ESSAYS IN HOUSEHOLD FINANCE

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Abstract

This dissertation consists of three essays on household finance. In the first essay, I analyze the impact of changes to collateral value on borrowers' default decision on auto loans using two types of natural experiments in Sri Lanka. Changes in vehicle import tax rates and loan-to-value ratio caps on auto loans generated plausibly exogenous variation in the resale value of vehicles already pledged as collateral. Using proprietary auto loan performance data, I estimate that a 10% drop in the collateral value corresponds to a 44% increase in the default rate. I also find that collateral value is more important for borrowers with higher outstanding loan balances.

In the second essay, we use a unique feature of California's property tax system to empirically identify the effect of selling homeowners' past property tax payments on their choice of listing price. Although past property taxes are sunk costs, we find that they have a significant positive effect on the sellers' choice of listing price, which is inconsistent with rational models of decision making. This effect is stronger when sellers expect to sell at a loss relative to their purchase price, for high-valued properties, and in zip codes with lower housing transaction volumes. Interestingly, the sunk-cost effect is also stronger for sellers with higher mortgage debt, especially when they expect to incur a loss on the sale. The effect of property taxes on listing price is mostly transmitted to the selling price, which is consistent with the idea that buyers use listing prices as anchors to assess property values. Overall, our results suggest that sunk costs affect prices in the housing market.

In the third essay, we show that modest differences in the interest rate at loan origination can have long-lasting effects on mortgagors. We use monthly fluctuations in the national mortgage rate at loan origination to study small changes in interest rates across home purchases made in the same year, in the same area, and which eventually reach similar levels of negative equity. A 50bp higher national rate at origination corresponds to an extra $550 in payments per year and, during the bust, an increase in defaults of 68-88 bp within 12 months of reaching negative equity. The effect is large relative average default rates of 3.78% (5.39%) for homes with 10% (30%) negative equity. Consistent with liquidity constraints, the magnitude of the effect is relatively constant across different levels of negative equity. The national mortgage rate is not correlated with worse borrower credit quality. During the boom, smaller mortgage payments result in increased consumption of non-durables and services from 2001-2007, while total expenditure is unchanged. If intermediaries resist large concessions to borrowers, small concessions may be more effective.

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Chapter 1

Collateral Value and Strategic Default: Evidence from Auto Loans

1. Introduction

Strategic default refers to the decision by a borrower to stop making payments on a debt despite having the ability to make the payments. Economic theory predicts 'strategic default' when the value of the collateral drops sufficiently (Hart and Moore, 1998; Hart, 2009; Titman and Torous, 1989; Campbell and Cocco, 2015). However, due to countervailing non-financial factors, borrowers may opt to continue repaying even when default is optimal (Bursztyn et al., 2015; Guiso et al., 2013; Bhutta et al., 2017).[1] Therefore, the extent to which the collateral value affects borrowers' default decision is an empirical question, which is of interest to both academics and policymakers.

The greatest challenge in empirically identifying the effect of collateral value on default is finding a source of variation in collateral value that is otherwise uncorrelated with unobserved factors–such as local economic conditions and borrowers' cost of default–that

[1]Non-financial factors may include moral aversion to default (Bursztyn et al., 2015; Guiso et al., 2013), emotional attachment (Guiso et al., 2013; Bhutta et al., 2017), fear over the perceived consequences of default (White, 2010; Seiler et al., 2012), people's subjective expectations (Kuhnen and Melzer, 2017), inattention (Andersen et al., 2015; Agarwal et al., 2015) and financial illiteracy (Burke and Mihaly, 2012).

influence borrowers' default risk. Although previous studies demonstrate that default is more likely, *ceteris paribus*, for a borrower who experiences a larger drop in collateral value (Deng et al., 2000; Bajari et al., 2008; Foote et al., 2008), failure to control for these confounding factors precludes causal inference.

In this paper, I isolate the collateral value channel in borrowers' default decision by exploiting several policy changes that generated plausibly exogenous variation in the resale value of vehicles already pledged as collateral for auto loans in Sri Lanka. My identification strategy hinges on the notion that while these variations in collateral value impact borrowers' incentives to strategically default, they are not correlated with unobserved factors that influence borrowers' default risk. Another feature of these policy changes is that they impacted certain classes of vehicles only, which allows me to use comparable unaffected vehicle types as control samples in a difference-in-difference setting. Moreover, unlike in more developed countries, defaulting on auto loans is an important financial decision for borrowers in Sri Lanka. Due to high import tax rates (approximately 200%), vehicles constitute a large fraction of a vehicle owner's wealth–sometimes more than their home.

I use a large proprietary database of auto loan transactions from a major auto loan lender in Sri Lanka. This dataset includes a wide span of loan-level data at origination and month-by-month stream of payments made by the borrower, while also indicating whether (and, if so, when) the loan is in default.

I exploit two types of policy changes: (1) changes to vehicle import tax rates and (2) revisions to loan-to-value ratio caps on auto loans. In the first set of tests, I use three unanticipated vehicle import tax rate changes: a November 2014 import tax rate cut for cars with an engine capacity less than 1L (henceforth, smaller-engine cars) and two import tax rate hikes for new 3-wheelers in November 2015 and April 2016.[2] In the case of the tax rate cut on smaller-engine cars, cars with engine capacity greater than 1L

[2]3-wheelers are also known as auto-rickshaws, tuk-tuks or trishaws. These are motorized vehicles with three wheels mainly used as taxis in Sri Lanka.

(henceforth, larger-engine cars) serve as the control sample.[3] When analyzing the impact of tax rate hikes on 3-wheeler loans, to ensure that my control group is comparable, I construct matched samples using loans offered for other unaffected vehicle types based on similar borrower profiles and type of vehicle use.[4]

Even though import tax rate changes did not directly impact used vehicles, we expect resale value of smaller-engine cars to drop due to increased relative demand for new smaller-engine cars following the tax cut. Similarly, resale value of 3-wheelers is expected to increase following the tax rate increases. As expected, I find that the decrease in import tax rates on smaller-engine cars led to a 10.2% decrease in the resale value of smaller-engine cars relative to that of larger-engine cars. The resale value of 3-wheelers increased on average by 8.7% following the import tax rate hikes, relative to the control sample.

Having established that import tax rate changes impacted the resale value of vehicles, I turn to estimating the effect of import tax rate changes on default. Using these tax rate changes as treatments, I apply the standard difference-in-difference methodology to loan-month observations. *Treated* loans finance vehicle types affected by a tax rate change and originated prior to that respective tax rate change. The detailed data allows for a rich set of controls, including fixed effects for loans and district-months. After the tax cut, I expect the default rate to increase for smaller-engine car loans in comparison to the default rate for the unaffected larger-engine car loans. Likewise, the default rate of 3-wheeler loans is expected to drop relative to the control sample, following tax rate hikes.

I find strong evidence for significant effect of import tax rate changes on borrower default decisions. The default rate on smaller-engine car loans rose by 0.4% following the tax rate cut. The unconditional probability of default prior to the tax change was 0.9% and the estimated effect corresponds to a 44% higher probability of default. The

[3] larger-engine cars were not affected by the November 2014 tax rate change

[4] 3-wheelers are mainly used as a productive asset (as opposed to a consumption asset) and one of the control samples is restricted to loans used to finance the purchase of vehicles that are used for productive purposes

default rate of 3-wheeler loans fell by 0.3%, which corresponds to a 24% lower probability of default.

In the second set of tests, I estimate the effect of loan-to-value ratio (LTV) cap changes on default rates. In Sri Lanka, auto loans originated prior to 2017 were subject to an LTV cap of 70%. In January 2017, the Central Bank of Sri Lanka revised the LTV caps applicable for auto loans financing *new* vehicles based on vehicle type– rising for some and decreasing for others. Specifically, according to new rules, a person buying a new car, SUV, or van can only obtain financing up to 50% of the value of the vehicle, while the LTV cap was reduced to 25% for new 3-wheelers. For new trucks and buses, lenders are allowed to finance up to 90% of the value. The LTV cap for *used vehicle* loans remained unchanged at 70%.

These changes affected the resale values of vehicles that were pledged as collateral before January 2017. Consider cars, whose LTV cap was reduced. This tighter LTV cap raises down-payment requirements to buy a new car, forcing some borrowers into the used-vehicle market. The down-payment requirement for a used car is then much lower since a higher LTV cap of 70% (as opposed to 50%) applied to lower valuations. With the emerging higher demand for used cars, the value of those pledged before the reform will thus rise, curbing borrower incentives to strategically default. Similarly, for vehicle types where LTV cap is increased, borrower incentives to strategically default would rise.

In this setting, I use a generalized difference-in-difference approach with dummy variables indicating loan-months of each vehicle type after new LTV rules. The sample comprises loan-month observations before and after the rule change on all loans originated prior to the rule change. Evidence from LTV cap changes presents effects similar to those from import tax rate changes: vehicle types with newly increased LTV caps, which curbed used-vehicle demand, lead to increased default rates; vehicle types with newly lowered LTV caps saw a drop in default rate.

The main assumption underlying my approach is that absent the tax-LTV policy changes, the average default rate in the treated and control groups would have trended in parallel. Difference-in-difference plots suggest that default rates moved in parallel before the treatments, providing evidence in support for parallel trends assumption. Furthermore, placebo tests simulating the reforms at earlier dates confirm that the results are not driven by preexisting trends inherent to specific vehicle types.

Another concern I address is equity extraction. If liquidity-constrained borrowers could extract increased equity in the form of a secondary loan following gains in collateral value, then my results may be picking up the impact of relaxed liquidity constraints. However, I do not observe any increase in loan balances in my data which allows me to track loan balances from month to month. Also, regulations do not allow borrowers to pledge the same asset as collateral for a secondary loan with another lender.

My findings contribute to two strands of literature. I contribute to the household finance literature by using policy-induced exogenous variation in collateral value to study the causal effect of collateral value changes on strategic default. The work of Palmer (2015), that uses long-run regional variation in house-price cyclicality as an instrument for house price declines, comes closest to my paper. To my knowledge, no other previous study has used actual transactional data to identify the causal link between collateral value and strategic default. Guiso et al. (2013) and Bajari et al. (2008) use survey data and structural estimation respectively to understand how borrowers' willingness to default changes with the home-equity shortfall. The existing work that identifies strategic default behavior does not provide evidence on the collateral value channel (Mayer et al., 2014; Yannelis, 2017; Blouin and Macchiavello, 2017; Artavanis and Spyridopoulos, 2018). Studies, such as Deng et al. (2000); Bajari et al. (2008); Foote et al. (2008); Scharlemann and Shore (2016), show a negative association between collateral value and default, but do not allow for causal inference. Identification of the collateral value channel is particularly relevant for ex-ante policy interventions, such as loan-to-value restrictions and mortgage insurance, which have been implemented under the assumption that collateral value is an important determinant of strategic default, despite limited empirical evidence.

This study also contributes to the empirical literature on contractual imperfections and defaults by examining how contractual defaults respond to unanticipated changes in market conditions. Theoretical models suggest that parties to a self-enforcing agreement have incentive to engage in 'hold-up' when market conditions change sufficiently to place the business relationship outside its self-enforcing range (Klein, 1996; Hart, 2009). A recent study by Blouin and Macchiavello (2017) provides evidence to support this prediction by showing that unanticipated rises in coffee market prices increase defaults on coffee pre-financing agreements. I complement their study by showing unanticipated drops in collateral value lead to more defaults.

In addition, this paper also adds to the literature on the determinants of auto loan defaults. Agarwal et al. (2008) study the relationship between borrower consumption choices and future auto loan performance. Heitfield and Sabarwal (2004), Ghulam and Hill (2017) and Wu and Zhao (2016) look at the determinants of auto loan default.

Finally, this paper presents direct evidence on the effect of unanticipated policy reforms in the Sri Lankan auto finance market. I show that an unintended consequence of selective import tax cuts and LTV cap increases was to increase the number of strategic defaults for certain types of loans. I also quantify the impact on the resale values of vehicles due to these policy reforms.

2. Theoretical and Institutional Background

2.1 Role of Collateral Value in Strategic Default

Option theoretic models predict strategic default when a drop in collateral value makes the present value of continued loan repayment less than that of defaulting (Foster and Van Order, 1984; Kau et al., 1987; Titman and Torous, 1989; Campbell and Cocco, 2015). In contract theory, the strategic default occurs when the collateral value drops sufficiently to place the relationship between a lender and a borrower outside its self-enforcing range (Klein, 1996; Hart, 2009).

Some studies, however, have suggested that borrowers strategically default less often than their financial incentives suggest. Foote et al. (2008) find that only 6.4% of "underwater" mortgage borrowers in Massachusetts chose to strategically default in early 1990s. Bhutta et al. (2017) show that borrowers serviced home loans until falling deeply underwater. Gerardi et al. (2017) note that 96% of low equity borrowers with ability to pay remained current on their mortgages. This is likely due to countervailing non-financial factors–such as moral aversion to default (Bursztyn et al., 2015; Guiso et al., 2013), emotional attachment (Guiso et al., 2013; Bhutta et al., 2017), fear over the perceived consequences of default (White, 2010; Seiler et al., 2012), people's subjective expectations (Kuhnen and Melzer, 2017), inattention (Andersen et al., 2015; Agarwal et al., 2015) and financial illiteracy (Burke and Mihaly, 2012)–that may cause borrowers to continue repaying loans regardless of collateral value. Therefore, the extent to which borrowers respond to changes in collateral value is an empirical question and there has been limited compelling empirical evidence on the causal effect of collateral value on strategic default (Deng et al., 2000; Bajari et al., 2008; Foote et al., 2008; Palmer, 2015; Scharlemann and Shore, 2016).

Isolating default due to collateral value is important for contract design, ex-ante policy issues (e.g: maximum loan-to-value ratio restrictions, underwriting standards, and mortgage insurance) and ex-post remedies (e.g: loan modification and foreclosure moratorium). If collateral value does not play an important role in borrowers' default behavior, then it limits the scope for credit market regulations such as loan-to-value restrictions or stricter underwriting standards. Understanding the role of collateral value in borrowers' default decision is important in gauging the cost of moral hazard in potential ex-post policy responses.

2.2 Auto Finance Market in Sri Lanka

This section details the Sri Lankan auto finance market. Due to severe import tax rates ($\approx$ 200%), vehicles are very expensive and often considered a luxury in Sri Lanka. New compact sedans, like the Honda Civic, were priced at nearly LKR 7 million ($\approx$ USD

47,000) in 2014. Popular small cars, such as Maruti Altos, cost around LKR 2 million ($\approx$ USD 14,000). According to Household Income and Expenditure Survey, the mean annual Sri Lankan household income for 2012-13 was LKR 550,000 ($\approx$ USD 3,700) and therefore a vehicle comprises a large fraction of a vehicle owner's wealth. This also means that both the auto loan liabilities and monthly repayment amounts are significant fractions of their balance sheets and monthly cash flow respectively.

In Sri Lanka, Non-Bank Finance Institutions (NBFI) are the leader in the auto finance market. NBFIs are regulated by the Central Bank of Sri Lanka. Regulatory requirements include minimum capital adequacy ratios and listing on the Colombo Stock Exchange. This NBFI segment is a key player in Sri Lanka's financial sector holding 13% of total assets among depository institutions. By year-end 2014, there were 56 NBFIs, many of which are involved predominantly in auto loan business. As of December 2014 auto loans represented more than 70% of the NBFI sector lending portfolios. These institutions attract higher risk borrowers who are unable to obtain auto financing from commercial banks. Auto loans through NBFIs are processed promptly, often within hours as opposed to a few weeks for a commercial bank, and borrowers are charged a high interest rate. NBFIs enlist aggressive collection practices to minimize losses. A typical auto loan is a fixed rate loan with a maturity of 4- to 6-years and a severe prepayment penalty.

The Credit Information Bureau of Sri Lanka (CRIB) provides credit information on current and prospective borrowers to lending institutions. Data include borrower identification, information regarding current and past credit facilities such as mortgages, auto loans and credit cards and payment histories. The CRIB report is one of the first documents analyzed by loan officers, before processing a request to finance any individual loan.

2.3 Policy Changes

In this paper, I use two types policy changes that generated variation in collateral value that is unrelated to unobserved factors that influence borrowers' default risk: (1) changes vehicle import tax rates and (2) changes to loan-to-value ratio caps.

Import Tax Rate Changes

I use three unanticipated import tax rate changes: a November 2014 tax cut on cars with engines under 1L (smaller-engine cars) and November 2015 and April 2016 tax hikes on 3-wheeler imports

Tax Cut on Smaller-Engine Cars. In November 2014, the Sri Lankan government announced a cut in import tax rates for smaller-engine cars, which came into effect the following day. This change came as a surprise to many since there was no dialogue regarding this change prior to November 2014. As the red line in Figure 1.1 depicts, this change triggered a price drop of about 25% for smaller-engine cars imported after the tax reduction. On the other hand, there was no change in the price for car imports with engines exceeding 1L (larger-engine cars). The tax cut posed an indirect negative effect on the demand for used smaller-engine cars when these new imports became more affordable. This resulted in a drop in resale values of smaller-engine cars imported prior to the tax cut (formal evidence is presented later in the paper). Therefore, we expect borrowers who purchased smaller-engine cars to default more following the tax cut, relative to the borrowers who purchased larger-engine cars.

Tax Hikes on 3-wheeler Imports. The number of 3-wheelers, which serve mainly as taxis, increased steadily from about 400,000 in 2007 to over one million in 2015. This rapid rise raised concerns regarding increased air pollution, traffic and road accidents caused by 3-wheelers. As a part of Sri Lankan government efforts to reduce the growth in 3-wheelers, the import tax was increased on two separate occasions: in November 2015 and April 2016. As a result, new 3-wheeler price rose from LKR 505,000 to 610,000 in November 2015 and to LKR 638,600 in April 2016. Each tax hike went into effect the day after the announcement. This rise in new 3-wheeler pricing caused an increase in resale values of existing 3-wheelers in the country. Demand for new three-wheelers dropped while demand for used counterparts increased their resale values. Figure 1.2 plots the mean price of a new Bajaj (the most popular brand) imported from India in each month and the value of used 3-wheeler originally imported in 2012-2014. After each tax hike, values for used 3-wheelers increases by about 10%. It is clear from Figure 1.3 that the tax hikes were not foreseen since the volume of new 3-wheelers purchases did not spike prior to the announcement. The sharp post-announcement rise reflects sale of dealer inventory imported before the tax hikes.

Changes in Maximum Loan-to-Value Ratio

In a bid to curtail the vehicle imports weighing on the country's balance of payment, on 14 September 2015, the Central Bank of Sri Lanka decreed a loan-to-value (LTV) cap of 70% per vehicle for all types of vehicles.

In November 2016, the government proposed that maximum LTVs be revised again in its 2017 national budget proposals. In January 2017, the Central Bank of Sri Lanka issued a directive clarifying the budget proposals for lender compliance, dramatically reducing LTV cap to 25% for new 3-wheelers and 50% for new motor cars, SUVs and vans from the previous 70% blanket cap. Moreover, LTV cap for trucks and buses were revised upwards to 90%. The order applied only to new vehicles, leaving cap of 70% for older ones intact. Panels A through C in Figure 1.4 show changes in LTV cap for each new vehicle type. The bunching at the LTV caps suggests that the new LTV restrictions are binding.

The key implication of this reduction in LTV cap for new cars, SUVs, vans and 3-wheelers was that it cut the demand for new vehicles in these categories while increasing the demand for similar used vehicle types. Some of the borrowers who wanted to purchase a new car, SUV, van or a 3-wheeler would not be able to afford the high down payment and would be forced to the used-vehicle market since the down-payment requirement for a used vehicle in these categories would be much lower due to higher LTV cap is applied to lower valuations. Thus, the resale value of vehicles already pledged as collateral would rise (formal evidence presented later). Secondary market value of used trucks and buses would drop with the demand for new (used) trucks and buses rising (falling) after the recent loosening in LTV cap up to 90%.

3. Data and Sample Construction

3.1 Data

I obtained access to the internal records of one of the five largest non-bank financial institutions (NBFI) in Sri Lanka, representing, in 2014, more than 10% of the total assets of NBFI. This data set includes a wide span of loan-level data at origination, including the amount, vehicle valuation, term, interest rate, and borrower characteristics for each loan issued by this NBFI from January 2012 to August 2017 (comprising 396,551 loans). The data set also includes month-by-month stream of payments made by the borrower, while also indicating whether (and, if so, when) the loan is in default. 3-wheelers make up 55% of the NBFI's portfolio, motorcycles account for 15%, cars amount to 11%, vans comprise 5%, and trucks constitute 4% of the total number of loans.

3.2 Sample Construction

This paper's empirical analysis deploys three primary data sets constructed from the above database. First data set is used in the tests that use tax cut for smaller-engine cars in November 2014 as the *treatment*. This data set is restricted to only those loans used to purchase vehicles before May 2014 (five months before the tax cut) and where the final payment came due after April 2015 (five months after the tax cut). Smaller-engine cars are the *treated* sample and larger-engine cars with valuation less than LKR 3 million are the *control* sample. Larger-engine cars were not impacted by the tax cut (*treatment*) and larger-engine cars with valuations greater than LKR 3 million were excluded to ensure that the control group would be comparable to the treatment group. The sample consists of loan-month observations five months surrounding the tax cut in November 2014 and the main outcome variable is whether the loan is in default in a particular month. Summary statistics for these samples are given in the first two columns of Table 1.1. Both treatment and control groups are similar based on the observable borrower and loan characteristics, with the exception of the fraction of brand new vehicles.

Second data set is used in tests that use November 2015 and April 2016 tax hikes for 3-wheeler imports as *treatments*. Two separate samples were constructed using loan-month observations within a period of five months surrounding each tax hike. The data is restricted to loans initiated for the purchase of vehicles before May 2015 (five months before the first tax hike), where the final payment date came after September 2016 (five months after the second tax hike). Here, 3-wheelers are the *treatment* sample and, in the baseline regression, all other types of vehicles are considered the *control* sample. I also construct control samples by matching borrower characteristics and the 'type of vehicle use' to ensure control samples are comparable to the treatment sample. 'Type of vehicle use' indicates whether the underlying vehicle is used for personal use (i.e. as a consumption good) or as a productive asset that generates income. The summary statistics of this sample are given in columns (3) and (4) in Table 1.1. Compared to other types of vehicles, loans for 3-wheelers have a shorter term, higher interest rates, and higher loan-to-value ratio, while also yielding more brand new vehicles.

Third data set is used in the tests that use the change in loan-to-value ratio (LTV) caps in January 2017 as the treatment. This sample is restricted to auto loans originated before July 2016, with a maturity date after June 2017, and it consists of loan-month observations from August 2016 to April 2017. Summary statistics for this sample are reported in the last three columns of Table 1.1. Compared to other types of vehicles, cars, SUVs and vans have longer terms and significantly lower interest rates.

4. Empirical Methodology

As mentioned above, I use policy changes on vehicle import tax rates and LTV caps to identify the causal effect of collateral value on borrower default (see section 1. for details). I now describe the empirical methodology.

4.1 Import Tax Changes

First, I verify the baseline hypothesis–that tax changes had an impact on resale values of treated vehicles (i.e., smaller-engine cars and 3-wheelers). Having no monthly valuations of all the vehicles pledged as collateral to the lender, I rely on the appraised valuations of used vehicles pledged as collateral for new loans within the five months surrounding each tax change. The central idea is to test whether the valuations of used smaller-engine cars (3-wheelers) dropped (increased) after the tax change(s). I estimate this specification separately for each tax change. In the case of smaller-engine cars, I use small larger-engine cars as the control sample, and for 3-wheelers I use all the other types of vehicles as the control sample.

$$log(valuation)_{ivmd} = \alpha_v + \alpha_{md} + \sum_m \beta_m \; Treated\ Vehicle_i \times m + \epsilon_{ivmd} \tag{1.1}$$

where i, v, m, and d represent borrower, vehicle model-manufacturing year, month of loan origination, and district, respectively. *Treated Vehicle* is a dummy variable that takes the value of one if the underlying vehicle is a treated vehicle (i.e., a smaller-engine car or a 3-wheeler). m is a dummy variable representing the number of months since the tax change, meaning m is negative for months before the tax change and positive for months after. This regression includes vehicle model-manufacturing year and district-month fixed effects. The coefficients of interest are β_ms, where β_m estimates the difference between the value of a used *treated* and a used *control* vehicle in month m relative to the same at $m = -5$, the beginning of the sample period. We expect β_m to be zero when m is less than zero and negative (positive) when m is greater than zero in the case of smaller-engine cars (3-wheelers).[5]

To estimate the impact of the tax rate changes on the default behavior of borrowers, I implement standard difference-in-difference research designs using each in tax rate change as treatments. In the smaller-engine car sample, I use larger-engine cars as the control group. For 3-wheelers, I use other types of vehicles as the control group in the baseline specification.[6] I use loan-month observations for loans that originated five months before the treatment and that have maturity dates five months after the treatment in both *treated* and *control* samples. The sample begins five months before the treatment and ends five months after the treatment. Specifically, I estimate the following equation:

$$Y_{imd} = \alpha_i + \alpha_{md} + \beta\ I(m > 0) \times \mathit{Treated\ Vehicle}_{imd} + e_{imd} \tag{1.2}$$

where Y_{imd} stands for the outcome variable for loan i in month m in district d. In the baseline specification Y_{imd} is a dummy variable that indicates whether the loan i is in default in month m, where m represents the number of months since the treatment; the period prior to the treatment is indicated by negative values of m. Loan level fixed effects (α_i) are included to capture any time-invariant characteristics of the borrower. $District - Month$ fixed effects (α_{md}) account for common shocks across all loans in a given district. $I(m > 0)$ is a dummy variable equal to zero for the period before the tax change and one after the tax change. $\mathit{Treated\ Vehicle}_{imd}$ is a dummy variable equal to one for treated vehicle type and zero for the control sample. The coefficient of interest is β, which captures the difference in the default rate between treated and control samples after the treatment and relative to the same prior to the treatment. I expect the β to be positive for smaller-engine cars and negative for 3-wheelers.

[5] A zero β_m when $m < 0$ provides support for the parallel trends assumption.
[6] I construct matched control samples in later tests

4.2 Loan-to-Value Ratio (LTV) Restrictions

As a result of the unanticipated changes to the LTV caps, the resale values of used cars, SUVs, vans, and 3-wheelers should increase as buyers who would have bought a new vehicle are now forced to buy a used vehicle. Similarly the values of used trucks and buses should decrease. We expect the default rates of vehicle types whose LTV cap dropped to increase and vice versa.

To verify that the LTV restrictions indeed had an impact on the valuations of vehicles previously pledged as collateral, I implement the following specification. As I did before, I rely on auto loans initiated to finance the purchase of *used* vehicles originated five months surrounding the policy change in January 2017.

$$log(valuation)_{ivmd} = \alpha_v + \alpha_{md} + \sum_p \beta_p \ I(m > 0)_i \times I(type = p)_{imd} + \epsilon_{ivmd} \tag{1.3}$$

where i, v, m, and d represent borrower, model-year of the vehicle, month of origination and district respectively. The dummy variable m represents the number of months since the rule change, meaning it is negative for months before the rule change and positive for months after. Dummy variable $I(m > 0) \times I(type = p)_{imd}$ takes the value one for auto loans to finance vehicle type p (where $p \in$ *(3-wheeler, bus/truck, car/SUV/van)*) after the rule change and zero otherwise. This specification includes model-year (α_v) fixed effects and origination month-district (α_{md}) fixed effects. We expect β_p to be positive when $p \in$ *(3-wheeler, car/SUV/van)* and negative when $p =$ *bus/truck*.

Next, I implement the following specification to understand the effect of changes to LTV caps on default rate.

$$Y_{imd} = \alpha_i + \alpha_{md} + \sum_p \sum_q \ \beta_{pq} \ I(type = p)_{imd} \times I(m \in q)_{imd} + e_{imd} \tag{1.4}$$

where for loan i, in month m, and in district d, this regresses Y_{imd}–a dummy variable that indicates whether the loan i is in default in month m–on a dummy variable which equals one only if vehicle type equals p (where $p \in$ *[3-wheeler, bus/truck, car/SUV/van]*) and m is in period q. q indicates whether m is before the announcement (i.e., $m <$ November 2016), before the implementation (i.e., December 2016 $< m <$ January 2017), or after the implementation (i.e., $m >$ February 2017). Coefficients of interest are β_{pq} which measure the difference-in-differences– that is, the change in the default rate difference between vehicle type p and omitted group in time period q relative to the omitted time period. Omitted vehicle type is 3-wheeler and omitted time period is period before the announcement (i.e. $m <=$ November 2016). Loan level fixed effects (α_i) are included to capture any time-invariant characteristics of the borrower. District-month fixed effects (α_{md}) capture monthly effects that affect all borrowers in a particular month m in a given district d.

5. Empirical Results

This section documents how unanticipated changes in vehicle import taxes (subsection 5.1) and changes to loan-to-value caps (subsection 5.2) impact borrowers default behavior. First, I verify that each change had significant impact on the resale values of vehicles pledged as collateral. Next, I provide evidence that borrowers respond strategically to these changes.

5.1 Impact of Import Tax Changes on Collateral Value and Default

Tax Cut on Smaller-Engine Car Imports

This section looks at the effects of tax cut on smaller-engine car imports. First, I estimate equation 1.1 to evaluate the impact on resale values of smaller-engine cars. Smaller-engine cars are the *treated* sample and larger-engine cars are used as the *control* sample. Samples consist of auto loans initiated to finance the purchase of *used* vehicles within the five months surrounding each tax change. Figure 1.5 presents the results. This figure plots the estimated β_m for each m. After the treatment (i.e., when $m > 0$), a significant drop in β_m is observed. This drop is in line with the prediction that the tax cut triggered a drop in resale values of smaller-engine cars relative to resale values of larger-engine cars. Estimates suggest that the value of used smaller-engine cars dropped by about 10% following the tax cut. Furthermore, insignificant β_m before the tax cut (i.e., when $m < 0$) suggests that resale values of both smaller-engine cars and larger-engine cars moved in parallel before the tax cut.

Having established that tax cut had a negative significant impact on the resale value of smaller-engine cars, I next turn to the impact on default. Table 1.2 presents these results. Here, smaller-engine car loans are the *treated* sample, while the *control* sample consists of loans initiated to finance purchases of larger-engine cars. Both samples consist of loan-month observations five months surrounding the tax cut. Column (1) presents results from estimation of equation 1.2. Column (2) includes loan-level and borrower-level control variables, but does not include loan fixed effects. Estimates suggest that default rate for cars with engines increased by 0.4% to 0.6% following the tax cut. This translates to an approximately 44% to 66% increase in the default rate compared to the 0.9% default rate prior to the tax cut. Control variables in column (2) have the expected sign. The high R^2 value in column (1) is due to loan fixed effects.

Identification using difference-in-difference method relies on the parallel trend assumption. Under this assumption, default rates of both treated and control groups would have trended similarly if there were no tax cut in the treatment sample. To test for the parallel trends assumption, I estimates the following equation:

$$Default_{itd} = \alpha_i + \alpha_{md} + \sum_m \beta_m \ Treated\ Vehicle_i \times m + \epsilon_{ivmd} \tag{1.5}$$

where i,m, and d represent borrower, month and district respectively. Dummy variable m represents number of months since the tax hike and a negative m indicates months prior to tax hike. This regression includes loan (α_i) and district-month (α_{md}) fixed effects. Figure 1.6 presents the estimates of β_m in equation 1.5 graphically. Y-axis plots the estimates of β_m against m. Vertical lines indicate 95% confidence intervals. Insignificant estimates of β_m when $m < 0$ provides support for the parallel trends assumption. As such, the difference in the default rate between treatment and control groups are not different from the rates seen at the beginning of the period.

Tax Hikes on 3-wheeler Imports

To verify that November 2015 and April 2016 tax hikes impacted the resale values of 3-wheelers, I implement equation 1.1 using loans initiated to finance the purchase of *used* vehicles five months surrounding each tax hike. I expect the valuations of used 3-wheelers (treated group) to increase after each tax hike relative to the valuations of other vehicles (control group), meaning that β_m to be positive and significant when $m > 0$. Results of this estimation is presented graphically in Figure 1.7. Panel A and B show the impact after November 2015 and April 2016 tax hikes respectively. Y-axis plots the estimated β_m coefficient with the 95% confidence interval. These figures suggest that, relative to other vehicle types, value of the used 3-wheelers increased by about 10% following each tax hike. The figure also suggests that values of all used vehicles moved in parallel prior to tax hikes.

Table 1.3 looks at the impact of increase in import taxes for new 3-wheelers on the default rate of loans initiated to finance the purchase of 3-wheelers prior to the tax hikes. This table implements the difference-in-difference specification 1.2 where 3-wheeler loans are the treated sample and other types of vehicles are the control sample. Loans in all the samples were originated prior to May 2015 (five months before the first tax hike). Columns (1) and (2) use November 2015 tax hike as the treatment, while the treatment in columns (3) and (4) is the April 2016 tax hike. Samples consist of loan-month observations five months surrounding each tax hike. Column (3) and (4) excludes loans to purchase cars from the control sample since in April 2016 there was an amendment to the import tax structure of the cars as well. The estimated coefficient of interest, which is the one on $I(m > 0) \times \textit{Treated Vehicle}_{imd}$, is negative and significant across all specifications. These results suggest that the monthly default rate of 3-wheeler loans originated prior to tax hikes reduced by 0.3% and 0.5% compared to other types of vehicles after November 2015 and April 2016 tax hikes respectively. Given the mean default rate of 1.7%, this reduction is highly economically significant. Thus, this result indicates that thee default rate reduced by 18% to 29% following the tax hike.

Columns (2) and (4) of Table 1.3 show that results are robust to the inclusion of loan and borrower related controls. Samples in these columns were constructed using the same restrictions. In these regressions I include loan origination month fixed effects to control for difference in the time of loan origination.

One possible concern with my methodology is that other vehicle type borrowers are not an adequate control group because they are too different from 3-wheeler borrowers. One key difference is that 3-wheelers are purchased by less affluent borrowers while other types of vehicles, like cars, are purchased by more affluent borrowers. Thus, one may worry about the possibility that an unobserved shock affected less affluent borrowers at the time of the tax hikes. To address this concern, I use a matched sample of motorbikes as the control sample. Unlike in the more developed countries, motorbikes in Sri Lanka are purchased mainly by less affluent borrowers who cannot afford cars. Matching was based on borrower characteristics, district and loan origination month using propensity score matching. Results of this exercise are reported in columns (1) and (3) in Table 1.4. Interestingly, results in this sample are much stronger than the results shown in Table 1.3.

Another concern may be that 3-wheeler owners are systematically different from other vehicle owners as the collateral is a productive asset for the owners and other vehicle types, such as cars, are mostly consumption goods. As such, it is not implausible that some unobservable shock affected only entrepreneurial borrowers around the tax hikes. To address this concern, I compare default rates of 3-wheeler loans with matched mini-truck loans in columns (2) and (4) in Table 1.4. Mini-trucks are very small trucks with small engines used to transport small loads. As is the case with 3-wheelers, mini-trucks generate the main source of income for many mini-truck owners. I select mini-truck borrowers who listed their occupation as "self-employed" or "business" so that they are comparable to the 3-wheeler borrowers in terms of the source of income. Matching is based on the same procedure as above. Descriptive statistics of the samples before and after are given in the Appendix x. Coefficients remain negative and highly statistically significant.

Having established that 3-wheeler default rates decreased following import tax hikes, I next look at whether already defaulted 3-wheeler borrowers self-cure more in response to the tax hikes. If a borrower's previous default was strategic–i.e. if the borrower was in default despite having the ability to pay–this borrower is more likely to self-cure following the tax hikes. Loan i, is defined as a self-cure in month m, if the loan is current in month m, conditional on the loan being in default in month $m-1$. Samples were constructed for each month $m-1$, using the loans are in default in that month. The self-cure rate is defined as the fraction of loans that are not in default in month m for 3-wheelers and other types of vehicles separately. Figure 1.9 shows that the self-cure rate of 3-wheeler borrowers increases sharply following the tax hikes. Table 1.5 Panel A presents the difference-in-difference estimates for the self-cure rates. Following the November 2015 tax hike the self-cure rate increases by 12.3% and self-cure rate increases by 8.1% following the April 2016 tax hike in 3-wheeler loans. Table 1.5 Panel B shows that the increase in self-cure was not temporary. This table looks at how many borrowers who self-cured just after the tax hikes re-default at any point in the future. While for other types of vehicles, a sizable fraction of the self-cured borrowers re-defaulted subsequently, almost no 3-wheeler borrowers who self-cured re-defaulted afterward. As such, this is consistent with the idea that borrowers who self-cured in response to tax hikes had the ability to pay when they defaulted before the tax hikes.

5.2 Impact of Changes to Maximum Loan-to-Value Ratios on Collateral Value and Default

Results in this section look at changes to borrowers' default behavior in response to changes to LTV caps. In January 2017, the Central Bank of Sri Lanka reduced the LTV cap for *new* cars, SUVs and vans to 50% and for 3-wheelers to 25%. For buses and trucks, the LTV cap was increased to 90%. Prior to this directive, all the vehicles had the same LTV cap of 70%. After this regulation was implemented, only new vehicles were affected and the LTV cap for used vehicles remained at 70%. This change was initially proposed in late November 2016.

In Table 1.6, I provide evidence that changes to the LTV caps had an impact on the valuations of used vehicles. Results in this table indicate that resale values of *used* trucks and buses dropped by about 20% relative to the values of used 3-wheelers, cars, SUVs and vans. This is due to the emerging lower demand for used trucks and buses caused by lower down-payment requirement for new ones as a result of loosening of LTV restrictions. The sample uses auto loans initiated surrounding the rule change to purchase used vehicles.

Table 1.7 presents the results of regression specification in equation 1.4, which estimates the magnitude of borrowers' response to changes in the LTV caps. The unit of observation is a loan-month. Column (1) includes loan level fixed effects and district-month fixed effects. Column (2) employs the full set of borrower/loan level controls and vehicle type, district-month and origination month fixed effects. The dummy variable $I(type = p)_{imd} \times I(m \in \{Feb2017, Mar2017, Apr2017\})$ captures the difference in default rate between a given vehicle type p, where $p \in \{Car/SUV/Van, Truck/Bus\}$, and the excluded vehicle type (3-wheelers) after the new regulations came in to effect relative to the period before new requirements were announced (i.e., before Dec 2016). The dummy variable $I(type = p)_{imd} \times I(m \in \{Dec2016, Jan2017\})$ captures the change in default rate after the announcement but before implementation.

Under these conditions, I expect the default rates of loans to finance purchases of trucks and buses to increase relative to 3-wheelers after the new regulations came into effect. As both personal vehicles (cars, SUVs and vans) and three wheelers were subject to higher LTV caps, it is not clear if the default rate of personal vehicles should increase or decrease relative to the 3-wheelers. According to the estimates, there is a positive and significant increase in default rates of trucks and buses relative to 3-wheelers after the implementation of new LTV requirement(i.e,. after February 2017) and a decrease in default rate for cars, SUVs and vans relative to 3-wheelers. Coefficients are not significant in the post-announcement period of Dec 2016 to Jan 2017.

This pattern is consistent with the idea that decreasing the maximum LTV for new vehicles increased the value of used vehicles, which in turn reduced the probability of default. The estimate of 0.005 for trucks and buses indicates that default rates increased by 0.5%, approximately a 10% increase in the pre-reform default rate. For cars, SUVs and vans a decrease in default rate of about 0.1% compared to 3-wheelers can be observed. Although the down-payment requirement for 3-wheelers increased by 50% compared to the 25% increase for personal vehicles, similar response among borrowers of both personal vehicles and 3-wheelers can be observed. This indicates that, compared to 3-wheeler borrowers, personal vehicle borrowers are more sensitive to changes in collateral value.

In addition, Figure 1.10 shows the same result graphically. As demonstrated in this figure, insignificant β_m before the treatment supports the parallel trends assumption.

6. Placebo Tests

Regarding results reported in the previous section, a potential concern centers on the notion that results may be driven by different borrower types who are selecting into different vehicle types. In difference-in-difference, it does not matter if borrowers who select in to different types of vehicle have different default behavior as long as they move in parallel before the treatment, meaning that there were no preexisting trends in default behavior. Graphical evidence presented earlier shows that the parallel trends assumption is likely to be satisfied for untreated periods. In this section, I provide further evidence of the identifying the assumption by simulating placebo policy changes before the actual policy change date, aimed at testing whether the results are driven by preexisting trends. If the previous results are driven by preexisting trends, then these simulations should also generate similar effects on default.

First, I simulate the tax hike for 3-wheeler imports in April 2015, before the actual dates of both tax hikes. 3-wheeler loans are considered the treated sample, while other types of vehicles are considered the control sample. Loan-month observations five months surrounding April 2015 (simulated date) constitute the sample. The dummy variable $I(m > 0)$ takes the value one if the loan-month observation is after the simulated tax change. Table 1.8 reports the results of this exercise where column (1) implements the equation 1.2 and column (2) includes control variables. Non-significant results in both columns suggest that there were no preexisting trends in default behavior. Thus, this exercise provides support for my identifying assumptions.

Next, I simulate the LTV cap changes, which were implemented in January 2017, in August 2016. Sample was constructed using loan-month observations five months before and after August 2016. Observations after August 2016 are considered as treated observations and the estimation results of the specification (2) is given in Table 1.9 column (1). Column (2) includes controls with fixed effects. Statistically not significant coefficients in both columns suggest that the identification assumption is valid.

7. Robustness

7.1 Prepayment

When a borrower is liquidity constrained, she has two options: prepay the loan by selling the collateral or default on the loan. If the net outcome of the prepayment is positive, the borrower would prepay instead of defaulting and vice versa. One concern with my analysis is that liquidity constrained borrowers may change their behavior in response to changes in the collateral value. In the case of the tax cut for smaller-engine cars, it is possible that the net positive benefit in prepayment is wiped out by the drop in the vehicle value following the tax cut. Therefore, liquidity constrained borrowers who were planning on prepaying their loan by selling the vehicle may now decide to default. My results could be simply capturing this change in response type by liquidity constrained borrowers, not strategic default. Similarly, following the value increases of 3-wheelers following tax hikes liquidity-constrained borrowers may decide to prepay their auto loans, instead of defaulting.

Yet, I note that this is unlikely to be the case due to severe prepayment penalties applicable to auto loan borrowers. If borrowers prepay, they are typically required to pay 75% of future interest in addition to the balance outstanding. As a result, prepayments are not common in auto loans. During my sample period, only 0.16% of the auto loans were prepaid. Regardless, I run a robustness test to rule out this alternative explanation.

As with the previous regressions, I run the equation 1.2 with Y_{imd} indicating prepayment by borrower i in district d in month m. I include loan fixed effects and district-month fixed effects. Results reported in Table 1.10 suggest that prepayments did not increase significantly following the tax increases.

As an additional test, I split the sample into three subsamples based on the loan origination date: The first sample includes loan originated between July 2013 and December 2014, the second sample includes loans originated between June 2013 and January 2012 and the third sample includes loans originated before January 2012. Most recently originated loans have the highest outstanding balance at the time of tax increase. As such, incentives to strategically default should be strongest in this sample. Results of this exercise are reported in Table 1.11 displaying the strongest effect coming from most recently originated loans. This behavior is more consistent with the strategic default hypothesis.

7.2 Equity Extraction

Another concern revolves around the idea that if liquidity-constrained borrowers are able to extract their equity in the form of a secondary loan following the increases in collateral value, my results may reflect the impact of relaxed liquidity constraints. Detailed monthly level data allows me to track the total loan balance from month to month. In this data, I do not observe any increase in loan balance after value increases and regulations do not allow borrowers to use the same asset as the collateral for a secondary loan with another lender. In addition, in private conversations, management of the lender also confirmed that there were no equity extractions following the collateral value increases.

8. Conclusion

This paper presents new evidence that auto loan borrowers respond strategically to changes in the value of the collateral. I use several policy changes in Sri Lanka to generate exogenous variation of collateral value that is unrelated to borrowers' unobservable characteristics and their ability to pay. Results show that following a drop (increase) in collateral value default rate rose (fell) by 44% (25%). The self-cure rate increased three fold after a 10% increase in the collateral value.

I take a number of steps to mitigate the scope for alternative interpretations of my evidence. I simulate regulation changes in earlier dates to confirm that these results are not driven by preexisting trends. I also match the control sample to treatment sample to address concerns about selection bias.

The findings in this paper contribute to the academic debate on the importance of borrowers' financial incentives on their default decisions and have direct policy implications. This finding is particularly relevant for ex-ante policies aimed at reducing future default rates. Economically significant impact of the collateral value can be used to justify some of the credit market regulations such as maximum LTVs or mortgage insurance.

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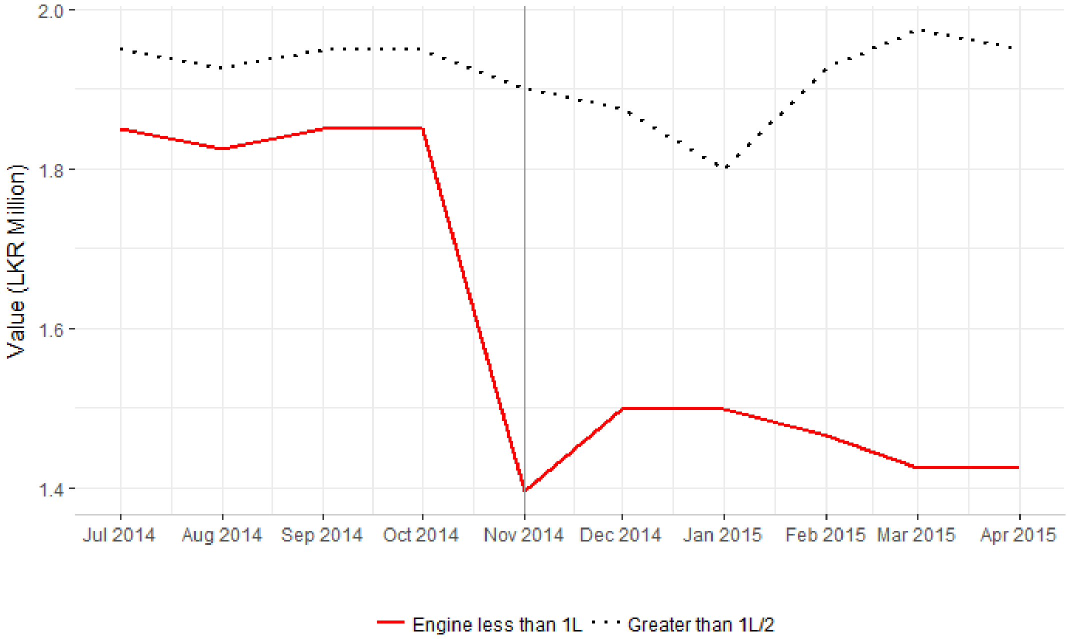
Figure 1.1: Price of New Cars

This figure plots the prices of new cars with engine capacity less than 1L (solid red line) and greater than 1L(dashed black line). Import tax rate was cut in November 2014 for cars with engine capacity less than 1L.



9. Figures

Figure 1.2: Value of New and Used 3-wheelers

This figure shows the monthly price of a new 3-wheeler (solid red line) and values of used 3-wheelers in each month. Dashed black lines plot the mean values of used 3-wheelers made 2012-2014. Gray line is the value of a used small car (2013 Honda Fit). Import tax rate was raised for 3-wheelers in November 2015 and April 2016.

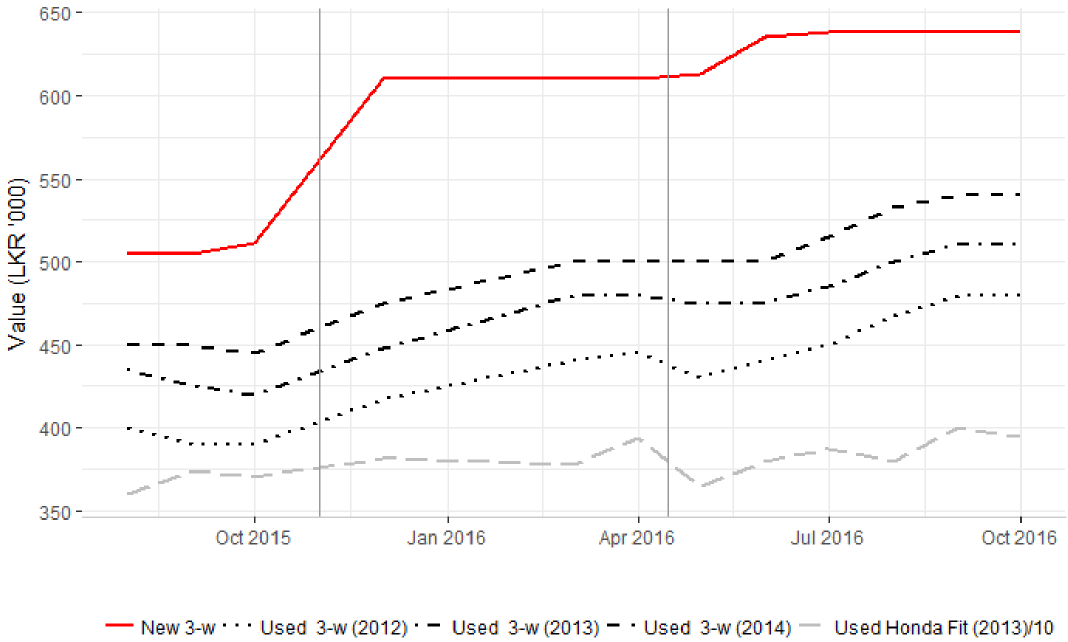


Figure 1.3: Number of 3-wheelers Purchased

This figure plots the monthly number of loans originated by the lender for new (solid red line) and used (dashed black line) 3-wheelers. Notable spikes just after the tax increases in November 2015 and April 2016 reflect sales of dealer inventory previously imported to the country.

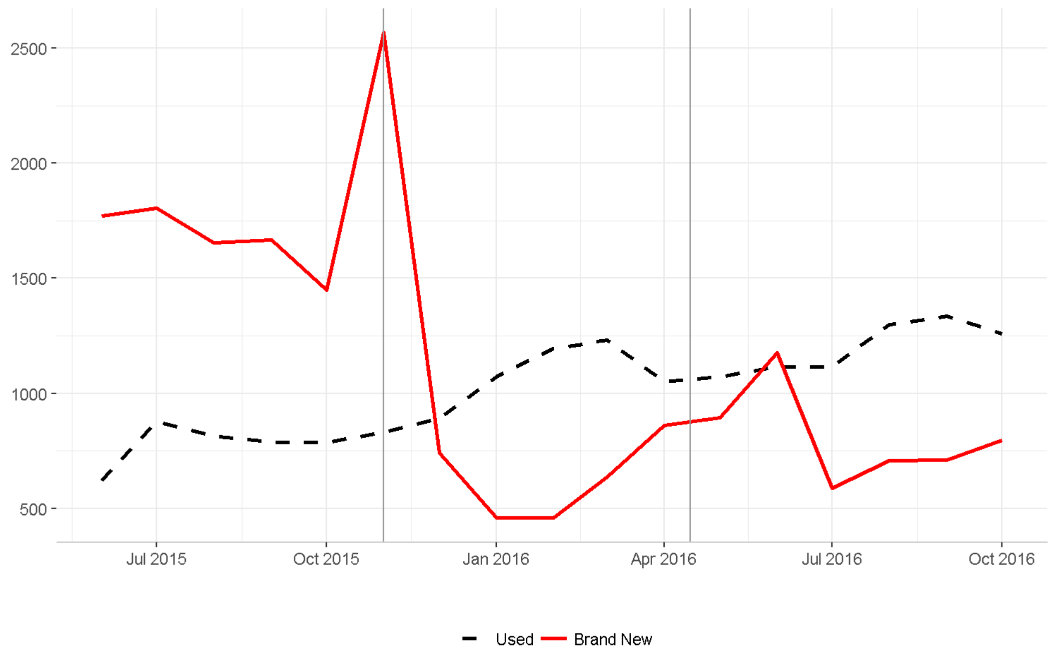
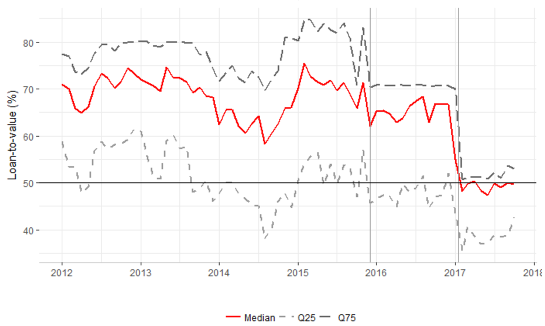


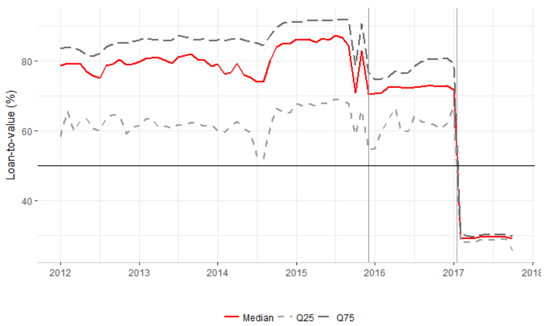
Figure 1.4: Loan-to-Value Ratios for New Vehicles

This figure shows the changes to maximum loan-to-value ratios for different types of *new* vehicles. Red solid line represents the median, and gray dashed lines represent first and third quartiles. Maximum loan-to-value ratio was restricted to 70% for *all* auto loans beginning December 2015. After January 2017, specific vehicle types were subject to different maximum loan-to-value ratios. The January 2017 revision applied only to loans for purchase of a new vehicle.

Panel A: Cars, SUVs and Vans



Panel B: 3-wheelers



Panel C: Trucks and Buses

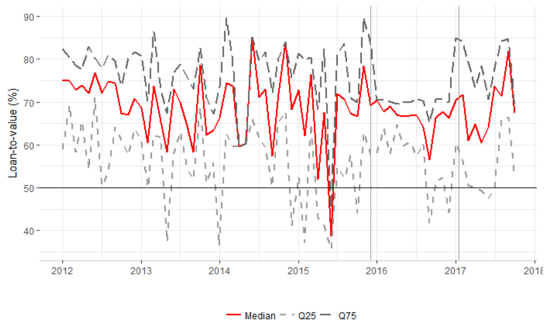


Figure 1.5: Effect of Tax Cut on the Value of Used Smaller-Engine Cars

This figure shows the impact of November 2014 import tax cut on the resale value of used smaller-engine cars (with engine capacity less than 1L). November 2014 tax cut applied only to new imports of smaller-engine cars. The Y-axis plots β_m: the difference in value between used smaller engine car and larger-engine cars (with engine capacity above 1L) in month m relative to the value difference at the beginning of the sample period. β_m is estimated from the equation below where i, v, m, and d represent borrower, vehicle model, months since the tax cut, and district respectively. Sample consists of auto loans initiated to purchase used cars five months before and after the tax cut.

$$log(valuation)_{ivmd} = \alpha_v + \alpha_{md} + \sum_m \beta_m \times \ \ smaller\text{-}engine\ car_i \times m + \epsilon_{ivmd}$$

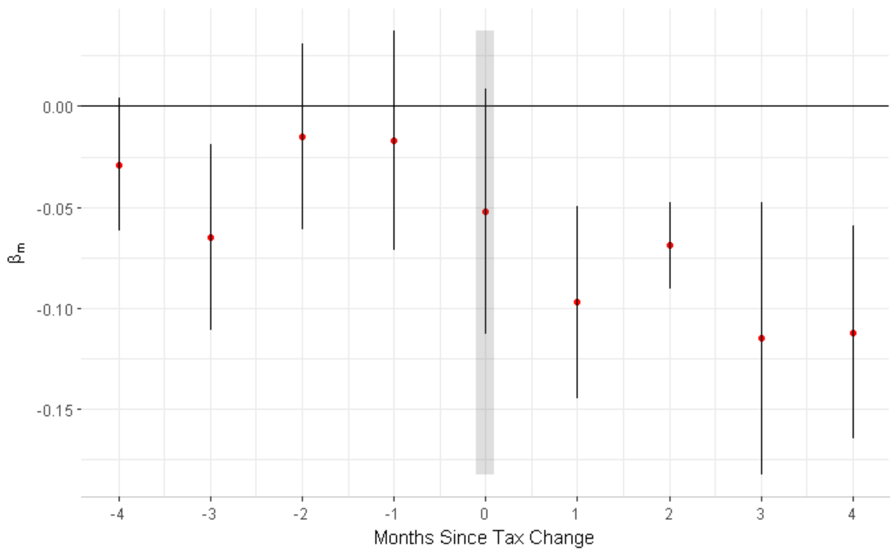


Figure 1.6: Effect of Tax Cut on Smaller-Engine Car Loan Default

This figure shows the impact of November 2014 import tax cut on the default rate of auto loans secured by smaller-engine cars (with engine capacity less than 1L). November 2014 tax cut applied only to new imports of smaller-engine cars. The Y-axis denotes the coefficient estimate β_m from the equation below for months since each tax increase, m. Subscripts i, m, and d represent borrower, months since tax cut, and district respectively. β_m estimates the difference between the default rate of smaller-engine and larger-engine car loans relative to the default rate difference at the beginning of the period. Sample consists of loan-month observations five months before and after the tax cut.

$$Default_{imd} = \alpha_i + \alpha_{md} + \sum_m \beta_m \times smaller\text{-}engine\ car_i \times m + \epsilon_{ivmd}$$

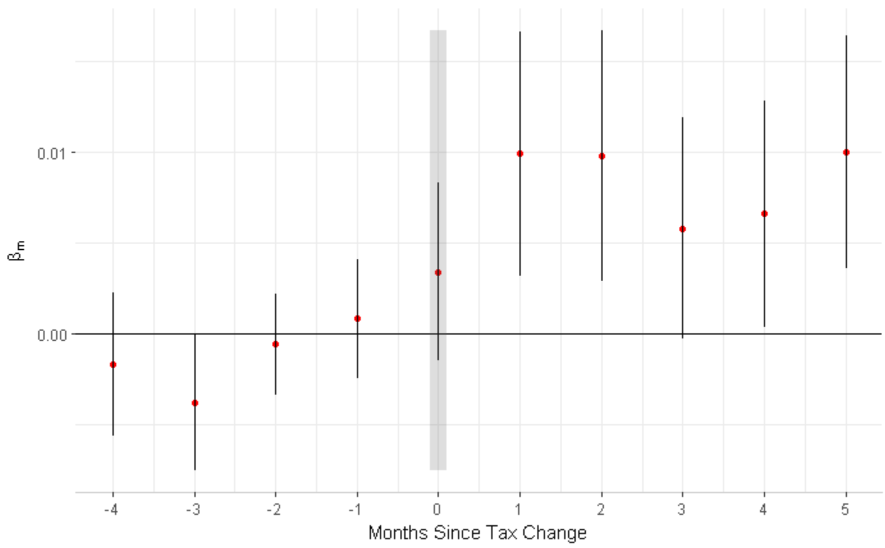
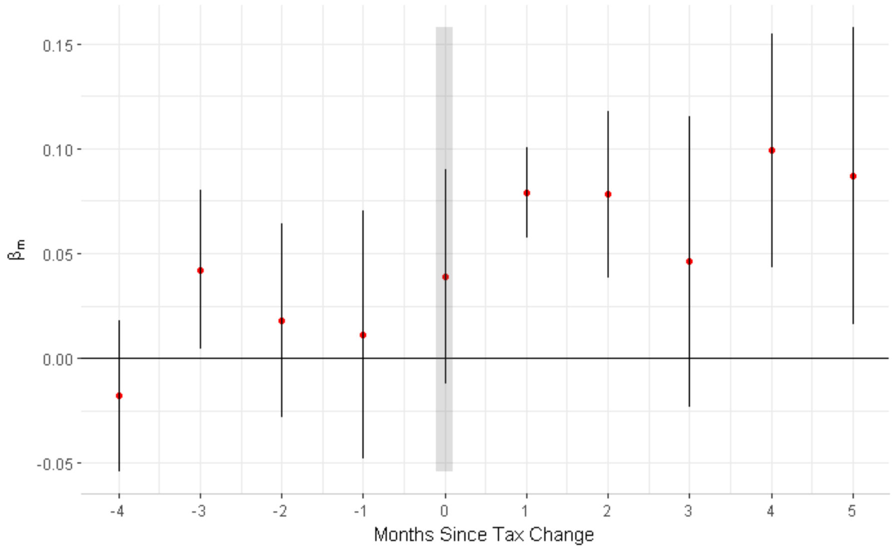


Figure 1.7: Effect of Tax Hikes on Used 3-wheeler Value

This figure shows the impact of November 2015 and April 2016 import tax hikes on the resale value of used 3-wheelers. These tax hikes applied only to new 3-wheeler imports. The Y-axis plots β_m: the difference in value between used 3-wheelers and other types of vehicles in month m relative to the value difference at the beginning of the sample period. β_m is estimated from the equation below where i,v,m, and d represent borrower, vehicle type, months since each tax hike, and district respectively. Samples consist of auto loans initiated to purchase used vehicles five months before and after each tax hike. Panel A and Panel B plot the effects of November 2015 and April 2016 tax hikes respectively.

$$log(valuation)_{ivmd} = \alpha_v + \alpha_{md} + \sum_m \beta_m \times \text{ 3-wheeler}_i \times m + \epsilon_{ivmd}$$

Panel A: Value after November 2015 Tax Hike



Panel B: Value after April 2016 Tax Hike

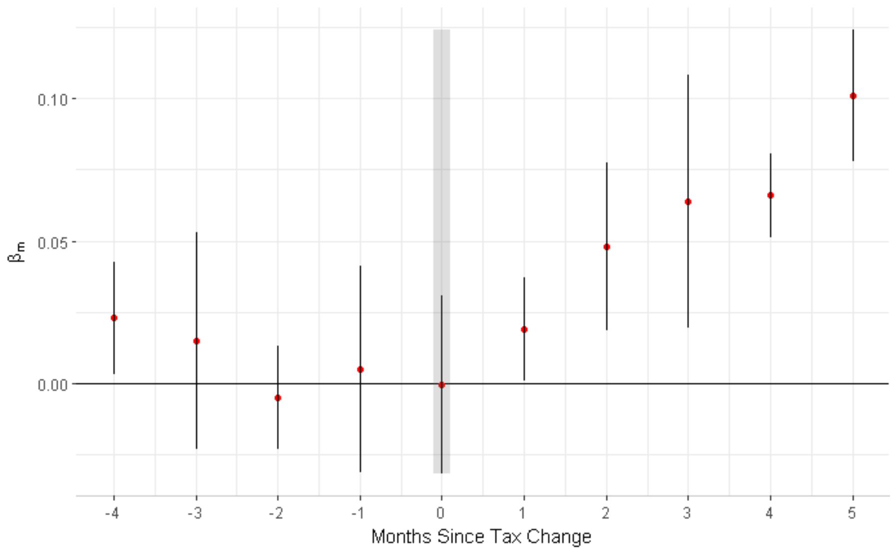
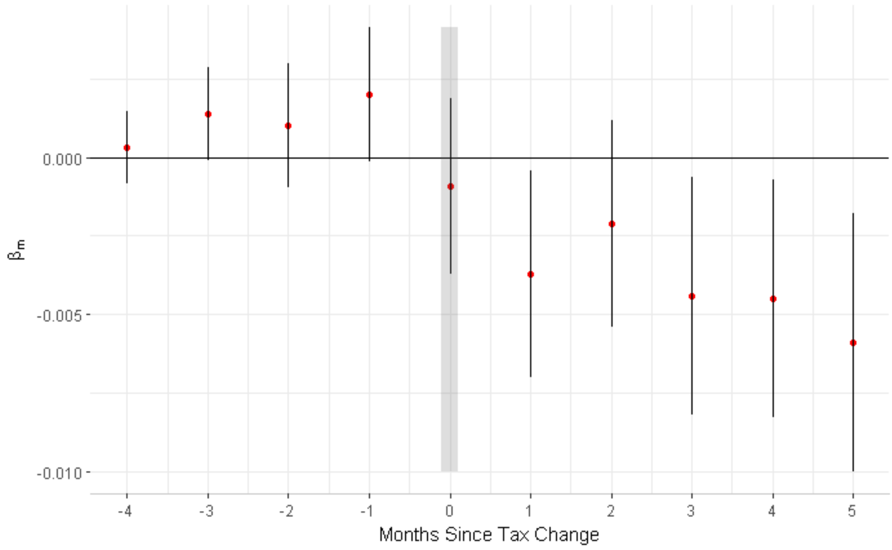


Figure 1.8: Effect of Tax Hikes on 3-wheeler Loan Default

This figure shows the impact of November 2015 and April 2016 import tax hikes on the default rate of auto loans secured by 3-wheelers. These tax hikes applied only to new 3-wheeler imports. The Y-axis denotes the coefficient estimate β_m from the equation below for months since each tax hike, m. Subscripts i, m, and d represent borrower, months since tax cut, and district respectively. β_m estimates the difference between the default rate of 3-wheeler and other vehicle loans relative to the default rate difference at the beginning of the period. Samples consist of loan-month observations five months before and after each tax hike. Panel A and Panel B plot the effects of November 2015 and April 2016 tax hikes respectively.

$$Default_{imd} = \alpha_i + \alpha_{md} + \sum_m \beta_m \times \ \textit{3-wheeler}_i \times m + \epsilon_{imd}$$

Panel A: November 2015 Tax Hike



Panel B: April 2016 Tax Hike

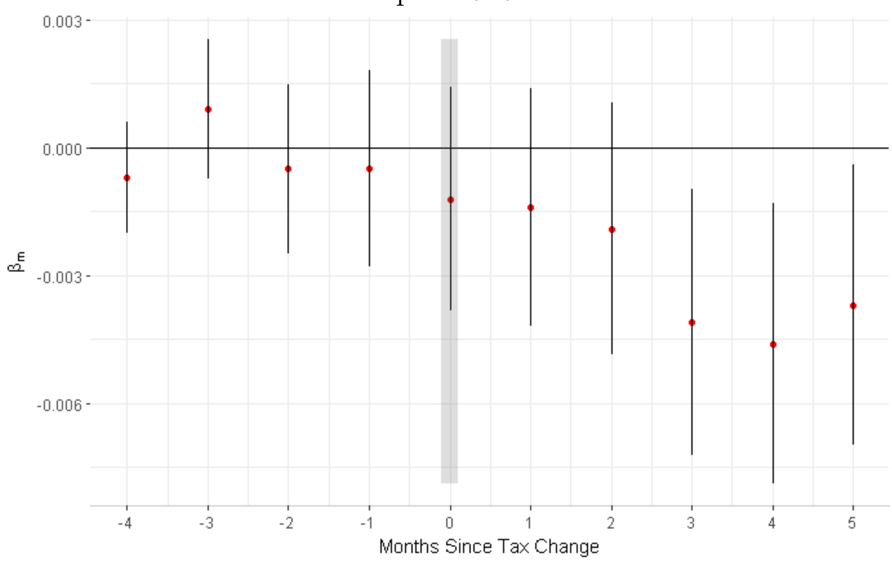


Figure 1.9: Monthly Self-Cure Rates for 3-wheeler Loans

This figure plots the monthly difference in mean self-cure rates for 3-wheelers and other vehicles from October 2015 through August 2016. Loan i is defined as a self-cure in month m when current in month m, after being in default at month $m-1$. Samples for each month $m-1$ comprise loans in default for that month. Import taxes for new 3-wheeler imports were increased in November 2015 and April 2016.

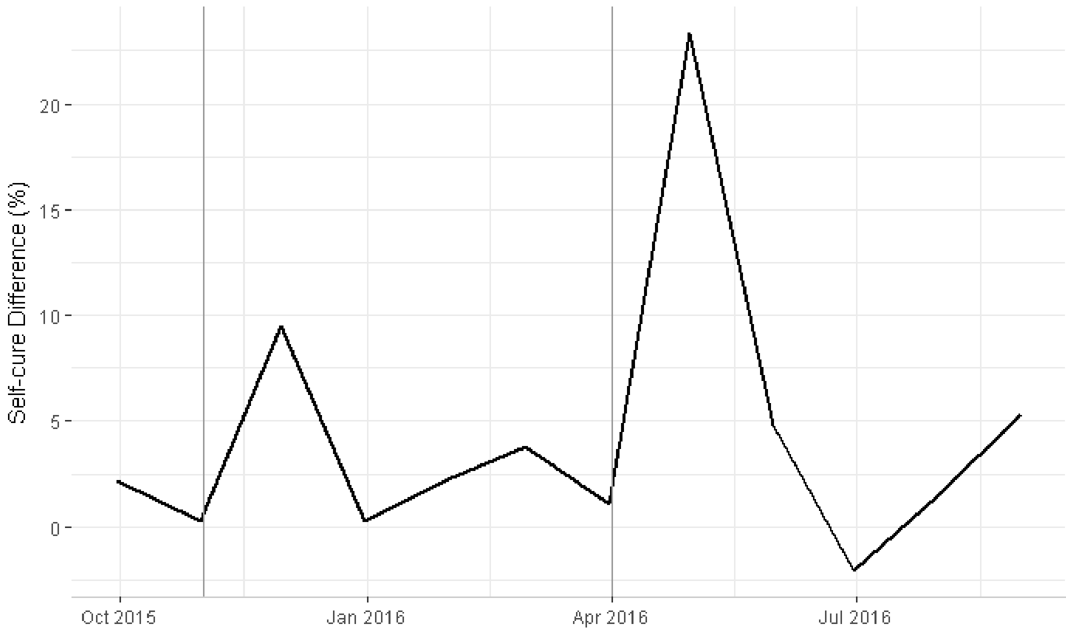
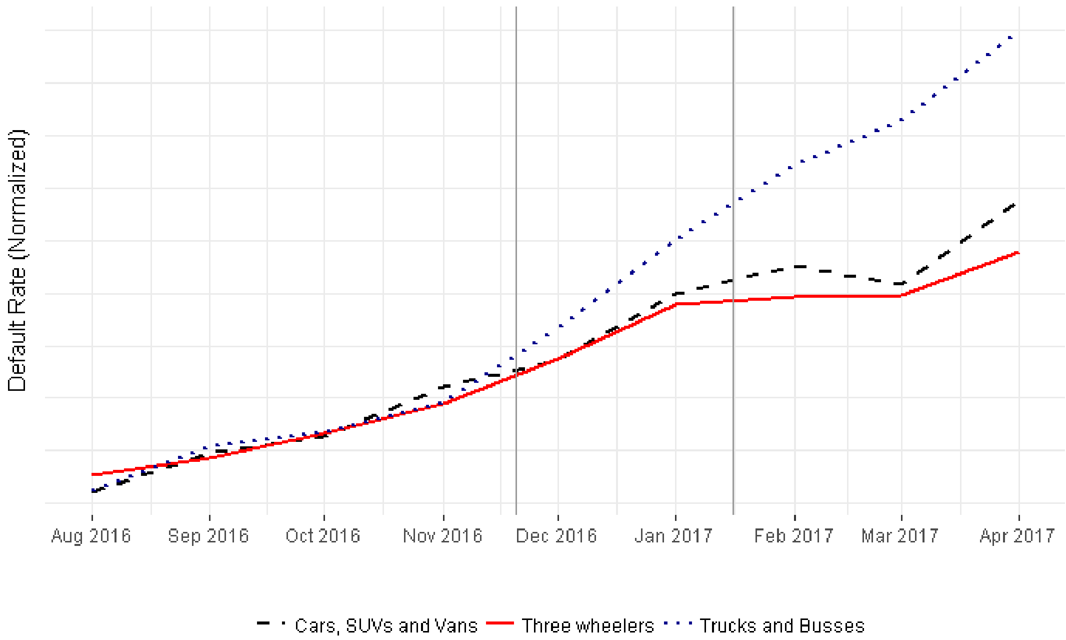


Figure 1.10: Effect of Loan-to-Value Changes on Monthly Default Rates

This figure plots the monthly mean default rate for each vehicle type in each month from August 2016 through April 2017. Changes to maximum loan-to-value ratios were proposed late November 2016 and implemented mid January 2017. Maximum loan-to-value ratio was cut to 50% for cars, SUVs and vans while dropping to 25% for 3-wheelers. The bus-truck maximum loan-to-value ratio was increased to 90%.



10. Tables

Table 1.1: Summary Statistics

This table shows sample means and standard deviations of key variables for each sample used. First two columns provide summary statistics separately for treatment and control samples used in the first set of tests that use unanticipated tax cut for new imports of smaller-engine cars (cars with engine capacity less than 1L) as the treatment. Third and forth columns provide summary statistics for 3-wheeler and other vehicle samples used in the second set of tests. Last three columns provide summary statistics for different vehicle types used in the third set of results, which uses maximum loan-to-value restrictions. Standard deviations are given in parenthesis.

	Tax Change Samples				LTV Restriction Sample		
	Three Wheelers (*Treatment*)	Other Vehicles (*Control*)	Cars with <1L engines (*Treatment*)	Cars with >1L engines (*Control*)	Cars, SUVs and Vans	Trucks and Buses	Three Wheelers
N	29,224	9,025	1,451	880	17,673	3,098	37,254
Loan Year	2013.53	2013.44	2014.44	2014.19	2015.12	2015.17	2015.06
	(0.77)	(0.86)	(0.88)	(0.84)	(0.69)	(0.73)	(0.67)
Loan Amount (LKR)	382,403	750,191	934,179	1,063,925	1,474,508	1,239,012	432,351
	(127,737)	(493,155)	(388,069)	(510,546)	(1,430,583)	(1,200,029)	(152,641)
Valuation (LKR)	419,294	1,030,202	1,300,993	1,646,840	2,290,953	1,950,972	473,238
	(95,637)	(666,554)	(495,619)	(552,988)	(2,077,911)	(1,555,818)	(116,372)
LTV (%)	75.38	74.06	64.99	60.66	63.68	61.45	73.72
	(20.74)	(51.51)	(30.46)	(26.89)	(54.44)	(39.08)	(20.17)
Interest Rate (%)	26.74	23.19	15.55	17.39	15.58	17.46	23.48
	(5.78)	(6.45)	(3.58)	(3.91)	(2.41)	(3.03)	(5.83)
Loan Term (months)	42.94	44.98	53.30	50.66	50.87	43.59	44.51
	(9.93)	(10.66)	(9.09)	(9.43)	(10.41)	(9.91)	(11.69)
Brand New	0.62	0.53	0.44	0.07	0.22	0.04	0.69
	(0.48)	(0.50)	(0.50)	(0.25)	(0.41)	(0.20)	(0.46)
Male	0.80	0.78	0.72	0.73	0.76	0.81	0.80
	(0.40)	(0.41)	(0.45)	(0.44)	(0.43)	(0.39)	(0.40)
Married	0.65	0.67	0.74	0.76	0.75	0.73	0.72
	(0.48)	(0.47)	(0.44)	(0.43)	(0.43)	(0.44)	(0.45)
Borrower Age	35.97	36.57	37.17	38.95	39.23	40.27	36.51
	(10.79)	(11.02)	(11.04)	(11.15)	(11.27)	(11.32)	(11.02)

Table 1.2: Effect of Tax Cut on Smaller-Engine Car Loan Default

This table shows the impact of November 2014 import tax cut on the default rate of smaller-engine car loans initiated prior to the tax cut. Smaller-engine cars (with an engine capacity less than 1L) were affected by the tax cut. The dependent variable is a dummy variable equal to one when borrower i is not current in month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after the tax cut. Dummy variable *Smaller-engine* is equal to one when the vehicle type received the treatment (i.e. a smaller-engine car). Larger-engine cars (with an engine capacity greater than 1L) formed the control sample. Column (1) reports the estimation results of equation 1.2 and column (2) includes controls. Samples consist of loan-month observations five months pre- and post- the tax cut. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
$I(m > 0)$ × Smaller-engine	0.004*	0.007***
	(0.002)	(0.003)
Smaller-engine		-0.009*
		(0.005)
Loan Age	-0.0002	-0.001
	(0.001)	(0.002)
Loan Age^2	0.000	-0.00001
	(0.000)	(0.000)
LTV		0.007**
		(0.004)
Interest Rate		0.002**
		(0.001)
Married		0.001
		(0.003)
Male		-0.002
		(0.004)
Borrower Age		-0.0002
		(0.000)
log(Valuation)		-0.0002
		(0.007)
Brand New		0.001
		(0.006)
Other Deposits		-0.002
		(0.002)
Loan FE	✓	✗
District × Month FE	✓	✓
Origination Month FE	✗	✓
Observations	18,943	18,943
Adjusted R^2	0.705	0.077

Table 1.3: Effect of Tax Hikes on 3-Wheeler Loan Default

This table shows the impact of November 2015 and April 2016 import tax hikes on the default rate of 3-wheeler-loans initiated prior to respective tax hike. New 3-wheeler imports were affected by the tax hikes. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after respective tax hike. Dummy variable *3-wheeler* is equal to one if the vehicle type received the treatment (i.e. a 3-wheeler). Other vehicle types formed the control sample. Columns (1) and (3) report the estimation results of equation 1.2 and columns (2) and (4) include controls. Samples consist of loan-month observations five months pre- and post-respective tax hike. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Nov 2015 Tax Hike		Apr 2016 Tax Hike	
	(1)	(2)	(3)	(4)
$I(m > 0) \times$ 3-wheeler	-0.003**	-0.004***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.002)	(0.002)
Loan Age^2	-0.00002***	-0.00004***	0.000	-0.00003***
	(0.000)	(0.000)	(0.000)	(0.000)
LTV		0.00003		0.00003
		(0.000)		(0.000)
Interest Rate		0.002***		0.002***
		(0.000)		(0.001)
Married		0.006***		-0.0001
		(0.001)		(0.002)
Male		-0.005***		-0.009***
		(0.002)		(0.002)
Borrower Age		-0.0004***		-0.0003***
		(0.000)		(0.000)
log(Valuation)		0.007**		0.008
		(0.004)		(0.006)
Brand New		-0.015***		-0.010**
		(0.004)		(0.005)
Other Deposits		-0.002		-0.003
		(0.003)		(0.006)
Loan FE	✓	✗	✓	✗
District $\times$ Month FE	✓	✓	✓	✓
Origination Month FE	✗	✓	✗	✓
Vehicle Type FE	✗	✓	✗	✓
Observations	378,837	378,837	226,462	226,462
Adjusted R^2	0.742	0.026	0.803	0.019

Table 1.4: Effect of Tax Hikes on 3-Wheeler Loan Default: Matched Difference-in-Difference

This table shows the impact of November 2015 and April 2016 import tax hikes on the default rate of 3-wheeler-loans initiated prior to respective tax hike. New 3-wheeler imports were affected by the tax hikes. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after respective tax hike. Dummy variable *3-wheeler* is equal to one if the vehicle type received the treatment (i.e., a 3-wheeler). The control sample in columns (1) and (3) consists of matched motorbike-loans and control sample in columns (2) and (4) consists of matched mini-truck-loans. Samples consist of loan-month observations five months pre- and post-respective tax hike. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Nov 2015 Tax Hike		Apr 2016 Tax Hike	
Control Sample	Motorbikes (1)	Mini-Trucks (2)	Motorbikes (3)	Mini-Trucks (4)
$I(m > 0) \times$ 3-wheeler	-0.004***	-0.006***	-0.008***	-0.003***
	(0.002)	(0.002)	(0.002)	(0.001)
Loan Age^2	-0.00002**	-0.00003***	-0.00004***	-0.00002***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan FE	✓	✓	✓	✓
District × Month FE	✓	✓	✓	✓
Observations	98,904	102,703	100,930	95,094
Adjusted R^2	0.8	0.808	0.752	0.791

Table 1.5: Effect of Tax Hikes on 3-wheeler Loan Self-Cure

Panel A of this table compares the self-cure rates of 3-wheeler and other-vehicles loans before and after the November 2015 and April 2016 import tax hikes for new 3-wheeler imports. Loan i is defined as a self-cure in month m when the loan is current in month m after being in default at month *m-1*. The final row in Panel A reports the difference-in-difference estimate for each tax hike. Panel B reports the percentage of borrowers redefaulted subsequent to self-curing in the month after the tax hikes.

Panel A: Self-Cure Rate

	Nov 2015 Tax Increase		April 2016 Tax Increase	
	t=Nov-15	t=Dec-15	t=Apr-16	t=May-16
3-wheelers	4.86%	36.87%	9.79%	19.84%
Other Vehicles	6.17%	25.88%	8.26%	10.17%
Difference-in-difference	12.30%		8.14%	

Panel B: Redefault Rate

	Nov 2015 Tax Increase	April 2016 Tax Increase
3-wheelers	0.27%	0.00%
Other Vehicles	16.94%	7.45%

Table 1.6: Effect of Maximum Loan-to-Value Ratio Changes on Used Vehicle Values

This table shows the impact of changes to maximum loan-to-value ratios on resale values of used vehicles. Maximum loan-to-value ratio was increased for new trucks and buses after January 2017, and maximum LTV was lowered for cars, SUVs, vans and 3-wheelers. This table reports the estimation results of the equation below where i, v, m, p and d represent borrower, vehicle model, months since the change, vehicle type, and district, respectively. The dummy variable $I(m > 0)$ indicates a loan initiated after the rule change. Sample consists of loans to purchase *used* vehicles five months pre- and post-rule change. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

$$log(valuation)_{ivmd} = \alpha_v + \alpha_{md} + \sum_p \beta_p \; I(m > 0)_i \times I(\mathit{Vehicle\ Type} = p) + \epsilon_{ivmd}$$

	(1)
$I(m > 0)$ × Cars, SUVs and Vans	-0.040
	(0.024)
$I(m > 0)$ × Trucks and Buses	-0.200***
	(0.011)
Model FE	✓
Manufacturing Year	✓
District × Month FE	✓
Observations	21,379
Adjusted R^2	0.729

Table 1.7: Effect of Maximum Loan-to-Value Ratio Changes on Default

This table shows the impact of changes to maximum loan-to-value ratio on default.Maximum loan-to-value ratio was increased for new trucks and buses after January 2017, and maximum LTV was lowered for cars, SUVs, vans and 3-wheelers. These changes were first announced in November 2016. This table reports the estimation results of equation 1.4. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variables *Feb 2017-Apr 2017* and *Dec 2016 - Jan 2017* denotes loan-month observations in post-implementation and post-announcement(and pre-implementation) periods, respectively. Dummy variables *Trucks and Buses* and *Cars, SUVs and Vans* denote respective vehicle types. Samples consist of loan-month observations from September 2016 through April 2017. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Feb 2017 - Apr 2017 × Trucks and Buses	0.005**	0.005***
	(0.002)	(0.002)
Feb 2017 - Apr 2017 × Cars, SUVs and Vans	-0.001	-0.001*
	(0.001)	(0.001)
Dec 2016 - Jan 2017 × Trucks and Buses	0.0004	0.001
	(0.001)	(0.001)
Dec 2016 - Jan 2017 × Cars, SUVs and Vans	0.000	0.000
	(0.001)	(0.000)
Loan Age^2	0.000	0.000
	(0.000)	(0.000)
Loan-to-Value		0.003***
		(0.001)
Interest Rate		0.001***
		(0.000)
Married		0.00002
		(0.001)
Male		-0.001
		(0.001)
Borrower Age		-0.0002***
		(0.000)
Valuation		-0.000***
		0.000
Brand New		-0.001
		(0.001)
Loan FE	✓	✗
Month × District FE	✓	✓
Vehicle Type FE	✗	✓
Origination Month FE	✗	✓
Observations	394,767	394,767
Adjusted R^2	0.729	0.009

Table 1.8: Placebo Test: Effect of Simulated Tax Hike on 3-wheeler Default Rate

This table simulates a 3-wheeler tax hike in April 2015 and estimates equation 1.2. The table shows the impact of simulated import tax hike on the default rate of 3-wheeler-loans initiated prior to the simulated tax hike. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after the simulated tax hike. Dummy variable *3-wheeler* is equal to one if the vehicle type received the treatment (i.e., a 3-wheeler). Other vehicle types formed the control sample. Samples consist of loan-month observations five months pre- and post-simulated tax hike. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
$I(m > 0) \times$ 3-wheeler	0.001	0.001
	(0.001)	(0.001)
Loan Age^2	-0.0001***	-0.0001***
	(0.000)	(0.000)
Loan-to-Value		0.0004***
		(0.000)
Interest Rate		0.002***
		(0.000)
Married		0.012***
		(0.001)
Male		-0.004**
		(0.002)
Borrower Age		-0.0004***
		(0.000)
log(Valuation)		0.000
		0.000
Brand New		-0.014***
		(0.003)
Other Deposits		-0.001
		(0.001)
Loan FE	✓	✗
Month × District FE	✓	✓
Vehicle Type FE	✗	✓
Origination Month FE	✗	✓
Observations	298,910	298,910
Adjusted R^2	0.802	0.028

Table 1.9: Placebo Test: Effect of Simulated Maximum Loan-to-Value Ratio Changes on Default

This table simulates the maximum loan-to-value changes in August 2016 and reports the estimation results of equation 1.4. Maximum loan-to-value ratio was increased for new trucks and buses after January 2017, and maximum LTV was lowered for cars, SUVs, vans and 3-wheelers. This table reports the estimation results of equation 1.4. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variables *Sep 2016 - Nov 2016* denotes a loan-month observations in post-simulated period. Dummy variables *Trucks and Buses* and *Cars, SUVs and Vans* denote respective vehicle types. Samples consist of loan-month observations from May 2016 through November 2017. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Sep 2016 - Nov 2016 × Trucks and Buses	0.002	0.001
	(0.001)	(0.001)
Sep 2016 - Nov 2016 × Cars, SUVs and Vans	0.000	0.000
	(0.001)	(0.001)
Loan Age^2	-0.00001***	-0.00001*
	(0.000)	(0.000)
Loan-to-Value		-0.00000**
		0.000
Interest Rate		0.001***
		(0.000)
Married		-0.001
		(0.001)
Male		-0.001*
		(0.001)
Borrower Age		-0.0001***
		(0.000)
log(Valuation)		-0.000***
		0.000
Brand New		0.000
		(0.001)
Loan FE	✓	✗
Month × District FE	✓	✓
Vehicle Type FE	✗	✓
Origination Month FE	✗	✓
Observations	317,356	317,356
Adjusted R^2	0.677	0.008

Table 1.10: Effect of Tax Hikes on 3-wheeler-loan Prepayment

This table shows the impact of November 2015 and April 2016 import tax hikes on the prepayment rate of 3-wheeler-loans initiated prior to respective tax hike. New 3-wheeler imports were affected by the tax hikes. The dependent variable is a dummy variable equal to one if borrower i prepays the loan in full in the month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after respective tax hike. Dummy variable *3-wheeler* is equal to one if the vehicle type received the treatment (i.e. a 3-wheeler). Other vehicle types formed the control sample. Samples consist of loan-month observations five months pre- and post-respective tax hike. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	November 2015 Tax Hike	April 2016 Tax Hike
	(1)	(2)
$I(m > 0) \times$ 3-wheeler	0.0004	0.0001
	(0.000)	(0.000)
Loan Age^2	0.000	0.000
	(0.000)	(0.000)
Loan FE	✓	✓
District × Month FE	✓	✓
Observations	378,837	226,462
Adjusted R^2	0.647	0.953

Table 1.11: Effect of Tax Hikes on 3-wheeler Default Rate by Origination Year

This table shows the impact of November 2015 and April 2016 import tax hikes on the default rate of 3-wheeler-loans initiated prior to respective tax hike. New 3-wheeler imports were affected by the tax hikes. I divide my sample in to three subgroups based on the whether the loans were originated between July 2013 and December 2014, between January 2012 and June 2013 or before January 2012. The dependent variable is a dummy variable equal to one when borrower i is not current in the month m (otherwise zero). The dummy variable $I(m > 0)$ denotes a loan-month observation after respective tax hike. Dummy variable *3-wheeler* is equal to one if the vehicle type received the treatment (i.e. a 3-wheeler). Other vehicle types formed the control sample. Samples consist of loan-month observations five months pre- and post-respective tax hike. Standard errors are clustered at loan level and reported in parentheses below coefficient estimates. I use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	November 2015 Tax Hike			April 2016 Tax Hike		
	Jul 13-Dec 14	Jan 12-Jun 13	<Jan 12	Jul 13-Dec 14	Jan 12-Jun 13	<Jan 12
	(1)	(2)	(3)	(4)	(5)	(6)
$I(m > 0) \times$ 3-wheeler	-0.014***	-0.01	0.001	-0.009**	-0.004	-0.003
	(0.004)	(0.006)	(0.001)	(0.004)	(0.004)	(0.003)
Loan Age^2	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan FE	✓	✓	✓	✓	✓	✓
District $\times$ Month FE	✓	✓	✓	✓	✓	✓
Observations	51,610	29,452	220,390	70,185	43,935	86,742
Adjusted R^2	0.767	0.804	0.736	0.815	0.816	0.770

Chapter 2

Do Sunk Costs Affect Prices in the Housing Market?

Introduction

The sale or purchase of a home is the largest financial transaction for most households, and can have significant impact on household wealth. Due to the illiquid nature of the market and lack of good comparable transactions, it is hard to objectively determine the fair market value (FMV) of residential properties. Transaction prices are determined by a bidding process which begins with the seller's choice of listing (or asking) price followed by negotiations between sellers and prospective buyers. Given the high valuation uncertainty in the housing market, sellers and buyers are highly susceptible to behavioral biases. In particular, past literature has highlighted that loss aversion can explain sellers' choice of listing price (**?**), and anchoring bias can explain buyers' inability to adjust away sufficiently from the listing price (**?**).

In this paper we examine whether listing prices and transaction prices in the housing market are affected by costs incurred by sellers that are unrelated to the properties' FMV ("sunk costs"). If buyers and sellers are rational and can objectively determine the FMV of residential properties, then sunk costs should have no effect on housing prices.

However, if FMVs are hard to determine, then it is possible that sellers may revalue their property upward after they have incurred a sunk cost (**?** and **?**). If so, sunk costs could have a positive effect on sellers' choice of initial listing price. Furthermore, if buyers use listing prices as anchors to assess property value, then sunk costs may also affect transaction prices.

The main empirical challenge in testing these hypotheses is that it is not easy to identify sunk costs or their causal effect on listing and transaction prices, because most costs incurred by selling homeowners are plausibly related to their properties' FMV. We overcome this challenge by using California's housing market as a laboratory, where the sunk costs that we focus on are past property taxes paid by selling homeowners. We use California for our study because its property tax system has a unique feature, called Proposition 13, as per which two identical properties may have very different property tax assessments depending on when they were purchased. Thus, we are able to identify variation in property tax payments that is unrelated to properties' FMV.

The key feature of California's Proposition 13 is that it ties property tax assessment value to the property's purchase price ("acquisition value") plus annual increases of at most 2%, and allows for significant upward reassessment only following an ownership change due to sale or transfer of the property. Since the passage of Proposition 13 in 1978, house prices across California have experienced annual increases far in excess of the 2% annual cap on increase in assessment values in all years except during the recent financial crisis.[1] As a result, two very similar properties in the same neighborhood can incur significantly different property taxes based on their past transaction history and the conditions in the housing market at the time of their purchase (see the example in Figure 2.2). We exploit the within-zip code variation in property tax bills to test for the causal effect of property taxes on listing prices and transaction prices.

We use Zillow listings from `www.zillow.com` and the Zillow Transaction and Assess-

[1] Another feature of the California system, called Proposition 8, allows for downward revision in assessment values in declining markets. We provide a detailed overview of California's property tax system in Section 1.4; figure 2.1 provides a simple illustration.

ment Dataset (ZTRAX) to obtain data on house characteristics, neighborhood characteristics, price history and tax history for a sample of recently sold homes in California. Zillow also provides us a housing pricing index (HPI) at a monthly frequency for each zip code, which denotes its estimate of the median sale price of a single-family home in that zip code and month. We use this information to compute each property's *Adjusted Purchase Price*, which is obtained by adjusting the property's purchase price for the change in HPI in its zip code from the month of purchase to the month of listing, and serves as a rough estimate of its FMV at the time of listing.

Our analysis aims to uncover the causal effect of property taxes paid by the seller on his/her choice of listing price. We use an instrumental variables (IV) specification which exploits the institutional features of California's property tax system to overcome the omitted variable problem. Specifically, we use the *Years of Ownership* (i.e., the time since the property was purchased) as an instrument for property taxes paid by the seller. Under Proposition 13 assessment, *Years of Ownership* has a significant negative effect on property taxes paid by the seller just prior to listing.[2] We control the regression for the property's *Adjusted Purchase Price*, a host of house and neighborhood characteristics, and include *Zip*$\times$*Listing month* fixed effects to control for unobserved heterogeneity across zip codes and months of listing. The key identifying assumption is that, conditional on all these controls, the instrument has no direct effect on the property's FMV or sellers' behavior at the time of its listing. We use the states of New York and Illinois, where property tax assessments do not depend on years of ownership, to show that years of ownership does not have any effect on house prices outside of the Proposition 13 system. The results of the IV regression indicate that sellers that have incurred higher property taxes are likely to choose a higher listing price, all else equal.

Loss aversion is often cited as a potential explanation for the sunk-cost effect (see **?**).

[2] In falsification tests, we verify that these relationships are unique to California's Proposition 13. Specifically, we assemble a sample of properties from New York and Illinois where property tax assessment values are tied to estimates of market value (i.e., where the property tax system is very different compared to California's Proposition 13). We then verify that *Years of Ownership* does not have any effect on property tax payments prior to listing in these states.

Therefore, in our regressions, we control for the estimated nominal loss that the seller expects to incur relative to his/her purchase price in order to differentiate the sunk-cost effect from loss aversion. To test whether the sunk-cost effect is stronger when sellers expect to incur a loss relative to their purchase price, we divide our sample of listings into two subgroups based on whether the level of HPI at the time of the property's listing (i.e., HPI_{List}) is higher or lower than $HPI_{Purchase}$. Note that the seller is more likely to expect to sell at a loss if the property is located in a zip code where the median house price has declined since the time the property was purchased (i.e., if $HPI_{List} < HPI_{Purchase}$). We find that the effect of sellers' property tax payment on listing price is present in both subgroups, but is significantly stronger in the subgroup of properties where the sellers are more likely to expect to incur a loss relative to their purchase price (i.e., properties for which $HPI_{List} < HPI_{Purchase}$).

Intuitively, the sunk-cost effect (and other behavioral biases) should be stronger when sellers face greater uncertainty regarding the value of their properties. Although we cannot measure price uncertainty, we hypothesize that the more expensive properties within any given zip code will face higher pricing uncertainty because they are more likely to be custom-built, less likely to be standardized, and will have fewer comparable transactions to benchmark against. By a similar logic, price uncertainty should be higher in less active housing markets because sellers in these markets will have fewer comparable transactions to benchmark against. Consistent with our intuition, we find that the effect of sellers' property tax payment on listing price is significantly stronger among high-valued properties relative to low-valued properties (within the same zip code), and in zip codes with low housing transaction volumes relative to zip codes with high transaction volumes.

How does the behavior of sellers affect the selling prices of properties? If buyers are rational and can accurately determine the FMV of properties, then any effect of property taxes on listing prices should be reversed while determining the selling price. However, the literature has shown that buyers in the housing market and even professional real estate agents use listing prices as an anchor to assess property values, and do not adjust away sufficiently from this initial anchor (see ? and ?). If so, it is possible that the effect of property taxes on listing prices is also transmitted to the selling price. Consistent with the presence of anchoring effects, we find that the effect of property taxes on listing price is mostly transmitted to the selling price. An important caveat to these results is that our sample only includes listings that resulted in sale during the 2015–17 period, and does not include listings that were withdrawn.

The main contribution of our paper is that it provides a real-world illustration of how sunk costs affect people's decisions. We do so in the context of residential real estate transactions, which have a significant effect on the wealth of the average buyer and seller. Although the sunk-cost effect is commonly implicated in a variety of contexts (e.g., see ?, ?, ?, and ?), the empirical evidence of this phenomenon is relatively thin and somewhat mixed (see Section 1.1). For instance, some studies find that sunk costs affect consumption decisions (e.g., ?, ?, ?, and ?), whereas other field experiments fail to find any link (e.g., ? and ?). Some other recent studies show that sunk costs affect bidding behavior of agents in penny auctions (?) and likelihood of default by borrowers in the housing market (?).

Our paper also contributes to literature on behavioral biases in the real estate market. In a seminal paper, ? use data from the condominium market in downtown Boston to show that loss aversion determines seller behavior. Specifically, condominium owners subject to a nominal loss set higher asking prices, attain higher selling prices, and exhibit a much lower sale hazard. ? extend the findings of ? to the commercial real estate market where the participants are professionals, unlike the average homeowner. They further show that buyers in this market are subject to the anchoring bias, and do not adjust away sufficiently from the asking price. Both these papers use hedonic regressions to estimate a property's fair market price.

1. Theoretical and Institutional Background

1.1 Sunk-Cost Fallacy

The sunk-cost fallacy arises when individual actions are influenced by costs that have already been incurred and cannot be reversed. Since sunk costs are irreversible, they should not play any role in rational decision making. Yet sunk costs have been implicated in apparently irrational decisions in many experimental studies. In their seminal study, **?** give unexpected price discounts to a randomly selected group of people who are buying season theater tickets, and find that those who pay full price attend more shows than those who receive the discount. **?** find that monthly attendance at an athletic club peaked when the members paid their half-yearly installment and then declined with time. **?** show in a field experiment at an all-you-can-eat pizza restaurant that people who received a discount ate less. However, other field experiments fail to find evidence in favor of the sunk-cost fallacy: **?** give unexpected price discounts to a randomly selected group of Zambians who are purchasing a chemical that cleans drinking water and find no effect on the use of the chemical. Similarly, **?** find no relation between the price that Kenyan consumers paid for insecticide-treated bed nets and their use of the nets.

A few recent papers examine the sunk-cost fallacy in non-experimental settings. **?** finds evidence of the sunk-cost effect in "penny" auctions run by online companies, in which players repeatedly choose to pay a non-refundable fixed bid cost ($0.75 in his data) to become the leader in the auction, and win a good if no other player chooses to bid within a short period of time. **?** exploit the time variation in the cost of Singapore government's license to purchase a car, and show that Singaporeans who pay more for the license drive the car more. Examining mortgage default, **?** find that individuals that pledge higher collateral have a lower hazard to default even after controlling for mark-to-market asset valuation, and they attribute this effect to the sunk-cost fallacy.

The sunk-cost fallacy may also be related to cognitive dissonance theory (**?**) because people may revalue an asset upward after they have incurred a sunk cost. **?** attributes the sunk-cost fallacy to people perceiving outcomes in terms of the prospect theory of **?**, which is characterized by reference dependence, loss aversion, and diminishing sensitivity. The relation between sunk costs and consumption may also arise because people seek to amortize the psychological burden of the irreversible sunk cost in a mental account (**?**), and because of people's desire to not appear wasteful (**?**).

1.2 Property Taxes as Sunk Costs

There are two disparate theories of local property taxes (see **?** and **?**). As per the so-called "benefits view" local property taxes are seen as simply the payment that households make for the bundle of local public services (e.g., schools) that all residents in the local community are entitled to. As per this view, past property tax payments made by the seller are sunk costs, and should have no effect on listing prices after conditioning on all the factors that affect FMV of properties. However, the sunk-cost effect predicts that homeowners who have incurred larger property tax bills in the recent past will choose higher listing prices for their properties, all else equal. This effect should be stronger for homeowners who expect to incur a loss on the sale of their property, and in case of properties that are harder to value, such as relatively high-valued properties and properties located in less active housing markets.

An alternative theory of property taxes is the so-called "capital-tax view" which treats property taxes as a levy on housing capital. As per this view, property values should be decreasing in expected future property tax payments. Therefore, to the extent that past property taxes are correlated with future property tax payments, the "capital-tax view" predicts a negative relation between past property taxes and listing/selling prices.

1.3 The Anchoring-and-Adjustment Heuristic

The anchoring-and-adjustment heuristic, which was first demonstrated by **?**, refers to the disproportionate influence on decision makers to make judgments that are biased toward an initially presented value. It is argued that anchoring effects can explain prices of paintings (?), offer prices in mergers and acquisitions (?), and credit spreads of borrowers (?). In the context of the housing market, the initial listing price could serve as an anchor or heuristic used by a buyer to judge the value of a property, and the buyer may not be able to adjust sufficiently away from the anchor to arrive at a rational market value. ? show that even real estate agents, who are likely to be more informed than the average buyer, are susceptible to such an anchoring bias. ? show that even the more sophisticated buyers in the commercial real estate market are subject to the anchoring bias.

If buyers are rational and can accurately determine the FMV of properties, then any effect of property taxes on listing prices should be reversed while determining the selling price. However, if buyers use listing prices as anchors to judge property values and do not adjust away sufficiently, then the effect of property taxes on the listing price may also be transmitted to the selling price.

1.4 California's Property Tax System

California's property tax system is governed by Proposition 13 (or "People's Initiative to Limit Property Taxation"), which was enacted after being approved through a statewide primary ballot in June 1978, and was subsequently amended by the passage of Proposition 8 (or "Senate Constitutional Amendment No. 67") in November 1978, which allowed for reassessment of real property values in a *declining* market. At the time of its passage, Proposition 13 was hailed as a revolutionary measure for reducing the level and growth of state and local government expenditure as well as sharply restricting the use of the property tax as a source of government revenue (?). A detailed overview of Proposition 13 is available on *BallotPedia* at https://ballotpedia.org/California_Proposition_13_(1978)#cite_note-time-2, and in ? who examines the genesis and fiscal consequences of Proposition 13. We provide a brief summary of Proposition 13 and Proposition 8 below; a more detailed overview of California's property tax system is available in ?. Figure 2.1, which we reproduce from

?, provides an illustration of California's property tax system for a hypothetical home purchased in 1991.

Proposition 13:

The main features of Proposition 13 are as follows: (1) it restricts the effective tax rate to no more than one percent of assessed value; (2) it sets assessed value for a property which has not been transferred since 1975-76 equal to its fair market value in that year plus annual increases of at most two percent (compounded);[3] (3) in the event that the property has been transferred since 1975-76, the market value at the time of sale is used plus annual increases of at most two percent (compounded);[4] and (4) it requires that new taxes or increases in existing taxes (except property taxes) receive a two-thirds approval of the legislature in the case of state taxes, or of the electorate, in the case of local taxes.

The key implication of this "acquisition value" system of taxation embodied by Proposition 13 is that assessment values and property taxes for two very similar properties could vary significantly based on when these properties were last sold. As an example, consider the two properties – labeled House A and House B – shown in figure 2.2, both of which were listed for sale in 2014. Both these properties are located on the same residential block in Annaheim, CA, 302 ft apart from each other, were built in the same year, and have very similar features. The key difference is in terms of their past transaction histories: House A was sold multiple times – once in 2003, then again in 2005 (during the housing market boom) and again in 2010 (following the housing bust) – whereas house B has never been transacted since it was built in 1955. As a result, the two houses have very different assessment values – \$334,439 versus \$59,763 – despite being very similar.

[3]The annual inflation factors applied to property assessment values under Proposition 13 can be found at https://www.boe.ca.gov/proptaxes/pdf/lta15055.pdf. The inflation factor is based on changes in the California Consumer Price Index (CCPI), with a ceiling of 1.02 to cap growth in assessment values at 2 percent per year. Over the period from 1976 to 2009, the inflation factor was exactly 1.02 in all but four years. As a result, market values of properties grew much faster than their assessment values.

[4]Proposition 60 allows homeowners older than 55 years who sell their primary residence and buy a new primary residence within the same county to transfer the base value of their current primary residence to the newly acquired primary residence; Proposition 90 also extends these protections to inter-county transfers among participating counties. More information is available at http://www.boe.ca.gov/proptaxes/faqs/propositions60_90.htm.

Panel C shows the property taxes paid by these properties since 2005. Consistent with Proposition 13, House A's property taxes (which were already high in 2005 due to the prior sale in 2003) increased substantially in 2006 due to the upward revision in its assessed value following the sale in 2005. Subsequently, House A's property taxes decreased substantially in 2009 because Proposition 8 allowed for a downward revision in its assessment values following the housing bust. By contrast, property B's tax bill is significantly lower and exhibits predictable year-on-year growth till 2014 due to the 2 percent per year cap imposed by Proposition 13.

The example in figure 2.2 illustrates that two similar properties in the same neighborhood, that consume the same bundle of local services, can incur very different property taxes based on their past transaction history and the conditions in the housing market at the time of their purchase. Indeed, this feature led to the most notable legal challenge against Proposition 13, *Nordingler v. Hahn*, which argued that acquisition-value assessments are unconstitutional under the federal constitution as a violation of the Equal Protection Clause of the Fourteenth Amendment (see `https://www.law.cornell.edu/supct/html/90-1912.ZS.html` for details). *Nordlinger* reached the Supreme Court of the United States, which upheld Proposition 13 against the *Nordingler* challenge by a vote of 8-1 in 1992. A key factor in Proposition 13's electoral victory was the sentiment that older Californians should not be priced out of their homes through high taxes. The proposition has been called the "third rail" of California politics and it is not politically popular for Sacramento lawmakers to attempt to change it.

Proposition 8:

The Passage of Proposition 13 necessitated the passage of Proposition 8 (or "Senate Constitutional Amendment No. 67") in November 1978, to allow for reassessment of real property values in a *declining* market. For this purpose it amended Article 13A of the state constitution, which had been added by Proposition 13. A reassessment based on a decline in market value is called a "Proposition 8" reassessment. Once a property receives a downward reassessment under Proposition 8, its assessment value moves in line with its estimated market value and is not subject to the 2% per annum annual cap specified under Proposition 13. A property under Proposition 8 may revert to the Proposition 13 assessment in two ways: either its market value increases beyond what its assessment value would have been under Proposition 13 or the property is sold/transferred to another owner (see **?** for details).

As can be imagined, the number of Proposition 8 properties increased dramatically during the crisis and was concentrated in counties hardest hit by the crisis. At the worst point of the crisis, around one-third of all properties had reduced assessments under Proposition 8, as compared to only around 3 percent of properties that had reduced assessments between 2002 and 2005 (**?**).

2. Data, Sample Selection, and Key Variables

2.1 Data Sources

Our primary source of data is Zillow Inc. First, we use www.zillow.com to select a random sample of "recently sold" single family homes in California. We use zillow listings to obtain data on listing price, address, house characteristics and neighborhood characteristics. House characteristics include information such as the address of the property, number of bedrooms and bathrooms, square footage, lot size, year built, and the year in which the house was last remodeled. Neighborhood characteristics include school ratings from the GreatSchools blog (www.greatschools.org), distance to school and close amenities, and availability of public transportation.

We then merge the data extracted from Zillow listings with the Zillow Transaction and Assessment Dataset (ZTRAX) using the house address. ZTRAX is compiled from public records and contains detailed information on property characteristics, transactions, and property taxes across about 3,000 U.S. counties. Property characteristics include address of the property, number of bedrooms, bathrooms, house area, lot size , build year, and type of property. Transactions data files include information such as the type of the deed transfer, transaction price, transaction date, and mortgage amount. Assessment data file contain information on historical assessment values and property taxes. Furthermore, we use ZTRAX data for states of Illinois and New York in our placebo tests.

Another feature of Zillow is that it provides a new home price index – the Zillow Home Value Index (ZHVI) – that is based on its own *estimates* of sale prices of all homes (called "*Zestimates*").[5] We use the ZHVI single-family time series to obtain information on the value of the median single-family home in each zip code area at a monthly frequency; we refer to this as the home price index (HPI).

In addition, we collect information on housing transaction volumes (i.e., number of single-family homes transacted) at the zip code level from www.redfin.com and census tract-level demographic characteristics from 2012-2016 American Community Survey (ACS) estimates.

Two drawbacks of our data are worth emphasizing. First, we only have information on listings that resulted in a sale, but not on listings that were withdrawn. Second, we do not have any information on buyer or seller characteristics, such as age, income, liquidity, and the reasons for selling or buying.

2.2 Sample Construction

As explained in the Introduction, we focus our study only on the state of California for purposes of empirical identification. Accordingly, we use an April 2017 extract of Zillow to obtain information on "recently sold" single-family homes in California. Zillow allows to view a maximum of 520 most recently sold homes for each zip code in California.

We exclude properties with missing addresses, missing listing prices, and properties

[5] This is in contrast to other home price indices that are based on actual sales prices, and hence, fail to cover houses that are not transacted. For instance, Figure 2.3 plots the house price index for California over the period 2000-2016 based on all transaction in California. Zillow argues that a major problem with existing indices is their inability to deal with the changing composition of properties sold in one time period versus another time period. See https://www.zillow.com/research/zhvi-methodology-6032/ for more details regarding the construction of ZHVI.

that were remodeled before being listed for sale. After these exclusions, we are left with 80,332 listings that we were able to match with ZTRAX data. We also remove properties that were listed prior to 2015, properties owned for less than two years, and properties whose listing price is either more than 150% or less than 50% of their estimated market value (see next section for the definition of *Adjusted Purchase Price*, which serves as a rough estimate of market value). After these exclusions, we have a sample of 53,500 single-family home transactions from 708 zip codes in California. Around 69% of these properties were sold in 2016, 23% were sold in 2015 and the remaining 6% of the properties were sold in early 2017.

2.3 Key Variables

We define a property's *Adjusted Purchase Price* as follows:

$$\text{Adjusted Purchase Price} = \text{Purchase Price} \times \frac{HPI_{List}}{HPI_{Purchase}}, \tag{2.1}$$

where *Purchase Price* is the price at which the current seller purchased the property, and HPI_{List} ($HPI_{Purchase}$) denotes the HPI in the property's zip code in the month in which the property is listed for sale (purchased). Thus, *Adjusted Purchase Price* adjusts the property's purchase price for the change in housing market conditions in the local zip code since the property's purchase.

The key dependent variable of interest in the first part of the paper is *Listing Price*, which denotes the initial price at which the seller lists the property for sale. In the second half of the paper, we also examine *Selling Price*, which denotes the price at which the property is finally sold to the buyer.

Our key independent variable of interest is *Property Taxes Paid*, which denotes the dollar amount of total property taxes paid by the seller in the year before listing. We define *Effective Tax Rate* as the ratio of *Property Taxes Paid* to the property's *Adjusted Purchase Price*.

2.4 Summary Statistics

As we explain in Section 3. below, for purposes of empirical identification, we conduct our regression analysis on a sample of properties that were purchased during the period from 1996–2007. We refer to this as the analysis sample. We provide summary statistics for the analysis sample in Table 2. As expected, the distribution of property values is positively skewed, regardless of the metric of property value. For instance, the mean value of *Adjusted Purchase Price* is around \$675,000, which is substantially larger than the median value of around \$528,000. The same patterns apply to the list price and the transaction price. Related to these patterns, we note that there is also substantial heterogeneity in property characteristics, such as number of rooms, square footage, yard size, age, and the quality of neighborhood schools.

There is not much cross-sectional variation in terms of the ratio of the *Listing Price* to the *Adjusted Purchase Price*: the median property is listed at 1.053 times the *Adjusted Purchase Price*, whereas the $25^{th}-$ and $75^{th}-$percentile values for this ratio are 0.965 and 1.179, respectively. This suggests that the *Adjusted Purchase Price* is a reasonable estimate of the property's market value.

Examining property tax bills, we find that the median homeowner in our sample has paid around \$5,000 in property taxes in the year before listing, which amounts to 1% of the property's *Adjusted Purchase Price*. The standard deviation of *Effective Tax Rate* is 0.3%, which is large in comparison to the mean value of 1%. As noted above, the variation in effective tax rates arises because, as per California's Proposition 13, assessment values vary depending on when the property was purchased and the housing market conditions that prevailed at the time of purchase. As can be seen, there is substantial variation in *Years of Ownership* across the properties in our sample. There is also substantial cross-sectional variation in the $HPI_{List}/HPI_{Purchase}$, which measures the change in median property value in the property's zip code from the month of its purchase to the month of its listing.

Figure 2.4 plots the relationship between average *Effective Tax Rate* in 2016 ($Y-$axis) and the year of purchase ($X-$axis) separately for single family homes in California, Illinois, and New York (in case of Illinois, the $Y-$axis represents the average *Effective Tax Rate* in 2014). We use Illinois and New York as a contrast to California because, unlike in California, property tax assessment values in Illinois and New York are based on estimated market values of properties.[6] As can be seen, *Effective Tax Rate* does not vary with the year of purchase in either Illinois or New York.

By contrast, in California, the relationship between *Effective Tax Rate-2016* and the year of purchase is an upward sloping curve for properties purchased between 1996 and 2007, after which it is either flat or has a decreasing slope. This pattern is a direct consequence of California's property tax system. Most of the properties purchased prior to 2007 were under Proposition 13 assessment in 2016, especially those purchased much earlier before the peak of the housing market. Because Proposition 13 links a property's assessment value to its purchase price and caps the annual increase in assessment values to 2%, properties purchased long ago are likely to have a lower *Effective Tax Rate* in 2016, all else equal. This explains the upward slope till around 2007.

The change in the slope of the curve after 2007 has to do with the housing crisis, as a result of which properties whose assessment values exceeded their estimated market value received downward reassessment under Proposition 8. Given the sharp decline in housing prices and the slow recovery, there is not much variation in *Effective Tax Rate-2016* based on year of purchase for properties purchased after 2007.

Figure 2.5 plots the distribution of within-zip code variation in *Effective Tax Rate* using a histogram where the $X-$axis denotes the difference between *Effective Tax Rate* of an individual property and the average effective tax rate across all properties in the same zip code, and the $Y-$axis is the percentage of listings. As can be seen, there is substantial within-zip variation in effective tax rates within California, which is to be

[6] Most states in the United States use a market-value-based system of assessment similar to that used in Illinois and New York. We choose Illinois and New York as contrasting examples because we were able to obtain a large sample of properties in these states for which we had information on property tax and purchase price histories. In some other large states, we could not obtain information on property tax payments and/or purchase prices.

expected based on Proposition 13.

3. Empirical Framework and Identification Strategy

3.1 Instrumental Variables Regression

The prediction of the sunk-cost hypothesis in our setting is that sellers that have incurred higher property taxes are ceteris paribus likely to choose a higher initial listing price. However, identifying the causal effect of property tax payments on the choice of listing price is challenging due to the omitted variable problem. Specifically, because most states link property assessment values (for levying property taxes) to their estimated market values, the relationship between listing price and past property tax payment may be driven by unobserved (or omitted) factors that reflect the underlying fair market value of the property.

We overcome the omitted variable problem by estimating an instrumental variables (IV) specification, which exploits the institutional features of California's Proposition 13 to generate exogenous variation in property taxes paid by the seller. Formally, we estimate variants of the following IV regression model using the 2-stage least squares (2SLS) estimator:

$$\begin{aligned} \text{Effective Tax Rate}_{it} &= a + \boldsymbol{b X_i t} + \boldsymbol{c} \times \boldsymbol{Z_{it}} + \mu_z \times \mu_t + \eta_{it} \\ \text{Listing Price}_{it} &= \alpha + \boldsymbol{\beta X_i t} + \gamma \times \text{Effective}\widehat{\text{ Tax}}\text{ Rate}_i t + \mu_z \times \mu_t + \epsilon_{it} \end{aligned} \tag{2.2}$$

We estimate the IV regression on a sample of properties that were purchased during the period 1996–2007. We exclude properties purchased after 2007 from the IV regression because these properties are highly likely to have received downward assessments under Proposition 8, and are unlikely to have reverted to Proposition 13 assessment by the time of their listing. On the other hand, properties purchased prior to or during 2007 are likely to be under the Proposition 13 assessment system at the time of their listing.[7]

In the above equation, subscript 'i' denotes the property, 'z' denotes the zip code in which the property is located, and 't' denotes the month of listing. We control the regression for the following property characteristics: *Adjusted Purchase Price* to serve as a crude estimate of the property's value based on the purchase price paid by seller, adjusted for the change in the housing price index (HPI) in the local zip code from the month of purchase to the listing month; *Expected Loss* to control for any nominal expected loss relative to the purchase price, because loss aversion has been shown to affect listing behavior (**?**); number of bedrooms and bathrooms; square footage; lot size; and age of the house. We also control for the following demographic characteristics measured at the census-tract level: median age, median household income, and fraction of rental properties. Finally, we include *Zip*×*Listing Month* fixed effects ($\mu_z \times \mu_t$) to control for unobserved heterogeneity across local markets and listing months.

The instrument for *Effective Tax Rate* in the first-stage regression is *Years of Ownership*, which is the number of years since the property was purchased by the seller. As per California's Proposition 13, we expect *Years of Ownership* to have a negative effect on the sellers' effective tax rate (see Figure 2.4).

3.2 Validating the Instrument

A valid instrument needs to satisfy two requirements. First, it must affect the property's effective tax rate at the time of listing ("relevance"). Second, it should not be correlated with omitted characteristics that are likely to affect listing price or transaction price ("exclusion"). In this section we provide evidence to validate both these requirements.

[7]Properties purchased prior to 2007 were either unlikely to have qualified for downward reassessment under Proposition 8 during the housing crisis (because their assessment values were likely lower than their estimated market values) or would have reverted to Proposition 13 by the time of their listing even if they did qualify for downward reassessment during some of the intervening years.

Relevance of the Instrument

As we showed in Figure 2.4, the instrument is relevant because of unique features of California's property tax system. To demonstrate this in a multivariate framework, we estimate the first-stage regression separately for single-family homes in California, Illinois, and New York. For each state, the regression includes all single-family homes for which we have information on past transaction details and property taxes in 2016, regardless of whether these properties were listed for sale. The dependent variable is *Effective Tax Rate*$\times$100 in 2016, and the main independent variable of interest is *Years of Ownership*. We control for property characteristics and include zip fixed effects.

The results presented in Table 2.2 indicate that years of ownership has a negative effect on effective tax rate only in California, but not in the other states. As explained above, the negative effect arises in California because Proposition 13 links property assessment values to their purchase prices and limits annual increases in assessment values to 2%. On the other hand, Illinois and New York use a market value-based system of assessment, as per which, years of ownership doesn't affect the effective tax rate.

In Table 2.3, we estimate the first-stage regression on our regression sample, i.e., single-family homes in California that are listed for sale and that were purchased during the 1996–2007 period. The specification in column (1) includes all property characteristics as controls and also includes *Zip*$\times$*Listing Month* fixed effects. The specification in column (2) also controls for median demographic characteristics measured at the census-tract level, which is a much finer classification of the local market compared to the zipcode area. The specification in column (3) is similar to that in column (1), except that we replace *Zip*$\times$*Listing Month* fixed effects with *Census Tract*$\times$*Listing Month* fixed effects.

In all specifications, the coefficient on *Years of Ownership* is negative and highly significant. Following **?**, it is common to examine first-stage power using $F-$statistics. With one exogenous instrument, the first-stage $F-$statistic must exceed 8.96 for the 2SLS inference to be reliable at the 5% significance level (see table I in **?**). As can be seen, the $F-$statistic in all three columns comfortably exceeds the cut-off value of 8.96, which allows us to reject the hypothesis of insufficient first-stage power.

Exclusion Restriction

A valid instrument must also satisfy the exclusion restriction. In the context of our paper, the identifying assumption is that years of ownership does not directly affect seller behavior at the time of listing, conditional on all the covariates and *Zip*$\times$*Listing Month* fixed effects. We believe that this is a reasonable assumption although two concerns come to mind. First, years of ownership may affect listing behavior if it is correlated with sellers' age, motivation for selling, and preferences; e.g., seniors who are selling their homes in order to downsize may behave differently from younger sellers that may be moving for work-related reasons. Second, years of ownership may also be correlated with unobserved property characteristics, such as quality of maintenance and remodeling.

Although there is no in-sample statistical test for validating the exclusion restriction, we perform "out of sample" regressions to show that *Years of Ownership* does not have a direct effect on listing price or selling price other than through its effect on *Effective Tax Rate* under Proposition 13 assessment. We describe these tests in Table 2.4.

In Panel A, we estimate OLS regressions to test for the effect of years of ownership on listing price in California separately for houses purchased during 1996-2007 (column (1)) and 2009-2012 (column (2)). Recall that the main distinction between these samples is that Proposition 13 mainly applies to homes purchased between 1996-2007, and not to homes purchased during 2009-2012. The results in column (2) indicate that *Years of Ownership* has no affect on listing price for homes that were unlikely to be under Proposition 13 assessment at the time of their listing.

In Panel B, we estimate the regression with *Log(Selling Price)* as the dependent variable separately for recently-sold homes in CA purchased that were purchased during 2009-2012 (column (1)), homes in Illinois (column (2)), and homes in New York (column (3)). We use selling price instead of listing price because the sample sizes for the non-California regressions would be much smaller with listing price as the dependent variable.

The results indicate that *Years of Ownership* has no direct effect on home prices outside of the Proposition 13 assessment system.

4. Effect of Property Taxes on House Prices

4.1 Effect of Property Taxes on Listing Prices

In Table 2.5 we present the results of regressions examining the effect of property tax payments on the sellers' choice of initial listing price. We present the results of a simple OLS specification in column (1) for comparison with the IV regression results. Columns (2) through (4) report the results of the second-stage regressions for variants of the IV regression model (2.2); the corresponding first-stage regressions are in Table 2.3. Columns (1) through (3) include *Zip*$\times$*Listing month* fixed effects whereas column (4) includes *Census tract*$\times$*Listing month* fixed effects. Standard errors are clustered at the zip code level.

The positive and significant coefficient on *Effective Tax Rate* indicates that sellers' property tax bill has a significant positive effect on their choice of initial listing price, even after controlling for property characteristics, census tract demographic characteristics, and unobserved heterogeneity at the *Zip*$\times$*Listing month* level. This is consistent with the sunk-cost effect. The results are also highly economically significant: the coefficient estimate in column (2) indicates that a one-standard deviation increase in *Effective Tax Rate* is associated with a 2.43% increase in listing price.[8]

The coefficients on the control variables are as expected. In particular, the positive and significant coefficient on *Log(Nominal loss)* indicates that sellers choose a higher listing price when they expect to incur a nominal loss relative to their purchase price, which is consistent with loss aversion behavior documented by **?** in the Boston condominium market. The negative coefficient on *LTV at Purchase* indicates that sellers who used a

[8] The standard deviation of *Effective Tax Rate* is 0.3%. Hence, the coefficient estimate of 8.087 translates into an increase in *Log(Listing Price)* of 2.43%.

higher leverage to purchase their properties choose a lower listing price, possibly because they also have a higher debt burden at the time of listing and prefer a quick sale.[9]

Variation in Sunk-Cost Effect by Expectations of Loss

As per the explanation offered by **?**, the sunk-cost effect should be stronger among sellers that expect to realize a loss on the sale of their property relative to their purchase price. To test this hypothesis, we divide our sample of listings into two sub-samples based on whether the seller expects to incur a nominal loss or not. Recall that, as per our definition, *Nominal Loss* is positive only if the property is located in a zip code where the median house price has declined since the time the property was purchased (i.e., $HPI_{Purchase} > HPI_{List}$) so that the property's *Adjusted Purchase Price* (which is a rough estimate of its current market value) is lower than its *Purchase Price.*

In columns (1) and (2) of Table 2.6, we estimate the OLS specification separately for the subgroup of properties for which the *Nominal Loss* is positive and zero, respectively. We include all the property-level controls in these regressions but suppress these coefficients to conserve space. It is clear that although the coefficient on *Effective Tax Rate* is significant among both subgroups, it is significantly stronger among the subgroup of properties in column (1) where the seller expects to incur a nominal loss.

We are unable to estimate the IV regression on the subgroup of properties with *Nominal Loss*>0 because most of these properties were purchased closer to the peak of the housing bubble, and hence, were mostly under Proposition 8 assessment till they were sold.[10] As a result, our instrument lacks first-stage predictive power in this subgroup, because the instrument is based on the assessment policy under Proposition 13. Therefore, we estimate the IV model only on the subgroup with *Nominal Loss*$=0$, and report the second-stage results in column (3). As can be seen, the coefficient on *Effective Tax Rate* is

[9] We do not have information on the sellers' financial condition at the time of listing, but it is plausible that sellers who used higher leverage to purchase their properties also have a higher debt burden at the time of listing compared to sellers who used lower leverage to purchase their properties.

[10] Indeed, 4,979 out of the 5,178 properties in this subgroup (i.e., 84.7% of the subgroup) were purchased during the 2005–07 period.

positive and significant and has a lower magnitude than the IV coefficient corresponding to the entire sample (column (2) of Table 2.5).

Another way to test the variation of sunk-cost effect by expectations of loss is to distinguish between properties that received significant downward revisions in assessment values under Proposition 8 during the 5-year period prior to their listing, and those that did not. Recall that the dummy variable *Affected by Prop. 8* identifies properties that received a downward revision in assessment value of greater than 3% in any of the 5 years prior to listing. We report the second-stage results of the IV regression model separately for the subgroup of properties that were affected by Proposition 8 (column (4)) and those that were not (column (5)). As can be seen, the coefficient on *Effective Tax Rate* is significantly stronger among properties that qualified for downward revisions under Proposition 8, which suggests that the sunk-cost effect is stronger when sellers' expectation of loss is higher.

Variation in Sunk-Cost Effect by Price Uncertainty

Intuitively, the sunk-cost effect (and other behavioral biases) should be stronger when sellers face greater uncertainty regarding the value of their properties. Although we cannot measure price uncertainty directly, we use the following strategies to identify properties that face higher price uncertainty.

First, we hypothesize that the more expensive properties within any given zip code will face higher pricing uncertainty because they are more likely to be custom-built, less likely to be standardized, and will have fewer comparable transactions to benchmark against. Accordingly, we divide our sample of listings into two subgroups based on whether the listing price is in the top quartile or the bottom three quartiles of all listing prices within the zip code; we refer to these as the "High Value" subgroup and "Low Value" subgroup, respectively.

Second, we hypothesize that properties located in less active housing markets face higher price uncertainty because of the relative difficulty in finding comparable transaction prices in such markets. Accordingly, we classify the zip codes in California into two subgroups based on whether they are in the top three deciles ("High Activity" markets) or in the bottom seven deciles ("Low Activity" markets) in terms of local market activity, which is measured using the total number of properties sold between 2012 and 2015.[11]

We then estimate the IV regression model separately for the two subgroups under each of these classification methods. The results of our estimation are summarized in Table 2.7. To conserve space, we suppress the other coefficients and only report the following statistics for each cross-sectional split: the IV coefficient on *Effective Tax Rate*, the corresponding standard error, the $F-$statistic from the first-stage regression, number of observations, adjusted R^2, and the $p-$value of the difference in coefficients on *Effective Tax Rate* between the two subgroups.

Panel A presents the comparison of regression results based on the price category split between the low-value and high-value subgroups, whereas Panel B presents the comparison based on the market activity split between the low-activity and high-activity markets. In both these cross-sectional splits, we find that the coefficient on *Effective Tax Rate* is stronger in the subgroup that we associate with higher price uncertainty, that is, the high-value homes within a zip code and the homes located in low-activity markets.

Variation in Sunk-Cost Effect by Demographic Characteristics

One potential concern with our instrument could be that years of ownership is correlated with some seller characteristic that also affects listing behavior; e.g., age, income or wealth. Specifically, the concern could be that sellers with high years of ownership may also be older people with lower incomes, who choose a lower listing price to quickly complete the sale. Although we do not have information on seller characteristics, we are able to obtain median age and median income at the census-tract level in which the property is located. We now examine how the results vary with median age and median

[11]Recall that we collect information on number of properties sold from www.redfin.com because it is unavailable on Zillow. Also, this information is not available for all zip codes, which explains the slight reduction in sample size.

income measured at the census-tract level.

In Panel C of Table 2.7, we present the comparison of regression results for the two subgroups stratified based on whether the median age of the property's census tract is below or above that of the median age across all census tracts in California. We find that the coefficient on *Effective Tax Rate* is significant in both subgroups but is significantly stronger for properties located in census tracts with higher median age.

In Panel D of Table 2.7, we present the comparison of regression results for the two subgroups stratified based on whether the median income in the property's census tract is below or above that of the median income across all census tracts in California. We find that the coefficient on *Effective Tax Rate* is significant in both subgroups and has a similar magnitude.

4.2 Effect of Property Taxes on Selling Prices

Thus far, we have shown that sellers that have incurred higher property taxes are likely to choose a higher initial listing price, all else equal. We now examine the consequent effect on the property's selling price. If buyers are rational and can accurately determine the FMV of properties, then any effect of property taxes on listing prices should be reversed while determining the selling price. However, past literature has highlighted that buyers in the housing market use listing prices as anchors to assess property values, and do not adjust away sufficiently from this initial anchor (**?**). If so, the effect of property taxes on listing prices may also be transmitted to the selling price.

To test these competing hypotheses, we estimate the IV regression model (2.2) with *Log(Selling Price)* as the dependent variable instead of *Log(Listing Price)*. The results of our estimation are presented in Table 2.8. We include all the control variables from the baseline regression but suppress these coefficients to conserve space.

In Panel A, we estimate the regression on the same sample as in earlier tables; i.e., the sample collected from Zillow for which we have information on both listing price and the transaction outcomes. Column (1) presents the results of the OLS specification, whereas column (2) presents the second-stage results of the IV regression. The positive and significant coefficient on *Effective Tax Rate* indicates that sellers' property tax payments also affect the selling price of the properties. Notice that the coefficient on *Effective Tax Rate* is only slightly smaller than the corresponding coefficient in column (2) of Table 2.5, which suggests that most of the effect of sellers' property tax payments on listing price is also transmitted to the selling price. in terms of economic significance, the coefficient estimate in column (2) indicates that a one-standard deviation increase in *Effective Tax Rate* is associated with a 2.28% increase in selling price.

In columns (3) and (4), we present the results of the OLS regression and IV regression, respectively, with *Log(Days-on-market)* as the dependent variable, were *Days-on-market* denotes the number of days between the initial listing and sale of the property. The insignificant coefficient on *Effective Tax Rate* indicates that sellers' property tax payment has no effect on the time to sale. Note that, because our sample only includes listings that resulted in a sale, we cannot estimate the effect of sellers' property tax payments on the probability of sale for the current listings.

In Panel B, we estimate the regressions with *Log(Selling Price)* as dependent variable on a much larger sample of California property sales obtained from the ZTRAX data. The main difference between the sample in the two panels is that the sample in Panel A only include properties for which we have both listing information and transaction information. The coefficient estimates in Panel B are consistent with those from Panel A, and indicate that sellers' property tax payments affect sale prices of properties.

5. Robustness Test: RDD Framework

One potential concern with the IV regression model (2.2) is that *Years of Ownership* may somehow be correlated with unobservable characteristics that affect the property's market value, thus violating the exclusion restriction. To ameliorate this concern, we showed using "out of sample" OLS regressions that *Years of Ownership* does not have a direct effect on listing price or selling price for single-family homes outside of California's Proposition 13 assessment system (see Table 2.4).

In this section, we present a regression discontinuity design (RDD) framework as an alternative to the IV regression design to identify the effect of *Effective Tax Rate* on listing price. The RDD framework relies on the fact that California has a June 1 cutoff for updating assessment values during the year, and exploits the variation in *Effective Tax Rate* due to variation in purchase month (i.e., whether the property was purchased before or after the June 1 cutoff) during a given year, after controlling for heterogeneity at the *Zip*×*Listing Year*×*Purchase Year* level. In other words, the RDD framework does not rely on variation in *Years of Ownership*.

As an example, consider two identical homes, denoted A and B, that were both purchased in 2009 for \$500,000, had identical assessment values of \$400,000 before their purchase in 2009, and were listed and sold in 2016. Suppose Home A was purchased in April 2009 whereas Home B was purchased in September 2009. Because Home A was purchased prior to the June 1 cutoff, its assessment value is increased to \$500,000 in 2009. On the other hand, because Home B was purchased after the June 1 cutoff, its assessment value is increased to \$500,000 only in 2010, so that it continues to have a lower assessment value of \$400,000 for 2009. As a result of this variation in purchase month (combined with the annual increases under Proposition 13), Home A will have a slightly higher *Effective Tax Rate* compared to Home B in 2015 (i.e., the year before their listing). We exploit this discontinuity in our RDD framework.

Formally, we estimate the following regression for properties in our sample that were purchased during the period 2009–2012, for different outcome variables Y:[12]

$$Y_{it} = \alpha + \beta \times \textit{Adjusted Purchase Price}_{it} + \text{Zip} \times \text{Listing Year} \times \text{Purchase Year} \quad (2.3)$$

[12] We choose this period because home prices increase in California during this period were relatively small so that homes purchased after June 1 are not substantially more expensive than those purchased prior to June 1.

We divide each year into 21 weeks before and 31 weeks after June 1 (which is denoted date 0), and group the properties in our sample into weekly subgroups based on the week they were purchased in relative to June 1 of the corresponding year. For example, the "Week -10" subgroup includes all the properties that were purchased 10 weeks prior to June 1 of their purchase year. We then compute the average residual from regression (2.3) for each weekly subgroup. In Figure 2.6, we plot these average residuals against the purchase week for different outcome variables, Y; the horizontal lines in the plots denote the average residual across all weekly subgroups before and after the June 1 cutoff.

The *Effective Tax Rate* panel shows that, all else equal, properties purchased after June 1 in California have a lower average effective tax rate at the time of their listing compared to properties purchased prior to June 1. Since we have controlled for unobserved heterogeneity at the *Zip*×*Listing Year*×*Purchase Year* level in regression (2.3), this difference reflects the effect of the June 1 cutoff for updating assessment values during the year. (We also verify in Table 2.9 that the average characteristics of properties purchased prior to June 1 are not significantly different from those of properties purchased after June 1).

The other panels show that there is a similar downward discontinuity around June 1 in the residuals from the regressions with *Log(Listing Price)* and *Log(Selling Price)* as dependent variables, which is consistent with a causal effect of effective tax rate on listing price and selling price. To ensure that the properties sold before and after June 1 are not fundamentally different from each other, we also estimate equation (2.3) with *Log(Purchase Price)* as the dependent variable and plot the weekly residuals. As can be seen, there is no downward discontinuity in these plots; if at all, there is a slight upward discontinuity in the purchase price plot.

One potential concern with plots in Figure 2.6 could be that the discontinuous patterns in listing price and selling price around the June 1 cutoff are driven by some unobservable differences between sellers who purchased their homes before June 1 and sellers who purchased their homes after June 1. To address this concern, we replicate the plots using a random sample of single-family home listings from New York and Illinois that we hand-collected in early 2019. These plots are presented in Figure 2.7, and show that there is no downward discontinuity in either the listing price residuals or the selling price residuals around the June 1 cutoff in either New York or Illinois.

6. Conclusion

In this paper we examine whether the sellers' choice of listing price in the housing market is affected by sunk costs, that is, costs incurred by sellers that are unrelated to the properties' FMV. The sunk costs that we focus on are past property taxes paid by selling homeowners in California. We use California for our study because its property tax system has a unique feature, called Proposition 13, as per which two identical properties may have very different property tax assessments depending on when they were purchased. Thus, we are able to identify variation in property tax payments that is unrelated to properties' current FMV.

We show that sellers' past property tax payments have an economically significant positive effect on listing price, which is inconsistent with rational models of decision making. This effect is stronger when sellers expect to sell at a loss relative to their purchase price, for high-valued properties, and in zip codes with lower housing transaction volumes. The effect of property taxes on listing price is mostly transmitted to the selling price, which is consistent with the idea that buyers use listing prices as anchors to assess property values. Overall, our results suggest that sunk costs affect prices in the housing market.

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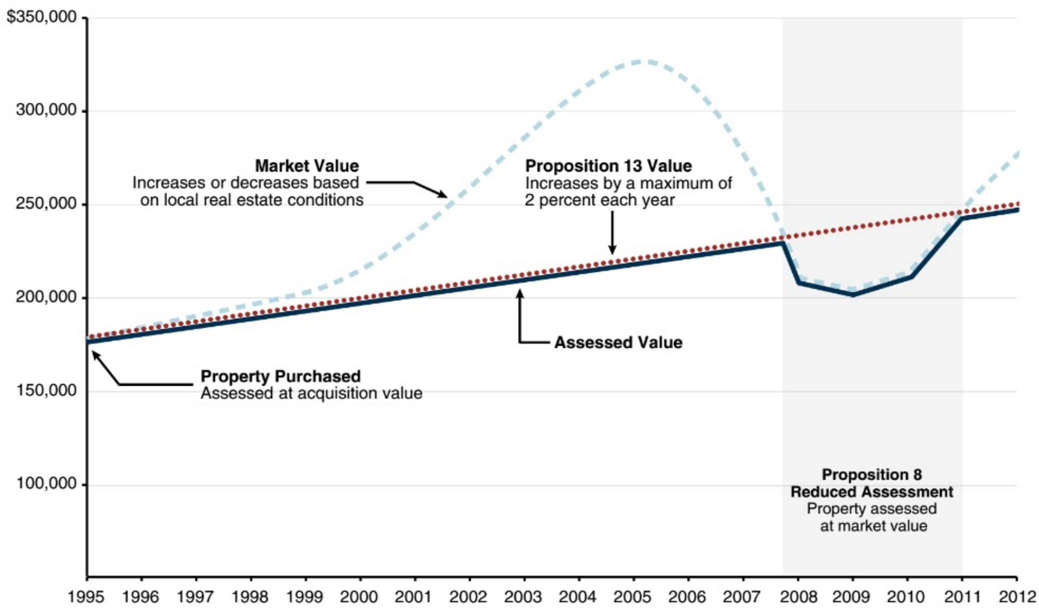
7. Figures

Figure 2.1: Illustration of California's Property Tax System

This figure illustrates the assessment of a hypothetical property purchased in 1995 under California's property tax system. The market value of the property stays above its Proposition 13 assessed value through 2007, after which it drops below the Proposition 13 assessed value due to the housing bust. After 2007, the property receives a reduced assessment under Proposition 8. For the next three years, the property receives a reduced assessment under Proposition 8, which may increase or decrease by any amount depending on changes in market value. The property reverts back to the Proposition 13 assessment after 2012, when its market value rises above what its assessment value would be under Proposition 13.



Source: Legislative Analyst Office Brief by **?**

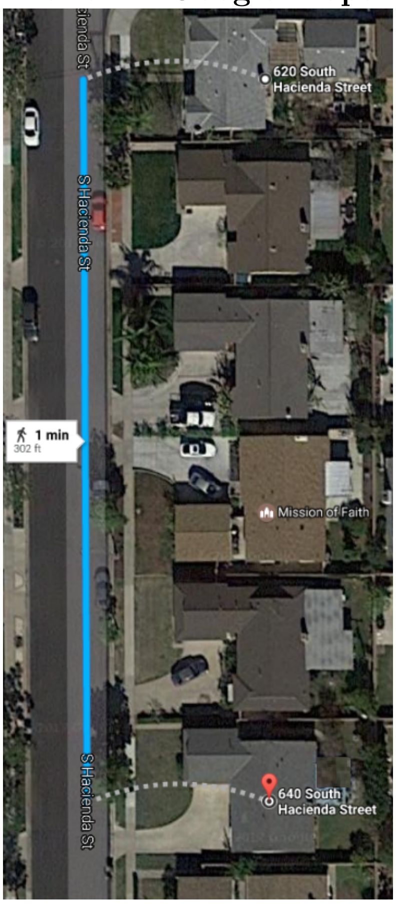
Figure 2.2: (Example) Differences in Property Tax Assessments of Similar Homes

Panel A of this figure compares the characteristics of two similar homes in Anaheim, California, both of which were sold in 2014. House A (620 S Hacienda St) was sold twice during our sample period: for $482,000 in 2005, and for $435,000 in 2010. On the other hand, House B (640 S Hacienda St) was sold for the first time in 2014. Panel B provides a snapshot from Google Maps, which shows that the two properties are located 302 ft from each other. Panel C shows the annual property tax payments of the two homes over the 2005–2016 period.

Panel A: Comparison of Property Characteristics

	House A	House B
Rooms	3 Beds, 2 Baths	3 Beds, 2 Baths
Area	1,336 sqft	1,314 sqft
Year Built	1955	1955
Previous Sales	2005 ($482,000); 2010 ($320,000)	None
Assessment Value (2014)	$334,439	$ 59,763
Selling Price (2014)	$ 450,000	$ 435,000

Panel B: Google Maps



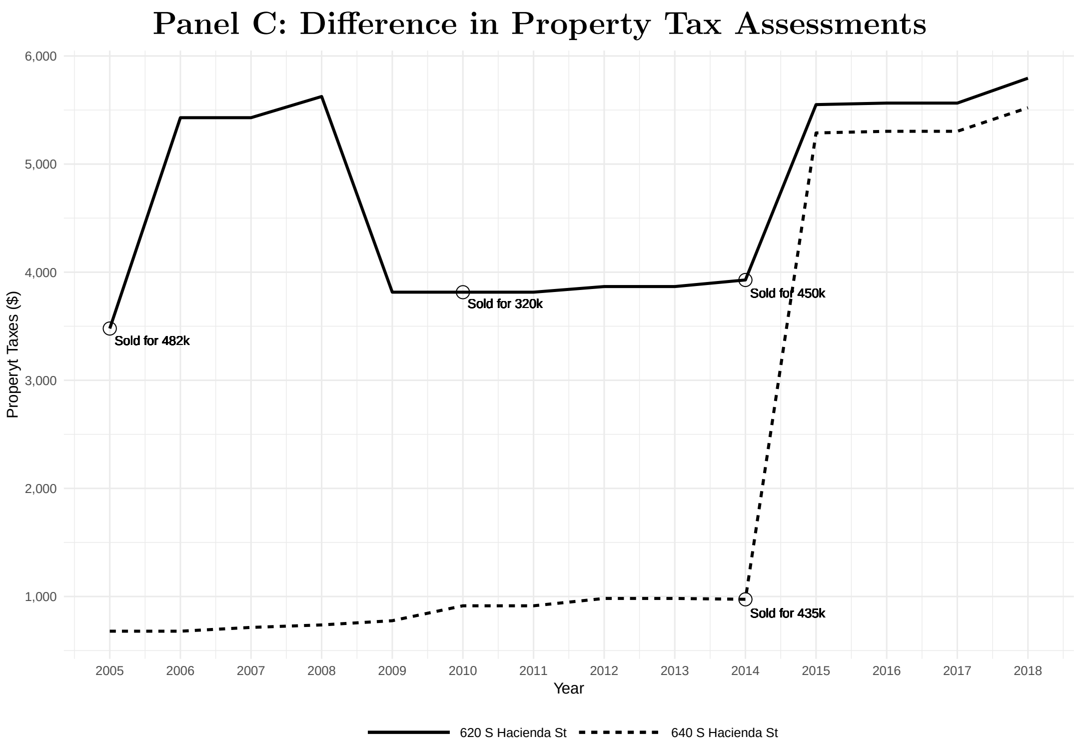

Panel C: Difference in Property Tax Assessments
6,000
5,000
4,000
3,000
2,000
1,000
Properyt Taxes ($)
Sold for 482k
Sold for 320k
Sold for 450k
Sold for 435k
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
2018
Year
620 S Hacienda St
640 S Hacienda St

Figure 2.3: All-Transactions House Price Index for California

This figure plots the all-transaction house price index for California.

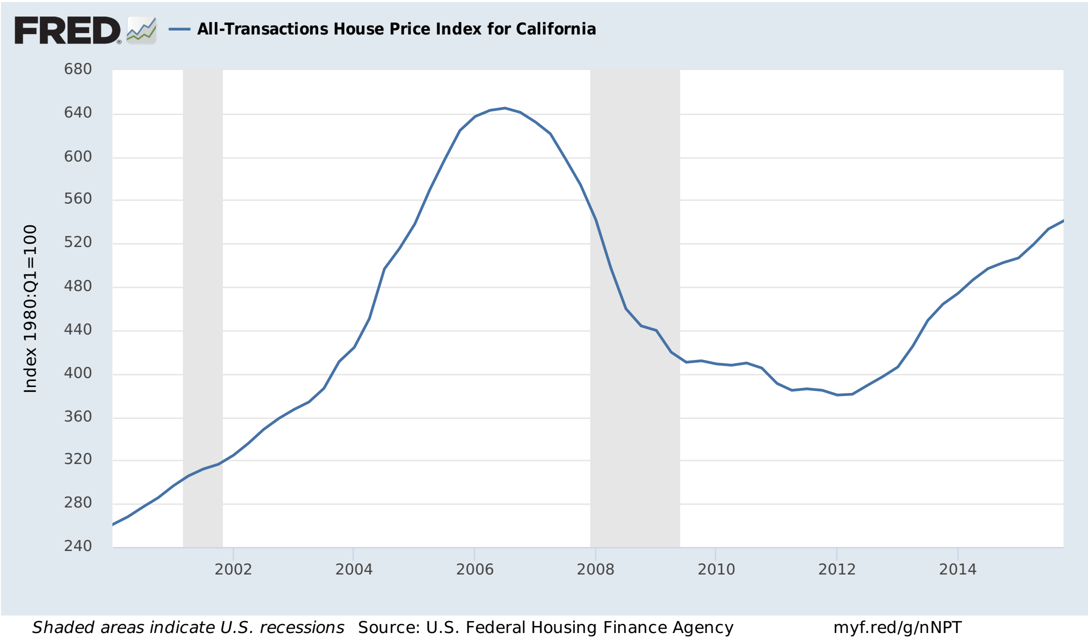


Figure 2.4: Effective Tax Rate and Years of Ownership

This figure shows the relationship between the *Effective Tax Rate* in 2016 (2014 for Illinois) (on the $Y-$axis) and the year of purchase (on the $X-$axis) for single family homes in California, Illinois, and New York. See Appendix A for variable definitions.

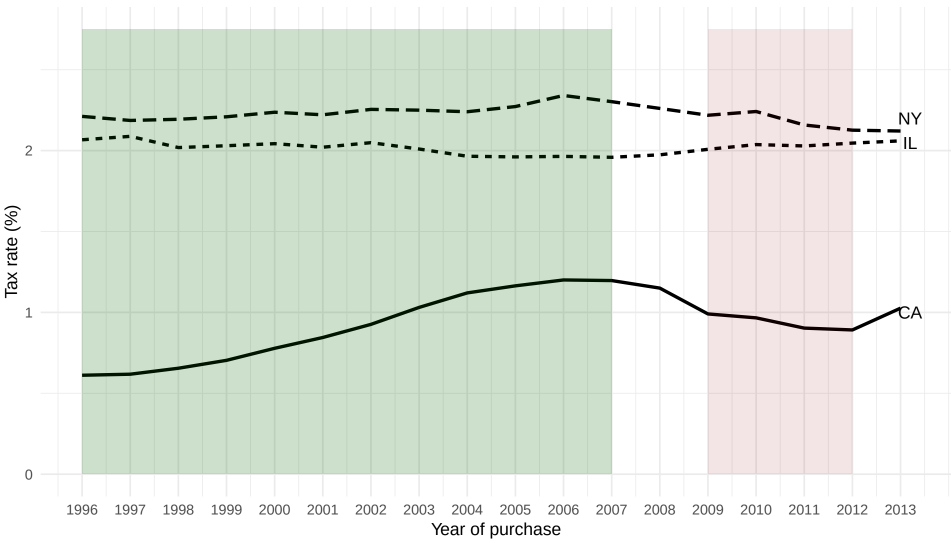


Figure 2.5: Within Zip Code Variation Effective Tax Rate

This figure plots a histogram of within zip code variation of *Effective Tax Rate*. The X-axis denotes the difference between the *Effective Tax Rate* of each house and the mean *Effective Tax Rate* in its zip code. See Appendix A for variable definitions.

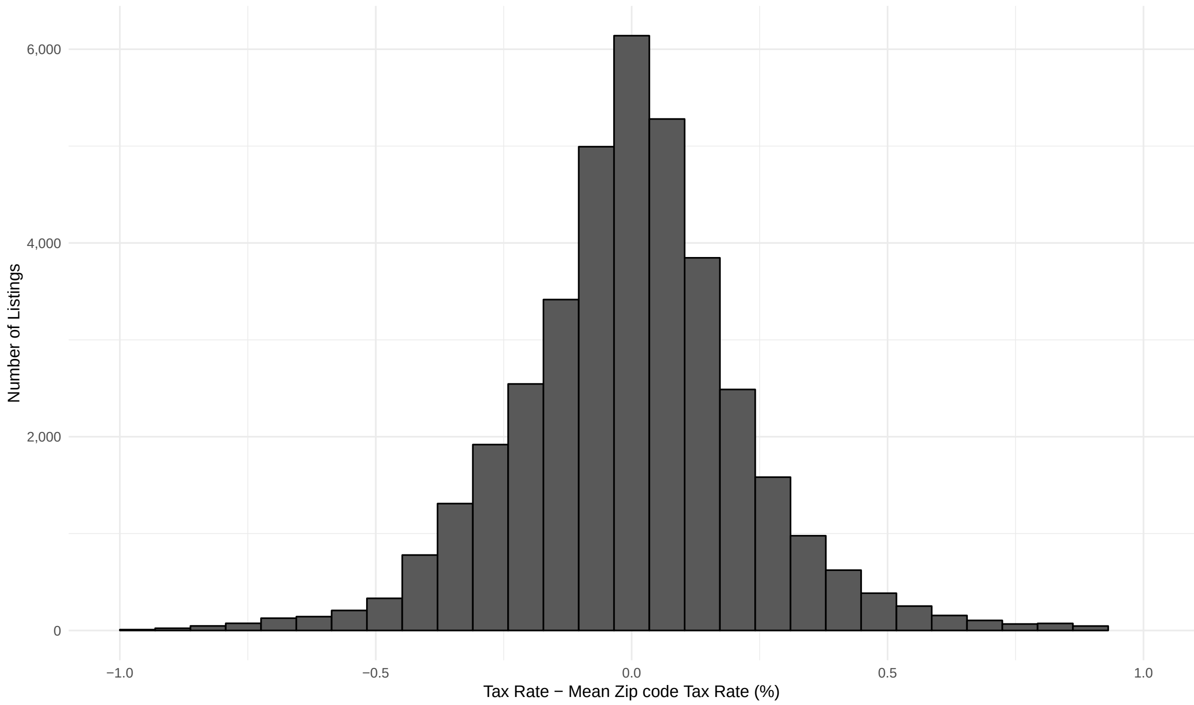


Figure 2.6: Impact of June 01 Cutoff: California

Each panel of this figure plots the weekly mean of residual of the regression Y on $Zip\ code \times Purchased\ year \times Listing\ year$ fixed effects, where Y is the plot title. In addition, when indicated by a $*$, regression controls for $log(Adjusted\ purchase\ price)$. x-axis represents the number of weeks from week of June 01 in each purchase year. All the homes were purchased between 2009 and 2012. See Appendix A for variable definitions.

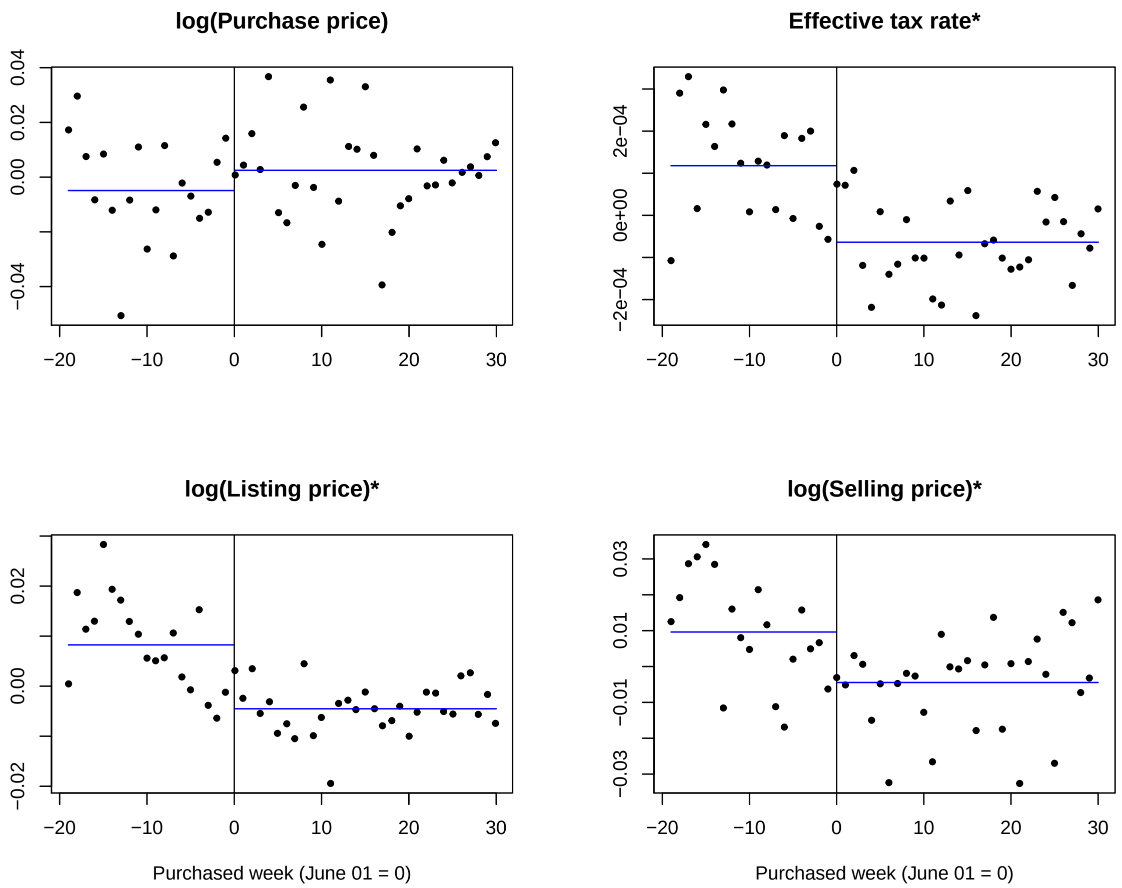
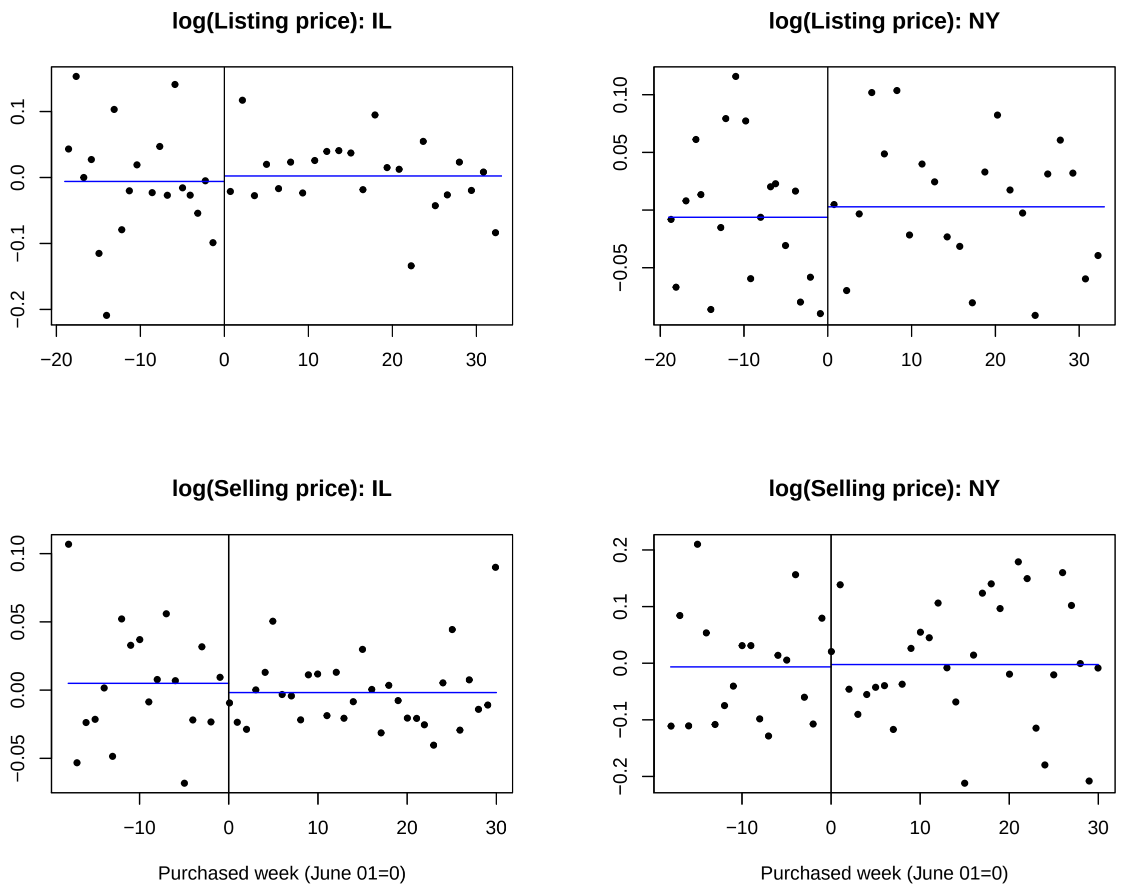


Figure 2.7: Impact of June 01 Cutoff: Other States

Each panel of this figure plots the weekly mean of residual of the regression Y on $log(Adjusted\ purchase\ price)$ and $Zip\ code \times Purchased\ year \times Listing\ year$ fixed effects, where Y is the plot title. x-axis represents the number of weeks from week of June 01 in each purchase year. *log(Listing price)* plots are based on small samples of listings in early 2019 merged with ZTRAX data and *log(Selling price)* plots are based on ZTRAX data. All the homes were purchased between 2009 and 2012. See Appendix A for variable definitions.



8. Tables

Table 2.1: Descriptive Statistics

This table shows descriptive statistics for the sample used in the main analysis. The data set consists of a random sample of homes that were sold in 2015-2017 period and purchased during the period 1996–2007.

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Purchase price ($)	510,231	466,700	253,500	403,500	627,000	22,487
Adj. purchase price ($)	675,184	636,421	352,604	528,036	788,394	22,487
Listing price ($)	714,460	665,121	379,000	559,000	819,000	22,487
Property taxes paid ($)	6,228	5,429	3,236	5,012	7,516	22,487
Effective tax rate	0.010	0.003	0.008	0.010	0.011	22,487
$\text{HPI}_{Purchase}$	433,885	278,684	242,300	380,200	559,400	22,487
$\text{HPI}_{List}/\text{HPI}_{Purchase}$	1.454	0.630	0.991	1.251	1.757	22,487
Years of ownership	12.61	2.85	11.00	12.00	14.00	22,487
Number of bedrooms	3.42	0.88	3.00	3.00	4.00	22,484
Number of bathrooms	2.51	1.04	2.00	2.00	3.00	22,394
House area (sq. ft)	2,031	895	1,413	1,829	2,442	22,460
Lot area (sq. ft)	17,300	135,446	5,600	7,196	10,018	21,291
Age of the house (years)	38.16	22.96	18.00	33.00	55.00	22,487
$\text{Loan-to-value}_{Purchase}$	0.619	0.355	0.464	0.781	0.800	22,458
GreatSchools rating	6.95	2.03	5.33	7.33	8.67	22,463
Distance to schools (miles)	1.45	1.30	0.80	1.17	1.70	22,469
Census tract population	5,882	2,977	4,081	5,366	6,801	22,479
Census tract median age	40.38	7.98	34.70	39.50	45.10	22,477
Census tract median income	82,688	32,703	58,541	77,292	101,590	22,476
Census tract fraction of renters	0.320	0.171	0.190	0.291	0.428	22,477

Table 2.2: Effect of Years of Ownership on Effective Tax Rate

This table reports the results of regressions that examine the relation between sellers' years of ownership and the effective tax rate ($\times 100$) in the year prior to listing for sale. Columns (1), (2), and (3) report the results for states of California, Illinois, and New York respectively. The samples consist of all homes in each state for which the previous purchase transaction details and most recent property tax information are available in ZTRAX. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Tax Rate $\times 100$		
	CA	IL	NY
	(1)	(2)	(3)
Years of ownership	-0.053***	0.004	-0.002
	(0.001)	(0.002)	(0.002)
log(Adj. purchase price)	-0.621***	-0.183***	-0.665***
	(0.017)	(0.030)	(0.051)
log(Nominal loss)	-0.0003	0.001	0.002**
	(0.001)	(0.001)	(0.001)
Number of bedrooms	0.025***	0.006	
	(0.002)	(0.010)	
Number of bathrooms	0.069***	0.260***	
	(0.003)	(0.015)	
log(Age of the house)	-0.203***	-0.079***	-0.250***
	(0.010)	(0.015)	(0.024)
Zip code	✓	✓	✓
Observations	1,722,228	532,796	99,191
Adjusted R^2	0.36	0.44	0.23

Table 2.3: Effect of Years of Ownership on Effective Tax Rate in California

This table reports the results of the first-stage regression of the IV regression model that examines the relation between sellers' years of ownership and the effective tax rate (×100) in the year prior to listing for the sample used in the main analysis. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Tax Rate ×100		
	(1)	(2)	(3)
Years of ownership	-0.054***	-0.054***	-0.052***
	(0.001)	(0.001)	(0.003)
log(Adj. purchase price)	-0.286***	-0.296***	-0.416***
	(0.024)	(0.024)	(0.060)
log(Nominal loss)	-0.002*	-0.002*	-0.002
	(0.001)	(0.001)	(0.002)
Number of bedrooms	0.029***	0.030***	0.025
	(0.006)	(0.006)	(0.016)
Number of bathrooms	0.01	0.01	0.028*
	(0.008)	(0.008)	(0.014)
House area	0.089***	0.089***	0.127
	(0.023)	(0.023)	(0.080)
GreatSchools rating	0.016***	0.012***	0.022
	(0.004)	(0.004)	(0.019)
Distance to schools	0.008	0.008	0.006
	(0.006)	(0.006)	(0.019)
Distance to amenities	-0.0004**	-0.0002	-0.0003
	(0.000)	(0.000)	(0.001)
log(Age of the house)	-0.096***	-0.095***	-0.111***
	(0.011)	(0.011)	(0.029)
log(Lot area)	0.013*	0.013*	0.023
	(0.007)	(0.007)	(0.022)
Loan-to-value$_{Purchase}$	0.027***	0.026***	0.025
	(0.009)	(0.009)	(0.024)
Census tract median age		0.0004	
		(0.001)	
log(Census tract median income)		0.099***	
		(0.022)	
Census tract fraction of renters		0.035	
		(0.039)	
Zip code × Listing month	✓	✓	
Census tract × Listing month			✓
Observations	20,367	20,360	20,367
Adjusted R^2	0.60	0.60	0.62

Table 2.4: Direct Effect of Years of Ownership on House Price

This table reports the results of OLS regressions that examine the relation between years of ownership and house price. In Panel A, we report the results of regressions with *log(Listing price)* as the dependent variable and *Years of ownership* as the main independent variable of interest using California sample. The sample in column (1) includes homes purchased during 1996–2007, whereas the sample in column (2) includes homes purchased during 2009–2012.

In Panel B, we report the results of OLS regressions with *Log(Selling Price)* as the dependent variable using a sample of properties constructed from ZTRAX. The regression samples vary in the three columns as follows: California homes purchased during 2009–2012 in column (1), Illinois homes purchase during 1996–2007 in column (2), and New York homes purchased during 1996–2007 in column (3).

We suppress the coefficients on control variables to conserve space. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Impact on *log(Listing Price)* (California)

	1996-2007 (1)	2009-2012 (2)
Years of ownership	-0.004***	0.003
	(0.001)	(0.002)
log(Adj. purchase price)	0.602***	0.625***
	(0.019)	(0.026)
log(Nominal loss)	0.002***	0.013
	(0.000)	(0.014)
Controls	✓	✓
Zip code × Listing month	✓	✓
Observations	20,367	13,190
Adjusted R^2	0.971	0.977

Panle B: Impact on *log(Selling Price)*

	CA (2009-2012) (1)	IL (1996-2007) (2)	NY (1996-2007) (3)
Years of ownership	0.003	0.004	-0.003
	(0.003)	(0.004)	(0.008)
log(Adj. purchase price)	0.441***	0.428***	0.045***
	(0.027)	(0.046)	(0.012)
log(Nominal loss)	-0.005	0.001	0.0004
	(0.012)	(0.003)	(0.008)
Controls	✓	✓	✓
Zip code × Listing month	✓	✓	✓
Observations	24,323	8,902	21,907
Adjusted R^2	0.827	0.621	0.299

Table 2.5: Effect of Property Taxes on Listing Price

This table reports the results of regressions that examine the effect of *Effective Tax Rate* on *log(Listing Price)*. We report the results of the OLS regression in column (1), and the results of various IV regressions (second-stage only) in columns (2) through (4). See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: log(Listing Price)			
	OLS	IV		
	(1)	(2)	(3)	(4)
Effective tax rate	11.613***	8.087***	8.055***	10.132***
	(0.823)	(1.214)	(1.211)	(3.432)
log(Adj. purchase price)	0.647***	0.632***	0.624***	0.585***
	(0.019)	(0.020)	(0.020)	(0.045)
log(Nominal loss)	0.001***	0.002***	0.002***	0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Number of bedrooms	-0.001	-0.0002	0.001	0.004
	(0.003)	(0.003)	(0.003)	(0.008)
Number of bathrooms	0.003	0.003	0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.008)
House area	0.0001***	0.0001***	0.0001***	0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)
log(Age of the house)	0.014***	0.010*	0.009	-0.002
	(0.005)	(0.005)	(0.005)	(0.014)
log(Lot area)	0.028***	0.029***	0.029***	0.033**
	(0.005)	(0.005)	(0.006)	(0.013)
Loan-to-value$_{Purchase}$	-0.014***	-0.014***	-0.012**	-0.012
	(0.005)	(0.005)	(0.005)	(0.012)
GreatSchools rating	0.011***	0.011***	0.009***	0.009
	(0.003)	(0.003)	(0.003)	(0.010)
Distance to schools	-0.001	-0.001	-0.002	0.006
	(0.003)	(0.003)	(0.003)	(0.011)
Distance to amenities	-0.0004***	-0.0004***	-0.0002**	-0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
Census tract median age			0.001***	
			(0.001)	
log(Census tract median income)			0.036***	
			(0.012)	
Census tract fraction of renters			-0.003	
			(0.019)	
Cond. F. Stat		160.01	126.11	23.16
Zip code × Listing month	✓	✓	✓	
Census tract × Listing month				✓
Observations	20,367	20,367	20,360	20,363
Adjusted R^2	0.972	0.972	0.972	0.975

Table 2.6: Variation by Expectations of Loss

This table reports the results of regressions aimed at understanding how the effect of *Effective Tax Rate* on *log(Listing Price)* varies with the expectation of loss. Columns (1) and (2) report the results of OLS regressions estimated separately for properties with *Nominal Loss* > 0 and *Nominal Loss* $= 0$, respectively. Column (3) reports the results of the IV regression (second-stage only) estimated on properties with *Nominal Loss* $= 0$. Columns (4) and (5) report the results of IV regressions (second-stage only) estimated separately for properties that received a downward revision in assessment values under Proposition 8 and properties that were not affected by Proposition 8, respectively. We employ the full set of controls and include *Zip*×*Listing month* fixed effects in all specifications, but suppress the coefficients on control variables to conserve space. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: log(Listing Price)				
	OLS		IV		
	Nominal Loss > 0	Nominal Loss $= 0$	Nominal Loss $= 0$	Affected by Prop. 8	Not affected by Prop. 8
	(1)	(2)	(3)	(4)	(5)
Effective tax rate	14.623***	11.131***	7.024***	20.369***	11.438***
	(2.329)	(1.041)	(1.327)	(5.224)	(1.702)
log(Adj. purchase price)	0.764***	0.613***	0.598***	0.805***	0.587***
	(0.044)	(0.023)	(0.023)	(0.034)	(0.029)
log(Nominal loss)	0.014**			0.004***	0.005**
	(0.006)			(0.001)	(0.002)
Controls	✓	✓	✓	✓	✓
Zip code × Listing month	✓	✓	✓	✓	✓
Cond. F. Stat			147.96	6.21	131.68
Observations	5,178	15,189	15,189	9,171	10,871
Adjusted R^2	0.969	0.972	0.972	0.973	0.972

Table 2.7: Other Cross-Sectional Splits

This table reports the results of regressions aimed at understanding how the effect of *Effective Tax Rate* on *Log(Listing Price)* varies in the cross section. In panel A, we divide our sample into two subgroups based on whether the listing price is in the top quartile (*High Value* subgroup) or the bottom three quartiles (*Low Value* subgroup) of all listing prices within the zip code. In panel B, we classify zip codes into deciles based on the number of properties sold between 2012 and 2015, and group them into a "Low Activity" group (bottom 7 deciles) and a "High Activity" group (top 3 deciles). In panel C, we divide our sample into two subgroups based on whether the median age of the census tract is less than the median age in the full sample (*Below median age* subgroup) or greater than the median (*Above median age* subgroup). In panel D, we divide our sample into two subgroups based on whether the median household income of the census tract is less than the median household income in the full sample (*Below median income* subgroup) or greater than the median (*Above median income* subgroup). For each cross-sectional split, we estimate the IV regression separately for each of the two subgroups. We report the IV coefficient in column (1), standard error in column (2), conditional F-statistic in column (3), number of observations in column (4), and adjusted R^2 in column (5). Column (6) reports the p-value for the null hypothesis that the difference between two IV coefficients is equal to 0.

	IV Coefficient (1)	Std. Error (2)	Cond. F. Stat (3)	Observations (4)	Adjusted R^2 (5)	P-Val. (Diff) (6)
Panel A: By Price Category						
Low Value	3.597*	1.871	54.9	10,002	0.975	0.000
High Value	14.066***	2.332	40.44	10,117	0.975	
Panel B: By Market Activity						
Low Activity	8.764***	2.122	64.71	8,963	0.968	0.420
High Activity	7.421***	1.454	98.36	10,328	0.969	
Panel C: By Median Age						
Below Median Age	5.721**	2.327	32.42	7,811	0.966	0.053
Above Median Age	10.349***	1.801	103.1	12,425	0.97	
Panel D: By Median Income						
Below Median Income	8.983**	3.567	18.18	7,098	0.956	0.615
Above Median Income	8.136***	1.514	122.09	13,239	0.967	

Table 2.8: Effect of Property Taxes on Selling Price and Days-on-Market

This table reports the results of regressions that examine the effect of *Effective Tax Rate* on transaction outcomes. We estimate the regressions on the Zillow sample in Panel A, where the dependent variable is *Log(Selling price)* in columns (1) and (2) and *Log(Days on Market)* in columns (3) and (4). Columns (1) and (3) report the results of OLS regressions, whereas columns (2) and (4) report the results of IV regressions (second-stage only).

In Panel B, we estimate the regressions with *Log(Selling price)* as dependent variable on a larger sample of property transactions extracted from ZTRAX. Columns (1) reports the results of the OLS regression, whereas column (2) reports the result of the IV regression (second-stage only).

We employ the full set of controls and include *Zip*×*Listing month* fixed effects in all specifications, but suppress the coefficients on control variables to conserve space. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Zillow Data

	log(Selling Price)		log(Days-on-market)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Effective tax rate	13.603***	7.587**	-3.271	1.082
	(1.665)	(2.975)	(3.069)	(5.704)
log(Adj. purchase price)	0.660***	0.634***	-0.004	0.015
	(0.033)	(0.034)	(0.040)	(0.046)
log(Nominal loss)	0.0001	0.001	0.004**	0.003
	(0.001)	(0.001)	(0.002)	(0.002)
Controls	✓	✓	✓	✓
Zip code × Listing month	✓	✓	✓	✓
Cond. F. Stat		160.01		160.01
Observations	20,367	20,367	20,367	20,367
Adjusted R^2	0.8	0.8	0.341	0.341

Panel B: ZTRAX Data

	log(Selling Price)	
	OLS	IV
	(1)	(2)
Effective tax rate	12.569***	5.364***
	(0.729)	(1.054)
log(Adj. purchase price)	0.836***	0.819***
	(0.010)	(0.011)
log(Nominal loss)	0.001***	0.003***
	(0.000)	(0.000)
Controls	✓	✓
Zip code × Listing month	✓	✓
Cond. F. Stat		123.71
Observations	88,961	88,961
Adjusted R^2	0.962	0.962

Table 2.9: Descriptive Statistics: RDD Sample

This table shows sample means and standard deviations (in parenthesis) for the sample used in the regression discontinuity design analysis. The data set consists of a random sample of homes that were sold in 2015-2017 period and purchased between 2009-2012.

	Purchased before June 01	Purchased after June 01	t-test p-value
Observations	6,768	9,935	
Purchase price ($)	443,090.40	461,367.60	0.022
	(505,761.10)	(508,995.60)	
Number of bedrooms	3.43	3.44	0.477
	(0.88)	(0.86)	
Number of bathrooms	2.47	2.48	0.346
	(0.94)	(0.93)	
House area (sq. ft)	1,949.94	2,009.61	0.257
	(736.34)	(896.07)	
Age of the house	38.39	38.53	0.700
	(24.67)	(24.22)	
Loan-to-valuePurchase	0.69	0.69	0.898
	(0.49)	(0.44)	
Month of sale	6.90	6.94	
	(3.38)	(3.41)	
Year of purchase	2010.6	2010.5	
	(1.1)	(1.1)	
Year of listing	2015.8	2015.8	
	(0.6)	(0.5)	

Table 2.10: Impact of June 01 Cutoff: Regression Evidence

This table reports the discontinuity estimates for the effect of June 01 cutoff for assessment value reset for newly purchased homes. These regressions use homes purchased in 2009-2012 period when the house prices were stable and listed for sale in 2015-2017. Dependent variable of each regression is given in the column headers and *Purchased after June 1* is a dummy variable indicating whether the home was purchased after June 01st in the purchase year. See Appendix A for variable definitions. Standard errors are clustered at the zip code level and reported in parentheses below coefficient estimates. We use *,**, and *** to denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Tax Rate ×100	log(Listing price)	log(Selling price)	log(Purchase price)	Sale Month No
	(1)	(2)	(3)	(4)	(5)
Purchased after June 1	-0.021***	-0.012***	-0.009*	0.020***	0.017
	(0.005)	(0.003)	(0.006)	(0.005)	(0.082)
log(Adj. purchase price)	-0.208***	0.618***	0.611***		
	(0.020)	(0.017)	(0.022)		
log($HPI_{Purchase}$)				0.631***	
				(0.081)	
Controls	✓	✓	✓	✓	✓
Zip code × Purchase Year × List Year	✓	✓	✓	✓	✓
Observations	14,486	14,486	14,486	14,486	14,486
Adjusted R^2	0.506	0.976	0.846	0.924	0.12

9. Appendix A: Variable Definitions

This appendix provides definitions for the variables used in the paper.

Variable	**Definition**
Property Taxes Paid	Total property taxes paid by the seller in the year prior to listing.
HPI	Value of the median single-family home in the zip code area during that month.
$HPI_{Purchase}$	HPI in the property's zip code in the month in which the property is purchased.
HPI_{List}	HPI in the property's zip code in the month in which the property is listed for sale.
Adjusted Purchase Price	The property's purchase price adjusted for the change in HPI in the local zip code since the property's purchase. Defined as Purchase Price $\times \frac{HPI_{List}}{HPI_{Purchase}}$.
Effective Tax Rate	Ratio of Property Taxes Paid to Adjusted Purchase Price.
Years of Ownership	Time from purchase of property to listing it for sale.
Listing Price	Initial listing price by the seller.
Selling Price	Price at which the property is sold.
Nominal Loss	$Maximum(\text{Purchase Price} - \text{Adjusted Purchase Price}, 0) + 1$

Chapter 3

The Enduring Effects of Interest Rates at Mortgage Origination

1. Introduction

The interest rate at mortgage origination can have long-term effects on a household. In the short-term, higher interest payments reduce household cash flow and can crowd out other consumption and investment. In the longer term, higher interest payments result in increased exposure to liquidity shocks and deflation if refinancing is difficult. When high interest rates are combined with high housing volume, many households will be particularly exposed to a negative macroeconomic shock. Paradoxically, central bank rate increases are sometimes motivated by "overheating" credit markets (e.g. Stein (2013)), potentially begetting fragility. This paper studies the persistent effects of interest rates at origination on household behavior.

There is a substantial literature on the effects of interest rates on household behavior, and these effects have been central to housing policy since the crisis. This work, however, focuses on contemporaneous interest rates—contemporaneous rates are particularly important for households who refinance their mortgage or who have an adjustable-rate mortgage (ARM). But most mortagors do not have ARMs, and there were substantial frictions to refinancing after the housing bust (e.g. Remy, Lucas and Moore (2011), Agarwal, Amromin, Chomsisengphet, Landvoigt, Piskorski, Seru, and Yau (2015), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017), DeFusco and Mondragon (2018), and Beraja, Fuster, Hurst, and Vavra (2019)). In the presence of these refinancing frictions, historical interest rates can have a long hangover effect.

Identifying the effect of modest differences in interest rates on borrower behavior is empirically challenging. The primary problem is that interest rates are a function of credit quality, so higher rates conflate worse credit and higher payments. To minimize this problem, we instrument for contracted rates using within-year changes to the 30-year fixed rate mortgage average from Freddie Mac's "primary mortgage market survey" (PMMS). Figure 1 plots the rate from 2004 to 2008. This rate fluctuates month-to-month, and is strongly predictive of the contracted rate at origination. This rate had a range of 84 basis points in 2005; 69 bps in 2006; 60bps in 2007; and 153 bps in 2008 (115 if we exclude the last month of 2008). The standard deviation of this rate is 36 bps in our sample, and 24 bps within-year. Further, the 30-year fixed rate is closely linked to the 10-year Treasury rate in our sample—from 2004-2007, the 30-year fixed rate moves one-for-one with the 10-year Treasury with a spread of 2 p.p. and an r-squared of over 80%. The primary driver of this rate, therefore, is likely investor's required return for long-term, safe debt, rather than borrower characteristics.

The intuition for our research design is as follows. Consider two identical homebuyers who purchase a home in the same year and in the same area. Further, each homebuyer will eventually encounter similar levels of negative equity in the same year after house prices crash. There is some randomness, however, in exactly when the buyer finds a suitable house and closes the deal. The first borrower may close when national rates are slightly higher than when the second borrower closes; going forward, the first borrower will have slightly higher payments. To give a sense of magnitudes, consider the median mortgage in our sample of $200,000. With a 6.5% rate in August 2007, the monthly payment is $1,264; with a 6% rate in December 2007, the monthly payment is $1,199, for an annual difference of $780. This modest difference in payments may impact borrower behavior.

The first stage of our analysis shows that contracted rates are indeed tied to the prevailing national rate. The relationship between the contracted rate and the national rate is mechanical (the national rate is the average of contracted rates), but also statistically strong amid other reasons for rate variation, like credit risk. A 50 basis point change in the national rate at origination leads to a predicted 36 basis point difference in the contracted rate, which amounts to about 3.7-4.7% of the total mortgage payment.

Our second stage uses variation in the national rate to study the effects of contracted rates on borrower behavior. A 50 basis point change in the national interest rate at origination leads to an increased default probability of 68-88 basis points for Freddie Mac loans within 12 months of reaching negative equity. This effect size is consistent across a range of samples—from homes with 10% negative equity to homes with 50% negative equity. Relative the unconditional default probability, the effect is large—the effect is 20% of the average default probability for homes with 10% and 20% negative equity, and between 11-12.5% of the average default for homes with 30-50% negative equity. Given the small size of the differences in payment size, and the stability of the coefficient across a wide range of negative equity, our results are consistent with severe financial constraints.

While it is difficult to distinguish liquidity-driven defaults from strategic defaults , we find little direct evidence that borrowers strategically default due to small differences in rates. Our estimates are similar for a wide range of negative-equity borrowers—borrowers with 10% negative equity respond roughly the same to a small increase in rates as borrowers with 30% or 50% negative equity. But the strategic motives to default should vary substantially when moving from 10% negative equity to 30% or 50% negative equity. This leads us to conclude that our instrument is picking up liquidity-driven defaults rather than strategic defaults. Further, our estimates are similar for homes in states with judicial foreclosures and homes in states without judicial foreclosures; likewise our results are largely similar for recourse and non-recourse states. But again, the strategic motives to default should vary substantially across these legal regimes, leading us to conclude that our results are primarily driven by liquidity defaults.

We next apply our instrument to households in the Consumer Expenditure (CE) survey that have recently purchased a home from 2001 to 2007. A 50 basis point change in the national rate at origination leads to a predicted change in monthly payment of 3-3.7%, similar to the Freddie Mac data. In the second stage, this reduction in payment increases consumption of nondurables by 2% and services by 20%, while durables decrease by a statistically insignificant 10%. At the median of the distribution, 3% of the mortgage payment is $330; 2% of nondurable spending is $360; 10% of durables is $351; 20% of services is $400. Thus, the reduction in mortgage payments is roughly equal to the increase in the sum of nondurable, durable, and service consumption. Further, we find no statistically distinguishable effect on total expenditures, suggesting that the mortgage payment largely crowds out consumption. The magnitude and timing of the differences in payment size across households correspond to the tax rebates of $300-$600 studied by Johnson, Parker, and Souleles (2006), who find a similar response for nondurable consumption.

The main threat to our identification strategy is the possibility that some homebuyers time their purchases when rates are low. If more sophisticated buyers are better at buying when rates are low, for example, then our identification strategy will conflate lower payment size with sophistication. We think this is unlikely for a variety of reasons. First, DeFusco and Paciorek (2017), using discontinuous changes in rates at the conforming loan limit that are much larger than what we study, find that borrowers respond little at origination to the change in rates. In particular, borrowers hardly reduce their loan amount even when doing so would substantially lower their rate. Second, we use within-year variation in interest rates, which should be difficult to forecast. Likewise, there is uncertainty regarding the time required to close a contract, which makes any attempt to time the market imprecise. Third, we follow Pei, Pischke, and Schwandt (2017) and perform balancing tests. We find that the instrument is not associated with poor credit quality beyond the higher payment size, nor is it correlated with total household expenditure in the CE survey. In fact, the instrument is correlated with lower loan balances and LTV ratios in the Freddie Mac data; this is consistent with higher interest rates deterring relatively low-quality borrowers, thus biasing our results toward zero. Finally, the instrument predicts prepayments. This is consistent with standard refinancing motives (the refinancing threshold is higher if the initial rate is higher), but is difficult to reconcile with worse credit quality or sophistication.

In looking at the persistent effects of interest rates, our paper most closely resembles Berger, Milbradt, Tourre, and Vavra (2018) and Eichenbaum, Rebelo, and Wong (2018). These papers study the effects of contemporaneous interest rate cuts, but note that their effect depends upon the level of past interest rates—if past rates were high, then a cut leads to many borrowers refinancing their mortgage and thus consuming more. We differ by focusing on the effects of past interest rates on behavior before the loan is refinanced. Our results have complementary implications for monetary policy: if households cannot refinance during a recession, then an increase in rates during the boom puts them in a dire position during the bust; if households can refinance, however, then an increase in rates during the boom gives the central bank more power to fight the bust through rate cuts.

We also contribute to the broader literature on contemporaneous interest rates and household behavior. Higher interest rates can lead to default during a housing bust, and this has been a focus of policy and research. There is vast evidence of liquidity constraints among households, so some defaults may be prevented through systematic payment reductions. Eberly and Krishnamurthy (2014) provide theoretical support for such policies, while Fuster and Willen (2017) and Tracy and Wright (2012) find empirical support by examining defaults around rate resets of adjustable rate mortgages (ARMs). Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) further show that large payment reductions of 50% from ARM resets cause durable consumption and deleveraging to increase. Policy makers, through the Home Affordable Refinancing Plan (HARP), attempted to reduce defaults with a payment reduction strategy by subsidizing lenders who refinance underwater borrowers that are still current on their payments. A primary challenge with HARP is low lender participation. The average 30-year fixed mortgage rate from 2004 to 2007 ranged from 5.5-6.8%; from 2009 to 2012 the rate ranged from 3.3-5.6%. Thus, refinancing a mortgage involved a substantial transfer from the lender to the borrower (Remy, Lucas and Moore (2011)). HARP's success was therefore tightly tied to the level of competition in the lending market (Amromin and Kearns

(2014), and Agarwal, Amromin, Chomsisengphet, Piskorski, Seru and Yao (2015)). In short, financial intermediaries play an important role in mortgage modifications of all kinds, and these intermediaries generally do not want to implement the modifications subsidized by policy makers during the crisis. It is worth asking, then, whether the smaller rate reductions we study (which may be more amenable to lenders) could still prevent defaults and insulate consumption.

Relatedly, we contribute an empirical design that can hone in on small changes in interest rates. If small rate reductions can also stimulate the economy, then policy makers have an extra arrow in their quiver. Further, our design can be applied across the business cycle, and can potentially be used to study the effects of interest rates on any household behavior.

Despite differences in our research design from the literature on ARM resets and HARP modifications, we find comparable magnitudes. Fuster and Willen (2017) find that cutting a borrower's payment in half reduces delinquency by 55%; likewise Ganong and Noel (2018) study smaller, 15% reductions in payment size, and find reductions in default rates of about 15%. We focus on heavily underwater borrowers and find that a more modest reduction in payment size of about 4% (about $600 per year) can still reduce delinquency by between 12-20%. Turning to consumption, Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) find a large response for durable consumption and deleveraging; we find no effect for durables, but an increase in nondurables and services. We find the opposite effect of interest rates on deleveraging: households with higher interest rates are more likely to prepay their mortgage, though the magnitudes are economically small. The composition of spending likely depends on the size of payment reductions—Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) study large rate resets and see an increase in big purchases and deleveraging, while we study small differences in rates and see effects for nondurables and services. Small differences in mortgage payments are not enough to stimulate large car purchases, so in this respect we our results are more closely aligned with the increase in nondurable spending resulting from small tax rebates (e.g. Johnson, Parker, and Souleles (2006)).

2. Data and Sample Construction

We use two main datasets to test the effects of small changes in interest rate. First we use the Freddie Mac loan performance data to study defaults and prepayments, conditioning on the level of equity in the home. Second, we use the Consumer Expenditure (CE) survey to study differences in consumption from 2001-2007 for buyers with slightly different interest rates.

2.1 Mortgage Performance Data

Our primary dataset is the single family home loan level dataset from Freddie Mac, downloaded in November 2016. This data set contains loan level data at origination such as FICO score, loan-to-value, loan amount, interest rate, property location to the 3-digit zip-code (ZIP3), and monthly loan performance data on a portion of fully amortizing fixed-rate mortgages guaranteed by Freddie Mac. The dataset covers approximately 22.1 million fixed-rate mortgages originated between January 1, 1999 and September 30, 2016. Monthly loan performance data is available through March 31, 2016. We use loans for new purchases (not refinancings) and exclude mortgages that were modified after origination, which leaves us with 4.81 million 30-year, fixed rate, single family, owner-occupied mortgages.

We supplement the Freddie Mac data with data from Zillow and from the Federal Reserve Economic Database (FRED). To calculate updated home values, we use Zillow's ZHVI Single-Family Homes Time Series, which provides the median single family home value in each zip code at a monthly frequency. We also use the 30-Year Fixed Rate Mortgage Average in the United States and 10-Year Treasury Constant Maturity Rate from FRED.

2.2 Mortgage Performance Sample Construction

Our central hypothesis is that borrowers with higher mortgage payments should default more frequently. We want to flexibly control for other factors that can drive default. In particular, there is a large literature on the effect of home equity on default and household behavior. We thus build samples of borrowers facing similar levels of home equity and run tests separately for each sample. This allows other control variables, such as credit score, to respond differently when the borrower becomes further underwater. We construct samples as follows.

First, we use the ZHVI Single-Family Homes Time Series (HPI) to calculate home equity for every borrower in the Freddie Mac sample for every month t using the following formula. We assume that the borrower paid fair market value for his home at time 0 and that the value of his home changes over time according to the HPI for his ZIP3 code.

Negative Equity Samples

We identify the time at which each borrower's equity first drops below a certain threshold (e.g., -10%,-20%,-30%,-40%, -50%). We exclude the loans that are not current (i.e. 'CURRENT LOAN DELINQUENCY STATUS' in the Freddie Mac performance data not equal to 0) when equity drops below the threshold. We observe each borrower for

12 months after his equity drops below the threshold, recording if he defaults, which we define as becoming 60 day delinquent, or voluntarily prepays. A borrower whose equity never drops below the threshold is not included in the sample, and a borrower whose equity drops below the threshold more than once is only included the first time his equity drops below the threshold.

Positive Equity Samples

To make our sample of loans with positive equity comparable to our samples of loans with negative equity, we require that the house value is falling. In particular, we require that the home equity drops by at least 10% before we add it to a sample. For example, in order to be added to the sample of loans with 10% positive equity, we require that the loan at one point had 20% positive equity, but suffered a fall. As soon as the equity of this loan crosses 10%, we add it to our sample. Just as we did for the negative equity samples, we exclude loans that are not current when the borrower first crosses the threshold. Once the loan is in our sample, we observe default and prepayment behavior for the following 12 months. We construct samples similarly for 0% positive equity through 50%.

2.3 Mortgage Performance: Key Variables

Default

For the purpose of this paper, we define default to mean delinquency of at least 60 days within 12 months of crossing the equity threshold. Delinquency is indicated by number 2 in the 'CURRENT LOAN DELINQUENCY STATUS' in the Freddie Mac performance data.

Prepayment

Prepayment is when the borrower repays the loan in full voluntarily before the maturity date, again within 12 months of crossing the equity threshold. Prepayment is indicated by 'ZERO BALANCE CODE' 01 in the Freddie Mac performance data.

2.4 Mortgage Performance: Descriptive Statistics

Descriptive statistics for selected samples are provided in Table 1. Panel A provides descriptive statistics for mortgages upon reaching -30% equity. Panel B compares the interest rate, default rate and prepayment rate in each negative equity sample, and panel C provides descriptive statistics for mortgages upon reaching +30% equity. Most 30% negative equity loans were originated between 2004 and 2007 and reach negative equity between 2009 and 2011. Default rate increases with negative equity and prepayment decreases. Most 30% positive equity loans were originated between 2003 and 2005 and reach the positive equity threshold between 2008 and 2010.

2.5 Consumption data

For our consumption analysis, we use data from the Consumer Expenditure (CE) Survey. The CE includes quarterly data on household expenditures, income, and demographics, which are obtained quarterly via interviews with 30,000 consumer units (households). Consumers are interviewed for four quarters, with new consumers being added every quarter. The CE contains comprehensive data on household expenditures divided into highly granular categories. For a list of categories we will use in our analysis, see Appendix Table A.1. The CE also contains detailed mortgage information, including mortgage amount, interest rate, month of issue, term, and type of loan.

For our analysis, we use the CE from Q1 2001-Q1 2007. Before Q1 2000 and after Q1 2007, the survey does not includes data on the interest rate at loan origination. Since origination interest rate is crucial to our experimental design, and current interest rate, which is reported, may not match the origination rate, we restrict ourselves to this period. We also exclude the year 2000 because some expenditure categories (specifically, vehicle purchase, transportation, and housing outlays) are not available in these (and earlier) surveys. Aggregate consumption is available for the year 2000 and results for aggregate consumption are unaffected by the inclusion of data from 2000. We consider only consumer units with 30-year fixed rate mortgages on their primary residence. We exclude refinanced and renegotiated mortgages by excluding all loan for which the interest rate at origination is different than the current rate, and by explicitly excluding all loans which are marked as renegotiated or refinanced. These maturity, refinancing, and renegotiation restrictions match those used for the default analysis. We include in the sample only borrowers with a mortgage origination in the last 2 years. This is because older mortgages are likely to have been refinanced. While these refinancings should, in principle, be reported in the CE, they often are not: mortgage rates as much as 10 p.p. below the benchmark rate are sometimes reported for older mortgages. We also exclude from the samples all CU-quarters for which total expenditure is less than $100, or where income is reported to be $0.

Quarterly interviews are not conducted at the end of the quarter. The interviews are distributed equally across each month in a quarter and record expenditure in the previous months in the quarter. For example, an interview conducted in February records expenditure in January and an interview conducted in March records total expenditure in January and February. We therefore adjust the recorded expenditure to arrive at the quarterly expenditure. If the interview was conducted in the second (third) month of the quarter, we multiply all the expenditures by 3 (1.5).

3. Research Design

We exploit within-year variation in the national mortgage rate. Regardless of local economic conditions or borrower characteristics, loans originated when the national rate is higher will have a higher contracted interest rate. Consequently, these borrowers will have larger mortgage payments (assuming, as we will check, that loan size does not shrink much with higher rates) and greater liquidity problems. The key assumption behind this design is that borrower and loan characteristics for loans originated in May of one year look comparable to loans originated in, say, October of the same year. We implement our design by including fixed effects for the year of mortgage origination; differences in rates across borrowers are thereby driven by the timing of loan origination within a given year.

3.1 Loan payment

We attempt to establish the causal effect of mortgage payment on loan repayment. Most of our results will use a two-stage least squares regression, with a linear probability model of default or prepayment as the second stage. We use the linear probability model to avoid imposing structure on the correlation between the error terms in the first- and second-stages. The benchmark interest rate is the instrument. We also report an ordinary least squares regression equivalent to the second stage, but use the actual mortgage rate rather than the instrumented rate. We use the samples constructed in the previous section 3.B, so that each mortgage will appear in the sample at most once upon reaching a specified equity threshold. By conditioning the samples on home equity, we can flexibly account for a primary driver of mortgage default.

In the Freddie Mac mortgage data we are able to control directly for variables such as FICO score, loan-to-value, the natural log of the loan amount, and the debt-to-income ratio. Further, we account for region-year fixed effects as well as region-cohort fixed effects, where the region is given by the three-digit zip code (ZIP3), the year fixed effects correspond to the year in which a loan reached the equity threshold, and the cohort fixed effects correspond to the year of mortgage origination. The region-year fixed effects control for time-varying local economic conditions that can drive payment behavior, similar to the county-time fixed effects in Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017). For context, there are 3.5 times as many counties in the U.S. as there are three-digit zip codes, so we lose some granularity with the Freddie Mac data. When measured by land area, the 25th percentile Zip3 is actually smaller than the 25th percentile county, though the average Zip3 is 3.5 times bigger than the average county. The cohort-year fixed effects allows for changes in lending standards over the course of the housing boom.

More formally, the first-stage regression has the following specification:

$$\text{Contracted Rate}_i = a + b_1 \text{Benchmark Rate}_i + X_i b_2 + e_i$$

Contracted Rate is interest rate on the mortgage, X is a matrix of control variables, including indicator variables for ZIP3 / origination year and ZIP3 / observation year, and Benchmark Rate is the average interest rate for all mortgages issued in the same month as mortgage i. From this first stage, we obtain fitted values: $Rate_i$.

The second stage then has the following specification:

$$Y_i = \alpha + \beta_1 \text{Rate}_i + X_i \beta_2 + \epsilon_i$$

where Y is an outcome variables, either default of prepayment. β_1 is thus the coefficient of interest, with a positive sign indicating that borrowers with larger mortgage payments are more likely to default (or prepay).

3.2 Consumption

Our regression specifications are analogous in the Consumption Expenditure (CE) data. The primary difference is the availability of control variables. We use state-level indicators when controlling for region-year and region-cohort fixed effects. Household controls include the natural logarithm of family income as in Green, Melzer, Parker and Rojas (2018), family size, and the squared age of the head-of-household as in Melzer (2017).

4. Main Results – How do differences in rates affect behavior?

4.1 Mortgage default – OLS results

We consider the relationship between rates and default. We first present suggestive, rather than causal evidence. For this evidence, we turn to the samples constructed in Section 3. Loans appear in the sample for one year after reaching a specified level of equity and are considered to have defaulted if they become at least 60 days delinquent at any point during this year.

We model default using an OLS regression with default, a binary indicator, as the lefthand-side variable. We report the regression statistics in Table 2. The five columns use samples that include loans when they reach different negative equity thresholds for the first time. The table's five columns all use the same specification: FICO, LTV, log(amount), and DTI are included as controls, ZIP3 Code/Negative Equity Year and ZIP3 Code/Origination Year fixed effects are included, and standard errors are clustered by ZIP3 Code/Negative Equity Year.

The key variable is the interest rate, which measures the relative size of the monthly cash flow. For all negative equity thresholds, the coefficient on the interest rate is about 0.032. A 50 bp change in interest rate leads default to rise by about 1.6%. A similar relationship holds for all samples. Clearly this variable is important in predicting default—the implied change in default is 25-44% of the sample average default rate. However, it is likely that the interest rate is not random—in particular, borrowers with worse credit will need to make higher payments. Thus, the OLS results are picking up both the effect of payment size and unobserved credit quality.

4.2 Mortgage default – Instrumental variable approach

We attempt to address the omitted variable problem by using the benchmark interest rate as an instrument for contracted rate. The benchmark interest rate mechanically affects the contracted rate, but it is not clearly related to default in any other way. In the long run, the benchmark interest rate and average creditworthiness of borrowers may be correlated, but given that we only consider within-ZIP3/Origination Year variation, creditworthiness should be absorbed in the fixed effects. We use a two stage least squares with the monthly benchmark interest rate (mean interest rate for all 30-year, single-family, Freddie Mac loans issued in a given month) as the instrument.

First stage results

In Table 3, we report regression statistics for the first stage regression, in which the contracted rate is regressed on the benchmark interest rate. We use the same samples as in Table 2 (equity cutoffs of -10%, -20%, -30%, -40%, and -50%), as well as the same set of control variables and fixed effects, and the same standard error clustering. The coefficient on benchmark interest rate is 0.75 for the -30% equity sample. This means that borrowers' contracted rate do not quite move 1:1 with the benchmark interest rate, likely because there is within-month variation in the benchmark rate that creates noise. Negative equity levels from -10% to -40% have similar coefficients of 0.72-0.76, whereas the most extreme negative equity level of -50% has a larger coefficient of 0.88.

Generally speaking, a 50 bp change in the benchmark rate causes the mortgage payment to change by less than 5% of the average mortgage payment. Given that the within-year benchmark rate has a standard deviation of 24bps, we are dealing with relatively small changes in payment size. The other coefficients behave as we would expect—interest rates decreases with FICO, and increases as the size of the loan increases (both in absolute terms and relative the value of the house and the income of the borrower).

Second stage results

We now turn to the second stage regression, a regression of default on the instrumented interest rate. With this result, we attempt to determine the causal effect of payment size on default. This second stage is a linear probability model identical to that in Table 2, except that it uses only the variation in interest rate coming from the benchmark interest rate to identify the relationship between rates and default.

We report the regressions statistics in Table 4. The coefficient on the interest rate varies between .018 and .024, depending on the sample used. This means that a change in the national rate of 50 bp (which leads the predicted interest rate to change from 36 bp to 44 bp) causes the default probability to rise by 66 bp to 88 bp. The constancy of the coefficient is striking relative the large differences in default rates across the samples. As a fraction of the average default probability, a 50bp increase in the benchmark rate causes default to rise between 10% and 19%; the -10% and -20% equity samples experience the largest increase in default probability relative the sample average (because the sample average is low, while the marginal effect of interest rates is relatively constant across samples). The relative constancy of the effect across samples suggests that the effect is driven by liquidity constraints rather than strategic considerations. Borrowers with -50% equity have much different incentives for strategic default than borrowers with -10% equity, but both react similarly to a change in their mortgage payment. Similar to the first-stage, the coefficients on the control variables generally behave as we would expect. Finally, the Sanderson and Windmeijer (2016) conditional F-statistic, a test of weak instruments, is substantially higher than conventional critical values.

Figure 2 illustrates a simple version of our two-stage results graphically for the -30% equity sample. Panel A plots the first-stage results: we absorb ZIP3*(year of origination) and ZIP3*(year of negative equity) from both the contracted rate and the benchmark interest rate. We plot the residuals, which we denote "FE-adjusted rate" and "FE-adjusted IV" against each other and fit a kernel regression. There is a strong and precise relationship between the benchmark rate and the contracted rate. Most of the data is within the middle 50 basis points, for which we see an increase in the contracted rate of about 25 bp.

Panel B of Figure 2 plots a version of our second-stage. The y-axis is the default probability after absorbing our fixed effects and adding back the sample average, and the x-axis is the effect of our instrument on the interest rate. More precisely, the x-axis is the predicted impact of our instrument on the contracted rate from a first-stage regression of contracted rate on our instrument and fixed effects; we standardize this quantity by its standard deviation. We see that a one standard deviation move in our instrument around 0 leads to an increase in default of 31.5 bps, or 6% of the sample average. A two-standard deviation move in the instrument has a proportional impact—defaults change by about 63 bp. This figure shows our basic result without any controls for the credit quality of individual loans beyond the fixed effects.

Our design is not limited to negative equity loans. If payment size is important for default, it may matter even for borrowers with positive equity, particularly if there are liquidity constraints. Table 5 shows our results for homes with between 0% and 50% positive equity. To make our samples comparable to the negative equity table, we require that home values have fallen by at least 10% before they cross a given positive equity threshold and are added to our sample.

Loans with less than 20% positive equity have similar default behavior as loans with negative equity. The coefficient on interest rate is around 0.015—indistinguishable from loans with negative equity despite the much smaller average default probability for positive equity loans of around 2%. Further, the sensitivity of default to interest rates becomes substantially weaker around 20% positive equity, which is a typical threshold for easy refinancing. These facts suggest that liquidity constraints are driving the defaults stemming from small changes in mortgage payments.

Figure 3 graphically illustrates our results for a range of samples. For a given level of equity, figure 3 plots the second-stage regression coefficient on the contracted interest rate along with the standard error. We see that a one standard deviation change in our instrument has a similar effect on default for all of our negative-equity samples. The marginal effect is roughly constant across negative-equity samples—a one standard deviation move in our instrument can explain between 5-10% of each sample's average default.

While our instrument continues to predict default for the positive equity samples, the coefficients shrink toward zero. The drop becomes particularly noticeable around 20% positive equity, where many borrowers are likely able to refinance.

To conclude this section, we perform balancing tests to address concerns about omitted variables. The primary threat to our identification is that better quality borrowers happen to buy when national rates are low, in which case our design may be picking up differences in credit quality rather than payment size. While we think it is difficult to time the market with precision within a given year, it is valuable to examine how observable borrower characteristics correlate with our instrument. To do this, we perform balancing tests by re-running our two-stage design as follows: in the first-stage, we regress the contracted rate on the benchmark rate and our fixed effects, but not the other control variables. The second stage regresses each control variable on our fixed effects and the predicted contracted rate from the first-stage. This method allows us to see how the control variables correlate with the instrument. Table 6 shows our balancing tests for loan-to-value, log(Loan amount), FICO score, and debt-to-income. For the first three variables, we see that our instrument for higher mortgage payments is correlated with better credit quality—a 1pp increase in the interest rate is associated with a reduction in loan-to-value of 1.6 (less than 2% of the sample average), a 3.2% reduction in loan balance, and 2 extra points on the FICO score, all of which work to reduce the probability of default. Particularly important is the reduction in loan-to-value of -1.62, which can reduce default by 81 basis points according to our second-stage coefficient. This suggests that our instrument may be an underestimating the effect of payment size on default because the instrument is correlated with better credit—that is, higher rates may cause marginal borrowers (who are more likely to default) to refrain from entering our sample. Debt-payment-to-income (D/I) is positively correlated with our instrument, as expected. When the interest rate increases by a greater proportion than the loan balance falls, then the monthly debt payment will rise, driving D/I. When we control for debt-to-income in our regressions, then, the interest rate variable will pick up the incremental effect of the higher mortgage payment on default beyond what is already incorporated in the higher debt-to-income. In any event, a 50 bp increase in our instrument causes debt-to-income to rise by 0.62 (0.5*0.75*1.64), which causes the predicted default probability in the second stage to rise by under 10 bp—a relatively small amount compared to the effect of our instrument on default.

4.3 Deleveraging through full prepayments

We follow Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) and next turn to the effect of interest rates on voluntary deleveraging and consumption. Our primary measure of deleveraging is whether the borrower prepays the remainder of the mortgage balance. In the data, this prepayment could be outright or it could be through a refinancing—we are unable to distinguish the two. Regardless, given the difficult refinancing environment and the negative equity of the sample's borrowers, any prepayment is likely to entail a reduction in leverage.

Interest rates have an ambiguous effect on deleveraging. On the one hand, a higher interest rate at origination gives the borrower greater incentive to refinance. The optimal refinancing decision is typically characterized as an interest rate threshold—once the market rate drops far enough below the origination rate, then the borrower should refinance. Given an equal drop in interest rates, the borrower with a higher rate at origination will want to refinance first. On the other hand, Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) find that borrowers use reductions in interest rates to delever. That is, it may be that constrained borrowers want to delever, but only those with lower interest rates will be able to afford to.

The economic effects are important during a recession. When negative equity mortgagors prepay their mortgage, they must delever and therefore consume less. Positive equity mortgagors, on the other hand, can extract equity from a prepayment, which can stimulate consumption. Tables 7 uses our instrument for payment size to predict prepayments for negative equity loans.

We see that a 50bp increase in the benchmark rate leads to an increased prepayment probability of 1.4% to 1.8% depending on the sample. Relative the sample average prepayment rate, the effect varies from 11.5% for the loans with -10% and -20% equity to 19% for loans with -50% equity. Our instrument therefore has power to potentially explain a large range of household decisions.

Table 8 replicates Table 7, but for the samples of homes with positive equity. We see that our instrument has power for homes that have positive equity and presumably face fewer financial constraints. In fact, the coefficients in Table 8 are higher than in Table 7, which suggests that positive equity borrowers are better able to use their improved position to prepay and avoid relatively high interest rates. While the sensitivity to interest rates is higher for positive equity borrowers, the level of prepayments is also higher, so the effect as a fraction of the sample average is very similar to the negative equity sample. A 50 bp move in the national rate can lead to higher prepayments of 13-18% of the sample average.

So far, we have shown that the interest rate at loan origination has important consequences during house price declines. In particular, higher interest rates cause higher defaults, but also induce higher prepayments across a wide range of home equity levels. For a fuller picture of the effect of interest rates over the business cycle, we apply our design to consumption during the house price boom.

4.4 Interest rate at origination and consumption

We now study the consumption response to lower interest rates. The empirical design is similar to the default analysis. The main differences, as stated in section 3.E, are 1) the consumption data covers 2001-2007, whereas the default analysis conditioned on negative equity (which occurred after 2008), and 2) We look at consumption for households that recently purchased a home (similar to Benmelech, Guren and Melzer (2017)). Table 9 reports summary statistics for the consumption data. We see that the mortgage balance and payments are 25% smaller in the CE than in the Freddie Mac data; this difference is likely driven by the earlier loan originations in the CE sample. Most loans in the CE sample are originated between 2001 and 2004, whereas loans in the Freddie Mac sample were originated from 2005 to 2007 after house prices had appreciated. It is also likely that the Freddie Mac borrowers are of higher quality than the population and thus able to secure larger mortgages. The median quarterly expenditure for a household is \$11,836; the plurality of this consists of nondurable consumption (\$4,460), followed by reported mortgage payments (\$3,300), durables (\$891) and services (\$510).

Table 10 applies our instrument to these spending categories. Column 1 shows the first-stage. The coefficient on the benchmark rate is similar to the coefficients in table 4, though slightly smaller. If the CE respondents do not perfectly recall the beginning month of their mortgage, then the first stage will have more measurement error than in the Freddie Mac sample (where we know the starting month exactly), leading to a lower estimate. The conditional F-stat is 11.8, so the national mortgage rate still strongly predicts the reported rates in the CE sample. A two standard deviation move in our instrument is roughly 50 basis points. Such a movement leads to an increase in the reported interest rate of 29 basis points—this will be our benchmark in interpreting the consumption results.

Columns 2-9 report coefficients for each spending category, alternating from the OLS specification to the IV model. The primary omitted variable in the OLS specification is credit quality—more risky borrowers will have higher interest rates. In columns 2 and 3, we see that higher interest rates lead to lower spending on non-durables in both specifications. In the OLS specification, a 30.6 bp move in the interest rate corresponds to a drop of 1.6% in nondurables; a similar move in the IV specification (which corresponds to a 50bp change in the national rate) leads to a drop of 2.0% in nondurables. Durables are not statistically impacted in either the OLS or IV specifications, though the IV points to an increase in durables while the OLS points in the opposite direction. Both specifications show a drop in services after a 30.6 bp move in the rate—6.8% in the OLS and 20% in the IV. In dollar terms, a higher interest rate can account for the changes in consumption spending. At the median of the distribution, a 50 bp increase in the national rate leads to an increase in the implied mortgage payment of \$320 per year (a 3.3% change). This change in interest rates corresponds to an estimated drop in nondurable spending by \$360, an increase in durables by an (imprecise) \$351, and a drop in services of \$400, for a total change in consumption of \$409—roughly in line with the increased mortgage payment.

Columns 8-11 of table 10 turn to mortgage payments rather than consumption. Columns 8-9 examine principal reductions, defined as mortgage payments in excess of the scheduled payment. Our IV estimate shows an increase in principal reductions for the borrowers with higher interest rates at origination. Economically, the effect is small given that the sample average for principal reductions is only $30 per quarter. The effect is at least consistent with our results on prepayments, where borrowers with higher interest rates are more likely to prepay their mortgage. Further, the economic magnitude of prepayments is in line with Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017)—for conforming ARMs, they find an increase of $12-$17 per month in mortgage prepayments after a 2-3 p.p. cut in interest rates. We find that a 3 p.p. cut in rates leads to $33.30 (3*30*0.371) extra per quarter in mortgage prepayments.

The dependent variable in columns 10-11 is the log of the total reported mortgage payment. The OLS coefficient in column 10 is particularly instructive when comparing the OLS and IV coefficients for consumption in columns 2-7. Column 10 shows that borrowers with a higher interest rate also have a 6.9% lower mortgage payment; this is consistent with reduced borrowing capacity for higher risk/interest borrowers. The coefficient also suggests a reason why our IV estimates for non-durable and service spending are lower than the OLS estimates. Worse borrowers take smaller mortgages, which attenuates the effect of interest rates on consumption. To the extent our IV cleans the estimates of unobserved credit quality, then the IV estimates of consumption spending in columns 3 and 7 will not be biased upwards by the endogenous choice of a smaller mortgage. Column 11 reports the IV estimate of interest rates on mortgage payment. A 1 p.p. move in interest rates should mechanically move mortgage payments on 30-year fixed rate mortgages by about 10%; our IV estimate is 14.5%, though the standard error is 12%. The large standard errors in column 11, like the coefficient in column 10 discussed above, imply that there is substantial variation in the choice of mortgage size and this reduces the power of our IV.

Column 12 reiterates the endogeneity of the contracted interest rate. We see that borrowers with higher interest rates spend 5.1% less for a 1p.p. increase in rates; the magnitude of this effect is very similar to the effect for non-durable consumption (the largest category of expenditure) as well as mortgage payments (the second largest category of expenditures). High interest borrowers appear, then, to have worse prospects, even after controlling for income. We see no statistically significant relation between our instrument and total expenditures in column 13. Column 13 therefore alleviates concerns that our instrument is correlated with an omitted variable that affects the overall credit quality or income of borrowers. Finally, we perform balancing tests on household characteristics. We put each control on the left of our second-stage IV regressions (where the only controls are the fixed effects and the instrument). The results are reported in table 11.

We find no statistically significant relationship between our instrument and three common control variables: squared age of the reference person, family size, and log(mortgage amount). We do, however, find a correlation between our instrument and log(family income)—a 50bp increase in our instrument corresponds to a drop in family income of 17%. It is therefore important to control for income in our regressions. Further, the magnitude of the correlation would yield a smaller effect on expenditures than our instrument, which alleviates concerns about omitted variables being correlated with our instrument.

As a final check, we expand our sample to all households that purchased a home in the previous year (n=5209) as opposed to those who also still have the same interest rate as at origination. One possible issue is that the households who changed their interest rate are of a different quality than those who did not. Further, the motive to refinance is tied to the initial interest rate, and thus may be correlated with our instrument. The last row of table 11 shows that our instrument is not correlated with the decision to change the interest rate within two years of buying the home.

5. Heterogeneity in default: liquidity constraints or strategic default?

We have shown that relatively small changes in mortgage payments can have a significant effect on default. While there is suggestive evidence that the effect is driven by liquidity constraints (the effect is relatively constant across a wide range of borrowers with different levels of negative equity, but the effect goes away when a borrower has 20% equity and can refinance), there could still be a strategic motive for default. Because we are looking at fixed-rate mortgages, a higher rate at origination leads to a future stream of higher payments; the present value of this stream of payments acts to further reduce a borrower's equity and can lead to strategic default.

Table 12 examines financing constraints by reporting results separately for each quartile of FICO score. The analysis is identical to prior tables, but is performed on each FICO subsample and for each negative equity sample. Generally speaking, we see that the effect is larger for lower-credit borrowers. For the sample of loans with 10% negative equity, the lowest quality borrowers are three times as responsive to interest rates as the highest quality borrowers, and this difference is highly statistically significant. We lose observations and statistical power with increasing negative equity, but the coefficient for the worst quartile is around 0.02 higher than the best-quality quartile for loans with 20-30% negative equity. Given that the average effect is 0.02, the heterogeneity is economically significant. Further, there is monotonicity in the coefficient for these samples as we move from low to high-quality credit. These results imply that lower-FICO borrowers have higher borrowing costs and limited access to credit (as in Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017)), making it more difficult to make slightly higher interest payments. The heterogeneity becomes weaker for the loans with 40-50% negative equity. This is consistent with strategic motives becoming more important (relative financing constraints) as negative equity increases, though the imprecision of the estimates limits any conclusions.

To study strategic behavior, we split our sample by state foreclosure law. In particular, we look at two components of foreclosure law: whether the state has judicial foreclosure and whether the state has recourse mortgages. Judicial foreclosure states are preferred by mortgagors because they require a lender to go through the court system when foreclosing. This requirement slows the foreclosure process, allowing mortgagors more time to make back payments and/or live in the home while looking for a new residence. Mortgagors in judicial states therefore have more leverage and have greater incentive to strategically default. Similarly, borrowers in non-recourse states have greater incentive to strategically default. If a state has non-recourse laws, then the mortgage lender cannot seize the borrower's other assets in foreclosure.

Table 13 shows no clear relationship between state foreclosure laws and the sensitivity of default to payment size for loans with -30% equity. The coefficient of interest bounces around the coefficient for the full sample (0.018) from Table 3, and is not statistically different for any subsample. Further, the direction of the effect is inconsistent. Judicial states are more favorable to mortgagors but do not exhibit a higher sensitivity to interest rates than non-judicial states; likewise, non-recourse states are more favorable to mortgagors and yet they are weakly less sensitive to payment size than recourse states. In short, there is little direct evidence that our instrument is picking up strategic incentives for default. This non-result is broadly consistent with the literature, which finds that consumers' sensitivity to strategic default incentives is moderated by non-economic factors (Guiso, Sapienza and Zingales (2013) and Bhutta, Dokko and Shan (2017))—the small effect of our instrument is likely second-order to these factors.

6. Application to monetary policy and robustness exercises

There is a growing literature on the consequences of monetary policy for household behavior (Hurst and Stafford (2004), Mian, Rao and Sufi (2013), Chen, Michaux and Roussanov (2013), Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017), Agarwal, Chomsisengphet, Mahoney and Strobel (2015), Auclert (2017), Beraja, Fuster, Hurst and Vavra (2017)). The primary mechanism is through refinancing—households have equity in their home that they only access when interest rates drop. Thus, monetary policy has a role in stimulating demand through the relaxation of this credit constraint. Our instrument can contribute to this literature. We have shown that small changes in mortgage rates at origination can influence default, deleveraging and prepayments, and consumption. To tie this paper more closely to monetary policy, we replicate our main result using the interest rate on the 10-year Treasury Bond instead of the benchmark mortgage rate.

Table 14 replicates our main results from Table 4, but uses the 10-year Treasury Bond rate at mortgage origination as the instrument instead of the benchmark mortgage rate. The resulting coefficients on contracted rate are generally stronger when using this different instrument, though not statistically distinguishable from what we reported in Table 4. A tight connection between mortgage rates and the 10-year Treasury Bonds suggests that monetary policy today can have long-acting consequences for the future. In particular, tight monetary policy during the housing boom had a hangover effect during the bust in the form of increased defaults stemming from high mortgage payments (which could not be refinanced due to the lack of home equity).

Table 14 can also address concerns about the exogeneity of our main instrument. If the benchmark mortgage rate is influenced by compositional changes in the mortgage market that also impact default (eg the benchmark rate tends to be higher within a given year when there are riskier borrowers) then it may be an invalid instrument. The 10-year Treasury Bond, however, is unaffected by these concerns as it is not directly connected to the mortgage market. The similarity in the estimated coefficients under both instruments suggests that variation in the benchmark mortgage rate is not driven by compositional changes in the mortgage market; rather, it seems that changes in the 10-year Treasury Bond rate are passed through to the benchmark mortgage rate for conforming loans.

6.1 Differences by house price dynamics

The housing bust exhibited substantial heterogeneity across the country. Some areas were largely untouched, while others faced declines of over 60%. When testing for the effects of interest rates on mortgage payment, we condition on implied negative equity, where we infer negative equity using an area's house price index. Further, there may be differences in expectations of future price declines for borrowers who purchased a home in the same area and in the same year. If within-year interest rate variation is correlated with mismeasurement in home equity or in house price expectations, then our estimates will not capture the effect of interest rates. To address these concerns, we perform our main default analysis from table 4 on different subsamples cut by house price dynamics.

Appendix Table A.2 separately shows results for areas with large house price collapses and areas with small house price collapses, with little difference in the coefficients. Appendix Table A.3 splits the samples by the degree of correlation in house prices within each ZIP3. The estimates are all similar to the baseline estimate, though we lose power when splitting the sample in quartiles. The coefficients imply that our results are strongest in areas with the highest degree of house price correlation (where the 10th percentile of the pairwise zip-code price correlations is over 90%), and it is these areas where mismeasurement within-ZIP3 is likely to be the smallest.

6.2 Within-quarter variation

Our primary specification uses within-year variation in interest rates. We can sharpen the analysis by using within-quarter variation, with some cost in statistical power. It seems particularly unlikely that borrowers can change their home purchase decisions in reaction to interest rates over a three-month period. Appendix Table A.4 replicates table 4 using quarterly fixed effects in place of yearly fixed effects. The table shows that the benchmark national mortgage rate is still strongly linked to default for homes that reach 10-20% negative equity. Appendix Table A.5 replicates the analysis using the 10-year Treasury bond rate as the instrument and finds very similar coefficients.

7. Conclusion

This paper studies the effect of modest differences in mortgage rates at origination on household behavior. There is substantial evidence in the literature that large reductions in rates (over 1.5 p.p) can prevent many defaults and spur consumption. However, policy makers had difficulty incentivizing financial intermediaries to make such large reductions during the crisis. It may be easier to successfully subsidize smaller reductions in interest rates, on the order of magnitude of 50bp or less. We show that these modest reductions can still significantly reduce defaults—cutting payments in half can reduce defaults by 55% (Fuster and Willen (2017)), but we show that cutting payments by as little as 5% can reduce defaults by 10-20%. Our results are consistent with severe liquidity constraints during the crisis; we find little evidence of strategic default as a response to small changes in payment size. By studying the interest rate at loan origination (as opposed to the contemporaneous interest rate), we can quantify the long-term consequences of monetary policy.

To identify our effect, we use small, within-year fluctuations in the 30-year fixed rate mortgage rate. For two borrowers who buy a home in the same year, within the same 3-digit zip code, and whose homes eventually reach similar levels of negative equity, monthly changes in the national rate mechanically impact the size of their payments but should not otherwise impact default or prepayment behavior. Balancing tests show that increases in our instrument tends to be slightly correlated with better credit quality except for the mechanical increase in payment size.

We validate our instrument's first-stage using the Consumer Expenditure survey, and find a relationship between payment size and nondurable consumption. Total expenditure is unchanged, which suggests that the change in payment size is substituting for a change in nondurable spending. This provides further evidence that the instrument is not picking up credit quality, income or wealth.

Our instrument is strong due to the mechanical effect of interest rates on payments, and has potentially wide applicability. We show that payment size effects default, prepayments and consumption. Conceivably, any household outcome can be studied with our simple instrument.

Implementing small changes in interest rates may be practically difficult in a crisis given the fixed cost of refinancing a loan. Somebody must be willing to bear this cost. It seems plausible that financially constrained borrowers would be willing to add the cost to their loan balance, in which case somebody (an intermediary or the government) would have to make the loan. Subsidizing closing costs through a government loan would resemble the "national line of credit" policy advocated by Kimball (2012). In any case, streamlining the refinance process could greatly reduce liquidity-driven defaults.

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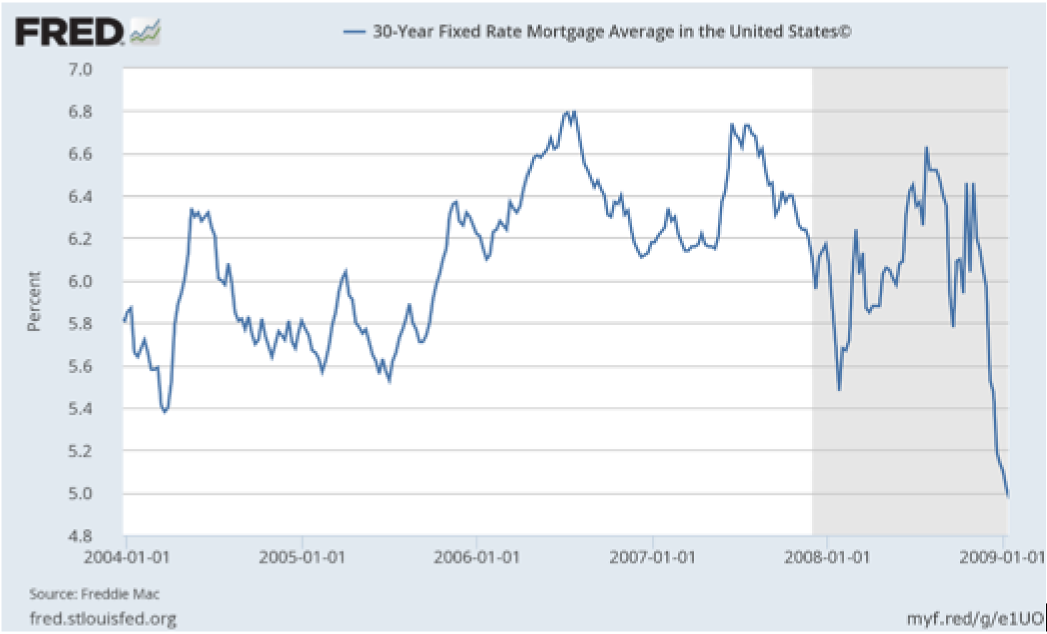
Figure 3.1: Variation in average mortgage rates

This figure plots our instrumental variable. We use within-year variation in the 30-year fixed rate mortgage average to estimate differences in payment size for mortgages originated in the same year and in the same 3-digit zip code. This series is plotted in Panel A. Panel B plots the 10-year Treasury Rate for comparison.

Panel A: 30-year fixed rate mortgage average in the U.S.



Panel B: 10-year Treasury constant maturity rate

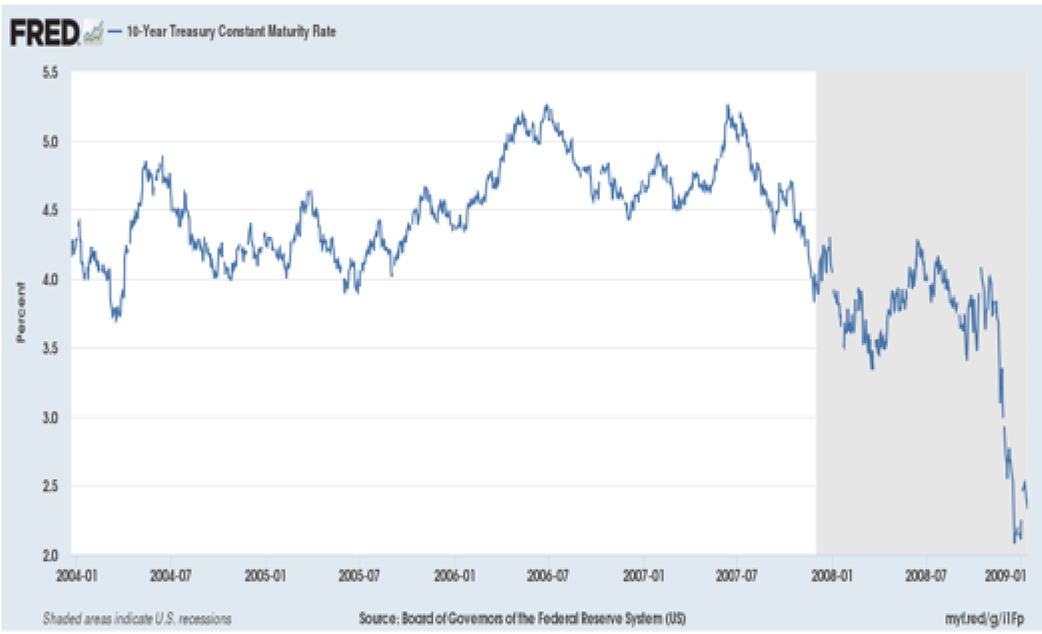
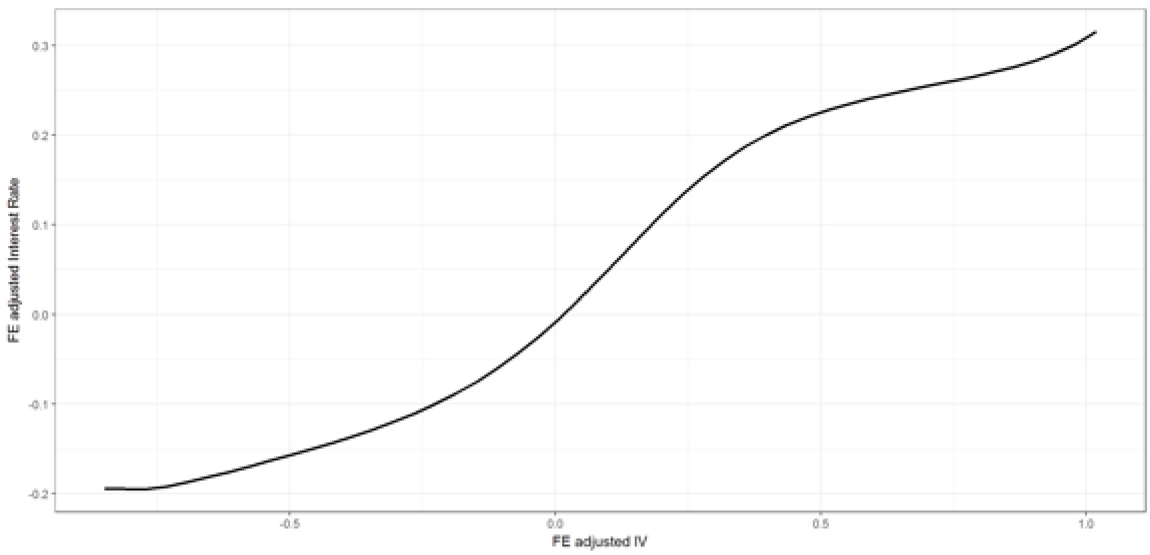


Figure 3.2: Graphical Illustration: 30% Negative Equity Sample

This figure shows the relationship between instrumented mortgage interest rate and default rate for the 30% negative equity sample. Panel A shows the relationship between the instrument (30 year benchmark mortgage rate) and the endogenous variable (mortgage interest rate). Both variables are adjusted for ZIP3 * Negative Equity Year and ZIP3 * Origination Year fixed effects. Panel B plots the Nadaraya-Watson kernel regression estimates (solid line) of default versus instrumented mortgage interest rate (normalized by a standard deviation). ZIP3 * Negative Equity Year and ZIP3 * Origination Year fixed effects are included in the instrumented mortgage interest rate regression, but both the x-and y-axis are fixed-effect adjusted. The bandwidth in panel A is 0.19, and 0.14 in panel B.

Panel A



Panel B

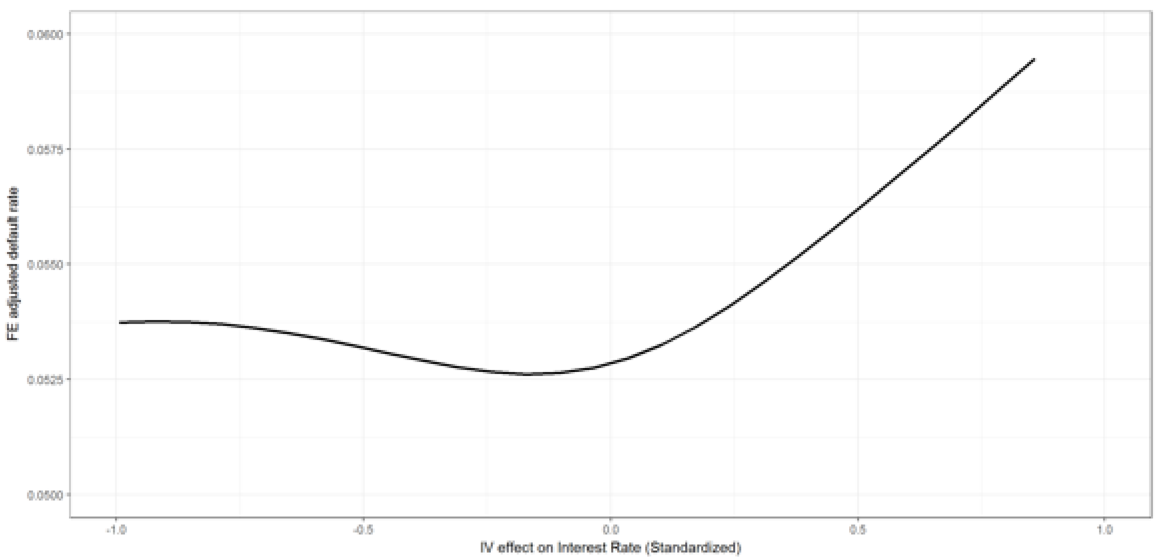


Figure 3.3: Impact of interest rate on default

This figure plots the coefficient estimates of the instrumental variable regression for each equity threshold where, mortgage interest rate is the independent variable being instrumented for and benchmark 30-year mortgage rate is used as the instrument. Error bars represent standard errors.

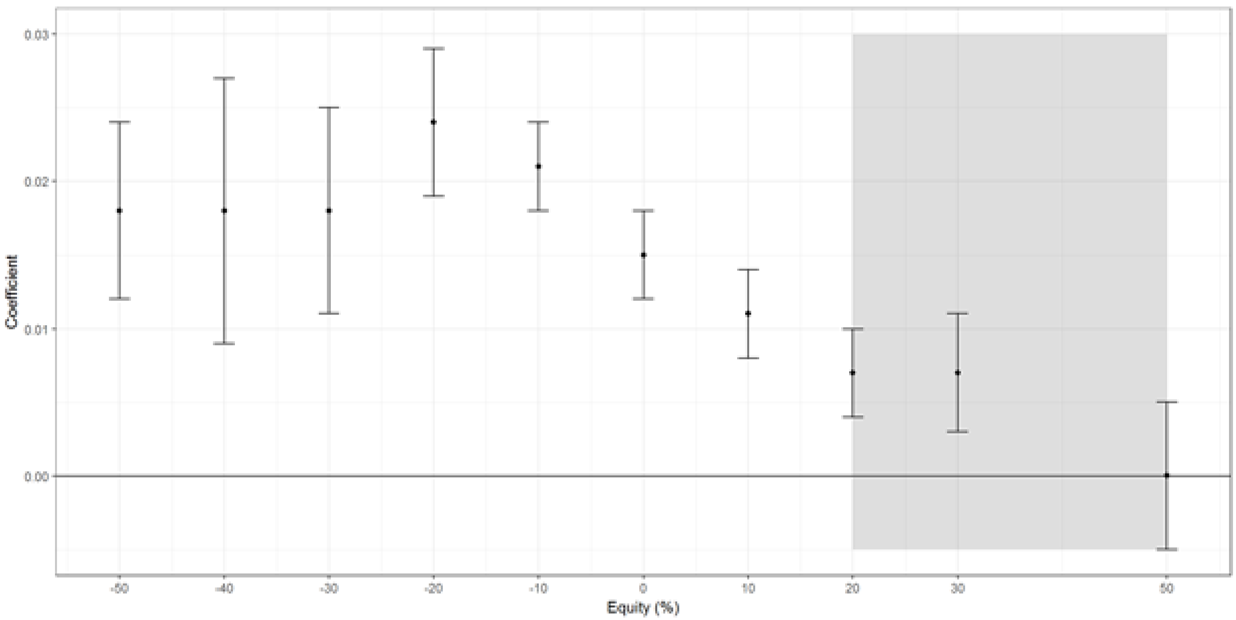


Table 3.1: Descriptive Statistics

Panel A of this table provides descriptive statistics for all Freddie Mac, 30-year fixed rate, single family, owner-occupied mortgages upon reaching -30% equity. Refinanced mortgages were excluded. A loan is considered in default (prepaid) if it becomes at least 60-days late (is prepaid) within 1 year of reaching -30% equity. Credit information is from origination date. Equity for each borrower for every month estimated using the initial house value, outstanding mortgage balance and Zillows ZHVI Single-Family Homes House price index (HPI). Panel B compares the interest rate, default rate and prepayment rate in each sample. These samples are constructed similarly to the -30% equity sample, but using other equity cutoffs instead of -30%. Panel C provides descriptive statistics for all Freddie Mac, 30-year fixed rate, single family, owner-occupied mortgages upon reaching +30% equity.

Panel A

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Default	0.0539	0.2258	0	0	0	45,282
Prepayment	0.1092	0.3119	0	0	0	45,282
Mortgage Interest Rate	6.2422	0.4824	5.8750	6.2500	6.5000	45,282
National mortgage rate	6.1947	0.3582	6.0300	6.2400	6.4200	45,282
FICO	726.99	52.19	690	732	769	45,251
Mortgage payment	1,367	550	941	1,287	1,737	45,282
Loan-to-value	90.43	8.49	80	95	95	45,282
Debt-to-income	38.57	10.83	31	39	46	44,431
Loan amount	207,548	85,329	141,000	195,000	265,000	45,282
Origination year	2006.31	1.31	2005	2006	2007	45,282
Negative equity year	2009.76	1.20	2009	2010	2011	45,282

Panel B

Equity (\%)	Interest Rate (\%)	Default Rate (\%)	Prepayment Rate (\%)
-50	6.25	6.34	9.64
-40	6.25	6.06	9.82
-30	6.24	5.39	10.92
-20	6.22	4.54	12.49
-10	6.17	3.78	14.45
0	5.99	2.98	15.84
10	5.93	2.06	17.03
20	5.92	1.56	16.37
30	5.92	2.12	15.48
50	6.23	1.81	16.52

Panel C

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Default	0.0211	0.1437	0	0	0	54,488
Prepayment	0.1547	0.3616	0	0	0	54,488
Mortgage Interest Rate	5.9174	0.5662	5.6250	5.8750	6.2500	54,488
National mortgage rate	5.9760	0.4912	5.7200	5.8800	6.2750	54,488
FICO	727.01	54.00	689	735	771	54,488
Mortgage payment	1,046	437	700	975	1,343	54,488
Loan-to-value	77.91	13.75	70	80	90	54,488
Debt-to-income	35.95	11.40	28	36	44	54,488
Loan amount	177,473	76,666	117,000	165,000	230,000	54,488
Origination year	2003.81	1.76	2003	2004	2005	54,488
Negative equity year	2009.16	1.49	2008	2009	2010	54,488

Table 3.2: Negative Equity, Default, OLS

This table examines the relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for ordinary least squares regressions (linear probability model) with default as the dependent variable. Loans are put into the sample upon reaching the designated equity levels and defined to have defaulted if the become 60 days delinquent within the next year. Loans are put into the sample at most once, the first time they reach a given level of negative equity. Control variables are from origination date. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10\% NE (1)	20\% NE (2)	30\% NE (3)	40\% NE (4)	50\% NE (5)
Interest Rate	0.033***	0.031***	0.034***	0.032***	0.032***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.004)
FICO	-0.0003***	-0.0003***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	0.001	0.004	0.006*	0.010**	0.012**
	(0.002)	(0.002)	(0.004)	(0.005)	(0.005)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.117	0.106	0.117	0.118	0.122

Table 3.3: Negative Equity, First Stage

This table examines the relationship between 30 year benchmark mortgage rate and mortgage interest rate. Regression statistics are reported for an ordinary least squares regression with mortgage interest rate as the dependent variable and 30 year benchmark mortgage rate as the independent variable. This regression is the first stage of a two stage least squares, in which monthly mean of 30-year fixed rate mortgage rate is an instrument for mortgage interest rate. Loans are put into the sample upon reaching the designated equity levels. Loans are put into samples at most once, the first time they reach a given level of negative equity. Control variables are from origination date. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10\% NE	20\% NE	30\% NE	40\% NE	50\% NE
	(1)	(2)	(3)	(4)	(5)
Mortgage Rate	0.725***	0.737***	0.751***	0.759***	0.888***
(US Average)	(0.006)	(0.008)	(0.010)	(0.012)	(0.013)
FICO	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.007***	0.008***	0.008***	0.009***	0.010***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
log(Loan amount)	-0.185***	-0.182***	-0.181***	-0.181***	-0.178***
	(0.003)	(0.004)	(0.005)	(0.007)	(0.008)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.67	0.592	0.548	0.544	0.538

Table 3.4: Effect of Interest Rate on Default, Negative Equity Samples, IV

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10\% NE	20\% NE	30\% NE	40\% NE	50\% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.021***	0.024***	0.018***	0.018**	0.018***
	(0.003)	(0.005)	(0.007)	(0.009)	(0.006)
FICO	-0.0003***	-0.0004***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	-0.001	0.003	0.003	0.007	0.009*
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	2,500	1,544	1,192	816	978
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.088	0.076	0.083	0.087	0.093

Table 3.5: Positive Equity, Default, IV

This table examines the causal relationship between mortgage interest rate and default for positive equity loans. Regression statistics are reported for instrumental variables regressions. Default (60-day delinquency within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. Loans are required to be above the specified levels of equity and then cross below the specified threshold for the first time. Standard errors are clustered at ZIP3 Code * Positive Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	0\% PE	10\% PE	15\% PE	20\% PE	25\% PE	30\% PE	50\% PE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Interest Rate	0.015***	0.011***	0.015***	0.007**	0.004	0.007	0.00003
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
FICO	-0.0003***	-0.0002***	-0.0003***	-0.0002***	-0.0002***	-0.0003***	-0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.003***	0.002***	0.002***	0.001***	0.001***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Loan amount)	-0.002	0.0005	0.001	-0.0001	-0.0002	-0.0004	-0.002
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Debt-to-income	0.001***	0.0003***	0.0003***	0.0002***	0.0002***	0.0002***	0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,645	120,611	88,672	73,542	62,825	54,488	16,803
Adjusted R^2	0.083	0.066	0.069	0.059	0.056	0.058	0.051

Table 3.6: Balancing tests for control variables

This table examines the relationship between instrumented mortgage interest rate and the control variables used in previous regressions for 30% negative equity sample. Estimates are reported for instrumental variables regressions with the control variables from our main IV regressions as dependent variables. The first stage of these regressions includes benchmark mortgage rate as the instrument, and fixed effects (ZIP3 * Neg. Equity Year and ZIP3 * Origination Year) as RHS variables. The second stage includes only fixed effects and the instrumented mortgage interest rate as RHS. Loans are included in the sample upon reaching -30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

Dependent Var	Coefficient	Std. Err	Observations	Adjusted R^2
Loan-to-value	-1.621***	(0.294)	44,401	0.741
log(Loan amount)	-0.032**	(0.013)	44,401	0.376
FICO	1.905	(1.631)	44,401	0.029
Debt-to-income	1.643***	(0.301)	44,401	0.025

Table 3.7: Negative Equity, Prepayment, IV

This table examines the causal relationship between mortgage interest rate and prepayment for negative equity loans. Regression statistics are reported for instrumental variables regressions. Prepayment (within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for by benchmark 30-year mortgage rate. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10\% NE	20\% NE	30\% NE	40\% NE	50\% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.047***	0.039***	0.037***	0.047***	0.041***
	(0.006)	(0.008)	(0.010)	(0.011)	(0.011)
FICO	0.0004***	0.0004***	0.0004***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	-0.003***	-0.003***	-0.003***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	0.107***	0.097***	0.099***	0.083***	0.077***
	(0.004)	(0.005)	(0.006)	(0.008)	(0.008)
Debt-to-income	-0.001***	-0.001***	-0.001***	-0.0004**	-0.0005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.087	0.104	0.12	0.118	0.118

Table 3.8: Positive Equity, Prepayments, IV

This table examines the relationship between mortgage interest rate and prepayment for positive equity loans. Regression statistics are reported for instrumental variables regressions. Prepayment (within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. Loans are required to be above the specified levels of equity and then cross below the specified threshold. Standard errors are clustered at ZIP3 Code * Positive Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	0\% PE	10\% PE	30\% PE	50\% PE
	(1)	(2)	(3)	(4)
Interest Rate	0.067***	0.085***	0.056***	0.073***
	(0.008)	(0.008)	(0.010)	(0.016)
FICO	0.0005***	0.0005***	0.0005***	0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	-0.002***	-0.002***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.001)
log(Loan amount)	0.123***	0.133***	0.115***	0.132***
	(0.004)	(0.005)	(0.006)	(0.010)
Debt-to-income	-0.001***	-0.001***	-0.001***	-0.0004
	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes
Observations	129,645	120,611	54,488	16,803
Adjusted R^2	0.091	0.095	0.127	0.126

Table 3.9: Descriptive Statistics: CE Survey

This table provides descriptive statistics for mortgage characteristics, household characteristics and main expenditure categories in the regression sample—borrowers with a 30 year fixed rate mortgage originated in the last 2 years since the interview year, who did not refinance and interview quarter from 2001 quarter 1 to 2007 quarter 1.

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Mortgage interest rate	6.65	1.09	5.88	6.50	7.35	4,782
National mortgage rate	6.43	0.80	5.81	6.24	7.01	4,782
Mortgage amount	149,630	90,363	90,000	132,000	189,000	4,782
Non-durable exp.	5,273	3,541	3,240	4,460	6,235	4,782
Durable exp.	1,473	3,404	195	891	1,680	4,782
Services exp.	1,063	2,376	165	510	1,243	4,782
Mortgage payment	3,766	2,394	2,340	3,300	4,500	4,782
Total exp.	15,069	12,534	8,460	11,836	17,170	4,782
Principal repayment	30	230	0	0	0	4,782
Origination year	2002.71	1.94	2001	2003	2004	4,782
Interview year	2003.71	1.77	2002	2004	2005	4,782
Family income after tax	70,983	49,315	40,095	61,279	89,001	4,782
Family size	2.96	1.52	2	3	4	4,782
Age of ref. person	39.83	11.82	31	38	47	4,782

Table 3.10: Impact of Interest Rate on Consumption

This table examines the causal relationship between mortgage interest rate and various expenditure types for borrowers with a 30 year fixed rate mortgage originated in the last 2 years since the interview year, who did not refinance and interview quarter from 2001 quarter 1 to 2007 quarter 1. Column (1) reports regression statistics for an ordinary least squares regression with mortgage-to-loan (multiplied by 100,000 to make coefficients readable) as the dependent variable and 30 year benchmark mortgage rate as the independent variable. This regression is the first stage of the IV regressions reported in this table. Columns (2), (4), (6), and (8) report regression statistics for ordinary least squares regression of each expenditure category on mortgage-to-loan. Columns (3), (5), (7), and (9) report regression statistics for instrumental variable regressions where each expenditure category is the dependent variable, mortgage interest rate is the independent variable being instrumented for and benchmark 30-year mortgage rate is used as the instrument. Standard errors are clustered at household and interview year and reported in parentheses below coefficient estimates. *, **, *** denote two-tailed significance at the 10%, 5% and 1% levels, respectively.

	Interest Rate	log(Non-durable exp.)		log(Durable exp.)		log(Services exp.)		log(Principal reduction)		log(Mortgage payment)		log(Total exp.)	
	First Stage	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Interest Rate		-0.055***	-0.070*	-0.058	0.342	-0.234***	-0.690**	0.065	0.371***	-0.069***	0.145	-0.051***	0.039
		(0.006)	(0.036)	(0.075)	(0.211)	(0.059)	(0.277)	(0.044)	(0.099)	(0.022)	(0.122)	(0.007)	(0.071)
Mortgage Rate	0.580***												
(US Average)	(0.084)												
log(Family income)	-0.114***	0.216***	0.214***	0.418***	0.470***	0.530***	0.471***	0.062	0.102**	0.183***	0.211***	0.297***	0.309***
	(0.029)	(0.026)	(0.025)	(0.046)	(0.068)	(0.077)	(0.076)	(0.038)	(0.046)	(0.028)	(0.039)	(0.026)	(0.026)
Family size	0.062***	0.093***	0.094***	0.112***	0.086***	0.194***	0.224***	0.032	0.012	0.049***	0.035***	0.066***	0.060***
	(0.015)	(0.007)	(0.008)	(0.029)	(0.033)	(0.033)	(0.028)	(0.030)	(0.029)	(0.011)	(0.012)	(0.007)	(0.009)
Age of ref. person^2	0.000	0.00003**	0.00003**	-0.0002***	-0.0002***	0.0003***	0.0003***	0.00005*	0.00005*	-0.00001	-0.00001	0.00002*	0.00002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
State * Org. Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Int. Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cond. F Stat			11.8		11.8		11.8		11.8		11.8		11.8
Observations	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782
Adjusted R^2	0.622	0.372	0.372	0.303	0.283	0.264	0.247	0.44	0.405	0.501	0.44	0.408	0.397

Table 3.11: Balancing regressions – Household characteristics on mortgage-to-loan

This table examines the relationship between instrumented mortgage interest rate and control variables used in previous regressions. Estimates are reported for instrumental variable regressions with the control variables from our consumption IV regression (See table 13) as dependent variables. First stage of these regressions includes benchmark mortgage rate as the instrument, and fixed effects (State * Origination year and State * Interview year) as RHS variables. The second stage includes only fixed effects and the instrumented mortgage interest rate as RHS. Standard errors are clustered by household and interview year.

Dependent Var	Coefficient	Std. Err	Observations	Adjusted R^2
log(Family income)	-0.379***	(0.142)	4,782	0.23
Age of ref. person^2	-1.578	(2.055)	4,782	0.33
Family size	0.130	(0.259)	4,782	0.31
log(Mortgage amount)	0.191	(0.117)	4,782	0.34
Org . Int $\neq$ Curr. Int	5.927	(7.539)	5,209	0.41

Table 3.12: Negative Equity, Default, IV, FICO Split

This table examines the causal relationship between mortgage interest rate and default for 20% negative equity loans with different FICO score levels. This table reports regression statistics for an instrumental variables regression. Default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. The sample is split into FICO score quartiles, and 2SLS regressions are performed separately on each quartile. Median FICO score of each sample is given in the column heading. Loans are considered to have defaulted if they become 60 days delinquent within 1 year of reaching -30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	Q1 ~ 662 (1)	Q2 ~ 712 (2)	Q3 ~ 752 (3)	Q4 ~ 787 (4)
10\% NE	0.037***	0.022***	0.015***	0.012***
	(0.008)	(0.006)	(0.005)	(0.004)
20\% NE	0.035***	0.026***	0.011	0.018***
	(0.012)	(0.009)	(0.008)	(0.007)
30\% NE	0.028*	0.022*	0.022*	-0.002
	(0.016)	(0.013)	(0.012)	(0.009)
40\% NE	0.027	0.023	-0.005	0.025*
	(0.022)	(0.018)	(0.016)	(0.014)
50\% NE	0.014	0.017	0.009	0.030***
	(0.016)	(0.013)	(0.013)	(0.011)

Table 3.13: Negative Equity, Default, IV, Judicial and Recourse State Split

This table examines the causal relationship between mortgage interest rate and default for 30% negative equity loans for Judicial foreclosure states vs non-judicial foreclosure states and recourse states vs non-recourse states . This table reports regression statistics for an instrumental variables regression. Default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. 2SLS regressions are performed separately on each quartile. Loans are considered to have defaulted if they become 60 days delinquent within 1 year of reaching -30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	Non-judicial (1)	Judicial (2)	Non-recourse (3)	Recourse (4)
Interest Rate	0.018**	0.018	0.006	0.025***
	(0.007)	(0.012)	(0.011)	(0.008)
FICO	-0.0004***	-0.0004***	-0.0004***	-0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.006***	0.005***	0.005***
	(0.000)	(0.001)	(0.000)	(0.000)
log(Loan amount)	0.001	0.007	0.015**	-0.002
	(0.004)	(0.006)	(0.006)	(0.004)
Debt-to-income	0.001***	0.0003	0.001***	0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes
Observations	29,390	15,011	15,442	28,959
Adjusted R^2	0.081	0.085	0.079	0.087

Table 3.14: Negative Equity, Default, IV=Tbond

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and 10-year treasury rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10\% NE (1)	20\% NE (2)	30\% NE (3)	40\% NE (4)	50\% NE (5)
Interest Rate	0.026***	0.044***	0.042***	0.058***	0.029***
	(0.006)	(0.009)	(0.012)	(0.016)	(0.009)
FICO	-0.0003***	-0.0003***	-0.0004***	-0.0003***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	-0.0002	0.007**	0.008*	0.015***	0.011**
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	663	416	260	235	112
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.089	0.076	0.084	0.085	0.093