Flippers in flood-exposed housing markets:
Intermediaries, information, and the flood risk capitalization puzzle

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Abstract

A growing literature suggests that asset prices do not fully reflect climate risk. This paper looks to flippers, intermediaries who buy and sell properties in a short period of time, for evidence of a mechanism causing mispricing in flood-exposed housing markets in the United States. I find that flipped homes in the 100-year floodplain transact at prices approximately 18% higher relative to non-flipped homes in the same floodplain. Higher information frictions could be one reason that flipped homes do not capitalize flood risk as much as non-flipped homes. However, I do not find evidence that flood risk disclosure laws—which are meant to reduce information frictions—affect flipped homes' flood risk capitalization. Further study is needed to understand how flipping exacerbates flood risk mispricing and what policies might alleviate that mispricing.

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1 Introduction

A core idea in asset pricing theory is that an asset's price should reflect the known risks embodied in it (Romer, 2019). In an equilibrium with risk-averse investors, a riskier asset should sell for a lower price than a safer but otherwise identical asset.

Two strands of the empirical literature investigate whether this prediction is borne out for assets that are exposed to climate risk. The first strand documents that various markets, including stock markets (Bolton and Kacperczyk, 2021), municipal bond markets (Goldsmith-Pinkham et al., 2021), and residential housing markets (Bernstein et al., 2019), capitalize climate risk to some degree.

A second strand of the literature asks if climate risk capitalization is *efficient*. Rather than focusing on *whether* markets price climate risk, these studies ask if the *level* of pricing is correct given available information. In contrast to the standard theory, these studies generally find that markets do not fully price environmental risk. For example, Hong et al. (2019) find that predictably poor profit growth due to drought is not fully incorporated into publicly traded food companies' stock prices. In the context of flood-exposed housing markets, Hino and Burke (2021) find that housing markets in the Continental US underprice flood risk by $43.8 billion,[1] while Bakkensen and Barrage (2017) estimate that coastal properties in Rhode Island are overpriced by 13% due to unpriced sea level rise risk.

These figures are striking both in their magnitude and in what they imply for the distributional effects of climate change. If prices revert to their fundamentals, perhaps due to a flood or other extreme weather event,[2] the homeowners bearing the brunt of the correction will be people who are already disproportionately exposed to climate risk.[3]

[1] The figure cited is Hino and Burke (2021)'s central estimate using a 5% discount rate. The 95% confidence interval around this estimate is ($32.6 billion, $55.6 billion). The authors also report results for 7% and 3% discount rates.

[2] In contrast to the literature on flood risk capitalization, which is more mixed, there is a substantial and consistent literature finding that flood-exposed property values fall subsequent to a nearby flooding event, even when the properties themselves were not affected by the flood. See, for example, Gibson and Mullins (2020), Gallagher (2014), and Kousky (2010).

[3] It is worth noting the distinction between present-day flood risk and changing flood risk. For example, Hino and Burke (2021) focus on present-day flood risk valuation, while Bakkensen and Barrage (2017) and

While several studies have established that flood risk capitalization is modest at best, fewer have investigated the mechanisms causing the lack of capitalization. A common hypothesis is that information and beliefs play a key role.[4] Hino and Burke (2021) find that flood risk capitalization is higher in states with more flood risk-related disclosure laws, suggesting that information provision may help the market better capitalize flood risk.[5] Hino and Burke (2021) and Bernstein et al. (2019) both find that real estate purchased by more "sophisticated" buyers, such as commercial buyers and investors, exhibits a higher flood risk discount than homes bought by owner-occupiers. Bernstein et al. (2019), Bakkensen and Barrage (2017), and Baldauf et al. (2020) also find that flood risk from sea level rise is capitalized more in areas with higher levels of belief in climate change, suggesting that beliefs may also be important in explaining the flood risk capitalization puzzle, at least in the context of flood risk attributable to sea level rise and other climate change impacts.

This paper focuses on the impact of flippers on flood-exposed housing markets. Flippers are real estate investors who purchase homes with the intention of re-selling them after a short period. While there is no general convention for what time period constitutes a "flip," I will focus on homes which are bought and sold within a one-year period, and will investigate robustness to alternative flip definitions. I seek to understand whether flipped homes capitalize flood risk more or less relative to homes that are not flipped.

Flipping is an important phenomenon to study in the context of disaster-exposed housing markets because flippers may have two competing effects on the markets they act in. On one hand, flippers may add liquidity to the market, helping to facilitate matches between

Bernstein et al. (2019) study future sea level rise. Another source of changing flood risk comes from the expected increase in extreme precipitation events, tropical cyclones, and storm surge under climate change (Patricola and Wehner, 2018; Gori et al., 2022). To my knowledge, no research so far has investigated capitalization of flood risk due to an increase in the frequency of extreme events.

[4] Another possibility is that distortions caused by federal insurance subsidies cause mispricing in the housing market, although this hypothesis is less plausible given low take-up of flood insurance despite the subsidies (Wagner, 2021).

[5] More broadly, flood risk valuation is an important and relevant area to study, both in the present and in light of expected sea level rise due to climate change: approximately 3.8 million homes currently lie in areas with a 1% or greater annual chance of flooding, while 1.7 million US homes may be exposed to sea level rise of 6 feet (Hino and Burke, 2021; Bernstein et al., 2019).

buyers and sellers (Bayer et al., 2020; Leung and Tse, 2017). On the other, there is anecdotal evidence that some flippers may work to conceal flood risk by covering up past damage or failing to disclose flood risk information (Worland, 2021). If this is the case, buyers of flipped homes may have less information about the property's flood risk, possibly leading them to under-value flood risk.

It is thus not clear how flippers interact with the flood risk capitalization puzzle: they may help alleviate it by making the market function more efficiently, or they may exacerbate it by creating an information asymmetry. I shed light on this tension by estimating the net effect of flipping on flood risk capitalization.

I begin by documenting a basic stylized fact about flippers in flood-exposed markets: homes in the 100-year floodplain are significantly more likely to be flipped than homes outside of it. Using a cross-sectional identification strategy that leverages extremely high-dimensional fixed effects and controls to try to account for omitted variables, I then estimate flood risk valuation and how it is affected by flipping.

I first find that homes in the 100-year floodplain transact at prices about 2% lower relative to observably equivalent homes not in the floodplain. This estimate is statistically indistinguishable from 0, and is quantitatively very similar to Hino and Burke (2021)'s main results—a reassuring fact given that their identification strategy differs significantly from mine.

I then go on to estimate flood risk valuation for flipped homes relative to non-flipped homes. I find that flipped homes in the floodplain transact at prices about 18% higher relative to non-flipped homes in the same floodplain, a result that is statistically significant at the 5% level. This result is robust to controlling for remodeling and does not appear to be mitigated by disclosure laws meant to improve buyers' access to flood risk information. Taken together, my results suggest that flood-exposed flipped homes capitalize flood risk less than flood-exposed homes that are not flipped, but I have no evidence that the mechanism giving rise to this result is information.

2 Background on flipping

Flips comprise a tiny proportion of the total transactions in the US residential real estate market: in my data, which span two decades of transactions across the continental US, 0.3% of transactions are the home's second sale within two years or less. Despite this small proportion, flipping carries outsized importance both in its cultural relevance and its estimated impact on housing markets. From daytime TV shows to get-rich-quick schemes advertised on telephone poles and social media, speculative real estate investment is often seen as an easy and even enjoyable way to make money (Shiller, 2017).

More concretely, economic research has linked flipping activity to the mid-2000s housing boom and bust. As a motivating example, figure 1 plots the two-year flip rates between 2000 and 2015 for Clark County, Nevada, Washington, D.C., and the whole country. Clark County (the county containing Las Vegas) and Washington, D.C. were chosen because they were the areas of the country that experienced some of the largest housing price bubbles in the country beetween 2001 and 2007. Flipping rates in these cities exceeded nationwide flipping rates by a factor of up to 4 during this period.[6]

As shown by Piazzesi and Schneider (2009), one reason that flippers can have an outsized impact on real estate markets is that a small number of like-minded investors can have a a large effect on prices, even without buying a large share of the housing stock. If these investors are optimistic—for example, about future housing prices, or about the future impacts of climate change—their presence in the market can cause or exacerbate price bubbles. Indeed, in the context of the mid-2000s housing price boom, Chinco and Mayer (2016) and Gao et al. (2020) find evidence that real estate investors, including flippers, causally

[6] Note that flipping rates estimated using my data are very likely underestimates, because ZTRAX does not include every transaction in any county, and because my conservative data cleaning process (described in detail in section 3 and appendix B) likely discards some repeated sales. Other researchers using more comprehensive data on specific markets have found much higher flipping rates. For example, using data from Clark County tax records between 1994 and 2007, Depken et al. (2009) estimate two-year flips peaked just above 20% of all sales in 2004. Similarly, using data on the Los Angeles Metropolitan Area, a region that also experienced a large price bubble in the early 2000s, Bayer et al. (2020) estimate that two-year flips peaked at 25% in 2006. Using national data, a report by the financial services company CoreLogic found that two-year flips peaked nationally just above 10% in 2006 (McLaughlin, 2019).

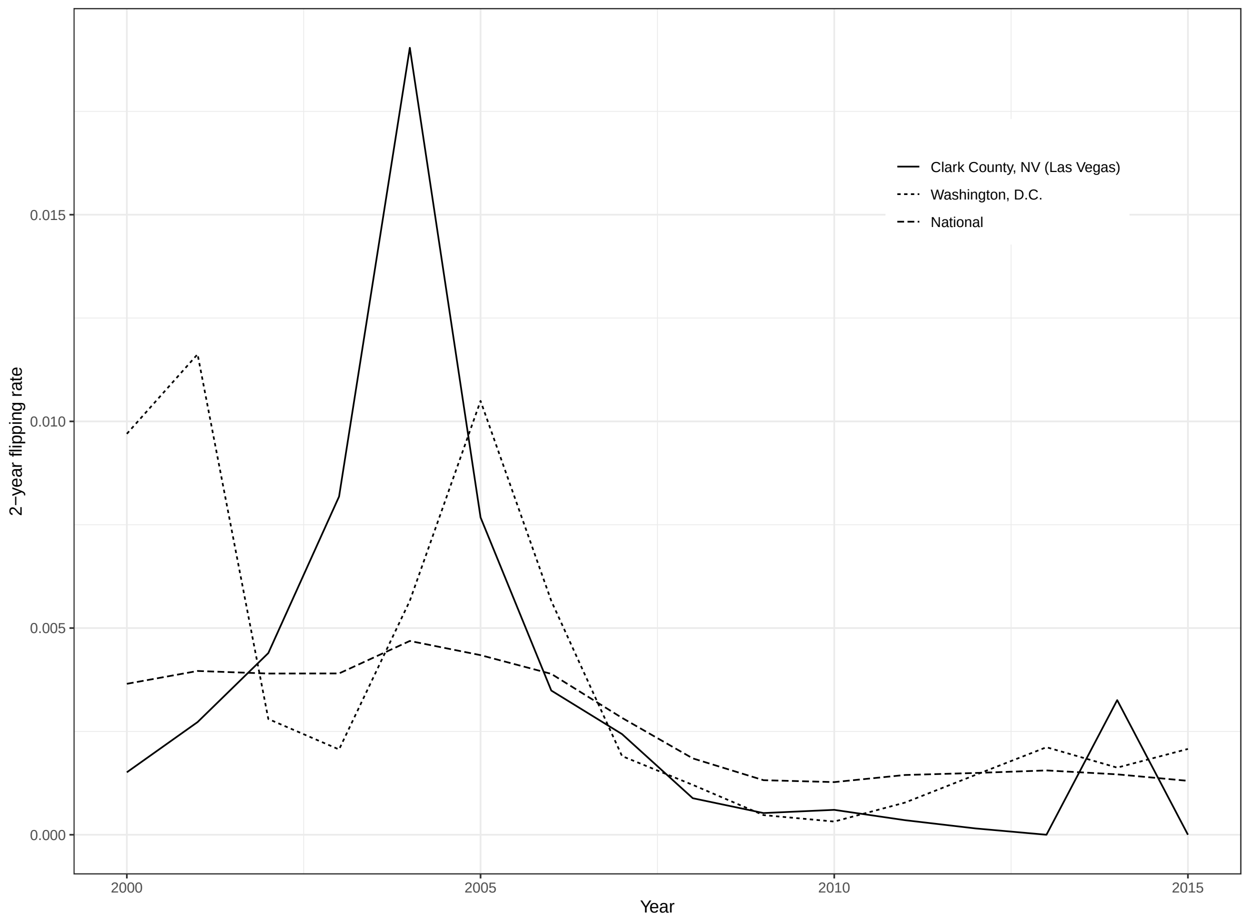


Figure 1: Two-year flip rates, 2000-2015.

increased prices.

In addition to their effect on prices through legitimate participation in the real estate market, there is concern that flippers may also inflate prices through illicit means. For example, flippers may collude with appraisers to obtain an artificially inflated appraisal on a flipped home, duping a buyer into paying too much. Federal, state, and local governments have implemented various policies to try to reduce illegal or predatory flipping. In 2003, the Federal Housing Administration, which provides mortgage insurance to around 10% of mortgages issued annually in the US, issued a ban on insuring mortgages on homes that had been sold in the past 90 days. In addition, it required additional appraisal on homes sold in the last 180 days. More recently, the states of Ohio and Illinois have effectively banned real estate wholesaling, a form of flipping in which an investor buys a contract to sell a home from a seller, and then sells that contract to a different buyer at a higher price. In practice, many wholesalers buy from distressed sellers at a discount, then reap large profits by selling at higher prices to other investors. In 2019 and 2020 respectively, Ohio and Illinois passed laws requiring wholesalers to be licensed real estate brokers. Real estate brokers in both states are required to advise sellers truthfully on their property's value, eliminating the wedge between the purchase and sale price wholesalers previously exploited. Finally, some jurisdictions are passing laws to prevent investors from aggressively soliciting homeowners, at least in part as a policy to prevent gentrification. Since September 2017, the State of New York has designated various neighborhoods in Queens and Brooklyn as "cease and desist zones," making it illegal for speculators to contact residents who add their names to a cease and desist list about selling their homes.[7]

Flippers and other real estate investors are also important players in disaster-exposed housing markets. Media reports abound with anecdotes of investors arriving shortly after disasters, looking for distressed homeowners and damaged homes to buy at a discount and promptly re-sell, sometimes without making any renovations to the properties (Brown, 2017; Campo-Flores, 2019; Putzier, 2019; Worland, 2021). Journalists and industry researchers have documented increased sales volumes in the wake of natural disasters (Putzier, 2019). Similar patterns appear in my data: table 1 demonstrates that homes in the 100-year floodplain as defined by the Federal Emergency Management Agency's Special Flood Hazard Area (SFHA) are up to 120% more likely to be flipped than homes which are not in the floodplain.

Flippers' presence in disaster-exposed real estate markets raises several interesting economic questions. On one hand, flippers may provide liquidity to disaster-exposed markets when they most need it. On the other, flippers may take advantage of distressed sellers, buying homes from them at below-market prices and re-selling them at a profit. If this is the case, flippers may represent a substantial transfer of wealth from disaster victims to investors—a transfer that raises important distributional issues, especially considering

[7] It may be possible to use one of these policies as a basis for estimating the causal effect of flipping on home prices and flood risk capitalization. I discuss this possibility and associated challenges in section 4.3.

	Dependent variable:		
	1-year flip probability		
	(1)	(2)	(3)
In SFHA	0.0013***	0.0005***	0.0008***
	(0.00004)	(0.0001)	(0.0001)
Zip code and year fixed effects		✓	✓
Size, age, and elevation controls			✓
Dependent variable mean	0.0011	0.0011	0.00063
Coefficient as fraction of dep. var. mean	1.2	0.45	1.2
Observations	25,247,295	25,245,058	13,595,781
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 1: Homes in the Special Flood Hazard Area are more likely to be flipped.

Notes: This table reports the results of regressions of an indicator for whether a home is flipped on an indicator for whether that home is in the Special Flood Hazard Area and controls. The home size control is a vector of indicator variables for the total number of rooms in the home, while the age control is a set of indicators for the decade in which the home was built. The home elevation controls consist of indicator variables for each percentile of the elevation distribution in my data. Heteroskedasticity-robust standard errors are shown in parentheses.

victims' vulnerability as a result of the disaster itself.

This paper focuses on another way in which flippers could affect disaster-exposed real estate markets: by exacerbating an information asymmetry between buyers and sellers. Past work on flood risk capitalization suggests that information frictions may be an important factor in explaining the low degree of flood risk capitalization in home prices (Hino and Burke, 2021; Bakkensen and Barrage, 2017). For example, a flipper might undertake cosmetic rennovations which make it more difficult for a potential buyer to observe evidence of past flood damage. Flippers may also be less likely to disclose flood risk—or may not even be fully aware of it themselves.

3 Data

3.1 Housing transactions

I obtain data on real estate transactions in the Continental US from Zillow's Transaction and Assessment Database (ZTRAX). ZTRAX includes information on homes' sale prices, as well as a rich set of observable characteristics such as homes' locations, age, number of bedrooms, lot size, owner-occupancy status, etc. ZTRAX has been widely used in the economics literature on the US residential real estate market. In addition to being used to study how real estate markets price environmental conditions (e.g. Bernstein et al. 2019; Moore et al. 2020; Murfin and Spiegel 2020; Wagner 2021), ZTRAX has been used in analyses of school choice (Zheng, 2022), homeowners associations (Clarke and Freedman, 2019), public goods (Albouy et al., 2020), and systematic risk (Peng and Zhang, 2021), among many other applications.

Because my analysis focuses on flipped homes, I must identify repeated sales in my data. While the presence of a unique parcel ID makes this task relatively straightforward, one challenge is that multiple records often appear for the same sale in the ZTRAX data. For example, one record might contain information on the transaction price, while a second record contains mortgage information, and a third includes home characteristics. To distinguish these from separate sales, I combine all records for a given parcel ID occuring on the same date. In cases where the transaction price is included in multiple records, I drop observations where the transaction price differs across records by more than \$1000. To further ensure data quality, I follow Bernstein et al. (2019) and keep only observations where the sale price is keyed directly by Zillow. This helps account for the fact that ZTRAX aggregates data from multiple sources, some of which may not report reliable price data.

An additional concern with the ZTRAX data is that the geographic coordinates provided by Zillow are often inaccurate (Nolte et al., 2021). Many of these appear to be Zip code centroids as opposed to actual address locations, leading to substantial differences between homes' actual locations and their reported locations in the ZTRAX data. These discrepancies are especially problematic in a context like mine, where I will leverage within-zip code variation to identify the effect of flood risk and flipping on home prices. To address this concern, I use a version of the ZTRAX data where locations have been geocoded directly from the addresses reported in ZTRAX.[8] As a final data quality control measure, I retain only arm's length transactions with strictly positive sale prices.[9]

One drawback of using ZTRAX to study flipping is that because ZTRAX does not allow me to observe every sale in the country, I am very likely underestimating the total number of flips in the market. As discussed in section 2, my estimates of aggregate flipping rates, such as those in figure 1, are an order of magnitude lower than other estimates in the literature that used different data. While this drawback does not affect the internal validity of my empirical strategy, it does affect my study's external validity to the extent that transactions are non-randomly included in ZTRAX, and means that I cannot use ZTRAX to study aggregated flip rates.

3.2 Flood risk

I merge the ZTRAX data with the National Flood Hazard Layer (NFHL), a map produced by FEMA that measures present-day flood risk for 90% of the U.S. surface area (Wagner, 2021). The NFHL includes maps of the Special Flood Hazard Area (SFHA), which is defined as the region that has a 1% annual chance of flooding, or the 100-year floodplain (FEMA, 2020). I obtain these data from Hino and Burke (2021)'s replication data, which were originally accessed from FEMA in January 2019. My analysis uses this 2019 cross-section of flood risk for the full sample, which spans 1996 to 2020. While doing so may introduce some measurement error, most flood maps updates are marginal, and are unlikely to differentially

[8] I am indebted to Maximilian Auffhammer and Wei Guo for allowing me to use their cleaned version of the ZTRAX data.

[9] "Arm's length" transactions are transactions between unrelated and unaffiliated parties, and thus are more likely to occur at market rate. For example, transactions between business partners or within families are not arm's length transactions and are dropped from my data.

affect flipped and unflipped homes. I thus do not attempt to reconstruct the full history of the NFHL in order to match it with my transactions in a time-varying way.

3.3 Elevation

I obtain elevation data from the US Geological Survey's 3D Elevation Program $\frac{1}{3}$ arc-second Digital Elevation Model, to use as a control in my main specifications. This model provides the elevation of each $\frac{1}{3}$ arc-second (approximately $10m^2$) grid cell in the continental US. I use the model to calculate the elevation of each property in my sample.

3.4 State-level disclosure laws

Finally, to investigate whether information provision increases flood risk capitalization, I obtain data on state-level flood risk disclosure laws from Hino and Burke (2021), which they compiled from National Association of Realtors (2019) and Nautral Resources Defense Council (2018). These data track three types of flood risk disclosure laws. The first type are laws requiring sellers to disclose whether the property is located in the floodplain. The second type are laws requiring disclosures about any past flood damage, such as drainage, leakage, water intrusion, and standing water. The third type are laws requiring disclosure about flood insurance, including whether it is required, its cost, and any recent claims. See Hino and Burke (2021) for further details.

3.5 Maps and summary statistics

Figure 2 shows the distribution of flood risk disclosure laws by state. While I have data on three types of disclosure laws, I will only use an indicator for whether a state has any law in my analysis to avoid multicollinearity between my fixed effects and dummies for the number of disclosure laws. I thus plot only whether a state has at least one law. Figure 2 demonstrates that there is substantial variation in laws across states that have large numbers of flood-exposed homes. For example, Florida and Louisiana have some of the most flood-

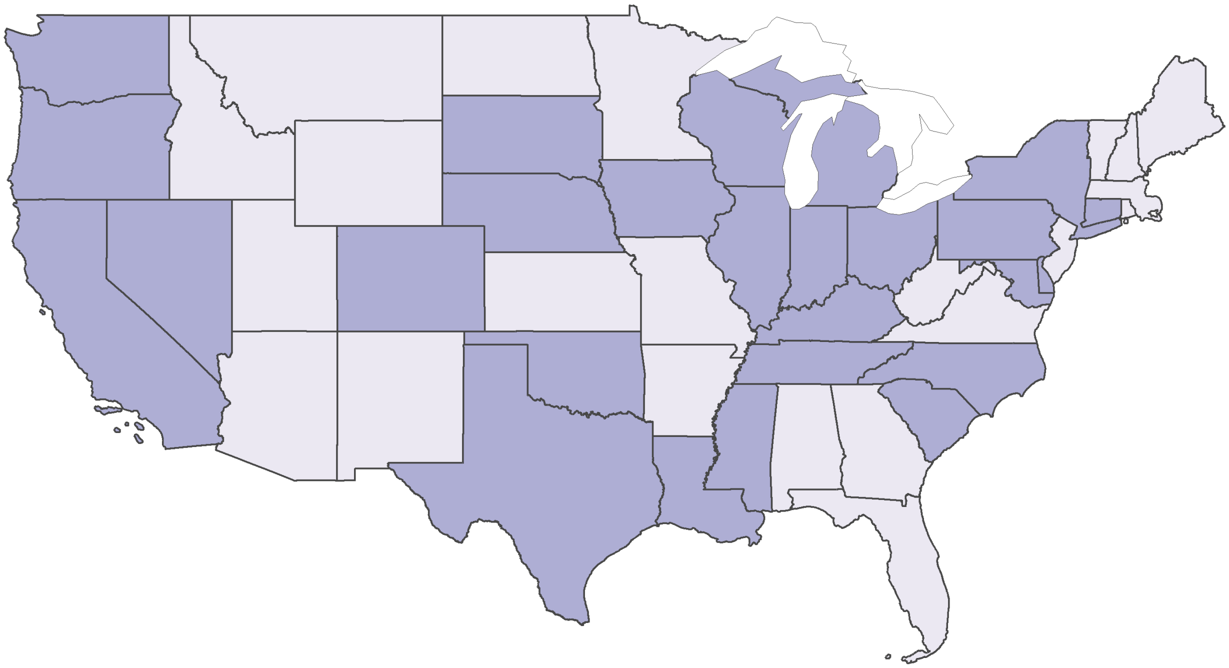


Figure 2: Flood risk disclosure laws by state

Notes: The map shows which states have at least one flood risk disclosure law. Types of laws include laws requiring disclosure of past flood damage, laws requiring disclosure of floodplain status, and laws requiring disclosure of flood insurance status, costs, and claims.

exposed real estate in the country (see figure 3), but Florida has no disclosure laws, while Louisiana does. Indeed, Louisiana has all three types of disclosure laws.

Table 2 displays the mean and standard deviations of the main variables used in the analysis. The table is broken down by relevant groups of the data: the first column shows statistics for the full sample, while the second, third, and fourth columns shows statistics for homes in the SFHA, flipped homes, and flipped homes in the SFHA, respectively. Table 2 highlights several interesting patterns in the data, as well as some of the challenges with using these data to analyze flood risk capitalization and property flipping.

Homes in the SFHA tend to sell for higher prices than homes not in the SFHA—this likely reflects the amenity value of being close to water, and foreshadows the importance of carefully controlling for such amenities to obtain a plausibly causal estimate of flood risk

Variable	Full Sample	Sales in SFHA	Flips	Flips in SFHA
Price (1000s)	254	295	211	308
	(2120)	(2750)	(820)	(917)
Total Rooms	3.95	2.06	2.27	0.912
	(3.75)	(3.29)	(4.36)	(2.66)
Year Built	1970	1980	1970	1990
	(47.7)	(43.6)	(71.6)	(26.1)
Elevation (m)	302	73.7	239	39.8
	(484)	(248)	(456)	(182)
Time since last sales (days)	3620	3540	176	140
	(2570)	(2140)	(116)	(119)
N	25,247,295	1,221,942	28,383	2,903

Table 2: Means and standard deviations of main variables in dataset.

Notes: Each row shows the mean of a variable with the variable's standard deviation below it in parentheses. The columns correspond to different subsets of the data which are relevant to this analysis. The first column shows statistics for the full sample. The second shows statistics for sales of homes in the Special Flood Hazard Area (SFHA). The third shows statistics for sales of homes that are bought and sold again within a one-year period. The fourth shows statistics for homes that are both in the SFHA and were bought and sold within a one-year period. Time between sales is calculated only for homes that are observed at least twice in the sample.

valuation. Flipped homes tend to be smaller than homes that are not flipped. This fits a story in which flippers tend to purchase and re-sell cheaper, lower-quality homes which likely also tend to be smaller. However, the fact that the mean number of rooms for flipped homes in the SFHA is below 1 is concerning, and suggests there is substantial misreporting in the ZTRAX data.[10] Mean elevation, even for homes in the SFHA, is well above sea level. This highlights the fact that flood risk due to coastal inundation is only one of many kinds of flooding that threaten US real estate. For homes that are observed at least twice in the dataset, the mean time between sales is nearly 10 years, with a large standard deviation of about 7 years. Indeed, the number of 1-year flips is only a tiny proportion of my data, and the number of 1-year flips of homes in the SFHA is even smaller. This foreshadows the challenges with attempting to use policy variation to identify the causal effect of interest,

[10] One approach to dealing with this is to use property square footage instead—however, this variable is not in the version of the data I am currently working with. Another approach is to control for bedrooms instead of total rooms, which might have fewer missing data issues. I intend to explore one or both of these possible avenues in the future.

which I discuss further in section 4.3.

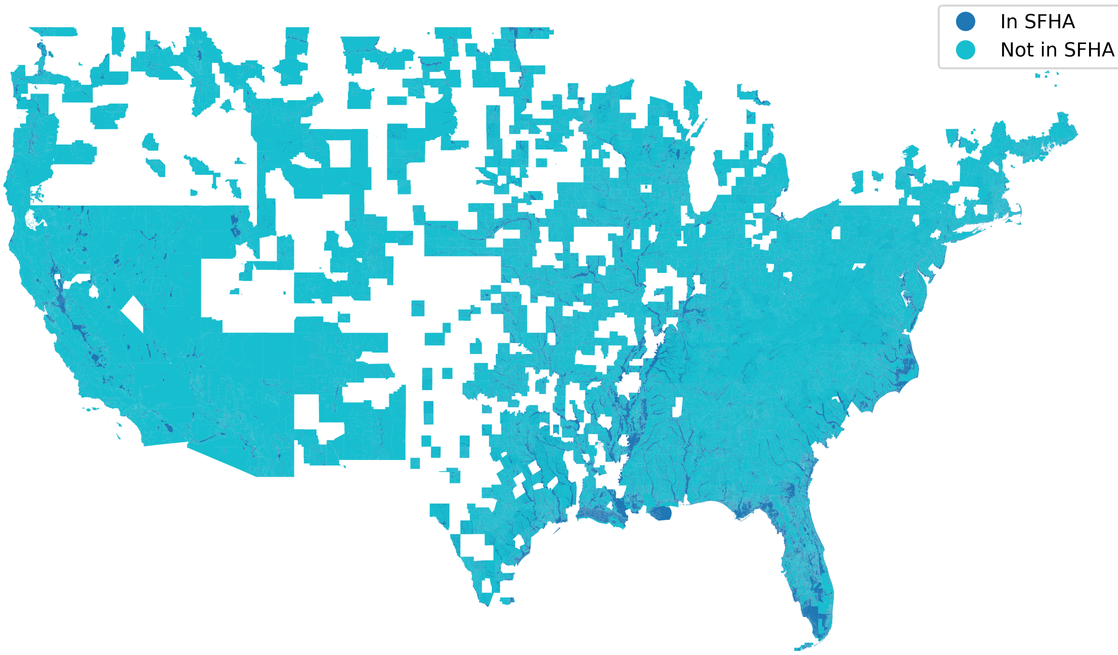
Figure 3 shows the spatial distribution of my flood risk and transaction data. Panel (a) maps the SFHA in the coterminus US. Areas in white are not mapped by the NFHL. Panel (a) highlights the fact that flood-threatened areas are not confined only to the coasts. Furthermore, while inland flood-exposed areas may not be as threatened by sea level rise, they are likely to flood more under climate change due to more intense precipitation events (Davenport et al., 2021). It is thus important to consider flood risk capitalization not just in coastal areas but nationally.

Panel (b) shows the spatial distribution of the number of transactions by zip code in the cleaned ZTRAX data. Zip codes outlined in white are zip codes where there are transactions both in the SFHA and outside of it; variation within these zip codes identify the parameters of interest in my empirical analysis. Areas in grey are zip codes where I observe no transactions. While ZTRAX covers most of the country, the coverage is extremely spatially uneven: the East Coast, Colorado, and parts of the Midwest and Southeast have an order of magnitude more transactions than the rest of the country. Zillow collects data for ZTRAX from where it is available, and does not attempt to produce a nationally comprehensive or representative dataset. This has implications for the external validity of my results: although my data are purportedly national, in truth the bulk of my observations lie in the eastern half of the country.

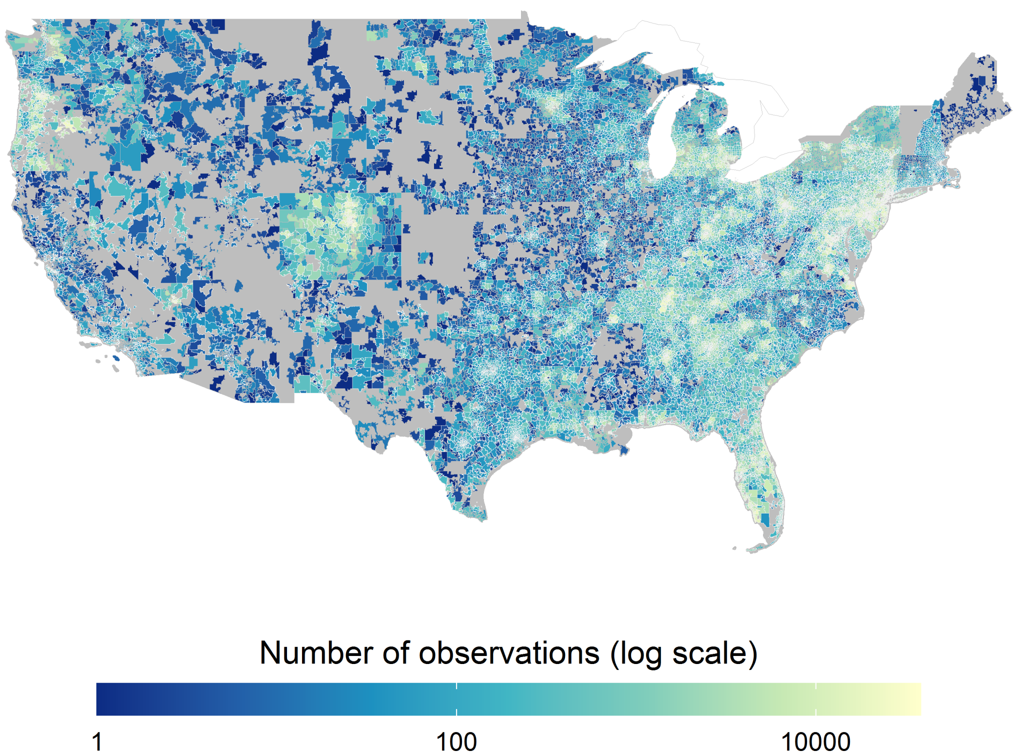
4 Empirical Strategy and Results

The causal effect of interest is the effect of flipping on flood risk capitalization. In the ideal experiment, homes' flip status and flood risk would both be assigned randomly, and thus estimating equation 1 via OLS would yield an unbiased estimate of τ, the causal effect of flipping on flood risk capitalization.

$$\log(\text{Price}_{it}) = \alpha + \beta\text{Risk}_i + \gamma\text{Flip}_{it} + \tau\text{Risk}_i \times \text{Flip}_{it} + \varepsilon_{it} \tag{1}$$



(a) Map of Special Flood Hazard Area (SFHA) in coterminus U.S.



(b) Map of observations by zip code.

Figure 3: Maps of flood hazard and transaction data.

These maps show the spatial coverage of my data. Panel (a) maps the SFHA in the coterminus U.S. Areas in dark blue are ares in the SFHA, while areas in light blue are not in the SFHA. Areas in white are not mapped by the NFHL. Panel (b) shows the number of transactions at the zip code level on a log scale. Zip codes outlined in white include homes both in the SFHA and outside of it. Variation within these zip codes is used to identify the main parameters of interest in the empirical analysis. Grey areas are zip codes with no observations in the cleaned ZTRAX data.

Of course, in the real world, neither flipping nor flood risk are randomly assigned, and both are correlated with unobserved factors entering ε_{it} in equation 1. For example, homes with a high level of flood risk may also have high amenities due to their proximity to water. Similarly, flipped homes may transact at higher prices if they are more likely to have been renovated more recently than non-flipped homes.

To address this issue, I must turn to an identification strategy that isolates good-as-random variation in flipping and flood risk. In an ideal case, I would leverage an exogenous policy shock and/or time series variation to estimate the causal effect of interest. Unfortunately, doing so has proved difficult in this setting. I briefly discuss some possible approaches and associated challenges in section 4.3.

In the absence of an identification strategy based on time series and/or policy variation, I follow Bernstein et al. (2019) and turn to a cross-sectional identification strategy. By using high-dimensional fixed effects and non-parametric controls, this approach attempts to account for every factor that might be correlated with flood risk or flipping and sale prices. As has been argued in the flood risk capitalization literature (Hino and Burke, 2021) and elsewhere (e.g., Dinardo and Pischke 1997; Angrist and Pischke 2009; Hsiang 2016), this approach is vulnerable to omitted variable bias. Nevertheless, the cross-sectional approach is common in the literature on housing prices, where authors have access to transaction-level data that includes rich covariates (see, for example, Bernstein et al. 2019; Stroebel 2016; Giglio et al. 2015; Bakkensen et al. 2022). Cross-sectional identification strategies also remain prevalent in the other areas, including in the literature on the economic impacts of climate change (e.g. Desmet et al. 2018; Costinot et al. 2016), and in particular in work showing that how housing prices capitalize local temperatures (Albouy et al., 2016). Using extremely rich fixed effects, as I do in this study, may improve the cross-sectional approach's prospects (Hsiang, 2016).

Figure 4 gives an example of the variation that remains after partialling out my preferred fixed effects, which I describe in detail below. In particular, the figure plots the locations

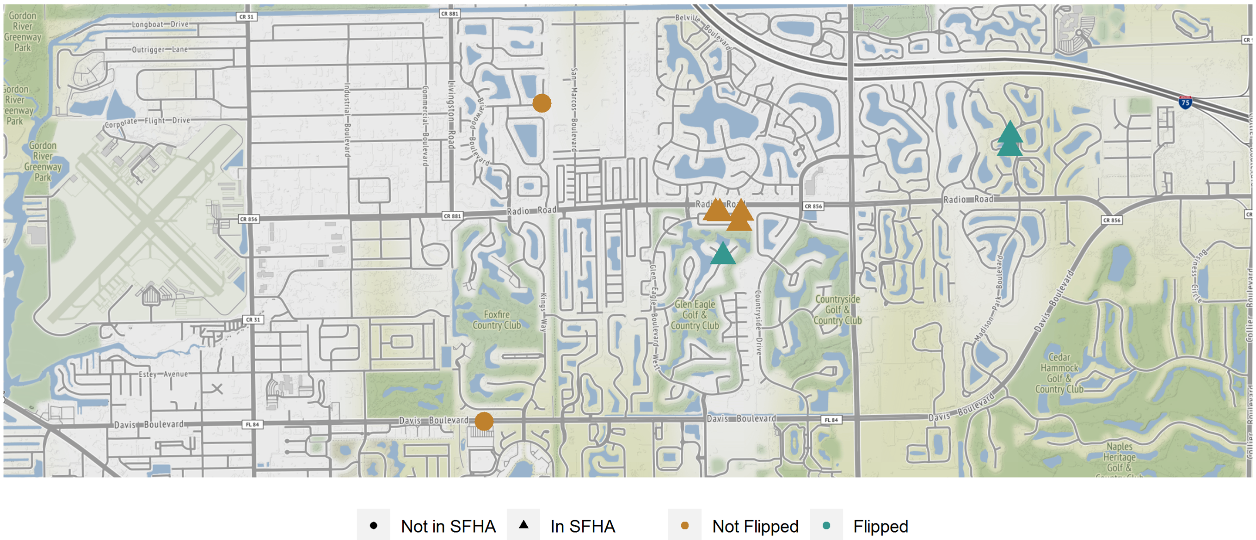


Figure 4: Example fixed effect group: Zip code 34104 (Naples, Florida), June 2003

Notes: This figure gives an example of the variation my fixed effects isolate. Specifically, the figure shows all of the observations for non-owner-occupied, non-condo transactions that occured in zip code 34104 in June 2003, with the homes' elevations falling in the 5th percentile of the elevation distribution, which ranges from 2.99 to 3.49 meters above sea level. This zip code is located in the eastern part of Naples, Florida, approximately 4 miles from the nearest coastline. Triangles indicate homes in the SFHA, while circles indicate homes not in the SFHA; brown points are flipped homes while teal ones are non-flipped homes. In my preferred specifications, I identify flood risk capitalization by comparing the sale prices of triangles to those of circles (see equation 3 and figure 5). I identify the difference in flood risk capitalization for flipped and non-flipped homes by comparing sale prices of teal triangles to sale prices of brown triangles (see equation 3 and table 3). All my preferred specifications also include non-parametric controls for the number of rooms in the home and the decade in which the home was built. See appendix figure A1 for additional examples showing different zip codes and time periods.

of transactions in zip code 34104 (a zip code in Eastern Naples, Florida) that were non-condo, non-owner occupied, fell within the 5th percentile of the elevation distribution (2.99 to 3.49 meters above sea level), and sold in June 2003. See the figure notes for details on the particular comparisons I make in my preferred specifications to measure flood risk capitalization and flood risk capitalization for flipped homes.

The figure highlights that even after partialling out extremely detailed fixed effects, there remains meaningful variation in my data. The figure also shows that there are many properties that are exposed to flooding despite being relatively far from the coastline and insulated from moderate sea level rise. In this case, the properties plotted are about 4 miles from the beach, and would thus are not included in analyses such as that of Bernstein et al.

(2019). See appendix figure A1 for additional examples of identifying variation in Fargo, North Dakota, New Orleans, Louisiana, Longmont, Colorado, and Eugene, Oregon.

4.1 Estimating flood risk capitalization

I begin by estimating flood risk capitalization. In the next section (4.2), I will interact the main specification presented in this section with an indicator for whether a property was flipped to estimate the impact of flipping on flood risk capitalization. As described above, my approach to causal inference amounts to including non-parametric controls and high-dimensional fixed effects to attempt to control for factors correlated with both flood risk and sale prices. In particular, my estimating equations in this section take the form

$$\log(\text{Price}_{it}) = \beta \text{Risk}_i + X_i \gamma + \mu_{g,t} + \varepsilon_{it}, \tag{2}$$

where Risk_i is a binary indicator for whether a home is located in the Special Flood Hazard Area (SFHA), X_i is a matrix of non-parametric controls, and $\mu_{g,t}$ are unit and time fixed effects. In my preferred specification, which I will carry forward to the analysis of flood risk capitalization in flipped homes in section 4.2, X_i includes indicator variables for the number of rooms in the home and for the decade the home was built, while the fixed effects $\mu_{g,t}$ include the interaction of zip code, elevation percentile, an indicator for whether property i is owner-occupied, an indicator for whether property i is a condo, month, and year.[11,12] That is, as illustrated by figure 4, my preferred specification compares properties in the same zip code, lying in the same elevation bin, of the same condo and owner-occupancy status, which sold in the same month and year, and further controls non-parametrically for property size and age.

Figure 5 plots the estimated coefficient $\hat{\beta}$ from equation 2 for specifications with increasingly stringent fixed effects. Controlling for elevation (columns (3) through (6)) stabilizes the

[11] See appendix B.1 for further details on how variables used in the regressions were constructed.

[12] A further control that would be desirable to include is distance to the nearest body of water (river, lake, ocean, etc.). I hope to be able to control for this in a future version of this paper.

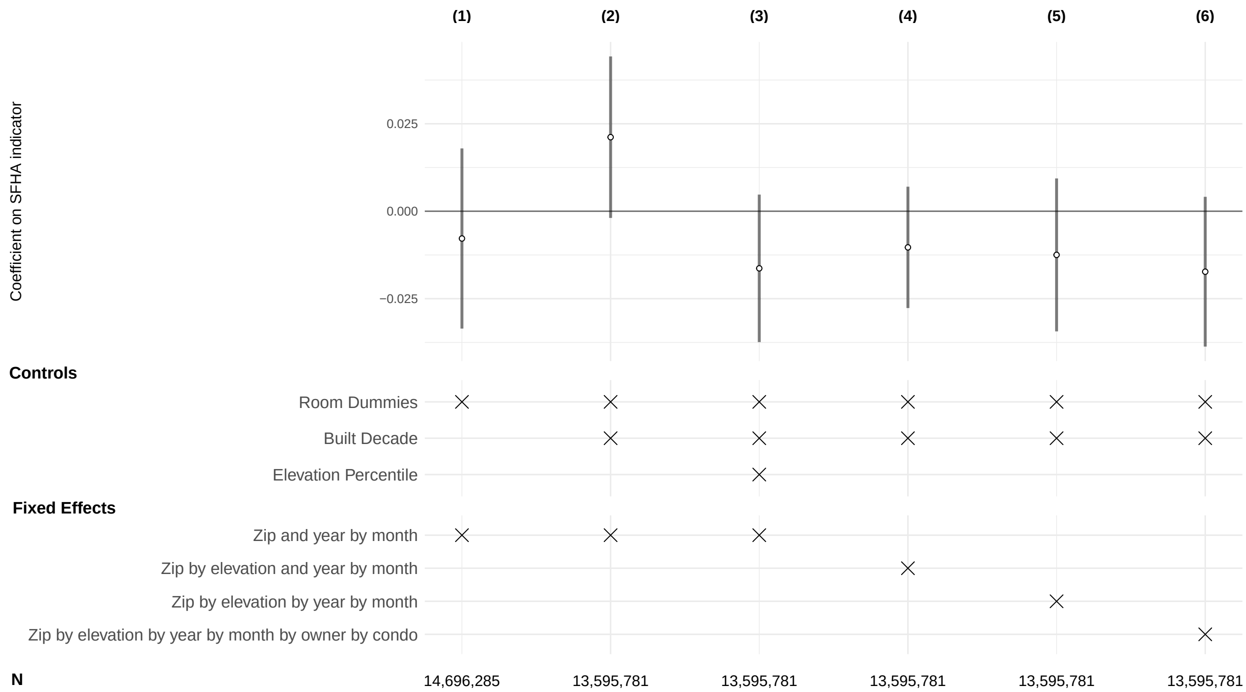


Figure 5: Specification chart for regressions estimating flood risk capitalization

Notes: Plotted are the estimated coefficients $\hat{\beta}$ and associated 95% confidence intervals for versions of equation 2 with various fixed effects and controls. All standard errors clustered at the zip code level. Regressions are ordered from the least stringent control strategy on the left to the most stringent on the right. For a tabular version of these results, see Appendix Table A1.

coefficient. Adding further controls yields almost identical results to the regression in column (3), which includes controls for number of rooms, building age, and elevation percentile, as well as zip code and year-by-month fixed effects.

The results presented in figure 5 are quantitatively similar to other estimates in the literature. The most comparable estimate is that of Hino and Burke (2021), who also use national data to estimate flood risk capitalization in residential home prices. In their preferred specification, they also estimate a negative but noisy point estimate of -2.1% a 95% confidence interval ranging from -4.2% to 0.1%. This is almost identical to the result from my preferred specification (column (6)), which yields a point estimate -1.7% with a 95% confidence interval ranging from -3.9% to 0.05%. Hino and Burke use a panel approach, where identification comes from repeated sales of properties whose flood zone status changes over time due to the re-drawing of NFHL maps. Because they are able to include property fixed effects, Hino and Burke's estimates are arguably less likely to be contaminated by omitted variable bias. Hino and Burke also report the results of a cross-sectional regression which yielded a comparable estimate to the one reported in column (2). The key difference between Hino and Burke's cross-sectional approach and mine is that they do not control for elevation and include individual fixed effects for various controls instead of interacting the fixed effects. The similarity between Hino and Burke's results and mine is reassuring in that two completely different identification strategies arrive at nearly identical results. In addition, the fact that the main difference between Hino and Burke's cross-sectional result and mine is their lack of an elevation control further suggests that accounting for elevation is a key step toward obtaining plausibly causal estimates in a cross-sectional setting.[13]

4.2 Estimating flood risk capitalization for flipped homes

I have replicated the result from the literature that flood risk is only modestly capitalized, if at all, in home prices. I now turn to my main results, which investigate whether flipping affects flood risk capitalization. To shed light on this question, I augment equation 2 with an interaction for whether a home was flipped:

$$\log(\text{Price}_{it}) = \beta \text{Risk}_i + \delta \text{Flip}_{it} + \tau \text{Risk}_i \times \text{Flip}_{it} + X_i \gamma + \mu_{g,t} + \varepsilon_{it}. \tag{3}$$

Columns 1 and 2 of table 3 report results from running equation 3. The first column simply carries forward my preferred specification from the previous section, the results of which are reported in column 6 of figure 5. This specification includes zip code by elevation by year by month by owner-occupancy status by condo fixed effects and non-parametric controls for the number of rooms and the decade the home was built in. The second column adds a control for whether the home was remodeled in between sales. Both specifications yield

[13] My results are also qualitatively similar to those of Bernstein et al. (2019), although theirs are both more negative and statistically distinguishable from zero. I do not give a detailed account of the similarities and differences between our results because, despite our similar methods, Bernstein et al. focus on capitalization of future sea-level rise rather than present-day flood risk and use a sample that includes only coastal states.

nearly-identical results: the estimated value of τ from equation 3 is positive and statistically significant at conventional levels. Conditional on being in the SFHA, flipped homes transact for about 17% more than homes in the SFHA which are not flipped. This result is consistent with the hypothesis that flipped homes do not capitalize flood risk as much as non-flipped homes do.

I investigate my results' robustness to alternative flipping definitions and fixed effect specifications in appendix figures A2 and A3. Appendix figure A2 investigates robustness to alternative flipping definitions. While the estimates of the coefficient on the interaction between flipping and flood risk are never statistically different from one another, the point estimate using a 2-year definition of flipping is very close to 0 and not statistically significant. On the other hand, the estimates using 1-year, 6-month, and 90-day definitions of flipping are more qualitatively similar to one another, with positive point estimates which are statistically different from 0 (although the coefficient on the 90-day definition is significant only at the 10% level).

One interpretation of these results is that there is heterogeneity in flood risk capitalization across different flipping windows: homes that are flipped in a shorter period of time exhibit less capitalization than homes flipped over a longer period. There are several plausible mechanisms for this heterogeneity. For example, investors who flip homes over longer periods may be systematically different from investors flipping homes over shorter periods. Perhaps longer-term flippers are more likely to make major renovations to the home, while shorter-term flippers might be more likely to make a few cosmetic changes before re-selling. Similarly, shorter-term investors might be more likely to attempt to conceal flood risk or deceive potential buyers in order to make a quick sale. However, it is important to emphasize that such an interpretation is speculative and cautious; the difference across flip definitions may simply be due to sampling variation and is not statistically significant.

Appendix figure A3 investigates robustness to alternative fixed effect specifications. Perhaps surprisingly, the only specifications that yield positive and statistically significant es-

	Dependent variable:		
	log(Price)	log(Price)	log(Price)
	(1)	(2)	(3)
Flood risk	−0.017	−0.017	−0.003
	(0.011)	(0.011)	(0.016)
Flip (1-year)	−0.344***	−0.345***	−0.331***
	(0.019)	(0.019)	(0.030)
Flood risk x Flip	0.175***	0.174**	0.183**
	(0.068)	(0.068)	(0.076)
Remodeled		0.059***	0.059***
		(0.007)	(0.007)
Flood risk x Any disclosure law			−0.039*
			(0.020)
Flip x Any disclosure law			−0.027
			(0.037)
Flood risk x Flip x Any disclosure law			−0.138
			(0.146)
Observations	13,595,781	13,595,781	13,595,781
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3: Flood risk capitalization including an interaction between risk and flipping

Notes: All regressions include zip code by elevation by year by month by owner-occupancy status by condo fixed effects and non-parametric controls for the number of rooms and the decade the home was built in. Standard errors clustered at zip code level.

timates are the specifications with the most stringent fixed effects. Given that these fixed effects are arguably necessary for causal inference, one interpretation of this result is that the estimated coefficient is biased in the other specifications.

To investigate how information about flood risk affects flood risk capitalization for flipped homes, I further augment my main specification with an indicator for whether the state's home has any flood risk disclosure law:

$$\log(\text{Price}_{it}) = \beta_1 \text{Risk}_i + \beta_2 \text{Flip}_{it} + \beta_3 \text{Risk}_i \times \text{Law}_i + \beta_4 \text{Flip}_{it} \times \text{Law}_i + \\ \tau_1 \text{Risk}_i \times \text{Flip}_{it} + \tau_2 \text{Risk}_i \times \text{Flip}_{it} \times \text{Law}_i + X_i\gamma + \mu_{g,t} + \varepsilon_{it}. \quad (4)$$

Note that the disclosure law indicator is not included by itself because it is collinear with the fixed effects. Furthermore, while I have data on how many and what types of disclosure laws exist in each state, I do not include more detailed interactions to avoid collinearity. The assumption implicit in interpreting these results as reflecting the effect of better information on flood risk capitalization is that potential home buyers are better informed about flood risk in states where flood risk must be disclosed at the time of sale.

The results of running equation 4 are reported in column 3 of table 3. Non-flipped home sales in states with at least one disclosure law capitalize flood risk an order of magnitude more than non-flipped homes sales in states with no disclosure laws. Indeed, it appears that most of the flood risk capitalization observed in prior estimates of $\hat{\beta}$ in equations 2 and 3 appear to be driven by states with disclosure laws. However, there is no statistical evidence that flood risk valuation changes for flipped homes in states with disclosure laws. Nonetheless, the point estimate for $\hat{\tau}_2$ from equation 4 is similar in magnitude and of opposite sign to $\hat{\tau}_1$, which is suggestive evidence that flood risk disclosure laws may play a role in mitigating mispricing of flood-exposed flipped homes.

However, I am unable to reject the null hypothesis that flood risk disclosure laws have no effect on flood risk valuation for flipped homes. As a result, I cannot distinguish between three possible conclusions. First, as suggested by the point estimate, disclosure laws may indeed have an effect on flood risk valuation for flipped homes, and I merely lack the statistical precision to detect the effect precisely. A second possible conclusion is that disclosure laws do not affect flood risk valuation for flipped homes because the lower flood risk valuation of flipped homes is not due to an information channel. For example, buyers of flipped homes could be fully aware of the home's flood risk, but may be convinced by the flipper or the selling agent that the flood risk is nothing to worry about—if this is the case, the valuation effect would operate through a beliefs channel, rather than an information channel. A final possibility is that flood risk disclosure laws have no effect on buyers' information sets. This would be possible if, for example, buyers do not pay attention to the additional disclosures mandated by the disclosure laws. However, for this explanation to hold, disclosure laws would need to differentially affect buyers of flipped homes relative to buyers of non-flipped homes, because my estimate of $\hat{\beta}_4$ is negative and statistically significant. Unless if the markets for flipped and non-flipped homes are segmented, such an explanation seems unlikely.

4.3 Other approaches to estimating the causal effect of interest

Interpreting my results causally requires the assumption that my fixed effects and controls have accounted for every factor that might be correlated with flood risk and flipping and/or sale prices. Despite the richness of my fixed effects and control strategy, this assumption remains strong. One approach to relaxing the assumption that flipping is as-good-as-randomly assigned after conditioning on fixed effects and controls is to leverage a policy that generates plausibly random variation in the probability a home is flipped.

Several such policies are mentioned in section 2. However, none of the identification strategies they suggest appear to be empirically tractable given the data I have. In general, the problem boils down to insufficient observations that are both "treated" by the policy and in the SFHA. For example, I explored the possibility of using exceptions to the FHA's flipping ban as a source of variation.[14] The FHA's flipping regulation includes an exception whereby the FHA, upon issuance of an official notice, will provide mortgage insurance to flipped homes in presidentially-declared major disaster areas. However, the FHA appears to employ this exemption only very rarely: out of the roughly 14 million transactions in my cleaned data, only about 400 have FHA mortgages and were flipped while in a presidentially-declared major disaster area. Of these, only one is also in the SFHA.[15]

Other potentially promising policies include wholesaling bans in Ohio and Illinois passed in 2019 and 2020 respectively. Again, however, I only observe a handful of homes that are in the SFHA and sold while these policies were in force. Similarly, the "cease and desist zones" enacted by the State of New York in 2017, where it is illegal for speculators to contact residents who opt-in to the list, appeared promising, but covered small geographic areas, again lacking sufficient data to proceed. In contrast to the FHA policy, it may be that these laws would constitute a viable identification strategy provided more exhaustive data on the areas where the policies apply and/or more time since their passage.

A final approach that may yield more reliable causal inferences would be to leverage time series variation in the SFHA, using a similar approach to Hino and Burke (2021). Such an approach would allow me to use property fixed effects, purging the unaccounted-for variation across properties that could be confounding my results. I have not pursued this approach, although I suspect it will be vulnerable to the same small-sample challenges mentioned above. Results in the time series approach are identified off of changes in the SFHA as FEMA updates the map over time; these changes are generally marginal and do not affect many properties. Nonetheless, this approach may be a worthwhile avenue for future work.

[14] See section 2 for further details on the FHA's flipping ban.

[15] My conversations with officials at the FHA also suggest that this exemption, despite being part of the FHA's flipping regulation, is seldom used. None of the officials I spoke with could point me to a list of the official notices the regulation requires the FHA to issue in order to waive its flipping ban in disaster areas.

5 Conclusion

Do real estate markets capitalize flood risk? A growing literature suggests that flood risk capitalization is modest at best. What, then, are the channels causing the under-valuation of flood risk?

This paper investigates how flippers, intermediaries who buy and sell homes in short periods, affect flood risk valuation. It is not ex ante obvious how flippers might interact with flood risk valuation: on one hand, flippers might reduce frictions in housing markets by making them thicker. On the other, there is anecdotal evidence that some flippers conceal flood risk information and/or past flood damage from potential buyers.

In my empirical analysis, I find that flipped homes capitalize flood risk less than homes that are not flipped. I also replicate the finding from the literature that flood risk is only modestly capitalized in home prices, if at all. In my preferred specifications, I find that homes in the 100-year floodplain transact at prices about 1.7% lower than homes not in the 100-year floodplain (95% confidence interval: (-3.9%, 0.05%)). Furthermore, flipped homes in the 100-year floodplain transact at prices 17% higher than non-flipped homes also in the floodplain (95% confidence interval: (4%, 31%)).

So far, I am not able to discern the mechanisms that give rise to this result. While information may be an important part of the story, I am not able to reject the null hypothesis that state-level disclosure laws do not affect flood risk valuation in flood-exposed flipped homes. Future work could attempt to further decompose flippers' effects on flood risk valuation and flood-exposed housing markets more broadly by unravelling the possibly competing liquidity and information effects I hypothesize. In addition, future work might further attempt to unpack the mechanisms behind my empirical results, such as investigating more carefully the types of renovations or other modifications flippers make before re-selling homes.

Understanding the causes of mispricing in disaster-exposed real estate markets remains an important topic for future analysis, particularly as climate change increases exposure to extreme events. Instituting policies to mitigate mispricing could help alleviate both direct and knock-on effects of climate change. On the other hand, inaction increases the likelihood that climate damages will be amplified by price swings as asset prices adjust to better reflect climate risk.

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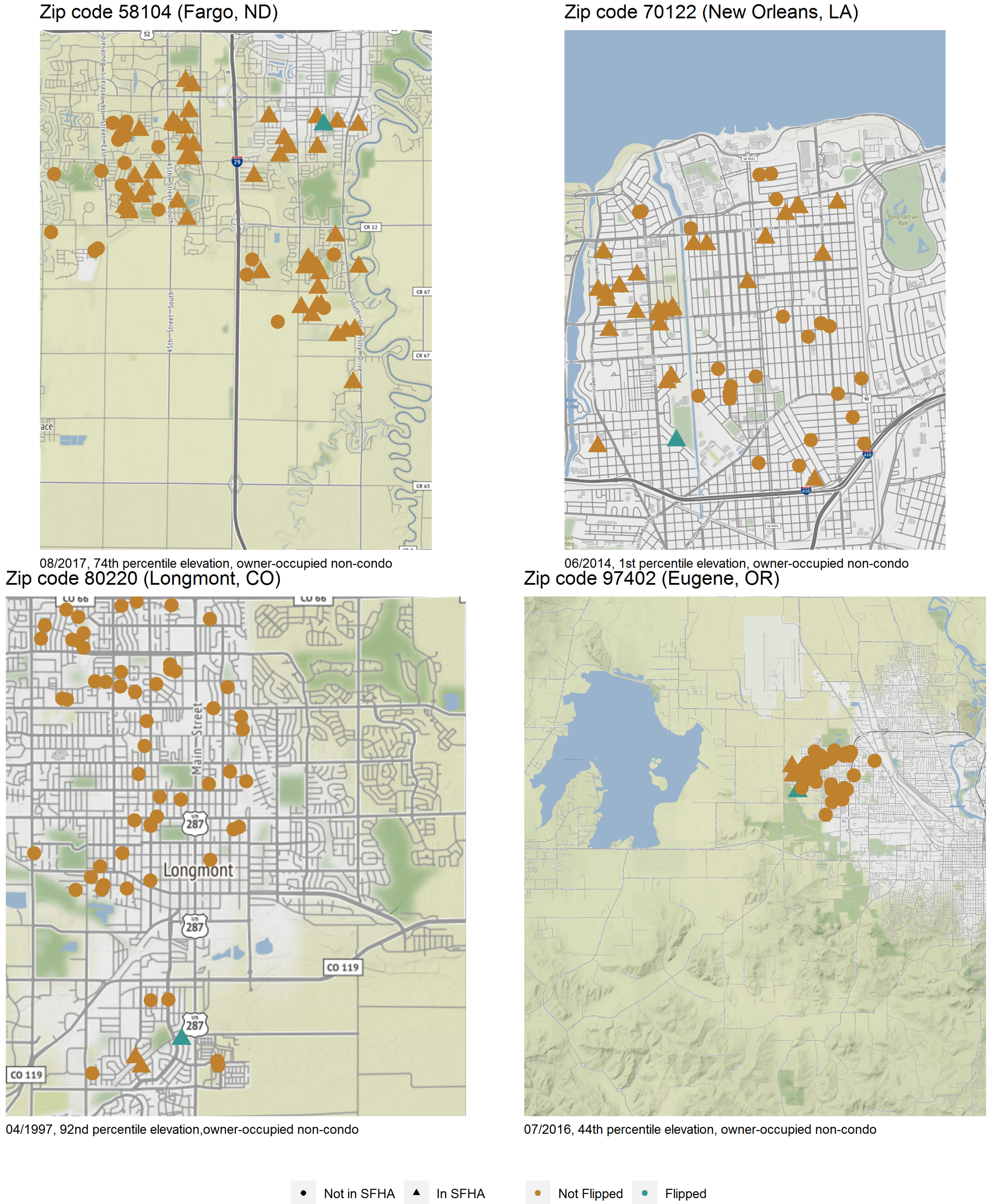
A Appendix Tables and Figures

	Dependent variable:					
	log(Price)					
	(1)	(2)	(3)	(4)	(5)	(6)
Flood risk	−0.008	0.021*	−0.016	−0.010	−0.012	−0.017
	(0.013)	(0.012)	(0.011)	(0.009)	(0.011)	(0.011)
Controls:						
Room dummies	✓	✓	✓	✓	✓	✓
Built decade dummies		✓	✓	✓	✓	✓
Elevation percentile dummies		✓				
Fixed Effects:						
Zip and year by month	✓	✓	✓			
Zip by elevation and year by month				✓		
Zip by elevation by year by month					✓	
Zip by elevation by year by month by owner by condo						✓
Observations	14,696,285	13,595,781	13,595,781	13,595,781	13,595,781	13,595,781
Note:						*p<0.1; **p<0.05; ***p<0.01

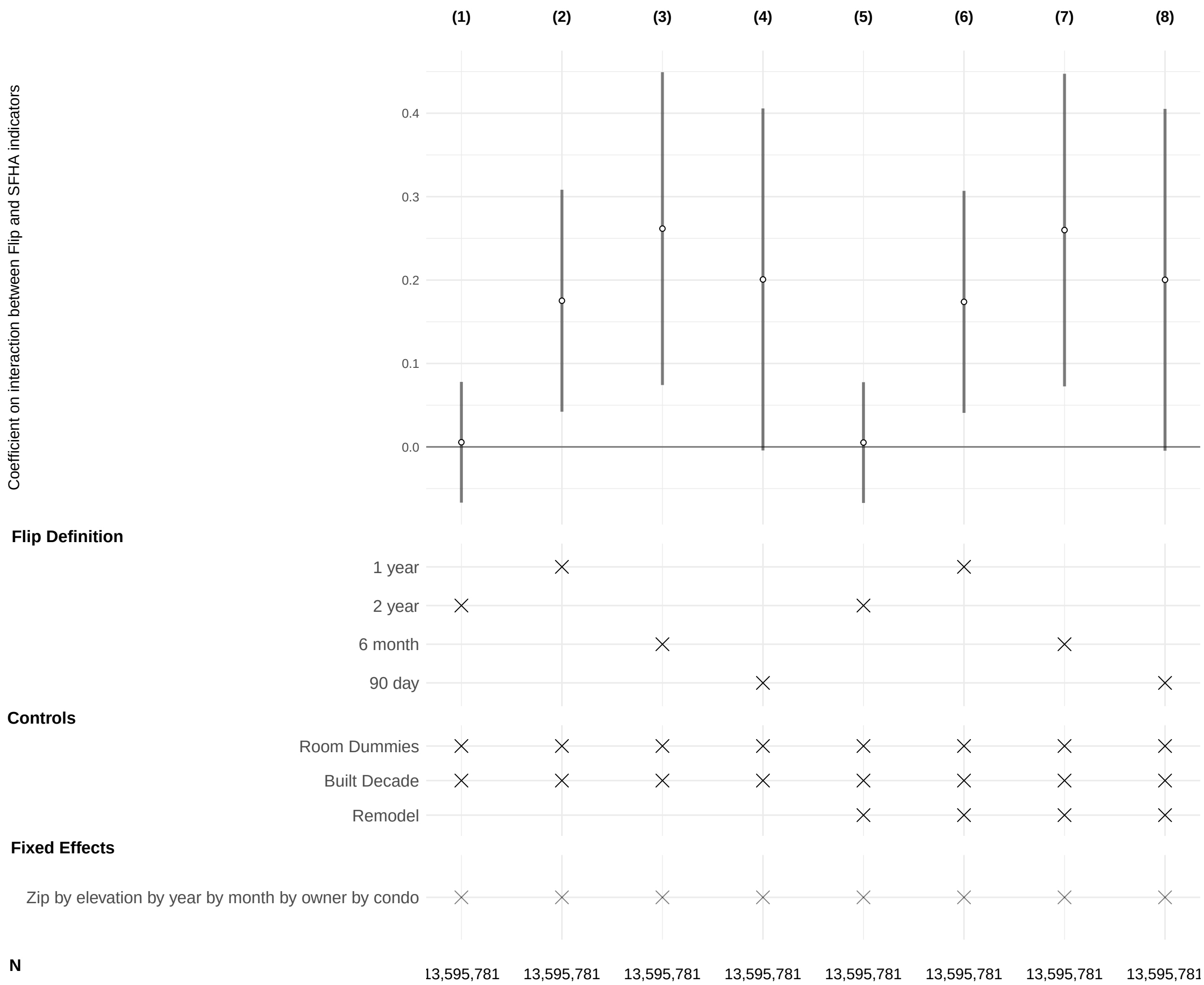
Appendix Table A1: Flood risk capitalization across a range of specifications.

Notes: this table displays the results presented graphically in Figure 5. The coefficient reported is the coefficient on and indicator for whether a home is located in the Special Flood Hazard Area (SFHA). All standard errors are clustered at the zip code level.



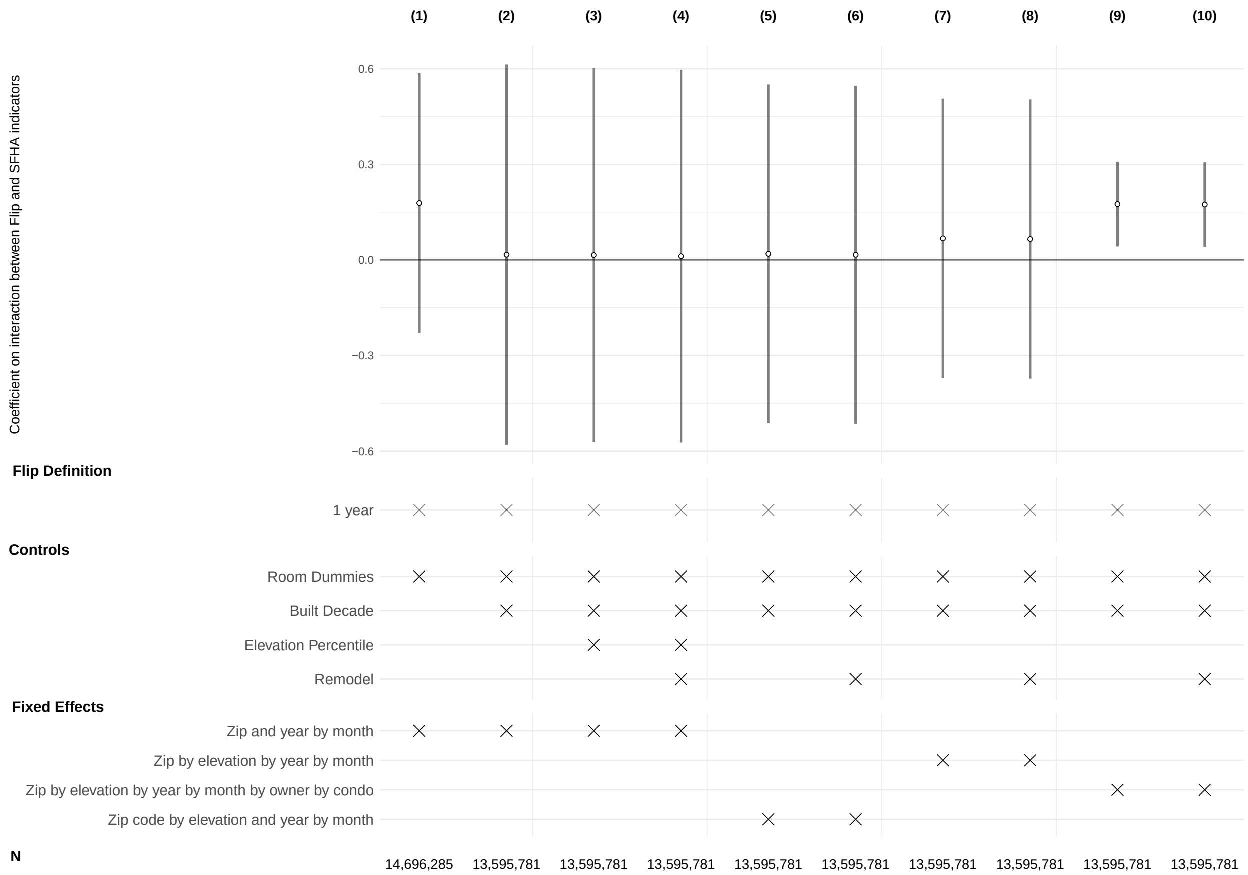
Appendix Figure A1: Additional examples of fixed effect groups

This figure gives additional examples of remaining variation after partialling out my preferred fixed effects, analogously to figure 4. Triangles indicate homes in the SFHA, while circles indicate homes not in the SFHA; brown points are flipped homes while teal ones are non-flipped homes. In my preferred specifications, I identify flood risk capitalization by comparing the sale prices of triangles to those of circles (see equation 3 and figure 5). I identify the difference in flood risk capitalization for flipped and non-flipped homes by comparing sale prices of teal triangles to sale prices of brown triangles (see equation 3 and table 3). All my preferred specifications also include non-parametric controls for the number of rooms in the home and the decade in which the home was built.



Appendix Figure A2: Robustness of main results to flip definition

Notes: Plotted is the coefficient $\hat{\tau}$ from equation 3, which is the coefficient on the interaction between flood risk and flipping. Different specifications explore the robustness of the main results to using alternative definitions of flipping. All standard errors are clustered at the zip code level.



Appendix Figure A3: Robustness of main results to fixed effect choices

Notes: Plotted is the coefficient $\hat{\tau}$ from equation 3, which is the coefficient on the interaction between flood risk and flipping. Different specifications explore the robustness of the main result using different fixed effects. All standard errors are clustered at the zip code level.

B Data Appendix

B.1 Constructing variables used in regressions

B.1.1 Property age and size controls

I control non-parametrically for property age and size by including indicator variables for each of the values topcoded versions of these variables take on. For number of rooms, I include 10 indicators, each corresponding to the number of rooms in the house. Houses with 10 or more rooms are all assigned the 10-room indicator. Approximately 2% of the homes in my dataset have 10 or more rooms.

For property age, I include indicators for the decade in which the home was built. I winsorize the property age data at the 1^{st} and 99^{th} percentiles. In practice, this means that all homes built in the 1880s or earlier and all homes built in the 2010s or later are assigned the same built decade indicator.

B.1.2 Remodeled year

ZTRAX includes information on the year of a property's most recent remodelling. However, I do not observe the exact date of the remodel, which makes it in some cases impossible to precisely determine whether or not some flipped homes were remodelled before or after the initial sale. For example, if a home is sold once in June 2015, remodeled in 2015, and subsequently re-sold in May 2016, it is not clear whether the flipped home was remodelled between sales, or if the remodelling was completed before the first sale (i.e. before June 2015). I conservatively account for this by assigning flipped homes a remodelling dummy value of 1 to all homes which were remodelled in the calendar year prior to the year of the one-year flip sale.[16] A property is assigned a value of 0 if it is not remodelled in the calendar year prior to the second sale or if the remodelling variables is missing for that observation.

[16] I implement analogous rules for 90-day, 180-day, and 2-year flips.