response to Income Changes. *Annu. Rev. Econ. 2*(1), 479–506.

Jappelli, T. and L. Pistaferri (2014). Fiscal policy and MPC heterogeneity. *American Economic Journal: Macroeconomics Forthcoming*.

Jimenez, G., S. Ongena, and J.-L. Peydro (2012). Credit supply and monetary policy: Identifying the bank balance-sheet channel with loan applications. *The American Economic Review 102*(5), 2301–2326.

Jimenez, G., S. Ongena, J.-L. Peydro, and J. Saurina (2014). Hazardous Times for Monetary Policy: What Do Twenty-Three Million Bank Loans Say About the Effects of Monetary Policy on Credit Risk-Taking? *Econometrica 82*(2), 463–505.

Johnson, D. S., J. A. Parker, and N. S. Souleles (2009). The response of consumer spending to rebates during an expansion: evidence from the 2003 child tax credit.

Kaplan, G. and G. L. Violante (2011). A model of the consumption response to fiscal stimulus payments. Technical report, National Bureau of Economic Research.

Kashyap, A. K. and J. C. Stein (2000). What do a million observations on banks say about the transmission of monetary policy? *American Economic Review*, 407–428.

Keys, B. J., T. Piskorski, A. Seru, and V. Yao (October 2014). Mortgage Rates, Household Balance Sheets, and the Real Economy. Technical report, National Bureau of Economic Research.

Keys, B. J., D. G. Pope, and J. C. Pope (2014). Failure to Refinance. Technical report, National Bureau of Economic Research.

Krainer, J. and E. Laderman (2014). Mortgage loan securitization and relative loan performance. *Journal of Financial Services Research 45*(1), 39–66.

Krishnamurthy, A. and A. Vissing-Jorgensen (2011). The Effects of Quantitative Easing on Interest Rates: Channels and Implications for Policy. *Brookings Papers on Economic Activity*, 215.

Krishnamurthy, A. and A. Vissing-Jorgensen (2012). The aggregate demand for treasury debt. *Journal of Political Economy 120*(2), 233–267.

Krueger, D. and F. Perri (2006). Does income inequality lead to consumption inequality? Evidence and theory. *The Review of Economic Studies 73*(1), 163–193.

Landier, A., D. Sraer, and D. Thesmar (2011). The risk-shifting hypothesis: Evidence from subprime originations. *TSE Working Paper 11*.

Landier, A., D. Sraer, and D. Thesmar (2013). Banks' Exposure to Interest Rate Risk and The Transmission of Monetary Policy. Technical report, National Bureau of Economic Research.

Lucas, D., D. Moore, and M. Remy (2011). An Evaluation of Large-Scale Mortgage Refinancing Programs: Working Paper 2011-04. Technical report.

Maddaloni, A. and J.-L. Peydró (2011). Bank risk-taking, securitization, supervision, and low interest rates: Evidence from the Euro-area and the US lending standards. *Review of Financial Studies 24*(6), 2121–2165.

Mian, A., K. Rao, and A. Sufi (2013). Household Balance Sheets, Consumption, and the Economic Slump*. *The Quarterly Journal of Economics 128*(4), 1687–1726.

Mian, A. and A. Sufi (2012a). The Effects of Fiscal Stimulus: Evidence from the 2009 Cash for Clunkers Program*. *The Quarterly journal of economics 127*(3), 1107–1142.

Mian, A. R. and A. Sufi (2012b). What Explains High Unemployment? The Aggregate Demand Channel. Technical report, National Bureau of Economic Research.

Mian, A. R. and A. Sufi (2014). House Price Gains and U.S. Household Spending from 2002 to 2006. *Fama-Miller Working Paper*, Available at SSRN: http://ssrn.com/abstract=2412263.

Nakamura, E. and J. Steinsson (2014). Fiscal Stimulus in a Monetary Union: Evidence from US Regions. *The American Economic Review 104*(3), 753–792.

Parker, J. A. (1999). The reaction of household consumption to predictable changes in social security taxes. *American Economic Review*, 959–973.

Parker, J. A., N. S. Souleles, D. S. Johnson, and R. McClelland (2013). Consumer Spending and the Economic Stimulus Payments of 2008. *The American Economic Review 103*(6), 2530–2553.

Piskorski, T. and A. Tchistyi (2010). Optimal mortgage design. *Review of Financial Studies 23*(8), 3098–3140.

Reis, R. (2006). Inattentive consumers. *Journal of monetary Economics 53*(8), 1761–1800.

Romer, C. D. and D. H. Romer (2013). The most dangerous idea in federal reserve history: Monetary policy doesn't matter. *The American Economic Review 103*(3), 55–60.

Scharfstein, D. S. and A. Sunderam (2013). Concentration in Mortgage Lending, Refinancing Activity and Mortgage Rates. Technical report, National Bureau of Economic Research.

Shapiro, M. D. and J. Slemrod (2003a). Consumer Response to Tax Rebates. *American Economic Review*, 381–396.

Shapiro, M. D. and J. Slemrod (2003b). Did the 2001 tax rebate stimulate spending? Evidence from taxpayer surveys. In *Tax Policy and the Economy, Volume 17*, pp. 83–110. MIT Press.

Shapiro, M. D. and J. Slemrod (2009). Did the 2008 Tax Rebates Stimulate Spending? *American Economic Review 99*(2), 374–79.

Shin, H. S. (2013). Commentary on Robert E. Hall'The routes into and out of the Zero Lower Bound'. In *Federal Reserve Bank of Kansas City Proceedings*.

Sims, C. (2003). Implications of rational inattention. *Journal of Monetary Economics 50*(3), 665–690.

Souleles, N. S. (2002). Consumer response to the Reagan tax cuts. *Journal of Public Economics 85*(1), 99–120.

Souleles, N. S., J. A. Parker, and D. S. Johnson (2006). Household Expenditure and the Income Tax Rebates of 2001. *American Economic Review 96*(5), 1589–1610.

Stein, J. C. (2012). Monetary Policy as Financial Stability Regulation. *The Quarterly Journal of Economics 127*(1), 57–95.

Stephens Jr, M. (2008). The consumption response to predictable changes in discretionary income: Evidence from the repayment of vehicle loans. *The Review of Economics and Statistics 90*(2), 241–252.

Sterk, V. and S. Tenreyro (2014). The Transmission of Monetary Policy Operations through Redistributions and Durable Purchases.

Swanson, E. T. and J. C. Williams (2012). Measuring the effect of the zero lower bound on medium-and longer-term interest rates. *Federal Reserve Bank of San Francisco Working Paper 2*, 2013.

Williamson, S. D. (2012). Liquidity, monetary policy, and the financial crisis: A new monetarist approach. *The American Economic Review 102*(6), 2570–2605.

Zeldes, S. P. (1989a). Consumption and liquidity constraints: an empirical investigation. *The Journal of Political Economy*, 305–346.

Zeldes, S. P. (1989b). Optimal consumption with stochastic income: Deviations from certainty equivalence. *The Quarterly Journal of Economics*, 275–298.

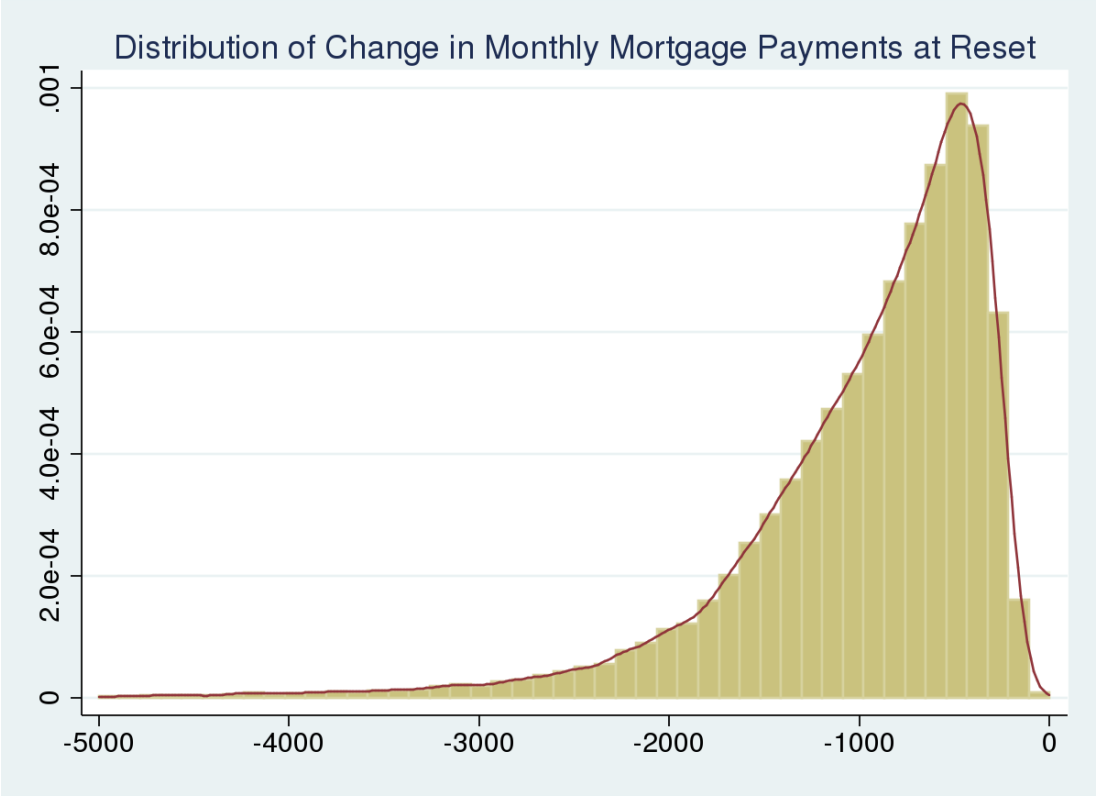

Figure 1 –Change in the Monthly Payment

This figure shows the average change in the monthly payment at the time of the interest rate adjustment, for our sample of hybrid ARMs with an interest-only period of 10 years and a reset date 60 months after origination.

.

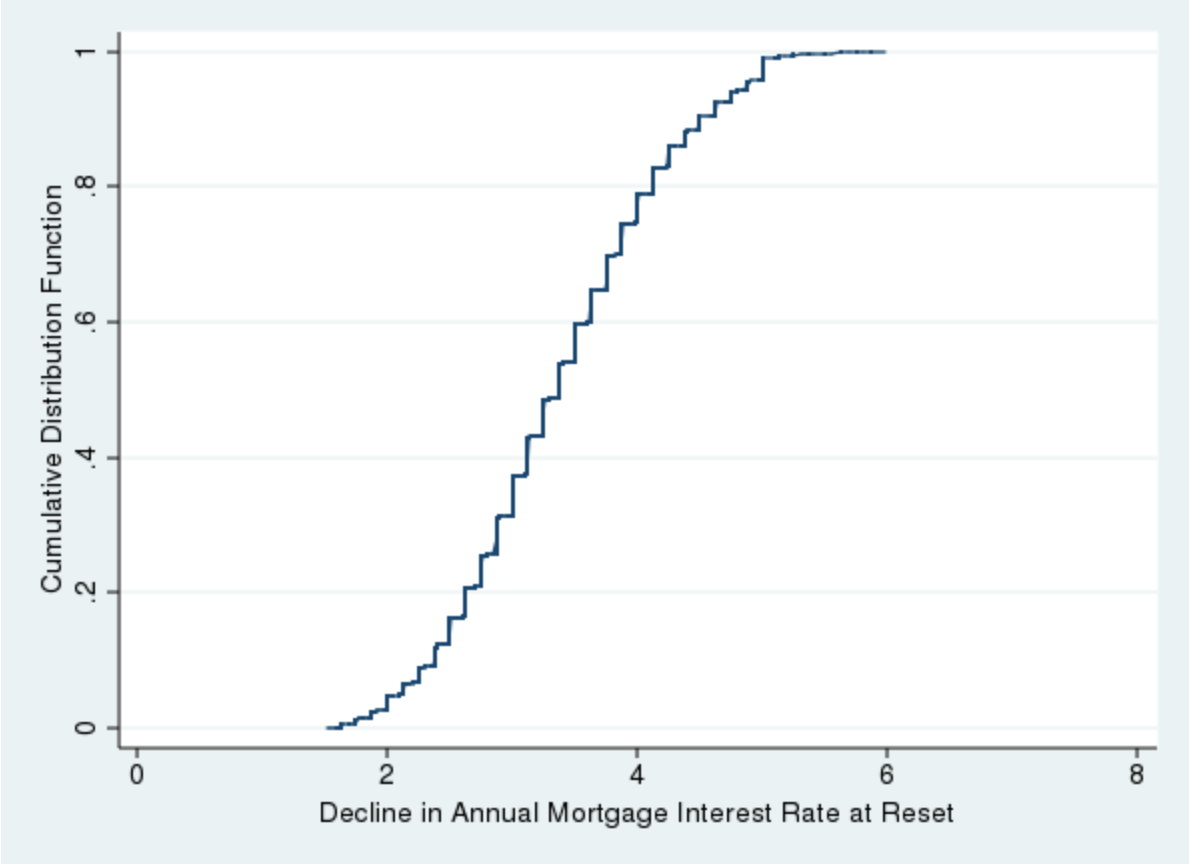

Figure 2 – Change in the Mortgage Interest Rate

This figure shows the cumulative distribution for the change in the mortgage interest rate between origination and the date of the adjustment, for our sample of 5-year ARMs with an interest-only period of 10 years and a reset date 60 months after origination. On average the interest rate declines by 3.3%.

.

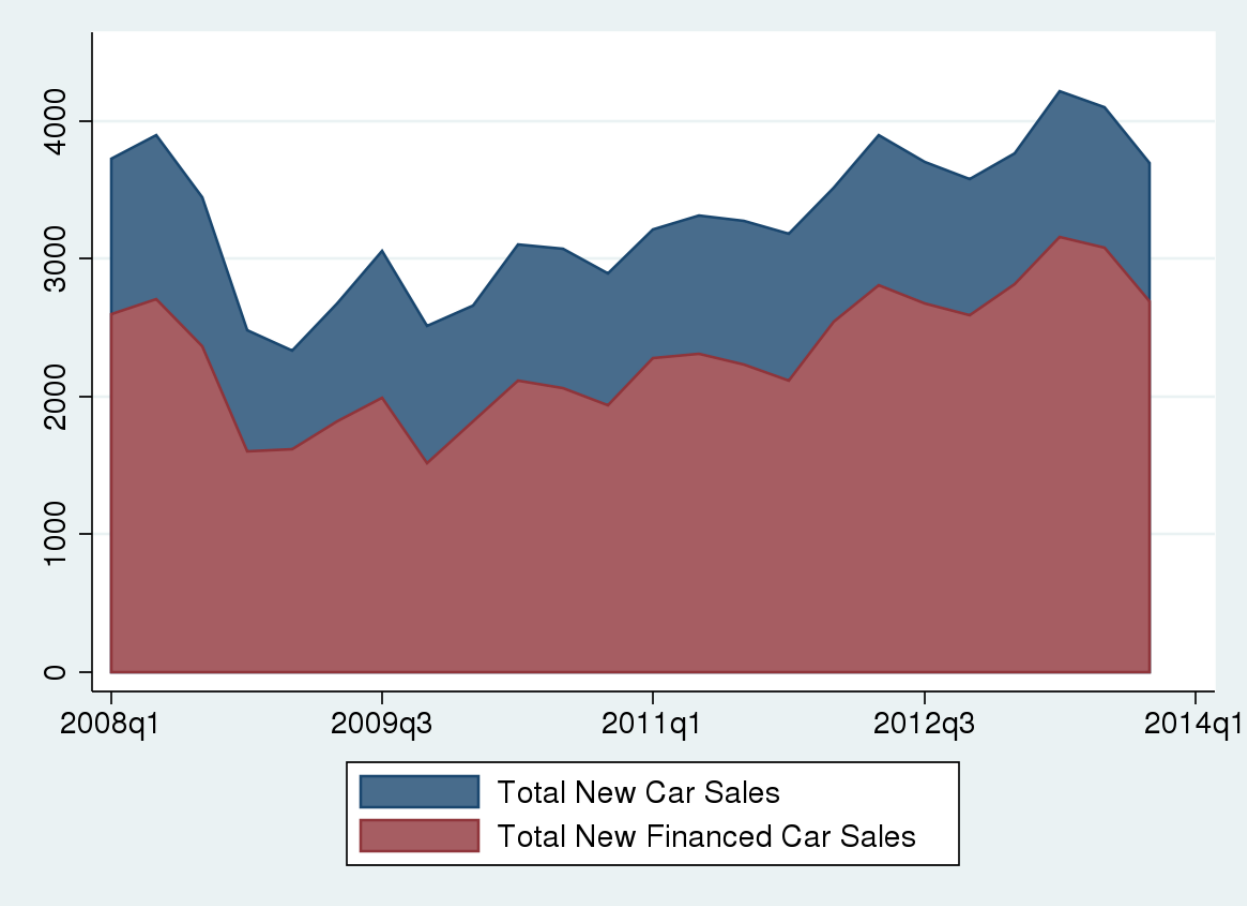

Figure 3 –Total Auto Sales

This figure shows the total new car sales financed by auto loans as a fraction of the total new car sales as provided by Polk.

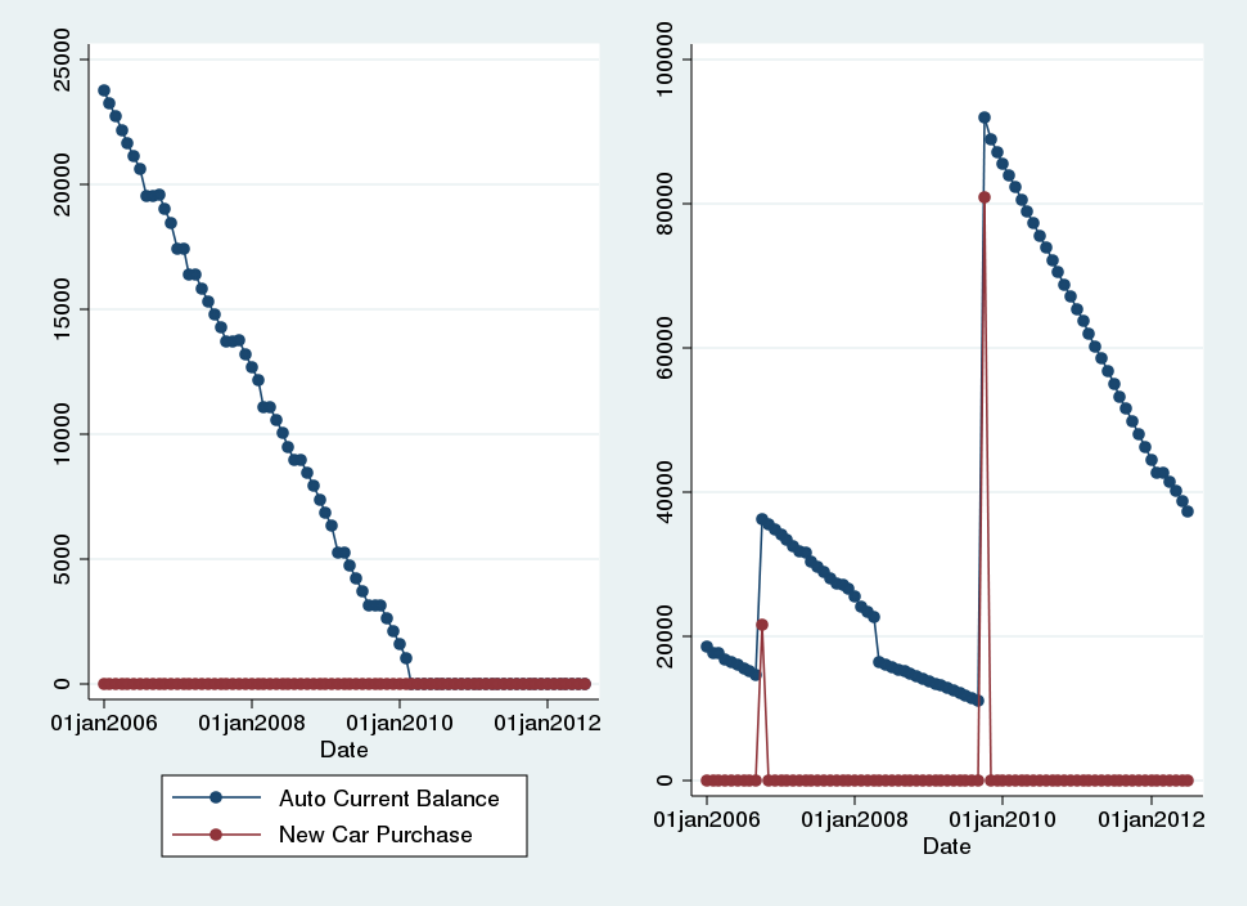

Figure 4 –Households Auto Loan Balances and Construction of Auto Sales Measure

The left panel is an example of individual who purchased her car before January 2006 and did not purchase any car until July 2012. The household in the right panel purchased two cars during the period. We assumed the value of the new car to be equal to the change in the auto loan balance at the time of purchase.

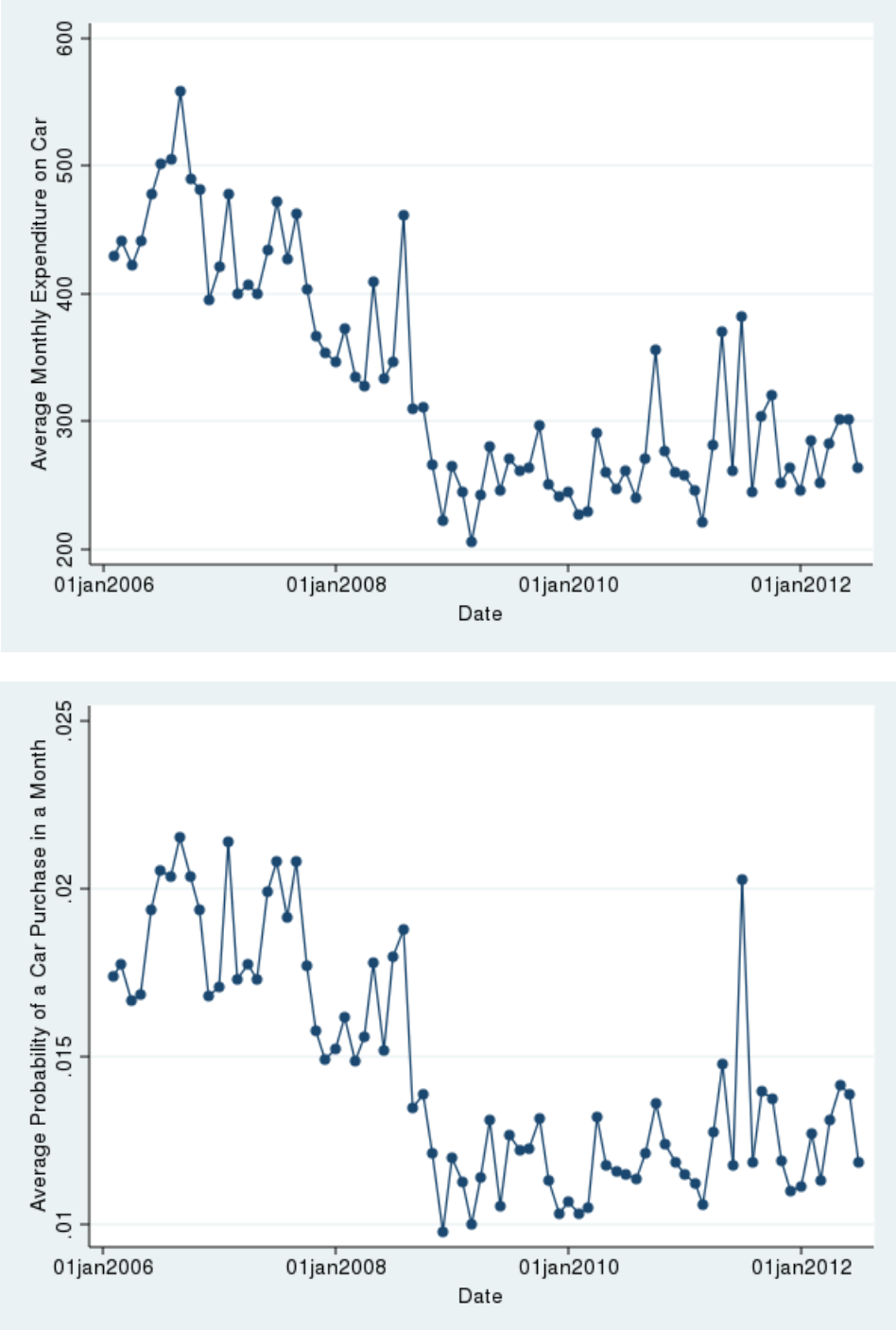

Figure 5 – Car Purchases of Households with 5 ARMs over Time

The top panel shows the average monthly car expenditure from January 2006 to July 2012 for those households who had a 5-year ARM mortgage originated between 2005 and 2007. The bottom panel shows the fraction of these households who purchased a car in each single month.

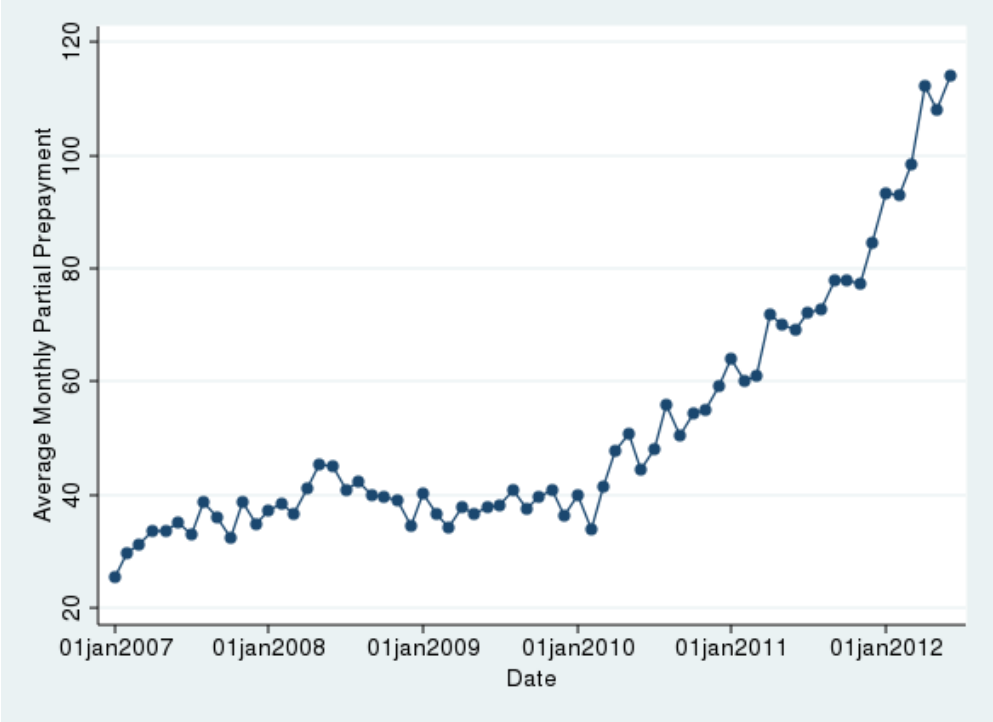

Figure 6 – Mortgage Partial Prepayment

This figure shows the average monthly prepayment of the mortgage for borrowers holding 5-year ARMs originated during the 2005-2007 period.

.

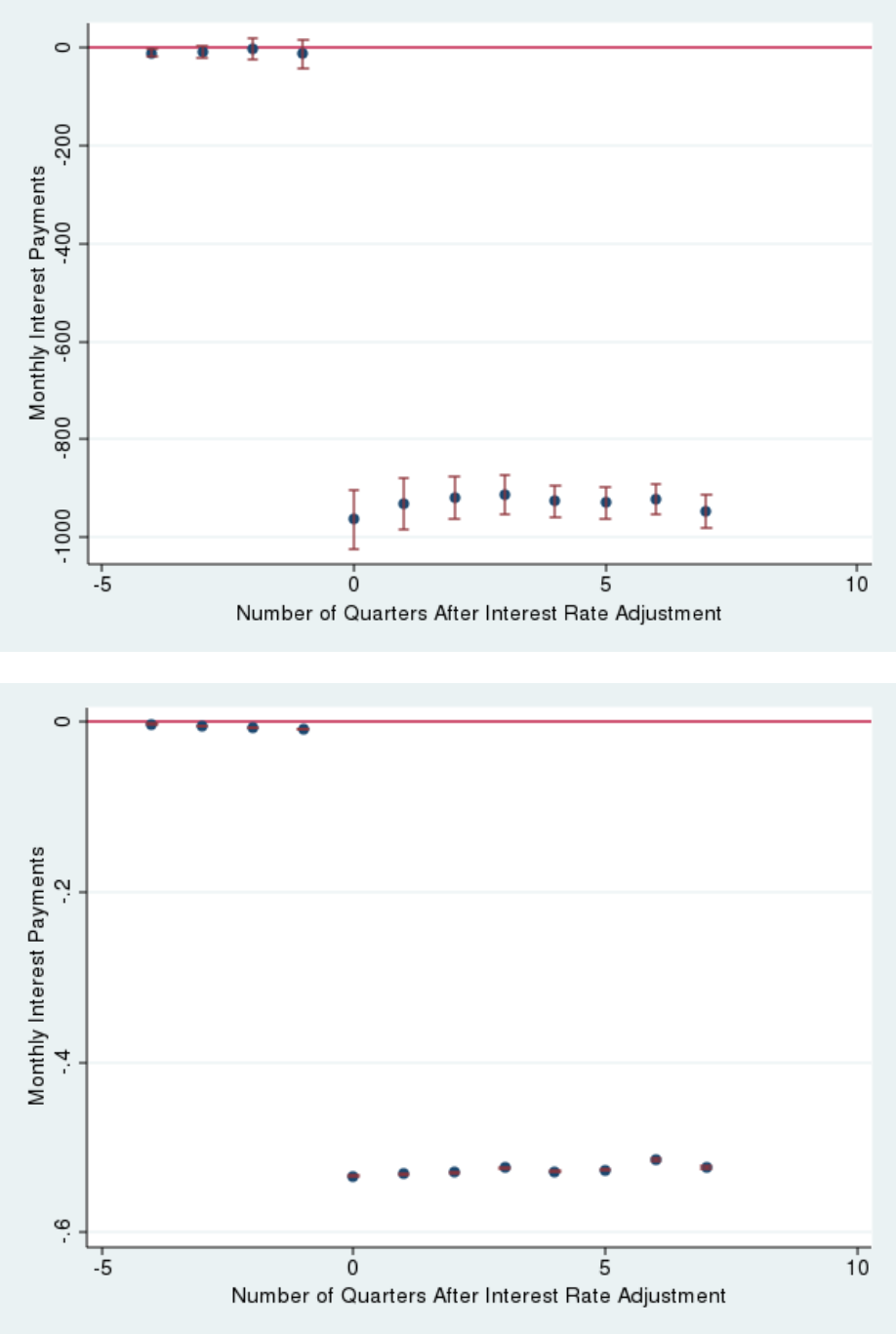

Figure 7 – Reduction in Monthly Payments

Interest rate adjustment and the monthly interest payments for 5-year ARMs originated during the 2005-2007 period. Top panel shows the change in dollars, while the bottom panel normalized it by the size of the monthly payment of the mortgage at the origination.

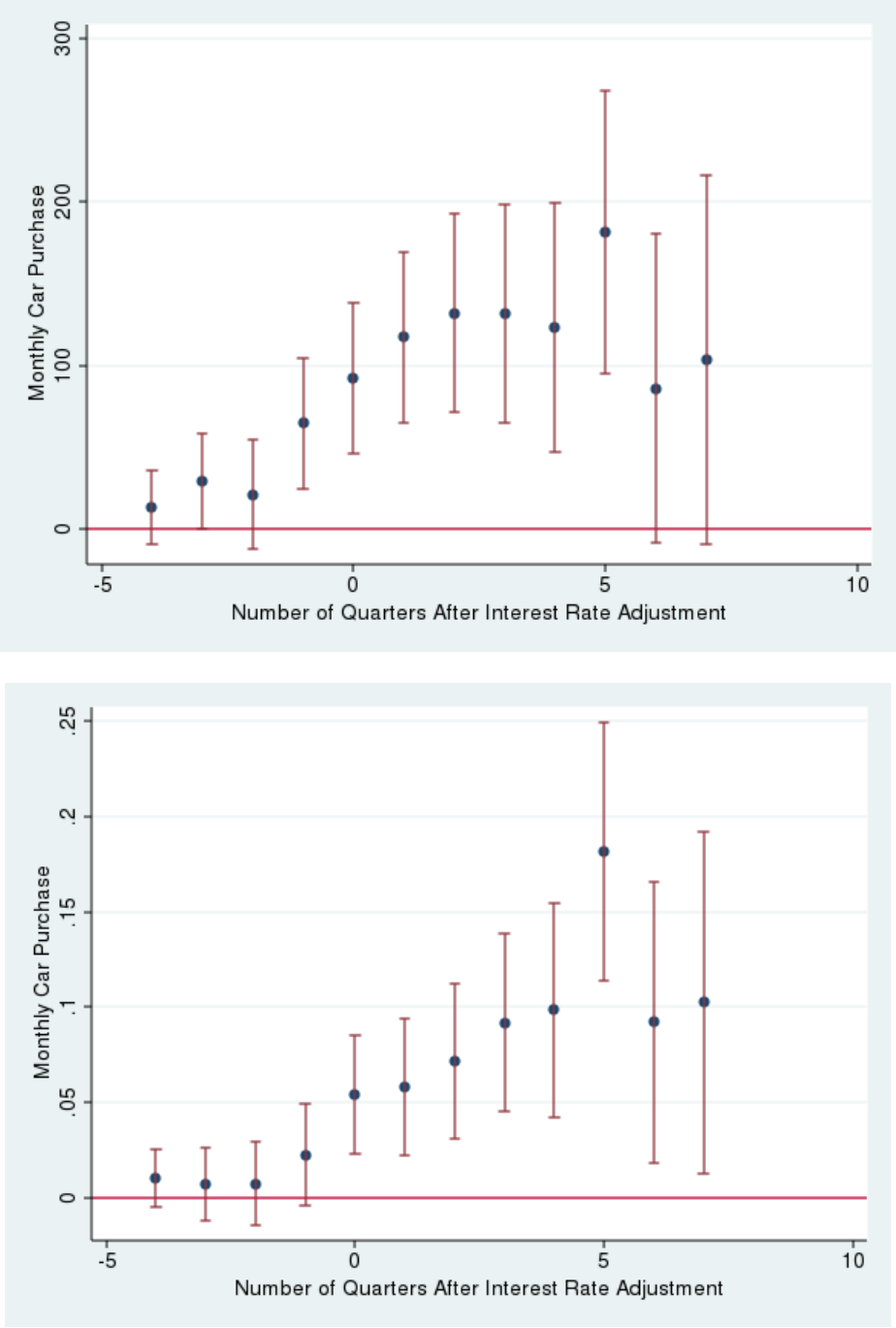

Figure 8 – Car Purchase

Monthly car purchase and interest rate adjustment for 5-year ARMs originated during the 2005-2007 period. Top panel shows the change in dollars, while the bottom panel normalized it by the size of the monthly payment of the mortgage at the origination.

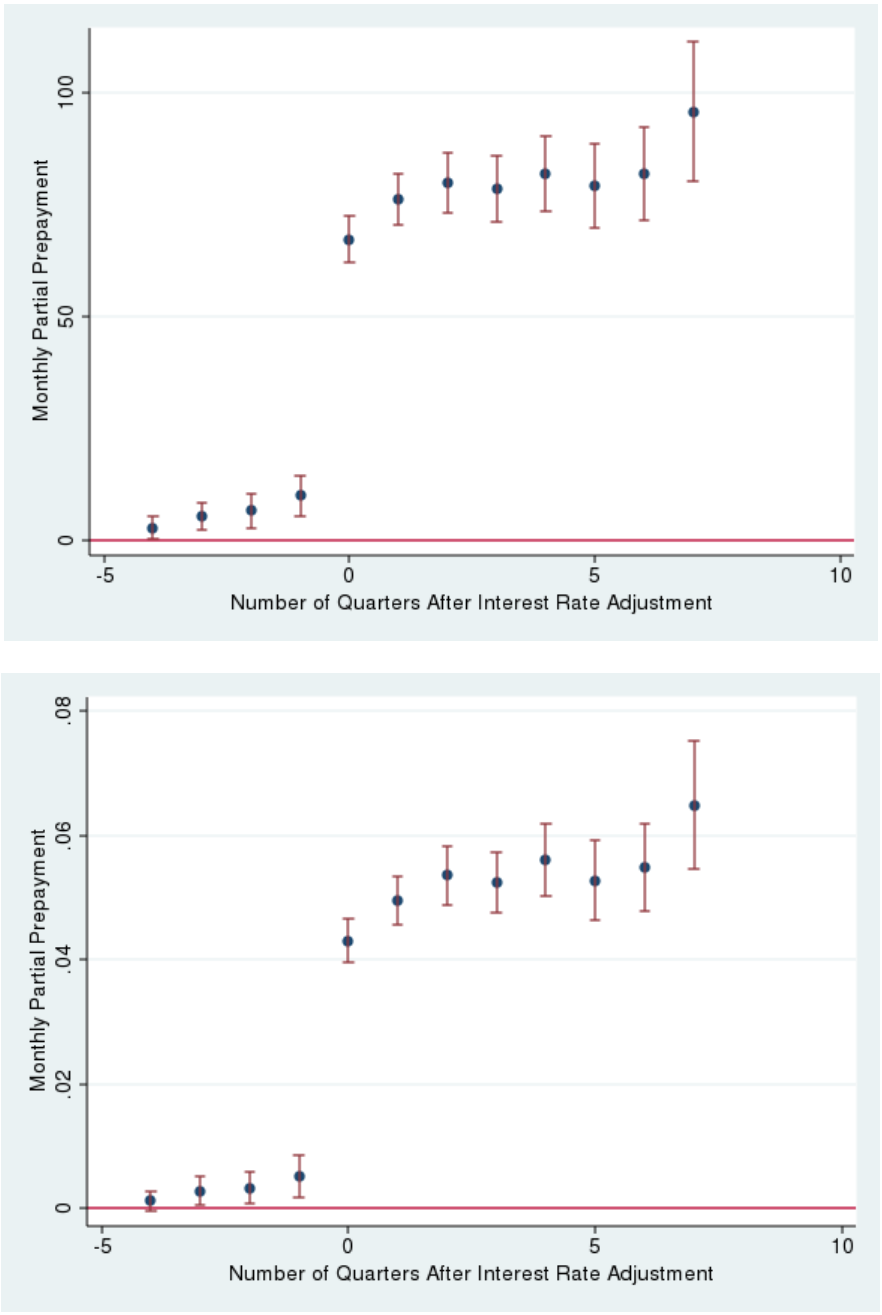

Figure 9 – Deleveraging

Monthly voluntary partial principal repayment and interest rate adjustment for 5-year ARMs originated during the 2005-2007 period. Top panel shows the change in dollars, while the bottom panel normalized it by the size of the monthly payment of the mortgage at the origination.

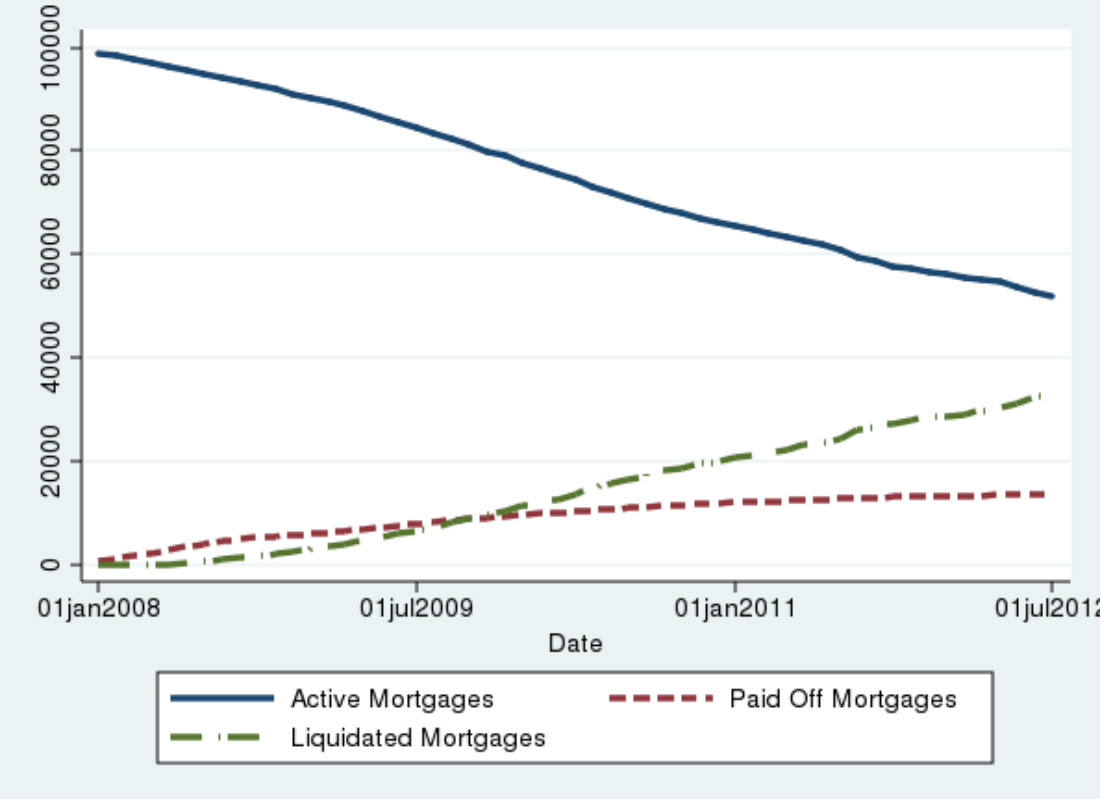

Figure 10 – Attrition

This plot shows the number of active loans (blue solid line), liquidated loans due to foreclosure, bankruptcy or real estate owned (green dash line) and paid off mortgages due to prepayment or refinancing (dash-dot line) over time.

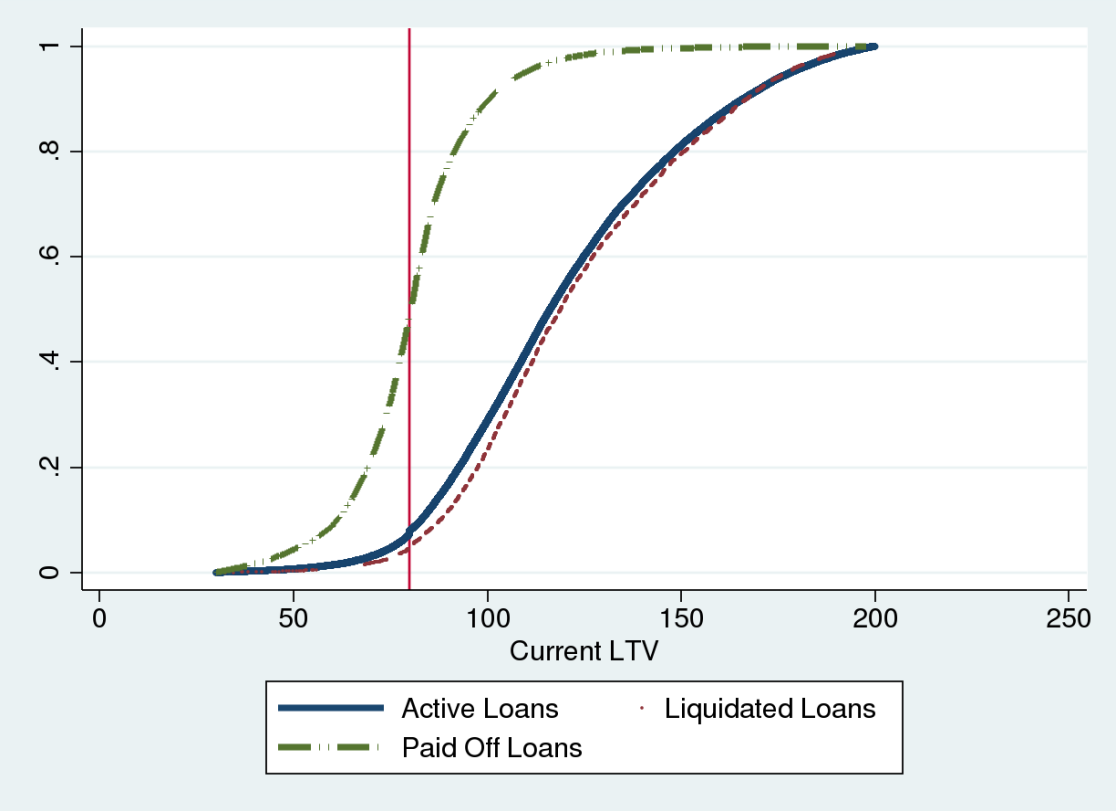

Figure 11 – Attrition and Current LTV

This plot shows the cumulative distribution of the number of active loans, liquidated loans due to foreclosure, bankruptcy or real estate owned and paid off mortgages due to prepayment or refinancing as a function of the current loan-to-value ratio. The vertical line shows a current LTV of 80%, which corresponds to the median of the current LTV for the paid off loans.

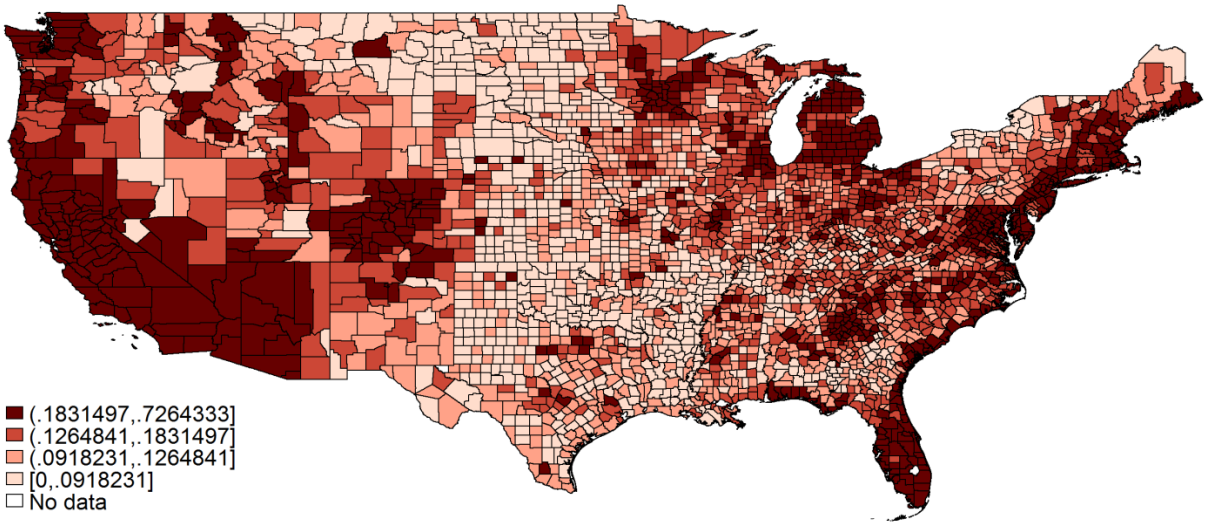

Figure 12 – Fraction of Adjustable-Rate Mortgages in 2006

This figure plots the fraction of ARMs originated in each county in 2006 using data from LPS.

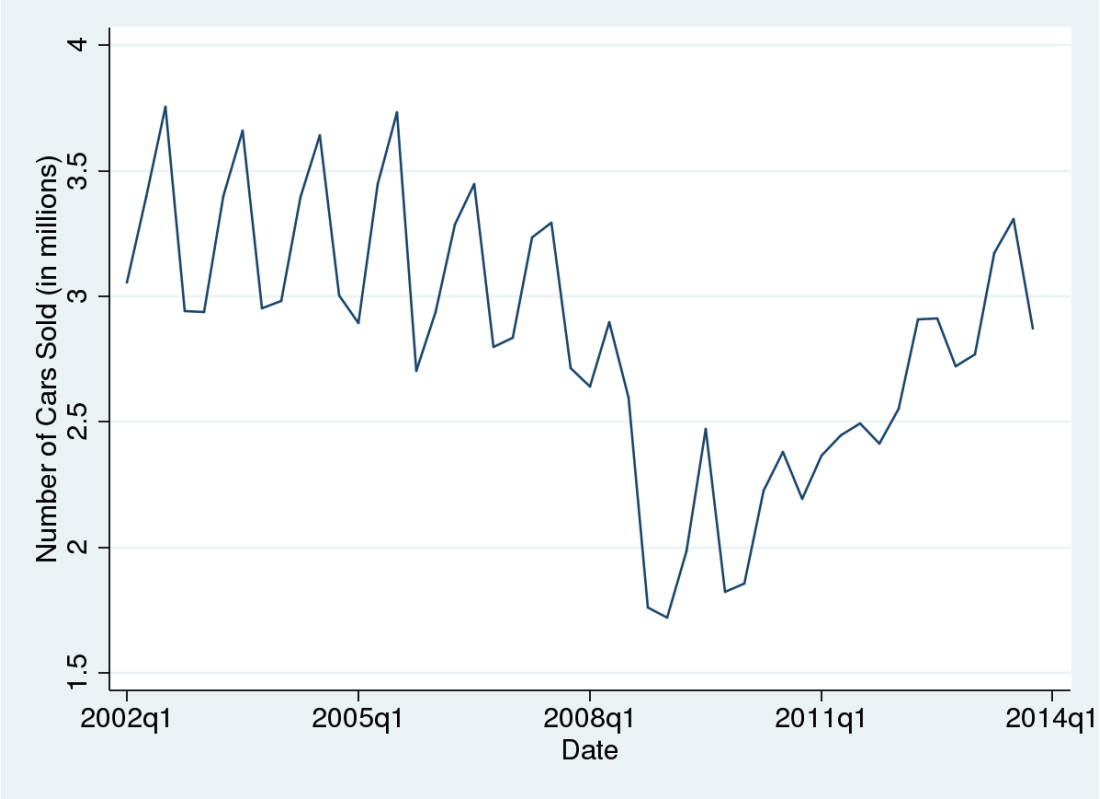

Figure 13 – Car Sales

The figure plots the number (in millions) of cars sold in the U.S. every quarter for the period 2002-2014.

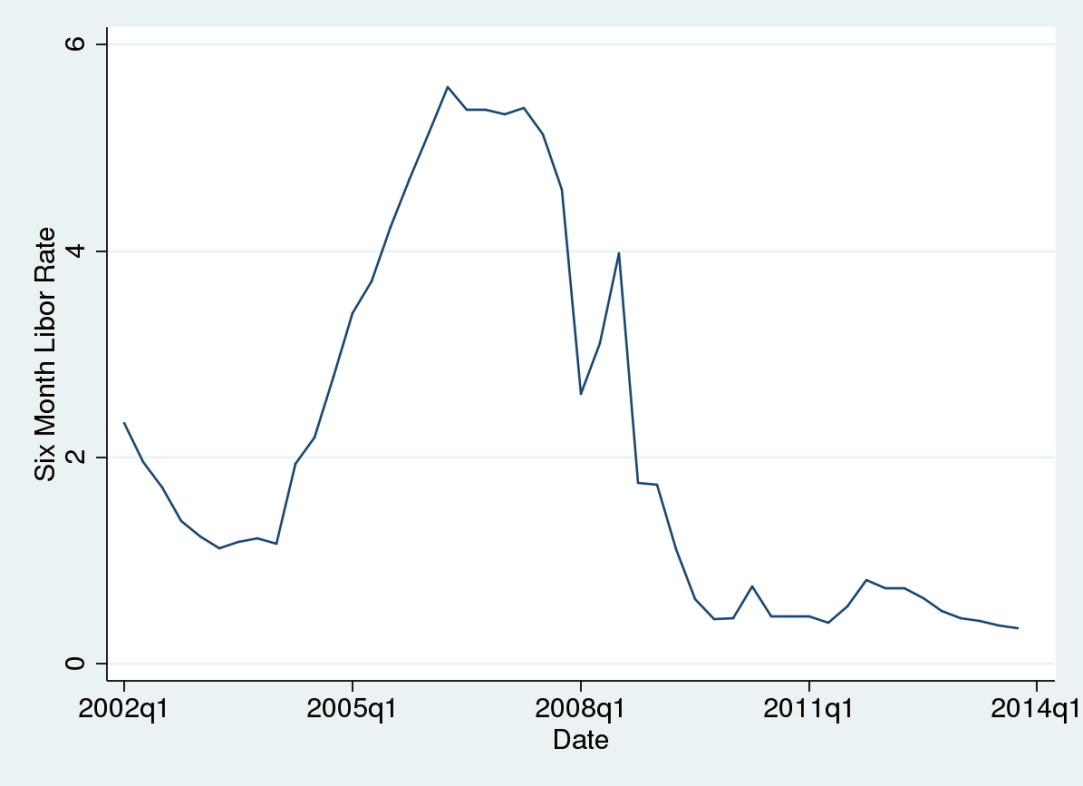

Figure 14 -- LIBOR

The figure plots the 6-month LIBOR rate, which serves as index for the majority of ARMs in our sample.

Table 1.A
Summary Statistics

The table reports descriptive statistics for the main variables employed in our analysis. In the top panel, we present the main mortgage characteristics at origination, as provided by BlackBox, for adjustable rate mortgages (ARMs) originated between 2005 and 2007 with an interest rate adjustment after five and after ten years of origination and with interest only period of ten years. The bottom panel presents county-level characteristics, computed aggregating loan-level data from LPS and household level data from NY Fed Consumer Credit Panel, the Census as well as car sales data provided by the Polk.

Panel A. Individual Level Characteristics

5-ARM Characteristics at Origination:

	N	Mean	St. Dev.	p1	p10	p50	p90	p99
FICO	46,578	736.2	348.5	661	673	719	781	809
Balance	46,578	357,949	271,600	79,200	132,000	288,000	630,000	1,393,000
Loan-to-Value Ratio	46,397	77.11	10.01	40.98	65	80	80	100
Interest Rate	46,497	6.449	0.765	5	5.500	6.375	7.500	8.375
Average Monthly Payment	45,424	1,921	1,471	427.5	697.8	1,547	3,392	7,309
Interest Rate After Adjustment	45,156	3.096	0.480	2.375	2.625	3	3.625	5.250
Monthly Payment After Adjustment	44,941	915.8	721.9	129.8	314.8	725.3	1,669	3,561

Data on Borrowers holding 5-ARM

	N	Mean	St. Dev.	p1	p10	p50	p90	p99
Monthly Expenditure on New Car	2,894,000	305.1	3,161	0	0	0	0	13,507
Fraction of Households Who Purchased a Car Each Month	2,894,000	0.0137	0.116	0	0	0	0	1
Partial Prepayment	2,627,000	52.21	400.1	0	0	59.61	210.8	1,004
Retail Sale	1,158,000	56.70	442.2	0	0	0	0	1,588

10-ARM Characteristics at Origination:

	N	Mean	St. Dev.	p1	p10	p50	p90	p99
FICO	26,543	793.7	736.3	661	681	737	790	815
Balance	26,538	536,342	347,622	89,600	186,000	486,280	928,000	1,866,000
Loan-to-Value Ratio	26,518	72.82	12.05	30.61	55.90	79.40	80	95
Interest Rate	24,348	6.149	0.525	5	5.500	6.125	6.800	7.625
Monthly Payment	23,765	2,700	1,819	488.0	936.5	2,430	4,623	9,465

Data on Borrowers holding 10-ARM

	N	Mean	St. Dev.	p50	p75	p90	p95	p99
Monthly Expenditure on New Car	1,702,000	364.4	3,711	0	0	0	0	16,000
Fraction of Households Who Purchased a Car Each Month	1,703,000	0.0148	0.121	0	0	0	0	1
Partial Prepayment	1,668,000	88.49	619.6	0	0	128.2	390.1	1,535
Retail Sale	616,705	68.84	522.1	0	0	0	0	1,816

Panel B: County Level Characteristics:

As of 2006

	N	Mean	St. Dev	p1	p10	p50	p90	p99
Fraction of ARM debt in 2006	867	0.207	0.110	0.0659	0.102	0.174	0.350	0.585
Log(Median Income)	867	10.82	0.239	10.32	10.55	10.79	11.16	11.45
Poverty Rate	867	13.52	5.327	4.100	7.200	13.10	19.90	30.60
Debt-to-Income Ratio	867	1.755	0.581	0.876	1.155	1.620	2.569	3.626
Change in Securitization Rate (2002-2006)	866	0.182	0.0920	0.0388	0.0827	0.166	0.306	0.467
Log(Population)	867	11.79	0.912	10.56	10.81	11.56	13.17	14.28

Quarterly Observations (2007-2013)

	N	Mean	St. Dev	p1	p10	p50	p90	p99
Interest	24,232	5.687	0.615	4.322	4.795	5.766	6.417	6.704
Log(Average Monthly Mortgage Payment)	24,232	6.956	0.305	6.401	6.606	6.911	7.368	7.802
Log(Quarterly Car Sales)	24,008	7.214	1.022	5.472	6.047	7.044	8.643	10.03
Log(Mortgage)	24,232	22.18	1.212	19.99	20.76	22.02	23.91	25.26
Log(Credit Card)	24,232	18.33	0.960	16.44	17.19	18.21	19.66	20.78

Table 1.B
External Validity

The table reports descriptive statisticsfor the main variables employed in our analysis, but for different types of mortgages as provided by Lender Processing Services, which covers about 64% of the origination count reported under the Home Mortgage Disclosure Act (HMDA) over the period 2005–07. We first report the statistics for the whole sample at origination, and then we focus on different subsamples comprising of fixed-rate mortgages, adjustable rate mortgages (ARMs) and 5-year ARMs. We only consider mortgages for owner occupied houses.

	N	Mean	St. Dev.
Mortgages Originated between 2005 and 2008			
FICO	15,520,963	703.76	68.55
Interest Rate	19,104,660	6.27	1.23
Loan-to-Value Ratio	18,452,315	74.53	17.51
Mortgage Size	19,106,272	239043.24	202721.63
Average Monthly Payment	17,300,637	1654.32	1514.99
Fixed-Rate Mortgages Originated between 2005 and 2007			
FICO	10,754,081	705.16	68.68
Interest Rate at Origination	13,263,190	6.30	0.89
Loan-to-Value Ratio	12,729,960	74.23	19.05
Mortgage Size	13,264,696	196125.18	139312.44
Initial Monthly Payment	11,812,181	1485.49	1258.87
Adjustable-Rate Mortgages Originated between 2005 and 2007			
FICO	2,039,025	687.97	73.22
Interest Rate at Origination	2,521,322	6.06	2.35
Loan-to-Value Ratio	2,441,813	76.06	13.77
Mortgage Size	2,521,297	312466.01	271243.03
Initial Monthly Payment	2,426,317	1765.34	1770.98
5-ARMs Originated between 2005 and 2007			
FICO	308,927	720.97	51.96
Interest Rate at Origination	341,728	5.92	0.71
Loan-to-Value Ratio	340,398	73.99	13.61
Mortgage Size	341,728	349099.78	287061.08
Initial Monthly Payment	334,572	2077.86	1831.93

Table 2
Monthly Payment and Interest Rate Reset

The table reports coefficient estimates of least square regressions relating the monthly payment of 5-year adjustable rate mortgages with a 10-year interest only period to the reset of interest rate 5 years after the origination. The dependent variable is the mortgage monthly payment for mortgages originated between 2005 and 2007 and is based upon data from BlackBox Logic. The main independent variables are dummies identifying different time periods before and after the reset date. Column (6) normalizes the monthly payment by the size of the monthly payment of the mortgage at the origination. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6
	Monthly Interest Payment					Normalized by Payment Size at Origination
Four Quarters Before	-11.27**	-5.429	-10.58**	-11.77***	-10.81**	-0.00265***
	(5.363)	(4.184)	(4.692)	(3.000)	(4.903)	(0.000683)
Three Quarters Before	-0.392	4.967	-4.113	-21.11***	-7.803	-0.00474***
	(7.425)	(6.222)	(6.745)	(4.614)	(7.659)	(0.000923)
Two Quarters Before	18.54*	20.48**	7.885	-25.73***	-0.651	-0.00690***
	(11.11)	(10.18)	(10.84)	(5.267)	(12.93)	(0.00103)
One Quarter Before	45.49***	28.59*	9.389	-23.22***	-12.30	-0.00897***
	(15.90)	(14.77)	(15.86)	(6.940)	(18.06)	(0.00161)
One Quarter After	-940.4***	-889.5***	-927.4***	-926.2***	-963.2***	-0.534***
	(34.33)	(34.42)	(34.43)	(43.03)	(36.47)	(0.00607)
Two Quarters After	-885.4***	-821.7***	-873.7***	-854.9***	-932.0***	-0.531***
	(30.23)	(25.92)	(26.21)	(33.57)	(31.28)	(0.00574)
Three Quarters After	-815.3***	-771.0***	-832.1***	-801.8***	-918.8***	-0.529***
	(23.57)	(18.62)	(19.15)	(32.67)	(26.46)	(0.00724)
Four Quarters After	-754.5***	-730.8***	-799.4***	-761.3***	-912.8***	-0.525***
	(19.11)	(14.86)	(16.88)	(33.58)	(23.77)	(0.00681)
Two Years After	-612.5***	-659.3***	-746.5***	-726.3***	-927.1***	-0.528***
	(30.83)	(17.14)	(17.85)	(33.99)	(18.54)	(0.00744)
FICO Score		-0.0182***	-0.0274***	-0.0200***	-0.0541***	-3.41e-05***
		(0.00641)	(0.00502)	(0.00588)	(0.00493)	(2.49e-06)
Log(House Prices)		-0.135***	-0.416***	-0.145***	-0.0476***	-5.82e-05***
		(0.0401)	(0.0648)	(0.0407)	(0.0112)	(3.91e-06)
Time Fixed Effects	Yes	Yes				
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
County X Time Fixed Effect			Yes			
Origination Cohort X Time Fixed Effect				Yes		Yes
Origination X Time Fixed Effect					Yes	
Observations	2,853,421	2,256,015	2,154,533	2,256,015	2,223,212	2,223,212
R-squared	0.962	0.975	0.633	0.976	0.981	0.781

Table 3
Car Purchases and Interest Rate Reset

The table reports coefficient estimates of least square regressions relating the monthly car purchases to the reset of interest rate 5 years after the origination. The dependent variable in columns (1)-(5) is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan. In Column (6) the dependent variable is the probability to purchase a car and we report the coefficients in percentage points, while in Column (7) we have normalized the car expenditures by the size of the monthly payment of the mortgage at the origination.. The main independent variables are dummies identifying different time periods before and after the reset date. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Monthly Payment at Origination identifies the quartiles of the monthly payment. In Column (3)-(5) we allow in turn for different trends for each county, for each origination cohort, and for different monthly payments at origination. The sample includes mortgages originated between 2005 and 2007 provided by BlackBox Logic. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6	7
	Car Purchase					Likelihood to Purchase a Car in %	Normalized by Payment Size at Origination
Four Quarters Before	43.30***	29.80**	20.19*	19.66*	23.20*	0.116**	0.00918
	(11.26)	(11.22)	(11.51)	(11.66)	(12.64)	(0.000440)	(0.00808)
Three Quarters Before	68.31***	41.88***	29.44**	33.13**	31.68**	0.174***	0.00862
	(11.71)	(12.00)	(13.54)	(13.08)	(12.60)	(0.000530)	(0.00912)
Two Quarters Before	43.24***	21.28*	9.460	17.62	14.21	0.120**	0.00798
	(9.841)	(10.78)	(10.66)	(14.55)	(11.52)	(0.000591)	(0.00983)
One Quarter Before	77.64***	47.45***	43.25***	52.44***	38.25**	0.222***	0.0231
	(12.29)	(13.69)	(14.97)	(18.92)	(14.48)	(0.000822)	(0.0150)
One Quarter After	101.0***	76.59***	66.67***	92.82***	66.91***	0.348***	0.0531***
	(12.27)	(13.51)	(13.40)	(20.16)	(13.77)	(0.000721)	(0.0145)
Two Quarters After	119.1***	92.91***	87.44***	117.2***	80.84***	0.450***	0.0579***
	(14.38)	(16.71)	(17.35)	(25.05)	(16.92)	(0.000900)	(0.0190)
Three Quarters After	146.3***	111.0***	90.68***	144.6***	101.5***	0.454***	0.0733***
	(18.67)	(21.23)	(22.21)	(29.10)	(22.14)	(0.000895)	(0.0201)
Four Quarters After	166.6***	113.6***	91.57***	152.5***	96.28***	0.526***	0.0954***
	(17.51)	(18.06)	(18.10)	(31.53)	(19.75)	(0.00117)	(0.0253)
Two Years After	203.0***	137.1***	107.9***	166.1***	117.5***	0.572***	0.116***
	(18.09)	(17.82)	(20.16)	(34.75)	(19.63)	(0.00144)	(0.0277)
FICO Score		1.797***	1.793***	1.789***	1.784***	6.99e-05***	0.00112***
		(0.0641)	(0.0692)	(0.0644)	(0.0634)	(2.00e-06)	(3.58e-05)
Log House Prices		0.192	0.380	0.161	0.167	1.16e-06	-1.99e-05
		(0.143)	(0.227)	(0.143)	(0.136)	(5.52e-06)	(0.000108)
Time Fixed Effects	Yes	Yes					
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County X Time Fixed Effect			Yes				
Origination Cohort X Time Fixed Effect				Yes		Yes	Yes
Monthly Payment at Origination X Time Fixed Effect					Yes		
Observations	2,893,823	2,291,225	2,188,424	2,291,225	2,223,758	2,252,352	2,223,758
R-squared	0.027	0.031	0.019	0.031	0.031	0.001	0.026

Table 4
Voluntary Deleveraging and Interest Rate Reset

The table reports coefficient estimates of least square regressions relating the monthly mortgage prepayment to the reset of interest rate 5 years after the origination. The dependent variable in columns (1)-(5) is the monthly reduction in the mortgage balance and is computed based on data from BlackBox. In Column (6) we have normalized the partial prepayment of the mortgage by the size of the monthly payment of the mortgage at the origination. The main independent variables are dummies identifying different time periods before and after the reset date. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Monthly Payment at Origination identifies the quartiles of the monthly payment. In Column (3)-(5) we allow in turn for different trends for each county, for each origination cohort, and for different monthly payments at origination. The sample includes mortgages originated between 2005 and 2007 provided by BlackBox Logic. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6
	Partial Prepayment					Normalized by Payment Size at Origination
Four Quarters Before	1.033	1.457	1.736	2.658	1.441	0.00104
	(1.195)	(1.306)	(1.299)	(1.615)	(1.266)	(0.000900)
Three Quarters Before	1.935	2.030	2.981*	5.006***	3.042**	0.00268*
	(1.340)	(1.404)	(1.574)	(1.799)	(1.432)	(0.00154)
Two Quarters Before	1.961	1.400	2.103	6.234**	3.493*	0.00303**
	(1.487)	(1.664)	(1.742)	(2.410)	(1.829)	(0.00151)
One Quarter Before	2.094	3.021**	4.907***	9.403***	6.668***	0.00496**
	(1.396)	(1.363)	(1.524)	(2.502)	(1.410)	(0.00211)
One Quarter After	61.33***	57.36***	60.83***	66.36***	64.10***	0.0430***
	(2.849)	(2.366)	(2.514)	(3.602)	(2.152)	(0.00238)
Two Quarters After	65.35***	63.05***	67.92***	75.11***	72.80***	0.0493***
	(2.733)	(2.382)	(2.311)	(4.054)	(2.366)	(0.00300)
Three Quarters After	61.40***	64.59***	70.10***	78.59***	76.93***	0.0533***
	(2.717)	(2.763)	(2.728)	(5.045)	(2.833)	(0.00353)
Four Quarters After	57.89***	61.56***	68.93***	77.20***	77.28***	0.0525***
	(2.437)	(2.649)	(2.643)	(5.440)	(2.775)	(0.00355)
Two Years After	56.20***	58.75***	68.80***	79.42***	79.75***	0.0553***
	(2.744)	(3.341)	(3.151)	(6.361)	(2.886)	(0.00415)
FICO Score		0.131***	0.135***	0.132***	0.132***	7.87e-05***
		(0.00987)	(0.0105)	(0.00995)	(0.0103)	(6.37e-06)
Log House Prices		0.0688***	0.0186	0.0722***	0.0566***	3.75e-05***
		(0.0161)	(0.0252)	(0.0160)	(0.0149)	(1.10e-05)
Time Fixed Effects	Yes	Yes				
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
County X Time Fixed Effect			Yes			
Origination Cohort X Time Fixed Effect				Yes		Yes
Monthly Payment at Origination X Time Fixed Effect					Yes	
Observations	2,626,647	2,101,428	2,006,743	2,101,428	2,038,591	2,038,591
R-squared	0.229	0.238	0.022	0.238	0.241	0.154

Table 5
IV

The table reports coefficient estimates of instrumental variable regressions relating the car purchases and mortgage principal prepayment to the reset of interest rate 5 years after the origination. The sample includes 5-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. The dependent variable in odd columns is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan, and in even columns the dependent variable is the monthly partial prepayment and is computed based on data from BlackBox. We instrument the monthly payments with the time dummies identifying the months before and after the interest rate reset. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Robust standard errors, clustered at the household level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)	(6)
	Car Purchase	Prepayment	Car Purchase	Prepayment	Car Purchase	Prepayment
Monthly Payment$_{t+3}$			-0.0218	-0.00102	-0.0260*	-0.00355
			(0.0134)	(0.00220)	(0.0135)	(0.00225)
Monthly Payment$_{t}$	-0.0608***	-0.0678***	-0.0561***	-0.0683***	-0.0424***	-0.0636***
	(0.0148)	(0.00261)	(0.0155)	(0.00257)	(0.0163)	(0.00237)
Monthly Payment$_{t-3}$					-0.0476***	-0.0191***
					(0.0181)	(0.00302)
FICO Score	1.785***	0.133***	1.787***	0.133***	1.755***	0.130***
	(0.0490)	(0.00748)	(0.0492)	(0.00749)	(0.0494)	(0.00753)
Log(House Prices)	0.156	0.0671**	0.148	0.0650**	0.140	0.0612*
	(0.141)	(0.0299)	(0.141)	(0.0300)	(0.142)	(0.0314)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,255,729	2,069,119	2,250,514	2,065,275	2,196,713	2,019,881
R-squared	0.001	0.003	0.001	0.003	0.001	0.003

Table 6
Heterogeneous Effects: Income

The table reports coefficient estimates of least square regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate 5 years after the origination. The sample includes 5-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. The dependent variable in Columns (1) is the mortgage monthly payment, while in column (2) it is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan, and in column (3) the dependent variable is the monthly partial prepayment and is computed based on data from BlackBox. The main independent variables are dummies identifying different time periods before and after the reset date. "One Year Before" identifies the twelve months before up to one month before the interest rate adjustment. "One Year After" includes the month of the adjustment up to eleven months after. "Two Years After" includes twelve month after the adjustment up to twenty-three months after. "High Income" is a dummy equal to one if the household income, averaged over 2 years to 1 year before the adjustment, is greater than the median income in our sample. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Robust standard errors, clustered at the household level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)
	Interest Payment	Car Purchase	Prepayment
One Year Before	-0.00554***	0.0273**	6.86e-05
	(0.000523)	(0.0112)	(0.00135)
One Year After	-0.543***	0.0706***	0.0369***
	(0.000787)	(0.0169)	(0.00201)
Two Years After	-0.545***	0.137***	0.0435***
	(0.00121)	(0.0260)	(0.00305)
One Year Before X High Income	0.00358***	-0.0405***	0.00165
	(0.000581)	(0.0125)	(0.00151)
One Year After X High Income	0.0303***	-0.0529***	0.00967***
	(0.000835)	(0.0179)	(0.00216)
Two Years After X High Income	0.0307***	-0.124***	0.00183
	(0.00124)	(0.0266)	(0.00317)
FICO	-2.87e-05***	0.00111***	8.21e-05***
	(1.53e-06)	(3.28e-05)	(3.99e-06)
Log(House Prices)	-5.25e-05***	-5.68e-05	2.73e-05**
	(4.29e-06)	(9.22e-05)	(1.10e-05)
Households Fixed Effects	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes	Yes
High Income X Time Fixed Effects	Yes	Yes	Yes
Low Income X Time Fixed Effects	Yes	Yes	Yes
Observations	2,213,765	2,214,311	2,030,646
R-squared	0.781	0.026	0.154

Table 7
Heterogeneous Effects: Loan to Value Ratio

The table reports coefficient estimates of least square regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate 5 years after the origination. The sample includes 5-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. The dependent variable in Columns (1) is the mortgage monthly payment, while in column (2) it is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan, and in column (3) the dependent variable is the monthly partial prepayment and is computed based on data from BlackBox. The main independent variables are dummies identifying different time periods before and after the reset date. "One Year Before" identifies the twelve months before up to one month before the interest rate adjustment. "One Year After" includes the month of the adjustment up to eleven months after. "Two Years After" includes twelve month after the adjustment up to twenty-three months after. "High LTV" is a dummy equal to one if the LTV one year before the adjustment is greater than 120 percent. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)
	Interest Payment	Car Purchase	Prepayment
One Year Before	0.00132**	-0.00850	0.000969
	(0.000609)	(0.0115)	(0.00140)
One Year After	-0.520***	0.0303*	0.0417***
	(0.000929)	(0.0176)	(0.00211)
Two Years After	-0.525***	0.00982	0.0438***
	(0.00155)	(0.0293)	(0.00346)
One Year Before X High LTV	-0.00958***	0.0339**	0.000368
	(0.000745)	(0.0141)	(0.00176)
One Year After X High LTV	-0.0377***	0.0485**	-0.00814***
	(0.00110)	(0.0209)	(0.00259)
Two Years After X High LTV	-0.0398***	0.111***	-0.000578
	(0.00179)	(0.0338)	(0.00410)
FICO	-3.41e-05***	0.00108***	7.20e-05***
	(1.78e-06)	(3.37e-05)	(4.19e-06)
Log(House Prices)	1.18e-05**	4.01e-05	2.83e-05**
	(5.90e-06)	(0.000112)	(1.36e-05)
Households Fixed Effects	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes	Yes
High LTV X Time Fixed Effects	Yes	Yes	Yes
Low LTV X Time Fixed Effects	Yes	Yes	Yes
Observations	1,838,722	1,838,983	1,666,847
R-squared	0.732	0.025	0.147

Table 8
Heterogeneous Effects: Fico Score

The table reports coefficient estimates of least square regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate 5 years after the origination. The sample includes 5-year ARMs originated between 2005 and 2007 as provided by BlackBox Logic. The dependent variable in Columns (1) is the mortgage monthly payment, while in column (2) it is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan and in column (3) the dependent variable is the monthly partial prepayment and is computed based on data from BlackBox. The main independent variables are dummies identifying different time periods before and after the reset date. "One Year Before" identifies the twelve months before up to one month before the interest rate adjustment. "One Year After" includes the month of the adjustment up to eleven months after. "Two Years After" includes twelve month after the adjustment up to twenty-three months after. "High FICO" is a dummy equal to one if the FICO one year before the adjustment is greater than 660. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)
	Interest Payment	Car Purchase	Prepayment
One Year Before	-0.00547***	-0.00532	0.000264
	(0.000575)	(0.0124)	(0.00150)
One Year After	-0.548***	0.00312	0.0247***
	(0.000867)	(0.0186)	(0.00223)
Two Years After	-0.547***	0.0255	0.0326***
	(0.00138)	(0.0297)	(0.00350)
One Year Before X High FICO	0.00324***	0.0178	0.00125
	(0.000611)	(0.0131)	(0.00159)
One Year After X High FICO	0.0317***	0.0592***	0.0263***
	(0.000888)	(0.0191)	(0.00230)
Two Years After X High FICO	0.0290***	0.0648**	0.0176***
	(0.00135)	(0.0290)	(0.00346)
FICO	-1.99e-05***	0.00105***	7.72e-05***
	(1.60e-06)	(3.45e-05)	(4.18e-06)
Log(House Prices)	-5.26e-05***	-7.81e-05	2.86e-05***
	(4.26e-06)	(9.14e-05)	(1.09e-05)
Households Fixed Effects	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes	Yes
High FICO X Time Fixed Effects	Yes	Yes	Yes
Low FICO X Time Fixed Effects	Yes	Yes	Yes
Observations	2,212,779	2,213,325	2,029,757
R-squared	0.781	0.026	0.155

Table 9
Difference-in-Differences Results

The table reports coefficient estimates of least square regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate 5 years after the origination. The sample includes both 5-year and 10-year mortgages originated between 2005 and 2007 as provided by BlackBox Logic. The dependent variable in Column (1) is the mortgage monthly payment. The dependent variable in Column (2) is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan. The dependent variable in Column (3) is the monthly reduction in the mortgage balance and is computed based on data from BlackBox. Columns (4)-(6) show the results when we normalized the dependent variables by the size of the monthly payment of the mortgage at the origination. The main independent variables are dummies identifying different time periods before and after the reset date, and effectively compare the mortgage payments, car purchases and principal prepayment for the 5-year mortgages that have their interest rate reset with the 10-year mortgages that do not. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Mortgage age fixed effects are the quarters since origination. Origination cohort is the quarter of origination of the mortgage, and loan type identifies the 5-year and 10-year ARMs. We allow for different trends for each loan type originated in different years. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6
				Normalized by Payment Size at Origination		
	Interest Payment	Car Purchase	Prepayment	Interest Payment	Car Purchase	Prepayment
Four Quarters Before	-10.75***	40.11*	8.442**	-0.00194**	0.0152	0.00312
	(3.135)	(21.58)	(3.206)	(0.000772)	(0.0133)	(0.00194)
Three Quarters Before	-19.75***	26.13	6.935	-0.00355***	0.00703	0.00272
	(4.852)	(31.32)	(4.462)	(0.00101)	(0.0154)	(0.00245)
Two Quarters Before	-23.72***	48.57	12.12***	-0.00508***	0.0207	0.00641***
	(5.342)	(34.26)	(4.511)	(0.00114)	(0.0154)	(0.00227)
One Quarter Before	-20.74***	99.45***	10.61**	-0.00668***	0.0356*	0.00436
	(7.376)	(33.88)	(4.931)	(0.00180)	(0.0197)	(0.00288)
One Quarter After	-922.3***	146.7***	66.26***	-0.530***	0.0601***	0.0402***
	(43.58)	(46.24)	(6.329)	(0.00626)	(0.0225)	(0.00406)
Two Quarters After	-848.7***	162.3***	75.30***	-0.527***	0.0682**	0.0436***
	(33.86)	(46.17)	(6.956)	(0.00598)	(0.0313)	(0.00413)
Three Quarters After	-793.6***	187.3***	71.74***	-0.523***	0.0740**	0.0445***
	(33.06)	(42.69)	(7.501)	(0.00743)	(0.0298)	(0.00478)
Four Quarters After	-750.6***	186.1***	67.25***	-0.517***	0.104**	0.0429***
	(33.88)	(60.19)	(8.844)	(0.00704)	(0.0401)	(0.00537)
Two Years After	-713.5***	137.7*	62.21***	-0.518***	0.0949**	0.0431***
	(34.58)	(80.01)	(9.658)	(0.00766)	(0.0419)	(0.00647)
FICO Score	-0.0266***	1.803***	0.152***	-2.91e-05***	0.00104***	7.71e-05***
	(0.00495)	(0.0564)	(0.0101)	(2.16e-06)	(3.22e-05)	(5.58e-06)
Log(House Prices)	-0.141***	0.157	0.129***	-5.51e-05***	3.42e-05	5.69e-05***
	(0.0262)	(0.139)	(0.0165)	(3.34e-06)	(9.04e-05)	(9.69e-06)
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Origination Cohort X Loan Type X Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Mortgage Age Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,530,401	4,596,110	4,294,354	3,507,231	3,563,218	3,368,065
R-squared	0.977	0.030	0.243	0.987	0.033	0.253

Table 10
2007 Decline in LIBOR

The table reports coefficient estimates of least square and IV regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate. The sample includes ARMs with interest rate resets between six months and five years after origination as provided by BlackBox Logic for the period January 2007-March 2008. Columns (1)-(3) report the OLS results, while Columns (4)-(7) reports the IV results. The dependent variable in Column (1) is the mortgage monthly payment. The dependent variable in Columns (3), (5) and (7) is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan. The dependent variable in even Columns is the monthly reduction in the mortgage balance and is computed based on data from BlackBox. Columns (4)-(7) show the results when we instrument the monthly payments with the time dummies identifying the months before and after the interest rate reset. We allow for different trends for each loan originated in different quarters. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS			*IV*			
	Interest Payment	Prepayment	Car Purchase	Prepayment	Car Purchase	Prepayment	Car Purchase
Post Rate Adjustment	-252.7***	87.65***	127.7*				
	(5.039)	(33.00)	(74.88)				
Monthly Payment$_{t+3}$						-0.131***	-0.00402
						(0.0463)	(0.245)
Monthly Payment$_{t}$				-0.393**	-0.408	-0.292*	-0.623**
				(0.153)	(0.314)	(0.158)	(0.316)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,177	82,461	119,792	71,242	104,023	70,266	102,352
R-squared	0.976	0.199	0.174	-0.001	0.106	-0.001	0.106

Table 11
Uncertainty and Prepayment

The table reports coefficient estimates of least square and IV regressions relating the monthly mortgage payment, car purchases and mortgage principal prepayment to the reset of interest rate. The sample includes ARMs with interest rate resets between six months and five years after origination as provided by BlackBox Logic for the period November 2008-December 2009. Columns (1)-(5) report the OLS results, while Columns (6)-(9) reports the IV results. The dependent variable in Column (1) is the mortgage monthly payment. The dependent variable in Columns (3), (5), (7) and (9) is the monthly amount spent on car purchase and is computed based on the balance of the household's auto loan. The dependent variable in even Columns is the monthly reduction in the mortgage balance and is computed based on data from BlackBox. Columns (6)-(9) show the results when we instrument the monthly payments with the time dummies identifying the months before and after the interest rate reset and their interaction with the VIX. "VIX" is the volatility index and is provided by CBOE. We allow for different trends for each loan originated in different quarters. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			OLS					IV	
	Interest Payment	Prepayment	Car Purchase	Prepayment	Car Purchase	Prepayment	Car Purchase	Prepayment	Car Purchase
Post Rate Adjustment	-403.2***	47.46***	2.400	-129.9***	0.642				
	(2.846)	(5.420)	(2.915)	(27.25)	(12.46)				
Post Rate Adjustment × VIX				5.007***	0.0489				
				(0.774)	(0.356)				
Monthly Payment$_t$						-0.117***	-0.00576	0.326***	-0.00113
						(0.0135)	(0.00748)	(0.0779)	(0.0363)
Monthly Payment$_t$ × VIX$_t$								-0.0119***	-0.000123
								(0.00208)	(0.000978)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	382,576	372,487	429,853	372,487	429,853	328,930	379,075	328,930	379,075
R-squared	0.957	0.202	0.084	0.203	0.084	0.000	0.004	-0.024	0.004

Table 12
Further Evidence on Consumption and Voluntary Deleveraging

The table reports coefficient estimates of least square regressions relating the monthly purchases on credit cards, equity loans and home equity line of credit to the reset of interest rate 5 years after the origination. Panel A focuses on consumption, while Panel B analyzes household deleveraging decisions. The dependent variables are computed based on the households' balance of each type of loan as provided by Equifax. Columns (1)-(3) analyze the effect of the interest rate reset on store credit cards, while Columns (4)-(6) focus on credit cards issued by banks. For bank credit cards, we focus on households that use them for monthly payment, identified as those for whom there is enough volatility in their monthly balance and the average balance is below $10,000. The dependent variables in Panel B are equity loans and home equity line of credit provided by Equifax. The main independent variables are dummies identifying different time periods before and after the reset date. "FICO" is provided monthly by Equifax. "Log(House Prices)" is the logarithm of zip-level house prices. Origination cohort is the year of origination of the mortgage. Monthly payment at origination identifies the quartiles of different monthly payment size at origination. The sample includes mortgages originated between 2005 and 2007 provided by BlackBox Logic. Robust standard errors, clustered at the month level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Interest Rate Adjustment and Consumption

	1 Store Credit Cards	2 Store Credit Cards (Normalized)	3 Store Credit Cards	4 Bank Credit Cards	5 Bank Credit Cards (Normalized)	6 Bank Credit Cards
Four Quarters Before	1.170	0.000343	1.025	19.34	-0.0268	-11.22
	(2.649)	(0.00194)	(2.245)	(26.00)	(0.0175)	(23.40)
Three Quarters Before	3.174	1.05e-05	3.175	19.09	-0.0534**	-19.56
	(3.194)	(0.00234)	(2.391)	(31.94)	(0.0202)	(25.22)
Two Quarters Before	0.451	-0.000841	-0.484	46.02	-0.0595*	2.855
	(3.760)	(0.00276)	(2.558)	(44.86)	(0.0312)	(32.74)
One Quarter Before	10.01**	0.00358	7.395***	87.69	-0.0566	23.17
	(4.324)	(0.00317)	(2.774)	(54.99)	(0.0423)	(32.45)
One Quarter After	14.25***	0.00733**	9.842***	129.0**	-0.0446	46.08
	(4.926)	(0.00362)	(3.048)	(61.27)	(0.0457)	(36.25)
Two Quarters After	15.32***	0.00795*	8.915***	125.3*	-0.0239	37.30
	(5.564)	(0.00409)	(3.397)	(71.48)	(0.0541)	(46.77)
Three Quarters After	15.22**	0.00516	7.858**	140.4	-0.0209	40.64
	(6.191)	(0.00455)	(3.764)	(89.33)	(0.0665)	(52.28)
Four Quarters After	20.87***	0.0113**	11.37***	275.6***	0.0806	153.0***
	(6.919)	(0.00508)	(4.184)	(98.48)	(0.0829)	(53.59)
Two Years After	27.85***	0.0147**	16.24***	330.0**	0.163*	176.3***
	(7.877)	(0.00579)	(4.089)	(123.4)	(0.0915)	(60.84)
FICO Score	0.215***	0.000143***	0.216***	-8.559***	-0.00563***	-8.402***
	(0.00856)	(6.27e-06)	(0.00872)	(0.494)	(0.000353)	(0.495)
Log House Prices	0.0432*	3.71e-05**	0.0465*	2.173***	0.00152***	1.714***
	(0.0245)	(1.80e-05)	(0.0254)	(0.412)	(0.000295)	(0.399)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes		Yes	Yes	
Monthly Payment at Origination X Time Fixed Effect			Yes			Yes
Observations	1,158,492	1,124,408	1,124,408	289,562	279,911	279,911
R-squared	0.060	0.049	0.060	0.365	0.423	0.366

Panel B: Interest Rate Adjustment and Deleveraging

	1	2	3	4	5	6
	Equity Loan	HELOC	Equity Loan (Normalized)	HELOC (Normalized)	Equity Loan	HELOC
Four Quarters Before	-1.063	-4.839*	-0.000396	-0.00426**	-0.852	-5.499**
	(1.349)	(2.801)	(0.00105)	(0.00205)	(1.151)	(2.388)
Three Quarters Before	-0.120	-7.802**	0.000646	-0.00828***	1.349	-7.984***
	(1.642)	(3.399)	(0.00128)	(0.00249)	(1.244)	(2.564)
Two Quarters Before	-0.880	-8.139**	0.00142	-0.00947***	2.832**	-7.974***
	(1.946)	(4.037)	(0.00151)	(0.00296)	(1.351)	(2.762)
One Quarter Before	-1.732	-8.626*	0.00178	-0.00981***	3.158**	-8.458***
	(2.254)	(4.688)	(0.00175)	(0.00344)	(1.478)	(3.010)
One Quarter After	7.465***	16.63***	0.0102***	0.00377	12.85***	17.68***
	(2.574)	(5.354)	(0.00200)	(0.00393)	(1.632)	(3.294)
Two Quarters After	7.767***	15.65***	0.0111***	0.00327	14.50***	16.75***
	(2.912)	(6.062)	(0.00226)	(0.00445)	(1.823)	(3.650)
Three Quarters After	7.442**	19.58***	0.0126***	0.00669	16.31***	20.02***
	(3.252)	(6.751)	(0.00253)	(0.00496)	(2.028)	(4.025)
Four Quarters After	3.157	19.58***	0.0109***	0.00786	12.50***	18.28***
	(3.648)	(7.484)	(0.00284)	(0.00550)	(2.274)	(4.424)
Two Years After	2.278	16.71**	0.0131***	0.00525	15.38***	8.588*
	(4.195)	(8.487)	(0.00327)	(0.00624)	(2.330)	(4.518)
FICO Score	0.136***	0.201***	9.69e-05***	0.000112***	0.135***	0.198***
	(0.00525)	(0.0114)	(4.07e-06)	(8.38e-06)	(0.00532)	(0.0116)
Log House Prices	0.188***	0.221***	0.000170***	0.000169***	0.198***	0.253***
	(0.0199)	(0.0445)	(1.55e-05)	(3.26e-05)	(0.0205)	(0.0454)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Origination Cohort X Time Fixed Effect	Yes	Yes	Yes	Yes		
Monthly Payment at Origination X Time Fixed Effect					Yes	Yes
Observations	532,163	396,952	513,391	384,551	513,391	384,551
R-squared	0.357	0.394	0.342	0.388	0.358	0.394

Table 13
Aggregate Evidence: ARMs and Mortgage Interest Rate

Panel A reports the correlations between the fraction of adjustable-rate mortgages as of 2006 and the characteristics of the county. We consider the debt-to-income ratio in 2006 as reported by the New York Fed Consumer Credit Panel. "Securitization Boom" is computed using data from BlackBox Logic and is the change in the fraction of loans in a county that were privately securitized in the period 2002-2006. Panel B reports coefficient estimates of least square relating the quarterly change in the average mortgage interest rate in a county, with the fraction of adjustable-rate mortgages in the same county interacted with the six-month LIBOR rate, which is the most common index rate for the ARMs employed in the individual-level analysis. County controls include the interaction between the variables in Panel A and the six-month LIBOR rate. The sample covers the period from 2007 to 2013. Fraction of $ARMs_{2006}$ is the fraction of outstanding mortgages that are adjustable-rate in each county in 2006. All columns in Panel B include time and county fixed effects and Columns (2) and (4) also include State X Time fixed effects. Robust standard errors, clustered at the county level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Fraction of ARMs and County Characteristics

	1 Fraction of ARMs in 2006
Debt-to-Income ratio in 2006	0.115***
	(0.00657)
Log(Median Income)	0.195***
	(0.0275)
Poverty Rate	0.00665***
	(0.00107)
Securitization Boom	0.225***
	(0.0362)
Observations	804
R-squared	0.715

Panel B: Fraction of ARMs and Mortgage Interest Rates

	1 Mortgage Interest Rate	2 Mortgage Interest Rate	3 Average Monthly Payment	4 Average Monthly Payment
Fraction of $ARMs_{2006}$ X Six-Month LIBOR	0.172***	0.197***	0.0587***	0.0332***
	(0.0113)	(0.0117)	(0.00813)	(0.00689)
County Controls	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State X Time Fixed Effects		Yes		Yes
Observations	24,204	24,176	24,204	24,176
R-squared	0.342	0.245	0.143	0.145
Number of Counties	865	864	865	864

Table 14
Aggregate Evidence: Aggregate Consumption and Deleveraging

The table reports coefficient estimates relating the consumption and deleveraging in a county with the average mortgage interest rate in that county, instrumented by the interaction of the Fraction of ARMs in 2006 and the six-month LIBOR rate. Fraction of $ARMs_{2006}$ is the fraction of outstanding mortgage in each county that is adjustable-rate computed in 2006. Panel A reports the results for the reduced form estimates, while Panel B reports the IV results, when the average mortgage interest rate and the average monthly mortgage payment are instrumented with the interaction of the fraction of ARMs and the six-month LIBOR, which is the mostly used index rate for these mortgages. "Log (Car Sales)" is derived from Polk data. "Log(Credit Card Balance)" and "Log(Mortgage Balance)" are the logarithm of total balance on credit cards and mortgages in a county, employing data from the New York Consumer Fed Credit Panel. The dependent variable in Panel B is the log of the average mortgage balances in a county and is reported by the New York Consumer Credit Panel. We control for the log of population in a county. Moreover, we also control for the debt-to-income, median income, poverty rate and securitization boom interacted with the six-month LIBOR rate. The "debt-to-income ratio" in 2006 is reported by the New York Fed Consumer Credit Panel, while "Securitization Boom" is computed using data from BlackBox Logic and is the change in the fraction of loans in a county that are securitized in the period 2002-2006. The sample covers the 2007-2013 period. All columns include time and county fixed effects. Columns (4)-(6) of Panel A include state-time fixed effects. Robust standard errors, clustered at the county level, are below the coefficients in parenthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A. Reduced Form Estimates

	1 Log(Car Sales)	2 Log(Credit Card Balance)	3 Log(Mortgage Balance)	4 Log(Car Sales)	5 Log(Credit Card Balance)	6 Log(Mortgage Balance)
Fraction of $ARMs_{2006}$ X Six-Month LIBOR	-0.0592***	-0.0729***	0.0262***	-0.0459***	-0.0473***	0.0967*
	(0.0140)	(0.0176)	(0.00995)	(0.0157)	(0.0171)	(0.0496)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State X Time Fixed Effects				Yes	Yes	Yes
Observations	23,980	24,204	24,204	23,980	24,176	24,176
R-squared	0.072	0.088	0.461	0.031	0.095	0.259
Number of Counties	857	865	865	857	864	864

Panel B. IV Estimates

	Log(Car Sales)	Log(Credit Card Balance)	Log(Mortgage Balance)	Log(Car Sales)	Log(Credit Card Balance)	Log(Mortgage Balance)
Mortgage Interest Rate (Instrumented by Fraction of ARMs2006 X Six-Month LIBOR)	-0.340***	-0.424***	0.152***			
	(0.0806)	(0.103)	(0.0589)			
Average Monthly Payment (Instrumented by Fraction of ARMs2006 X Six-Month LIBOR)				-1.009***	-1.243***	0.447***
				(0.272)	(0.349)	(0.146)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,981	24,204	24,204	23,980	24,204	24,204
R-squared	0.059	0.075	0.439	0.000	0.020	0.567
Number of Counties	858	865	865	857	865	865