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Housing Wealth and Consumption: The Role of Heterogeneous Credit Constraints
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ABSTRACT

We quantify the role of heterogeneity in households' financial constraints in explaining the large decline in consumption between 2006 and 2009. Using household-level data, we show that in addition to a direct effect of changes in house prices, there are sizable indirect effects from general equilibrium feedback and bank health. About 60% of the aggregate response of consumption to changes in house prices is explained by ex-ante and ex-post financial constraints, where only a specific set of households face binding ex-post financial constraints as a result of declining house prices. We find a negligible wealth effect once we account for the role of heterogonous financial constraints.

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1 Introduction

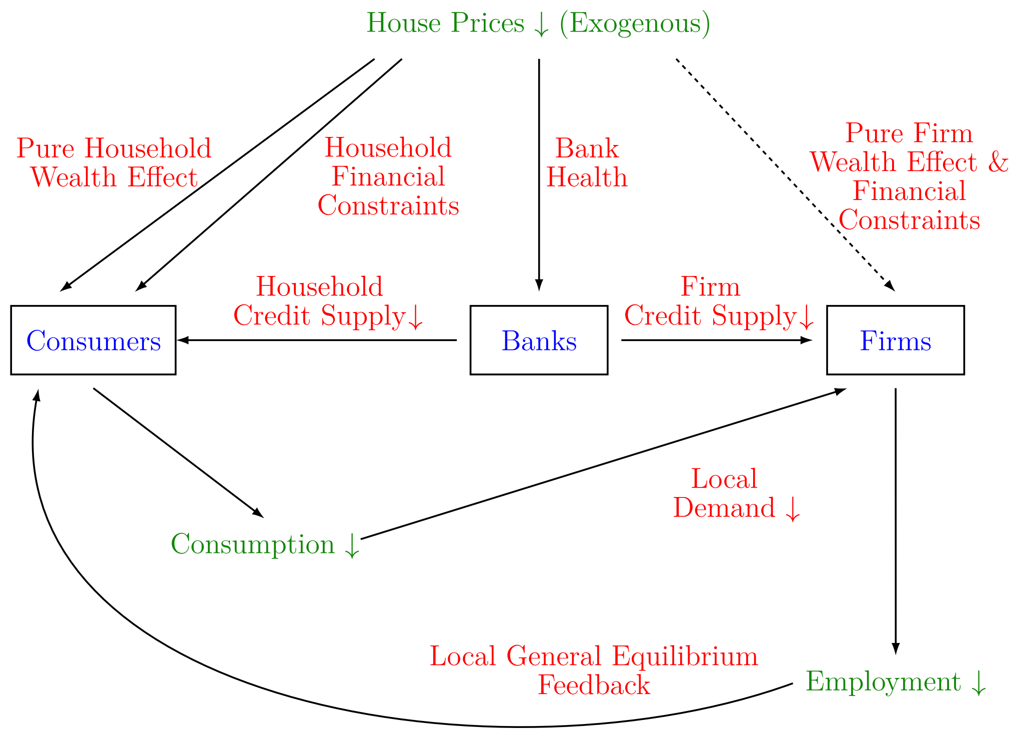
The U.S. economy experienced a large financial crisis together with a housing bust in 2007–2008. A deep recession with significant declines in consumption, investment, and employment followed. Although there is an extensive theoretical and empirical literature on the causes and consequences of the crisis, so far, there is still no consensus on the role of the channels linking the housing bust to the recession. We empirically investigate the role of household heterogeneity in terms of financial constraints to quantify the role of the possible channels linking house price declines to decline in output, focusing on durable consumption.

There is an extensive theoretical literature studying monetary policy transmission in heterogeneous agent macro models focusing on household heterogeneity for the consumption channel (e.g. McKay et al. (2016), Kaplan et al. (2018), Auclert (2019), Wong (2021), Guerrieri et al. (2022a) and Guerrieri et al. (2022b)). So far this literature did not consider the heterogeneity in household-level financial constraints in explaining aggregate consumption under wealth shocks. The work of Berger et al. (2015) and Kaplan et al. (2020a) investigate the *combined* effect of heterogeneity in wealth and financial constraints. We quantify these effects separately since, given different policy implications, it is important to know the magnitude of each effect. A pure wealth effect channel underscores the importance of stability in house prices, whereas a financial constraint channel may imply that household leverage should be limited during booms with macroprudential policy.[1]

There is an important data constraint in undertaking this exercise. The household heterogeneity in wealth and the household heterogeneity in financial constraints may not map one-to-one under shocks to house prices given confounders. Mian and Sufi (2009), Mian and Sufi (2011), and Mian et al. (2013) have documented that an increase in household leverage predicts the subsequent crisis, de-leveraging and consumption decline. Jones et al. (2020), on the other hand, argue that household de-leveraging by itself cannot explain a large part of decline in employment and output. Both results are right if wealth accumulation and increasing leverage go hand-in-hand for rich households only, while during the bust lever-

[1]Following Buiter (2010) we would argue that there is no pure wealth effect if consumption does not respond to fundamental changes in the value of housing. Thus, if there is no pure wealth effect, then the only direct effect of change in the value of housing would be through changes in the degree at which various constraints bind.

Figure 1: House Prices and Consumption: Channels



age will act as a financial constraint more so on the poor households. Macro data cannot speak to this issue of heterogeneity. Thus, most of the existing empirical literature, while acknowledging the importance of heterogeneity, is unable to separately identify the pure wealth effect and the effect of financial constraints.[2] We employ micro data at the household level to distinguish these channels.

To be able to focus on the role of heterogeneity in financial constraints, we have to control other channels that will lead to a decline in consumption under a shock to house prices. Figure 1 shows all of the possible channels that we identify as linking a decline in house prices to lower consumption. Figure 1 shows three players in the same locality: consumers, banks and firms. First, on the household side, as a result of declining house prices, there will be both a pure wealth effect, denoted with the arrow "pure household wealth," and a

[2] Cloyne et al. (2020) show, using U.S. and U.K. household survey data, that the consumption of those who own their home without a mortgage is not affected by changes in monetary policy, while the consumption of those that have a mortgage is. Bunn and Rostom (2016) show that the level of mortgage debt is an important determinant of the consumption response of U.K. consumers to the financial crisis.

balance sheet effect (if housing is an important source of collateral for borrowing), denoted with the arrow "household financial constraints."[3]

Next, as shown in the figure, there is the effect of house price declines on bank health. If banks are exposed to the real estate market, housing price declines would constitute a negative balance sheet shock to banks, which can result in banks cutting credit supply to both households and firms. This importance of the credit supply channel has found support in the work of Justiniano et al. (2019); they show that an *increase* in credit supply is necessary to match the empirical regularities in the *boom* period (where the *increase* in house prices served as a positive shock to bank balance sheets).[4]

If wages are sticky in the short-run, lower credit supply to firms will lead to lower employment and investment. While Duygan-Bump et al. (2015) and Greenstone et al. (2020) find that reduced credit supply can account for only less than one-tenth of the decline in employment, Chodorow-Reich (2014), Chen et al. (2017) and Gilchrist et al. (2017) find that up to one-third of the employment decline may be driven by bank shocks. Similarly, García (2020) finds that the reduction in credit supply explains about 15% of the actual employment decline observed.

A similar channel can also occur with a direct shock to firm balance sheets instead of bank balance sheets, when firms' owners use their own housing wealth as collateral to obtain loans to invest and to produce. Bahaj et al. (2020) provide direct evidence for this channel for the U.K. We are not able to directly identify this channel, since we do not have information on firms' or their owners' real estate wealth, though it is a part of the local general equilibrium effect. This final channel works via general equilibrium, where, due to low consumption, demand for firms' output will be lower, which also leads firms to decrease employment, as shown by the "local demand" arrow in the figure. Any firm-level response via lower employment will feed back to lower consumption because of this local general equilibrium effect, as shown by the bottom arrow.

[3] Many argued that to be able to match the large responses of consumption to house prices changes found in the data, one needs collateralized lending that amplifies the impact of housing wealth on consumption. See Berger et al. (2015), Guerrieri and Iacoviello (2017), Iacoviello (2005).

[4] Gropp et al. (2014) show empirically that renters, compared with homeowners, reduced their levels of debt more in counties where house prices fell more, suggesting a more important role for credit supply than a demand-based response to lower housing wealth. Hryshko et al. (2010) also show a stronger response from renters using PSID.

The key shortcoming in most of the literature so far is the unavailability of individual-level data on house values, wealth, consumption, financial constraints and individual characteristics such as age. We use individual-level data from two sources that give us the most detail to date in terms of consumption, mortgages and creditworthiness. Our first dataset is the Federal Reserve Bank of New York Consumer Credit Panel / Equifax (henceforth CCP), a quarterly database of consumer credit bureau records for a random 5 percent anonymized sample of consumers with a credit bureau record. Our second dataset is a match between credit bureau data with more detailed information on residential first mortgages from loan servicing data. This anonymized matched dataset is Equifax Credit Risk Insight Servicing (Equifax Credit) and Black Knight McDash (McDash), known as CRISM.[5] These datasets give us borrower-level information on all loans of the borrower, including any auto loans; the borrower's Equifax Risk Score (henceforth Risk Score); the borrower's age; and detailed characteristics of the borrower's mortgages, most notably the appraised value of the property and the type of mortgage. It is important to emphasize that having access to the appraised value of the borrower's primary residence allows us to condition on the dollar change in value of the borrower's house, as opposed to merely relying on an aggregate (e.g. ZIP code-level) house price growth.

Other papers using similar individual household-level data are Adelino et al. (2017), Albanesi et al. (2017), and Aladangady (2017). The former two papers argue that credit growth between 2001 and 2007 was concentrated in the prime segment, debt to high risk borrowers was virtually constant for all debt categories during this period, and default among high income prime borrowers was common during the post period.[6] The work of Aladangady (2017) is closest to our paper. To the best of our knowledge, this is the only other paper using individual-level data (restricted-access geographical files from the Consumer Expenditure Survey) to investigate the consumption response to a change in house prices, though he focuses on the period before the 2007-2009 Great Recession. He finds results similar to ours in terms of importance of household-level financial constraints. Other than the time period

[5] Some of the other papers that also use CRISM data are Beraja et al. (2019) to investigate the response of auto consumption to QE; Agarwal et al. (2020) and Di Maggio et al. (2020) to investigate refinancing; García (2019) to investigate secondary housing market.

[6] Albanesi et al. (2017) also use individual-level data from one of the datasets we use, CCP, but focus on growth in mortgage debt prior to crisis and subsequent defaults rather than consumption response as we do.

of analysis, there are two main differences between our paper and his. First, we can account for general equilibrium effects and the effect of bank health. Second, we have much larger and detailed individual-level data that helps us identify both ex-ante and ex-post borrowing constraints. His key variables to identify constrained households are refinancing, household leverage and debt service, whereas we also have direct data on loan types and individuals' credit risk.

As a proxy for consumption, we use a binary variable at the *individual level* that represents the origination of an auto loan in 2009. This resembles the ZIP code-level new car registration data that Mian et al. (2013) use in their analysis. Using detailed information on mortgage (type of mortgage and the loan-to-value ratio) and borrower characteristics (Risk Score and payment history) in our data, we are also able to describe the possible reasons why financial constraints affect consumption. We distinguish between ex-ante and ex-post credit constraints. Ex-ante constraints are those that existed prior to 2006 and ex-post constraints are those triggered by the decline in house values. We find that a large fraction of the direct response of consumption to changes in house values can be attributed to a particular ex-post financial constraint that we identify using mortgage payment history in our data. We also use mortgage characteristics and borrower's Risk Score to identify the ex-ante constraints. Finally, we turn to the identification of the pure wealth effect. To do so, we focus on consumers that are unlikely to face any credit constraints and we show that these consumers do not react to changes in their house value.

The decline in house prices are not exogenous. To remedy this issue, we use an instrumental variables (IV) approach, where we instrument the change in house prices with standard instruments related to housing supply elasticity, as also done in the literature. This allows us to isolate the effect of house price changes from all other factors that may have affected consumption during this period. The MSA-level instruments we use measure lower land availability for development and tighter land use regulations, both of which create variation in housing supply elasticity. We find the change in house values has a large overall effect on consumption – the average decline in house values between 2006 and 2009 leads to a decline in consumption in 2009, in the form of auto purchases, that is about $1,200. Referring to Figure 1, 39% of this response is indirect, and it is due to the Local General Equilibrium

(24%) and the Household Credit Supply channels (15%), highlighting the importance of controlling for these additional channels.

Our results are consistent with the broader housing wealth and permanent income literature. Many papers generally estimate a small pure wealth effect; 5 cents out of 1 dollar in Pistaferri (2016) with aggregate data, and 2 cents out of 1 dollar using PSID in Carroll et al. (2011).[7] In the standard permanent income model, a shock to housing wealth will have no effect on consumption since positive endowment effects will be canceled out by negative cost of living effects, as shown by Buiter (2010). In the context of a life-cycle model, if homeowners are likely to sell their house in the future, there can be positive wealth effects via rising house prices, as modeled by Sinai and Souleles (2005). Garriga and Hedlund (2020) present a rich incomplete-markets macro-housing model where consumers with larger mortgages (and illiquid wealth) respond much more to house price changes, in line with our empirical results. Guren et al. (2021), use historical data and show that responses to changes in housing wealth are consistent with a standard life-cycle model with borrowing constraints, uninsurable income risk, illiquid housing, and long-term mortgages. They also find that housing wealth effects were not particularly large in the 2000s. Accounting for the *heterogeneity* in financial constraints, as we do, seems to be key to account for explaining the large aggregate response of consumption.

We proceed as follows. Section 2 discusses the data in detail. Section 3 presents our empirical strategy. Section 4 presents our main results regarding the importance of financial constraints and Section 5 focuses on identifying the pure wealth effect for the households. Finally, Section 6 concludes.

2 Data

This section introduces our data in detail. We first introduce our individual-level data followed by aggregate (ZIP-, county- and MSA-level) data.

[7] Kessel et al. (2019) estimate a pure wealth effect of only 0.13 cents out of 1 dollar using a quasi-experiment in Sweden.

2.1 Individual-Level Data Sources

2.1.1 Credit-Bureau Data

Our main dataset is CCP, a quarterly database of consumer credit bureau records for a random 5 percent anonymized sample of consumers with a bureau record. From the CCP we can observe total balances and aggregate delinquency status on a variety of consumer credit obligations such as mortgages, auto loans and credit cards, Risk Score, as well as some loan-level information on first and second mortgages. We are also able to calculate the age of consumers based on the birth year that is provided in the CCP.

Our second dataset is a match between credit bureau data with more detailed information on residential first mortgages from loan servicing data. This anonymized matched dataset is Equifax Credit Risk Insight Servicing (Equifax Credit) and Black Knight McDash (McDash), known as CRISM.[8] CRISM captures approximately two-thirds of all mortgage originations during this time period, and it gives more detailed information on the borrower's mortgages than found in CCP itself: most notably the appraised value of the property; interest rate; other characteristics such as whether it is fixed or adjustable rate, low documentation; and monthly mortgage performance information. While CRISM includes basic credit bureau information from the period the mortgage is active, we further restrict attention to CRISM borrowers who are also in CCP. This gives us a much richer set of credit bureau variables, which allows us, for example, to identify auto loan originations (below), and also determine the borrower's age and whether they have moved. Finally, we restrict attention to homeowners with a single first-mortgage in December 2006 and December 2009 (though not necessarily the same one) and to those who have not moved between December 2006 and in December 2009; we are left with about 349,000 unique individuals.[9]

Being able to observe the appraised value of houses is a major strength of our dataset, as it allows us to compute the dollar change in the value of houses between 2006 and 2009.[10]

[8] The exact details of the matching procedure are proprietary, but it is an anonymous match, using loan amount and other loan characteristics, and is similar to that in Elul et al. (2010).

[9] The former restriction is designed to drop investors and those with multiple residences. We exclude those that move because we are using individual-level house value changes, based on the house they are in as of the end of 2006.

[10] We start with the appraised value of the house associated with the first mortgage that is active as of December 2006. Using the ZIP-level house price change from CoreLogic Solutions single family combined

Table 1: Summary Statistics

(a) Individual-Level Variables

	Mean	**Std. Dev.**	**5%**	**Median**	**95%**
Originate Auto Loan in 2009	0.135	0.342	0	0	1
Change in House Value ($1,000)	-78.1	81.0	-229.3	-52.8	1.3
Bad Mortgage	0.070	0.255	0	0	1
2006 Non-Housing Net Worth ($1,000)	130.5	5,132.6	11.6	86.2	285.6

(b) Aggregate Variables

	Mean	**Std. Dev.**	**5%**	**Median**	**95%**
Change in Unemployment Rate (county, p.p.)	5.5	1.8	3.0	5.3	8.7
2003 ZIP-Code Auto Sales (per-capita, $)	3,265.9	855.3	1926.7	3219.0	4747.0
Credit Supply Shocks (county, $\times$100)	-2.8	8.8	-13.2	-4.8	14.1
Bank Health (county, $\times$100)	0.64	0.12	0.41	0.65	0.78
Land Unavailable for Development (MSA)	0.29	0.21	0.03	0.251	0.67
WRLURI (MSA)	0.25	0.67	-0.81	0.31	1.60

Notes: Change in House Value and Change in Unemployment Rate are computed between December 2006 and December 2009.

Furthermore, using the information in CRISM, we estimate the first-mortgage loan-to-value (LTV) ratio for the house as of December 2006, by dividing the remaining balance in the first mortgage by the value of the house.[11] Table 1 shows the descriptive statistics for the key variables we use in the analysis. It shows that the average and median declines in house values in our sample from December 2006 to December 2009 are $78,100 and $52,800, respectively, where the average decline is about 20 percent. All but about 5% of the individuals experience a house value decline, with the fifth percentile at a decline of $229,300.

Another individual-level variable we create from CRISM is one we label **Bad Mortgage**. We use both the payment status for the mortgage in CRISM as well as that reported in CCP to identify households that are seriously delinquent (at least 90 days behind) in any

house price index (in percentage), we compute an estimate of the updated property value in December 2006 and in December 2009. If ZIP-level house price index is not available, then we use the next smallest unit (county or state) that is available. Naturally, the percentage change in the house value will equal the percentage change in ZIP-level house prices, but within the same ZIP code dollar changes in house values will differ across individuals.

[11] We drop individuals with an estimated LTV ratio of 125% or higher.

Table 2: Distribution of Characteristics

LTV Category	**Prime**	**Non-Prime**	**Total**
LTV0 (LTV ratio less than 25%)	14.6%	2.5%	17.2%
LTV1 (LTV ratio between 25% and 50%)	28.8%	8.6%	37.4%
LTV2 (LTV ratio between 50% and 80%)	22.6%	10.3%	32.9%
LTV3 (LTV ratio greater than 80%)	8.5%	4.0%	12.5%
Total	74.6%	25.4%	100.0%

	Prime				**Non-Prime**				
Mortgage	**LTV0**	**LTV1**	**LTV2**	**LTV3**	**LTV0**	**LTV1**	**LTV2**	**LTV3**	**Total**
Fixed Rate	8.9%	16.1%	11.6%	4.2%	1.5%	5.2%	6.0%	2.3%	55.8%
ARM $<$ 5yr	0.4%	0.6%	0.8%	0.4%	0.2%	0.6%	1.1%	0.5%	4.6%
ARM $\geq$ 5yr	0.3%	1.0%	1.3%	0.6%	0.1%	0.2%	0.3%	0.1%	3.9%
CE Second	0.7%	2.1%	2.3%	0.8%	0.2%	0.9%	1.3%	0.4%	8.6%
HELOC	4.4%	9.1%	6.7%	2.6%	0.5%	1.6%	1.6%	0.6%	27.0%
Total	14.6%	28.8%	22.6%	8.5%	2.5%	8.6%	10.3%	4.0%	100.0%

Notes: See the text for the definitions of the categories.

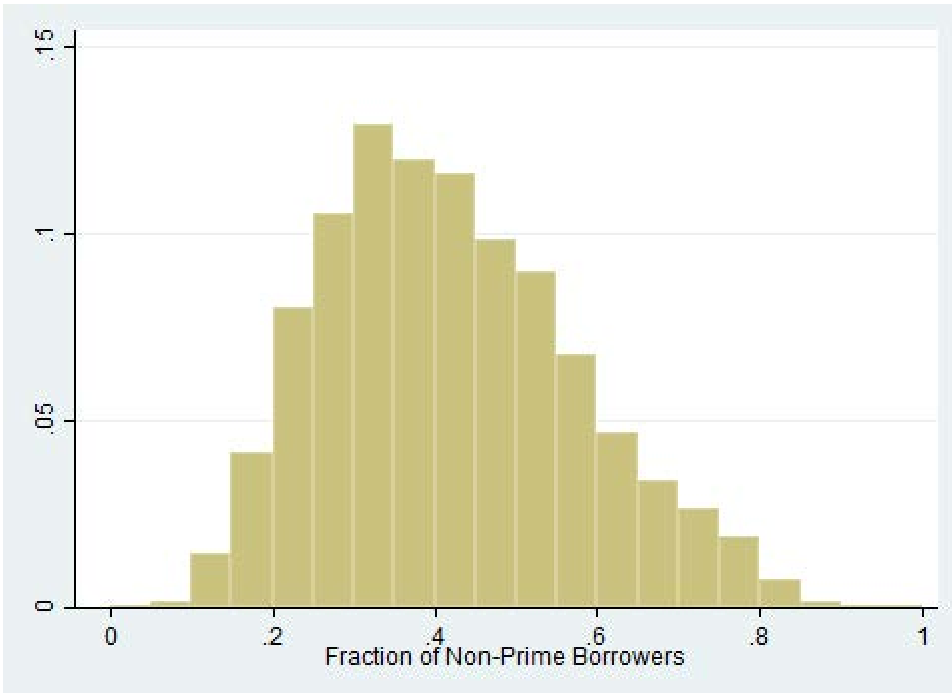
mortgage payment during 2007-2009. Note that since we require the presence of a mortgage in December 2009, we generally drop those that completed the foreclosure process by that point. As the majority of defaults in our sample occurred in 2008 and 2009, and the average foreclosure timeline in this period exceeded a year, this is not a significant limitation. According to Table 1, 7% of households had a bad mortgage in this period.

Both CRISM and CCP are available as panels, though in a majority of our analysis we draw information from individual years and conduct a cross-sectional analysis. In Section 5.2 we utilize the panel structure of CCP in a limited way. We postpone the introduction of this analysis and the data to that section.

2.1.2 Household Classifications

We classify the 349,000 households in our data in three different ways, all using *ex-ante* criteria that are observed as of 2006Q4, the start of our analysis. Table 2 shows the fraction of households that fall in each group. First, we label households with a Risk Score of 700 or

Figure 2: Distribution of Fraction of Non-Prime Borrowers Across ZIP Codes



higher as **Prime** and the others as **Non-Prime**. About a quarter of households are in the Non-Prime category. To be sure, 700 is a fairly high cutoff for prime borrowers, reflecting the fact that our analysis focuses on homeowners, who tend to be more creditworthy. Figure 2 shows the distribution of ZIP codes with respect to the fraction of non-prime borrowers. This shows that a vast majority of ZIP codes have a mixture of Prime and Non-Prime borrowers, and thus ZIP code-level variables and the individual-level indicator of prime status will contain largely independent information.

Second, we use the imputed first-lien LTV ratio as of December 2006 to create four groups: those with a LTV ratio of less than 25%, 25% or higher and below 50%, 50% or higher and below 80% and greater than or equal to 80%. We refer to these groups as **LTV0**, **LTV1**, **LTV2** and **LTV3**, respectively. Over half of the households have a LTV ratio below 50%. About a third of households have a LTV ratio between 50% and 80% and about 13% of households have a LTV ratio of 80% or higher. Not surprisingly, LTV ratio and Prime Status (or Equifax Risk Score) are somewhat negatively correlated: while the ratio of prime to non-prime is 3 to 1 in the general population, it is over 5 to 1 for LTV0 and about 2 to 1 for LTV3.

Finally, CRISM contains more detailed information about the type of the mortgages the households have. Using this information, we create five categories: those with a **Fixed-Rate First Mortgage** (and no second lien); those with an **Adjustable-Rate Mortgage (ARM)** that has an initial fixed-rate duration of **less than five years** or **greater than or equal to five years** (and no second lien); those with a **Closed-End Second Mortgage** (and any first mortgage); and those with a **Home Equity Line of Credit (HELOC)** (and any first mortgage), all as of December 2006.[12] Over 55% of households have only a fixed-rate first mortgage and no second mortgage and a large majority of these households are prime. Only 8.5% of households have an ARM and about 9% of households have a closed-end second mortgage with about two prime households for every non-prime household. Finally 27% of households have a HELOC with an overwhelming majority being prime. We drop individuals (about 1% of our sample) that have both types of second mortgages.

2.2 Construction of the Consumption Proxy

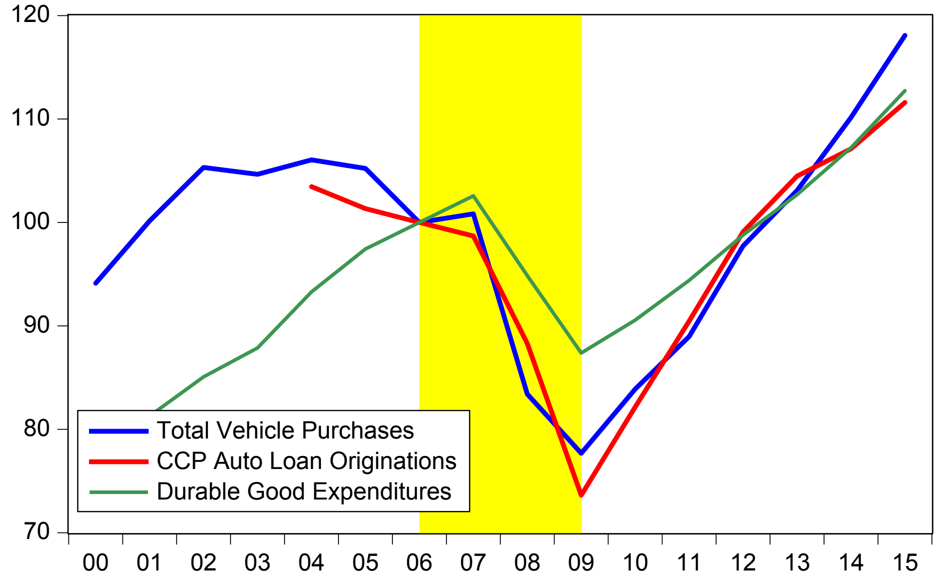
We proxy for individual-level consumption by **Auto Loan Originations**. The Auto Loan Tradeline Panel of CCP provides data on auto loan and leases, which includes the month of origination. This data tracks the incidence of auto loan originations in other sources very well: for example, we find that 10.1% of all consumers have an auto loan origination in 2008 in the CCP, whereas from the Panel Study of Income Dynamics (PSID) the origination rate in that year is 10.8%.[13] Table 1 shows that in 2009, the year we focus on our analysis, 13.5% of households in our sample originated an auto loan.

The aggregated auto loan originations from CCP also track alternative aggregate vehicle expenditure measures very well, all of which are highly cyclical. The red line in Figure 3 shows the aggregated Auto Loan Originations and the blue line is Total Vehicle Purchases from the Bureau of Economic Analysis (BEA), both normalized to 100 in 2006. Our measure

[12] A closed-end second mortgage is one that is junior to the first mortgage, and also does not allow any further draws following the origination date (in contrast to a Home Equity Line of Credit).

[13] Panel Study of Income Dynamics, public use dataset. Produced and distributed by the Institute for Social Research, University of Michigan, Ann Arbor, MI (2017). This figure is computed from the 2009 wave of the PSID, using the number of respondents with a vehicle that was acquired in 2008, and the share of these which were acquired using a loan or lease.

Figure 3: BEA vs. CCP: Consumption Expenditures



tracks the BEA measure almost perfectly over time, both in terms of magnitudes and also turning points. The figure also shows Durable Good Expenditures from the BEA, which contains much more than Vehicle Purchases. For Durables the peak occurs a year later and the trough is not as deep.

As Mian and Sufi (2016) also emphasize, individual-level consumption data, combined with detailed asset and liability information, is hard to come by for the United States. This motivates our use of auto loan originations as a proxy for individual consumption. By contrast, Mian et al. (2013) use New Car Registrations from Polk at the ZIP code level.[14] Relative to Mian et al. (2013), our measure has some pros and cons. First and foremost, the most important advantage of our data is that it is at the individual level, and since it is obtained from credit bureau data, we are also able to exploit other individual-level characteristics found in our datasets, rather than basing our analysis solely on aggregate measures. Second, by focusing on consumer credit records, we are able to isolate auto purchases by consumers, as opposed to businesses. Finally, our measure also captures purchases of used cars, and not just new ones. Johnson et al. (2014) report that both used car purchases

[14]Kaplan et al. (2020b) replicate all the results of Mian et al. (2013) using publicly available aggregate data.

and the propensity to use auto loans for auto purchases are countercyclical, and thus we conclude that our measure is relatively more likely to capture auto purchases in the recessionary period that we consider. The clear disadvantage of our data is of course that we miss cash auto purchases. However, Johnson et al. (2014) report that about 70 percent of household purchases of new vehicles and 35 percent of household purchases of used vehicles are financed with auto loans. Furthermore, the Federal Reserve Board (2016) reports that new vehicles make up only 38% of all consumer auto purchases, which means it is important to capture purchases of used vehicles as well. Thus on balance we believe that our measure is a reasonable proxy for auto purchases by consumers.[15]

2.2.1 Wealth Imputation Using Survey of Consumer Finances

While the CRISM data allows us to capture housing assets and liabilities, households' consumption decisions may be influenced by their non-housing net worth, defined as assets minus liabilities excluding the value of the house and the loans that are secured by the house. This measure, in turn, may be at least partially correlated with the value of their house. As such, the omission of non-housing net worth may bias our results. Unfortunately, while it has extensive coverage of households' liabilities, credit bureau data does not contain information on their assets. To overcome this problem we use the Survey of Consumer Finances (SCF) to impute non-housing assets of households.[16] To do this, using the 2007 wave for SCF, we regress non-housing assets on variables that are common in both the SCF and CCP/CRISM such as auto loan balance (+), number of auto loans (+), an indicator of having a HELOC (+), balance of HELOC (−), number of credit cards (+), credit limit (+), credit utilization (−), first mortgage balance (+) and LTV ratio (−), where signs in parentheses show the signs we get in this regression. The SCF regression has 8,032 observations and an R^2 of about 0.40. Using the estimated equation and the information we have for these right-hand side

[15] Astute readers may recall that a government rebate program designed to stimulate new car sales called Cash for Clunkers (CfC) was in effect in July-August 2009 and one may be worried that this could influence the usefulness of our consumption proxy. Based on the results of Hoekstra et al. (2017), we conclude that CfC may have only slightly increased our measure by moving to 2009 a small amount of sales (around 3%) that would have occurred in early 2010. Moreover, we find that at the state level, the state's share of all CfC registrations is uncorrelated with change in house prices between 2006 and 2009. Thus we conclude that CfC is unlikely to influence our results in any meaningful way.

[16] Coibion et al. (2020) do a similar imputation for the income of consumers in CCP.

variables in CCP/CRISM, we compute the implied non-housing assets for each of our households. We then combine this with the non-housing liabilities from CCP to get a measure of non-housing net worth. To account for the measurement error that may arise due to the imputation of assets, we create four equally-sized bins. Our computations yield mean and median non-housing net worth of \$130,500 and \$86,200, respectively. The 5 to 95 percentile range is \$11,600 to \$285,600.

2.3 Aggregate Data

In addition to individual-level variables, we use some ZIP and county-level variables to provide controls as well as help us identify the portions of the change in consumption that are due to local general equilibrium effect and credit supply.

To proxy for local economic conditions we use the county-level unemployment rate published by the Bureau of Labor Statistics. **Change in Unemployment Rate** from December 2006 to December 2009 will be an important control variable. Table 1 shows that the unemployment rate increased by about 5.5 percentage points for the average county during this time period, with a 5-95 percentile range from 3% to 8.7%.

We also use a ZIP code-level measure of **Auto Sales in 2003**, which we compute by aggregating the individual-level auto loan origination variable.[17] This is meant to capture permanent geographical differences in the prevalence of car ownership, holding other things constant (e.g. between Manhattan and Los Angeles). We choose 2003 for this because it is sufficiently far from the 2006-2009 period we consider to represent a baseline.

To capture the effect of changes in banks' credit supply on consumption, we follow the methodology in Gilchrist et al. (2017) and Greenstone et al. (2020). In particular we use Home Mortgage Disclosure Act (HMDA) and bank balance sheet data from call reports to identify the part of credit growth in a county that can be exclusively attributable to changes

[17]We start with the ZIP code-level sum of loan originations. Along the lines of Mian et al. (2013), we then allocate annual national retail auto sales (from the Census Bureau) across ZIP codes in proportion to their share of auto loan originations in our data; for example, if a ZIP code in our dataset accounted for 5% of all auto loan originations for that year, it would be allocated 5% of national retail auto sales. We then divide by the number of households in the ZIP code, which we obtain by applying the national population growth rate to the ZIP code populations in the 2000 census.

in credit supply.[18] We obtain the credit supply shocks in 2006-2007, 2007-2008 and 2008-2009 and sum these to get the appropriate credit supply shock that corresponds to the period from 2006 to 2009. Table 1 shows that while the mean and median of **Credit Supply** are negative at -2.8% and -4.8%, respectively, the 5-95 percentile range is very wide at -13.2% to 14.1%, indicating very different credit supply shocks across counties in this period.

Finally, as an alternative to the Credit Supply variable, we create a variable we call **Bank Health**. This is a county-level version of bank health indicator provided by Chodorow-Reich (2014), who uses, among others, a bank-level measure of the fraction of the syndication portfolio where Lehman Brothers had a lead role. Using information about the number of branches / affiliates each bank has in each of the U.S. counties, we distribute this measure to counties. The resulting variable shows each county's exposure to Lehman Brothers and, since this exposure is determined before 2008, it can also serve as an exogenous measure of bank health in each county relative to auto loan originations in 2009. Table 1 shows that this measure has a 5-95 percentile range of 0.41% to 0.78% with an average of 0.64%.

2.4 Local Housing Supply Instruments

Most of our analysis is undertaken using an Instrumental Variable (IV) approach in order to address the endogeneity of individual-level house values and omitted variables. To that end, we use two instruments. Both instruments capture the elasticity of housing supply and therefore the response of house prices to demand shocks. First is the **Share of Land that is Unavailable for Real Estate Development**, from Saiz (2010), which measures the share of land within a 50km radius of the MSA centroid that cannot be developed based on geographic features. It ranges from 0.004 to 0.86 in our sample, with higher values corresponding to more unavailable land. In addition, we use the MSA-level **Wharton Residential Land Use Regulation Index (WRLURI)** developed using a survey by Gyourko et al. (2008). This is a standardized measure across all municipalities, and lower

[18] More specifically, we follow the approach in Gilchrist et al. (2017) and first regress the change in mortgage lending in a county, by a bank and in a year on a county-time fixed effect (to capture demand) and on a bank-time fixed effect (to capture supply). Next we project the bank-time fixed effect on bank balance sheet variables that capture bank health. This step ensures that we keep only the changes in bank credit supply that are related to banks' fundamentals. Finally, this bank-time variable is distributed to counties using the market share of each bank in each county.

values can be thought of as reflecting the adoption of more laissez-faire policies toward real estate development.

Following Mian and Sufi (2009), Mian and Sufi (2010), Mian and Sufi (2011), Mian et al. (2013) and Guren et al. (2021), we use these instruments as a measure of housing supply elasticity. High values of both instruments indicate more inelastic housing supply. The adverse housing demand shock in the period we study represents a reversal of an earlier boom. Thus locations with more inelastic housing supply display larger declines in house prices, since the previous boom lasted longer, and prices rose further, in these areas (see Glaeser et al. (2008)).

3 Empirical Strategy

In this section we lay out our empirical strategy. Before we turn to the full individual-level analysis, we first link our analysis to the ZIP code-level analysis in Mian et al. (2013).

3.1 From ZIP Code to Individual Level

We begin by defining the consumption measure used by Mian et al. (2013). Let $R_{z,t}$ denote the number of new car registrations in ZIP z in year t, and S_t the aggregate dollar value of new car sales in the U.S. in year t. Also let $h_{z,t}$ be the number of households in ZIP z in year t. This is obtained from the 2000 Census data, and then the national population growth rate is applied to arrive at the household counts for later years. Given these we can define $C_{z,t}^{MRS}$, the relevant consumption measure in Mian et al. (2013), as $C_{z,t}^{MRS} \equiv S_t \frac{R_{z,t}}{h_{z,t} \sum_{z'} R_{z',t}}$, which simply allocates S_t to each ZIP using the share of new car registrations in that ZIP out of the whole U.S. and then normalizes by the number of households in that ZIP. Furthermore, let $\Delta HP_z^{2006-2009}$ denote the average dollar change in house prices in the ZIP between 2006 and 2009, which is computed as the change in the local house price index applied to the per capita house value from the 2000 Census.

Given these definitions, one of the headline results at the ZIP-level in Mian et al. (2013), as shown in column 5 of their Table V, is

$$C^{MRS}_{z,2009} - C^{MRS}_{z,2006} = \alpha^{MRS} + \underset{(0.001)}{0.018} \Delta HP_z^{2006-2009} + \varepsilon_z^{MRS} \text{ with } R^2 = 0.153, \text{ and } N = 6,263,$$

which shows a highly significant effect of the change in house prices on change in consumption: an \$18 decline for every \$1,000 drop in house prices. Their Table I shows that the weighted average (by population) of the change in house prices from 2006 to 2009 at the county level is −\$47,500 in their data (they do not report the same statistic for their ZIP code analysis). This means that, at the average house price decline, the change in consumption between 2006 and 2009, as proxied by new car sales, is $\overline{\Delta C^{MRS}} = -\$855$.

Next, we replicate the results of Mian et al. (2013) by building a ZIP code-level data from our individual-level origination data. To do this in the most representative way possible, we use all consumers in CCP and not just the matched CRISM sample. Let $y_{i,z,t} \in \{0, 1\}$ denote whether or not individual i who lives in ZIP z originated an auto loan in year t. Then our measure of consumption in ZIP z and year t is $C_{z,t} \equiv S_t \frac{\sum_{i \in z} y_{i,z,t}}{h_{z,t} \sum_{z'} \sum_{i \in z'} y_{i,z',t}}$, which again allocates S_t to ZIP codes and normalizes by the number of households in that ZIP. We obtain the following result:

$$C_{z,2009} - C_{z,2006} = \alpha + \underset{(0.0004)}{0.012} \Delta HP_z^{2006-2009} + \varepsilon_z \text{ with } R^2 = 0.088, \text{ and } N = 6,217,$$

which also shows a highly significant effect: \$12 for every \$1,000.[19] In our data, the weighted average of the house price decline is \$41,706, and this implies a change in consumption from 2006 to 2009 at the mean of $\overline{\Delta C^{ZIP}} = -\$506$, which is about 60% of the number reported in Mian et al. (2013). We believe that the majority of the difference between their estimate and ours stems from the differences between the "new car registrations" concept in Mian et al. (2013) and the "auto loan originations" concept we use: the former contains registrations by businesses and omits used cars. Therefore their estimates will be biased upward to the extent that people substitute used cars for new cars during a recession. The average house

[19] We have 46 fewer ZIP codes relative to Mian et al. (2013). The difference may be due to the fact that we do not need to restrict to ZIP codes represented in the Polk new car registration data and we also use a more recent release of the CoreLogic Solutions house price index, which affects the availability of the house price index used in the analysis.

price decline in their sample is also 10% larger, which further contributes to the difference.

Now we turn to our first individual-level results, in a more stylized and simplified form. While our main analysis takes an IV approach, we use an ordinary least squares linear probability model for this illustration. One of the key differences with respect to the ZIP-level regressions above (that used house *prices*) is that we are now able to relate individual-level change in house *values* (in \$100,000, denoted $HV_i^{2006-2009}$) to the likelihood of originating an auto loan in 2009, by using the matched CRISM sample of mortgage holders.[20] Using i to denote an individual and z the ZIP code they live in, the results of our estimation are:

$$\begin{aligned} y_{i,2009} &= \gamma + \underset{(0.0007)}{0.0125}\Delta HV_i^{2006-2009} + \delta y^*_{i,2004-2006} + \sigma C_{z,2003} + \varepsilon_i \qquad (1) \\ &\text{with } R^2 = 0.023, \text{ and } N = 349{,}030. \end{aligned}$$

In this specification, in addition to the change in house value, we have two additional controls: the first one, $y^*_{i,2004-2006}$, is a collection of categorical dummy variables that show how many auto loans the individual has originated between 2004 and 2006 and the second one, ($C_{z,2003}$), is the same ZIP-level measure we used above, but for the year 2003. The former controls the individual-specific frequency of buying a car – we find that those individuals who originated more auto loans in the period 2004-2006 are more likely to originate an auto loan in 2009, everything else equal. The latter controls for location-specific differences in the propensity to purchase a vehicle (as proxied by a "normal" year like 2003), which also positively affects the likelihood that an individual originates an auto loan in 2009.

The estimated coefficient in (1) shows that for every \$100,000 decline in house values, the probability of originating an auto loan falls by 1.25 percentage points. Using the average of individual-level house value changes in our data, which is \$78,136, the marginal effect at the mean is a decline of 0.98 percentage points. It is useful to convert this to a dollar value. In our data, in the year 2009, 13.5% fraction of households originate an auto loan. Based on Bureau of Transportation Statistics data, the average price of a car (new or used) in 2009 was \$12,518. Combining these we find that the change in consumption in 2009 at the mean house value change is $\overline{\Delta C} = \$12,518 \times \frac{0.0125\times(-\$0.78136)}{0.135} = -\$906$.

[20]Since this sample is restricted to mortgage borrowers, there are some differences with the general population, most notably higher average house values and better Risk Scores.

To sum up, in their ZIP code-level analysis Mian et al. (2013) find a decline of $855 in per-capita purchase of autos in response to the average decline in house prices. Our replication of their results using our data yields a decline of $506, which is smaller than the results from Mian et al. (2013). We attribute this difference to the more broad definition of consumption in our data. Our (simplified) individual-level results show a decline of $906. This is larger than in the ZIP-level analysis of all consumers because this estimation uses only homeowners with mortgages (about a third of U.S. households) who would be expected to react more to the change in house values than the general population that also contains renters. Nevertheless, our individual-level estimates replicate the large aggregate response of consumption to housing wealth changes that Mian et al. (2013) find. In the remainder of the paper our goal will be to demonstrate that, once other key channels (local general equilibrium and bank health) are controlled for, a large fraction of this consumption response is due to heterogeneity in credit constraints across consumers. Some consumers do not react at all to the change in housing wealth and some react many times larger than the average response, where all this heterogeneity will be accounted for by various credit constraints.

3.2 Individual-Level Empirical Strategy

Our full individual-level specification builds on the simplified one in (1). The most important difference is that instead of using OLS, we use instrumental variables with the two housing-supply instruments introduced in Section 2.4 serving as instruments for ΔHV_i. We do so primarily to eliminate the effects of an omitted factor, at either the ZIP code or county level, that might drive changes in house values and auto loan originations simultaneously, and also to address the possible endogeneity of changes in house values.

The other changes relative to the simplified version in (1) are as follows. First, we add a quadratic polynomial in age and also add non-housing net worth controls (using four bins as described in Section 2.1) to the specification. Second, we run the estimations over J categories where each category j corresponds to a particular cut of the general population. For example, we group consumers into eight categories based on their LTV ratio and their Risk Score, both as of 2006. We estimate the model for each category separately, allowing for all coefficients to vary across categories. Then we take the coefficient of interest for each category (typically the coefficients for the key regressors such as changes in house values) and aggregate to the full sample weighing each coefficient by the share of the category in the full sample. In the tables in the main text we report these aggregated coefficients, while the detailed estimation results are available in the Appendix. In all estimations standard errors are clustered at the ZIP code level.

4 Main Results

We start by demonstrating that a part of the large aggregate response of consumption to housing wealth changes is due to two indirect channels we highlight in Figure 1; the local general equilibrium feedback and bank health. We show our baseline results in Section 4.1 and consider their robustness in Section 4.2. Once we eliminate the effect of indirect channels, we then turn to identifying the direct effects of constraints in Section 4.3. We close this section by considering the dynamic effects of credit constraints in Section 4.4.

4.1 Decomposing The Aggregate Effect

As we explained in detail in the previous section, we estimate a separate equation via IV for a number of categories, which are mutually exclusive and collectively exhaustive. In this section we use eight categories, cutting the data two ways: by Risk Score (prime / non-prime) and by LTV ratio (the four LTV categories we defined earlier). Once we obtain the coefficients of interest for each of the eight categories, we aggregate them to the full sample using sample weights. The analysis in this section will be based on these aggregated responses because we want to measure the effect of including controls to identify the local general equilibrium and bank health channels. In subsequent sections we focus on group-specific results to highlight the effects of various credit constraints.

We start with a specification that, in addition to all the standard controls described earlier (quadratic age, non-housing net worth bins, 2004-2006 origination controls and 2003 ZIP code auto sales), has only individual-level Change in House Values (ΔHV). Next we add county-level Change in Unemployment Rate (ΔU) followed by Credit Supply (CS) to the specification. Detailed results for each group as well as all the first-stage results from

Table 3: Decomposition Summary Results

Marginal Effects			
	(1)	(2)	(3)
ΔHV	0.0165***	0.0143***	0.0111***
	(0.0020)	(0.0028)	(0.0031)
ΔU	-	−0.0024***	−0.0024***
		(0.0006)	(0.0006)
Credit Supply	-	-	0.0325***
			(0.0095)
Marginal Effects at the Mean (in p.p.)			
ΔHV (average: -$78,136)	-1.29	-1.12	-0.87
ΔU (average: -5.5 p.p.)	-	-1.33	-1.33
Credit Supply (-1 s.d.)	-	-	-0.29
Marginal Effects at the Mean (in Dollars)			
ΔHV (average: -$78,136)	-$1,193	-$1,037	-$804
ΔU (average: -5.5 p.p.)	-	-$1,231	-$1,230
Credit Supply (-1 s.d.)	-	-	-$265

Notes: First panel aggregates the estimates in Table A-2 using the sample weights. The second panel converts these marginal effects to marginal effects at the mean, by multiplying with the average of the respective variable, except for Credit Supply we look at the effect of a one standard deviation decline. The third panel converts the numbers in the second panel to dollar values by using $926 for each one percent decline. This is obtained by combining the average probability of originating an auto loan in 2009 and the average price of a car in 2009, which are 0.135 and $12,518, respectively.

the IV estimation are in Appendix B.1.[21] Whenever it is significant, the sign of ΔHV is positive, indicating that a decline in house values reduces our measure of consumption, the probability of originating an auto loan in 2009. Similarly ΔU has a negative sign – an increase in unemployment reduces consumption – and Credit Supply has a positive sign (except one instance where it is negative and only marginally significant) – a decline in credit supply reduces consumption. Table 3 presents the aggregate results in three different ways. The first panel shows the aggregated marginal effects across the eight groups using

[21]Both instruments enter the first stage with negative and highly statistically significant coefficients in virtually all estimations. This is the expected sign since larger values of the instruments indicate more inelastic supply and a large negative aggregate housing demand shock, such as the one that happened between 2006 and 2009, would lead prices to fall by more in areas with more inelastic housing supply. The first-stage F-statistics are all very large, with the smallest one at 419. These are well beyond the critical values provided by Stock and Yogo (2005) and thus tests of weak identification and, separately, tests of underidentification are all easily rejected.

sample weights. For example the weighted average of the ΔHV marginal effect when it is the only regressor is 0.0165, indicating a decrease of 1.65 percentage points in origination propensity in response to a \$100,000 decline in HV. Recall that in 2009 13.5% of consumers originated an auto loan. The second panel evaluates the marginal effects at the means of the respective variables for ΔHV and ΔU, which are a house value decline of \$78,136 and an unemployment rate change of -5.5 percentage points. For Credit Supply, since it is a flow variable in levels, and its mean is roughly zero, we consider the marginal effect of moving one standard deviation below the mean. Finally in the last panel we convert these marginal effects to dollar values using the same method we used in Section 3.1: in 2009 the fraction of households that originated an auto loan is 0.135 and the average value of a car was \$12,518. Combining these, a one percentage point decline in the probability of buying a car amounts to a decline of \$926 in auto purchases. We multiply the marginal effects in percentage points in the second panel by this amount to get the dollar values in the third panel.

Two important conclusions emerge from this table. First, the importance of the decline in House Values shrinks with the addition of the Change in Unemployment Rate and Credit Supply: in dollar values, the effect of the average decline in house values goes down by \$389 from \$1,193 to \$804 once the other variables are included. Second, the other two variables, particularly Change in Unemployment Rate, are also important contributors in their own right to the decline in consumption: looking at column (3), the average increase in unemployment rate reduces consumption by \$1,230, at par, if not larger than the effect of the change in house values. A one standard deviation decline in Credit Supply reduces consumption by \$265.

Finally, using a methodology adapted from the derivations of the omitted variable bias, we can decompose the overall effect of the change in house values on consumption into its various channels as we identified in Figure 1. The methodology is explained in detail in Appendix A. We calculate that the Local General Equilibrium channel accounts for 24% and Household Credit Supply channel accounts for 15% of the overall response. This leaves 62% as the **direct** response of consumption to the change in house values, which is a combination of the pure wealth effect and the effect of Household Financial Constraints.[22] This 62%

[22]Numbers do not add up to 100% due to rounding.

Table 4: **Robustness of Main Results (Marginal Effects at the Mean, in p.p.)**

Main Results			
ΔHV (average: -$78,136)	-1.29	-1.12	-0.87
ΔU (average: -5.5 p.p.)	-	-1.33	-1.33
Credit Supply (-1 s.d.)	-	-	-0.29
Bank Health Instead of Credit Supply			
ΔHV (average: -$78,136)	-1.29	-1.12	-0.86
ΔU (average: -5.5 p.p.)	-	-1.33	-1.59
Bank Health (+1 s.d.)	-	-	-0.33
Probit-IV			
ΔHV (average: -$78,136)	-1.25	-0.90	-0.82
ΔU (average: -5.5 p.p.)	-	-1.54	-1.35
Credit Supply (-1 s.d.)	-	-	-0.31

Notes: First panel repeats the results in the second panel of Table 3. The other panels report the summary results analogous to the first panel with the change described in the title.

covers both the effect of credit constraints and the pure wealth effect. We turn to the detailed analysis of the former in Section 4.3 and the latter in Section 5.

4.2 Robustness of the Decomposition of the Aggregate Effect

We consider two variations to investigate the robustness of our results presented so far. First, we replace Credit Supply with Bank Health. Second, we use a Probit instead of a linear probability model in the second stage of our IV. In Table 4 we present the marginal effects in percentage points for the baseline results and these two variations. Full results are presented in Appendix B.2. These results confirm the robustness of our main results. Considering the specifications with Bank Health, obviously the only changes that should be expected are in the third column. The response to ΔHV is almost unchanged while the response to ΔU is modestly larger. The decomposition into channels is largely unchanged at 29% for Local General Equilibrium, 11% for Household Credit Supply and 61% for Direct House Value Response. When we use probit instead of a linear probability model, there are small changes in the estimated marginal effects and these change the decomposition to 25%, 15% and 59%, respectively.

4.3 Household Financial Constraints and House Values

In contrast to previous work, we categorize financial constraints in two ways: ex-ante and ex-post. By ex-ante financial constraints, we mean those that affected consumers in 2006 or earlier, before house values declined. These constraints shaped the decisions the consumers made at the time, which then directly or indirectly led to different levels of vulnerability to changes in house values. Ex-post constraints are those that are tightened by the decline in house values, and in turn make it harder for the consumer to get access to credit. We consider one observable measure of ex-post constraints in our data, which we label Bad Mortgage. This is a binary variable that captures whether the consumer has been seriously delinquent (at least 90 days behind) in any mortgage payment or has experienced a foreclosure at any point in 2007-2009. As we demonstrate below, this variable is very strongly associated with house value changes and is an important predictor of consumption in 2009. Next we investigate the effect of ex-ante constraints by focusing on the effect of change in house values on the consumption of various subsamples of consumers, controlling for all standard controls as well as Bad Mortgage. The categories are created by the LTV or the Risk Score of the consumer in 2006 as well as the type of mortgage the consumer held as of 2006. We explain below how each of these are linked to particular ex-ante constraints.

4.3.1 Ex-Post Financial Constraints

We start by demonstrating how Bad Mortgage links changes in house values to consumption. To summarize our argument, we demonstrate that: (a) house value declines have a very strong effect on Bad Mortgage, (b) Bad Mortgage increases the likelihood of a major decline in the borrower's Risk Score and (c) this decline makes it less likely that the consumer originates an auto loan.

As a mortgage delinquency has a large effect of Risk Scores, in this section we use a slightly finer categorization of Risk Score to demonstrate how the low end of the Risk Score distribution is affected. In particular, we split the Non-Prime group we defined earlier – those with Risk Scores less than 700 – into two groups, using 600 as the cutoff between the two. For the purposes of this section we label the group with a Risk Score less than 600 as

Table 5: Ex-Post Financial Constraints – Bad Mortgage and Deep Subprime

(a) ΔHV between 2006-2009 Predicts Bad Mortgage

	LTV0	**LTV1**	**LTV2**	**LTV3**
Risk Score 2006 < 600	9.2**	13.0***	24.9***	24.9***
Risk Score 2006 $\in [600, 700)$	3.6***	5.8***	16.0***	24.1***
Risk Score 2006 ≥ 700	0.4**	1.2***	4.6***	8.2***
Marginal Effect of Average ΔHV: 5.8 p.p.				

(b) Bad Mortgage Predicts Deep Subprime in 2009

Without Bad Mortgage	**LTV0**	**LTV1**	**LTV2**	**LTV3**
Risk Score 2006 < 600	3.7	6.1***	6.4***	11.6***
Risk Score 2006 $\in [600, 700)$	5.0***	5.7***	13.0***	17.1***
Risk Score 2006 ≥ 700	0.4**	1.0***	4.1***	6.4***
Marginal Effect of Average ΔHV: 4.2 p.p.				

With Bad Mortgage	**LTV0**	**LTV1**	**LTV2**	**LTV3**
Risk Score 2006 < 600	−0.2	0.6	−4.6**	0.2
Risk Score 2006 $\in [600, 700)$	2.8*	2.1**	3.0***	1.4
Risk Score 2006 ≥ 700	0.2	0.4**	1.4***	1.8***
Marginal Effect of Average ΔHV: 0.76 p.p.				
Marginal Effect of Having Bad Mortgage: 56.6 p.p.				

Notes: The table shows the marginal effects of an average change in house values on various outcomes, in p.p. Each number is obtained from a separate IV estimation with all standard controls. These are shown in Appendix B.3.1 and B.3.2. In panel (a) the outcome is having a bad mortgage, whose unconditional probability is 7.6% and it varies from 0.8% for Risk Score ≥ 700 / LTV0 group to 47% for the Risk Score < 600 / LTV3 group. In panel (b) the outcome is being Deep Subprime (having Risk Score less than 600) in 2009, whose unconditional probability is 10.4% and it varies from 1.1% for Risk Score ≥ 700 / LTV0 group to 65% for the Risk Score < 600 / LTV3 group. In panel (b) two versions are shown: one without Bad Mortgage as a control and one with.

Deep Subprime. Of the 25.4% of consumers who are classified as Non-Prime in 2006, 7% are Deep Subprime. For 2009, 24.2% of consumers are Non-prime, of which, 10.4% (or 43%) are Deep Subprime.

Table 5 shows the first set of results that establishes our findings, which are obtained with the same IV strategy and the controls used in the previous section. In panel (a) we use Bad Mortgage as the dependent variable. In panel (b) we use Deep Subprime 2009 as the dependent variable. We present two sets of results, with and without Bad Mortgage as a control. The detailed results from these estimations are shown in Appendix B.3. In Table 5 we summarize the results using the marginal effect of the average change in house value, expressed in percentage points, analogously to the first panel in Table 3, but now broken down for each category – 12 in this case.

The unconditional probability of Bad Mortgage is 7.6%. As panel (a) shows, the change in house values between 2006 and 2009 has a very strong effect on Bad Mortgage, especially for consumers with a low Risk Score and high LTV ratio. For example those in the lowest Risk Score category and the highest two LTV categories face an increase of almost 25 percentage points in their probability of Bad Mortgage when they face the average decline in house values. The effect almost uniformly declines as LTV ratio falls and as Risk Score increases, falling to only 0.4 percentage points for consumers with the highest Risk Score and lowest LTV categories. This shows an interesting interaction between ex-ante constraints and house value changes in triggering ex-post constraints.

The first part of panel (b) in Table 5 shows how the average change in house values between 2006 and 2009 affects the probability of being Deep Subprime in 2009 for each Risk Score and LTV category combination. The unconditional probability of Deep Subprime is 7%. The change in house values has a very large effect, as large as 17 percentage points for those with Risk Scores in the middle range and high LTV ratios. When aggregated, the overall effect of the house value change is a sizable 4.2 percentage points. In the bottom part of panel (b) we include Bad Mortgage in the estimation. There are a number of interesting outcomes. First, Bad Mortgage itself increases the probability of being Deep Subprime by about 57 percentage points (bottom row). Second, the overall effect of house value changes falls by about 82% to 0.8 percentage points. Third, once again there are interesting interactions with ex-ante constraints. For example when Bad Mortgage is included, house value changes are no longer significant for the Deep Subprime category, but continue to be significant for the two non-prime categories, albeit with smaller coefficients.[23] Similarly,

[23]It is important to emphasize that this result seems to also be relevant for the debate in the literature in terms of whether during the boom period mortgage borrowing was driven by prime or subprime borrowers. Albanesi et al. (2017) show that borrowing in subprime ZIP codes is driven by prime borrowers in those ZIP

Table 6: Ex-Post Financial Constraints – Bad Mortgage and Consumption

Decomposition Revisited
Converted to Marginal Effects at the Mean (in p.p.)

	(1)	(2)	(3)	(4)
ΔHV (average: -$76,000)	-1.29	-1.12	-0.87	-0.32
ΔU (average: -5.3 p.p.)	-	-1.33	-1.33	-1.36
Credit Supply (-1 s.d.)	-	-	-0.29	-0.32
Bad Mortgage	-	-	-	-8.51

Notes: Table shows the decomposition of the overall house value effect into channels, extending the middle panel of Table 3 to include Bad Mortgage. Results that underlie this decomposition are in Appendix B.3.4.

for the middle Risk Score group when Bad Mortgage is omitted, as LTV ratio goes up the effect of house value changes increases, while with Bad Mortgage the influence of house value changes is roughly flat and becomes insignificant for the largest LTV group. In fact when we aggregate the marginal effects only for the LTV3 group, they go down from 9.3 percentage points without Bad Mortgage to 1.5 percentage points with Bad Mortgage (not shown in the table).

The final step of the argument involves returning to our baseline estimation where we use originating an auto loan in 2009 as the dependent variable. Table 6 revisits the decomposition of the overall house value effect in Table 3 while adding a fourth column to the second panel of Table 3 that includes Bad Mortgage as a control. For comparability, for these results we return to the specification with two Risk Score categories. They show that while Change in Unemployment Rate and Credit Supply maintain the magnitude of their effects, the marginal effect of Change in House Values is about a third of what it was without the Bad Mortgage control. Originally, we showed that without other controls the average change in house values led to a decline of $1,193 in 2009 consumption, which fell to $804 once Change in Unemployment and Credit Supply were controlled for. Now with Bad Mortgage also controlled for, the effect of house value change falls to $301. The estimate for Bad Mortgage

codes, whereas borrowing by subprime individuals was constant in the boom period. Our result of prime borrowers turning into non-prime because of a decline in house values is consistent with these findings, given that prime borrowers did most of the borrowing during the boom period, and they then fell behind on their payments.

shows that experiencing the delinquency captured by this variable reduces the probability of originating an auto loan by 8.5 percentage points, which is almost two thirds of the unconditional probability. Turning to the decomposition into channels, the direct response in the previous results is now split into the effect of Bad Mortgage at 36%, leaving 23% to ex-ante constraints, other ex-post constraints and the pure wealth effect.[24]

4.3.2 Ex-Ante Financial Constraints

Having examined ex-post constraints, we now turn to the remaining impact of ex-ante constraints. Our detailed individual-level data allows us to cut the data in various ways to identify ex-ante financial constraints. We consider three ways of observing these constraints at work: Risk Score, LTV ratio and the type of mortgage, all measured in 2006. Being Non-Prime indicates the presence of some prior adverse credit activity, which can directly limit future credit access. It can also reflect other (unobserved) financial constraints that may make future credit access more difficult. LTV ratios in 2006 directly reflect the severity of one of the most important financial constraints, the collateral constraint of a mortgage. The higher the LTV ratio, the more constrained the consumer, and thus the more vulnerable they are to house value changes.[25]

To see how mortgage type is a sign of ex-ante constraints, it is important to keep in mind that borrowers are not allocated randomly to different mortgage types, but they select the mortgage that best suits their situation, including financial constraints they face. For example, borrowers with Closed-End Second Mortgages typically get these mortgages because they lack the resources to make a 20% down payment, the standard amount in most mortgages. Further analyzing the distribution of consumers in Table 2, we see a few more interesting patterns that suggest choices by consumers. For example, short-maturity ARMs seem to be chosen by prime low-LTV borrowers (perhaps because they intend to pay off their loan in a short period of time) or Non-Prime moderate-LTV borrowers (perhaps because this was the only product they qualified for and they hope to refinance before the ARM resets).

[24] These two numbers add up to 59%, which is slightly different from the 62% we report in Section 4.1 due in part to differences in the underlying IV estimates when Bad Mortgage is included.

[25] Furthermore with a higher LTV ratio the consumer is more likely to be "under water" – have negative equity in the house – and thus fall behind in his payments, or simply walk away from the house. However, this is already captured by the Bad Mortgage variable we used in the previous section.

Table 7: Ex-Ante Financial Constraints

(a) Risk Score and LTV

	Prime	Non-Prime
LTV0 (First-mortgage LTV $<$ 25%)	0.14	-1.58
LTV1 (First-mortgage LTV between 25% and 50%)	0.01	-1.36^{*}
LTV2 (First-mortgage LTV between 50% and 80%)	-0.69	0.38
LTV3 (First-mortgage LTV $\geq$ 80%)	0.83	-3.63^{**}

(b) Mortgage Type

	Prime				Non-Prime			
	LTV0	**LTV1**	**LTV2**	**LTV3**	**LTV0**	**LTV1**	**LTV2**	**LTV3**
Fixed Rate	–0.14	-0.07	–0.67	0.59	–2.95**	–2.44**	–1.50	–3.88
ARM $<$ 5yr	2.53	3.76	–1.63	1.75	–5.17*	–2.87	0.73	0.05
ARM $\geq$ 5yr	0.54	–2.27	0.86	–5.53	9.49	3.35	0.88	–3.05
CE Second	4.71*	–0.68	–2.75	–0.58	–4.08	–1.27	3.14	–10.32**
HELOC	–0.40	0.09	–0.63	2.76*	1.49	1.08	3.41*	–5.69

Notes: Table shows the marginal effects of an average change in house values on originating an auto loan in 2009 in p.p., whose unconditional probability is 13.5% and it varies from 9.4% for the Non-Prime / LTV0 group to 16.4% for the Non-Prime / LTV3 group. Prime status, LTV category and mortgage type are all measured as of 2006. Each number is obtained from a separate IV estimation with all standard controls including Bad Mortgage. These are shown in Appendix B.3.5.

HELOCs seem to be favored by Prime borrowers with low-to-moderate LTV ratios. It is plausible that these consumers use the extra liquidity from their HELOCs to finance some consumption expenditures. Thus, a decline in house values would make their constraints bind, since banks can (and did) reduce HELOC limits for consumers with increased LTV ratios.[26]

To sum up, all three of these characteristics have implications about how easy it is for the consumers to refinance their mortgage, how likely it is for them to default and more generally how much their consumption would be affected by changes in house values. Panel (a) of Table 7 shows how each of the eight categories of LTV and Risk Score react to House

[26]One may be tempted to think consumers can use cash they get from their HELOCs to finance an auto purchase completely without the need for an auto loan. If this was the case, then it is not clear how we could identify our results using auto loan originations for people with HELOCs. Results reported by McCully et al. (2019), however, show — using data from three nationally representative surveys — that very few consumers purchase cars outright using HELOCs or cash-out refinancing.

Value Change once all controls including Bad Mortgage are included. Each coefficient is obtained from a separate IV estimation, which is reported in Appendix B.3.5 and the table reports them as marginal effects at the average of House Value Change in percentage points. The results show that Prime homeowners do not react to changes in house values, regardless of LTV ratio, once we control for Bad Mortgage. Only the highest LTV category for Non-Prime homeowners shows a significant reaction (at 5% significance) at -3.63 p.p., which is over eleven times the average response. This small group of 4% of consumers makes up about 60% of the overall response.

Panel (b) shows a deeper cut of the results where we also condition on the type of mortgage the consumer held in 2006. It is useful to interpret the results alongside the distribution of characteristics reported in Table 2. We find the following results noteworthy. Consumers that are Prime that only have a Fixed Rate first mortgage are about 41% of the population, and they show no reaction to Change in House Values (regardless of LTV, after controlling for Bad Mortgage). In fact with the exception of two marginally significant responses, Prime consumers, 74.6% of the population, do not react to Change in House Values, which is consistent with the results in panel (a). Focusing on the three estimates from Non-Prime consumers that are significant with at least 5% significance, (Fixed Rate LTV0 and LTV1 and CE Second LTV3), even though they are only 7% of the population, they contribute about half to the overall marginal effect attributed to ex-ante constraints. Notably the Non-Prime CE-Second LTV3 group, which is only 0.4% of the population, has a reaction that is over 22 times the average.

Our reading of these results is as follows. Only a negligible part of the overall response comes from Prime consumers with a Fixed Rate, arguably those that are least likely to have ex-ante constraints. In fact none of the groups with Prime consumers show a significant response. The three most important groups are all those that have significant ex-ante constraints: they are Non-Prime and some of them have a Closed-End Second mortgage. The other marginally significant responses come from those that have situations that suggest that they may have been constrained when they originated their mortgage: some have ARMs with a reset horizon of less than 5 years, and others have a HELOC.

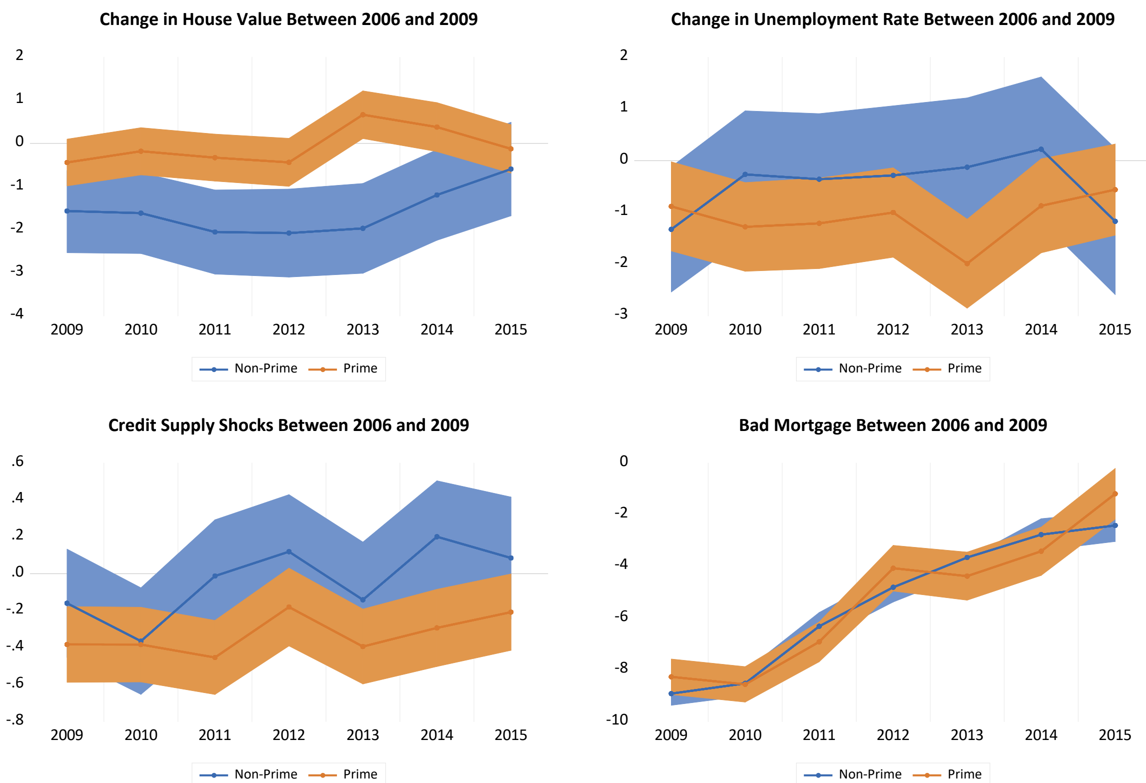
4.4 Dynamic Effects of Credit Constraints

It is well known that financial constraints have long-lasting effects. To see this in our data, we repeat our baseline estimations replacing the dependent variable with origination of auto loans in subsequent years, while keeping the explanatory variables unchanged. That is, we investigate if house value changes between 2006 and 2009 have long-lasting effects that stretch beyond 2009, and if so whether we can find evidence of continuing effects of credit constraints. We proceed as we did for the baseline results: for each year, we estimate eight models, one for each Risk Score-LTV category as of 2006 and for each variable aggregate the marginal effects at the mean.

Figure 4 shows the results, broken down by Prime and Non-Prime in 2006. Each panel shows the marginal effect at the mean for each of the four key explanatory variables: Change in House Values, Change in Unemployment Rate, Credit Supply (for a one standard deviation decline, as before) and Bad Mortgage, all computed from 2006 to 2009 as in the benchmark specification. The first observation in each panel is the benchmark results for auto loan originations in 2009 as used in Section 4.3.1 and reported in Table A-9.

Change in House Values between 2006 and 2009 continues to affect the consumption choices of borrowers who were non-prime in 2006 until at least 2014 at roughly the same level as it did in 2009 – a decline of over 2 p.p. in the probability of an auto loan origination. Prime borrowers in 2006, whose 2009 consumption wasn't affected by house value changes, continue to show little reaction in later years – in fact there seems to be a bounceback in 2013. There does not seem to be much of an effect of the Change in Unemployment Rate or Credit Supply for non-prime consumers in 2006, but for prime consumers in 2006 both variables seem to depress consumption in later years. Turning to Bad Mortgage, a very clear picture emerges. Those homeowners who experienced a Bad Mortgage between 2006 and 2009 continue to have a significant decline in their probability of auto loan origination through 2015, though the effect gets smaller over time. The effect of Bad Mortgage is much larger in magnitude than that of any of the other variables we consider. These results show that the effects of both ex-ante credit constraints (comparing Prime in 2006 with Non-Prime in 2006) and ex-post credit constraints (Bad Mortgage) are very persistent and continue to

Figure 4: Dynamic Effects of Credit Constraints



Notes: The figures show the marginal effects at the mean for each explanatory variable, as shown in each panel. See text for the explanation of how the results are obtained.

influence outcomes for many years – even after 7 years, Bad Mortgage (in 2007-2009) reduces the probability of an auto loan origination in 2015 by over 2 p.p.

5 Identifying the Pure Wealth Effect

To take stock of the results so far, we showed that there is a large response of consumption to house value changes and that this can be decomposed into various channels. Local General Equilibrium and Credit Supply channels jointly capture 41% of the overall effect. Ex-post constraints proxied by Bad Mortgage are responsible for 36%, which leaves 23% for ex-ante constraints and the pure wealth effect. In the previous section we demonstrated the importance of the combined effect of pure wealth effects and ex-ante constraints. In this section we show that the pure wealth effect is in fact neglible.

We do this in two ways. In Section 5.1 we repeat our baseline estimation for various subsets of consumers for which we would expect, a priori, that credit constraints should not be important. Thus, if these consumers display a reaction to house value changes, it would likely be due only to the pure wealth effect. In Section 5.2 we use the panel structure of the CCP dataset, which has information on non-mortgage holders as well. Once again, a priori, the expectation would be that consumers who own a house without a mortgage would be less likely to be affected by credit constraints and any consumption response would reflect the pure wealth effect. Our results will show that in all of these cases there is indeed no consumption response.

5.1 Cross-Section Subsample Results

Our first approach to identifying the pure wealth effect relies on the assumption that a consumer who was Prime and had an LTV ratio less than 25% in 2006 would be unlikely to be affected by ex-ante credit constraints. Under this assumption, once we estimate our benchmark specification that includes Bad Mortgage, the response to change in house values reflects the pure wealth effect.

Table 8 reports the coefficient for Change in House Values in a series of subsamples. The first row shows the key subsample for our argument, which is Prime consumers with an LTV ratio less than 25%, LTV0. The estimate -0.0018 has a p-value of 0.76 and it is clearly insignificant.[27] This is our key evidence that the pure wealth effect is negligible. The next two rows show the results when we replace Credit Supply with Bank Health and when we use Probit instead of a linear probability model. In both cases the estimates are insignificant.

There are two possible concerns with this identification, both of which arise from the fact that Prime and low LTV status are not random. First, someone who brought their first mortgage LTV ratio to a low level may in fact have enough liquid wealth to buy a car without an auto loan. For these individuals we may incorrectly conclude that they did not consume (purchase a car) even though they may have done so using cash. Returning to the

[27] It is perhaps useful to point out that even without the controls of Change in Unemployment, Credit Supply and Bad Mortgage, for this group of consumers, the coefficient of House Value Change is 0.0036 with a p-value of 0.27.

Table 8: Identifying the Pure Wealth Effect

Sample	ΔHV Coefficient	Number of Obs
LTV0-Prime (Benchmark)	-0.0018	51,059
	(0.0058)	
LTV0-Prime (with Bank Health)	-0.0080	51,059
	(0.0069)	
LTV0-Prime (using Probit)	0.0001	51,059
	(0.0058)	
LTV0-Prime, Non-Housing Net Worth $\leq 25^{th}$ Pct	0.0034	3,475
	(0.0183)	
LTV0-Prime, Non-Housing Net Worth $\in (25, 50]$ Pct	-0.0025	5,908
	(0.0153)	
LTV0-Prime, Non-Housing Net Worth $\in (50, 75]$ Pct	-0.0169	15,288
	(0.0137)	
LTV0-Prime, Non-Housing Net Worth $> 75^{th}$ Pct	0.0017	26,388
	(0.0079)	
LTV0-Prime, Age < 41	-0.0396	2,866
	(0.0285)	
LTV0-Prime, Age $\in [41, 60]$	-0.0027	29,199
	(0.0078)	
LTV0-Prime, Age > 60	0.0058	18,994
	(0.0092)	

first row in Table 8, the estimation includes non-housing net worth buckets as well as the age polynomial, as it is standard in all our models. The coefficients for non-housing net worth buckets are all insignificant. Similarly the fitted value of the age polynomial is fairly flat. Both of these results indicate that once we control for all standard controls, auto loan originations do not vary by non-housing net worth or age (a wealth proxy). These results address this concern, because we show that the likelihood of origination for the Prime-LTV0 group does not vary by wealth. Thus it is unlikely that we would be missing the auto purchases of these individuals any more than we would miss them for a random consumer.

The second concern is that an individual who has a low LTV ratio may have high non-housing wealth and thus the decline in housing wealth may constitute a small share of their overall wealth. To address this concern, the rest of Table 8 shows the same marginal effects for two sets of subsamples, one broken down first by non-housing net worth, and next by age. None of the marginal effects are significant, which indicates that the response to changes in house value does not vary with wealth or age and remain insignificant. Thus we conclude that there is no evidence of a significant pure wealth effect.

5.2 Panel Results

In this section we take a different approach to identifying the pure wealth effect. Our analysis thus far has focused on homeowners with a mortgage, in large part in order to utilize the detailed data we have in CRISM, including individual-level House Value Change. However, this meant we had to leave out an important group of homeowners, one that can help significantly in identifying the pure wealth effect: homeowners without a mortgage, whom we term free-and-clear homeowners. In this section we use panel data from CCP covering the period 2002-2010, which will allow us to capture this group of homeowners. While we can no longer use house value changes at the individual level and have to rely on house price changes at the ZIP code level, and we do not have non-housing net-worth controls, the panel structure addresses these issues through the use of individual fixed effects. Moreover, given that the period 2002-2010 contains house price increases as well as decreases, we are able to investigate the asymmetry of the house price effect on consumption.

Unfortunately, the credit bureau data does not contain any direct information on the homeownership status of consumers. As such, we identify two categories of consumers that we are fairly certain are homeowners (and not renters, for example) and limit the analysis to these consumers: mortgage holders and free-and-clear homeowners.[28] We also use the Risk Score of the consumers in our analysis. Similar to our analysis earlier, we define a consumer as Prime in year t if their Risk Score in Q4 of year t is greater than 700 and Non-Prime otherwise. Obviously this indicator is allowed to change over time as the Risk Score of the consumer changes. At the end, we generate four categories combining the two

[28]Mortgage Holders are identified as follows: in year t, a consumer is identified as a mortgage holder if they have the same address in Q4 of year $t-1$ and Q4 of year t and they have exactly one first mortgage on their credit record in Q4 of year t. To identify free-and-clear homeowners, we use the following algorithm. If the consumer was not a Free-and-Clear Homeowner in year $t-1$, then if they have a mortgage on their record in Q4 of year $t-1$ and no mortgage in Q4 of year t, they do not have a mortgage foreclosure in year $t+1$ (so the lack of a mortgage reflects paying it off), and their address is the same in Q4 of year $t-1$ and Q4 of year t, then they are labeled as a free-and-clear consumer. If the consumer was identified as a Free-and-Clear Homeowner in year $t-1$, then as long as they continue to have no mortgage in Q4 of year t, they do not have a mortgage foreclosure in year $t+1$ and their address is the same in Q4 of year $t-1$ and Q4 of year t, then they are again labeled as a Free-and-Clear Homeowner in year t.

homeownership categories in a year with the two Risk Score categories in the previous year, paralleling our "ex-ante" analysis in the rest of the paper.

We conduct our analysis using a linear probability model with an IV strategy. The dependent variable is whether or not the consumer originates an auto loan in the current year. We use two separate house price variables, one for house price increases and one for decreases, to capture the possible asymmetric effect of house prices on consumption. Our IV strategy adapts to the panel structure. Following Aladangady (2017), we interact the two housing supply instruments we used throughout the paper with a national variable that captures shifts in housing demand and use them to instrument for the two house price growth variables. In Aladangady (2017), this national variable is the 10-year real interest rate, which is negatively correlated with the national house price changes in his sample of 1985-2008. This correlation is in fact stronger before the 2000s and in the 2000s it turns strongly positive. This suggests that it is likely not a good housing-demand shifter in the period of our analysis. Instead we use a measure of mortgage credit availability that we create along the lines of Anenberg et al. (2019). This measure is created using data on mortgage originations from Black Knight McDash and Corelogic Solutions and it is steady around 55% in the early 2000s, falling drastically between 2007 and 2010 and reaching 25% by the end of the sample. This pattern closely matches that of national house price growth, with a correlation of 0.84.

Table 9 shows the panel estimation results. We show four results, one for each of the subsamples. In all the first stages for Positive Housing Price Growth, the instruments have a positive sign, and for Negative Housing Price Growth, they have a negative sign. This is consistent with the earlier results from a linear model where, when faced with a positive demand shock (that increases prices nationwide), areas with more inelastic housing supply see large price increases and this is reversed when the demand shock turns negative. The table also shows the first-stage F-statistics that are in the thousands, well clear of any threshold for the relevant statistical tests. All estimations include Change in Unemployment Rate and Credit Supply as well as individual, ZIP code and year fixed effects. Standard errors are clustered at the individual level.

The first two columns show the results for Free-and-Clear Homeowners: neither positive

Table 9: Panel Results

	Prime Free-and-Clear	Non-Prime Free-and-Clear	Prime Mortgage Holder	Non-Prime Mortgage Holder
Positive House Price Growth	0.1755	−0.0382	0.5412***	0.7463***
	(0.2427)	(0.2574)	(0.1094)	(0.1377)
Negative House Price Growth	0.3323	−0.4006	0.7870***	0.9054***
	(0.2130)	(0.2437)	(0.1160)	(0.2799)
First-Stage F-statistics	9344 / 6207	4870 / 2568	53664 / 29639	24305 / 3868
ΔU and Credit Supply	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	396,947	262,285	1,828,786	758,800

Notes: Each column shows the results of an IV estimation. The dependent variable is a binary variable showing if the consumer originated an auto loan in a particular year. The mean of the dependent variables across columns are: 0.077, 0.098, 0.139, and 0.164. See the text for details.

nor negative House Price Growth over the last year leads to a significant change in the probability of originating an auto loan. This result, once again, shows that pure wealth effect is not important in shaping reaction of consumption to changes in house prices. Turning to the last two columns, we find that for Mortgage Holders there is a significant effect of House Price Growth of both signs on consumption. If we convert the marginal effects reported to those at the mean, the effects are large: they range between 2.1 percentage points and to 3.4 percentage points. Recall that Table 7 showed that for Prime consumers there was no significant effect of House Value Changes, which seems to contradict the results in the third column of Table 9 at first pass. However, Table 9 uses auto loan origination across the nine years in the panel sample, and there seems to be more evidence of an effect of House Price Growth when more years are considered. Moreover, unlike the results that underlie Table 7, the specification in Table 9 does not include Bad Mortgage. One novel finding in this table is that when we test the equality of the estimates for Positive and Negative House Price Growth, we reject this for Prime Mortgage Holders and fail to reject for Non-Prime Mortgage Holders. This is yet another sign of financial constraints. For Prime consumers when house prices increase by one percent, the probability of originating an auto loan goes up by 0.54

percentage points, but if the house prices fall by one percent, then this probability goes down by 0.79 percentage points. This is consistent with a decline in house prices triggering an (ex-post) constraint as shown earlier in the paper, and is also along the lines of the asymmetry results that arise in the macro-housing model of Garriga and Hedlund (2021).

6 Conclusion

We set out to empirically investigate the role of household heterogeneity in terms of wealth and financial constraints to quantify the role of the different channels linking declines in house values to decline in output, focusing on durable consumption during the period 2006-2009. Unlike most studies that focus on the link between house values and consumption we use individual-level data drawn from consumer credit bureau records linked to mortgage data. This allows us to use not only several key characteristics of consumers such as their age, their creditworthiness and the type of mortgage they have, but also the change in house values at the individual level. We proxy consumption by auto loan originations.

The change in house values has a large total effect on consumption – the average decline in house values between 2006 and 2009 leads to a decline of about \$1,200 in auto purchases in 2009. We decompose this effect into four channels: 25% for the Local General Equilibrium, 16% for the Household Credit Supply, 36% for the effect of ex-post financial constraints (captured by the Bad Mortgage variable) and 23% for the effect of ex-ante household credit constraints. We show that the pure wealth effect of house value changes on consumption is negligible. We also show that there is a large degree of heterogenity across households driven by financial constraints.

Our results have important theoretical and policy implications. We find that financial constraints households faced in the run up to the financial crisis have profound effects on their consumption during crisis, leading to a substantial aggregate response. A drop in house prices makes these households more likely to default on their mortgage, thereby lowering their Risk Score and making it more difficult for them to qualify for future credit. A countercyclical macroprudential policy, limiting household leverage during booms and relaxing it during the busts will smooth out this friction, limiting the aggregate response of consumption to declining house prices.

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Appendix (For Online Publication)

A Details of the Decomposition of the Overall Effect

In this Appendix we provide detailed derivations for the omitted variable bias that is used as the basis for the decompositions in Section 4.1. For completeness, we first go over the well-known OLS case, followed by the IV case with one instrument and finally the general case we use in the paper, which is IV with k instruments.

Consider the **true** model (model A), written in matrix form as

$$y = x_1 b_1^A + X_2 b_2^A + u^A \tag{A-1}$$

where y and x_1 are $N \times 1$ (N is the sample size), and X_2 matrix is $N \times n_2$ (n_2 is the number of variables in X_2). x_1 is the change in house values, and X_2 contains unemployment and credit supply variables, among others. Our full specification includes further controls as we explain in the main text. Thus, for the derivations here, y is the residual from a regression that contains all these controls on the right hand side and the individual origination variable on the left hand side. We assume, without loss of generality, all variables are demeaned. (Otherwise we can include a column of ones in X_2 and all arguments would go through.)

We also assume that x_1 and X_2 are related via

$$X_2 = x_1 \gamma' + w \tag{A-2}$$

where γ is a $n_2 \times 1$ vector and w is a $N \times n_2$ variable.

We also consider a model that drops X_2, which we call model B.

$$y = x_1 b_1^B + u^B \tag{A-3}$$

In what follows we first show the standard OLS omitted-variable bias derivations and then extend them to the IV case. However, instead of considering the omission of X_2 as a mistake, we interpret $\hat{b}_1^B$ as the *total effect* of x_1 on y and seek to decompose this into a *direct* *effect*, given by $\hat{b}_1^A$ and an *indirect effect*, that goes from X_2 to y via x_1.

In the derivations here we used a generic X_2 of size n_2. In our main application we have $n_2 = 2$; X_2 is composed of the Change in the Unemployment Rate and Credit Supply. In this case the bias will be the sum of two terms, each of which represents the contribution of one of the two variables in X_2 to the decomposition. Furthermore, we consider the indirect channel due to the Change in the Unemployment Rate to represent the Local General Equilibrium channel and the indirect channel due to Credit Supply to represent the Household Credit Supply channel.

A.1 OLS

We assume x_1 and X_2 are both exogenous satisfying $\mathbb{E}\left(x_1' u^A\right) = 0$ and $\mathbb{E}\left(X_2' u^A\right) = \mathbf{0}$. Moreover, $cov(w, u^A) = \mathbf{0}$ and $cov(w, x_1) = 0$. The OLS estimate for b_1^B is then given by

$$\hat{b}_1^B = (x_1' x_1)^{-1} x_1' y. \tag{A-4}$$

Using the definition of y in model A and using $\hat{u}^A$ to denote the residuals from the OLS estimation of (A-1), this can be written as

$$\begin{aligned} \hat{b}_1^B &= (x_1' x_1)^{-1} x_1' (x_1 \hat{b}_1^A + X_2 \hat{b}_2^A + \hat{u}^A) && \text{(A-5)} \\ \hat{b}_1^B &= \hat{b}_1^A + (x_1' x_1)^{-1} x_1' X_2 \hat{b}_2^A + (x_1' x_1)^{-1} x_1' \hat{u}^A && \text{(A-6)} \\ \hat{b}_1^B &= \hat{b}_1^A + \hat{\gamma}' b_2^A && \text{(A-7)} \end{aligned}$$

where in (A-6) the first expression is simplified because $(x_1' x_1)^{-1}(x_1' x_1) = I$, the second expression can be simplified using the OLS estimate of γ in (A-2) as $\hat{\gamma} = (x_1' x_1)^{-1} x_1' X_2$ and the last expression drops out because in the OLS estimation of (A-1), the condition $x_1' \hat{u}^A = 0$ holds. Thus (A-7) shows that the bias in the model A estimate is given by $\hat{\gamma}' \hat{b}_2^A$.

A.2 IV

Consider the same **true** model (model A) as in (A-1) but now we have $\mathbb{E}\left(x_1' u^A\right) \neq 0$, violating the key condition for OLS to be valid. Through (A-2), we see that $\mathbb{E}\left(X_2' u^A\right) \neq \mathbf{0}$

also must hold, but we assume $\mathbb{E}\left(w'u^A\right) = \mathbf{0}$. We have an instrument Z, which is collected in a $N \times k$ matrix that satisfies $\mathbb{E}\left(Z'u^A\right) = \mathbf{0}$. Note that (A-2) can no longer be estimated consistently via OLS since x_1 may be correlated with ω, or in other words $\mathbb{E}\left(x_1'w\right) \neq 0$.

The second stage of Model B is given by (A-3). In estimating this model, we ignore X_2 but we still instrument using Z. This means in the first stage we only use Z, and X_2 is omitted.

Define $P_Z \equiv Z(Z'Z)^{-1}Z'$ and the IV estimate for b_1^B is given by

$$\hat{b}_1^B = (x_1'P_Z x_1)^{-1}x_1'P_Z y. \tag{A-8}$$

A.2.1 IV - Single Instrument

We now focus on the case where $k = 1$, that is we have a single instrument. The IV estimate for b_1^B can be further simplified

$$\begin{aligned}
\hat{b}_1^B &= (x_1'Z(Z'Z)^{-1}Z'x_1)^{-1}x_1'Z(Z'Z)^{-1}Z'y && \text{(A-9)}\\
&= (Z'x_1)^{-1}(Z'Z)(x_1'Z)^{-1}x_1'Z(Z'Z)^{-1}Z'y && \text{(A-10)}\\
&= (Z'x_1)^{-1}Z'y && \text{(A-11)}
\end{aligned}$$

which we can do since $Z'Z$, $Z'x_1$ and x_1Z' are all square matrices of the same size. Note that, the IV estimator, written this way, solves for the b_1^B that satisfies $Z'\hat{u}^B = 0$. Similarly, though we do not explicitly write it down, the IV estimation of Model A sets $Z'\hat{u}^A = 0$ and $X_2'\hat{u}^A = 0$.

Using the definition of y in model A, this can be written as

$$\begin{aligned}
\hat{b}_1^B &= (Z'x_1)^{-1}Z'(x_1\hat{b}_1^A + X_2\hat{b}_2^A + \hat{u}^A) && \text{(A-12)}\\
\hat{b}_1^B &= (Z'x_1)^{-1}Z'x_1\hat{b}_1^A + (Z'x_1)^{-1}Z'X_2\hat{b}_2^A + (Z'x_1)^{-1}Z'\hat{u}^A && \text{(A-13)}\\
\hat{b}_1^B &= \hat{b}_1^A + \hat{\gamma}'b_2^A && \text{(A-14)}
\end{aligned}$$

where the last term in the second line drops out because $Z'\hat{u}^A = 0$ and $\hat{\gamma}'$ is the IV estimate of γ' in (A-2) with x_1 instrumented by Z, $\hat{\gamma}' = (Z'x_1)^{-1}Z'X_2$. The bias in this case is given by $\hat{\gamma}' b_2^A$.

A.2.2 IV - Multiple Instruments

If $k > 1$ then the system is over-identified and the simplifications in (A-11) will not hold. Thus the generalized version of (A-12) is given by

$$\hat{b}_1^B = (x_1' P_Z x_1)^{-1} x_1' P_Z (x_1 \hat{b}_1^A + X_2 \hat{b}_2^A + \hat{u}^A) \tag{A-15}$$

$$\hat{b}_1^B = (x_1' P_Z x_1)^{-1} (x_1' P_Z x_1) \hat{b}_1^A + (x_1' P_Z x_1)^{-1} x_1' P_Z X_2 \hat{b}_2^A + (x_1' P_Z x_1)^{-1} x_1' P_Z \hat{u}^A \tag{A-16}$$

$$\hat{b}_1^B = \hat{b}_1^A + \hat{\gamma} \hat{b}_2^A + \hat{\delta} \tag{A-17}$$

where once again we use $\hat{\gamma}'$ to represent the IV estimate of γ' in (A-2) as $\hat{\gamma}' = (x_1' P_Z x_1)^{-1} x_1' P_Z X_2$ and define $\hat{\delta} \equiv (x_1' P_Z x_1)^{-1} x_1' P_Z \hat{u}^A$. It is easy to see that $\hat{\delta}$ refers to the IV estimate of regressing $\hat{u}^A$ on x_1 with instruments Z. While asymptotically $\mathbb{E}\left(Z' u^A\right) = 0$ would hold and $\hat{\delta} \to 0$, in finite samples, $\hat{\delta}$ will not drop out from this expression since $Z' \hat{u}^A \neq 0$.

In order to do the decomposition, we proceed as follows. Rewrite (A-17) as

$$\hat{b}_1^B - \hat{\delta} = \hat{b}_1^A + \hat{\gamma}' \hat{b}_2^A \tag{A-18}$$

where we consider the left-hand side of the equation to be the total effect of x_1 on y and the two terms on the right-hand side as the direct effect of house value changes on consumption and the indirect effect of X_2 that comes via house value changes, respectively. It is convenient to report these as shares and we use $\hat{b}_1^A / (\hat{b}_1^B - \hat{\delta})$ as the share of the total effect that's direct and $\hat{\gamma}' \hat{b}_2^A / (\hat{b}_1^B - \hat{\delta})$ as the share of the total effect that's indirect and due to X_2.[29]

[29] An alternative is to follow the approach in Chen et al. (2016) and compute the decomposition twice, each with only one of the instruments, and take the average. Doing so does not alter the results in a meaningful way.

B Detailed Results

B.1 Main Results

Table A-1 shows the first stages for each of the eight categories when only ΔHV is used. To save space these are the only first stages we show in the paper but all others are similar to these with respect to the significance of the instruments and the F-statistic. Both instruments enter the first stage with negative and highly statistically significant coefficients, except one instance where one of them has a coefficient which is not statistically significant. Recall that the instruments capture housing supply (in)elasticity – in areas where building regulations are more restrictive or in areas where little land is available to develop, housing supply will be more inelastic – larger values of the two instruments indicate more inelastic supply. This means that in response to a demand shock, we expect a larger price reaction in such areas since supply cannot respond as much. The period we are considering, between 2006 and 2009, can be thought of as a period with a large negative aggregate housing demand shock. Thus prices should fall by more in areas with inelastic housing supply, which is what the negative coefficients in the first stage would indicate.[30] The first-stage F-statistics are all very large, with the smallest one at 419. These are well beyond the critical values provided by Stock and Yogo (2005) and thus tests of weak identification and, separately, tests of underidentification are all easily rejected.

Table A-2 presents the second stages from 24 estimations: for each of the eight prime status-LTV combinations, we estimate three models. All models have a constant, origination controls, age polynomials and net-worth buckets. In this table we present the estimated coefficients for each category for ΔHV, ΔU and credit supply. Whenever it is significant, the sign of ΔHV is positive, indicating that a decline in house values reduces our measure of consumption, the probability of originating an auto loan in 2009. Similarly ΔU has a negative sign – an increase in unemployment reduces consumption – and Credit Supply has a positive sign (except one instance where it is negative and only marginally significant) – a decline in

[30] Mian et al. (2013) use one of the same instruments, Unavailable, and obtain the same sign for the period 2006 to 2009. Mian and Sufi (2009) also show that between 2002 and 2006, when there was a strong increase in housing demand across the country due to cheaper credit, regions with inelastic housing supply showed larger house price increases. Aladangady (2017) relies on the same pair of instruments interacted with an aggregate demand shifter in a panel structure. We do the same in Section 5.2 when we use panel data.

Table A-1: Sample First Stages

(a) Prime

	LTV0	LTV1	LTV2	LTV3
	Dependent Variable: ΔHV			
WRLURI	0.0035	-0.0654***	-0.0753***	-0.0697***
	(0.0185)	(0.0129)	(0.0114)	(0.0107)
Unavailable	-2.1587***	-1.7373***	-1.5027***	-1.4079***
	(0.0802)	(0.0503)	(0.0503)	(0.0473)
First Stage F-stat	419.03	869.41	738.07	708.4
Observations	51,059	100,604	78,916	29,687

(b) Non-Prime

	LTV0	LTV1	LTV2	LTV3
	Dependent Variable: ΔHV			
WRLURI	-0.1299***	-0.1543***	-0.1329***	-0.1104***
	(0.0206)	(0.0109)	(0.0094)	(0.0098)
Unavailable	-1.7936***	-1.4105***	-1.1808***	-1.1571***
	(0.0652)	(0.0433)	(0.0453)	(0.0444)
First Stage F-stat	511.45	980.83	821.63	697.62
Observations	8,867	30,047	35,860	13,990

Notes: Each first-stage equation is estimated separately and includes a constant, and all the controls that are used in the second stage (including age, age^2, loan origination count dummies, net-worth bin dummies, and 2003 ZIP auto sales). The coefficients for these are not shown. The table shows the coefficients for the two instruments Wharton Residential Land Use Regulation Index (WRLURI) and Share of Land that is Unavailable for Real Estate Development. The means of the dependent variable in each estimation are -1.23, -0.86, -0.67 and -0.57, respectively, for panel (a) and -1.01, -0.67, -0.51 and -0.43, respectively, for panel (b). (***), (**) and (*) denote significance at 1%, 5% and 10% levels, respectively.

credit supply reduces consumption. Considering the controls' effects on consumption (not reported), we find that households that originated more auto loans in the period 2004-2006 or those that have higher non-housing net worth in 2006 are more likely to originate auto loans in 2009. Similarly, households that live in ZIP codes that had a large number of auto loan originations in 2003 tend to have more originations in 2009. Finally, age polynomials show that, in general, auto loan originations fall after about age 45. For most categories the decline is actually monotonic but for some categories (for example the Non-Prime-LTV3 category) there is a mild hump shape, that is loan originations initially rise slightly before they start falling.

B.2 Robustness Results

B.2.1 Bank Health Instead of Credit Supply

See Table A-3.

Table A-2: Decomposition - Second Stage Marginal Effects

Dependent Variable: Originate Auto Loan						
	Prime - LTV0			**Prime - LTV1**		
ΔHV	0.0036	-0.0004	-0.0015	0.0085***	0.0034	0.0011
	(0.0032)	(0.0053)	(0.0058)	(0.0031)	(0.0047)	(0.0053)
ΔU	-	-0.0029*	-0.0027*	-	-0.0036***	-0.0035***
		(0.0017)	(0.0016)		(0.0012)	(0.0012)
Credit Supply	-	-	0.0258	-	-	0.0318*
			(0.0275)			(0.0181)
Obs / Clusters	51,059 / 4,296			100,604 / 7,620		
	Prime - LTV2			**Prime - LTV3**		
ΔHV	0.0161***	0.0187***	0.0139*	0.0115	0.0117	0.0028
	(0.0047)	(0.0066)	(0.0074)	(0.0084)	(0.0110)	(0.0121)
ΔU	-	0.0000	0.0001	-	-0.0031	-0.0032
		(0.0015)	(0.0014)		(0.0022)	(0.0022)
Credit Supply	-	-	0.0505**	-	-	0.0826***
			(0.0200)			(0.0315)
Obs / Clusters	78,916 / 7,563			29,687 / 5,062		
	Non-Prime - LTV0			**Non-Prime - LTV1**		
ΔHV	0.0242***	0.0236*	0.0246*	0.0245***	0.0218**	0.0243**
	(0.0085)	(0.0144)	(0.0147)	(0.0061)	(0.0087)	(0.0096)
ΔU	-	-0.0011	-0.0023	-	-0.0020	-0.0023
		(0.0038)	(0.0035)		(0.0019)	(0.0018)
Credit Supply	-	-	-0.0781	-	-	-0.0494*
			(0.0584)			(0.0290)
Obs / Clusters	8,867 / 4,296			30,047 / 7,620		
	Non-Prime - LTV2			**Non-Prime - LTV3**		
ΔHV	0.0286***	0.0234**	0.0173	0.0811***	0.0829***	0.0784***
	(0.0076)	(0.0100)	(0.0111)	(0.0134)	(0.0177)	(0.0190)
ΔU	-	-0.0047**	-0.0048***	-	-0.0007	-0.008
		(0.0018)	(0.0018)		(0.0030)	(0.0030)
Credit Supply	-	-	0.0587**	-	-	0.0341
			(0.0263)			(0.0414)
Obs / Clusters	35,860 / 7,563			13,990 / 5,062		

Notes: Each equation is estimated separately and includes a constant, origination controls, age polynomial and net-worth buckets. The coefficients for these are not shown. ΔHV and ΔU refer to the change in individual-level house value and change in county-level unemployment rate, both computed from 2006 to 2009. Credit Supply refers to the credit supply shocks as explained in Section 2.3. The means of the dependent variables in each estimation, going columnwise first and then in rows are 0.095, 0.126, 0.158, 0.186, 0.094, 0.118, 0.139, 0.164. (***), (**) and (*) denote significance at 1%, 5% and 10% levels, respectively.

Table A-3: Bank Health Results

	Prime - LTV0	Prime - LTV1	Prime - LTV2	Prime - LTV3
	Dependent Variable: Originate Auto Loan (Mean = 0.1356)			
ΔHV	-0.0080	-0.0021	0.0120	-0.0070
	(0.0069)	(0.0057)	(0.0076)	(0.0124)
ΔU	-0.0037**	-0.0042***	-0.0003	-0.0044*
	(0.0018)	(0.0014)	(0.0016)	(0.0023)
Bank Health	-7.7258***	-4.1145***	-0.9964	-3.8132*
	(2.4898)	(1.4485)	(1.5504)	(2.2731)
Bad Mortgage	-0.070***	-0.078***	-0.084***	-0.123***
	(0.010)	(0.006)	(0.006)	(0.009)

	Non-Prime LTV0	Non-Prime LTV1	Non-Prime LTV2	Non-Prime LTV3
	Dependent Variable: Originate Auto Loan (Mean = 0.1356)			
ΔHV	0.0187	0.0144	-0.0002	0.0496***
	(0.0158)	(0.0097)	(0.0109)	(0.0186)
ΔU	-0.0020	-0.0024	-0.0052***	-0.0011
	(0.0038)	(0.0020)	(0.0019)	(0.0031)
Bank Health	1.3298	-0.7435	-1.9195	0.0830
	(4.6286)	(2.0260)	(1.8616)	(2.9773)
Bad Mortgage	-0.070***	-0.073***	-0.099***	-0.112***
	(0.006)	(0.004)	(0.004)	(0.006)

Notes: See notes to Table A-2. Number of observations and clusters are the same as Table A-2. This table only reports the estimates of the full model with all controls including Bad Mortgage.

B.2.2 Probit - IV

See Table A-4 and Table A-5.

Table A-4: Probit-IV Results (1)

(a) Prime (LTV0, LTV1)

	LTV0				LTV1			
	Dependent Variable: Originate Auto Loan							
ΔHP	0.0025	0.0006	0.0003	0.0001	0.0085***	0.0044	0.0036	0.0031
	(0.0032)	(0.0053)	(0.0058)	(0.0058)	(0.0031)	(0.0047)	(0.0052)	(0.0053)
ΔU	-	-0.0024	-0.0022	-0.0021	-	-0.0033***	-0.0030**	-0.0028**
		(0.0017)	(0.0016)	(0.0016)		(0.0012)	(0.0012)	(0.0012)
Credit Supply	-	-	0.0208	0.0209	-	-	0.0300*	0.0300*
			(0.0263)	(0.0263)			(0.0175)	(0.0175)
Bad Mortgage	-	-	-	-0.064***	-	-	-	-0.077***
				(0.010)				(0.006)

(b) Prime (LTV2, LTV3)

	LTV2				LTV3			
	Dependent Variable: Originate Auto Loan							
ΔHP	0.0133***	0.0109	0.0093	0.0068	0.0217**	0.0166	0.0144	0.0074
	(0.0048)	(0.0067)	(0.0075)	(0.0076)	(0.0085)	(0.0112)	(0.0123)	(0.0127)
ΔU	-	-0.001	-0.0006	-0.0003	-	-0.0024	-0.0017	-0.0013
		(0.0015)	(0.0014)	(0.0014)		(0.0022)	(0.0022)	(0.0025)
Credit Supply	-	-	0.0565***	0.0543***	-	-	0.0718**	0.0689**
			(0.0196)	(0.0196)			(0.0308)	(0.0307)
Bad Mortgage	-	-	-	-0.086***	-	-	-	-0.124***
				(0.006)				(0.008)

Notes: See notes to Table A-2. Number of observations and clusters are the same as Table A-2.

Table A-5: Probit-IV Results (2)

(c) Non-Prime (LTV0, LTV1)

	LTV0				LTV1			
	Dependent Variable: Originate Auto Loan							
ΔHV	0.0065	0.0000	0.0006	-0.0005	0.0121*	0.0047	0.0058	0.0026
	(0.0085)	(0.0138)	(0.0146)	(0.0144)	(0.0062)	(0.0088)	(0.0097)	(0.0097)
ΔU	-	-0.0054	-0.0057*	0.0056*	-	-0.0045	-0.0047**	-0.0042**
		(0.0037)	(0.0034)	(0.0034)		(0.0019)	(0.0019)	(0.0019)
Credit Supply	-	-	-0.0220	-0.0236	-	-	-0.0251	-0.0176
			(0.5540)	(0.0549)			(0.0279)	(0.0276)
Bad Mortgage	-	-	-	-0.072***	-	-	-	-0.075***
				(0.006)				(0.004)

(d) Non-Prime (LTV2, LTV3)

	LTV2				LTV3			
	Dependent Variable: Originate Auto Loan							
ΔHV	0.0430***	0.0357***	0.0337***	0.0218**	0.0664***	0.0555***	0.0528***	0.0306
	(0.0077)	(0.0100)	(0.0111)	(0.0113)	(0.0143)	(0.0185)	(0.0199)	(0.0204)
ΔU	-	-0.0033*	-0.0031*	-0.0020	-	-0.0044	-0.0040	-0.0027
		(0.0018)	(0.0018)	(0.0018)		(0.0031)	(0.0031)	(0.0031)
Credit Supply	-	-	0.0445*	0.0486*	-	-	0.0568	0.0611
			(0.0251)	(0.0249)			(0.0397)	(0.0392)
Bad Mortgage	-	-	-	-0.099***	-	-	-	-0.117***
				(0.004)				(0.006)

Notes: See notes to Table A-2. Number of observations and clusters are the same as Table A-2.

B.3 Results Regarding Ex-Post Constraints

B.3.1 Step 1: ΔHV Between 2006-2009 Predicts Bad Mortgage

See Table A-6.

Table A-6: ΔHV Between 2006-2009 Predicts Bad Mortgage

(a) Risk Score 2006 < 600

	LTV0	**LTV1**	**LTV2**	**LTV3**
	Dependent Variable: Bad Mortgage			
ΔHV	-0.1211**	-0.1706***	-0.3261***	-0.3265***
	(0.0476)	(0.0302)	(0.0305)	(0.0562)
F-stat	136.86	385.13	436.18	243.75
Obs / Cluster	2,188 / 1,695	8,162 / 4,329	10,259 / 4,417	3,859 / 2,192

(b) Risk Score 2006 $\in [600, 700)$

	LTV0	**LTV1**	**LTV2**	**LTV3**
	Dependent Variable: Bad Mortgage			
ΔHV	-0.0477***	-0.0754***	-0.2103***	-0.3164***
	(0.0154)	(0.0108)	(0.0133)	(0.0246)
F-stat	216.14	513.28	557.86	460.82
Obs / Cluster	6,679 / 3,663	21,885 / 6,927	25,601 / 6,971	10,131 / 4,484

(c) Risk Score 2006 > 700

	LTV0	**LTV1**	**LTV2**	**LTV3**
	Dependent Variable: Bad Mortgage			
ΔHV	-0.0046**	-0.0151***	-0.0604***	-0.1073***
	(0.0019)	(0.0020)	(0.0036)	(0.0079)
F-stat	186.74	444.47	474.58	480.49
Obs / Cluster	51,059 / 7,171	100,604 / 9,131	78,916 / 9,044	29,687 / 7,328

Notes: See notes to Table 5. Each estimation includes a full set of controls that are not reported. The means for the dependent variable (Bad Mortgage) in each category (from top to bottom and then left to right) are 27.7%, 31.9%, 39.4%, 44.3%, 9.1%, 11.1%, 16.6%, 20.4%, 1.0%, 1.8% 3.8%, 5.2%.

B.3.2 Step 2: Bad Mortgage Predicts Deep Subprime in 2009

See Table A-7.

Table A-7: Bad Mortgage Predicts Deep Subprime in 2009

(a) Risk Score 2006 < 600

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Deep Subprime in 2009							
ΔHV	-0.0484	0.0033	-0.0803***	-0.0080	-0.0835***	0.0600**	-0.1518***	-0.0026
	(0.0523)	(0.0479)	(0.0307)	(0.0278)	(0.0305)	(0.0276)	(0.0529)	(0.0477)
BM	-	0.4260***	-	0.4238***	-	0.4403***	-	0.4556***
		(0.0209)		(0.0102)		(0.0086)		(0.0139)
F-stat	136.86	136.54	385.13	382.18	436.18	423.39	243.75	237.45
Obs / Cluster	2,188 / 1,695		8,162 / 4,329		10,259 / 4,417		3,859 / 2,192	

(b) Risk Score 2006 $\in [600, 700)$

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Deep Subprime in 2009							
ΔHV	-0.0650***	-0.0365*	-0.0745***	-0.0275**	-0.1711***	-0.0400***	-0.2245***	-0.0189
	(0.0209)	(0.0188)	(0.0139)	(0.0121)	(0.0158)	(0.0135)	(0.0277)	(0.0237)
BM	-	0.5976***	-	0.6240***	-	0.6233***	-	0.6493***
		(0.0192)		(0.0092)		(0.0075)		(0.0107)
F-stat	216.14	214.32	513.28	506.97	557.86	540.75	460.82	439.54
Obs / Cluster	6,679 / 3,663		21,885 / 6,927		25,601 / 6,971		10,131 / 4,484	

(c) Risk Score 2006 ≥ 700

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Deep Subprime in 2009							
ΔHV	-0.0050**	-0.0026	-0.0137***	-0.0051**	-0.0535***	-0.0188***	-0.0837***	-0.0230***
	(0.0024)	(0.0020)	(0.0025)	(0.0022)	(0.0038)	(0.0031)	(0.0074)	(0.0055)
BM	-	0.5287***	-	0.5734***	-	0.5743***	-	0.5655***
		(0.0252)		(0.0132)		(0.0103)		(0.0136)
F-stat	186.74	185.98	444.47	443.31	474.58	467.40	480.49	461.33
Obs / Cluster	51,059 / 7,171		100,604 / 9,131		78,916 / 9,044		29,687 / 7,328	

Notes: See notes to Table 5. Each estimation includes a full set of controls that are not reported. The means for the dependent variable (Deep Subprime in 2009) in each category (from top to bottom and then left to right) are 48.3%, 54.2%, 62.3%, 64.7%, 17.5%, 21.0%, 26.2%, 28.9%, 1.1%, 2.0%, 3.5%, 4.1%.

B.3.3 Step 3: Bad Mortgage Reduces Consumption

See Table A-8.

Table A-8: Bad Mortgage Reduces Consumption

(a) Risk Score 2006 < 600

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Originate Auto Loan							
ΔHV	0.0154	0.0090	0.0703***	0.0611***	0.0649***	0.0411**	0.1054***	0.0715*
	(0.0258)	(0.0256)	(0.0172)	(0.0173)	(0.0191)	(0.0193)	(0.0366)	(0.0367)
BM	-	-0.0533***	-	-0.0537***	-	-0.0731***	-	-0.1039***
		(0.0100)		(0.0063)		(0.0061)		(0.0110)
F-stat	136.86	136.54	385.13	382.18	436.18	423.39	243.75	237.45
Obs / Cluster	2,188 / 1,695		8,162 / 4,329		10,259 / 4,417		3,859 / 2,192	

(b) Risk Score 2006 $\in [600, 700)$

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Originate Auto Loan							
ΔHV	0.0276	0.0241	0.0098	0.0036	0.0051	-0.0179	0.0768***	0.0425*
	(0.0172)	(0.0172)	(0.0114)	(0.0115)	(0.0132)	(0.0137)	(0.0224)	(0.0236)
BM	-	-0.0733***	-	-0.0817***	-	-0.1088***	-	-0.1079***
		(0.0085)		(0.0055)		(0.0052)		(0.0089)
F-stat	216.14	214.32	513.28	506.97	557.86	540.75	460.82	439.54
Obs / Cluster	6,679 / 3,663		21,885 / 6,927		25,601 / 6,971		10,131 / 4,484	

(c) Risk Score 2006 ≥ 700

	LTV0		LTV1		LTV2		LTV3	
	Dependent Variable: Originate Auto Loan							
ΔHV	-0.0015	-0.0018	0.0011	-0.0001	0.0139*	0.0088	0.0028	-0.0106
	(0.0058)	(0.0058)	(0.0053)	(0.0053)	(0.0074)	(0.0075)	(0.0121)	(0.0125)
BM	-	-0.0695***	-	-0.0780***	-	-0.0841***	-	-0.1244***
		(0.0104)		(0.0061)		(0.0060)		(0.0087)
F-stat	186.74	185.98	444.47	443.31	474.58	467.40	480.49	461.33
Obs / Cluster	51,059 / 7,171		100,604 / 9,131		78,916 / 9,044		29,687 / 7,328	

Notes: See notes to Table 6. The means for the dependent variable (Auto Loan Originations in 2009) in each category (from top to bottom and then left to right) are 7.1%, 9.5%, 10.8%, 13.0%, 10.1%, 12.7%, 15.1%, 17.8%, 9.5%, 12.6%, 15.8%, 18.6%.

B.3.4 Full Results with Bad Mortgage

See Table A-9.

Table A-9: Full Estimation Results - With and Without Bad Mortgage

(a) Prime

	LTV0		**LTV1**		**LTV2**		**LTV3**	
	Dependent Variable: Originate Auto Loan							
ΔHV	-0.0015	-0.0018	0.0011	-0.0001	0.0139	0.0088	0.0028	-0.0106
	(0.0058)	(0.0058)	(0.0053)	(0.0053)	(0.0074)	(0.0075)	(0.0121)	(0.0125)
ΔU	-0.0027*	-0.0026	-0.0034***	-0.0034***	0.0001	0.0000	-0.0032	-0.0035
	(0.0016)	(0.0016)	(0.0012)	(0.0012)	(0.0014)	(0.0014)	(0.0022)	(0.0022)
CS	0.0258	0.0263	0.0318*	0.0324*	0.0505**	0.0510**	0.0826***	0.0849***
	(0.0275)	(0.0275)	(0.0181)	(0.0180)	(0.0200)	(0.0200)	(0.0315)	(0.0314)
BM	-	-0.070***	-	-0.078***	-	-0.084***	-	-0.124***
		(0.010)		(0.006)		(0.006)		(0.009)
Observations	51,059		100,604		78,916		29,687	
Clusters	4,296		7,620		7,563		5,062	

(b) Non-Prime

	LTV0		**LTV1**		**LTV2**		**LTV3**	
	Dependent Variable: Originate Auto Loan							
ΔHV	0.0246*	0.0203	0.0243**	0.0174*	0.0173	-0.0049	0.0784***	0.0465**
	(0.0147)	(0.0146)	(0.0096)	(0.0096)	(0.0111)	(0.0114)	(0.0190)	(0.0195)
ΔU	-0.0023	-0.0027	-0.0023	-0.0024	-0.0048***	-0.0049***	-0.0008	-0.0010
	(0.0035)	(0.0034)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0030)	(0.0040)
CS	-0.0781	-0.0728	-0.0494*	-0.0370	0.0587**	0.0721***	0.0341	0.0417
	(0.0584)	(0.0595)	(0.0290)	(0.0288)	(0.0263)	(0.0263)	(0.0414)	(0.0409)
BM	-	-0.0070***	-	-0.073***	-	-0.0100***	-	-0.112***
		(0.006)		(0.004)		(0.004)		(0.006)
Observations	8,867		30,047		35,860		13,990	
Clusters	4,296		7,620		7,563		5,062	

Notes: See notes to Table A-2. Each estimation includes a full set of controls that are not reported.

B.3.5 Ex-Ante Constraints - Type of Mortgage

See Table A-10 and Table A-11.

Table A-10: Ex-Ante Constraints - Type of Mortgage

(a) Fixed First Mortgage, No Second

	Non-Prime				Prime			
LTV	0	1	2	3	0	1	2	3
Δ HV	0.0378**	0.0312**	0.0192	0.0497	0.0017	0.0008	0.0085	-0.0075
	(0.0190)	(0.0126)	(0.0169)	(0.0318)	(0.0046)	(0.0076)	(0.0117)	(0.0226)
p-value	0.047	0.013	0.255	0.118	0.82	0.912	0.465	0.739
First Stage Signs	Neg	Neg	Neg	Neg	Neg	Neg	Neg	Neg
First Stage F-stat	211.59	484.57	483.28	354.21	200.73	458.35	494.16	417.02
N	5,397	18,293	20,813	8,101	30,963	56,317	40,522	14,510

(b) ARM <5 Years, No Second

	Non-Prime				Prime			
LTV	0	1	2	3	0	1	2	3
Δ HV	0.0661*	0.0367	-0.0094	-0.0007	-0.0324	-0.0481	0.0209	-0.0224
	(0.0395)	(0.0304)	(0.0273)	(0.0423)	(0.0289)	(0.0378)	(0.0294)	(0.0451)
p-value	0.094	0.227	0.731	0.987	0.263	0.203	0.477	0.620
First Stage Signs	Neg	Neg	Neg	Neg	Neg	Neg	Neg	Neg
First Stage F-stat	67.41	152.71	259.1	136.19	83.83	67.89	105.33	86.7
N	754	2,196	3,875	1,797	1,485	1,951	2,659	1,378

(c) ARM ≥5 Years, No Second

	Non-Prime				Prime			
LTV	0	1	2	3	0	1	2	3
Δ HV	-0.1215	-0.0429	-0.0112	0.0390	-0.0070	0.0291	-0.0110	0.0707
	(0.0810)	(0.0571)	(0.0483)	(0.0913)	(0.0307)	(0.0223)	(0.0253)	(0.0479)
p-value	0.134	0.453	0.816	0.669	0.821	0.193	0.665	0.14
First Stage Signs	0	Neg	Neg	Neg	0	Neg	Neg	Neg
First Stage F-stat	9.98	38.18	66.73	32.88	28.66	78.57	85.99	59.02
N	175	688	1,014	444	1,091	3,497	4,467	2,095

Notes: See notes to Table A-2. This table only reports the coefficients for ΔHV, the sign of instruments and the F-stat in the first stage and the number of observations in each estimation.

Table A-11: Ex-Ante Constraints - Type of Mortgage

(d) Closed-End Second

	Non-Prime				Prime			
LTV	0	1	2	3	0	1	2	3
Δ HV	0.0522	0.0163	-0.0402	0.1320**	-0.0603*	0.0086	0.0352	0.0074
	(0.0815)	(0.0410)	(0.0371)	(0.0661)	(0.0346)	(0.0285)	(0.0320)	(0.0548)
p-value	0.522	0.691	0.278	0.046	0.082	0.762	0.273	0.893
First Stage Signs	Neg	Neg	Neg	Neg	Neg	Neg	Neg	Neg
First Stage F-stat	42.29	167.92	261.97	93.01	100.04	201.53	222.13	149.14
N	748	3,216	4,451	1,546	2,270	7,239	7,931	2,768

(e) HELOC

	Non-Prime				Prime			
LTV	0	1	2	3	0	1	2	3
Δ HV	-0.0191	-0.0138	-0.0437*	0.0728	0.0051	-0.0012	0.0081	-0.0354*
	(0.0326)	(0.0196)	(0.0234)	(0.0449)	(0.0100)	(0.0081)	(0.0121)	(0.0201)
p-value	0.558	0.482	0.062	0.105	0.611	0.885	0.504	0.078
First Stage Signs	Neg	Neg	Neg	Neg	Neg	Neg	Neg	Neg
First Stage F-stat	93.43	266	300.79	174.87	101.41	301.88	314.54	266.5
N	1,793	5,654	5,707	2,102	15,250	31,600	23,337	8,936

Notes: See notes to Table A-2. This table only reports the coefficients for ΔHV, the sign of instruments and the F-stat in the first stage and the number of observations in each estimation.