The Price and Distributional Impact of Flood Risk Disclosure: Evidence from US Housing Platforms *

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Abstract

How does information disclosure reshape the allocation of environmental risk in the housing market? We quantify the price and distributional impact of the nationwide disclosure of property-level flood risk from First Street on U.S. housing platforms. Using a difference-in-RD design that exploits discrete cutoffs in the disclosed risk categories, we find that homes labeled as "extreme" flood risk experience a 3.3% price discount and stay on the market for a week longer, relative to those labeled as "severe" risk. These effects are strongest for coastal properties and remain pervasive across different FEMA floodplain designations. Disclosure also generates significant household resorting: buyers of extreme-risk homes have 5.3% lower income, are more likely to use FHA financing, and are older. Through a discrete choice model, we find that the redistribution of risk to lower-income households is primarily driven by price changes rather than heterogeneous preferences for flood risk.

Keywords: Climate Risk; Disclosure; House Prices; Distributional Impacts; Online Platforms.
JEL Codes: G50, G14, R21, R30, Q51, Q54

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1 Introduction

Flood-related natural disasters, such as Hurricane Katrina (2005), Sandy (2012), Harvey, Irma, and Maria (2017), Dorian (2019), Ida (2021), Ian (2022), and Milton (2024), have caused devastating losses of life, homes, and infrastructure. These events are of paramount concern for homeowners, investors, and policymakers due to their enormous economic impact. A report by the Joint Economic Committee estimates that flood damages cost the United States up to $496 billion and homeowners up to $32 billion every year (Heinrich, 2024). For the average household, increasing climate-related natural disasters are re-pricing their most valuable asset: their house. Yet understanding a property's future flood risk is not straightforward: it requires an understanding of surrounding geological features and historical flood events, as well as the changing probability of future natural disasters. If homeowners cannot easily price flood risk on their own, then the public release of modeled flood risk, such as First Street's Flood Factor, has the potential to reclassify and reallocate housing for millions of properties across the United States. In equilibrium, the impact of this information disclosure remains unknown.

To date, there is no comparable substitute for the flood risk information provided by the First Street Flood Factor. Homebuyers previously relied on Federal Emergency Management Agency (FEMA) flood maps to understand if a house is in a Special Flood Hazard Area (SFHA), also known as a 100-year floodplain. However, these FEMA maps measure risk coarsely, often use outdated data, and do not reflect projections of future weather patterns (Kousky, 2018; Wing et al., 2018; Eby, 2019). Flood insurance could provide another estimate of flood risk, but it is only required for mortgaged homes within SFHAs and is rarely purchased elsewhere (Congressional Budget Office, 2024).

In this paper, we make two main contributions. First, we estimate the price effects of a large-scale disclosure of property-level flood risk made available during the initial housing search process. Second, we provide novel estimates of the distributional impacts of flood disclosure and decompose the role of heterogeneity in tastes from price effects on who ultimately incurs higher flood risk. While measuring these equilibrium impacts is paramount for both homeowners and policymakers, it is empirically challenging. Changes in climate salience and flood insurance pricing that are concurrent with this disclosure would bias any event studies. In addition, it is difficult to disentangle high flood risk from dozens of confounding factors that it is correlated with, such as an ocean view amenity. We address these identification challenges by exploiting an institutional feature of the First Street product: its underlying continuous measure of flood risk is mapped into the discrete Flood Factor shown to consumers via a series of internal cutoffs. We successfully recover this internal mapping and compare homes just above and just below a given score cutoff, before and after disclosure, to obtain plausibly

causal estimates through a difference-in-regression-discontinuity (diff-in-RD) design. Because the underlying expected risk and the exact cutoffs are never shown to the public, market participants cannot manipulate around them. We further validate our design through several standard diagnostics: continuity in the running variable's density, balance on observable covariates, and lack of pre-disclosure trends around the cutoff.

Key events in the First Street Flood Factor disclosure are summarized in Figure 1. In August 2020, Realtor.com first began displaying the First Street Flood Factor for all homes listed on its site across the US. In February 2021, Redfin.com followed suit after an initial period of experimental roll-out starting in October 2020 (Fairweather, Kahn, Metcalfe and Sandoval Olascoaga, 2024). Finally, in September 2024, Zillow.com was the last major platform to start listing First Street's Flood Factors.[1] We define the post-disclosure period as from August 2021 onward, allowing at least 6 months to lapse after the full rollout of the Flood Factor on Redfin.com. Figure 2 illustrates a user's view of a Redfin listing, where each property is given a discrete Flood Factor score from 1 (lowest risk) to 10 (highest risk), each situated within a qualitative category labeled as minimal (1), minor (2), moderate (3-4), major (5-6), severe (7-8), and extreme (9-10), respectively. Given that these are the three largest online listing platforms for housing, accounting for over 85% of online traffic,[2] this disclosure was monumental as it marks the first time that prospective homebuyers can obtain a free, easily accessible measure of flood risk at the earliest stage of their homebuying process.

Using a diff-in-RD design, our paper offers two key empirical findings: (1) Homes disclosed to have extreme flood risk experience a significant 3.3% decline in transaction prices relative to similar homes disclosed to have severe risk, with effects most pronounced for coastal properties (11% decline) and pervasive across FEMA flood zone designations. (2) Disclosure also generates significant household resorting: buyers of extreme-risk homes are, on average, 5.3% lower in income, 1.8 percentage points more likely to use FHA financing, and 1.4 years older. Finally, through the lens of a discrete choice model, we estimate the redistribution of risk to lower-income households as being primarily driven by price changes rather than heterogeneous preferences for flood risk.

To begin, we assemble a new nationwide transaction-level dataset that combines property-level flood-risk measures from First Street with housing transactions and property characteristics from CoreLogic. For each transaction, we further obtain data on the buyer's household income and de-

[1] As of November 2025, Zillow has removed the First Street flood information amid complaints made by the California Regional Multiple Listing Service, which claims that displaying flooding probabilities "can have significant impact on the perceived desirability of that property"(Brown, 2025). For our paper, since our sample ends in December 2024, we do not directly address the impact of changing disclosure policy in 2025.

[2] https://www.rubyhome.com/blog/most-visited-real-estate-websites/

mographics from HMDA (Home Mortgage Disclosure Act) and from voter registration records from L2. Our data span the years 2016 to 2024 and cover more than 25 million housing transactions. This combined dataset allows us to observe the sale outcome for each property, its disclosed flood risk, and buyer demographics, all at the individual property level. Because flood risk varies at extremely local levels, having property-level data is particularly important to avoid potential aggregation bias (e.g., households resorting within a neighborhood).

Our main price results, estimated using a diff-in-RD design, show that disclosure has economically meaningful price effects, especially for the riskiest houses. Focusing on the 8 versus 9 cutoff, where homes move from being labeled "severe" to "extreme," we find that disclosure causes a 3.3% decline in sale prices for homes just above the cutoff relative to those just below it. We similarly find strong price effects around all Flood Factor score cutoffs for high-risk properties (7 vs 8 and 9 vs 10). In addition, these homes remain on the market for about 7 additional days, consistent with a reduced demand for the most risky homes. Importantly, we find significant heterogeneity across geographies. Price discounts are particularly significant for coastal properties, which drop 11% on average. Within the subsample of coastal properties, price effects are pervasive across FEMA-designated 100-year floodplains. Yet, we find the strongest effects (16% average discount) for coastal properties that are *outside* of 100-year floodplains, suggesting that areas without robust flood insurance markets are where First Street's flood information provided the largest update on consumers' perception of risk.

Given the estimated price effect, we perform a back-of-the-envelope comparison with average annual loss estimates. We find that the 3.3% average price decline is modest relative to the implied flood cost difference, which is measured at 9.3% of home value between Flood Factor 8 and 9 homes. Again, the location of the property is key for predicting how far prices fall. For coastal properties, the estimated 11% price decline roughly matches the expected cost difference. In contrast, inland or non-coastal homes show little price response despite having similar cost exposure. This evidence is consistent with a theory of inattention, where coastal housing markets are historically more attentive to flood risk, being located next to a body of water, and thus, flood risk is physically more salient to its market participants.

We next turn to the distributional consequences of the information disclosure by examining the changes in who buys risky homes. Linking transactions to borrower characteristics, we find that after disclosure, buyers of homes just above the 8/9 cutoff have 5.3% lower income and are 1.8 percentage points more likely to use FHA financing. Thus, once flood risk is disclosed at scale and becomes incorporated into prices, there is a significant resorting to risky homes by those who are more financially

constrained. These effects are strongest for coastal properties, consistent with the stronger price responses in those areas. Controlling for measures of financial resources (i.e., income), we also find that buyers of the riskiest homes are older on average following disclosure. This suggests further heterogeneity in taste against flood risk, but we do not detect significant shifts in demand by race/ethnicity or party registration.

Finally, we estimate a mixed-logit model of housing choice to decompose household resorting by income into a price channel and a taste channel. The model allows for heterogeneity in price sensitivity that varies with income and heterogeneity in flood-risk aversion that varies with household demographics, both of which are well-identified with our rich household-level choice data. We then use a Shapley decomposition to assess the sources of post-disclosure resorting and reveal that the main driver of the redistribution of flood risk by income is price changes rather than heterogeneity in tastes. In other words, lower-income households do not disproportionately sort into risky homes primarily because they like those homes more (e.g., they are more risk tolerant); they do so because, once risk is disclosed to all market participants, risky homes become relatively cheaper, and financially constrained households are the ones most willing to trade price for risk exposure.

We contribute to several strands of the existing literature. First, our work naturally extends the literature that studies frictions in insurance and mortgage markets as residential homes face increasing natural disaster risk. Traditionally, homeowners bear the expected cost of natural disasters, including floods, through the price of their insurance policies (Georgic and Klaiber 2022; Ge, Lam and Lewis 2022, Mulder 2024). However, as the number of natural disasters continues to increase, the property and casualty insurance market faces growing frictions, including sharply increasing and uneven premiums due to reinsurance (Keys and Mulder 2024); riskier mortgage originations that amplify systemic and taxpayer risks (Sastry, Sen and Tenekedjieva 2023); insufficient coverage in the event of total loss (Cookson, Gallagher and Mulder 2025); as well as misclassified risk leading to both suboptimal insurance take-up and adaptations (Mulder 2024). In addition, widespread underinsurance (Ge, Johnson and Tzur-Ilan, 2025) and increasing insurance delinquency rates (Sastry, Sen, Tenekedjieva and Scharlemann, 2024) have distributional impacts by shifting risk to the most financially constrained households. Since flood damage is not covered under homeowner's insurance but is covered separately by flood insurance, and flood insurance is much harder to obtain outside of SFHAs, our work addresses a gap in this literature by providing a key estimate of how households incorporate flood information in the absence of a robust insurance market.

We also contribute to the general literature that studies how the market responds to novel infor-

mation on environmental risks. Previous literature has documented capitalization effects of flood risk into residential home prices from FEMA flood zone designations (Hino and Burke (2021) and surveyed in Beltrán et al. (2018); Contat et al. (2024)), beliefs about climate risk (Bernstein et al., 2019; Murfin and Spiegel, 2020; Baldauf et al., 2020; Bakkensen and Barrage, 2022), and realized flooding events (Bin and Landry, 2013; Ortega and Taşpınar, 2018). There remains a major concern that the degree of capitalization is weak in comparison to the true costs of flooding in part due to a lack of high-quality information, especially outside of FEMA flood zones (Gourevitch et al., 2023).

Relative to the literature on the pricing of flood risk, our paper provides the first credible estimate of the price and distributional impact of the widespread provision of flood risk information through online housing platforms. Our RD research design is also novel in this context, taking advantage of a previously unknown feature of the institutional setup, where a continuous measure of underlying flood risk is mapped into discrete categories shown to online users. In addition, by focusing on the disclosure of a non-governmental, third-party source of flood information from First Street, we can isolate the role of information alone, which differs from existing work that utilizes FEMA flood zone map boundaries or map changes (Bakkensen and Ma, 2020; Gibson et al., 2017), which alter not only information about a property's flood risk but also its mandatory insurance requirement and the availability of subsidized flood insurance. Finally, we find that the property-level risk information from First Street provides meaningful, incremental value *above and beyond* the pre-existing information environment *outside* of SFHAs, while to our knowledge, previous studies have found that only properties *within* SFHA capitalize flood risk (Hino and Burke, 2021).

Related to the disclosure of information from First Street, Fairweather et al. (2024) randomizes users on Redfin.com to receive flood risk information from First Street and finds that there is a significant impact on their search behavior, tilting away from risky homes. We develop and extend Fairweather et al. (2024) in several important ways. First, during our study period, flood risk information became freely available to all market participants, including buyers, sellers, and real estate agents, so we are the first to estimate the price impact of flood risk disclosure under equilibrium as the market transitions into a new information environment. In other words, while an RCT research design is well-positioned to isolate the impact on homebuyer search behavior, when flood information is provided to a limited fraction of experimental users, it is inherently limited in estimating what would happen in a new equilibrium once information becomes available to all.[3] Second, and more importantly, because we collect individual-level buyer demographics for realized transactions, we can study the

[3] Fairweather et al. (2024) also provides an extrapolation of the anticipated price effect, but it requires additional assumptions to extrapolate out-of-sample linearly.

distributional changes in the new post-disclosure equilibrium, and thus, are the first to document a striking redistribution of risk by household income.

Finally, we contribute to the literature on the impact of digitization in the housing market. Despite the real estate sector's slow adoption of technology overall, online platforms continue to play a crucial role in housing market intermediation, shaping the information environment for market participants. Buchak, Matvos, Piskorski and Seru (2022) finds that iBuyers[4] are able to provide automated intermediation through algorithmic pricing only in liquid markets with rich housing and transaction information, and Harrison, Seiler and Yang (2024) finds both elevated equilibrium prices and localized pricing spillovers after the entry of iBuyers. In addition, Fu, Jin and Liu (2022) shows that Zillow's automated valuations ("Zestimates") influence both listing and sale prices, while Raymond (2024) finds that housing valuation with better data can have market-level effects, closing historical racial gaps in housing values. Fuster, Goldsmith-Pinkham, Ramadorai and Walther (2022) finds that machine learning algorithms have distributional impacts on the mortgage market, being the least helpful to Black and Hispanic borrowers.

The rest of the paper proceeds as follows. Section 2 describes the data and provides details on the institutional background. Section 3 presents the price effects of flood-risk disclosure and the main validation exercises. Section 4 studies the distributional effects on buyer characteristics. Section 5 develops and estimates the structural housing-choice model and decomposes the resulting resorting into price and taste channels. Section 6 concludes.

2 Data and Summary Statistics

2.1 Data Sources

CoreLogic

We begin with a nationwide property-level dataset from local tax assessor records provided by CoreLogic, which offers detailed information on property attributes as well as transaction price, transaction date, and mortgage information. Variables from the assessor files provide the exact geocoded location of each property, along with information on a broad set of housing characteristics, including the number of bedrooms/bathrooms, square footage, lot size, and build year. We additionally merge in listing-level records from the CoreLogic MLS database to obtain Days on Market (DOM). Last but not least, we utilize the CoreLogic mortgage information to create an indicator variable equal to one

[4] iBuyers stands for "instant buyer," referring to real estate companies "that use algorithms and technology to buy and resell homes quickly." https://www.nerdwallet.com/article/mortgages/understanding-ibuyers, accessed August 12, 2024.

if the buyer utilized an FHA loan.

First Street Flood Factor

The flood risk information disclosed on various housing platforms comes from First Street Technology (formerly First Street Foundation), which develops the First Street Foundation Flood Model (FSF-FM), a probabilistic flood model that shows the risk of flooding due to rainfall (pluvial), riverine flooding (fluvial), and coastal surge flooding. The model provides nationwide coverage across the entire continental US, Puerto Rico, Hawaii, and Alaska, extending into areas that have no previous flood modeling. Another important feature is that FSF-FM takes changing environmental factors into account by using global climate model projections to forecast how flood risk will change over the next three decades.[5]

Our data from First Street includes a number of measures of future flood risk assigned to the centroid of each parcel's building footprint. It includes a Flood Factor score ranging from 1 (lowest risk) to 10 (highest risk), which is displayed on the housing websites. Table 2 breaks down the transaction data by the property's Flood Factor. While over 80% of all sold properties have a minimal risk score of 1, a substantial fraction still faces material flood risk. For example, 0.80% and 1.84% of homes sold have a Flood Factor of 10 and 9, respectively, under the "extreme" category, which has over a 95% likelihood of flooding over the next 30 years. Another 1.86% and 1.74% of homes have a Flood Factor of 8 and 7, under the "severe" category.

Importantly, First Street provides intermediate measures of flood risk that are weighted inputs into its internal and continuous measure of expected flood depth. In particular, they provide parcel-level measures of flood depths at various return periods (2y, 5y, 20y, 100y, 200y, and 500y) across different climate scenarios both in the current year and thirty years later in the future. By taking a weighted sum of these modeled flood depths across various horizons (i.e., return periods) under a middle-of-the-road climate change scenario SSP245 (IPCC, 2023), we compute the "expected flood depth," which is subsequently mapped to the discrete Flood Factor scores.[6] Figure 3 illustrates that we can cleanly infer the cutoff points by plotting the observed Flood Factor scores (y-axis) against the logarithm of the expected flood depth (x-axis), which forms the basis of our regression discontinuity design. Note that only the discrete Flood Factor scores are displayed on the online housing platforms, not the

[5]For more details on the First Street Foundation Flood Model, see https://firststreet.org/methodology/flood.

[6]We are very grateful for the helpful discussions with the research team at First Street to construct this data. Additionally, the uncovered cutoffs are for V3 of their flood model, released in January 2023. Prior to V3, Flood Factor V2 used a different rainfall model but still partially overlapped with our post-disclosure period. As such, for the RD design, we focus on the subset of properties for which V3 and V2 Flood Factor scores remain unchanged—for which we can uncover the true cutoff points across the entire sample period.

underlying depth estimates.

We merge the CoreLogic property database with the First Street dataset by matching each parcel in the transaction data to the nearest parcel in the First Street data based on latitude and longitude coordinates of the parcel centroids. This spatial merge assigns a unique flood risk measure from First Street to each property in our sample. Table 1 summarizes our main dataset after merging housing transactions with property-level measures of flood risk. The full transaction sample contains 25,677,071 property records with a median home price of $280,000. Among transactions with mortgages, about 16% are FHA loans; within the available MLS data, the median number of days-on-market is 24 days.

First Street vs. FEMA Flood Zones

As part of the data distribution, First Street also provides information on parcel-level location within flood zones designated by the Federal Emergency Management Agency (FEMA). An area is considered to be in the "100-Year floodplain" if it is expected to be "inundated by the flood event having a 1-percent chance of being equaled or exceeded in any given year," also known as the Special Flood Hazard Area (SFHA).[7] Within the 100-Year floodplain, the National Flood Insurance Program (NFIP) enforces floodplain management regulation and requires mandatory flood insurance for homes purchased with federally backed or regulated mortgages.[8]

Flood risks measured using the First Street Flood Factors and the FEMA floodplains are positively correlated. Table 2 shows that properties with higher First Street Flood Factors are generally more likely to be located in the 100-Year floodplain. For example, 65% of the properties with a Flood Factor of 10 are in the 100-Year floodplain. However, not all properties with a high Flood Factor are in a 100-Year floodplain, and not all properties in the 100-Year floodplain have high Flood Factors. The differences likely stem from a combination of methodology and spatial precision. FEMA floodplain designations are area-level classifications based primarily on land elevation contour lines and statistical analysis of historical flood events and thus are not inherently forward-looking (National Research Council, 2009). In contrast, First Street Flood Factors are property-level assessments that account for finer geographic variation and explicitly incorporate risks associated with climate-driven changes.

As an illustrative example, Figure A1 zooms into a select number of census tracts in Hillsborough County, Florida, showing the categorization of flood risk by SFHA status in the top panel and by First Street Flood Factor in the bottom panel. First, by construction, we see much greater variation in risk levels in the First Street data. Second, the FEMA map is much coarser, showing segmented SFHA

[7] Flood Zones, https://www.fema.gov/glossary/flood-zones, accessed July 5, 2024.

[8] While the mandatory flood insurance requirement applies only to federally backed or regulated mortgages, in practice, most lenders extend this requirement to all residential mortgages in the 100-year floodplain (Ge et al., 2022).

regions with distinct edges, whereas the First Street map shows more nuanced variation. Taking tract 006200 as an example (middle right side of the graph), it is completely blue in the top panel, indicating that it is entirely not in the 100-Year floodplain according to FEMA. However, in the bottom panel, we see a large range in Flood Factor scores, from 3 (moderate) to 8 (severe).

Figure A2 further zooms in, illustrating that due to the discrete cutoffs, properties with similar levels of risk can have disparate Flood Factor scores even within a small neighborhood. In the top panel, it shows that the properties of Flood Factors 8 and 9 can be interspersed and located close to each other. In the bottom panel, we show that adjacent homes with different discrete flood factors can have similar continuous underlying risk, by plotting a color gradient based on the underlying continuous measure of expected flood depths.

While there remains some debate over the accuracy of the data (Meyer, 2024), it is outside the scope of our paper to comment on the accuracy of the First Street flood estimates. That said, they are strongly positively correlated with FEMA flood zones and are also found to correlate strongly with realized flood losses (Mulder, 2024). For the purpose of our paper, we do not take a direct stand on the quality of the First Street measure but instead focus on its market impact once the First Street information is provided widely to consumers.

Household Income and Demographics (HMDA)

We combine property transactions with the Modified Loan Application Register (MLAR), the public version of LAR submitted by financial institutions under the Home Mortgage Disclosure Act (HMDA). It allows us to capture the income and demographic characteristics of the mortgage applicant(s), including race, ethnicity, gender, and age at the time of their mortgage application.

Since HMDA does not include transaction-level identifiers, following Bayer et al. (2016) and Bernstein et al. (2022), our matching process is based on the combination of transaction year, lender name, sales price, loan amount, and census tract.[9] In the end, about 60% of transactions with a mortgage match uniquely to HMDA records. Among unmatched transactions, approximately 66% are associated with cash purchases that did not involve a mortgage.

Table 1 summarizes the HMDA data in the second to last panel. We were able to match roughly 8.4 million transactions. For this matched sample, the average income was about $128,936, and 39% identified themselves as non-white.

[9]Since 2018, HMDA reports loan amounts and sales prices as midpoints of $10,000 bins. We round loan amounts and sales prices in the transaction data to the same midpoints when matching to HMDA.

L2 Voter Registration Data

Lastly, we obtain political party affiliation and voter age using L2 Voter Registration data. For each transaction in CoreLogic, we match its address to the address of a household recorded in the L2 data in the next general election following the sale. Additionally, we check to make sure that at least one of the names registered to that household matches the buyer's name in CoreLogic. Sometimes, voter registrations take some time to update, so we also use the following year of L2 data when necessary.

Table 1 summarizes the voter registration data in the last panel. We were able to match roughly 11 million sales to a household in the L2 data. Among these households, the average age is 44. Roughly 39% of households have at least one member registered as a Democrat, and the same fraction (39%) of households have at least one member registered as a Republican.

2.2 Summary of Price Changes Following Disclosure

To form a baseline, we summarize stylized patterns in our data by estimating log price changes across homes in different flood-risk categories after the release of First Street's Flood Factor on online housing platforms. While this difference-in-differences specification only shows non-causal, correlative patterns, one advantage of this analysis is that it provides a broad overview of the market across all homes sold in the U.S., rather than focusing on a narrow band of homes around a score threshold, which becomes necessary when we next move to a regression discontinuity design to provide causal estimates.

Specifically, we use the following specification:

$$\ln(\text{price})_{jlt} = \vec{\beta_1}\vec{F_j} + \vec{\beta_2}\vec{F_j} \cdot \text{Post}_t + \vec{\beta_3}\vec{X_{jt}} + \alpha_t + \mu_{l\tau(t)} + \varepsilon_{jlt} \tag{2.1}$$

where each observation is a transaction for home j, in location l, at time t. α_t denotes year-month fixed effects, and $\mu_{l\tau(t)}$ denotes location-period fixed effects, allowing for different location fixed effects in the pre- and post-period. $\vec{X_{jt}}$ represents a vector of property characteristics, including building age, number of bedrooms, number of bathrooms, and log lot size.

Importantly, Post_t is a scalar dummy variable equal to 1 if the transaction occurred in the post period. As shown in Figure 1, we define the post period as from August 2021 onward, allowing at least 6 months to lapse after the full rollout of the Flood Factor on Redfin.com.[10] Conversely, we denote a transaction to be in the pre-period if it took place prior to August 2020, which was when Realtor.com

[10] In our transaction data, the median number of days on market from MLS is 24, and 180 days represents the 90th percentile. Therefore, we think six months represents a reasonable cutoff to capture homes listed after the flood information is disclosed.

started the disclosure. For all our analysis, unless otherwise noted, we exclude transactions during the period from August 2020 to August 2021 to avoid issues associated with the partial roll-out of the Redfin experiment from October 2020 to February 2021.

Our explanatory variables are represented by the vector $\vec{F_j}$, which is a 5x1 vector of dummy variables, each equal to one if the property's Flood Factor falls above one of the five qualitative risk categories. More specifically, the dummies are equal to one if the property is classified as greater than minimal (>1), greater than minor (>2), greater than moderate (>4), greater than major (>6), and greater than severe (>8, or equivalently, being "extreme"). Subsequently, the coefficients in front of these indicators capture the average *incremental* price change between homes of that category and homes in the category directly above. For the first category, the incremental change is with respect to the omitted category, i.e., homes with minimal flood risk. The coefficient of interest is $\vec{\beta_2}$ in front of the interaction term, capturing the change in prices from before to after flood disclosure.

$$\vec{F_j} = \begin{pmatrix} \mathbb{1}\{\text{Flood Factor}_j > \text{Minimal}\} \\ \mathbb{1}\{\text{Flood Factor}_j > \text{Minor}\} \\ \mathbb{1}\{\text{Flood Factor}_j > \text{Moderate}\} \\ \mathbb{1}\{\text{Flood Factor}_j > \text{Major}\} \\ \mathbb{1}\{\text{Flood Factor}_j > \text{Severe}\} \end{pmatrix}$$

Table 3 summarizes the results using the full nationwide sample. From column (1) to column (4), we add progressively more stringent geographic fixed effects from county FE, to census tract FE, and to census block-group FE, allowing for unobservable changes at increasingly local levels. In column (5), we further control for bins of building age, indicators for number of bedrooms (1–6) and number of bathrooms (1–5), and log lot size. Lastly, in column (6), we add an indicator for SFHA and its interaction with the post-period dummy, accounting for differential changes that only affect properties inside SFHAs.

In general, Table 3 shows that being labeled as "extreme" (i.e., "above severe") is associated with an incremental price decline of about 1%, meaning that, in the post-period, properties labeled as "extreme" are about 1% cheaper relative to properties labeled as "severe" than they were in the pre-period. This "extreme" effect is statistically significant at the 1% level in all specifications with some location controls. Interestingly, there are no other significant and consistent price declines at lower-risk category cutoffs.

While the disclosure of First Street flood information on housing platforms could contribute to the empirical patterns found in Table 3, we do *not* interpret the estimates as causal because any

unobserved time-varying change correlated with flood risk during this period would introduce bias. Such confounders could include changing beliefs about future intensity or severity of flood events. For example, several hurricanes that drew national attention to flooding during our study period likely increased the salience of flood risk. Furthermore, any concurrent policy changes in mortgage or insurance markets that are tied to underlying flood risk (e.g., Risk Rating 2.0) would also be a confounder. Lastly, other confounders could include changes in the valuation of amenities correlated with flood risk (e.g., increased preference for coastal views and water access). While a lack of pre-trend in our estimate can ameliorate some of these concerns (see the dynamic version of Eq. (2.1) presented in Figure B5), such checks remain inadequate if changes in these unobservable factors coincide with the disclosure timeline. Therefore, to plausibly address the inherent identification concerns above, we use a diff-in-RD design for our main empirical analyses, which we discuss in the next section.

3 Price Effects

To understand the impact of nationwide flood disclosure on the residential housing market, we begin by estimating its effect on housing prices using a diff-in-RD design.

3.1 Main Empirical Specification

To provide plausibly causal estimates on the effect of First Street's Flood Factor, we leverage a diff-in-RD research design for different risk factor cutoffs, comparing their price changes from before to after disclosure. A key innovation of our research design is that we can reproduce the mapping from the underlying continuous measure of flood risk, *expected flood depth*, to the discrete First Street Flood Factor Scores that are released to the public. Figure 3 illustrates that we can cleanly infer the cutoff points by plotting the observed Flood Factor scores (y-axis) against the logarithm of the expected flood depth (x-axis). We use the discrete cutoff points, shown as the vertical black dotted lines, as corresponding thresholds for our regression discontinuity (RD) design. In Appendix Figure A2, we depict an example where neighboring homes with similar expected flood depth are pseudo-randomly assigned different Flood Factor scores by having underlying risks close to a cutoff point.

Moreover, given that we have data across both the pre- and post-periods, our empirical specification is more precise when utilizing a diff-in-RD approach. This allows us to control for any unobserved pre-existing differences that may occur at the cutoff.[11] For example, suppose First Street drew the cutoff such that homes to the right of it are physically more likely to be located on the water. Since

[11] However, if the impact of these unobservables on prices is changing over our time period, it would pose a threat to our identification. That said, it is at least reassuring that we do not find any discontinuity in home prices in the pre-period, as shown in Appendix Figure C7.

we cannot check all possible physical characteristics across all cutoffs and all geographies, we cannot rule this concern out directly. Instead, we make the assumption that these physical characteristics are fixed and thus do not change after disclosure. If so, then by comparing changes in home prices from the pre- to post-period, right above a specific Flood Factor cutoff with those right below the cutoff, we can cleanly identify the causal impact of flood disclosure. Therefore, while a standard RD specification also works well in this setting (as shown in Appendix Figure C7 and Table C8), we utilize the more stringent Diff-in-RD as our main specification.

The key identifying assumption of the Diff-in-RD design is that, other than the disclosure of First Street Flood Factors, there are no unobserved interventions also occurring exactly at the Flood Factor cutoff over the same time period. This assumption is plausible: because the computation of the expected flood depth and the location of the cutoff are entirely internal to the construction of the Flood Factor by First Street, it is unlikely that market participants are aware of the underlying expected depths that determine the Flood Factor. Therefore, prospective home sellers do not know the proximity of a given house's expected flood depth to its score cutoff, and thus, would not be able to manipulate their Flood Factor scores. Figure 4 confirms this by showing a seamless histogram of houses across the 8 versus 9 cutoff, with no evidence of bunching. Similarly, it is unlikely that a prospective homebuyer is aware of the underlying expected flood depth, and thus, their attention to flood risk would not jump discontinuously at the cutoff in the absence of the disclosed Flood Factor.

Additionally, to the best of our knowledge, the modeled expected flood depth by First Street is not a direct input into FEMA methodology. As a result, mortgage requirements and insurance products such as Risk Rating 2.0 that otherwise refer to FEMA products are unlikely to have features that jump discontinuously at the First Street Flood Factor cutoff. As such, the Diff-in-RD design is unlikely to be confounded by changing mortgage or insurance policies that are otherwise correlated with flood risk.

Concretely, for each cutoff between a pair of Flood Factor scores, we estimate the following diff-in-RD regression

$$\begin{aligned} y_{j,t} =& \beta_1 \mathbb{1}\{R_j > 0\} \times \text{Post}_t + \beta_2 \text{Post}_t + \beta_3 \mathbb{1}\{R_j > 0\} \\ &+ \sum_{d=1}^{D} \nu_d R_j^d \ \mathbb{1}\{R_j > 0\} + \sum_{d=1}^{D} \gamma_d R_j^d \ \text{Post}_t + \sum_{d=1}^{D} \rho_d R_j^d \\ &+ \sum_{d=1}^{D} \delta_d R_j^d \ \mathbb{1}\{R_j > 0\} \times \text{Post}_t + \varepsilon_{jt} \end{aligned} \tag{3.1}$$

where each observation is a transaction for home j at time t. The left-hand-side outcome variable

$y_{j,t}$ represents the conditional log transaction prices, created by residualizing log prices on year-month fixed effects, a vector of property characteristics, including bins of building age, indicators for number of bedrooms (1–6) and number of bathrooms (1–5), log lot size, as well as location fixed effects.[12] Post_t is a scalar dummy for the post period as indicated in Figure 1. R_j represents our running variable of log expected flood depth, re-centered by the Flood Factor score cutoff. $\mathbb{1}\{R_j > 0\}$ captures the discontinuous jump when the underlying modeled risk is above a score cutoff. Our main estimator uses local linear regression with optimal bandwidth (Calonico, Cattaneo and Titiunik, 2014), but we also allow alternative estimators based on higher-order polynomials up to degree $D = 3$.

Table 4 reports the results from estimating Eq (3.1) and shows that the disclosure of First Street flood information has a significant impact on home prices, especially for homes disclosed as particularly risky. The columns of Table 4 correspond to estimations using a linear auto-selected band, a polynomial of order two, and a polynomial of order three, respectively.[13] Each panel shows the disclosure effect estimated over the cutoff points from different pairs of Flood Factor scores: Panel A shows 3 vs 4, Panel B shows 4 vs 5, Panel C shows 5 vs 6, etc. In particular, Panels B, D, and F may be particularly interesting because they focus on thresholds that also correspond to a change in qualitative risk category as well.[14]

Table 4 Panel F compares homes with a Flood Factor of 8 (also labeled as "severe" risk) and those with a Flood Factor of 9 (labeled as "extreme" risk), showing that the disclosure of flood risk leads to a significant price drop of 3.3% (based on local linear regressions in column 1), 2.4% (based on second-order polynomials in column 2), or 3.0% (based on third-order polynomials in column 3). Similarly, Panels E and G show that there is a significant and similar-magnitude price drop at other Flood Factor score cutoffs when the flood risk is relatively high: 2.7% decline at the 7/8 cutoff and 3.4% decline at the 9/10 cutoff, respectively. We do not find consistently significant results for Flood Factor scores lower than 7.[15] The concentration of negative price effects among the riskiest homes is consistent with the stylized patterns documented using the full sample of all homes in Section 2.2.

Figure 5 Panel (a) illustrates the diff-in-RD for homes with scores of 8 and 9. Specifically, we

[12] In theory, an RD design does not require geographical fixed effects. However, given large variations in prices across geographic regions due to location-specific amenities, residualizing the outcome variable on these fixed effects reduces significant noise and provides more precise estimates. This allows us to measure variations in home prices within a reasonable neighborhood.

[13] The reported sample size varies between each estimate based on the auto-selected bandwidths.

[14] The qualitative flood risk category is what users see first when they scroll through a home's listing on the online platform. To see the exact numerical Flood Factor scores, the user has to click on the box containing the risk factor to get more information, as shown in Figure 2.

[15] We omit the comparison for the minimal and minor categories because they pose very little flood risk for the homeowner. In addition, the minor category includes very few homes, so estimates around this cutoff are especially noisy.

plot a binscatter graph with log expected flood depth on the x-axis and change in log price on the y-axis within the optimal bandwidths on each side.[16] Properties with a flood score of 8 are shown in blue, while properties with a flood score of 9 are shown in red. Each dot represents the price difference between the post-disclosure and pre-disclosure periods. Generally, the diff-in-RD graph shows a linear upward trend, suggesting that there is an omitted variable (e.g., distance to the water) that is correlated with the continuous measure of flood risk and (since we are taking a difference) is becoming increasingly valuable over the disclosure time period. This would also be consistent with the pattern of OLS estimates being smaller in magnitude than the diff-in-RD estimates, since increasingly valuable amenities such as an ocean view would dampen the observed price change. Most importantly, Figure 5 displays a sharp and significant drop at the cutoff, where homes labeled as 9 experience a sharp decline in price immediately to the right of the cutoff, despite being similar in risk to homes labeled as 8 immediately to the left of the cutoff.

Finally, Figure 6 illustrates the diff-in-RD at the 7v8 and 9v10 cutoffs. In both cases, we observe a notable jump downward at these other cutoffs as well, consistent with what we find in Table 4. For conciseness, we focus on one set of cutoffs for future analyses, and the 8v9 cutoff is arguably the most salient to consumers, since homebuyers only see the qualitative rather than quantitative scores on websites' splash pages. Unlike 7v8 or 9v10, 8v9 also represents a change in qualitative risk, going from "severe" to "extreme." Thus, we focus on comparing homes with Flood Factors of 8 and 9 for the remainder of the paper.

3.2 Heterogeneous Effects

In this subsection, we test whether the baseline price effect for 8 vs 9 homes differs across subsamples. In particular, we examine two key subsample cuts: homes exposed to coastal flooding versus those not exposed, and SFHA versus non-SFHA properties.

First, we examine whether certain sources of flooding are priced differently from others. In the First Street data, the main sources of flooding are pluvial (from rain), fluvial (from the river), and coastal. Each home may be subject to a combination of these three sources. Conditioning on homes that have beyond-minimal flood risk, Table 1 shows that about 25% of them are subject to coastal flooding, 69% subject to pluvial flooding, and 27% subject to fluvial flooding. Appendix Figure A3 provides an illustrative example of properties subject to coastal flooding in Hillsborough County, FL. The detailed breakdown of the combination of these three sources is shown in Appendix Table B4.

[16] We also show a binscatter plot for all homes with an 8/9 score fitted using a third-order polynomial in Appendix Figure C6.

Table 5 Panel (A) column (2) shows that properties subject to coastal flooding experience a substantially larger price drop at 11%, compared to the average effect of 3.3% for all homes. Column (3) shows that, for homes not subject to coastal flooding, disclosure has no significant price impact. Consistent with this, Figure 5 Panel (b) illustrates that the price discontinuity is large and pronounced for coastal properties. Conversely, Panel (c) shows no significant discontinuity for non-coastal properties. The concentration of price effects among coastal properties suggests that the release of First Street data has an especially strong effect in areas where flood risk was likely already salient, rather than providing new pricing information to inland areas that may have been considered less risky. This finding also aligns with behavioral papers that have found that flooding is only priced in areas where individuals believe in the flood risk (Baldauf et al., 2020) or that individuals largely respond to disaster risk only when it is salient (Dessaint and Matray, 2017; Gallagher, 2014).

Next, we examine the impact on homes in an SFHA and homes outside of any SFHA. A priori, it is not immediately obvious whether homes in SFHAs would be more or less affected. On the one hand, because flood insurance is mandated for federally-backed mortgages on homes in an SFHA, whether a home is in a 100-year floodplain is usually already salient to a prospective buyer. Thus, one might expect the price impact of additional information disclosure to be limited. On the other hand, because insurance quotes are typically not obtained until after a home goes under contract, the price impact of flood risk information disclosed at an early stage of housing search may be particularly relevant if the buyer expects that the disclosed Flood Factor is correlated with their expected, mandated, insurance costs as well as with their expected flood damages not covered by insurance.

Table 5 Panel (A) columns (4) and (5) show that the disclosure of First Street flood information has a negative price impact on homes regardless of their SFHA status: homes in SFHAs experience a price decline of 3.47% and homes outside of SFHAs experience a price decline of 3.35%. While the estimate for homes inside SFHAs is statistically insignificant (with a t-stat of 1.2), when broken down further by coastal status, Table 5 Panel (B) shows a strong negative price effect of 6.6% for coastal homes in SFHAs. Such a strong and significant effect for coastal properties within SFHAs suggests that First Street disclosure provides a significant incremental impact even when the floodplain designations are already known, likely affecting buyers' expectations of future insurance and flood damage costs beyond what can already be inferred from the SFHA designation.

For homes outside of SFHAs, Table 5 Panel A shows a significant negative effect of 3.35%, indicating that homebuyers have reacted to the disclosed flood risk information even when these properties are not in a 100-year floodplain, and flood insurance is not mandated. Additionally, because we do not

expect concurrent policy changes affecting homes in SFHAs (e.g., Risk Rating 2.0 affecting the price of mandated insurance) to vary discontinuously at the Flood Factor cutoffs, a significant price effect on non-SFHA homes suggests that the estimated treatment effects cannot be explained away by these policies, further lending credibility to our research design.

Moreover, we find a strikingly large effect of 16% for coastal homes outside of SFHAs, as shown in Table 5 Panel (B) column (3), suggesting that buyers respond most strongly to homes for which flood risk may have been a concern (i.e., due to proximity to coasts) but that were not previously designated as risky (i.e., not in a 100-year floodplain). As a result, our paper demonstrates that disclosure of First Street flood information may have shifted buyers' prior perceptions of the flood risk in the subsample of coastal, non-SFHA homes the most.

3.3 Additional Validation and Robustness

Covariate Balance

In addition to the absence of bunching at the cutoff discussed before (Figure 4), we further assess the validity of the research design through a covariate balance check. Table 6 tests whether observable characteristics for transacted properties exhibit a discontinuity at the cutoff. Using the same diff-in-RD specification of Eq. (3.1), we examine log lot size, log living area, number of bedrooms, number of bathrooms, and building age, as well as indicators for pluvial, fluvial, and coastal flood sources and SFHA status, as outcomes. None of the estimates show a statistically significant discontinuity, indicating that the estimated price effects are not attributable to systematic changes in dwelling attributes across the threshold.

Pre-Period Placebo

Appendix Figure C7 estimates the standard RD separately within the pre- and post-disclosure periods. Consistent with Figure 5, we find a significant price drop at the cutoff, but we do not find a statistically significant jump at the cutoff during the pre-disclosure period.

Moreover, Figure 7 expands this test by estimating the cross-sectional RD coefficient year by year. The treatment effect is statistically indistinguishable from zero throughout the pre-disclosure years (2016–2019), ruling out differential pre-trends at the cutoff. Post-disclosure, the effects are estimated to be negative and growing in magnitude over time.[17] Overall, the absence of a significant discontinuity in the pre-disclosure period suggests that pre-existing confounding interventions at the cutoff are

[17] Our conjecture is that this may be driven by increasing salience of climate risk due to a rising incidence of major flooding events in the most recent years and the expansion of Flood Factor disclosure to the app version of Zillow in September 2024, which further increases the reach of the flood information.

unlikely.

Alternative Geographic Fixed Effects

Table 7 assesses the sensitivity of the baseline estimated treatment effect to the choice of geographic controls. While, in theory, our RD design does not require additional controls, residualizing the outcome variable on geographic fixed effects allows us to account for large variations in location-specific amenities and thus yields more precise estimates. Column (1) includes no geographic fixed effects; columns (2) to (4) progressively add county, census tract, and block-group fixed effects. Across all columns with geographic controls, the estimated effects are stable, ranging from -2.6% under county fixed effects to -3.3% under block-group fixed effects, and precision increases monotonically with granularity as expected. While the specification with no geographic controls has a smaller point estimate of -1.2%, given its much larger standard error of 2.3%, the estimate is not statistically distinguishable from those in the other columns. Thus, while our findings do not hinge solely on using block-group fixed effects, they allow us to obtain the sharpest estimates and thus are used as our main specification.

3.4 Benchmarking the Magnitude

While one of our main contributions is to provide a plausible and precise estimate of the price impact of flood disclosure, a key question remains: is the size of the observed price drop reasonable? To address this, we assess the magnitude of the estimated price effect by comparing it with the estimated costs associated with higher flood risk.

Recall that, when comparing properties of Flood Factor 8 vs. 9, we estimate about a 3.3% price decline for the full sample and a much stronger effect at 11% for coastal properties. To put this magnitude into context, we use First Street's average annualized loss (AAL), which estimates the annualized damage costs to the structure from flooding based on the full risk profile. It applies the depth-damage functions from USACE (United States Army Corps of Engineers) and FIA (Federal Insurance Administration) to the First Street Foundation Flood Model to derive AAL estimates.[18]

Here, we find that the 50th-percentile AALs of owning a home with a Flood Factor of 8 and 9 are $3,441 and $10,730 on average, representing 0.8% and 2.1% of their average home value, respectively. Taking the difference between these two percentages results in a difference of 1.3%. A back-of-envelope calculation assuming a 5.0% discount rate (Poterba and Solomon, 2025) and staying in the house for

[18] For methodological details, see https://assets.firststreet.org/uploads/2021/02/The_Cost_of_Climate_FSF20210219-1.pdf.

nine years (Mateyka and Marlay, 2010) implies a difference in flood damage cost of 9.3% of home value between owning a home with a Flood Factor of 8 versus 9.

Relative to the 9.3% cost difference, the estimated 3.3% price decline appears significantly smaller and would be consistent with a theory of inattention. However, this "under-reaction" is better interpreted through heterogeneous effects: coastal homes have an average price decline of 11% post disclosure, which is roughly comparable to the estimated cost difference for those homes; whereas the price of non-coastal homes appears barely affected by disclosure (as shown in Table 5), despite also having an estimated cost difference of comparable magnitude. One potential explanation is that flood risk is more salient for coastal properties, which are generally located close to a body of water, and thus, have buyers who are more attentive to flood information such as the Flood Factor. Conversely, buyers of non-coastal properties may be less attentive to flood risk and do not fully price it in.[19]

We caveat that there remains a conceptual challenge in comparing the estimated price effect, driven by the marginal homebuyer, with the estimated cost difference, computed as an average across all homes. Moreover, while the Flood Factors are displayed free of charge on the housing platforms, First Street's cost estimates are not provided directly or free of charge to consumers.[20] Thus, besides the conceptual distinction in the estimated quantities, the difference can be further attributed to a variety of factors, including a lack of attention paid to the risk information, a lack of information on the expected costs associated with the risk, and potentially other explanations such as buyers' liquidity constraints.

3.5 Days on Market

To obtain a more complete picture of the effect of flood disclosure on the housing market, we also examine the total days on the market for each home. We estimate the same diff-in-RD specification shown in Eq (3.1), replacing the left-hand-side variable with total days on market. The results are reported in Table 8 Panel A and visually depicted in Figure 8 Panel A. Consistent with reduced demand for the riskiest homes, we find that homes disclosed as having "extreme" flood risk experience significantly longer listing durations, approximately 7 additional days. Given that the median number of days-on-market is 24 days, it constitutes a substantial increase. As with the price effect, this increase in days-on-market is significantly larger for homes exposed to coastal sources of flooding (Figure 8 Panel B and Appendix Table C9), which remain on the market approximately 13 additional days on

[19] The average cost difference between Flood Factor 8 vs 9 is \$7,289 (=\$10,730 - \$3,441). For coastal homes, the estimated difference is \$8,210; for non-coastal homes, the estimated difference is \$5,964.

[20] A homebuyer can click on the risk profile of any home on Redfin, Realtor.com, or Zillow and request a free 7-day trial from First Street that provides access to this detailed cost data for an unlimited number of homes.

average. Together with our pricing results, these findings suggest that the First Street disclosure had a remarkable market impact, shifting demand away from the riskiest homes.

4 Distributional Effects

In this section, we examine the impact of disclosure on *who* buys these risky properties. We first examine the effect of disclosure on two proxies of homebuyers' financial constraints: (1) household income and (2) whether they took out an FHA loan. We then examine how disclosure affects other household characteristics, including age, race/ethnicity, and political affiliation.

4.1 Distributional Effects on Measures of Financial Constraints

We begin by estimating the model specified in Equation 3.1, replacing the dependent variable (previously log price) with two proxies for homebuyers' financial constraints: income and an indicator for FHA loans. As discussed in Section 2, we obtain the income of the mortgage applicant by combining HMDA and CoreLogic at the transaction-level. We obtain the FHA status from the CoreLogic Mortgage module. Obtaining the income and mortgage type at the individual-transaction level allows us to more precisely characterize the redistribution of environmental risk, a notable improvement over existing work that studies such redistributive effects through changes at the neighborhood level only (Davis, 2011).

Table 8 Panel B shows that, after disclosure, households that purchase homes right above the cutoff with a Flood Factor 9 ("extreme" risk) are 5.3% *lower* in income than those who purchase homes right below the cutoff with a Flood Factor of 8 ("severe" risk). Figure 9 Panel (a) visually depicts a clear jump downward at the cutoff. In addition, when broken down further by subsamples, Appendix Table C10 column 2 shows that the difference in income is more pronounced for coastal homes, estimated at 13%. While similar in magnitude, we find a more precise estimate for properties outside of SFHAs (column 5) than those within SFHAs (column 4). Thus, while we lose sample size and power in the subsample analysis, the evidence is consistent with the patterns found for the price effect, with the redistribution effect on income also strongest for homes with salient (i.e., coastal) sources of flooding and is pervasive regardless of SFHA status.

It is worth considering our findings alongside those of Fairweather et al. (2024), where information provision did not differentially affect search behavior by household income. They found that households across income levels reacted similarly by substituting away from risky properties during their searches. While this may appear somewhat contradictory on the surface, the two findings are in fact consistent under binding liquidity constraints: even if households across income levels have similar degrees of

distaste for flood risk, it is the lower-income households who can least afford to avoid it. The key distinction is that Fairweather et al. (2024) provides information to a limited fraction of experimental subjects, whereas in our setting, flood risk information is provided to all market participants at the same time. Once all participants are informed, a new equilibrium emerges in which riskier properties are significantly lower in price. As we show in the next section, under a standard model of discrete choice subject to a budget constraint, households become more inelastic (i.e., less price-sensitive) as their income increases, and consequently their willingness-to-pay to avoid a disamenity also increases with income.

Relatedly, beyond the reported income of the mortgage applicant, we also consider the impact on the probability that the mortgage is an FHA loan. Since FHA loans have a much lower down payment requirement than conventional mortgages and more flexible credit requirements, they are more commonly used by first-time homebuyers with limited savings and those with lower credit scores, which we consider as alternative proxies for financial constraints. Table 8 Panel C shows that, after disclosure, households that purchase homes with "extreme" flood risk are 1.8 percentage points more likely to use FHA financing than those who purchased homes with "severe" risk. Given that the average fraction of mortgages that have FHA financing is 16%, this represents a proportional increase of 11%. As with previous findings, Appendix Table C11 further illustrates that the probability of using an FHA loan is also stronger among coastal homes, estimated with a significant increase of 5.6 percentage points.

Overall, our financial constraint measures based on mortgage data tell a consistent story: following flood disclosure, the most risky homes are increasingly held by lower-income and lower-credit borrowers. This effect is the strongest for homes exposed to coastal flooding, and is also often present across SFHAs. As such, our findings highlight a potential tension between risk pooling and better information provision from a distributional perspective. On the one hand, because households that are more financially constrained are more willing to trade additional consumption for greater flood risk, and thus sort into riskier properties, better information improves their welfare and overall allocative efficiency. On the other hand, it also naturally opens a question of climate justice, where such environmental risks now fall more on the shoulders of those who are least willing to pay for them.[21]

[21] As with any distributional concerns, there is the question of how to appropriately assign welfare weights across households of different levels of income, which is beyond the scope of this paper.

4.2 Distributional Effects on Additional Household Characteristics

To test whether homebuyers also sort on other demographic characteristics besides measures of financial constraints, we re-estimate Equation 3.1 by replacing the left-hand-side variable with various buyer demographics. The buyer demographics we consider include the household's average age, race/ethnicity, and a dummy variable indicating whether anyone in the household is registered as a Democrat. In these specifications, we additionally control for the correlation between these demographics and income by adding both an income control variable and an interaction between income and the post-disclosure dummy.

Table 9 Panel A shows that the buyers of above-severely risky homes become older following disclosure. Column (1) shows the estimation without income controls, while column (2) repeats the analysis using income controls. Columns (3) and (4) show the results with income controls but using alternative specifications. We find that the average homebuyer becomes 1.4 years older, without income controls, which is also visually depicted as a positive jump at the cutoff in Figure 9 Panel (c). Further, the effect on age increases to 2.0 years when we add in income controls. These findings are based on average household age using L2 data, which includes the exact age of the registered voter. For robustness, we obtain similarly significant results when using age categories from HMDA. Taken together, these findings suggest that older homebuyers appear less concerned about the disclosed flood risk.

Next, Table 9 Panel B shows that the buyers of above-severely risky homes are not significantly more or less likely to be a registered Democrat following disclosure. Across all columns, we find that the effect is very small and statistically insignificant, displaying no jump in Figure 9 Panel (d).[22] Although one might expect a negative coefficient given that the Democratic party is generally more concerned about climate change, we also note that our finding is consistent with Fairweather et al. (2024), who found no differential search after disclosure between Democratic and Republican-leaning counties. That said, the null results here could also be driven by the measurement issue, where registered party affiliation remains a poor proxy for one's specific beliefs about climate risk. Similarly, using the HMDA data, we do not find any statistically significant impact on buyer race/ethnicity after disclosure, as shown in Appendix Table C14.

Overall, the results in this section show that the disclosure of flood information has led to significant changes in *who* buys riskier properties: they tend to be lower-income, more likely to have taken out

[22] We also note that, when constructing the indicator for the presence of registered Democrats, we set it to zero for any housing transaction for which we cannot find a registrant at the same address in L2. That said, when considering alternative measures of partisanship, such as the fraction of Democrats among all registrants in a household, we also do not find any statistically significant results. Further, we also do not find significant results when using an indicator for the presence or the fraction of registered Republicans.

an FHA loan, and, on average, older.

5 Household Resorting Through a Discrete Choice Model

Since heterogeneity in both financial constraints and (dis)tastes for flood risk can drive one's willingness-to-pay for the environmental disamenity, in this section, we estimate a structural discrete-choice model that allows us to unpack the drivers of household resorting. In doing so, we can separately trace out a price channel and a taste channel.

We emphasize that the goal of this section is to provide a model-based framework for quantifying the drivers of resorting, based on the observable changes in household demographics estimated in the previous sections. Accordingly, the model incorporates heterogeneity in price sensitivity by income and in flood-risk preferences by age and political affiliation, but is intentionally simplified in all other dimensions. In particular, it is *not* intended as a fully specified structural model of housing choice, nor does it include a supply side; as such, the estimated model should not be used to compute counterfactual market equilibria.

5.1 A Structural Model of Housing Choice

Given the heterogeneity in both financial constraints and preferences for flood risk, we use a mixed-logit discrete choice model with random coefficients, a common approach to modeling housing choices (Bayer et al., 2007; Calder-Wang, 2021). Specifically, consider household i chooses among homes j with the following utility

$$u_{ij} \;=\; -\alpha_i \ln p_j \;+\; \beta_{r,i} \cdot \mathbb{1}\{R_j > 0\} \;+\; \beta_u \, R_j \;+\; \varepsilon_{ij}, \tag{5.1}$$

$$\alpha_i = \alpha_0 + \pi_{p,y} \ln y_i \tag{5.2}$$

$$\beta_{r,i} = \beta_r + \pi_{r,\text{age}} \cdot \text{age}_i + \pi_{r,\text{dem}} \cdot \text{is_dem}_i \tag{5.3}$$

As before, R_j denotes the running variable, re-centered log expected flood depth, which represents a continuous underlying measure of predicted flood risk. $\mathbb{1}\{R_j > 0\}$ is a dummy variable indicating whether the property is disclosed to be of extreme risk (i.e., Flood Factor 9).

Importantly, we allow the price coefficient α_i and the distaste for properties disclosed with extreme flood risk $\beta_{r,i}$ to vary across households. By allowing the price coefficient to vary with household income, we naturally incorporate the notion that price elasticity varies with income. Namely, if $\pi_{p,y} < 0$, then the price coefficient α_i is decreasing in income. In addition, we allow the preference for disclosed flood risk, $\beta_{r,i}$, to vary at the household level, modeled as a function of household

demographics such as age and political affiliation. In light of Fairweather et al. (2024), income itself does not enter into the flood risk preferences directly.

Consequently, assuming disclosed flood risk is a disamenity (i.e., $\beta_{r,i} < 0$), a household's willingness-to-pay to avoid a property that is disclosed to have "extreme" risk can be expressed as

$$\text{WTP}_i = -\frac{\beta_{r,i}}{\alpha_i} \tag{5.4}$$

which increases with household income (as α_i decreases with income) and with household aversion to disclosed flood risk (i.e., $-\beta_{r,i}$). In other words, the willingness-to-pay measure is determined by the joint distribution of household price sensitivity and taste heterogeneity with respect to disclosed flood risk.

Incidentally, we also include a term in front of R_j, in addition to the disclosed Flood Factor, to account for the impact of unobservable amenities that are generally correlated with flood risk but do not jump discontinuously at the disclosure cutoff, thereby capturing the value of such correlated amenities (e.g., waterfront view). The error ε_{ij} is i.i.d. Type-I extreme value, and thus implies standard logit choice probabilities.

5.2 Estimation

Since we have the underlying household choices at the transaction level, we follow standard estimation procedures using optimal micro-moments (Berry et al., 2004; Conlon and Gortmaker, 2025).

We construct the moment condition as follows. Let d_{ij} be the indicator for the observed choice that equals 1 when household i chooses home j in the data. Define the per-alternative derivative vector

$$z_{ij} \equiv \frac{\partial u_{ij}}{\partial \theta} = \begin{bmatrix} -\ln p_j \\ -\ln y_i \ \ln p_j \\ \mathbb{1}\{R_j > 0\} \\ \text{age}_i \times \mathbb{1}\{R_j > 0\} \\ \text{is_dem}_i \times \mathbb{1}\{R_j > 0\} \\ R_j \end{bmatrix}$$

where the vector of parameters $\theta = (\alpha_0, \pi_{p,y}, \beta_r, \pi_{r,\text{age}}, \pi_{r,\text{dem}}, \beta_u)'$. The score, and thus, the optimal micro-moments are

$$g_i(\theta) = \sum_j z_{ij} \left(d_{ij} - P_{ij}(\theta)\right), \tag{5.5}$$

and the sample moments are $G(\theta) = \frac{1}{N}\sum_i g_i(\theta)$. The GMM objective is $Q(\theta) = G(\theta)' W G(\theta)$. Since the GMM is just-identified, one-step GMM suffices.

Notably, since we have detailed microdata on household choices, the coefficients capturing preference heterogeneity are well-identified. For the estimation sample, we use the set of transactions within the bandwidth of homes around the 8 vs. 9 cutoff during the last year of our sample. Naturally, we further restrict to observations for which income and voter registration records are available through HMDA and L2.[23] Table 10 reports the estimated heterogeneity parameters.

Both heterogeneity parameters $\pi_{p,y}$ and $\pi_{r,\text{age}}$ are precisely estimated and statistically significant. The parameter $\pi_{p,y} = -2.13$ (s.e. $= 0.28$, $p < 0.001$) indicates that price sensitivity decreases substantially as household income increases. The parameter $\pi_{r,\text{age}} = 0.075$ (s.e. $= 0.044$, $p = 0.088$) suggests that tolerance for homes disclosed as risky (i.e., Flood Factor of 9) increases with age. The parameter $\pi_{r,\text{dem}} = -0.082$ (s.e. $= 0.123$, $p = 0.506$) is not statistically significant, consistent with our earlier RD results showing weak effects of political affiliation.

While the heterogeneity parameters $(\pi_{p,y}, \pi_{r,\text{age}}, \pi_{r,\text{dem}})$ are directly estimated from the choice data, we calibrate the mean-level parameters (α_0, β_r) to match two external targets. In other words, while our model can identify heterogeneity in preferences over housing characteristics, it cannot identify the mean coefficients because we do not have price instruments. Concretely, we first calibrate the mean price coefficient α_0 to match an own-price elasticity of -0.67, consistent with Albouy et al. (2016). Then, we also calibrate the mean coefficient on the "extreme" risky properties β_r to match the estimated -3.3% price differential between Flood Factor 8 and 9 in the post-disclosure period. [24]

That said, since the decomposition of resorting is primarily driven by the heterogeneous coefficients, we perform this calibration exercise to ensure that the mean coefficients used are of a comparable order of magnitude to existing estimates in the literature; moreover, we confirm that the main findings of our decomposition analysis are not sensitive to the calibration choices.

5.3 Three-Way Shapley Decomposition of Resorting

Using the estimated model, we assess how much observable redistribution of flood risk by income is due to price changes versus preference heterogeneity. We employ a three-way Shapley decomposition that separately accounts for (1) price changes, (2) age-based preference heterogeneity, and (3) political

[23] As a simplifying assumption, all homes in this sample are in the choice set. To make the homes comparable, log price is residualized to block-group fixed effects and hedonic characteristics. Household demographics are centered and scaled (e.g., age is measured in decades).

[24] Further, the calibration of the mean parameters in a second-step does not affect the estimated heterogeneity parameters π as they are already pinned down with the micro data in a first-step (Bayer et al., 2007).

preference heterogeneity.

5.3.1 Decomposition Method

We compare eight scenarios representing all combinations of three binary switches: the estimated price wedge due to disclosure (PRE vs. POST), preference heterogeneity due to age (OFF vs. ON), and preference heterogeneity due to political affiliation (OFF vs. ON):

M_{000} : (price=PRE, age=OFF, dem=OFF), M_{001} : (price=PRE, age=OFF, dem=ON),
M_{010} : (price=PRE, age=ON, dem=OFF), M_{011} : (price=PRE, age=ON, dem=ON),
M_{100} : (price=POST, age=OFF, dem=OFF), M_{101} : (price=POST, age=OFF, dem=ON),
M_{110} : (price=POST, age=ON, dem=OFF), M_{111} : (price=POST, age=ON, dem=ON).

For any metric M, such as the covariance between household income and their exposure to flood risk (e.g., $\text{cov}(\ln y_i, R_i)$), the total change is $\Delta M = M_{111} - M_{000}$. The three-way Shapley decomposition allocates this total change across the three channels by averaging over all possible orderings of switching the options on. Specifically:

$$\Delta M_{\text{price}} = \frac{1}{3}(M_{100} - M_{000}) + \frac{1}{6}(M_{110} - M_{010}) + \frac{1}{6}(M_{101} - M_{001}) + \frac{1}{3}(M_{111} - M_{011}), \tag{5.6}$$

$$\Delta M_{\text{age}} = \frac{1}{3}(M_{010} - M_{000}) + \frac{1}{6}(M_{110} - M_{100}) + \frac{1}{6}(M_{011} - M_{001}) + \frac{1}{3}(M_{111} - M_{101}), \tag{5.7}$$

$$\Delta M_{\text{dem}} = \frac{1}{3}(M_{001} - M_{000}) + \frac{1}{6}(M_{101} - M_{100}) + \frac{1}{6}(M_{011} - M_{010}) + \frac{1}{3}(M_{111} - M_{110}). \tag{5.8}$$

Here, each channel's contribution is computed by averaging its marginal effect across all possible states of the other two channels. This ensures that the decomposition is symmetric and the three contributions sum exactly to the total change: $\Delta M = \Delta M_{\text{price}} + \Delta M_{\text{age}} + \Delta M_{\text{dem}}$.

5.3.2 Results and Interpretation

Table 11 presents the three-way decomposition results. We focus on the covariance between log household income and expected flood depth as a measure of exposure to flood risk. In the calibrated specification, the total change in this covariance is -0.0066, indicating that after disclosure, lower-income households are *more* exposed to flood risk. The decomposition reveals that price changes account for 91.9% of this resorting effect, with a contribution of -0.0061. This dominant role of prices reflects the fact that risky properties became relatively cheaper after disclosure, and lower-income

households, who are more price-sensitive, disproportionately selected these properties.

Age-based preference heterogeneity contributes 10.7% (-0.0007) to the total resorting. This positive contribution arises because older households exhibit less aversion to properties labeled as risky ($\pi_{r,\text{age}} > 0$), and older households tend to have slightly lower income in our estimation sample of mortgage borrowers and registered voters. This age-based sorting thus reinforces the shift of risk toward lower-income households.

On the other hand, preference heterogeneity by political affiliation contributes -2.6% ($+0.0002$) to the total resorting. The negative sign indicates that this channel *weakens* the resorting of lower-income households into riskier properties. This occurs because Democrats exhibit a stronger aversion to risky properties ($\pi_{r,\text{dem}} < 0$), and Democrats also have a lower average income in our sample. This countervailing force slightly offsets the income-based resorting driven by prices.

Overall, the decomposition demonstrates that the redistributive impact of flood-risk disclosure primarily operates through the price channel, with preference heterogeneity playing a much smaller role. The uncalibrated estimates (Table 11) yield nearly identical conclusions, with price changes explaining 93.2% of the resorting, confirming that our results are not sensitive to calibration choices for the mean coefficients.

6 Conclusion

When improved information about a financial asset becomes widely available, it is important to understand not only its pricing implications but also how that information affects who holds it in equilibrium. In this paper, we study the disclosure of novel flood risk information in the housing market, *the* most important consumer finance market. Previously constrained by coarse and backward-looking risk categorization at the 100-year floodplain level, the disclosure of First Street Flood Factors on major online housing platforms marks the first time that prospective homebuyers can access granular, property-level, forward-looking flood risk information freely throughout the entire United States.

We are the first to study the equilibrium price and distributional effects of such large-scale disclosure in the consequential housing market. Our diff-in-RD research design allows us to credibly estimate the impact of First Street disclosure on housing prices, with an average negative effect of 3.3% for homes disclosed as having "extreme" flood risk. The effects are particularly strong for coastal properties, where we find a price drop of 11%, and are pervasive across homes in and out of floodplains. With detailed data linking each housing transaction with buyer demographics, we also find striking distributional effects. Buyers of riskier homes tend to be financially more constrained, with 5.3% lower

income, and are 1.8 percentage points more likely to hold an FHA loan; they are also, on average, 1.4 years older.

A structural model of housing choice allows us to decompose the drivers of post-disclosure household resorting into a price channel and a taste channel. Using the estimated model parameters, we find that the price effect, where lower-priced, riskier homes attract lower-income buyers, is the dominant driver of the redistribution of flood risk by income. On the other hand, the heterogeneity in the taste for flood risk also plays a role but to a much smaller degree. This finding of a regressive redistribution of risks, while fully consistent with an interpretation of improved allocative efficiency, also raises a question of climate justice: as environmental risks become more priced in, should they fall more on the shoulders of those who are least willing or able to pay to avoid them?

Ultimately, our research opens up several questions for future work. On the demand side, we focus on the intensive margin, specifically, who chooses to buy existing high-risk homes after disclosure. On the supply side, an important open question is how disclosure influences the extensive margin, or where new homes are built as risk becomes more accurately priced. In addition, better flood risk information may shape household decisions in other ways, such as demand for insurance, investment in adaptation measures, and broader migration patterns. We leave these topics for future research.

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Figures

Figure 1: A Timeline of the Disclosure of First Street Flood Factor on Housing Platforms

This figure shows the main timeline of when First Street Flood Factors were rolled out on Realtor.com, Redfin.com, and Zillow.com. The dotted vertical lines and gray arrows further show the pre-period and post-period chosen for the main specification of our empirical diff-in-RD analysis.

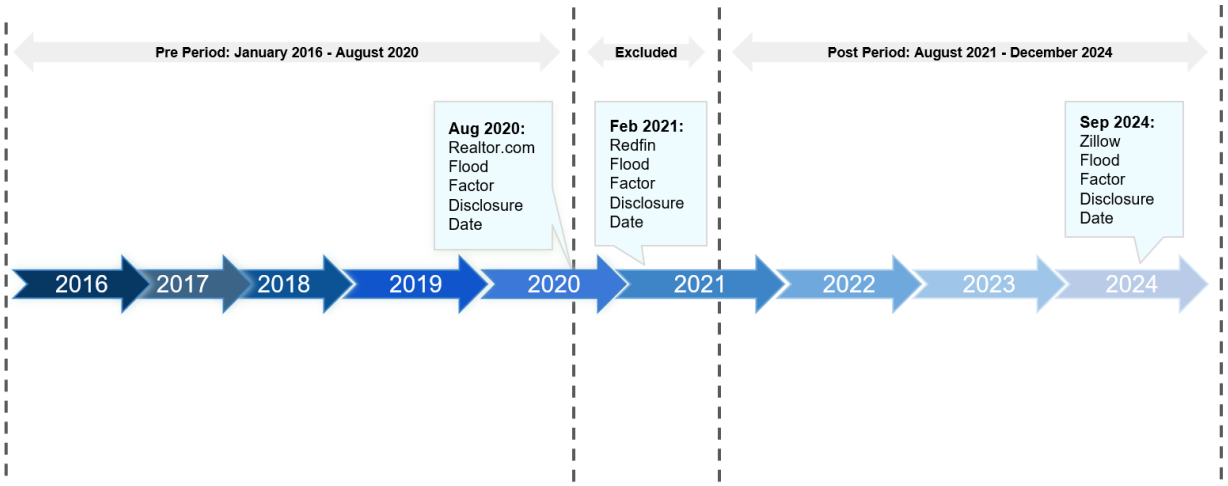


Figure 2: An Example of First Street Flood Factor Shown on Redfin.com

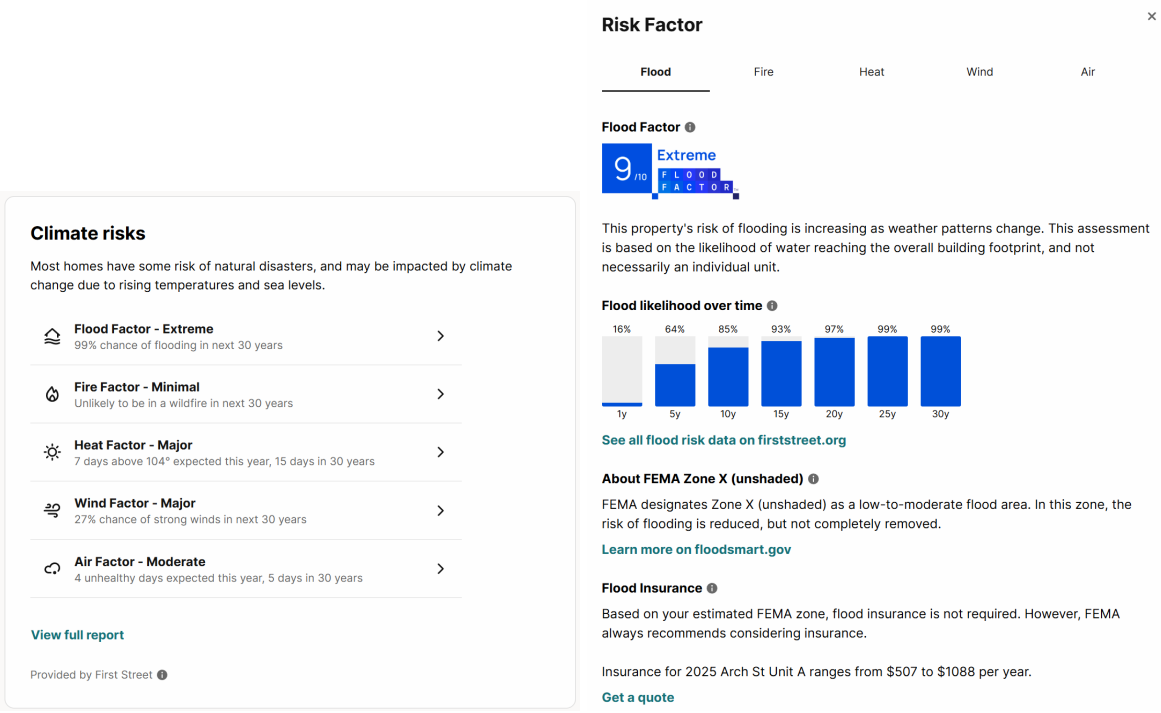


Figure 3: Mapping of Continuous Risk to Discrete Flood Factor Scores

An underlying, continuous measure of flood risk, measured by the log of *expected flood depth* (x-axis), is mapped into the discrete Flood Factor categories published by First Street (y-axis) and disclosed on housing platforms. The expected flood depth is computed based on the sum of the flooding depth across a series of return periods weighted by their respective probabilities.

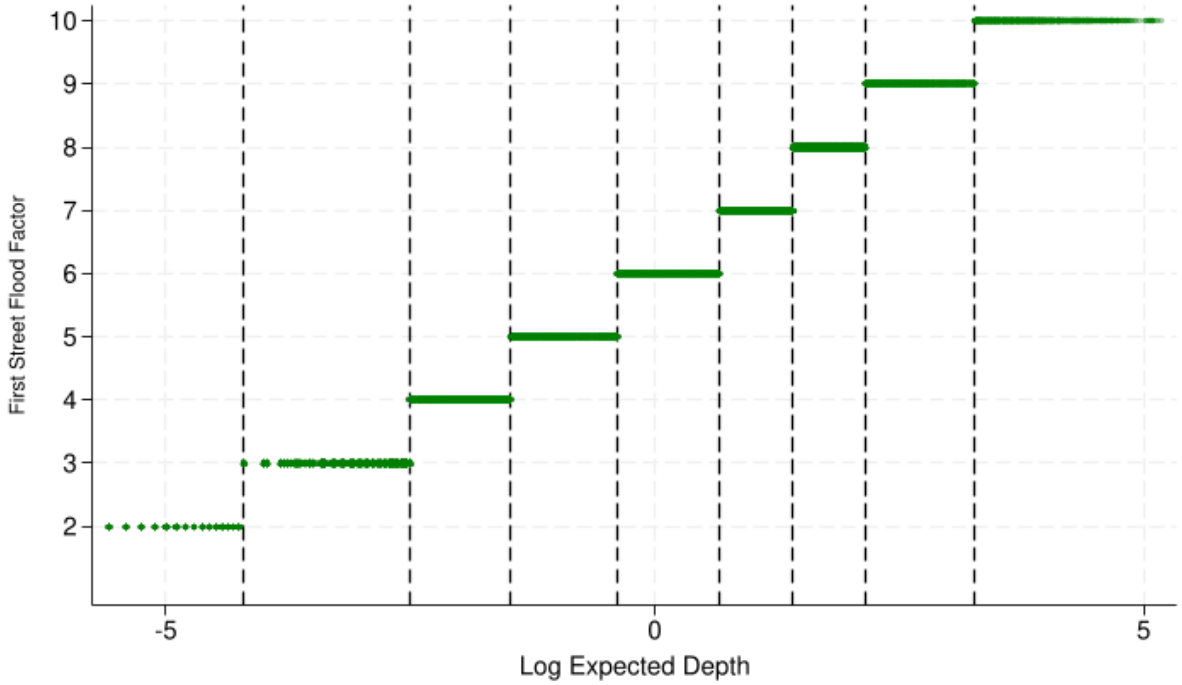


Figure 4: Distribution of Homes by Distance to the Cutoff (Bunching Test)

The figure shows the distribution of the running variable (recentered log expected flood depth) around the Flood Factor 8 vs 9 cutoff. Each bar represents the frequency of observations within equal-width bins. The vertical dashed line shows the recentered cutoff at zero. This plot is used to assess whether the running variable is smoothly distributed around the cutoff, providing a visual diagnostic for potential sorting or manipulation. The sample includes all housing transactions in the post-disclosure period from August 2021 to December 2024.

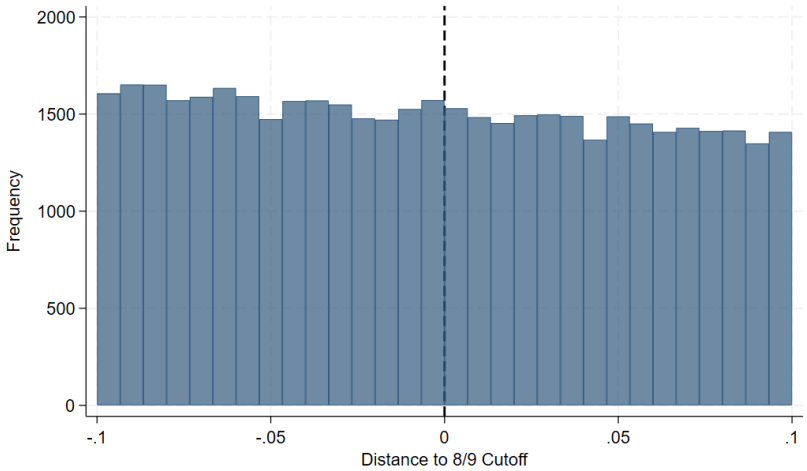
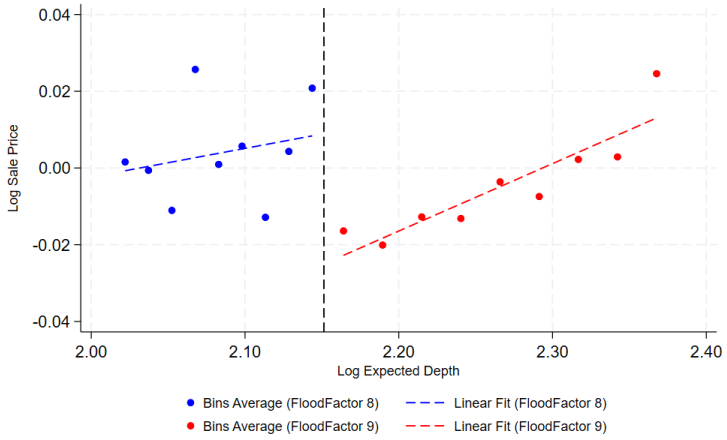


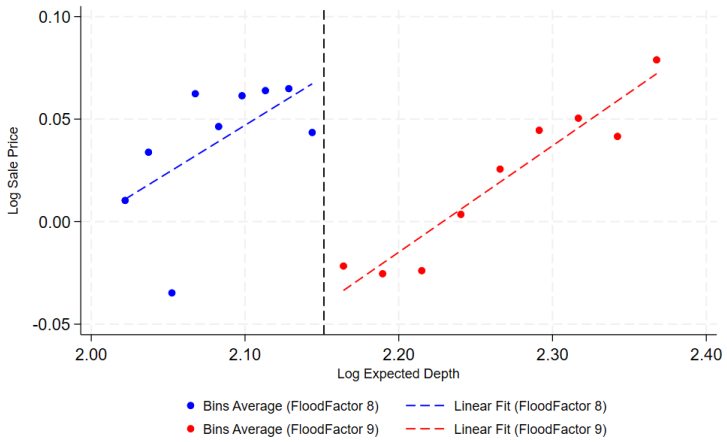
Figure 5: Difference-in-RD Plots for Price

The figures below show the difference in outcomes across the 8/9 cutoff with auto-selected bandwidths. Panel (a) uses the full data sample, panel (b) uses the coastal sample, and panel (c) uses the non-coastal sample. The dependent variable, log home price, is residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the difference between the average residualized log home prices in the post-period minus the average residualized log home prices in the pre-period. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024).

(a) Log Price: Full Sample



(b) Log Price: Coastal Sample



(c) Log Price: Non-Coastal Sample

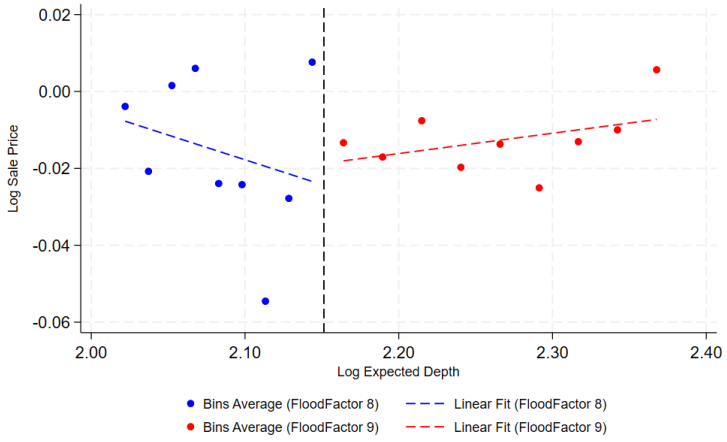
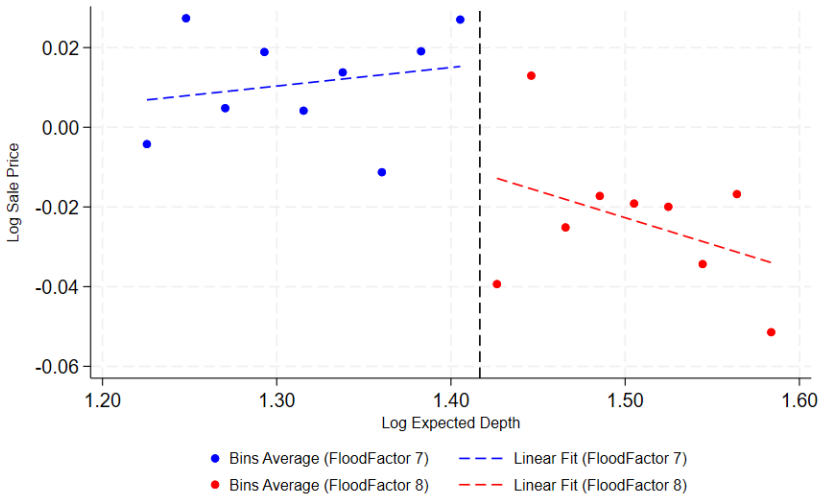


Figure 6: Difference-in-RD with Alternative Flood Factor Cutoffs

The figures below show the difference in outcomes across various alternative cutoffs with auto-selected bandwidths. The dependent variable, log home price, is residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the difference between the average residualized log home prices in the post-period minus the average residualized log home prices in the pre-period. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024).

(a) Flood Factor 7 vs 8



(b) Flood Factor 9 vs 10

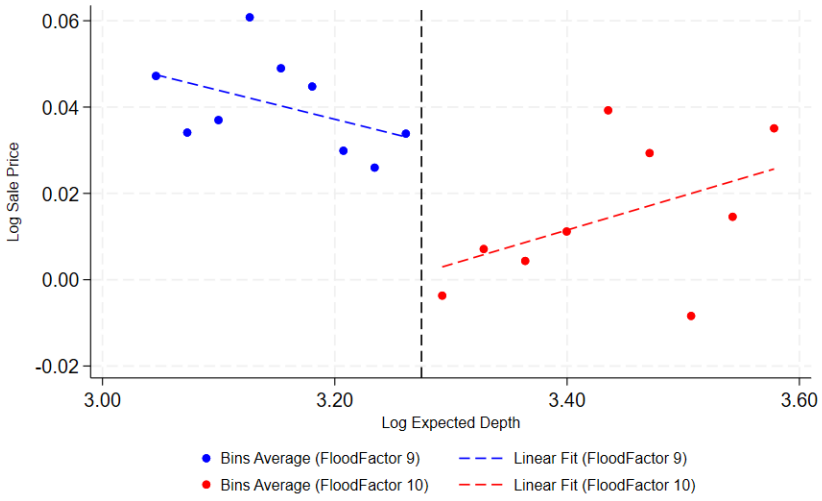


Figure 7: Dynamic RD for Log Home Prices

The figure below plots the coefficients (β_1) and standard errors from a dynamic RD specification at the 8/9 cutoff. Each point corresponds to a sample that starts at the date shown on the x-axis and ends one year later. Our data comes from First Street and CoreLogic, covering 2016 to 2024. The estimating equation is

$$y_{j,t} = \sum_{\tau} \left(\alpha_\tau + \beta_{1,\tau} \mathbb{1}\{R_j > c\} + \beta_{2,\tau} R_j + \beta_{3,\tau} \mathbb{1}\{R_j > c\} \times R_j \right) \times \mathbb{1}\{t = \tau\} + \varepsilon_{j,t}.$$

The dependent variable, $y_{j,t}$, is log home price residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. The running variable, R_j, is log expected flood depth.

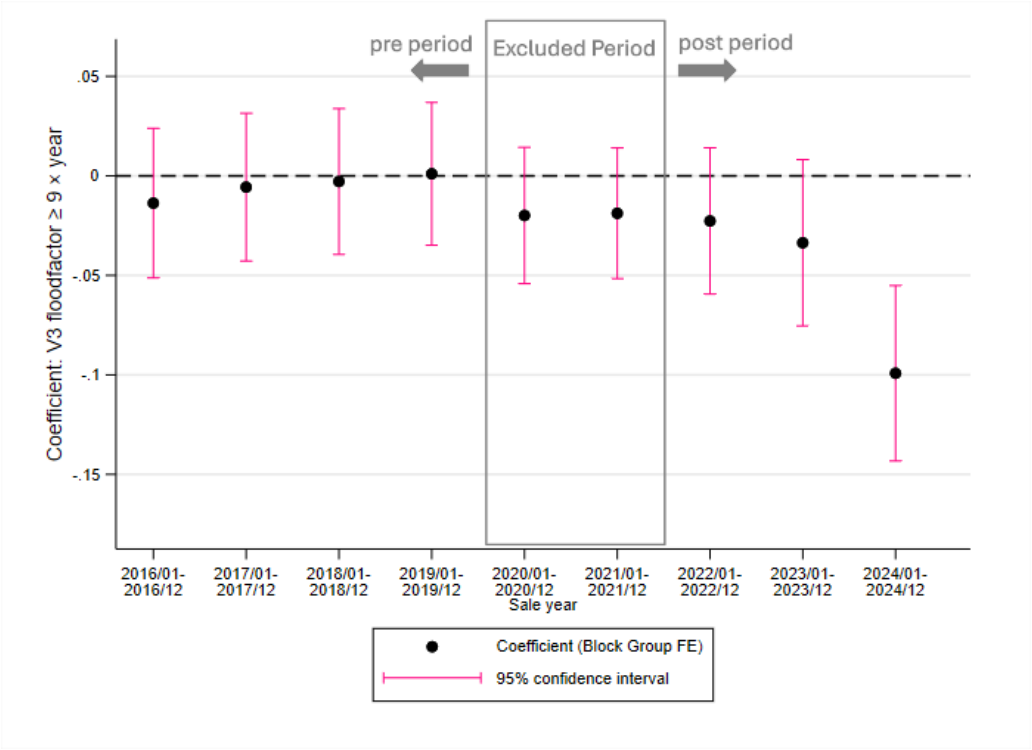
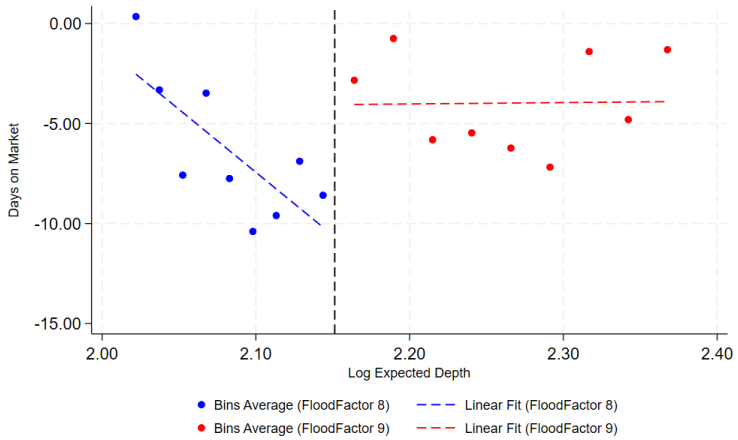


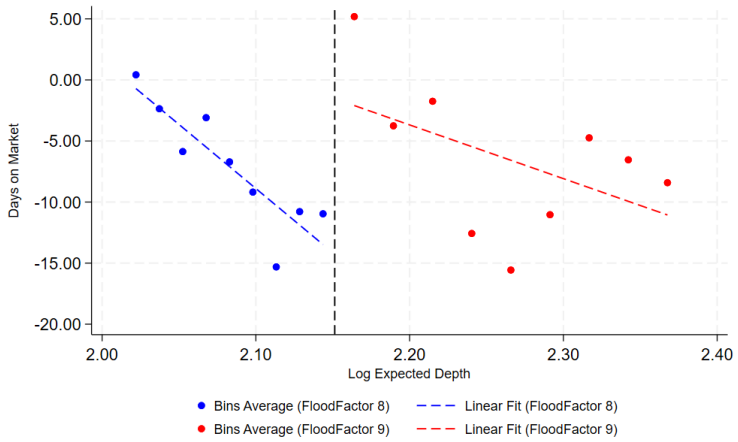
Figure 8: Difference-in-RD Plots for Days on Market

The figures below show the difference in days-on-market across the 8/9 cutoff with auto-selected bandwidths. Panel (a) uses the full data sample, panel (b) uses the coastal sample, and panel (c) uses the non-coastal sample. The dependent variable, days-on-market (DOM), is residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the difference between the average residualized outcome in the post-period minus the average residualized outcome in the pre-period. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024).

(a) Days on Market: Full Sample



(b) Days on Market: Coastal Sample



(c) Days on Market: Non-Coastal Sample

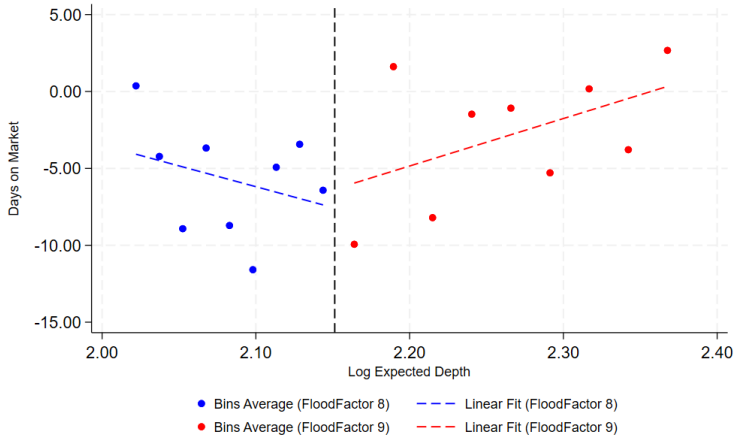
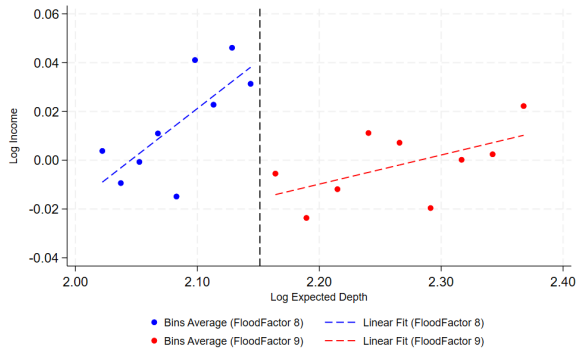


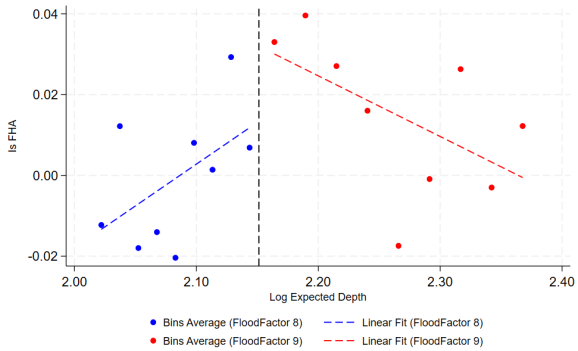
Figure 9: Difference-in-RD Plots for Income, Age, Democrat, and FHA Status

The figures below show the difference in various household attributes across the 8/9 cutoff with auto-selected bandwidths. For panels a-d, the dependent variables are log income, a dummy for whether the home was purchased using an FHA loan, average household age, and a dummy for a registered Democrat in the household, respectively. Each dependent variable is residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the difference between the average residualized outcome in the post-period minus the average residualized outcome in the pre-period. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024).

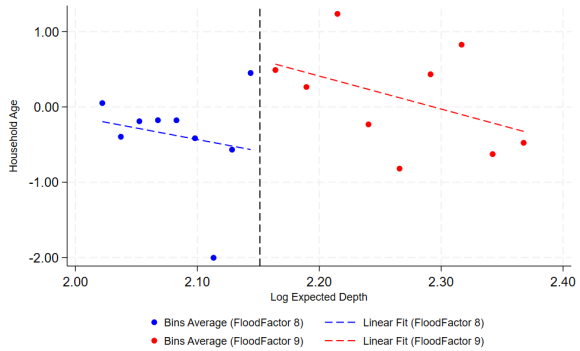
(a) Log Income



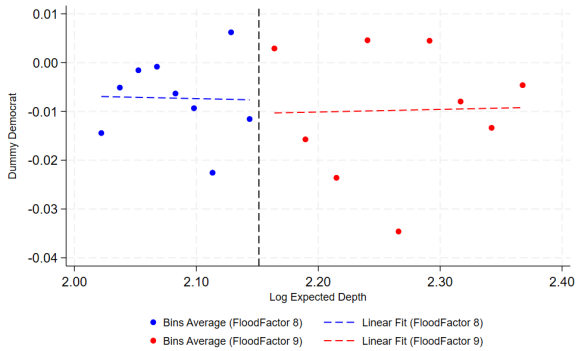
(b) FHA Mortgage (Dummy)



(c) Average Household Age



(d) Has a Registered Democrat (Dummy)



Tables

Part 1. Summary Statistics

Table 1: Summary Statistics

This table summarizes the key variables used in our DID and diff-in-RD Regressions. Flood risk data is sourced from First Street, while housing price and property characteristics data come from CoreLogic. Our demographic variables come from HMDA and L2. Our data sample covers the years 2016 to 2024. We observe fewer observations for the expected-depth variable because properties classified as minimal flood risk have no expected-depth values.

	Count	Mean	SD	p25	p50	p75
CoreLogic:						
Owner Transfer:						
Sale Price ($k)	25,677,071	372.17	349.62	172.00	280.00	439.90
Living Area (Sqft)	25,677,071	1,785.64	992.81	1,232.00	1,680.00	2,289.00
Lot Size (Sqft)	25,677,071	20,063.94	37,422.48	6,325.00	9,400.00	16,117.00
Number of Bathrooms	25,677,071	1.97	1.23	1.00	2.00	3.00
Number of Bedrooms	25,677,071	2.27	1.66	0.00	3.00	3.00
Building Age	25,677,071	32.50	30.53	6.00	25.00	52.00
Sale Year	25,677,071	2,019.70	2.58	2,017.00	2,019.00	2,022.00
Mortgage:						
Loan Type: Is FHA	14,087,764	0.16	0.36	0.00	0.00	0.00
MLS Listing:						
Days on Market	10,144,325	47.17	59.62	7.00	24.00	63.00
First Street:						
V3 Expected Depth (cm)[25]	4,809,448	4.69	11.31	0.12	0.76	3.83
Flood Source include Coastal	4,809,448	0.25	0.43	0.00	0.00	0.00
Flood Source include Pluvial	4,809,448	0.69	0.46	0.00	1.00	1.00
Flood Source include Fluvial	4,809,448	0.27	0.44	0.00	0.00	1.00
HMDA Variables:						
Applicant Race: NonWhite	8,362,975	0.39	0.49	0.00	0.00	1.00
Applicant Race: Is Black	8,362,975	0.08	0.27	0.00	0.00	0.00
Applicant Race: Is Hispanic	8,362,975	0.13	0.34	0.00	0.00	0.00
Applicant Income ($)	8,362,975	128,936.07	143,332.12	65,000.00	98,000.00	150,000.00
Voter Registration (per household):						
Average Age	10,970,839	43.71	13.83	33.00	40.00	52.00
Dummy for a Registered Democrat	10,970,839	0.39	0.49	0.00	0.00	1.00
Dummy for a Registered Republican	10,970,839	0.39	0.49	0.00	0.00	1.00

[25] After merging CoreLogic and First Street, we have a sample with 25,677,071 properties. Properties classified as having minimal flood risk (i.e., Flood Factor 1) have no flood depth value (interpreted as zero) and no defined flood source.

Table 2: Summary Statistics - Flood Factor

This table tabulates homes' First Street risk factor against whether they are classified as being in an SFHA. Each observation is one house sale transaction. Flood risk data and SFHA designations are sourced from First Street, while home sale data comes from CoreLogic. Our data sample covers the years 2016 to 2024.

First Street Flood Factor	First Street Risk Category	Count	Fraction	Fraction in SFHA
1	Minimal	20,867,623	81.27%	0.73%
2	Minor	79,439	0.31%	4.83%
3	Moderate	618,135	2.41%	4.33%
4	Moderate	985,266	3.84%	6.49%
5	Major	613,168	2.39%	7.96%
6	Major	912,544	3.55%	9.84%
7	Severe	445,763	1.74%	14.10%
8	Severe	478,213	1.86%	21.38%
9	Extreme	472,380	1.84%	44.78%
10	Extreme	204,540	0.80%	65.07%
Total		25,677,071	100%	3.484%

Table 3: Correlative Evidence based on DID Regression

This table presents progressive difference-in-difference regressions of the log of sale amount on FloodFactor. Regression data is constructed from Flood Factor (1–10), across the pre-period (January 2016 to August 2020) and post-period (August 2021 to December 2024). FloodFactor groups properties into six categories: Minimal (1), Minor (2), Moderate (3-4), Major (5-6), Severe (7-8), and Extreme (9-10). Observations with sale prices below $50,000 and properties with more than one building are excluded, and sale prices are winsorized at the 99th percentile. All hedonic controls are winsorized at the 1st and 99th percentile. Hedonic controls include building age bin, bedroom (1–6), bathroom (1–5), and log lot size. Flood risk data is sourced from First Street, while housing price and property characteristics data come from CoreLogic. Significance levels are denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	Log Sale Price					
	(1)	(2)	(3)	(4)	(5)	(6)
Risk is Above Severe × Post Disclosure	-0.00321	-0.0140***	-0.0118***	-0.0106***	-0.0103***	-0.00894***
	(-1.29)	(-7.10)	(-6.68)	(-6.04)	(-6.21)	(-5.37)
Risk is Above Major × Post Disclosure	0.00218	-0.00393**	-0.00159	-0.00143	-0.00153	-0.00107
	(1.06)	(-2.39)	(-1.12)	(-1.03)	(-1.18)	(-0.82)
Risk is Above Moderate × Post Disclosure	-0.00417**	-0.00137	0.00159	0.00163	0.00173	0.00194
	(-2.02)	(-0.85)	(1.17)	(1.22)	(1.38)	(1.55)
Risk is Above Minor × Post Disclosure	-0.0215***	-0.00575**	-0.00135	-0.00182	-0.00234	-0.00225
	(-6.19)	(-2.11)	(-0.60)	(-0.83)	(-1.14)	(-1.10)
Risk is Above Minimal × Post Disclosure	0.0458***	0.00396	-0.00152	-0.00138	-0.000593	-0.000433
	(14.64)	(1.60)	(-0.74)	(-0.68)	(-0.31)	(-0.23)
Obs	24484572	24484453	24480349	24464328	24464328	24464328
R2	0.0465	0.4192	0.6187	0.6569	0.6973	0.6973
R2-Adj	0.0465	0.4191	0.6164	0.6511	0.6921	0.6921
County-Period FE		✓				
Tract-Period FE			✓			
Block Group-Period FE				✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓	✓
SFHA-Period FE						✓
Hedonic Controls					✓	✓

Part 2. Regression Discontinuity

Table 4: Diff-in-RD: The Impact of Flood Risk Disclosure on Sale Price

This table presents the diff-in-RD results examining the relationship between conditional log sale price and log expected flood depth after disclosure of First Street. Each panel represents a different pair of flood risks, and for each pair, we show the RD estimate from three specifications: a linear fit within an auto-selected bandwidth around the cutoff (column 1), a polynomial fit of order 2 (column 2), and a polynomial fit of order 3 (column 3). The outcome variable, conditional log sale price, is created by taking the residual from a regression of log price on year-month, hedonic, and block-group fixed effects. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Conditional log price is obtained by taking a residual after log price is regressed on stated fixed effects and hedonic controls. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Dep. Var: Conditional Log Sale Price	Linear (Auto Bandwidth) (1)	2nd Order (2)	3rd Order (3)
	Panel A: Risk Factor 3 vs 4		
FloodFactor = 4 × Post Disclosure	-0.0169	-0.0190**	-0.0271**
	(-1.59)	(-2.23)	(-2.44)
Obs	102503	246225	246225
	Panel B: Risk Factor 4 vs 5		
FloodFactor = 5 × Post Disclosure	-0.0106	-0.0075	-0.0049
	(-0.74)	(-0.73)	(-0.35)
Obs	60407	293224	293224
	Panel C: Risk Factor 5 vs 6		
FloodFactor = 6 × Post Disclosure	0.0067	-0.0036	-0.0022
	(0.64)	(-0.49)	(-0.22)
Obs	142019	487371	487371
	Panel D: Risk Factor 6 vs 7		
FloodFactor = 7 × Post Disclosure	0.0192	0.0169*	0.0184
	(1.57)	(1.86)	(1.49)
Obs	116132	481969	481969
	Panel E: Risk Factor 7 vs 8		
FloodFactor = 8 × Post Disclosure	-0.0266*	-0.0313***	-0.0320**
	(-1.95)	(-3.12)	(-2.35)
Obs	82580	358971	358971
	Panel F: Risk Factor 8 vs 9		
FloodFactor = 9 × Post Disclosure	-0.0330**	-0.0241***	-0.0302**
	(-2.32)	(-2.60)	(-2.39)
Obs	83027	481433	481433
	Panel G: Risk Factor 9 vs 10		
FloodFactor = 10 × Post Disclosure	-0.0335**	-0.0274***	-0.0443***
	(-2.06)	(-2.65)	(-3.21)
Obs	90382	409928	409928
Block Group FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Table 5: Subsample Analysis: Coastal Properties and SFHAs

This table presents the diff-in-RD estimates by geographic subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional log sale price is calculated as the residual from a regression of log price on year-month, hedonic, and block-group fixed effects. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. One-Way Subsamples

	Sub-Samples				
Dep. Var: Conditional Log Sale Price	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.0330**	-0.110***	0.00762	-0.0347	-0.0335**
	(-2.32)	(-4.60)	(0.43)	(-1.22)	(-2.05)
Obs	83027	26352	56675	22790	60237
BlockGroup FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Panel B. Two-Way (Coastal × SFHA) Subsamples

	Sub-Samples			
Dep. Var: Conditional Log Sale Price	SFHA-Coastal	SFHA-NonCoastal	NonSFHA-Coastal	NonSFHA-NonCoastal
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	-0.0662**	0.0378	-0.161***	0.000501
	(-1.98)	(0.72)	(-4.75)	(0.03)
Obs	14839	7951	11513	48724
Block Group FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓

Table 6: Balance Test for Covariate Variables

The table below checks covariate balance through the diff-in-RD estimates for various outcomes, including log lot size (sqft), log living area (sqft), number of bathrooms, number of bedrooms, and building age. In addition, in Panel B we check covariate balance for various indicators related to flood source, including indicators for whether the property is exposed to pluvial, fluvial, or coastal flooding, as well as an indicator for whether the property is in an SFHA. All specifications use a linear fit within the same bandwidth as the price regression around the Flood Factor 8/9 cutoff. All outcome variables are residualized on year-month and block-group fixed effects. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. Main Covariate Variables

	Full Sample				
Dep. Var:	Log Lot size	Log Living area	Num of bathrooms	Num of bedrooms	Building age
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.00996	0.0488	-0.00437	-0.00948	-0.522
	(-0.65)	(1.24)	(-0.20)	(-0.40)	(-1.03)
Obs	79382	79382	79382	79382	79382
BlockGroup FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓

Panel B. Flood Source Variables

Dep. Var:	Coastal	Pluvial	Fluvial	SFHA FloodZone
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	-0.000957	-0.00190	-0.000664	-0.00277
	(-0.61)	(-0.42)	(-0.19)	(-0.35)
Obs	79382	79382	79382	79382
BlockGroup FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓

Table 7: Robustness Check: Alternative Geographic Fixed Effects

This table presents the diff-in-RD estimates by alternative geographic fixed effects, including no geographic fixed effects, county fixed effects, census tract fixed effects, and census block-group fixed effects. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. The dependent variable, conditional sale price, is created by taking the residual from a regression of log price on year-month, hedonic, and varying geographic fixed effects. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

	Full Sample			
Dep. Var: Conditional Log Sale Price				
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	-0.0120	-0.0261	-0.0325**	-0.0330**
SE	(0.0233)	(0.0178)	(0.0148)	(0.0142)
t-stat	(-0.515)	(-1.463)	(-2.196)	(-2.323)
95%CI	[-0.0576,0.0336]	[-0.0611,0.0089]	[-0.0615,-0.0035]	[-0.0608,-0.0052]
Obs	83061	83061	83051	83027
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓
Level of Controls	None	County	Tract	Block Group

Table 8: The Impact of Disclosure on Days-on-Market, Income, and FHA Status

The panels below present the diff-in-RD estimates for various non-price outcomes, including days-on-market, buyer's income (log), and FHA status. Each panel represents a different outcome. For each, we show the RD estimate from three specifications: a linear fit within an auto-selected bandwidth around the cutoff (column 1), a polynomial fit of order 2 (column 2), and a polynomial fit of order 3 (column 3). Data are from First Street, CoreLogic, and HMDA, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Conditional outcome variables are obtained by taking the residual from a regression of the variable on year-month, hedonic, and block-group fixed effects. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. Days on Market

Dep. Var: Conditional Days on Market	Linear(Auto Bandwidth) (1)	2nd Order (2)	3rd Order (3)
FloodFactor = 9 × Post Disclosure	6.756**	5.363***	9.532***
	(2.36)	(2.85)	(3.72)
Obs	32994	200193	200193
Block Group FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Panel B. Log Income

Dep. Var: Conditional Log Income	Linear(Auto Bandwidth) (1)	2nd Order (2)	3rd Order (3)
FloodFactor = 9 × Post Disclosure	-0.0529**	-0.0247	-0.0438*
	(-2.07)	(-1.46)	(-1.91)
Obs	23600	132311	132311
BlockGroup FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Panel C. FHA Status

Dep. Var: Conditional Is FHA Dummy	Linear(Auto Bandwidth) (1)	2nd Order (2)	3rd Order (3)
FloodFactor = 9 × Post Disclosure	0.0180	0.0152*	0.0266**
	(1.27)	(1.73)	(2.22)
Obs	41883	236227	236227
Block Group FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Table 9: The Impact of Disclosure on Household Characteristics: Age and Dummy for Democrat

The panels below present the diff-in-RD estimates for various non-price outcomes, including average household age and an indicator for registered Democrat in the household. Column 1 does not include any income controls, while columns 2 through 4 control for income and income interacted with a post-disclosure dummy. The alternate regression discontinuity specifications are a polynomial fit of order 2 (column 3) and a polynomial fit of order 3 (column 4). All conditional outcome variables are obtained by residualizing the outcome variable on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. Average Household Age

Dep. Var: Conditional Household Age	Linear(Auto Bandwidth) (1)	Linear(Auto Bandwidth) (2)	2nd Order (3)	3rd Order (4)
FloodFactor = 9 × Post Disclosure	1.384**	2.014**	0.880	1.811**
	(2.21)	(2.38)	(1.63)	(2.47)
Obs	32381	14345	81436	81436
Control for Income		✓	✓	✓
BlockGroup FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓

Panel B. Dummy for a Registered Democrat in the Household

Dep. Var: Conditional Dummy for Democrat	Linear(Auto Bandwidth) (1)	Linear(Auto Bandwidth) (2)	2nd Order (3)	3rd Order (4)
FloodFactor = 9 × Post Disclosure	-0.00444	0.00777	0.00103	-0.0105
	(-0.44)	(0.35)	(0.07)	(-0.54)
Obs	83027	23600	132311	132311
Control for Income		✓	✓	✓
BlockGroup FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓

Table 10: Discrete Choice Model: Parameter Estimates

The table below shows the key estimation results of our discrete-choice housing model. For our estimation sample, based on empirical findings, we focus on the set of homes within the bandwidth near the 8 vs. 9 cutoff in 2024.

Parameter	Estimate	Std. Error	p-value
$\pi_{p,y}$	-2.1279	(0.2796)	0.0000
$\pi_{r,\text{age}}$	0.0749	(0.0438)	0.0876
$\pi_{r,\text{dem}}$	-0.0822	(0.1234)	0.5055

Table 11: Discrete Choice Model: Shapley Decomposition

This table reports the three-way Shapley decomposition of Cov(ln income, expected flood depth) into contributions from price changes, age-based preference heterogeneity, and political preference heterogeneity. "Calibrated" refers to estimates where the mean price coefficient is calibrated to match an elasticity of -0.67 Albouy et al. (2016) and the mean risky-property coefficient is calibrated to match an estimated -3.26% price effect. "Uncalibrated" reports raw GMM estimates. Expected flood depth is the continuous running variable measuring underlying flood risk.

Specification	Total	Price (%)	Age Hetero. (%)	Dem. Hetero. (%)
Calibrated	-0.0066	91.9	10.7	-2.6
Uncalibrated	-0.0067	93.2	9.7	-2.9

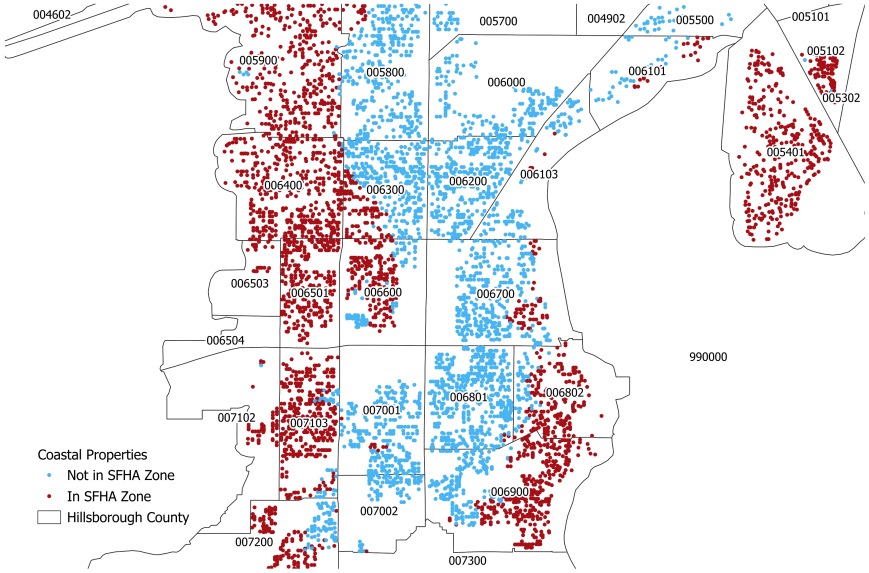
Internet Appendix

Part A: Geographic Maps

Figure A1: Flood Risk Categorization in Hillsborough County, FL

These maps display census tract-level shapefiles for a zoomed-in area of Hillsborough County, Florida. Panel (a) shows all properties subject to coastal flooding that are (not) in a designated SFHA as a red (blue) dot, while Panel (b) shows the same properties as dots colored based on the severity of their First Street Flood Factor.

(a) FEMA Flood Zone



(b) First Street Flood Factor

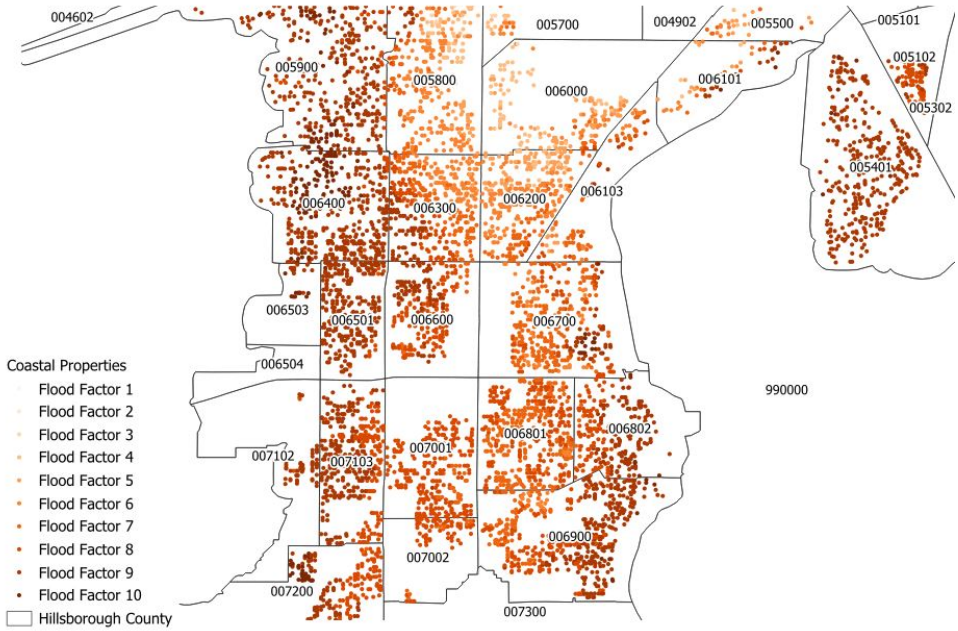
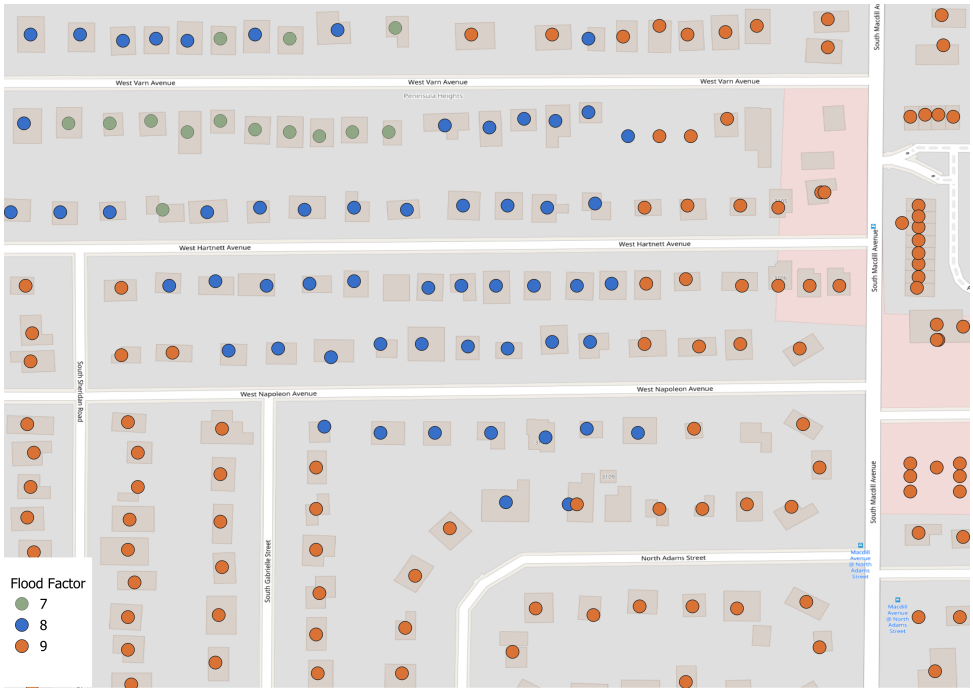


Figure A2: Close-up Map of Flood Risk Categorization in Hillsborough County, FL

These maps present a zoomed-in view of a small neighborhood in Hillsborough County, Florida, corresponding to a subset of the area shown in Figure A1. Panel (a) displays all properties as dots colored by the disclosed First Street Flood Factor. Panel (b) displays the same properties colored by the running variable (log expected flood depth), focusing on properties with Flood Factor 8 or 9.

(a) First Street Flood Factor



(b) Log Expected Depth

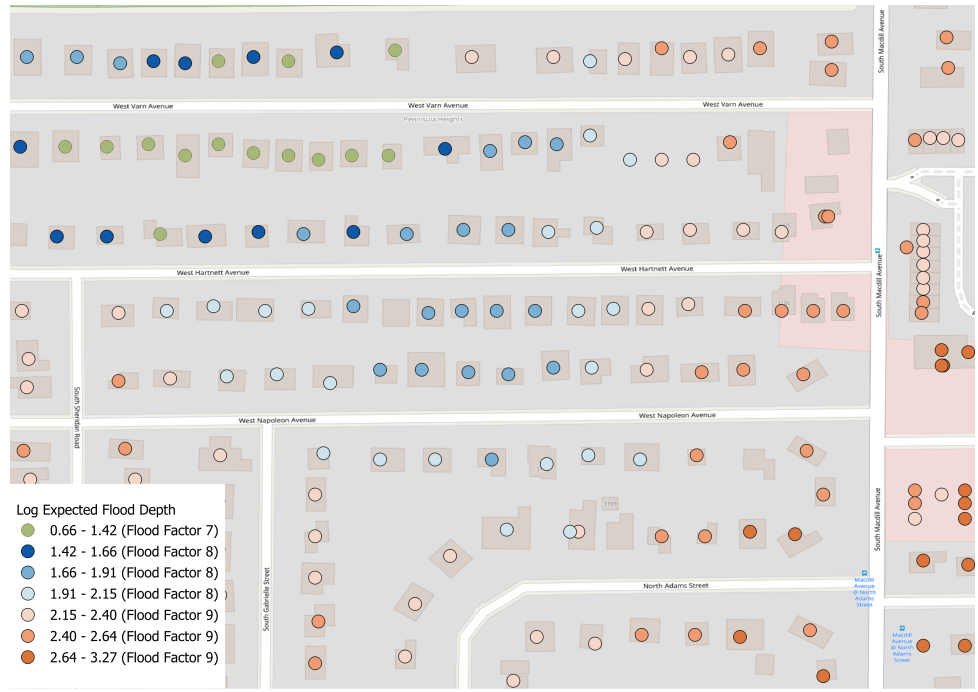
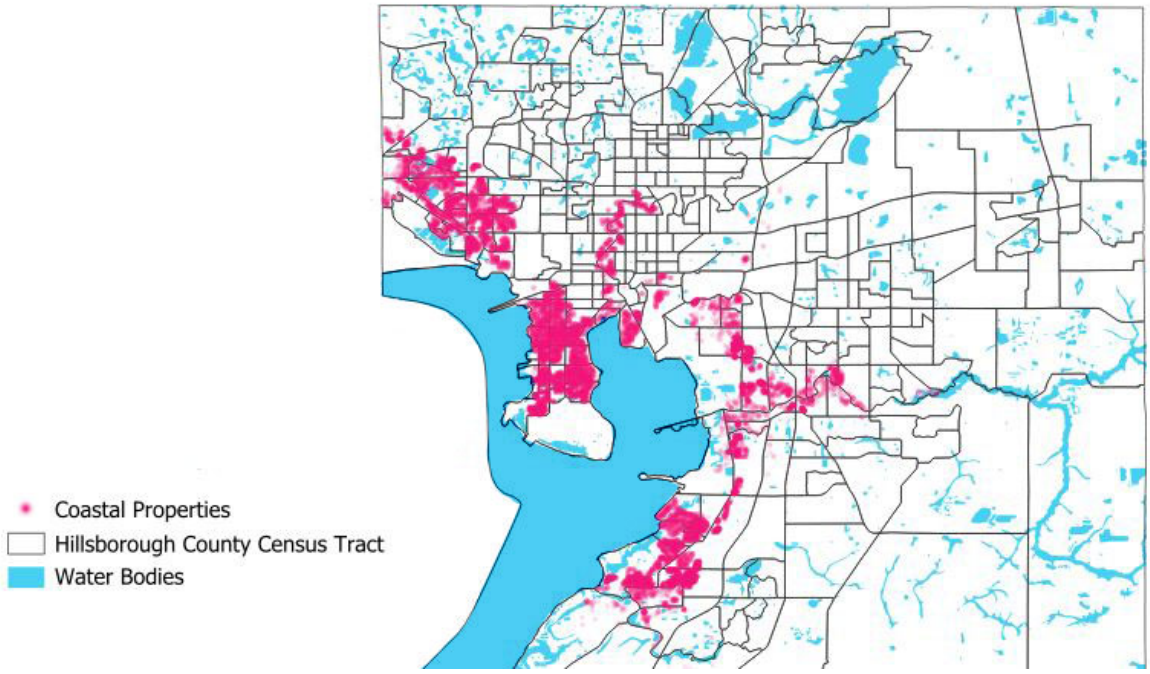


Figure A3: Map of Coastal Properties and Bodies of Water for Hillsborough County, FL

The map below displays the location of properties subject to coastal flooding risk in Hillsborough County, Florida. Each dot represents a property that is designated as being subject to "coastal" flooding by First Street; we also overlay local bodies of water in blue.



Part B: Additional Summary Statistics and Empirical Patterns

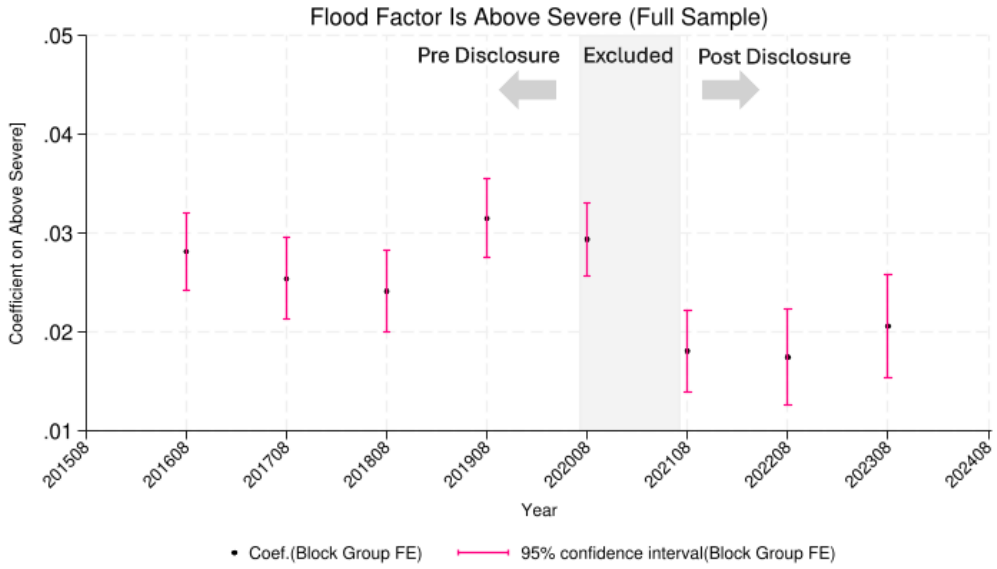
Table B4: Summary Statistics - Flood Source

This table reports the distribution of flood source classifications for properties in the sample with above-minimal flood risk (Table 1). Flood source codes follow First Street's classification of the dominant flooding mechanism affecting the property: 1 = pluvial; 3 = fluvial; 4 = fluvial and pluvial; 5 = coastal; 6 = coastal and pluvial; 8 = coastal and fluvial; 9 = coastal, fluvial, and pluvial. The count column reports the number of property transactions associated with each flood source category, and fraction reports the share relative to all transactions with above-minimal flood risk (i.e., those with flood source information). The total reflects the number of observations for which flood source information is available.

Source Code	Flood Source	Count	Fraction
1	Pluvial	2,465,296	51.26%
3	Fluvial	634,937	13.20%
4	Fluvial and pluvial	521,011	10.83%
5	Coastal	829,603	17.25%
6	Coastal and pluvial	216,804	4.51%
8	Coastal and fluvial	50,033	1.04%
9	Coastal, fluvial, and pluvial	91,764	1.91%
Total		4,809,448	100%

Figure B5: Dynamic DiD

To test whether there are notable pre-trends in the prices of flood-prone properties prior to disclosure, each point on the graph represents the average difference between properties with "extreme" risk (Flood factor = 9 and 10) relative to "severe" risk (Flood factor = 7 and 8) of the corresponding year, controlling for hedonics, year-month fixed effects, and block-group fixed effects. Our data comes from First Street and CoreLogic, and our data sample for this analysis covers the years 2016 to 2024.



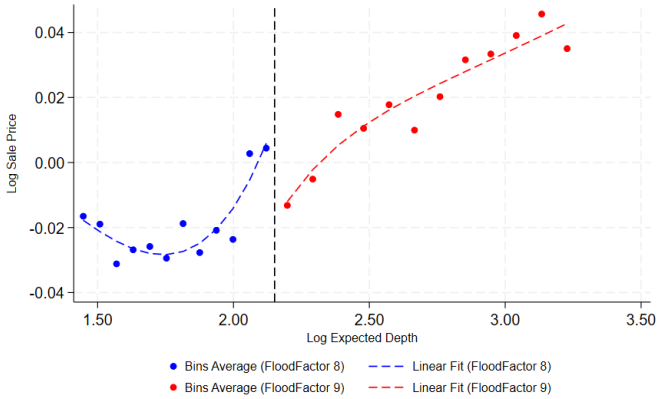
Part C: Additional RD Analyses

C.1 3rd Degree Polynomial Fit

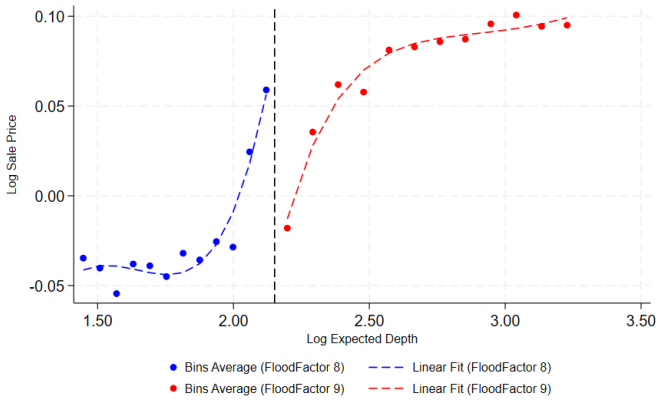
Figure C6: Full Sample RD Plots for Price: 3rd Polynomial Specification

The figures below show the difference in outcomes across the cutoff with 3rd order polynomials. Panel (a) uses the full data sample, panel (b) uses the coastal sample, and panel (c) uses the non-coastal sample. The LHS variable, log home price, is residualized with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the difference between the average residualized log home prices in the post-period minus the average residualized log home prices in the pre-period. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street and CoreLogic, covering the years from 2016 to 2024.

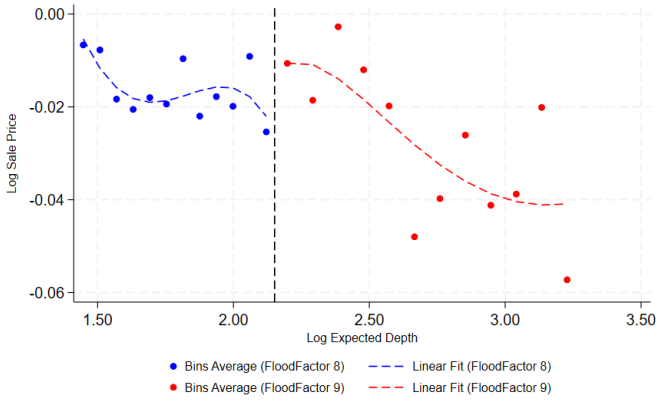
(a) Log Price: Full Sample



(b) Log Price: Coastal Sample



(c) Log Price: Non-Coastal Sample

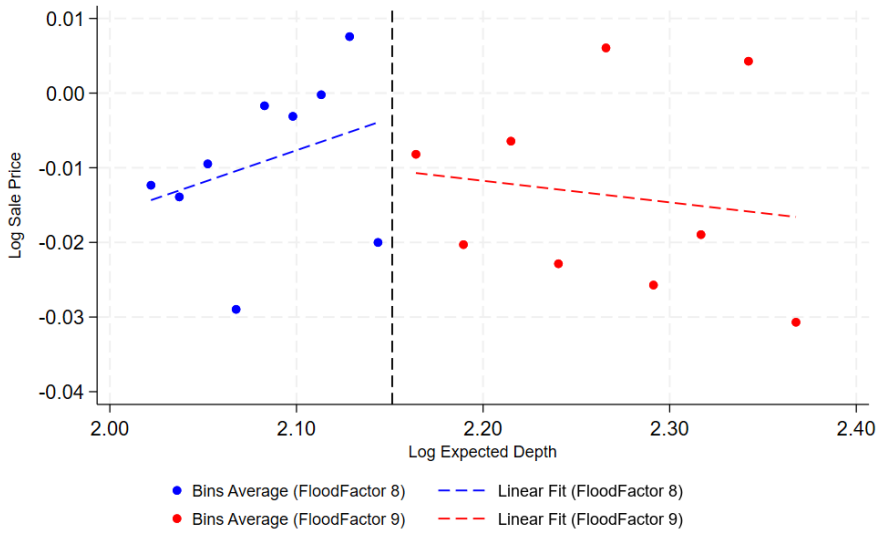


C.2 Standard RD instead of Diff-in-RD

Figure C7: Standard RD Output

The figures below show the average conditional log sale price across the cutoff within the optimal bandwidth. Panel (a) uses only the pre-period and panel (b) uses only the post-period. Conditional prices are obtained by residualizing log price with year-month fixed effects, hedonic characteristics, and block-group fixed effects. Each dot represents the average residualized log home prices. The point on the x-axis represents the midpoint of each equal-width bin. Our data comes from First Street and CoreLogic, covering the years from 2016 to 2024.

(a) RD in Log Price: Pre-Period Only



(b) RD in Log Price: Post-Period Only

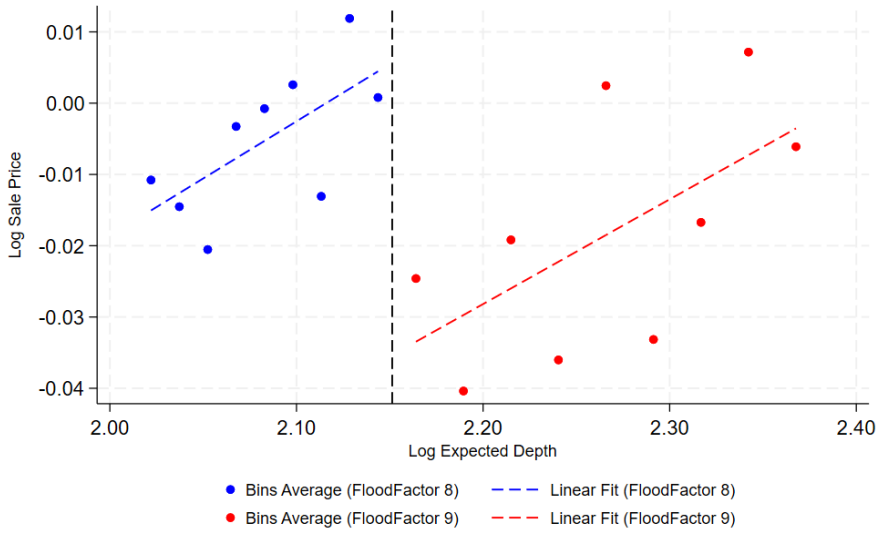


Table C8: Flood Risk Impact on Log Prices using Standard RD: Post-Period Only

The table below presents the standard RD estimates using the post-period only. We show the RD estimate from three specifications: a linear fit within an auto-selected bandwidth around the cutoff (column 1), a polynomial fit of order 2 (column 2), and a polynomial fit of order 3 (column 3). Conditional log home price is obtained by residualizing log sale price on year-month, hedonic, and block-group fixed effects. Data are from First Street and CoreLogic, covering only the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy and its interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

	Full Sample		
Dep. Var: Conditional Log Sale Amount	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)
FloodFactor = 9	-0.0405***	-0.0292***	-0.0348***
	(-3.42)	(-3.81)	(-3.33)
Obs	32297	189458	189458
BlockGroup FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

C.3 Subsample Analyses for All Non-Price Variables

Table C9: Subsample Analysis of Flood Risk Impact on Days on Market

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional days on market is obtained by taking the residual from a regression of days on market on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Days on Market	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	6.756**	13.07***	1.767	14.64***	3.037
	(2.36)	(2.84)	(0.49)	(2.67)	(0.91)
Obs	32994	13757	19237	10269	22725
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C10: Subsample Analysis of Flood Risk Impact on Log Income

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional log income is obtained by taking the residual from a regression of log income on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Log Income	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.0529**	-0.126***	-0.0233	-0.0491	-0.0528*
	(-2.07)	(-2.60)	(-0.78)	(-0.91)	(-1.82)
Obs	23600	6505	17095	5579	18021
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C11: Subsample Analysis of Flood Risk Impact on FHA Dummy

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional FHA is obtained by taking the residual from a regression of FHA on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

	Sub-Samples				
Dep. Var: Conditional isFHA dummy	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	0.0180	0.0561**	-0.00116	0.0463*	0.00750
	(1.27)	(2.31)	(-0.07)	(1.77)	(0.45)
Obs	41883	12445	29438	10822	31061
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C12: Subsample Analysis of Flood Risk Impact on Average Age

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional age is obtained by taking the residual from a regression of age on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

	Sub-Samples				
Dep. Var: Conditional Average Age	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	1.384**	1.348	1.401*	0.296	1.737**
	(2.21)	(1.20)	(1.86)	(0.23)	(2.42)
Obs	32381	9127	23254	7755	24626
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C13: Subsample Analysis of Flood Risk Impact on Dummy Democrat

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional democrat dummy is obtained by taking the residual from a regression of the democrat dummy on year-month, hedonic, and block-group fixed effects. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Dummy Democrat	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.00444	0.0131	-0.0131	0.00141	-0.00777
	(-0.44)	(0.82)	(-1.03)	(0.08)	(-0.64)
Obs	83027	26352	56675	22790	60237
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C14: Subsample Analysis of Flood Risk Impact on Dummy for Non-White

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional dummy for non-white is obtained by taking the residual from a regression of dummy for non-white on year-month, hedonic, and block-group fixed effects. Observations with missing race or ethnicity information are excluded. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Non-White Dummy	Full	Coastal Only	NonCoastal	SHFA Only	NonSHFA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	0.00223	0.0484	-0.0198	0.0312	-0.00811
	(0.09)	(1.12)	(-0.67)	(0.63)	(-0.29)
Obs	20199	5578	14621	4773	15426
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C15: Subsample Analysis of Flood Risk Impact on Dummy for Is Black

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional isBlack dummy is obtained by taking the residual from a regression of the isBlack dummy on year-month, hedonic, and block-group fixed effects. Observations with missing race or ethnicity information are excluded. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Is Black Dummy	Full	Coastal Only	NonCoastal	SHFA Only	NonSHFA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.00952	0.0125	-0.0199	0.0331	-0.0246
	(-0.68)	(0.44)	(-1.25)	(1.24)	(-1.50)
Obs	20520	5666	14854	4857	15663
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Table C16: Subsample Analysis of Flood Risk Impact on Dummy for Is Hispanic

This table presents the diff-in-RD estimates by subsamples. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Conditional isHispanic dummy is obtained by taking the residual from a regression of the isHispanic dummy on year-month, hedonic, and block-group fixed effects. Observations with missing race or ethnicity information are excluded. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

		Sub-Samples			
Dep. Var: Conditional Is Hispanic Dummy	Full	Coastal Only	NonCoastal	SHFA Only	NonSHFA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.0174	0.0361	-0.0432**	0.0306	-0.0341*
	(-0.98)	(1.09)	(-2.04)	(0.81)	(-1.69)
Obs	18021	5001	13020	4284	13737
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Part D: Directly Using Control Variables instead of Conditional Outcomes

Table D17: The Impact of Flood Risk Disclosure on Sale Price

This table presents the regression discontinuity results examining the relationship between log sale price and log expected flood depth after First Street. Each panel represents a different pair of flood risks, and for each pair, we show the diff-in-RD estimate from three specifications: a linear fit within an auto-selected bandwidth around the cutoff (column 1), a polynomial fit of order 2 (column 2), and a polynomial fit of order 3 (column 3). Unlike the baseline specification, the dependent variable is not residualized. Instead, property hedonic controls, year-month fixed effects, and block-group fixed effects are included directly in the regression. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Dep. Var: Log Sale Price	Linear (Auto Bandwidth) (1)	2nd Order (2)	3rd Order (3)
	Panel A: Risk Factor 3 vs 4		
FloodFactor = 4 × Post Disclosure	-0.0176	-0.0177**	-0.0313***
	(-1.51)	(-1.96)	(-2.66)
Obs	89531	229390	229390
	Panel B: Risk Factor 4 vs 5		
FloodFactor = 5 × Post Disclosure	-0.0272*	-0.0093	-0.0165
	(-1.74)	(-0.89)	(-1.17)
Obs	54096	275103	275103
	Panel C: Risk Factor 5 vs 6		
FloodFactor = 6 × Post Disclosure	-0.0044	-0.0031	-0.0063
	(-0.39)	(-0.40)	(-0.61)
Obs	124669	464434	464434
	Panel D: Risk Factor 6 vs 7		
FloodFactor = 7 × Post Disclosure	0.0280**	0.0122	0.0165
	(2.10)	(1.31)	(1.30)
Obs	101224	458565	458565
	Panel E: Risk Factor 7 vs 8		
FloodFactor = 8 × Post Disclosure	-0.0462***	-0.0402***	-0.0280**
	(-3.00)	(-3.84)	(-1.97)
Obs	69459	338381	338381
	Panel F: Risk Factor 8 vs 9		
FloodFactor = 9 × Post Disclosure	-0.0564***	-0.0294***	-0.0346***
	(-3.53)	(-3.08)	(-2.67)
Obs	70001	463132	463132
	Panel G: Risk Factor 9 vs 10		
FloodFactor = 10 × Post Disclosure	-0.0143	-0.0158	-0.0376***
	(-0.85)	(-1.54)	(-2.74)
Obs	81963	394769	394769
Block Group FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Table D18: Alternative Geographic Fixed Effects

This table presents the diff-in-RD estimates using alternate fixed effects. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Unlike the baseline specification, the dependent variable is not residualized. Instead, property hedonic controls, year-month fixed effects, and block-group fixed effects are included directly in the regression. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

	Full Sample			
Dep. Var: Log Sale Price				
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	-0.0157	-0.0265	-0.0583***	-0.0564***
SE	(0.0218)	(0.0171)	(0.0158)	(0.0160)
t-stat	(-0.722)	(-1.548)	(-3.703)	(-3.526)
95%CI	[-0.0584,0.0270]	[-0.0601,0.0071]	[-0.0892,-0.0275]	[-0.0878,-0.0251]
Obs	83061	82822	75656	70001
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓
Level of Controls	None	County	Tract	Block Group

Table D19: Subsample Analysis

This table presents the diff-in-RD estimates by subsamples. Unlike the baseline specification, the dependent variable is not residualized. Instead, property hedonic controls, year-month fixed effects, and block-group fixed effects are included directly in the regression. All specifications use a linear fit within an automatically selected bandwidth around the Flood Factor 8/9 cutoff. Data are from First Street and CoreLogic, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. One-Way Subsamples

	Sub-Samples				
Dep. Var: Log Sale Price	Full	Coastal Only	NonCoastal	SFHA Only	NonSFHA
	(1)	(2)	(3)	(4)	(5)
FloodFactor = 9 × Post Disclosure	-0.0564***	-0.145***	0.0194	-0.0759**	-0.0509***
	(-3.53)	(-5.99)	(0.91)	(-2.52)	(-2.67)
Obs	70001	25391	44525	19841	48559
Block Group FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓	✓

Panel B. Two-Way (Coastal × SFHA) Subsamples

	Sub-Samples			
Dep. Var: Log Sale Price	SFHA-Coastal	SFHA-NonCoastal	NonSFHA-Coastal	NonSFHA-NonCoastal
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	-0.104***	0.0369	-0.200***	0.0152
	(-3.04)	(0.57)	(-5.75)	(0.67)
Obs	14148	5684	10902	37608
Block Group FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓

Table D20: The Impact of Flood Risk Disclosure on Days-on-Market, Income, and FHA

The panels below present the diff-in-RD estimates for various non-price outcomes, including days-on-market, buyer's income (log), and FHA status. Each panel represents a different outcome. For each, we show the RD estimate from three specifications: a linear fit within an auto-selected bandwidth around the cutoff (column 1), a polynomial fit of order 2 (column 2), and a polynomial fit of order 3 (column 3). Unlike the baseline specification, the dependent variable is not residualized. Instead, property hedonic controls, year-month fixed effects, and block-group fixed effects are included directly in the regression. Data are from First Street, CoreLogic, and HMDA, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with log expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. Days on Market

	Full Sample		
Dep. Var: Days on Market	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)
FloodFactor = 9 × Post Disclosure	9.906***	4.760**	9.413***
	(2.74)	(2.27)	(3.29)
Obs	26150	188259	188259
BlockGroup FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Panel B. Log Income

	Full Sample		
Dep. Var: Log Income	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)
FloodFactor = 9 × Post Disclosure	-0.0887**	-0.0463**	-0.0742***
	(-2.43)	(-2.30)	(-2.73)
Obs	14753	114910	114910
BlockGroup FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Panel C. FHA Status

	Full Sample		
Dep. Var: Is FHA	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)
FloodFactor = 9 × Post Disclosure	0.0362*	0.0252**	0.0365***
	(1.90)	(2.51)	(2.68)
Obs	30301	217131	217131
BlockGroup FE	✓	✓	✓
Year-Month FE	✓	✓	✓
Hedonic Controls	✓	✓	✓

Table D21: The Impact of Flood Risk Disclosure on Household Characteristics: Age and Democrat

The panels below present the diff-in-RD estimates for various non-price outcomes, including average household age and an indicator for registered Democrat in the household. Column 1 does not include any income controls, while columns 2 through 4 control for income and income interacted with a post-disclosure dummy. The alternate regression discontinuity specifications are a polynomial fit of order 2 (column 3) and a polynomial fit of order 3 (column 4). Unlike the baseline specification, the dependent variable is not residualized. Instead, property hedonic controls, year-month fixed effects, and block-group fixed effects are included directly in the regression. Data are from First Street, CoreLogic, HMDA, and L2, covering both the pre-period (January 2016 to August 2020) and the post-period (August 2021 to December 2024). Suppressed coefficients include the flood factor dummy, post-disclosure dummy, and their interactions with expected flood depth. Significance levels are indicated by ***, **, and * for 1%, 5%, and 10%, respectively.

Panel A. Average Household Age

	Full Sample			
Dep. Var: Household Age	Linear(Auto Bandwidth)	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	2.183**	3.194**	1.526**	2.887***
	(2.51)	(2.34)	(2.23)	(3.12)
Obs	21698	7539	65973	65973
Control for Income		✓	✓	✓
BlockGroup FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓

Panel B. Dummy for a Registered Democrat in the Household

	Full Sample			
Dep. Var: Dummy Democrat	Linear(Auto Bandwidth)	Linear(Auto Bandwidth)	2nd Order	3rd Order
	(1)	(2)	(3)	(4)
FloodFactor = 9 × Post Disclosure	0.00342	0.0293	0.00462	-0.0128
	(0.28)	(0.88)	(0.27)	(-0.55)
Obs	70001	14753	114910	114910
Control for Income		✓	✓	✓
BlockGroup FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
Hedonic Controls	✓	✓	✓	✓