Do Sunk Costs Affect Prices in the Housing Market?*

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Abstract

We use a unique feature of California's property tax system to empirically identify the causal 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, and for properties whose value is harder to assess. 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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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 which seem puzzling in the context of rational economic models. In a seminal paper, Northcraft and Neale (1987) show that ordinary buyers and professional real estate agents alike are subject to an anchoring bias, whereby their assessments of property values are heavily influenced by the listing prices posted by sellers; and recent literature highlights that even sophisticated institutional buyers in the commercial real estate market are not immune to the anchoring bias (Bokhari and Geltner (2011)). In another influential paper, Genesove and Mayer (2001) identify behavior consistent with nominal loss aversion, whereby selling homeowners who expect to incur a nominal loss relative to their purchase price set higher listing prices, attain higher selling prices, and exhibit a much lower sale hazard.

In this paper we examine whether listing prices and transaction prices in the housing market are affected by historical costs incurred by sellers that are unrelated to the properties' FMV ("sunk costs"). Rational economic theory implies that only incremental costs and benefits should affect decisions, and historical costs should be irrelevant. However, behavioral economists have hypothesized that people find it hard to ignore sunk costs while making economic decisions, and term this phenomenon as the sunk-cost effect (Thaler (1980)). There are multiple explanations for such behavior (see Section 1.1 for details): Thaler (1999) suggests that people use mental accounting to classify costs as losses, and perceive outcomes in terms of the prospect theory of Kahneman and Tversky (1979); the cognitive dissonance theory (Festinger (1957)) suggests that people may revalue an asset upward after they have incurred a sunk cost. In the context of the housing market, sunk costs can affect house prices only if buyers are also unable to objectively determine the FMV of residential properties, and rely on listing prices posted by sellers as anchors to assess property values.

The main empirical challenge in testing these hypotheses is that sunk costs are not easy to identify; specifically, it is hard to identify costs incurred by selling homeowners which are plausibly unrelated 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 within the local market level 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 3). 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.

Data on listing prices and listing dates are not readily available in any of standard databases. Therefore, we use an April 2017 extract of Zillow listings from www.zillow.com to hand-

[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 1 provides a simple illustration.

collect data on listing prices and listing dates, as well as detailed information on house and neighborhood characteristics for a sample of recently sold single-family homes in California. We refer to this as the listing sample. We use the Zillow Transaction and Assessment Dataset (ZTRAX) to obtain detailed information on the transaction history and property tax history for a large sample of residential property transactions both within and outside California over the 2000–2017 time period. Zillow provides us a monthly house 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 the detailed information in ZTRAX to estimate the *Predicted FMV* of properties in our listing sample at the time of their listing; we do this using Hedonic regressions that relate selling price to property characteristics using recent transactions within the same zip code as the property that is listed for sale.

We use an instrumental variables (IV) specification, which exploits the institutional features of California's Proposition 13, to identify the causal effect of property taxes paid by the seller on his/her choice of listing price. 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. We estimate the IV regression on a sample of single-family homes that were purchased during the 1996-2007 period, because these are highly likely to be under Proposition 13 assessment at the time of their listing. We control the regression for the property's *Predicted FMV*, other house and neighborhood characteristics, and include *Zip*×*Listing Year-Month* fixed effects to control for unobserved heterogeneity across zip codes and year-month 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, which use a market-value-based system of assessment, to verify 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 Thaler (1980)). Moreover, Genesove and Mayer (2001) show that sellers' choice of listing price is affected by nominal loss aversion relative to their purchase price. Therefore, we control for the estimated nominal loss that the seller expects to incur relative to his/her purchase price, which is defined as the maximum of zero and the amount by which the property's purchase price exceeds its *Predicted FMV* at the time of listing for sale. When we divide our sample into two groups based on whether the seller expects a nominal loss or gain relative to his/her purchase price, we find that the effect of sellers' property tax payment on listing price is present in both groups, but is significantly stronger in the group of properties where the seller expects to incur a nominal loss.

Apart from nominal loss relative to their purchase price, sellers' notion of loss may also be based on more recent price losses in their local markets, especially because the housing crash in California had a very severe adverse affect on property values in the residential market. Accordingly, when we divide the zip codes in California into two groups based on the extent of average house price loss they suffered during the 2006–14 period (i.e., from the peak of the housing bubble to the aftermath of the housing crash), we find that the effect of sellers' property tax payment on listing price is present in both groups, but is significantly stronger in zip codes which saw more severe average house price losses during this period.

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 and larger 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 (and large-sized) properties within a zip code relative to normal-valued (and normal-sized) properties. However, the effect of sellers' property tax payment on listing price does not vary significantly between zip codes with low transaction volumes and 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 anchors to assess property values, and do not adjust away sufficiently from the anchors (see Northcraft and Neale (1987) and Bokhari and Geltner (2011)). 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. We also find that higher property taxes are associated with longer days-on-the-market but this effect is not economically significant. An important caveat to these results is that our sample only includes listings that resulted in sale, 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 Thaler (1980), Arkes and Blumer (1985), Whyte (1986), and Thaler (1999)), 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., Arkes and Blumer (1985), Gourville and Soman (1998), Just and Wansink (2011), and Ho et al. (2017)), whereas other field experiments fail to find any link (e.g., Ashraf et al. (2010) and Cohen and Dupas (2010)). Some other recent studies show that sunk costs affect bidding behavior of agents in penny auctions (Augenblick (2016)) and likelihood of default by borrowers in the housing market (Agarwal et al. (2015)).

Our paper also contributes to literature on behavioral biases in the real estate market. In a seminal paper, Genesove and Mayer (2001) 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. Bokhari and Geltner (2011) extend the findings of Genesove and Mayer (2001) 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.

1 Theoretical and Institutional Background

1.1 Sunk-Cost Effect

Rational economic theory implies that only incremental costs and benefits should affect decisions, and historical costs should be irrelevant. However, behavioral economists have hypothesized that people find it hard to ignore sunk costs while making economic decisions, and term this phenomenon as the sunk-cost effect (Thaler (1980)). For example, in their seminal study, Arkes and Blumer (1985) 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. Similarly, Gourville and Soman (1998) find that monthly attendance at an athletic club peaked when the members paid their half-yearly installment and then declined with time.

As with other behavioral biases, there is no single theoretical model to explain the sunk-cost effect. Thaler (1999) uses two ingredients to argue why people find it hard to ignore sunk costs while making economic decisions: First, people use mental accounting to classify costs as losses. Second, they perceive outcomes in terms of the prospect theory of Kahneman and Tversky (1979), which is characterized by reference dependence and loss aversion. An alternative explanation is that the sunk-cost effect may be related to cognitive dissonance theory (Festinger

(1957)) which suggests that people may revalue an asset upward after they have incurred a sunk cost. Arkes and Blumer (1985) suggest another alternative explanation based upon peoples' motive to avoid appearing wasteful. Importantly, Thaler (1999) emphasizes the importance of salient sunk costs when he notes that "although sunk costs influence subsequent decisions, they do not linger indefinitely... People do ignore sunk costs eventually." Experimental evidence from Arkes and Blumer (1985) and Gourville and Soman (1998) demonstrates the gradual reduction over time in the relevance of prior expenditures.

We note that experimental studies have found mixed evidence regarding the effect of sunk costs on consumption decisions. While some studies find that sunk costs lead to apparently irrational decisions (e.g., Arkes and Blumer (1985); Gourville and Soman (1998); Just and Wansink (2011)), other field experiments fail to find evidence in favor of the sunk-cost effect: Ashraf et al. (2010) 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, Cohen and Dupas (2010) find no relation between the price that Kenyan consumers paid for insecticide-treated bed nets and their use of the nets.

The evidence in favor of the sunk-cost effect outside of experimental settings is quite sparse. Augenblick (2016) 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. Ho et al. (2017) 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, Agarwal et al. (2015) 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 effect.

1.2 Property Taxes as Sunk Costs

In the United States, property taxes paid by homeowners are used to finance local public services (e.g., schools) that all residents in the local community are entitled to. Property tax is assessed as a percentage rate (which is the same for all homeowners in the local community) of the property's assessment value, which is determined by the local county/city assessor each year. As we explain below in Section 1.4, California uses a property tax assessment system in which property assessment values may be very different from fair market values (FMVs). Therefore, in this setting, past property tax payments made by the sellers are sunk costs, that 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.

1.3 The Anchoring-and-Adjustment Heuristic

The anchoring-and-adjustment heuristic, which was first demonstrated by Tversky and Kahneman (1974), 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 (Beggs and Graddy (2009)), offer prices in mergers and acquisitions (Baker et al. (2012)), and credit spreads of borrowers (Dougal et al. (2015)). 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. Northcraft and Neale (1987) show that even real estate agents, who are likely to be more informed than the average buyer, are susceptible to such an anchoring bias. Bokhari and Geltner (2011) show that sophisticated buyers in the commercial real estate market are also subject to the anchoring bias.

Anchoring bias is crucial to the existence of sunk-cost effect in our setting. If buyers and real estate agents can accurately determine the FMV of properties and do not use listing prices as anchors, 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 will 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 (Oakland (1979)). A detailed overview of Proposition 13 is available on *BallotPedia* at https://ballotpedia.org/California_Proposition_13_(1978)#cite_note-time-2, and in Oakland (1979) 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 Taylor (2014). Figure 1, which we reproduce from Taylor (2014), provides an illustration of California's property tax system for a hypothetical home purchased in 1995.

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);[2] (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);[3] 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 3, 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 main 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. 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

[2] 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.

[3] 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.

percent per year cap imposed by Proposition 13.

The example in figure 3 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 the illustration in Figure 1 and Taylor (2014) 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 (Taylor (2014)).

2 Data, Sample Selection, and Key Variables

2.1 Data Sources

Our primary source of data is Zillow Inc. (www.zillow.com). We use Zillow to hand-collect data on listing prices and listing dates of single-family homes, as well as detailed information on 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 use the Zillow Transaction and Assessment Dataset (ZTRAX) to obtain detailed information on the transaction history and assessment history of single-family homes. ZTRAX is compiled from public records and contains information on properties across 3,000 U.S. counties. 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 going back to 1996. We use the property's address to match the listings data from Zillow with ZTRAX.

Another feature of Zillow is that it provides a home price index – the Zillow Home Value Index (ZHVI) – that is based on its own *estimates* of sale prices of all homes (called "*Zesti-*

mates").[4] 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).

We collect information on housing transaction volumes at the zip code level from www.redfin.com and census tract-level demographic characteristics from the 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. To partially mitigate the latter concern, we will control our regressions for the average demographic characteristics at the census-tract level.

2.2 Sample Construction

At any given point in time, Zillow allows users to view the listing information for a maximum of 520 most recently sold homes for each zip code. We used this feature in the April 2017 extract of Zillow to obtain information on recently sold single-family homes in California. After excluding observations with missing addresses and missing listing information, we match the Zillow listing price data with ZTRAX data. Next, we use the deed transfer information in ZTRAX to exclude property transactions involving intra-family transfers or gifts because such transactions may not occur at market prices.

We exclude properties that were remodeled before being listed for sale because it is difficult to predict the fair market value (FMV) of such properties based on recent comparable transactions (see next section for the definition of *Predicted FMV*, which is an important control variable in our regressions). For a similar reason, we exclude properties whose listing price is

[4]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 4 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.

more than 150% of their predicted FMV because these are likely to be upgraded properties. As is standard in the real estate literature, we also exclude properties owned for less than two years, because these are likely to be property flippers whose behavior may be different from that of regular sellers. 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, 6% were sold in early 2017, and the remaining 2% were sold in late 2014.

2.3 Key Variables

A seller's choice of listing price will depend on the estimated fair market value of her property at the time of listing, which we denote as *Predicted FMV*. In practice, sellers estimate the fair market value of their homes by examining recent transaction prices of similar homes. Accordingly, and following Genesove and Mayer (2001), we define *Predicted FMV* as the predicted selling price estimated from a Hedonic regression model which relates the selling price of recently-sold homes to underlying home characteristics. Specifically, to avoid look-ahead bias, we estimate the following regression on a rolling monthly basis using all single-family home transactions in California over the 12-month period preceding the month of listing of the property whose FMV we are trying to estimate:

$$\text{Log(Selling Price)}_{it} = \alpha + \boldsymbol{\beta X_{it}} + \mu_z + \epsilon_{it} \tag{1}$$

The property characteristics (X_{it}) we control for are: number of bathrooms and bedrooms, house and lot area, house age, and *Adjusted Purchase Price*, which is obtained by multiplying the property's purchase price with $\frac{HPI_{Sale}}{HPI_{Purchase}}$ (i.e., change in HPI in the property's zip code from the month of purchase to the month of sale). To control for a non-linear relationship between selling price and home characteristics, we also interact all property characteristics with dummy variables that identify the top four quintiles of *Adjusted Purchase Price* (with the lowest quintile being the omitted category). We also include zip code fixed effects (μ_z) to control for unobserved heterogeneity across zip codes.[5]

To assess the out-of-sample predictability of the Hedonic regression model, in Figure 2, we provide a scatter plot of the *Predicted FMV* versus the actual *Listing price* for the properties in our listing sample. The blue line in this figure denotes the corresponding OLS relationship (the slope coefficient is 0.85 and the R^2 is 0.94).

The 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. We show below that our results are very similar if we instead use *Listing Premium*, defined as the ratio of *Listing price* to *Predicted FMV*, as the dependent variable. 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. As we explain below, our results are unchanged if we scale the listing price and selling price by the property's *Predicted FMV*.

Our main independent variable of interest is *Effective tax rate*, which is obtained by dividing the dollar amount of property tax paid by the seller in the year before listing (*Property Tax Paid*) with the property's *Predicted FMV*. Note that instead of relying on only one year's property tax history, we could define an alternative measure, *Effective 5-year Tax Rate*, by scaling the cumulative property taxes paid by the seller in the five years before listing with the property's *Predicted FMV*. One could argue that *Effective 5-year Tax Rate* better reflects the salient sunk costs incurred by the seller. However, the pairwise correlation between *Effective tax rate* and *Effective 5-year Tax Rate* in our sample is 0.95, which is to be expected because California's Proposition 13 ensures that property assessment values do not fluctuate wildly from year to year. Hence, we use *Effective tax rate* as our independent variable of interest, because *Effective 5-year Tax Rate* requires information on five years of property taxes, which

[5] Our results are highly robust to the methodology we use for estimating the *Predicted FMV*. We obtain very similar results using the following alternative approaches to estimate *Predicted FMV*: fitting higher-order polynomials in terms of property characteristics; estimating a variant of regression (1) on our (much smaller) hand-collected listing sample where we can also control for school characteristics and other amenities; or simply using *Adjusted Purchase Price* as the *Predicted FMV*.

would lower our sample size by more than 10%.

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 1.

[Insert Table 1 here]

As expected, the distribution of property values is positively skewed, regardless of the metric of property value. For instance, the mean value of *Predicted FMV* is around $600,000, which is substantially larger than the median value of around $487,000. The same patterns apply to the listing price and selling 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.

The median homeowner in our sample has paid around $4,700 in property taxes in the year before listing, which amounts to 1% of the property's *Predicted FMV*. The standard deviation of *Effective tax rate* is 0.4%, 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.

2.5 Variation in Effective Tax Rate

Figure 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 number of listings. As can be seen, there is substantial within-zip variation in effective tax rates within California, which is due to the Proposition 13.

Figure 6 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

[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.

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.

3 Empirical Framework and Identification Strategy

3.1 Instrumental Variables Regression

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 FMV 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:[7]

$$\begin{aligned} \text{Effective tax rate}_{it} &= a + \boldsymbol{b X_{it}} + \boldsymbol{c} \times \boldsymbol{Z_{it}} + \mu_z \times \mu_t + \eta_{it} \\ Log(\text{Listing price}_{it}) &= \alpha + \boldsymbol{\beta X_{it}} + \gamma \times \widehat{\text{Effective tax}} \text{ rate}_{it} + \mu_z \times \mu_t + \epsilon_{it} \end{aligned} \tag{2}$$

We estimate the IV regression on a sample of homes that were purchased during the period 1996–2007. The instrument Z 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. As we showed in Figure 6, this is indeed true on average for homes purchased during the period 1996–2007, which were highly likely to be under Proposition 13

[7]We obtain qualitatively similar results if we use the ratio of listing price to *Adjusted Purchase Price* as the dependent variable in the second-stage regression.

assessment at the time of their listing.[8] We exclude properties purchased after 2007 from the IV regression because these properties are very 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.

In equation (2), 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: *Predicted FMV* to serve as a rough estimate of the property's fair market value at the time of its listing; loan-to-value at purchase because the listing behavior of sellers may be affected by mortgage debt (see Stein (1995));[9] 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 Year-Month* fixed effects ($\mu_z \times \mu_t$) to control for unobserved heterogeneity across local markets and listing year-months.

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.

[8]Properties purchased prior to 2007 are 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.

[9]We use loan-to-value at purchase because we do not have information on sellers' mortgage balance at the time of listing. We obtain qualitatively similar results if we replace loan-to-value at purchase with the ratio of initial loan balance to the property's *Predicted FMV*.

Relevance of the Instrument

As we showed in Figure 6, the instrument is relevant because of unique features of California's property tax system. Panel B of the Figure 6 clearly shows that years of ownership has a negative effect on effective tax rate only in California, but not in the other states. 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, we estimate the first-stage regression on our analysis 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) controls for all property characteristics and median demographic characteristics measured at the census-tract level (which is a much finer classification of the local market compared to the zipcode area), and includes *Zip*×*Listing Year-Month* fixed effects. In column (2), we replace *Zip*×*Listing Year-Month* fixed effects with *Census Tract*×*Listing Year-Month* fixed effects, and drop the median census-tract demographic characteristics as controls.

[Insert Table 2 here]

In both specifications, the coefficient on *Years of ownership* is negative and highly significant. Following Staiger and Stock (1997), 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 Stock et al. (2002)). As can be seen, the $F-$statistic in both 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*×*Listing Year-Month* fixed effects and *Census Tract*×*Listing Year-Month* fixed effects.

The main threat to our identification strategy is if years of ownership is correlated with some other property or seller characteristics that lead to a lower listing price; that is, if there is a *negative relation* between years of ownership and house prices for reasons unrelated to property taxes. For example, if long years of ownership is systematically associated with poor quality of maintenance and/or lack of remodeling, that could generate a spurious positive correlation between effective tax rate (which is negatively related to years of ownership) and listing price.

Although there is no in-sample statistical test to rule out this alternative story and validate our exclusion restriction, we perform "out-of-sample" tests to show that there is no support for this alternative explanation. We describe these tests in Table 3. The main idea behind these tests is that if there is a negative relation between years of ownership and house price for reasons unrelated to the property tax system, then we should detect this negative relationship outside of California's Proposition 13 system. We show that this is not the case.

[Insert Table 3 here]

In Panel A we examine the relation between selling price and years of ownership using a large sample of housing transactions in New York and Illinois. The sample comprises all single-family home transactions in these states since 2000 for which we are able to extract all our regression variables from ZTRAX. We pick New York and Illinois because these are large states with market-value based assessment systems, with good coverage in ZTRAX.[10] We use selling price instead of listing price as dependent variable in these regressions because ZTRAX does not provide listing prices, and Zillow does not maintain historical data on listing prices for us to hand-collect.

We control the regression for *Predicted FMV* and all other property characteristics from ZTRAX, and also include *Zip code*×*Selling Year-Month* fixed effects. The insignificant coefficients on *Years of ownership* in columns (1) and (2) indicate that there is no relationship between years of ownership and selling price in either New York or Illinois.

In Panel B we focus on a subsample of CA properties which were purchased during 2009–2012 and listed for sale during 2015–2017. As we showed in Figure 6, the effective tax rate in the year prior to listing does not vary with years of ownership in this subsample because houses purchased during this period were highly likely to have received downward reassessment under Proposition 8. The regressions in columns (1) and (2) are based on properties in our listing sample for which we have more detailed hand-collected information from Zillow, and can separately examine the effects on listing price (column (1)) and selling price (column (2)). The regression in column (3) is based on a larger sample of ZTRAX transactions for which we only have selling prices. As can be seen, the coefficients on *Years of ownership* are either statistically insignificant or positive but economically insignificant.

Overall, the results in Table 3 establish that there is no reason to expect a negative relation between years of ownership and house prices for reasons unrelated to property taxes.

[10] Texas is another large state with a market-value based assessment system. Unfortunately, however, ZTRAX does not provide transaction price data for most properties in Texas (presumably because Zillow couldn't find this information from public records in Texas). Hence, we decided to drop Texas from this analysis, and focus only on New York and Illinois. Florida is another large state with good coverage in ZTRAX. However, We exclude Florida because the property tax assessment system in Florida is similar to California's Proposition 13: assessment value is tied to acquisition price plus annual increases of at most 3 percent.

4 Effect of Property Taxes on House Prices

4.1 Effect of Property Taxes on Listing Prices

In Table 4 we present the results of regressions examining the effect of past property tax payments on the sellers' choice of initial listing price.

[Insert Table 4 here]

We present the results of a simple OLS specification in column (1) for comparison with the IV regression results. Column (2) presents the results of the second-stage regression of the IV regression model (2) with *Zip*$\times$*Listing Year-Month* fixed effects; the corresponding first-stage results are in column (1) of Table 2. The positive and significant coefficient on *Effective tax rate* indicates that sellers' past 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 Year-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 about a 4.8% increase in listing price.[11]

Column (3) presents the results of the IV regression specification in which we replace *Zip*$\times$*Listing Year-Month* fixed effects with *Census Tract*$\times$*Listing Year-Month* fixed effects, and drop the median census-tract demographic characteristics as controls; the corresponding first-stage results are in column (2) of Table 2. Because the census tract is a much smaller area than a zip code, this specification allows us to better control for unobserved heterogeneity across different neighborhoods. As can be seen, our result is not only robust to this alternative specification but the coefficient on *Effective tax rate* has a larger magnitude compared to column (2).

Genesove and Mayer (2001) show that the listing behavior of sellers is affected by nominal

[11] The standard deviation of *Effective tax rate* is 0.38%. Hence, the coefficient estimate of 12.723 translates into an increase in *Log(Listing price)* of about 4.8%.

loss aversion relative to their purchase price. Therefore, in column (4), we repeat the regression in column (3) after also controlling for *Log(Nominal Loss)*, where *Nominal Loss* denotes the loss that the seller expects to incur relative to her purchase price. We define *Nominal Loss* as the greater of zero and the amount by which the property's purchase price exceeds its current *Predicted FMV*. The coefficient on *Nominal loss* is positive but narrowly misses significance at the 10% level (the $p-$value is 0.12).[12] The coefficient on *Effective tax rate* continues to be positive and significant.

In column (5) we present the results of the second-stage IV regression with *Listing Premium* as the dependent variable instead of *Listing price*; the empirical specification and control variables are otherwise similar to that in column (2). As can be seen, the coefficient on *Effective tax rate* is positive and has a similar magnitude as in column (2).[13]

Variation in Sunk-Cost Effect by Expectations of Loss

In this section we attempt to delineate the effects of loss aversion and the sunk-cost effect in more detail, because the sunk-cost effect is partly attributed to loss aversion (Thaler (1980)), and the latter has been shown to affect the listing behavior of sellers in the housing market (Genesove and Mayer (2001)). We do this by dividing our listing sample into two groups based on the sellers' expectation of loss; the idea being that loss aversion should be a concern only in the group where the expectation of loss is high. We then estimate our regressions separately on the two groups to see how the sunk-cost effect varies with expectation of loss.

Our first classification relies on nominal loss relative to the seller's purchase price, which Genesove and Mayer (2001) highlight as an important determinant of listing behavior. Recall

[12]Given the skewed distribution of nominal loss, we test for the nonlinear effects of nominal loss on listing price in an unreported test that uses a quadratic specification in nominal loss. We find that the coefficient on the quadratic term is positive and significant, which suggests a convex relation between nominal loss and listing price.

[13]In an unreported test, we also verify that if we use *Log(Listing Premium)* as dependent variable, then the results will be almost identical to those in column (2). This is a mechanical effect which is to be expected because *Log(Listing Premium)* equals *Log(Listing price)* minus *Log(Predicted FMV)*. Hence, we obtain identical coefficients on *Effective tax rate* and other control variables as in column (2), except for the coefficient on *Log(Predicted FMV)* which is now lower by 1 compared to that in column (2).

that *Nominal Loss* is defined as the greater of zero and the amount by which the property's purchase price exceeds its current *Predicted FMV*. Along similar lines, we define *Nominal Gain* as the greater of zero and the amount by which the property's current *Predicted FMV* exceeds its purchase price. We then estimate the OLS regression specification separately for the groups of properties with positive *Nominal Loss* and positive *Nominal Gain*; these results are presented in columns (1) and (2), respectively, of Table 5. We include all the property-level controls in these regressions but suppress these coefficients to conserve space. It is clear that the coefficient on *Effective tax rate* is significant among both groups, but has a larger magnitude group with a positive *Nominal Loss*; the difference in these coefficients between columns (1) and (2) is statistically significant at the 1% level.

[Insert Table 5 here]

We are unable to estimate the IV regression on the group of properties with *Nominal Loss*>0 because most of these properties were purchased closer to the peak of the housing bubble, and hence, were highly likely to be under Proposition 8 assessment at the time of their listing.[14] As a result, our instrument lacks first-stage predictive power in this group, because the instrument is based on the assessment policy under Proposition 13.

Apart from nominal loss relative to their purchase price, sellers' notion of loss may also be based on more recent price losses in their local markets, especially because the housing crash in California had a very severe adverse affect on property values in the residential market. We note that the median value of change in HPI over the 2006–14 period across all zip codes in California is -29%; that is, the median zip code experienced a 29% drop in average house price over this period. Therefore, for this test, we classify all zip codes into two groups– "High Price Loss 2006–14" group and "Low Price Loss 2006–14" group– based on whether their change in HPI over the 2006–14 period was lower than or higher than, respectively, the median value across all zip codes. We then estimate our regressions separately over these two groups.

The results of the OLS regression are presented in columns (3) and (4), and the results of

[14] Indeed, around 85% of the properties in this group were purchased during the 2005–07 period.

the IV regression are presented in columns (5) and (6). We find a similar contrast in both the OLS and IV results: the coefficient on *Effective tax rate* is positive and significant in both groups, but has a larger magnitude in zipcodes that experienced higher price loss over the 2006–14 period (i.e., the "High Price Loss 2006–14" group in columns (3) and (5)). We have verified that the differences in coefficients on *Effective tax rate* between these two groups are statistically significant at the 1% level. We also note that the $F-$statistic of the first-stage regression is strong even for "High Price Loss 2006–14" group in column (5). This is because most of these properties were purchased several years before the onset of the housing crash, and hence, were under the Proposition 13 assessment system at the time of their listing despite suffering large decline in value over the 2006–14 period.[15]

Overall, the results in Table 5 support the idea that the sunk-cost effect is stronger when the seller is more worried about incurring a loss.

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 and larger 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 groups 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" group and "Normal Value" group, respectively. Similarly, we divide our sample of listings into two groups based on whether the house is in the

[15]Recall that a property qualifies for a downward reassessment under Proposition 8 only if its market value drops below its Proposition-13-implied assessment value. Properties purchased several years before the onset of the housing crash are less likely to have satisfied this requirement.

top quartile or the bottom three quartiles within its zip code in terms of either house area or number of bedrooms; we refer to these as "Large Size" and "Normal Size" homes, 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. We measure each zip code's market activity in terms of the total number of properties sold as a fraction of the population within the zip code between 2012 and 2015.[16] We then classify the zip codes in California into two groups based on whether their level of market activity is lower than ("Low Activity" group) or higher than ("High Activity" group) the median market activity across all zip codes.

We then estimate the IV regression model separately for the two groups under each of these classification methods. The results of our estimation are summarized in Table 6. 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 groups.

[Insert Table 6 here]

Panel A presents the comparison of regression results based on the price category split between the normal-value and high-value groups, Panel B presents the comparison for normal-size versus large-size properties, and Panel C presents the comparison based on the market activity split between the low-activity and high-activity markets. In panel A, we find that the coefficient on *Effective tax rate* is significantly stronger in the group that we associate with higher price uncertainty, that is, the high-value homes within a zip code. Along similar lines, in panel B, we find that the coefficient on *Effective tax rate* is significantly stronger for large-size homes relative to normal-size homes. On the other hand, the results in panel C indicate that the sunk-cost effect does not vary across zip codes based on the level of market

[16] 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 in the corresponding test in Panel C.

activity. Specifically, the coefficient on *Effective tax rate* is positive and significant for both the low-activity and high-activity subgroups, and the difference between these coefficients is not statistically significant (the $p-$value of the difference is 0.799).

Variation in Sunk-Cost Effect by Demographic Characteristics

One of the shortcomings of the Zillow data is that they do not provide any information on the sellers' age, income or wealth. Hence, we are unable to directly test how the sunk-cost effect varies based on these seller characteristics. However, we are able to measure demographic characteristics at the level of the census tract, which is a much smaller area than a zip code and contains 2,118 homes on average. In this section we examine how the sunk-cost effect varies based on demographic characteristics at the census-tract level.

In Panel D of Table 6, we present the comparison of regression results for the two groups 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 groups but is stronger for properties located in census tracts with higher median age. One potential explanation for this differential effect is that younger sellers may prefer a quicker sale by choosing a lower listing price because they are more likely to be moving for work-related reasons. We also note that there are fewer listings in the below-median age group, which may be because census tracts with younger populations have a higher fraction of renters.

In Panel E we present the comparison of regression results for the two groups 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 groups, but has a larger size in the low-income group, which may reflect the higher sensitivity of low-income sellers to sunk costs.

In Panel F we present the comparison of regression results for the two groups stratified based on whether the fraction of renters in the property's census tract us below or above that of the median across all census tracts in California. We find that the coefficient on *Effective tax rate* is significant in both groups, but the difference in coefficients across the two groups is not statistically significant.

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 (Northcraft and Neale (1987)). 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) with *Log(Selling Price)* as the dependent variable instead of *Log(Listing price)*. The results of our estimation are presented in Table 7. We include all the control variables from the baseline regression but suppress these coefficients to conserve space.

[Insert Table 7 here]

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* has almost the same magnitude as the corresponding coefficient in column (2) of Table 4, which suggests that 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 almost 4.8% 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, where *Days-on-market* denotes the number of days between the initial listing and sale of the property. Although we find a positive coefficient on *Effective tax rate*, we note that this effect is not economically significant: the coefficient in column (4) indicates that a one-standard deviation increase in *Effective tax rate* increases *Days-on-market* on market by 3.1%, which represents an economically insignificant increase of 2.88 days compared to the average days-on-market of 93 days. An important caveat to this result is that our sample only includes listings that resulted in a sale, because Zillow only preserves listing information for recently sold homes. That is, we do not have any information on listings that did not result in a sale and were withdrawn after spending several days on the market. Hence, our regressions will underestimate the true effect on days on market.[17] This caveat also means that we cannot estimate the effect of sellers' property tax payments on the probability of sale.

5 Robustness Test

The key identifying assumption behind the IV regression model (2) is that *Years of ownership* does not directly affect seller behavior at the time of listing other than through its effect on property taxes under the Proposition 13 assessment system. We believe that this is a reasonable assumption because we have shown in Table 3 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. Nonetheless, in this section we present an alternative test that does not rely on this identifying assumption.

The alternative test we present uses a regression discontinuity design framework. It relies on the fact that California has a June 1 cutoff for updating assessment values during the

[17] Yavas and Yang (1995) show that listing price has a positive effect on days on market. In an unreported test, we verify that this finding also holds in our sample. However, the coefficient on *Effective tax rate* in column (4) is not economically significant, which may be because the variation in listing price due to the exogenous variation in *Effective tax rate* caused by Proposition 13 is not large enough to materially affect the days on market.

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 property characteristics and unobserved heterogeneity at the *Zip code*×*Listing year*×*Purchase year* level. That is, this alternative test 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 to identify the effect of *Effective tax rate* on listing price and selling price.

We estimate the following regression discontinuity specification for different outcome variables Y:

$$\begin{aligned} Y_{it} = \alpha + \beta \times \textit{Purchased after June 1} + \sum_{p=1}^{2} \gamma_p \times (\textit{Week of Purchase})^p \\ + \boldsymbol{\Psi X_{it}} + \mu_z \times \mu_s \times \mu_t. \end{aligned} \tag{3}$$

In the equation above, subscripts z,s, and t denote the zip code, purchase year, and listing year, respectively. *Purchased after June 1* is a dummy variable that takes the value one if the property was purchased after June 1 in year s, and zero otherwise. *Week of purchase* identifies the calendar week during which the property was purchased in year s, and is the running variable in the regression discontinuity specification. We estimate a quadratic specification in

Week of purchase, but obtain very similar results with a linear specification. We control for *Predicted FMV* and other property characteristics included in previous tables, but suppress most of these coefficients to conserve space. We also include *Zip code* × *Purchase year* × *Listing year* fixed effects in the regression to control for unobserved differences across zip codes, purchase year and listing year combinations.

We estimate regression 3 over all the properties in our listing sample, regardless of the year of purchase. The results are presented in Table 8. The negative coefficient on *Purchased after June 1* in column (1) indicates that there is a discontinuity, albeit small, in the *Effective tax rate* based on whether the property was purchased before or after the June 1 cutoff in its year of purchase. As expected, properties purchased after the June 1 cutoff in their year of purchase have a slightly smaller effective tax rate in the year prior to their listing; the effect is small because it is driven by differences between properties purchased before and after the June 1 cutoff within the same combination of zip code, purchase year, and listing year.

The negative coefficients on *Purchased after June 1* in columns (2) and (3) indicate that there is a discontinuity in the *Listing price* and *Selling price*, respectively, based on whether the property was purchased before or after the June 1 cutoff in its year of purchase. Specifically, all else equal, properties purchased after the June 1 cutoff in their year of purchase are subsequently listed for sale at a lower price and also sell for a low price.

We note that the results in columns (2) and (3) cannot be explained by any salient differences in property characteristics based on the June 1 purchase cutoff, because we have controlled these regressions for a wide variety of property characteristics and also included *Zip code* × *Purchase year* × *Listing year* fixed effects. Nonetheless, in column (4), we present the regression results with *Purchase Price* as the dependent variable. The idea behind this test is that if the results in columns (2) and (3) are driven by some salient differences in property characteristics based on the June 1 purchase cutoff, we should detect this effect in the purchase price. The insignificant coefficient on *Purchased after June 1* in column (4) allays these concerns.

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, and for properties whose value is harder to assess (e.g., larger and high-value properties within a zip code for which it is harder to find comparable transactions). 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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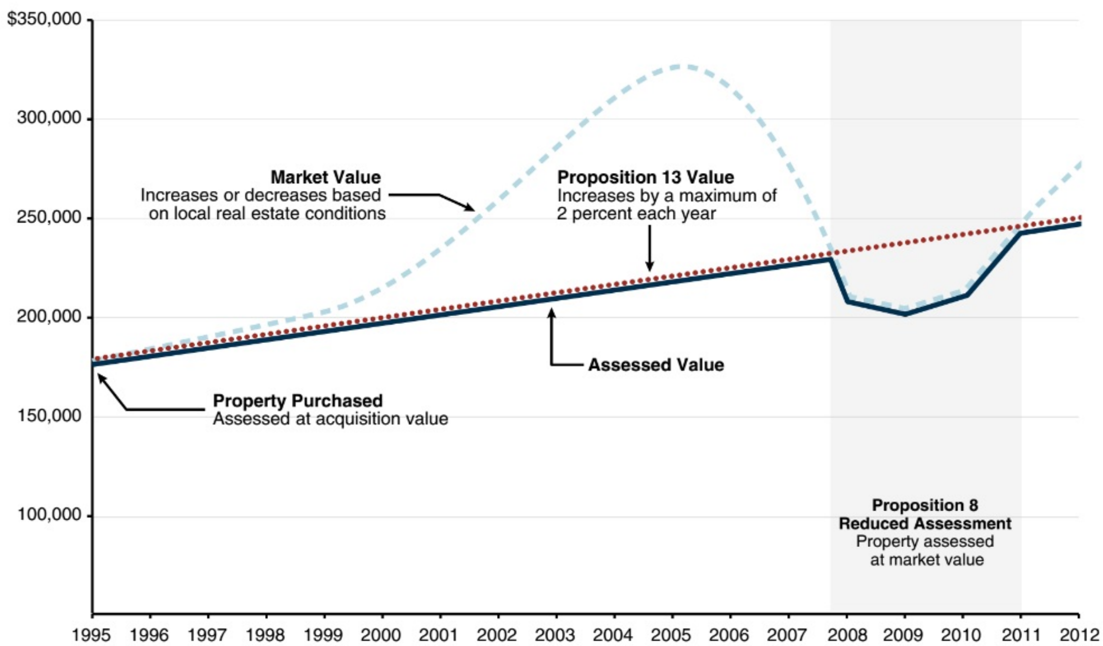
Figure 1: Illustration of California's Property Tax System

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 Taylor (2014)

Figure 2: Predictability of Hedonic Model

This figure plots the mean predicted FMV (y-axis) versus the binned actual listing price (x-axis) for the main analytic sample.

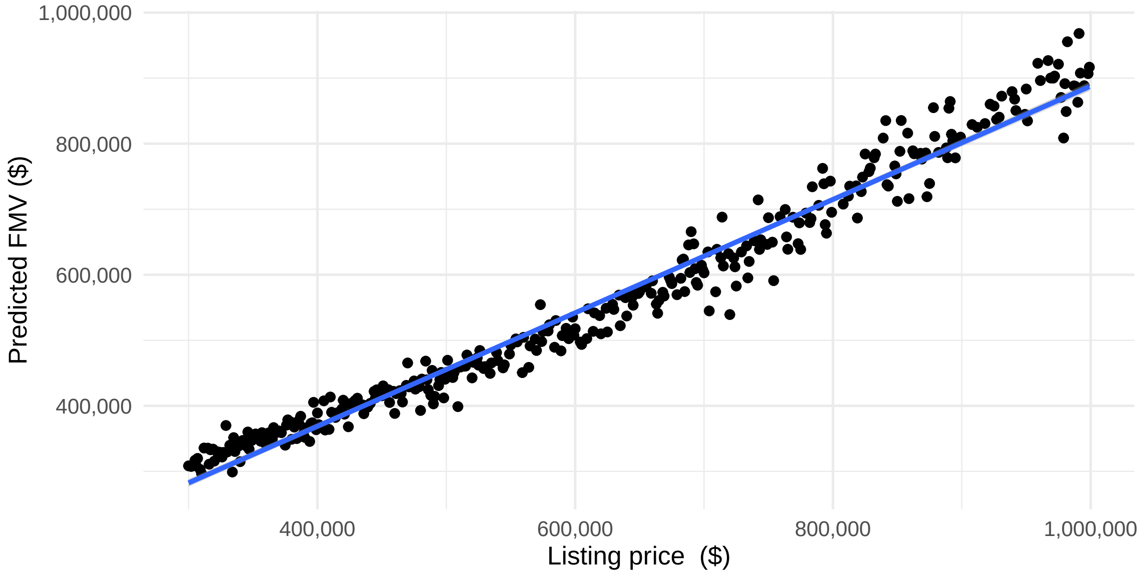


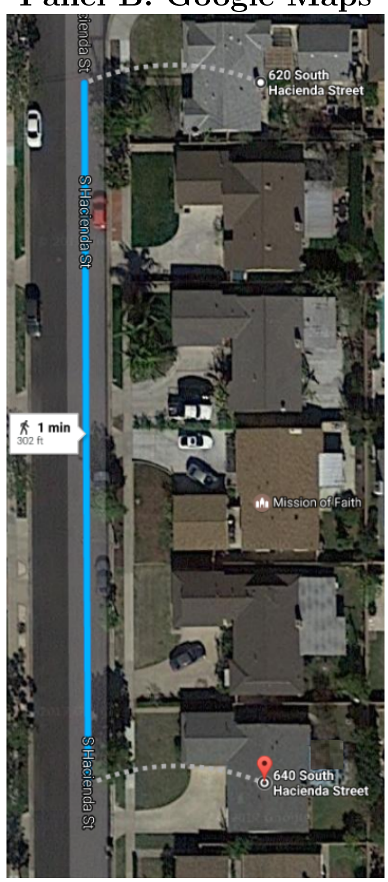
Figure 3: (Example) Differences in Property Tax Assessments of Similar Homes

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

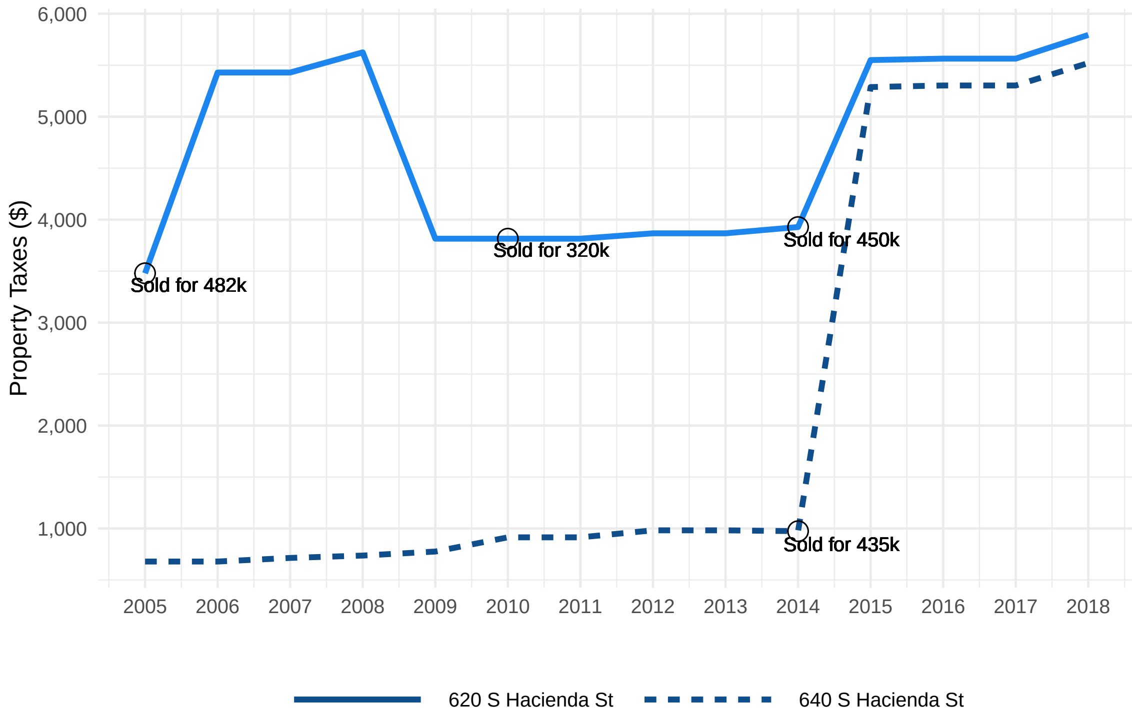


Figure 4: Mean House Price Index for California

This figure plots the All-Transactions House Price Index for California from FRED, Federal Reserve Bank of St. Louis.

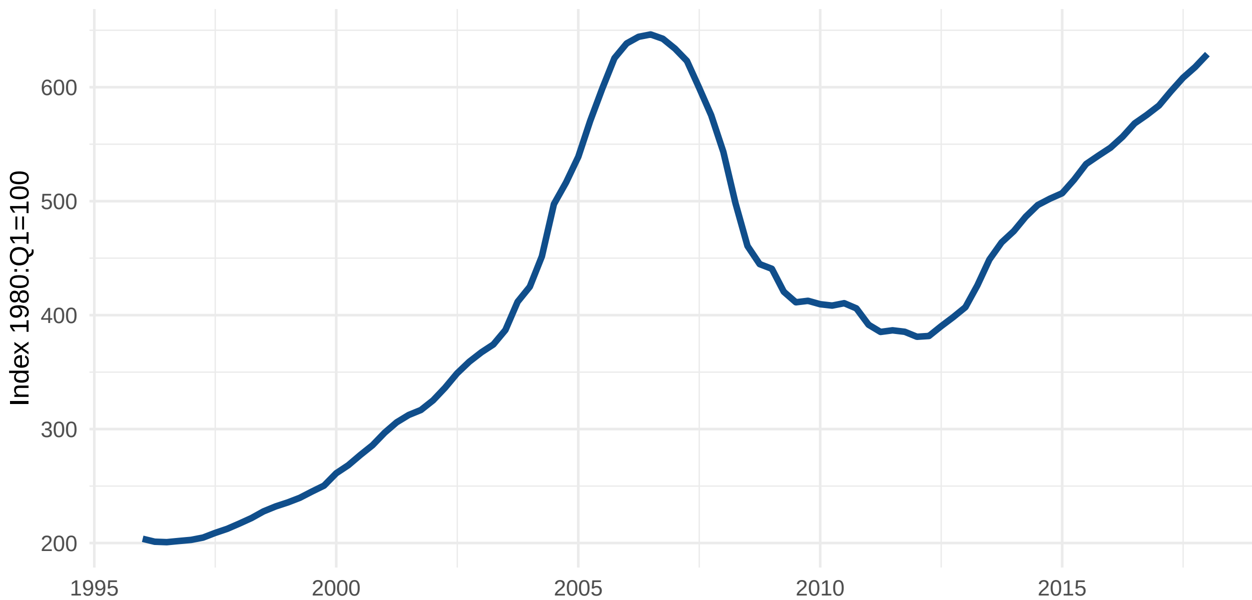


Figure 5: Within Zip Code Variation Effective Tax Rate

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.

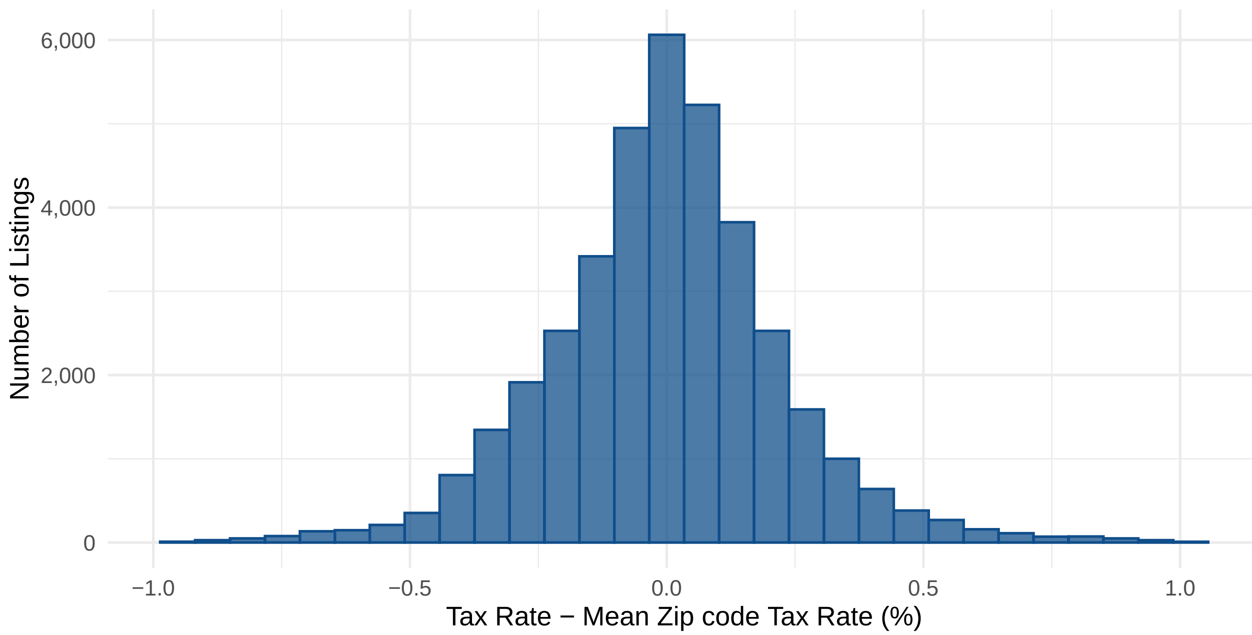
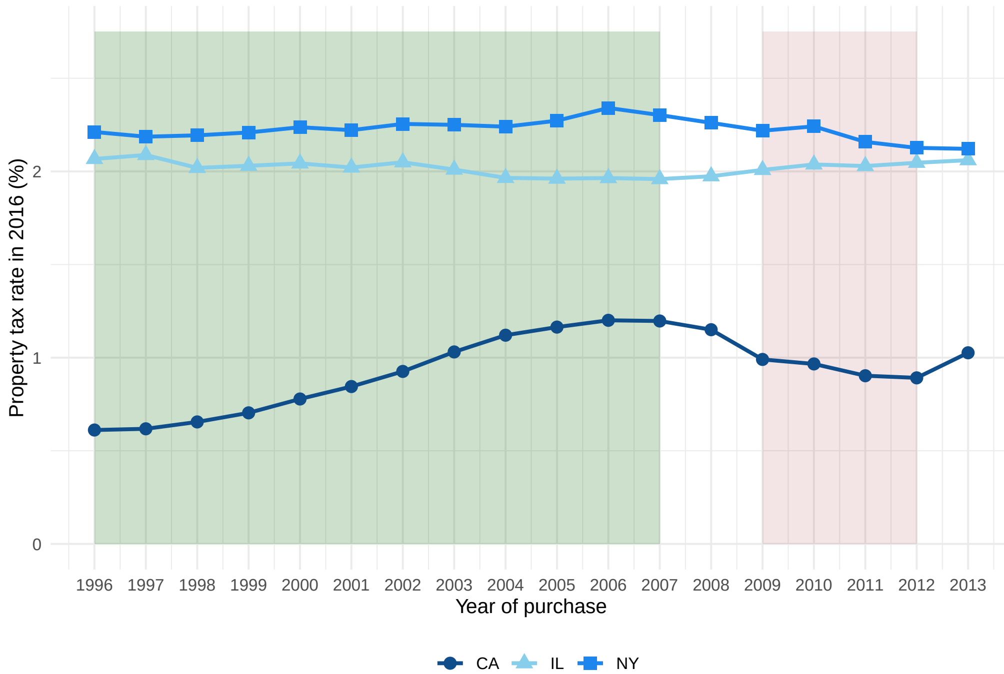


Figure 6: Effective Tax Rate and Years of Ownership

Panel A of 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. Panel B shows the relationship between years of ownership and property tax rate for the 1996 to 2007 period. See Appendix A for variable definitions.

Panel A



Panel B

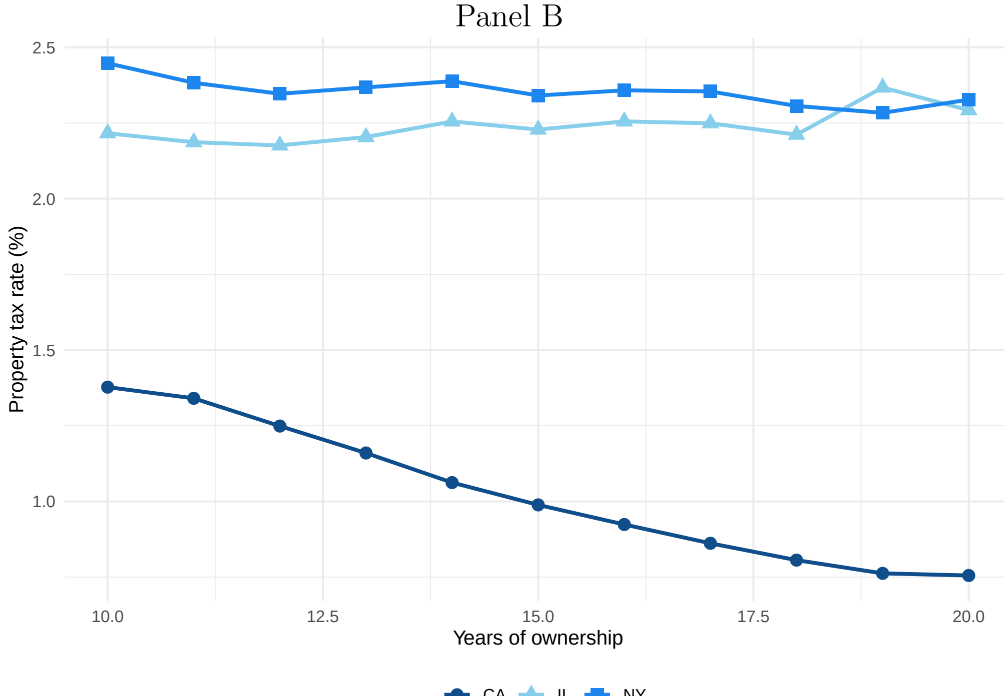


Table 1: Descriptive Statistics

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
Purchased price ($)	451,528	367,313	225,000	361,000	575,000	25,028
Predicted FMV ($)	599,513	431,524	346,011	487,213	715,323	25,028
Listing price ($)	655,924	470,873	386,083	550,838	779,113	25,028
Listing price/Predicted FMV	1.1343	0.5025	0.9304	1.0559	1.2267	25,028
Selling price/Predicted FMV	1.1009	0.4818	0.9079	1.0285	1.1876	25,028
Property taxes paid ($)	5,663	4,172	3,116	4,710	7,000	25,028
Effective tax rate	0.0104	0.0038	0.0081	0.0099	0.0119	25,028
$\text{HPI}_{Purchase}$	421,561	246,055	247,200	382,200	543,800	25,028
$\text{HPI}_{List}/HPI_{Purchase}$	1.4375	0.6145	0.9898	1.2438	1.7102	25,028
Years of ownership	12.57	2.7685	11	12	14	25,028
Number of bedrooms	3.42	0.86	3	3	4	25,028
Number of bathrooms	2.47	0.99	2	2	3	25,028
House area (sq. ft)	1,9812	823	1,406	1,805	2,382	25,028
Lot area (sq. ft)	17,373	113,152	5,662	7,200	10,018	25,028
Age of the house (years)	39	23	19	35	56	25,028
$\text{Loan-to-value}_{Purchase}$	0.6405	0.4103	0.3951	0.7842	0.8000	25,003
GreatSchools rating	6.8549	2.0249	5.3333	7.0000	8.6667	25,001
Distance to schools (miles)	1.4146	1.1362	0.8000	1.1333	1.6667	25,008
Census tract population	5,876	2,924	4,122	5,375	6,813	25,024
Census tract median age	40.0718	7.8140	34.6000	39.2000	44.4000	25,022
Census tract median income	81,058	31,008	57,961	76,185	99,632	25,021
Census traction fraction of renters	0.3228	0.1708	0.1923	0.2937	0.4298	25,022

Table 2: 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 var: Effective tax rate	
	(1)	(2)
Years of ownership	−0.054***	−0.052***
	(0.001)	(0.001)
log(Predicted FMV)	−0.725***	−0.798***
	(0.020)	(0.025)
Number of bedrooms	0.039***	0.033***
	(0.007)	(0.009)
Number of bathrooms	0.019*	0.043***
	(0.011)	(0.009)
log(House area)	0.290***	0.290***
	(0.034)	(0.049)
GreatSchools rating	0.013***	0.014*
	(0.004)	(0.009)
Distance to schools	0.004	0.002
	(0.004)	(0.010)
Distance to amenities	−0.0003*	−0.001**
	(0.0002)	(0.0003)
log(Age of the house)	−0.095***	−0.109***
	(0.009)	(0.014)
log(Lot area)	0.047***	0.050***
	(0.007)	(0.010)
Loan-to-value$_{Purchase}$	0.039***	0.031***
	(0.007)	(0.012)
Census tract median age	0.002***	
	(0.001)	
log(Census tract median income)	0.130***	
	(0.020)	
Census tract fraction of renters	0.104***	
	(0.035)	
Zip code × Listing Year-Month	Yes	No
Census tract × Listing Year-Month	No	Yes
F-statistic	460	269
N	23,962	23,962
Adjusted R^2	0.619	0.653

Table 3: 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. Panel A examines the direct impact of years of ownership on the selling price of homes in Illinois and New York, two states where property taxes are not dependent on the purchase price. The samples in Panel A uses all the homes sold from 2000 and 2016 for which we were able to identify the corresponding purchase transaction. Panel B uses the sample of homes in California that was purchased during 2009 to 2012 period where house price was fairly stable. The property taxes for homes purchased during this period in California are not correlated with the years of ownership. Columns (1) and (2) in Panel B use the main analytic sample, and the column (3) uses a sample based on the full set of transactions during 2009 and 2012.

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:

	IL	NY
	(1)	(2)
Years of ownership	−0.0002	−0.001
	(0.001)	(0.002)
log(Predicted FMV)	0.899***	1.036***
	(0.046)	(0.076)
Controls	Yes	Yes
Zip code × Selling Year-Month	Yes	Yes
N	127,214	263,949
Adjusted R^2	0.764	0.217

Panel B:

	log(Listing price)	log(Sales price)	
	(1)	(2)	(3)
Years of ownership	0.002*	0.0005	0.004**
	(0.001)	(0.003)	(0.002)
log(Predicted FMV)	0.488***	0.514***	0.453***
	(0.017)	(0.024)	(0.015)
Controls	Yes	Yes	Yes
Zip code × Listing/Selling Year-Month	Yes	Yes	Yes
N	14,436	14,436	56,409
Adjusted R^2	0.948	0.809	0.831

Table 4: 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 (5). 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 var:	log(Listing price)				Listing premium
	OLS	IV			
	(1)	(2)	(3)	(4)	(5)
Effective tax rate	18.784***	12.723***	13.354***	11.953***	12.388***
	(0.921)	(0.993)	(1.564)	(1.038)	(1.387)
log(Predicted FMV)	0.460***	0.410***	0.398***	0.402***	−0.880***
	(0.020)	(0.019)	(0.025)	(0.019)	(0.033)
Number of bedrooms	0.028***	0.031***	0.025***	0.031***	0.082**
	(0.007)	(0.007)	(0.008)	(0.007)	(0.040)
Number of bathrooms	0.023*	0.025*	0.047***	0.025*	0.056
	(0.012)	(0.013)	(0.008)	(0.013)	(0.037)
log(House area)	0.154***	0.173***	0.166***	0.177***	0.042
	(0.034)	(0.036)	(0.052)	(0.036)	(0.214)
GreatSchools rating	0.013***	0.013***	0.013**	0.013**	0.023***
	(0.003)	(0.003)	(0.006)	(0.003)	(0.007)
Distance to schools	−0.004	−0.003	−0.00001	−0.003	−0.002
	(0.003)	(0.003)	(0.006)	(0.003)	(0.005)
Distance to amenities	−0.0003***	−0.0003***	−0.001***	−0.0003***	−0.001*
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.001)
log(Age of the house)	−0.014**	−0.023***	−0.030***	−0.023***	−0.055**
	(0.007)	(0.007)	(0.010)	(0.007)	(0.027)
log(Lot area)	0.069***	0.072***	0.070***	0.072***	0.114***
	(0.007)	(0.007)	(0.010)	(0.007)	(0.018)
Loan-to-value$_{Purchase}$	0.001	0.002	−0.003	−0.002	0.002
	(0.004)	(0.004)	(0.006)	(0.004)	(0.016)
log(Nominal loss)				0.001	
				(0.0004)	
Census tract median age	0.003***	0.003***		0.003***	0.004***
	(0.0005)	(0.0005)		(0.0005)	(0.001)
log(Census tract median income)	0.082***	0.091***		0.093***	0.162***
	(0.012)	(0.012)		(0.012)	(0.040)
Census tract fraction of renters	0.069***	0.075***		0.076***	0.138***
	(0.021)	(0.021)		(0.021)	(0.035)
Zip code × Listing Year-Month	Yes	Yes	No	Yes	Yes
Census tract × Listing Year-Month	No	No	Yes	No	No
Cond. F. Stat		217.5	109.44	185.56	217.5
N	23,962	23,962	23,962	23,962	23,962
Adjusted R^2	0.941	0.940	0.948	0.940	0.395

Table 5: 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 sellers' expectation of loss. Columns (1) and (2) report the results of OLS regressions estimated separately for properties with positive nominal loss and positive nominal gain, respectively, where nominal loss (gain) is the amount by which *Predicted FMV* is lower than (exceeds) the property's purchase price. Columns (3) and (4) report the results of the OLS regressions estimated separately for zip codes classified into two groups based on whether their change in HPI over the 2006–14 period is lower than ("High Loss 2006–14" group) or higher than ("Low Loss 2006–14" group) the median value across all zip codes. Columns (5) and (6) report the results of the IV regressions (second-stage only) for the "High Loss 2006–14" group and "Low Loss 2006–14" group, 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.

	OLS				IV	
	Nominal Loss	Nominal Gain	High Loss 2006–14 zip codes	Low Loss 2006–14 zip codes	High Loss 2006–14 zip codes	Low Loss 2006–14 zip codes
	(1)	(2)	(3)	(4)	(5)	(6)
Effective tax rate	21.995***	16.140***	20.016***	18.234***	17.218***	11.024***
	(2.449)	(1.041)	(1.468)	(0.991)	(1.973)	(1.047)
log(Predicted FMV)	0.656***	0.385***	0.536***	0.422***	0.506***	0.369***
	(0.048)	(0.022)	(0.034)	(0.020)	(0.036)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Zip code × Listing Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat					11.27	218.78
N	5,078	18,884	9,459	16,094	9,459	16,094
Adjusted R^2	0.931	0.951	0.896	0.918	0.896	0.917

Table 6: 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* group) or the bottom three quartiles (*Normal value* group) of all listing prices within the zip code. In panel B we divide our sample of listings into two groups based on whether the house is in the top quartile (*Large size* group) or the bottom three quartiles (*Normal size* group) within its zip code in terms of either house area or number of bedrooms. In panel C we classify the zip codes in California into two groups based on whether their local market activity, which is measured using the total number of properties sold between 2012 and 2015, is higher than (*High activity* zip codes) or lower than (*Low activity* zip codes) the median level of market activity across all zip codes. In panel D we divide our sample into two groups based on whether the median age of the property's census tract is lower than (*Low age* group) or higher than (*High age* group) the median age across all census tract. In panel E we divide our sample into two groups based on whether the median household income of the property's census tract is less than (*Low income* group) or greater than (*High income* group) the median household income across all census tracts. In panel F we divide our sample into two groups based on whether the fraction of renters in the property's census tract is lower than (*Low fraction of renters*) or higher than (*High fraction of renters*) than the median value across all census tracts. 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), the 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 (within zip code)						
Normal value	7.946***	0.958	183.98	18,327	0.950	0.001
High value	20.783***	3.014	24.43	5,635	0.955	
Panel B: By property size (within zip code)						
Normal size	9.315***	1.112	148.39	15,539	0.944	0.001
Large size	18.964***	2.225	76.87	8,423	0.950	
Panel C: By market activity in the zip code						
Low activity	10.421***	1.249	127.28	10,789	0.926	0.799
High activity	11.475***	1.364	112.83	11,903	0.946	
Panel D: By median age in the census tract						
Low age	7.974***	1.591	59.11	9,351	0.941	0.008
High age	14.619***	1.294	146.68	14,611	0.942	
Panel E: By median income in the census tract						
Low income	16.098***	1.976	44.1	8,441	0.919	0.024
High income	11.322***	1.139	186.17	15,521	0.941	
Panel F: By fraction of renters in the census tract						
Low fraction of renters	12.180***	1.308	131.83	15,423	0.949	0.605
High fraction of renters	13.595***	1.685	69.58	8,539	0.941	

Table 7: 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. 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). We employ the full set of controls and include *Zip×Listing Year-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.

	log(Selling Price)		log(Days-on-market)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Effective tax rate	18.407***	12.545***	3.401*	8.090**
	(0.899)	(1.044)	(1.791)	(3.180)
log(Predicted FMV)	0.448***	0.399***	0.045*	0.084**
	(0.019)	(0.019)	(0.026)	(0.034)
Controls	Yes	Yes	Yes	Yes
Zip code × Listing Year-Month	Yes	Yes	Yes	Yes
Cond. F. Stat		217.5		217.5
N	23,962	23,962	23,962	23,962
Adjusted R^2	0.916	0.915	0.336	0.336

Table 8: 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 1996-2015 period 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.

	Effective tax rate (×100)	log(Listing price)	log(Selling price)	log(Purchased price)
	(1)	(2)	(3)	(4)
Purchased after June 01	-0.014^{***}	-0.009^{**}	-0.014^{*}	0.002
	(0.005)	(0.004)	(0.007)	(0.001)
Week of purchase	0.0004^{*}	-0.0001	0.0001	0.001^{***}
	(0.0002)	(0.0002)	(0.0003)	(0.0001)
Week of purchase^2	0.00000	0.00001^{**}	0.00001	0.00000^{**}
	(0.00001)	(0.00001)	(0.00001)	(0.00000)
log(Predicted FMV)	-0.485^{***}	0.273^{***}	0.294^{***}	0.992^{***}
	(0.010)	(0.012)	(0.009)	(0.001)
Controls	Yes	Yes	Yes	Yes
Zip code × Purchase year × Listing year	Yes	Yes	Yes	Yes
N	52,489	52,489	52,499	52,489
Adjusted R^2	0.608	0.938	0.798	0.995

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.
Predicted FMV	Predicted selling price estimated from a Hedonic regression model which relates the selling price of recently-sold homes to underlying home characteristics.
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 Predicted FMV.
Listing premium	$\frac{Listing\ price}{Predicted\ FMV}$
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	$Maximum(\text{Purchase price} - \text{Predicted FMV}, 0) + 1$

Appendix B: Description of Analytic Samples

B.1 Exclusion Restriction Analysis

B.1.1 New York and Illinois

Single-family home transactions in New York and Illinois since 2000 for which the regression variables are available. This sample is used to study the direct relationship between years of ownership and transaction prices. The states of New York and Illinois have market value based property tax systems, and years of ownership is not correlated with the property taxes. Therefore this regression captures the direct effect of the instrument on the outcome variable.

B.1.2 California 2009-2012

As a result of stable house prices during this period, the property taxes for the properties purchased during this period are not correlated with the years of ownership. We use the listings and transactions during this period to show that years of ownership is not directly correlated with the outcome variable. We have two sub samples for California: 1) properties purchased during 2009-2012 period that are in our listing sample from Zillow.com and 2) all properties purchased during 2009-2012 and subsequently sold. The second sample is limited to transactions for which we were able to obtain regression variables from ZTRAX.

B.2 Regression Discontinuity Analysis

This sample is used to generate Table 8. Unlike the main analytic sample, this sample is not restricted to properties purchased before 2008. This sample includes all the properties purchased during or after year 1996. The regression discontinuity design exploits the differences in property taxes due to June 01st administrative cutoff to reassess properties following a sale.