Information, Timing, and Risk Capitalization in Housing Markets

David Wilson*
Georgia Institute of Technology, Federal Reserve Bank of Atlanta

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Abstract

Does providing flood risk information before buyers submit offers change how that risk is capitalized into housing prices? I study South Carolina's 2013 disclosure reform, which required standardized pre-offer disclosure of flood zone status, prior flooding, and insurance information. Using property transactions from 2010 to 2018 linked to parcel-level flood risk measures, I estimate that floodplain property prices declined by approximately 7 percent relative to comparable properties following implementation of the reform. The reform also strengthened the negative relationship between flood risk and property values within floodplains. These results indicate that information frictions, in addition to risk preferences and amenity values, are an important and policy relevant driver of incomplete flood-risk capitalization in U.S. housing markets. The results highlight the potential for disclosure policies to improve the incorporation of natural disaster risk information into real estate prices.

JEL Codes: R31, Q54, D83, R38

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1 Introduction

Climate risk is increasingly embedded in household finances, mortgage markets, insurance portfolios, and public budgets, yet U.S. housing markets routinely fail to price flood exposure accurately (Hino and Burke, 2021; Giglio et al., 2021). The welfare consequences of this mispricing are large: households overpay for vulnerable homes and underinsure against losses (Gallagher, 2014); lenders originate and securitize mortgages against overvalued collateral (Gibson and Mullins, 2020); insurers cannot align premiums with risk when transaction prices do not reflect it (Kousky and Cooke, 2012); and governments absorb the residual cost through disaster relief and the National Flood Insurance Program (Michel-Kerjan and Kousky, 2010). Because housing investment and migration decisions respond to price signals, incomplete capitalization also slows climate risk adaptation by failing to discourage building in high-risk locations.

Despite extensive public information about flood hazards, housing markets often fail to fully price flood risk. Properties with high modeled damages frequently sell at small or negligible discounts, and coastal homes in high-risk zones often exhibit positive price premia (Bin and Polasky, 2004; Hino and Burke, 2021). This muted response is surprising given the availability of FEMA maps, mandatory National Flood Insurance Program (NFIP) insurance within flood zones, and repeated flood events that should reinforce the salience of risk. These patterns suggest that buyers either misunderstand or underweight flood risk, raising the question of whether standardized, transaction-level disclosure can correct these information failures. Recent experimental evidence shows that this second channel is not merely theoretical. Fairweather et al. (2024) uses a large scale randomized experiment on Redfin, assigning site visitors to see First Street flood risks scores alongside listings, and find that exposure to standardized risk information at the point of search causally shifts buyer attention and search behavior toward lower risk properties. This suggests that the timing and placement of risk information in the home buying process can shape housing demand even if that data is publicly available.

This paper asks a narrower version of that question in a market-price setting: does moving standardized flood risk information earlier in the transaction process increase the capitalization of flood risk into transaction prices? Distinguishing this information timing channel from insurance pricing and amenity-based explanations matters for policy, because the appropriate remedy differs sharply across mechanisms. If underpricing reflects subsidized insurance, the fix is actuarial reform; if it reflects preference-driven sorting toward water amenities, no intervention is warranted. However, if it reflects limited attention or late arriving information, low-cost disclosure requirements could meaningfully improve price discovery without touching insurance markets or the underlying hazard.

This paper studies a policy intervention that directly targets the third channel. Beginning in July 2013, South Carolina's 2013 statewide flood disclosure reform required sellers to disclose SFHA status, prior flood damage, and flood insurance history to buyers before an

offer could be submitted. This moved this information from the closing stage, where it had previously been enforced through mortgage underwriting, to the search and bargaining stages. Because the reform changed only the timing and delivery of information, rather than the underlying hazard, FEMA zone definitions, or federal insurance pricing, it offers a direct test of whether earlier disclosure increases risk capitalization independent of these competing explanations.

I examine this question using transaction-level single-family home sales from Cotality (2010–2018) linked to parcel-level FEMA flood-zone status and First Street Foundation flood-risk scores for South Carolina, excluding transactions closing between January 2013 and July 2014 to separate the disclosure form phase-in from the contemporaneous phase-in and rollback of federal insurance reforms. I implement three complementary designs: (i) a difference-in-differences comparison of floodplain and non-floodplain properties within South Carolina before and after the mandate; (ii) a triple-difference design benchmarking South Carolina against neighboring Georgia, which was exposed to the same federal insurance reforms but did not adopt a comparable disclosure requirement, to separate the disclosure effect from the federal insurance-cost shock; and (iii) a boundary-based design exploiting the discrete SFHA cutoff to limit comparisons to spatially proximate properties.

The results show that mandatory pre-offer disclosure meaningfully increased the capitalization of flood risk. Floodplain properties in South Carolina experienced an additional relative price decline of approximately 7 percent after the mandate, an effect that remains negative and statistically significant across boundary, donut, and triple-difference specifications, and that is attributable specifically to the state disclosure law rather than to the contemporaneous federal insurance reform. Disclosure also steepened the price-risk gradient within floodplains, indicating that the policy also improved the market's ability to differentiate risk within the SFHA. These effects are substantially larger in coastal markets and are concentrated outside the highest-income census tracts, which is consistent with disclosure mattering most where the gap between amenity value and flood risk is largest and where information frictions are most likely to bind on household decisions.

This paper's primary contribution is to islolate the timing of information provision as a distinct channel shaping flood risk capitalization, separate from changes in underlying hazard, floodplain designation, insurance pricing, or disaster salience. Doing so speaks to three literatures that I review in detail in Section 2: the capitalization of flood risk in housing markets, the design and effects of flood-disclosure policy, and the broader economics of salience and limited attention in asset pricing. Rather than restate that positioning here, I use Section 2 to situate the paper's identification strategy and findings within each literature.

The remainder of the paper proceeds as follows. Section 2 reviews related literature. Section 3 describes institutional background on flood insurance and South Carolina's disclosure mandate. Section 4 describes the data. Section 5 outlines the empirical strategy and presents the main results. Section 6 examines spatial and boundary robustness. Section 7 concludes.

2 Related Literature

This section situates the paper within three literatures and clarifies the specific empirical and conceptual gap it fills. Section 2.1 reviews evidence on the capitalization of flood and climate risk into housing prices which establishes the puzzle motivating this paper: properties with high modeled flood damages often sell at small or negligible discounts despite extensive public information on flood hazards. Section 2.2 reviews work on NFIP pricing, flood maps, and federal insurance reform, which raises an identification concern this paper must address directly, since South Carolina's disclosure reform coincided with the phase-in and rollback of federal insurance reform. Section 2.3 reviews the literature on disclosure, salience, and limited attention, including recent experimental and quasi-experimental evidence that information provision changes housing search and valuation behavior. Within this third literature, this paper's setting is distinguished by its focus on the timing of standardized disclosure relative to offer formation, rather than its scope, content, or delivery mechanism.

2.1 Flood and Climate Risk Capitalization in Housing Markets

A large literature examines whether flood risk is capitalized into residential property values. Early hedonic studies using cross-sectional variation in flood exposure find that properties located in floodplains often sell at a discount, although the magnitude of the discount varies across markets and depends on recent flood experience. Bin and Polasky (2004), for example, estimate that properties in the 100-year floodplain in coastal North Carolina sell at a discount, with stronger effects in areas with prior flood experience. Harrison et al. (2001) find that flood-zone designation reduces property values in Florida, while Atreya et al. (2013) document sizable post-flood discounts in Georgia that decay over time as memories of the disaster fade.

A related set of studies emphasizes that flood-risk capitalization depends on salience and experience. Bin and Landry (2013) show that flood-risk discounts increase after local flood events but dissipate as the salience of those events declines, suggesting that buyers update risk perceptions in response to recent storms but underweight risk once those experiences recede. Atreya and Ferreira (2015) find similar dynamics in Georgia, where homes that were visibly inundated during the 1994 flood sold at substantial discounts shortly afterward, with those discounts declining over time as the memory of the event faded. Gallagher (2014) documents analogous patterns in flood-insurance take-up: demand rises sharply after disasters but returns toward baseline within several years. Together, these findings suggest that buyers may respond strongly to flood risk when it is salient but underweight it during normal periods.

More recent work studies the capitalization of forward-looking climate risks, including sea-level rise. Bernstein et al. (2019) show that properties exposed to projected sea-level rise began selling at a discount only after climate risk became more publicly salient. Giglio

et al. (2021) review evidence on climate-risk pricing across asset classes and conclude that real estate markets increasingly incorporate climate exposure, though capitalization remains incomplete. Hino and Burke (2021) similarly argue that incomplete capitalization may reflect information frictions, limited attention, and behavioral biases rather than fully rational expectations.

This paper builds on the flood-risk capitalization literature by focusing on a policy change that altered the timing of information provision while leaving the underlying physical risk unchanged. Existing hedonic estimates often face two challenges: flood exposure is correlated with amenities such as water proximity, and cross-sectional price differences cannot distinguish rational capitalization from misinformation or limited attention. I contribute by isolating a policy-induced change in information timing, rather than relying on variation from disasters, map updates, or cross-sectional floodplain status. By exploiting South Carolina's disclosure reform, I estimate how housing prices respond when flood-risk information becomes salient before buyers submit offers, rather than later in the transaction process.

2.2 Flood Insurance, Flood Maps, and Institutional Distortions

A related literature examines how federal flood insurance policy and flood-zone regulation shape housing markets. The National Flood Insurance Program provides flood insurance in participating communities and historically offered subsidized premiums for many older structures. These subsidies can weaken the capitalization of true flood risk by lowering the private cost of locating in high-risk areas. Kousky and Cooke (2012) estimate sizable NFIP subsidies for pre-FIRM structures, while Michel-Kerjan and Kousky (2010) show that a small share of repeatedly flooded properties accounts for a disproportionate share of NFIP claims. These studies raise concerns that insurance subsidies may distort location decisions and property values in flood-prone areas.

Subsequent work examines market responses to reforms that changed expected insurance costs. The Biggert-Waters Flood Insurance Reform Act of 2012 sought to move some NFIP policies toward actuarial rates, while the Homeowner Flood Insurance Affordability Act of 2014 later slowed or reversed parts of that transition. Studies of these reforms find that changes in expected premiums can be capitalized into property values. Gibson and Mullins (2020) show that reductions in NFIP subsidies led to measurable price declines for affected New York properties, with comparable effects observed in high-risk flood zones. Hennighausen et al. (2023) provide further evidence that NFIP reforms affected both insurance and housing markets: they estimate that the 2012 and 2014 reforms increased flood-insurance prices, reduced insurance demand, lowered property prices, and reduced transaction volumes among affected properties. Their findings underscore that federal insurance reforms can change the expected cost of flood exposure and therefore represent an important contemporaneous force to separate from state-level disclosure effects.

This literature highlights an important identification challenge: changes in flood-zone prices may reflect insurance costs, information, updated maps, storm experience, or some

combination of these forces. The period surrounding South Carolina's disclosure reform overlapped with major federal NFIP reforms, including Biggert-Waters and the Homeowner Flood Insurance Affordability Act, as well as Hurricane Sandy, increased media attention to flood risk, and updates to FEMA flood maps in many communities. My setting helps separate these mechanisms. South Carolina's 2013 disclosure reform changed the timing of flood-risk disclosure in real-estate transactions but did not directly change NFIP premiums. By comparing South Carolina with neighboring Georgia, which was exposed to the same federal insurance environment but did not adopt the same disclosure reform, the triple-difference design helps isolate the effect of information disclosure from contemporaneous changes in insurance pricing. This distinction is important because the empirical window overlaps with NFIP reform; the cross-state design explicitly separates disclosure from insurance-cost shocks.

2.3 Disclosure, Salience, and Information Frictions

Mandatory disclosure laws are a common policy tool for reducing information asymmetries between buyers and sellers. In housing markets, disclosure may matter not only because it provides new information, but also because it reduces search costs and makes existing information salient at the moment of decision-making. Pope (2008) studies disclosure requirements for flood risk in North Carolina and finds that disclosure increased the capitalization of flood risk into housing prices. This evidence suggests that mandatory disclosure can affect prices even when flood-zone information is publicly available. However, because the implementation period coincided with major flood and hurricane events, the estimated effects may combine the impact of disclosure with disaster-induced changes in risk salience. Recent national evidence reinforces the importance of information frictions: Hino and Burke (2021) show that even salient updates to FEMA flood maps generate only modest price responses, suggesting that buyers under-react to publicly available flood-risk information.

Closest to this paper in spirit is Fairweather et al. (2024), who partner with Redfin to randomize the display of First Street flood-risk scores to home shoppers browsing listings. They find that exposure to standardized, parcel-level risk information shifts search behavior and willingness to view and bid on high-risk properties, providing direct experimental evidence that limited information, rather than fully informed preferences for risk, constrains flood-risk capitalization. This result is important for interpreting my setting: it establishes that when continuous, parcel-level flood risk is made salient to shoppers their behavior changes, which supports treating South Carolina's disclosure mandate as an information-timing shock rather than a change in underlying preferences or amenities. Fairweather et al. (2024) study randomized platform-based information provision, whereas this paper studies a statewide legal disclosure requirement embedded in real-estate transactions. Together, the two studies offer complementary evidence, experimental and quasi-experimental, that standardized flood-risk information affects housing market outcomes when it is made available before buyers form valuations.

A small but growing literature has since studied flood-risk disclosure more directly. McClain and Mota (2024) examine a Texas reform that expanded property disclosures from the 100-year floodplain to include 500-year floodplain exposure. Their setting provides evidence on how broadening the scope of disclosed flood risk affects housing-market outcomes. Chiang Hsieh (2021) studies flood-risk disclosure in Taiwan and shows that disclosure can affect real-estate markets outside the U.S. institutional context. These papers provide important evidence that flood-risk disclosure matters, but they study disclosure on margins that differ from the one considered here. In contrast, South Carolina's 2013 reform changed the timing of standardized flood-risk information within the transaction process, moving disclosure from a later stage to the pre-offer stage. This allows me to examine whether making flood information salient before buyers submit offers affects willingness to pay, while holding the underlying floodplain definition fixed.

This paper also relates to the broader literature on environmental and neighborhood disclosure. Mastromonaco (2015) finds that public disclosure of cancer clusters reduced nearby home values. Bui and Mayer (2003) show that property values around Superfund sites respond strongly to public announcements, suggesting that information timing can matter as much as the underlying environmental condition. Bae (2016) finds that the federal lead-paint disclosure rule reduced prices for older homes, indicating that standardized environmental disclosures can meaningfully affect housing-market outcomes. Related work on school accountability also shows that standardized public information can sharpen price gradients: Figlio and Lucas (2004) and Bento et al. (2015) find that report cards and school-quality disclosures strengthen the relationship between measured school quality and home prices.

The mechanism linking disclosure to prices is closely related to the literature on salience and limited attention. When information is costly to acquire or difficult to interpret, buyers may rely on heuristics or underweight low-probability risks. The behavioral literature shows that decision-makers often respond more strongly to information that is prominent at the point of choice. Chetty et al. (2009) show that consumers are more responsive to taxes displayed at the point of purchase, while Finkelstein (2009) finds that electronic toll collection reduces the salience of tolls and weakens behavioral responses. In financial markets, DellaVigna and Pollet (2009) and Barber and Odean (2008) show that limited attention affects trading and asset-price responses.

Flood risk is a setting in which salience is likely to be especially important. Flooding is a low-probability, high-consequence event, and buyers who have not recently experienced flooding may underweight risk when evaluating homes. Consistent with this view, prior studies show that flood-risk capitalization, insurance demand, and risk perceptions increase after disasters but decline as memories fade. Disclosure policy may therefore affect prices by making flood risk salient during search and bargaining, when buyers are forming willingness to pay.

This paper contributes to the disclosure and salience literatures by studying a reform that changed when flood-risk information was provided within the transaction timeline. Unlike settings in which disclosure reforms coincided with major flood events or expanded the

definition of disclosed risk, South Carolina's reform primarily shifted the timing of standardized disclosure. Relative to prior disclosure studies, the South Carolina reform identifies the effect of moving information before offer formation, rather than merely expanding the set of disclosed hazards. This setting allows me to test whether moving standardized flood-risk information to the pre-offer stage affects housing prices and steepens the price-risk gradient within flood-prone areas.

3 Background

This section describes the institutional setting governing flood insurance and risk disclosure in U.S. housing markets and the 2013 South Carolina reform that provides the basis for identification.

3.1 Flood Insurance and Disclosure Institutions

Flood insurance in the United States is primarily provided through the National Flood Insurance Program (NFIP), established by the National Flood Insurance Act of 1968 and administered by the Federal Emergency Management Agency (FEMA). The general purpose of the NFIP is both to offer primary flood insurance to properties with significant flood risk, and to reduce flood risk through the adoption of floodplain management standards. Communities volunteer to participate in the NFIP in order to have access to federal flood insurance, and in return are required to adopt minimum standards. FEMA delineates flood hazard zones through Flood Insurance Rate Maps (FIRMs), which designate Special Flood Hazard Areas (SFHA) as sites with at least a 1 percent annual probability of flooding. This designation is called a "100-year floodplain."

Federal law requires properties located within SFHAs to carry flood insurance if the purchase is financed with a federally backed mortgage. The Flood Disaster Protection Act of 1973 mandated that federal agencies, federally regulated lending institutions, and government sponsored entities must require these property owners purchase flood insurance as a condition of any mortgage that these entities make, guarantee, or purchase. Critically, this mandatory purchase requirement is enforced by the lender which means that enforcement is tied to the mortgage underwriting process. This administrative detail is central to the timing argument developed later in the paper, because compliance is checked by lenders rather than communicated by sellers. This means that mortgage-financed buyers typically learn of the SFHA designation and associated insurance obligation only once the mortgage underwriting is underway during closing. Cash buyers, who do not interact with a lender and are therefore not subject to the federal statutory rules, face no equivalent mechanism at any stage of the transaction.

The federal insurance mandate creates a regulatory distinction between how SFHA and non-SFHA properties are handled and generates predictable differences in expected costs of

ownership across the two groups. It is worth noting, however, that this binary regulatory distinction does not perfectly track underlying risk since the SFHA boundary may create a false belief that flood risk changes abruptly at the boundary and that properties outside the SFHA are markedly safer, thus not needing insurance coverage. However, about 40% of NFIP claims are for properties outside SFHAs.

A notable exception to this zone-based pricing regime occurred in 2012, when Congress passed the Biggert-Waters Flood Insurance Reform Act (BW-12) which authorized and funded the national mapping program and certain rate increases to ensure the fiscal soundness of the program by transitioning the program from artificially low subsidized rates to offer full actuarial rates reflective of risk. Because BW-12 was enacted roughly one year before South Carolina's 2013 disclosure mandate, its rate changes were being phased in during the same period as the reform studied here. However, this window was short-lived since the Consolidated Appropriations Act of 2014, which passed in October 2014, prohibited the implementation of certain sections of BW-12 effectively stopping certain rate increases while Congress worked to reconfigure new laws. Three months later, the Homeowner Flood Insurance Affordability Act of 2014 (HFIAA) repealed certain parts of Biggert-Waters restoring grandfathering insurance rates, limiting certain rate increases, and updating the approach to ensuring the fiscal soundness of the fund by applying an annual surcharge to all policyholders. As a result, the most significant BW-12 rate increases were in effect for only a narrow window overlapping the start of the South Carolina disclosure reform, and were substantially curtailed by early-to-mid 2014. For this reason, the main empirical specifications exclude transactions closing between January 2013 and July 2014. This omitted window removes the period during which BW-12 implementation, HFIAA rollback expectations, and the South Carolina disclosure-form transition were simultaneously affecting buyer and seller expectations.

This timeline raises a natural concern for identification: some portion of the estimated price response attributed to disclosure in the initial months of treatment could instead reflect buyers' and sellers' responses to federally mandated premium increases rather than to the earlier release of flood risk information. Because BW-12 and its subsequent rollback applied nationally rather than to South Carolina specifically, however, this concern can in principle be separated from the effect of the state disclosure mandate by benchmarking South Carolina's price response against neighboring states that were exposed to the same federal rate reform but not to South Carolina's disclosure law. Section 5.3 implements this comparison directly.

Many states do not require sellers to provide standardized information about flood zone status or prior flood damage before an offer is submitted (e.g., California and Texas require pre-offer disclosure; Georgia and Alabama do not). Mortgage financed buyers would typically learn of insurance requirements through the lending process, while cash buyers face no mandated disclosure at any stage of the transaction. Lenders, instead of sellers, typically communicate flood zone determinations to mortgage-financed buyers during underwriting or closing, usually after an offer had been submitted and often after inspections had been completed. At that point, buyers faced meaningful transaction costs of renegotiation or exit.

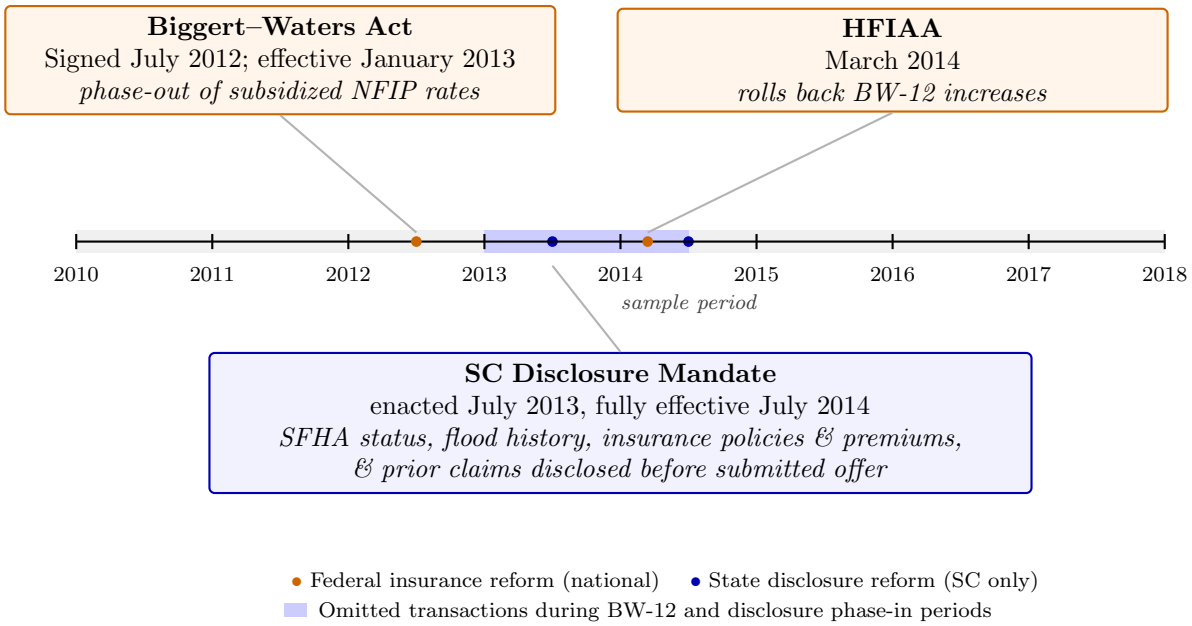


Figure 1: Timing of flood insurance and disclosure reforms. Federal reforms (top) changed the cost of flood insurance nationwide: the Biggert–Waters Act of 2012 began phasing out subsidized NFIP premiums, and the Homeowner Flood Insurance Affordability Act of 2014 reversed most of those increases. The South Carolina disclosure mandate (bottom), enacted July 2013 and fully effective July 2014, changed the information available to buyers in South Carolina only. The shaded band marks the phase-in period between enactment and full effectiveness; transactions closing in this window are excluded from the main estimates. Because the federal reforms applied uniformly on both sides of the state border, the cross-state triple-difference design helps separate the disclosure effect from contemporaneous insurance-cost changes.

As a result, the institutional structure ensured that parcel-level flood designation was often revealed late in the process and, for some buyers, not at all. This timing structure implies that although SFHA status carried financial consequences, it was not necessarily salient when buyers are selecting among competing properties and forming bids.

In the absence of a federal mandate for pre-offer flood risk disclosure, states have adopted heterogeneous disclosure regimes. Some states require sellers to disclose flood zone status before an offer is accepted, while others require only limited disclosure at or after contract signing. A smaller subset mandates disclosure of prior flood damages, insurance coverage, insurance premium amounts, or past insurance claims. Many states impose no flood-related disclosure obligations beyond general property condition statements. Figure 2 illustrates the geographic distribution and scope of these requirements, showing that only a minority of states require comprehensive pre-offer disclosure while most states cluster toward partial or minimal requirements.

This heterogeneity creates meaningful variation in the timing and completeness of risk information available to buyers. In states with comprehensive disclosure laws, sellers must provide standardized information prior to offer submission, ensuring that flood risk is known before prices are negotiated. In states without such requirements, flood risk information may reach buyers late in the transaction or not at all. The variation across states motivates

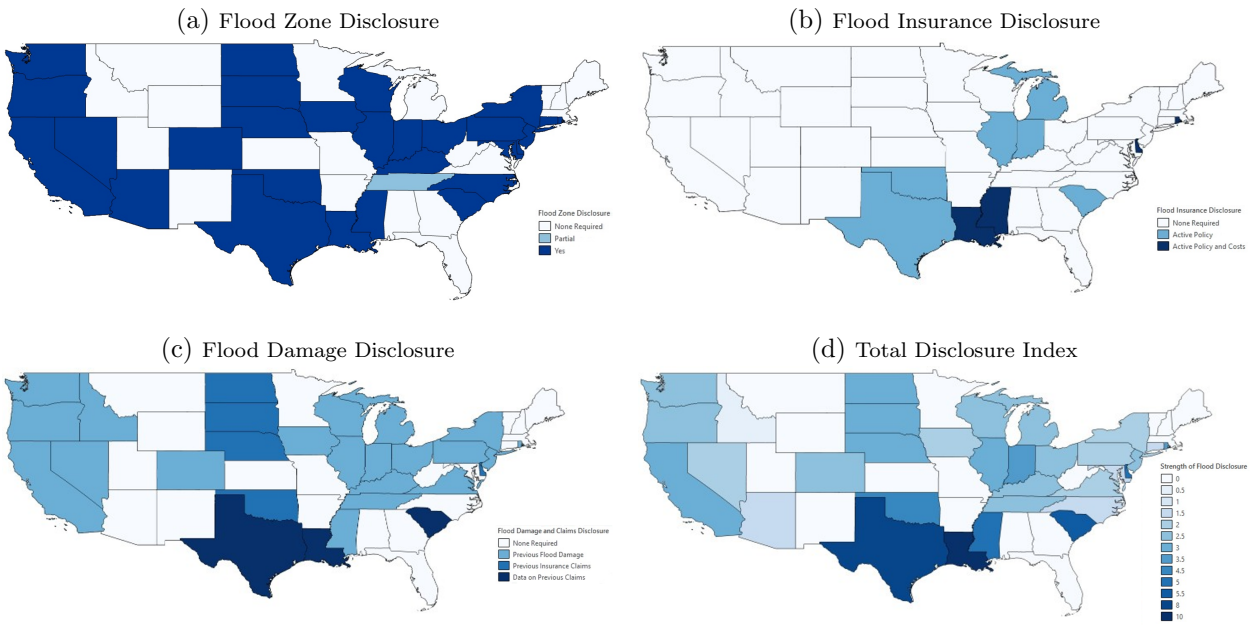


Figure 2: Geographic distribution of flood-related disclosure laws as of March 2023. Panel (a) shows whether states require sellers to disclose flood zone status to buyers. Panel (b) indicates requirements to disclose insurance coverage and premium costs. Panel (c) shows requirements to disclose prior flood damage and insurance claims. Panel (d) presents a composite index based on sum of disclosure requirements across all dimensions.

a natural question: does moving flood risk information earlier in the transaction timeline change how that risk is priced? The remainder of this section describes a policy change that isolates this timing shift while holding other features of the housing market fixed.

3.2 The 2013 South Carolina Reform

South Carolina provides a particularly clean setting for studying the effects of disclosure timing for three reasons. First, the reform altered when standardized flood information reached buyers without altering the underlying flood risk itself. Second, because the policy was implemented uniformly at the state level, it did not generate cross county variation in treatment intensity and thus avoids concerns about local policy endogeneity. Third, the timing of the reform was not triggered by a contemporaneous natural disaster or a sudden shift in insurance pricing specific to South Carolina. The one federal policy change coinciding with the reform window, BW-12, applied uniformly across all states rather than to South Carolina alone, which allows this concern to be addressed empirically, as discussed in Section 3.1.

In July 2013, the South Carolina Real Estate Commission adopted a revised property disclosure form requiring sellers to report: (i) whether the property lies within a FEMA designated SFHA, (ii) whether the property has experienced a prior flood, and (iii) whether a flood insurance policy was in place, including information about coverage and claims history.

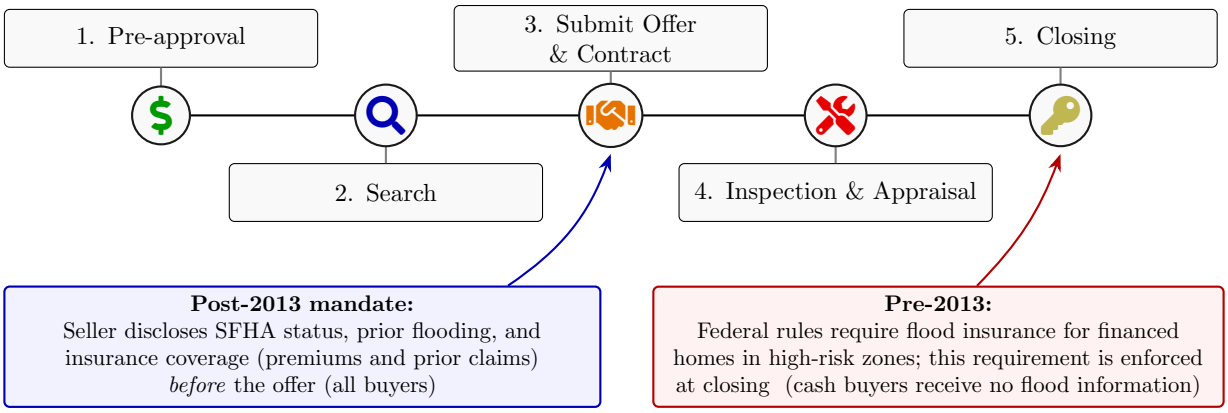


Figure 3: Timing of flood risk information in the home purchase process. Prior to July 2013, mortgage-financed buyers generally learned of SFHA status and flood insurance requirements during underwriting or closing, after transaction prices were largely determined, while cash buyers faced no comparable lender-based disclosure mechanism. The 2013 disclosure mandate requires sellers to provide SFHA status, prior flooding, and insurance coverage information including premiums and prior claims before an offer is submitted. This reform moves flood risk information ahead of price formation for all buyers and provides the basis for identifying the effect of flood risk disclosure on transaction prices.

Critically, the revised form had to be delivered to prospective buyers before submission of an official offer. Figure 3 summarizes the change in the timing of flood risk information within the home purchasing process. Prior to July 2013, standardized disaster disclosure occurred, if at all, through the lending process after contract formation. The reform moved standardized flood risk information forward in the transaction timeline, from the closing stage to the pre-contract stage, and applied uniformly to all buyers regardless of financing methods which eliminated the prior disparity between cash and mortgaged-financed buyers.

The policy included a one-year transition period during which the prior disclosure form remained valid, generating a gradual phase-in between July 2013 and July 2014. This transition coincided with the implementation and subsequent rollback of Biggert-Waters flood-insurance rate changes, making price changes during this window difficult to attribute uniquely to either the state disclosure mandate or federal insurance reform. To address this concern, I exclude transactions closing between January 2013 and July 2014 from the main estimating sample. The pre-period consists of transactions closing before January 2013, and the post-period consists of transactions closing after July 2014, when the revised disclosure regime was fully in effect. Conditional on this restriction, the statewide scope, exogenous timing of adoption, and uniform application across financing types make the reform well suited to identifying the effect of disclosure timing on price formation, rather than the effects of disaster-driven attention or contemporaneous federal insurance-rate changes.

4 Data

4.1 Single Family Home Transactions

The primary data source for this analysis is a comprehensive nationwide property-level dataset collected by Cotality, which aggregates information from local tax assessor records and Multiple Listing Service (MLS) databases. This dataset contains over 41.4 million arm's-length single-family home sales from 2000 through 2024. A key strength of this dataset is the inclusion of precise geocoded location coordinates for each property structure, which enables precise spatial merging with FEMA flood zone boundaries and structure-level risk data. The tax assessor records provide a rich array of structural and parcel characteristics that serve as hedonic controls in my analysis. These include: interior square footage, number of bedrooms and bathrooms, lot size (in square feet), year built (from which structure age is derived), property type classifications, zoning designations, and presence of features such as garages, pools, and basements. Additional variables include construction quality indicators which may influence both property values and flood vulnerability(i.e., roof type and exterior wall materials).

The MLS component of the data captures detailed transaction information, including initial listing prices, final closing prices, listing and closing dates (enabling calculation of days on market), and transaction types. I restrict the sample to arm's length transactions which excludes non-market transfers such as foreclosures, estate sales, inter-family transfers, and bulk sales to ensure that observed prices reflect genuine market valuations. Following standard practices in the hedonic literature, I also exclude properties with extreme values (top and bottom 1% of price per square foot) to minimize the influence of outliers or data errors.

For the South Carolina disclosure analysis, I use transactions from 2010 through 2018, excluding closings between January 2013 and July 2014. The resulting sample contains approximately 463,000 transactions. The pre-period consists of transactions before January 2013, and the post-period consists of transactions after July 2014, when the revised disclosure regime was fully in effect. This nine year window spans three years before and four years after the full implementation of the disclosure form in July 2014, providing sufficient pre-treatment periods to test for parallel trends and adequate post-treatment periods to estimate both the immediate impact and the dynamic evolution of the treatment effect over time. This choice of temporal window balances several considerations. The three pre-reform years (2010-2013) allows me to assess whether properties in flood zones and non-flood zones were following similar price trends prior to the policy change. Within this sample, approximately 17.6% of properties are located within SFHAs, which constitute our primary treatment group. Notably, SFHA properties in our sample are disproportionately located near water amenities such as rivers, lakes, and coastal areas, reflecting the spatial correlation between flood risk and desirable location characteristics.

To support my border discontinuity analysis, I calculate the distance from each property

to the nearest SFHA boundary using the precise geocoded coordinates and FEMA flood zone shapefiles. This enables me to construct increasingly restrictive samples that compare properties on either side of the SFHA boundary at varying buffer widths (1000, 750, 500, and 250 meters). Properties immediately adjacent to the SFHA boundary are plausibly similar in terms of observable and unobservable characteristics, including proximity to water amenities, while differing in regulatory flood zone designation and thus disclosure requirements. I also implement a donut difference-in-differences specifications that exclude properties within 25 or 50 meters of the SFHA boundary to address potential measurement error arising from GPS coordinate precision and flood map digitization.

4.2 Flood Risk Measurement

I measure flood exposure using two complementary sources that capture both the regulatory dimension of flood risk and the underlying physical hazard. First, FEMA Flood Insurance Rate Maps (FIRMs) identify whether a property lies inside or outside of a SFHA, defined as areas with a 1% or greater annual chance of flooding (the "100-year floodplain"). SFHA designation serves as the regulatory trigger for mandatory flood insurance purchase requirements under the NFIP for properties with federally backed mortgages and represents the primary disclosure unit under the South Carolina reform. I assign SFHA status to each property by spatially overlaying the geocoded property coordinates with digitized FEMA flood zone boundaries thus ensuring precise classification even for properties located near zone boundaries.

While SFHA designation is a binary regulatory indicator, properties within the SFHA vary substantially in their actual flood exposure depending on elevation relative to base flood levels, proximity to waterways, local topography, and hydrodynamic characteristics such as drainage patterns and soil permeability. Two adjacent properties within the same SFHA may face meaningfully different flood probabilities and expected damages. Conversely, properties just outside the SFHA boundary may still face non-trivial flood risk, particularly in areas vulnerable to pluvial (rainfall-driven) flooding not captured by riverine and coastal flooding.

To capture this heterogeneity in physical flood risk beyond the binary SFHA classification, I supplement the regulatory measure with a continuous parcel-level flood risk score derived from First Street Foundation's Flood Model. This proprietary risk score integrates multiple dimensions of flood hazard including expected inundation depth, frequency of flooding across multiple return periods, proximity to designated flood channels, and property specific elevation data. The flood risk measure is constructed using hydrodynamic modeling that accounts for precipitation patterns, storm surge potential, river discharge, terrain characteristics, and built environment features such as levees and drainage infrastructure. Importantly, the flood risk variable varies continuously both within and across FEMA flood zones, providing a more granular measure of actual flood exposure than the binary SFHA indicator alone.

For consistency across markets and to facilitate interpretation, I construct a standardized flood-risk index at the county level. Using the full stock of single-family homes in the

United States, I calculate each property's flood probability and standardize it relative to the distribution of flood risk among all single-family homes in the same county. Specifically, the index is a county-level z-score, where zero represents the average flood risk among the full county housing stock and one unit represents one standard deviation of flood risk within that stock. Positive values therefore indicate flood risk above the countywide average, while negative values indicate below-average risk. Because the index is normalized using the full housing stock rather than the estimation sample, its mean and variance need not equal zero and one among sold properties. In the South Carolina estimation sample, properties within SFHAs have a mean risk score of 5.06 (SD = 2.91) compared to the 2.17 (SD = 1.68) for properties outside SFHAs. These patterns show that SFHA status is strongly aligned with the continuous risk index, while also leaving substantial within-category variation.

Figure 4 illustrates the geographic distribution of properties and flood zone boundaries across South Carolina. The state's coastal geography and extensive riverine network produce substantial within-market variation in flood exposure, with SFHA and non-SFHA properties frequently located in adjacent subdivisions and, in many cases, on opposite sides of the same street. This fine-grained spatial interleaving of flood zone designations is particularly evident in coastal markets, including the Charleston and Myrtle Beach areas (a) and along major river corridors where SFHA boundaries often bisect neighborhoods based on subtle topographic features. Panels (c) and (d) provide zoomed-in views of a specific neighborhood in Myrtle Beach revealing sharp discontinuities in flood designations at highly localized boundaries. In some cases, properties separated by less than 50 meters differ in their SFHA status due to minor differences in elevation or proximity to flood channels. This granular spatial variation is a key feature of the data that supports my identification strategy: treated (SFHA) and control (non-SFHA) properties can be observed within the same census block groups, school attendance zones, and neighborhood markets sharing exposure to common local amenities, infrastructure, and housing market dynamics while differing in flood zone designation.

This spatial pattern has important implications for identification. Unlike settings where treated units are clustered in entirely different regions (e.g., coastal vs. inland), the interleaved geography of South Carolina's flood zones means that SFHA and non-SFHA properties often compete in the same local housing markets and appeal to similar buyer pools. This reduces concerns about confounding from unobserved neighborhood characteristics, though it does not eliminate the challenge posed by the correlation between flood risk and water amenities, which I address through the border discontinuity specifications described in Section 5.

5 Empirical Strategy & Results

The empirical strategy uses complementary quasi-experimental designs to estimate whether mandatory pre-offer disclosure changed the capitalization of flood risk in housing prices. I

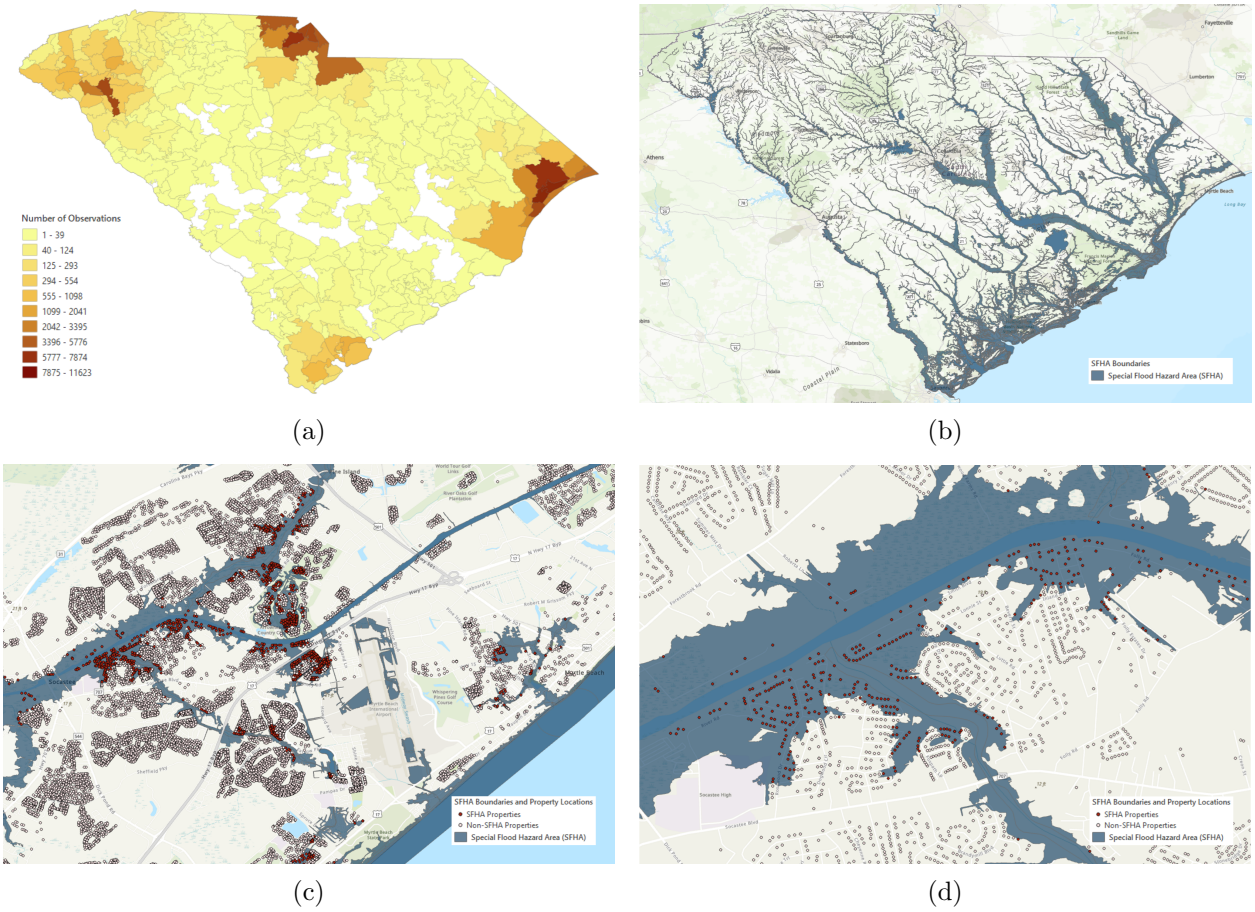


Figure 4: (a) Heatmap of the distribution of residential properties in the study sample by ZIP code, illustrating the concentration of transactions across the state. (b) FEMA Flood Insurance Rate Maps (FIRMs) showing designated Special Flood Hazard Areas (SFHAs). (c) Zoomed-in view of the Myrtle Beach area overlaying the FIRM boundaries; homes located within SFHAs are highlighted in red and those outside in white. (d) Further zoom into a representative neighborhood, showing parcel-level variation in exposure across adjacent subdivisions and the spatial intermixing of SFHA (red) and non-SFHA (white) homes.

begin by comparing price changes for properties inside and outside mapped floodplains before and after South Carolina's disclosure reform. I then test whether disclosure steepened the price-risk gradient by interacting the reform with continuous measures of flood risk. Finally, I use Georgia as a comparison state in a triple-difference framework to separate the effect of the disclosure mandate from contemporaneous changes in federal flood insurance policy.

5.1 Flood Disclosure Policy: Difference-in-Differences Model

To estimate the effect of South Carolina's flood-risk disclosure mandate on housing prices, I begin with a difference-in-differences design comparing properties inside and outside FEMA Special Flood Hazard Areas before and after the mandate became fully effective. The estimating equation is:

$$ln(price_{ict}) = \alpha + \beta_1 SFHA_i + \beta_2 Post_t + \beta_3 (SFHA_i \times Post_t) + \gamma X_i + \theta_{ct} + \epsilon_{ict}$$

where $Price_{ict}$ is the transaction price of property i sold at time t in county c. $SFHA_i$ is an indicator equal to one if the property is located inside a flood zone, and $Post_t$ is an indicator equal to one for transactions occurring after the disclosure mandate became fully effective in July 2014. I exclude transactions closing between January 2013 and July 2014 from the main sample to avoid contamination from the overlap between the disclosure-form transition period and the implementation and subsequent rollback of federal flood-insurance rate changes under Biggert-Waters and HFIAA.

The identifying assumption is that, absent the disclosure mandate, prices for SFHA and non-SFHA properties within the same local housing market would have followed parallel trends. This assumption is supported by the event-study evidence below, which shows no detectable differential pre-trends between SFHA and non-SFHA properties prior to the policy change. Under this assumption, a negative estimate of β_3 indicates that the mandate increased the capitalization of flood risk into housing prices.

Table 1: Difference-in-Differences Estimates: Full Sample

	Log Sale Price
SFHA	0.227*** (0.008)
SFHA × Post	-0.073*** (0.010)
Observations	463,899
Adjusted R^2	0.633
Property Controls	Yes
County × Month-Year FE	Yes

Notes: The dependent variable is the natural log of transaction price. Post coefficient is absorbed by the County × Month-Year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1 reports the baseline difference-in-differences estimates. The coefficient on $SFHA_i$

is positive and statistically significant, indicating that SFHA properties sold at a premium relative to non-SFHA properties prior to the mandate. This pre-disclosure premium is consistent with the fact that many floodplain properties are located near water-related amenities, including coastal areas, rivers, lakes, and marshes. Thus, before disclosure, flood-zone status was associated not only with higher expected flood costs, but also with locational amenities valued by buyers.

The coefficient on $SFHA_i \times Post_t$ is negative and statistically significant, indicating that the disclosure mandate reduced the relative price of SFHA properties. The estimate implies that, after standardized flood-risk information became available before offer submission, floodplain properties sold at a lower relative price than otherwise comparable non-floodplain properties in the same county-month-year. This pattern is consistent with the mandate increasing the salience of flood risk at the point of price formation and causing buyers to place greater weight on expected flood costs when forming bids.

Importantly, the disclosure mandate did not eliminate the amenity premium associated with floodplain locations. Rather, the results indicate that mandatory pre-offer disclosure compressed that premium by increasing the extent to which flood risk was capitalized into transaction prices. In this sense, the policy appears to have changed the information environment surrounding high-risk properties rather than the underlying physical risk or the amenity value of those locations.

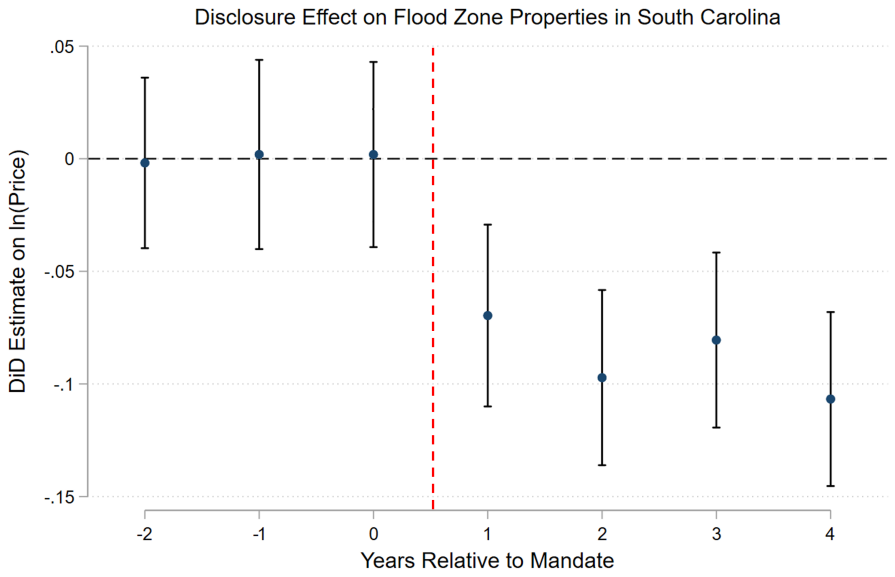


Figure 5: Event-study estimates of the SFHA × year coefficients around South Carolina's 2013 disclosure mandate. Year 0 marks the year the mandate took effect. Coefficients are relative to non-SFHA properties in the same county-month. Bars show county-clustered 95% confidence intervals.

Figure 5 presents event-study estimates that allow the relative SFHA price differential to vary by year around the disclosure mandate. The pre-policy coefficients are small and sta-

tistically indistinguishable from zero, supporting the parallel-trends assumption underlying the difference-in-differences design. After the mandate became effective, the coefficients turn negative and remain below zero in subsequent years. This dynamic pattern suggests that the relative decline in SFHA prices emerged after the disclosure requirement and persisted over the post-mandate period.

The event-study evidence also helps distinguish the disclosure effect from general housing-market appreciation or statewide macroeconomic changes. Because the estimates compare SFHA and non-SFHA properties within the same county-month-year, common local price shocks are absorbed by the fixed effects. The post-mandate decline therefore reflects a change in the relative valuation of flood-zone properties rather than a general weakening of the South Carolina housing market.

To examine whether this average effect differs across markets and whether it is robust to controlling for time-invariant property characteristics, Table 2 reports estimates separately for coastal and inland counties and then repeats the exercise using a repeat-sales sample. The coastal-inland split is motivated by the fact that flood risk and water-related amenities are more tightly linked in coastal markets. As a result, coastal SFHA properties may have larger pre-disclosure premia but may also experience larger post-disclosure discounts if the mandate caused buyers to more fully internalize flood-related ownership costs.

The repeat-sales columns provide an additional check on identification by restricting the sample to properties that transact more than once during the sample period. This sample uses property-level fixed effects in addition to the county by month-year fixed effects in the other specifications. This design compares price changes for the same property over time and therefore absorbs time-invariant property characteristics that may be correlated with both flood-zone status and transaction prices.

Table 2 shows that the disclosure effect is substantially larger in coastal markets than in inland markets. In the all-sales sample, the SFHA $\times$ Post coefficient is -0.104 in coastal counties and -0.040 in inland counties, compared with -0.073 statewide. This pattern is consistent with disclosure having a larger effect where flood exposure and water-related amenities are more closely intertwined and where underlying flood exposure, insurance-related costs, and the tension between amenity value and hazard risk are likely to be strongest. In these markets, standardized risk information may cause buyers to place greater weight on flood-related ownership costs when valuing properties that also offer desirable coastal amenities.

The repeat-sales estimates show that these results are not driven solely by fixed cross-sectional differences between SFHA and non-SFHA properties. Columns (4)–(6) introduce property fixed effects using the repeat-sales sample. The estimated relative price changes remain negative and statistically significant, but are smaller: -0.017 statewide, -0.036 in coastal counties, and -0.009 inland. These specifications compare transactions for the same property over time, accounting for permanent property characteristics, including persistent locational amenities.

The attenuation indicates that the estimated magnitude is sensitive to sample composition and the treatment of time-invariant property heterogeneity. Because the repeat-sales

Table 2: Disclosure Effects by Coastal Status and Repeat-Sales Properties

	All Sales			Repeated Sales		
	(1) Full Sample	(2) Coastal	(3) Inland	(4) Full Sample	(5) Coastal	(6) Inland
SFHA	0.227***	0.371***	0.124***	—	—	—
	(0.008)	(0.015)	(0.009)			
SFHA × Post	-0.073***	-0.104***	-0.040***	-0.017**	-0.036**	-0.009***
	(0.010)	(0.018)	(0.011)	(0.007)	(0.002)	(0.002)
Observations	463,899	94,310	369,589	83,756	24,959	58,797
Adjusted R^2	0.633	0.556	0.688	0.960	0.868	0.938
Property Controls	Yes	Yes	Yes	Yes	Yes	Yes
Property-level FE	No	No	No	Yes	Yes	Yes
County × Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the natural log of transaction price. SFHA indicates properties located in a Special Flood Hazard Area. The interaction term SFHA × Post is the difference-in-differences estimate. Columns (1)–(3) report estimates using the full transaction sample, while Columns (4)–(6) report repeated-sales estimates. Within each sample, estimates are reported for the full sample, coastal counties, and inland counties. All specifications include property-level controls and County by Month-Year fixed effects. The repeat sales estimates also use property-level fixed effects. Post is absorbed by the County × Month-Year fixed effects and SFHA is absorbed by the property-level fixed effects. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

specifications change both the estimation sample and the fixed-effects structure, the difference from the all-sales estimates cannot be attributed uniquely to either factor. Nevertheless, the persistence of negative estimates and the larger coastal response provide complementary evidence of a post-disclosure change in the relative valuation of SFHA properties.

Taken together, the baseline, event-study, coastal-inland, and repeat-sales results provide consistent evidence that South Carolina's pre-offer disclosure mandate increased the capitalization of flood risk into housing prices. The effects are strongest in coastal markets, where flood risk and amenity value are most closely intertwined, but remain negative and statistically significant in inland markets and in repeat-sales specifications.

5.2 Risk Interacted Difference-in-Differences Model

To estimate heterogeneous effects of flood-risk disclosure on housing prices, I extend the baseline design with a risk-interacted specification that varies across space (SFHA vs. non-SFHA), time (pre vs. post disclosure), and risk intensity (continuous standardized risk). This specification remains a difference-in-differences design in the sense that identification comes from two dimensions: flood zone status and the timing of the mandate. It does not incorporate an out-of-state comparison group, and is therefore distinct from the triple-difference (DDD) design implemented in Section 5.3. The three-way interaction term (Post × SFHA × Risk) captures how disclosure altered the slope of the price–risk relationship

specifically within the floodplain, rather than identifying a separate triple-difference contrast. The risk interacted model is:

$$\begin{aligned}\ln(\text{Price}_{ict}) &= \alpha + \beta_1 \,\text{SFHA}_i + \beta_2 \,\text{Post}_t + \beta_3 \,\text{Risk}_i \\ &\quad + \beta_4 \,(\text{Post}_t \times \text{SFHA}_i) + \beta_5 \,(\text{Post}_t \times \text{Risk}_i) \\ &\quad + \beta_6 \,(\text{SFHA}_i \times \text{Risk}_i) + \beta_7 \,(\text{Post}_t \times \text{SFHA}_i \times \text{Risk}_i) \\ &\quad + \gamma \, X_i + \theta_{ct} + \varepsilon_{ict}.\end{aligned}$$

where β_1 measures the average difference between properties located inside and outside the SFHA prior to the disclosure, holding flood risk constant. County-by-time fixed effects absorb the main effect of $Post_t$. The coefficient β_3 represents the pre-disclosure price–risk gradient outside the SFHA, indicating how prices varied with continuous flood risk absent mandatory insurance requirements.

The interaction term β_4 captures the standard difference-in-differences effect, comparing how prices inside the SFHA changed after the mandate relative to outside, evaluated at mean risk. The coefficient β_5 identifies the change in the price–risk gradient outside the SFHA following disclosure, while β_6 measures the difference in the pre-disclosure risk gradient inside relative to outside the SFHA. Our parameter of interest, β_7, is the parcel-level risk interaction with the DiD term capturing how the disclosure mandate differentially changed the capitalization of flood risk inside the SFHA compared to outside. $\beta_7 < 0$ would indicate that the price-risk gradient steepened more inside the SFHA than outside following disclosure. This differential steepening would suggest that the policy increased risk capitalization specifically among properties for which SFHA status was directly disclosed and most likely to carry insurance-related financial consequences. Using these coefficients, the post-disclosure risk gradient inside the SFHA is given by $(\beta_3 + \beta_5 + \beta_6 + \beta_7)$, whereas outside the SFHA it is $(\beta_3 + \beta_5)$. The contrast between these expressions identifies the heterogeneous within-state effect of disclosure on risk capitalization.

Table 3 presents the risk-interacted difference-in-differences results across two specifications. Column (1) shows a standard binary difference-in-differences estimate without the risk interactions, Column (2), my preferred specification, introduces the full risk-interacted model with the three-way interaction term (Post × SFHA × Risk) to capture heterogeneous policy effects by both SFHA status and risk intensity.

As discussed above, the binary DiD estimate in Column (1) indicates a 7.1% relative price decline for SFHA properties following disclosure. Column (2) shows that introducing continuous flood risk and its interactions substantially alters this interpretation. SFHA properties exhibit a sizable positive baseline premium ($\beta_1 = 0.247$, $p < 0.01$), consistent with amenity or waterfront value. The main effect of flood risk is negative ($\beta_3 = -0.064$, $p < 0.01$), indicating that even outside the SFHA and before disclosure, higher flood risk was associated with lower property values. The Post × SFHA coefficient becomes slightly more negative when risk interactions are included (-0.078 vs. -0.073), reflecting that the binary specification averages across heterogeneous effects. Since SFHA properties tend to have

Table 3: Effect of Flood Risk Disclosure on Housing Prices

	(1) Binary DiD	(2) Risk Interacted DiD
SFHA	0.227*** (0.008)	0.2470*** (0.0611)
Post X SFHA	-0.073*** (0.010)	-0.0783*** (0.0250)
Parcel-level Risk		-0.0638*** (0.0154)
SFHA X Risk		0.0324 (0.0199)
Post X Risk		0.0028*** (0.0009)
Post X SFHA X Risk		-0.0409** (0.0179)
Observations	463,899	463,899
Adjusted R^2	0.633	0.659
Property Controls	Yes	Yes
County by Month-Year FE	Yes	Yes

This table reports estimates from two specifications evaluating the impact of the 2013 flood-risk disclosure mandate on log sale prices. Column (1) presents a binary difference-in-differences model comparing SFHA and non-SFHA properties before and after disclosure. Column (2) includes interactions among Post, SFHA status, and parcel-level flood risk to capture heterogeneous disclosure effects on the risk–price gradient inside versus outside the floodplain. All regressions include County by Month-Year fixed effects and property-level controls. Clustered standard errors are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

higher risk, the binary estimate understates the policy effect on high-risk properties while overstating it for low-risk ones. Because SFHA status and flood risk are strongly correlated, the binary specification conflates the average treatment effect with heterogeneous effects across the risk distribution.

The key coefficient of interest, Post $\times$ SFHA $\times$ Risk, is negative and statistically significant ($\beta_7 = -0.041$, $p < 0.05$). This indicates that disclosure steepened the price-risk gradient inside the SFHA relative to outside the floodplain. The positive coefficient on SFHA $\times$ Risk ($\beta_6 = 0.032$, n.s.) reveals that prior to disclosure, the risk gradient was actually flatter within the SFHA—high-risk SFHA properties were discounted less than would be expected based on their risk levels alone. Following disclosure, this pattern reversed: buyers discounted high-risk SFHA properties more heavily, while risk sensitivity outside the SFHA changed little (Post $\times$ Risk $= 0.003$, $p < 0.01$). Collectively, these results indicate that the policy's effect is concentrated where the information revelation was most salient—within areas subject to mandatory flood insurance requirements.

To visualize these estimates, Figure 6 plots the model-implied hedonic price schedules against First Street Flood Factor risk scores, separately by SFHA and non-SFHA properties and pre- and post-disclosure periods. I generate predictions from the risk-interacted difference-in-differences model in Table 3 Column 2, holding observed property characteristics and fixed effects constant. The figure reveals several important patterns. First, the slopes of the price-risk gradient differ systematically by SFHA status and time period. Second, SFHA properties still command higher predicted prices than non-SFHA properties after disclosure. Third, the price-risk relationship experiences a pivot along the hedonic schedule instead of a lateral move up or down. The slope for SFHA properties steepens noticeably after disclosure, while the relationship for non-SFHA properties remains relatively stable. This differential steepening is captured by the negative and significant β_7 coefficient.

This pattern warrants careful interpretation through the lens of hedonic theory. Following Banzhaf (2021), we can distinguish between movements along a hedonic price schedule and shifts of the entire schedule. The former occurs when new information causes properties to be re-evaluated within an existing market equilibrium between amenities and prices, while the latter reflects fundamental changes to the underlying willingness-to-pay for flood risk. Our results align with the movement-along interpretation: the SFHA disclosure policy did not fundamentally alter how the market values flood risk per se, but rather improved the market's ability to correctly assess which properties face higher flood probability.

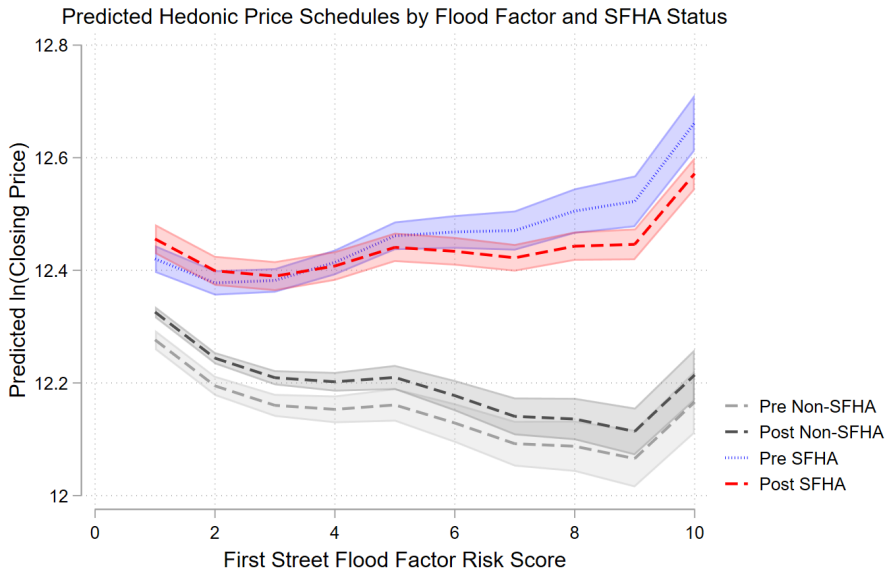


Figure 6: The figure plots predicted log sale prices across the First Street Flood Factor score, separately for properties inside and outside FEMA Special Flood Hazard Areas before and after the South Carolina disclosure mandate. Predictions are generated from the risk-interacted difference-in-differences specification reported in Table 3, holding property characteristics and fixed effects constant. Shaded regions denote 95 percent confidence intervals.

Before mandatory disclosure, buyers relied on imperfect signals of flood risk, such as observable elevation, proximity to water, and neighborhood characteristics. These signals produced a price-gradient even in the pre-period, as evidenced by the negative coefficient on Risk (β_3). However, SFHA designation was not systematically disclosed during transactions, meaning that official flood zone status, which determined flood insurance requirements, was not disclosed mandatorily until closing and only for mortgage-financed buyers. Consequently, properties were priced based on perceived rather than officially designated risk.

The SFHA and flood information disclosure mandate did not change the actual flood risk properties faced, nor did the market tradeoff between flood risk and other property characteristics. Instead, the mandate likely allowed buyers to better differentiate properties along the existing risk-price schedule. Properties revealed to be in the SFHA with high flood risk scores moved to the lower portion of the hedonic schedule, experiencing larger discounts relative to pre-disclosure. Properties with low risk scores in the SFHA appear to have maintained much of their price premiums and actually sold for marginally higher prices than pre-disclosure, as buyers could now differentiate within the floodplain based on disclosed risk-related information, such as prior flooding, insurance coverage, premiums, and claims history.

Critically, the stability of the hedonic schedule for non-SFHA properties (minimal changes in β_5) supports this interpretation. If disclosure had fundamentally changed preferences or the cost structure of flood risk throughout the market, we would have expected to see similar shifts in the risk gradient both inside and outside the SFHAs. Instead, the effect is

concentrated where the policy provided new information, SFHAs, and with an intensity in which higher risk would most likely have more information disclosed. This information was previously obscured but carried significant financial implications through insurance mandates and higher insurance premiums.

The convergence of hedonic schedules around risk scores of 3-4 reveals an important dimension of heterogeneous information revelation across the risk distribution. This pattern reflects not only SFHA designation itself but also the intensity of disclosed information, which varied systematically with flood risk severity. Critically, properties with higher risk scores are plausibly more likely to have experienced floods, carried insurance, paid higher premiums, and filed claims. Disclosure intensity therefore increased with objective flood risk. This creates differential disclosure intensity even within the SFHA. Low-risk SFHA properties near the boundary (score ≤ 3) might have had minimal damage or claims history to disclose, revealing primarily their SFHA designation only. High-risk SFHA properties (score > 7) were more likely to have additional information to disclose, such as prior flooding, insurance coverage, premiums, or claims history. If so, disclosure intensity may have increased with objective flood risk. Properties in the moderate range (scores 4-7) occupied an informational blind spot where properties are deep enough in the floodplain to be more likely to have accumulated flood history, but not so obviously vulnerable that buyers would anticipate this history from property observation alone.

Before disclosure, buyers valued SFHA properties based primarily on observable amenities (water access, views), producing the positive SFHA premium ($\beta_1 = 0.247$). The mandate disrupted this by revealing concrete financial consequences: explicit premium costs, past damages, and claims history. The steepened risk gradient ($\beta_7 = -0.041$) reflects buyers responding to this information intensity. High-risk properties experienced large declines as disclosure revealed substantial obligations and loss history while low-risk properties maintained prices as disclosure clarified their nominal risk despite SFHA designation.

Outside the SFHA, disclosure had minimal effect ($\beta_5 \approx 0$) because these properties rarely carried insurance and lacked flood history to disclose. The market priced them based on observable signals that remained unchanged by the policy. The convergence zone thus represents the threshold where disclosure transitioned from confirming designation alone to revealing substantial consequence history—precisely where the mandate had its largest informational impact.

To quantify the economic magnitude of the steepened risk gradient, consider two SFHA properties that differ by one standard deviation in flood risk (approximately 3 points on the First Street scale). Before disclosure, a one standard deviation increase in flood risk was associated with a 3.2% price decline in SFHA properties ($\beta_3 + \beta_6 = -0.032$) compared to a 6.4% decline outside ($\beta_3 = -0.064$). After disclosure, this SFHA gradient steepened to 7.3% ($\beta_3 + \beta_5 + \beta_6 + \beta_7 = -0.073$), representing more than a doubling of risk sensitivity, indicating substantial information revelation. Conversely, lower-risk SFHA properties experience little additional discount, consistent with disclosure allowing buyers to distinguish lower-risk floodplain homes from higher-risk ones.

The consistency of results across specifications and the statistical significance of the triple interaction term support the internal validity of the findings. Standard errors clustered at the county level account for potential within-county correlation in unobserved determinants of housing prices and do not materially affect inference. The inclusion of county by month-year fixed effects controls for all time varying county-level factors including macroeconomic conditions, local housing market trends, and county-specific policy changes ensuring that the estimated effects are not confounded by broader market dynamics.

Collectively, these results demonstrate that the flood-risk disclosure mandate meaningfully altered the capitalization of flood risk in housing markets, but primarily within areas already subject to mandatory flood insurance requirements. The policy increased the salience of existing risk information rather than creating entirely new costs, leading to improved sorting along the hedonic price schedule. While these within-state patterns are consistent with a disclosure effect, they cannot rule out confounding from contemporaneous state or national policies. Section 5.3 implements a triple-difference design using Georgia as a control state to address this concern.

5.3 Cross-State Triple Difference Model

The risk-interacted difference-in-differences estimates in Section 5.2 establish that disclosure altered both the average discount and the within-floodplain risk gradient in South Carolina. A remaining concern, discussed in Section 3.1, is that the timing of South Carolina's disclosure mandate overlapped with the phase-in of federally mandated rate increases under the Biggert-Waters Flood Insurance Reform Act (BW-12), and with their subsequent partial rollback under the Homeowner Flood Insurance Affordability Act (HFIAA). Because BW-12 and HFIAA applied nationally, any price response attributed to South Carolina's disclosure mandate could instead partly reflect buyers' and sellers' responses to these federal insurance changes rather than to the earlier release of flood risk information.

To separate these two channels, I implement a triple-difference (DDD) design that benchmarks South Carolina against a neighboring state, Georgia, that was exposed to the same federal insurance rate changes but did not adopt a comparable pre-offer disclosure mandate during the sample period. The estimating equation is:

$$\begin{aligned}\ln(\text{Price}_{ict}) &= \alpha + \beta_1 SFHA_i + \beta_2 Post_t + \beta_3 SC_i + \beta_4 (SFHA_i \times Post_t) \\ &\quad + \beta_5 (Post_t \times SC_i) + \beta_6 (SFHA_i \times SC_i) \\ &\quad + \beta_7 (SFHA_i \times Post_t \times SC_i) + \gamma X_i + \theta_{ct} + \epsilon_{ict}\end{aligned}$$

where SC_i is an indicator equal to 1 for properties located in South Carolina and zero for properties in Georgia, and all other terms are defined as in Section 5.1. Because BW-12 and HFIAA applied uniformly to both states, any differential change in the SFHA discount in South Carolina relative to Georgia around the same event window cannot be attributed to the federal rate reform. The coefficient of interest, β_7, therefore isolates the portion of the

post-mandate SFHA discount attributable specifically to South Carolina's state disclosure law.

Table 4: Difference-in-Difference-in-Differences Models

	(1) Baseline DDD Log Price	(2) Risk Interacted DDD Log Price
SFHA	0.329*** (0.010)	0.352*** (0.031)
SFHA × Post	-0.039*** (0.011)	-0.002 (0.015)
SFHA × South Carolina	0.007** (0.014)	-0.006 (0.075)
SFHA × Post × SC	-0.041** (0.017)	-0.057** (0.019)
Parcel-level Risk	—	0.063** (0.029)
Risk × SFHA	—	-0.082*** (0.010)
Risk × South Carolina	—	-0.076 (0.056)
Risk × SFHA × South Carolina	—	0.081*** (0.016)
Risk × Post × South Carolina	—	0.027 (0.031)
Risk × SFHA × Post × SC	—	-0.017*** (0.005)
Observations	893,504	818,304
Adjusted R^2	0.477	0.480
Property Controls	Yes	Yes
County × Month-Year FE	Yes	Yes

Notes: This table reports triple-difference (DDD) estimates benchmarking South Carolina against Georgia, a neighboring state exposed to the same federal flood insurance rate changes under the Biggert–Waters Act but without a comparable pre-offer disclosure mandate. Column (1) presents the baseline DDD specification and Column (2) extends this specification by interacting all terms with parcel-level flood risk; the coefficient of interest, $SFHA \times Post \times SC \times Risk$, identifies whether the post-mandate steepening of the within-floodplain price-risk gradient is similarly specific to South Carolina relative to Georgia. All specifications include property-level controls and County by Month-Year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column (1) in Table 4 reports these estimates. The triple interaction term, SFHA × Post × SC, is negative and statistically significant (-0.041, p < 0.05), indicating that the post-mandate discount on SFHA properties in South Carolina exceeds the corresponding change in Georgia by roughly 4 percentage points, even after netting out any common response to BW-12. This result confirms that the level effect documented in Table 1 is not simply a byproduct of the contemporaneous federal rate reform, and provides direct support for the disclosure-timing mechanism developed in Section 3 rather than an insurance-pricing alternative.

5.4 Risk Interacted Triple-Difference Model

The DDD specification in Section 5.3 establishes that the average SFHA discount responded specifically to South Carolina's disclosure mandate. I next extend this design to test whether the steepening of the within-floodplain risk gradient documented in Section 5.2 is similarly attributable to the state mandate rather than to the federal insurance reform. I augment the DDD specification with the continuous flood risk measure and its interactions:

$$\begin{aligned}\ln(Price_{ict}) = {} & \beta_1 SFHA_i + \beta_2 Risk_i + \beta_3(Risk_i \times SFHA_i) \\ & + \beta_4(SFHA_i \times Post_t) + \beta_5(Risk_i \times Post_t) \\ & + \beta_6(Risk_i \times SFHA_i \times Post_t) \\ & + \beta_7(SFHA_i \times SC_i) + \beta_8(Risk_i \times SC_i) \\ & + \beta_9(Risk_i \times SFHA_i \times SC_i) \\ & + \beta_{10}(SFHA_i \times Post_t \times SC_i) \\ & + \beta_{11}(Risk_i \times Post_t \times SC_i) \\ & + \beta_{12}(Risk_i \times SFHA_i \times Post_t \times SC_i) \\ & + \gamma X_i + \theta_{ct} + \epsilon_{ict}.\end{aligned}$$

The coefficient of interest, β_{12}, measures the post-minus-pre change in the difference between SFHA and non-SFHA price–risk gradients in South Carolina, relative to the corresponding change in Georgia.

Column (2) in Table 4 reports these results. The quadruple interaction term is negative and significant ($-0.017, p < 0.01$), indicating that the steepening of the within-floodplain risk gradient documented in Table 3 is also specific to South Carolina relative to a neighboring state exposed to the same federal insurance environment. Together, the results in this section support the interpretation that both the average discount and the within-zone risk gradient responded to the state disclosure mandate itself, rather than to the national insurance rate changes that were phased in over the same period. Having established this result along both the extensive (level) and intensive (slope) margins, and having ruled out the leading alternative explanation tied to federal insurance reform, Section 6 next examines whether these findings are sensitive to sample construction, spatial measurement error, and unobserved property-level heterogeneity.

As a complementary check on mechanism, I also examine whether disclosure affected the speed of transactions, not just their price. Re-estimating the baseline specification with log days-on-market as the outcome, I find that SFHA properties took approximately 14.6 percent longer to sell following the mandate relative to non-SFHA properties ($\beta = 0.136, p < 0.001$; Appendix Table A1), with no significant pre-mandate difference in time-to-sale between SFHA and non-SFHA properties. This result is consistent with disclosure increasing buyer scrutiny of flood-exposed properties and suggests that the price effects documented above were accompanied by reduced liquidity in the SFHA segment of the market.

5.5 Heterogeneity by Neighborhood Income

This section examines whether the capitalization effects documented above vary with neighborhood income. Because disclosure may be more binding in some market segments than others, estimating a single average effect may obscure meaningful heterogeneity across buyers and neighborhoods. I classify Census tracts into median-household-income quartiles within counties and estimate the baseline and risk-interacted specifications separately by quartile. The results indicate substantial heterogeneity: the post-disclosure SFHA discount is concentrated outside the highest-income tracts, with the largest effects in the middle of the within-county income distribution. Table 5 and Figure 7 report the baseline difference-in-differences estimates, while Figure 8 shows the corresponding risk-interacted hedonic schedules.

Table 5: Heterogeneous Effects of Flood Disclosure Mandate by Tract Income Quartile

	(1) Highest	(2) Upper-middle	(3) Lower-middle	(4) Lowest
SFHA Property	0.107*** (0.012)	0.342*** (0.014)	0.166*** (0.019)	0.220*** (0.025)
SFHA × Post	-0.002 (0.014)	-0.172*** (0.016)	-0.107*** (0.022)	-0.064** (0.030)
Observations	160,509	151,695	93,012	58,683
Adjusted R^2	0.679	0.661	0.534	0.657
Property Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes

Notes: The dependent variable is log sale price. Each column estimates the difference-in-differences model separately by tract income quartile. The coefficient on SFHA × Post captures the post-disclosure change in SFHA property prices relative to non-SFHA properties. All models include structural controls and county by month–year fixed effects. Robust standard errors clustered at the county level are reported in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 reports the estimates from the baseline difference-in-differences specification by within-county tract-income quartile. The coefficient on $SFHA_i \times Post_t$ is close to zero and statistically indistinguishable from zero in the highest-income quartile, indicating little evidence that the disclosure mandate changed the average relative price of SFHA properties in these neighborhoods. In contrast, the estimated coefficients are negative in the remaining quartiles. The largest discounts appear in the middle of the within-county income distribution, while the lowest-income quartile also exhibits a negative but smaller estimate. Thus, the pattern is not strictly monotonic. The central finding is instead that the highest-income tracts are distinct in showing no detectable average price response to disclosure.

Figure 7 visualizes the same baseline estimates and their 95 percent confidence intervals. The figure reinforces the pattern from Table 5: disclosure induced price effects are concentrated outside the highest-income neighborhoods. The highest-income quartile estimate is centered near zero, whereas the other three quartiles show negative point estimates. The

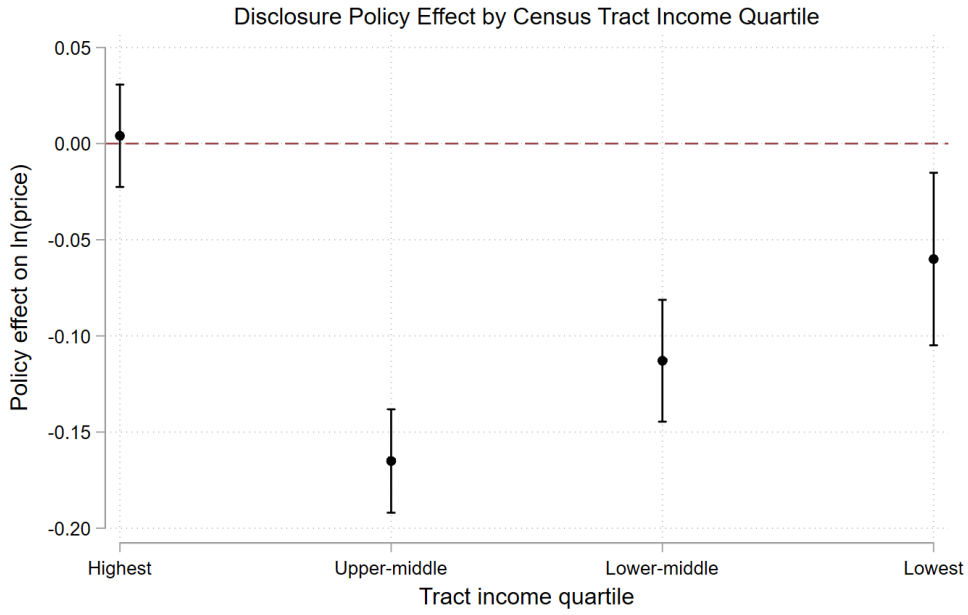


Figure 7: This figure plots the estimated post-policy change in the SFHA price differential by Census tract income quartile, where income quartiles are defined within counties using tract-level median household income. Estimates are measured in log sale price points. Vertical bars denote 95 percent confidence intervals, and the dashed horizontal line marks zero effect.

largest estimated effects occur in the middle-income quartiles, suggesting that the average disclosure effect documented in the full sample is driven primarily by price adjustments in middle- and lower-income neighborhood segments rather than by high-income tracts.

Figure 8 provides complementary evidence from the risk-interacted specification. The predicted hedonic schedules show little post-disclosure change in the highest-income quartile, consistent with the baseline estimates. In the middle-income quartiles, however, the post-disclosure SFHA schedule shifts downward relative to the pre-disclosure schedule, indicating stronger capitalization of flood exposure after the mandate. The lowest-income quartile also shows a post-disclosure decline, though estimates are less precise at higher levels of flood risk where transactions are sparse. The confidence bands widen at the extremes of the risk distribution, reflecting fewer observations in these ranges and greater sampling variability.

Taken together, these results suggest that the disclosure mandate did not affect all neighborhoods equally. The strongest price responses occur outside the highest-income tracts, especially in middle-income neighborhoods, while the highest-income tracts show little detectable response. This suggests that disclosure was least binding in high-income housing markets. Buyers in these neighborhoods may have already incorporated flood risk into their bids before the mandate, or they may have been less sensitive to disclosed ownership costs because they could more easily absorb higher insurance premiums, expected damages, or repair costs. High-income SFHA properties may also include offsetting amenities, such as

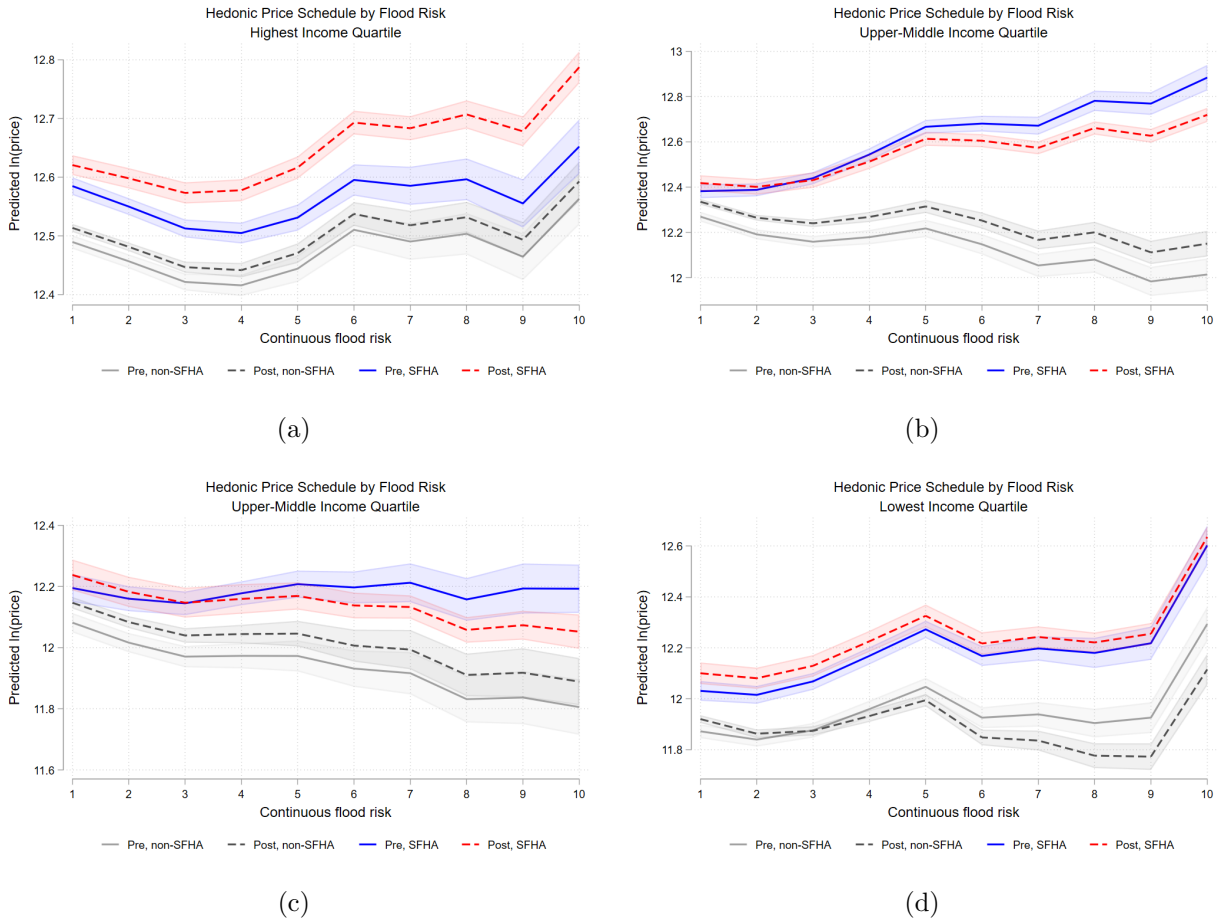


Figure 8: This figure plots predicted log sale prices across the continuous flood-risk distribution separately for SFHA and non-SFHA properties, before and after the disclosure-policy period. Panels (a) through (d) correspond to Census tracts in the highest-income, upper-middle-income, lower-middle-income, and lowest-income quartiles, respectively. Shaded regions denote 95 percent confidence intervals.

waterfront access, that limit the observed price response. By contrast, disclosure may have mattered more in middle- and lower-income tracts because information frictions or flood-related ownership costs were more salient in price formation.

I interpret these estimates as reduced-form evidence of heterogeneous market responses rather than definitive evidence of a single mechanism. The data does not observe buyer beliefs, insurance choices, liquid wealth, or willingness to self-insure. Therefore, I cannot separately identify whether the weaker response in high-income tracts reflects better prior information, lower sensitivity to ownership costs, stronger amenity offsets, or greater capacity to bear risk. The contribution of this section is to show that disclosure-induced capitalization varies systematically across neighborhood income segments, with the strongest effects outside the highest-income tracts.

6 Robustness

The statewide estimates compare SFHA and non-SFHA properties that may differ in their locations as well as their flood exposure. Properties deep within floodplains are often closer to water-related amenities than properties outside them, and these locations may experience different price trends even within the same county. A separate concern arises immediately around the SFHA boundary, where coordinate imprecision or flood-map digitization error may affect the assignment of flood-zone status. This section addresses these concerns in turn. Section 6.1 restricts the sample to properties within progressively narrower bands around the SFHA boundary to limit differences in location and amenity exposure between treated and control properties. Section 6.2 examines the local price pattern at the boundary directly, both before and after disclosure, using a differences-in-discontinuities design. Section 6.3 implements donut specifications that exclude properties nearest the boundary to address GPS and digitization error directly. Section 6.4 summarizes the combined evidence from these checks.

6.1 Spatial Proximity and Bandwidth Restrictions

Does the relative price response persist when SFHA and non-SFHA properties are drawn from more spatially comparable locations? County-by-month-year fixed effects absorb common county-level shocks, but they do not eliminate differences in price trends across locations within a county. Restricting the comparison to properties near the SFHA boundary reduces the geographic separation between treated and control properties and limits differences in their proximity to water-related amenities. These restrictions do not guarantee comparable counterfactual trends, but they provide a more spatially focused test of the baseline result.

Table 6 reports the estimated relative price changes within progressively narrower distance bands. Although the estimates do not attenuate monotonically, they are smaller in magnitude than the statewide estimate, particularly in the narrowest band. However, the

Table 6: Difference-in-Differences Estimates Near the SFHA Boundary

	(1) Full Sample	(2) 1000 meters	(3) 750 meters	(4) 500 meters	(5) 250 meters
SFHA	0.227*** (0.008)	0.198*** (0.009)	0.191*** (0.009)	0.178*** (0.010)	0.134*** (0.012)
SFHA × Post	-0.073*** (0.010)	-0.062*** (0.010)	-0.058*** (0.011)	-0.060*** (0.011)	-0.037*** (0.014)
Observations	463,510	351,249	101,226	54,688	35,042
Adjusted R^2	0.633	0.628	0.621	0.608	0.565
Property Controls	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes

Notes: This table re-estimates the baseline specification from Table 2 on samples restricted to properties located within increasingly narrow bands around the SFHA boundary, using geocoded coordinates and digitized FEMA flood zone shapefiles to measure distance. Column (1) reports the full-sample estimate; Columns (2)–(5) restrict the sample to properties within 1,000, 750, 500, and 250 meters of the boundary. Narrower bands limit comparisons to spatially proximate homes with more similar unobserved locational amenities while differing in SFHA status. All models include property-level controls and County by Month-Year fixed effects. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

post-disclosure interaction remains negative and statistically significant throughout. The coefficient is -0.062 within 1,000 meters, -0.058 within 750 meters, -0.060 within 500 meters, and -0.037 within 250 meters, compared with -0.073 in the full sample. Thus, the relative price adjustment persists even when the comparison excludes properties situated far from the boundary. The pre-disclosure SFHA premium also becomes smaller as the distance band narrows, consistent with the restricted samples reducing some of the locational differences between floodplain and non-floodplain properties.

The smaller interaction estimate in the 250-meter sample indicates that the magnitude of the price response differs between near-boundary properties and the broader sample. This attenuation may reflect differences in flood exposure, the information available to disclose, or other property and market characteristics. Narrower samples also yield less precise estimates, but reduced precision alone does not explain the smaller point estimate. The results therefore support the persistence of a relative price response near the boundary without implying that its magnitude is uniform across locations.

6.2 Difference-in-Discontinuities at the SFHA Border

To estimate whether disclosure changed the price discontinuity at the SFHA boundary, I extend the baseline difference-in-differences specification by including signed distance to the boundary and its interactions with SFHA status and the post-disclosure indicator. I estimate the model using transactions within a bandwidth h on either side of the boundary and allows

separate linear distance slopes for each side-by-period group.

$$\begin{aligned}\ln(price_{ict}) = \alpha &+ \beta_1 SFHA_i + \beta_2 Post_t + \beta_3(SFHA_i \times Post_t) \\ &+ \beta_4 d_i + \beta_5(SFHA_i \times d_i) + \beta_6(Post_t \times d_i) \\ &+ \beta_7(SFHA_i \times Post_t \times d_i) + \gamma X_i + \theta_{ct} + \epsilon_{ict}, \qquad |d_i| \leq h.\end{aligned}$$

Here, d_i denotes signed distance from the SFHA boundary, with negative values indicating properties inside the SFHA and positive values indicating properties outside. The bandwidth h determines the maximum distance included on either side. The distance interactions allow separate linear slopes for each side-by-period group. The main effect of $Post_t$ is absorbed by the county-by-time fixed effects.

The coefficient of interest, β_3, measures the post-minus-pre change in the fitted inside-minus-outside log price discontinuity at the boundary. The pre-disclosure discontinuity is β_1, and the post-disclosure discontinuity is $\beta_1 + \beta_3$. Unlike the bandwidth-restricted DiD estimates in Section 6.1, this specification targets the change in the fitted gap exactly at the cutoff rather than the relative price change throughout the selected band. A causal interpretation requires that, absent the disclosure reform, the boundary discontinuity would not otherwise have changed, together with appropriate continuity conditions for the underlying price relationships.

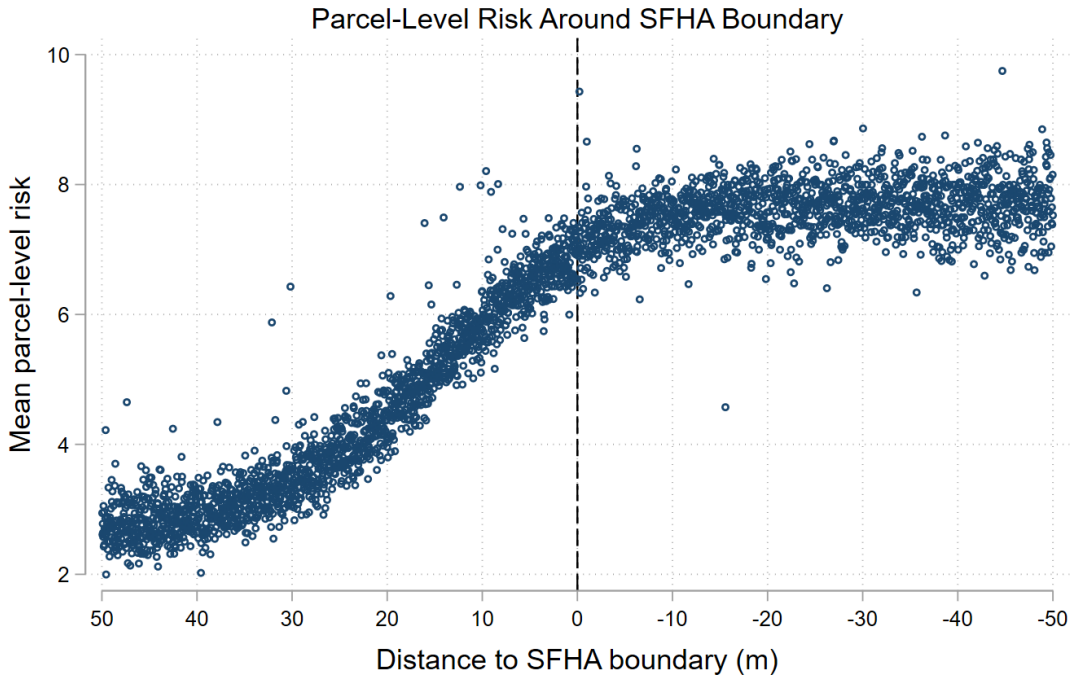


Figure 9: Parcel-level risk near the SFHA boundary. Points show binned mean parcel-level risk within 50 meters of the boundary. The vertical dashed line marks the SFHA boundary.

Figure 9 provides context for this comparison. The continuous flood-risk measure appears

to transition smoothly through the SFHA boundary, with no visually apparent jump. This pattern is consistent with the boundary marking a discrete regulatory designation rather than an abrupt change in physical hazard. However, visual continuity in this measure does not establish continuity of all other price determinants or rule out contemporaneous changes in the capitalization of boundary-linked insurance costs.

Figure 10 illustrates the local price patterns using residualized transaction prices. Within 150 meters, the fitted inside-minus-outside gap narrows from approximately 0.17 log points before disclosure to 0.025 afterward. Within 350 meters, the change in the fitted cutoff gap is smaller. These plots illustrate sensitivity to the fitting window. Because they use residualized outcomes, their fitted gaps need not equal estimates from the regression that jointly includes the boundary terms, property controls, and fixed effects.

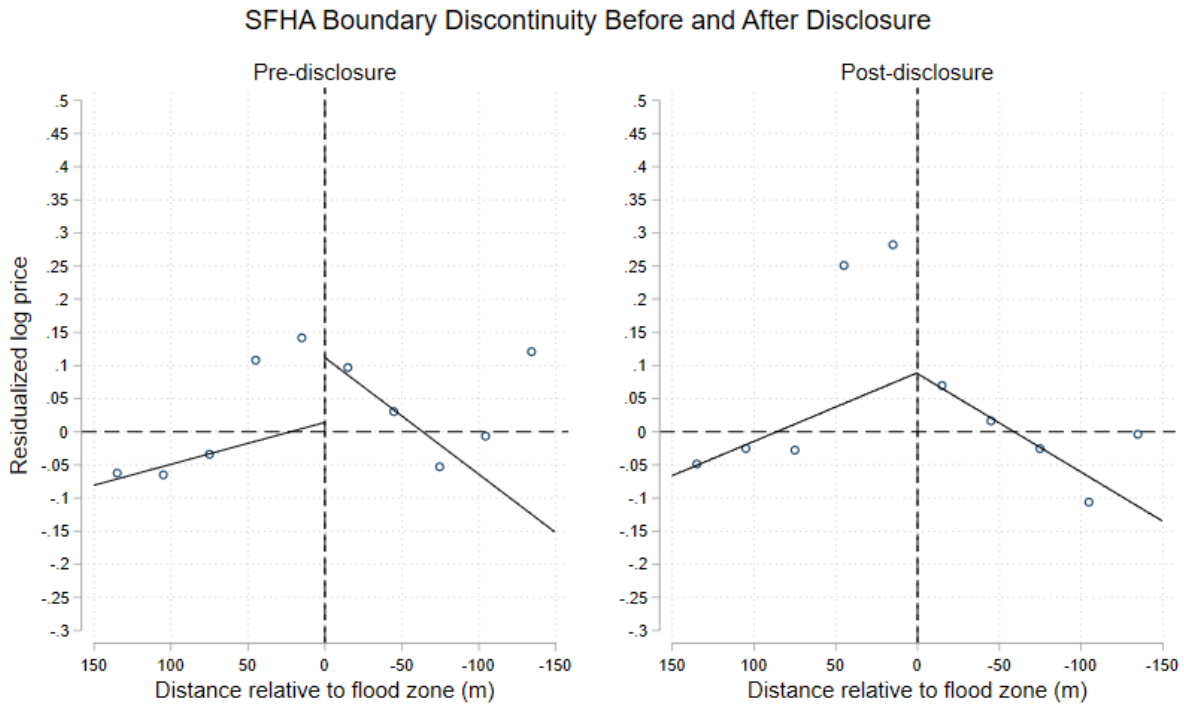
Table 7 reports the formal difference-in-discontinuities estimates across bandwidths ranging from 50 to 350 meters. The point estimates are negative in every specification, but their magnitude and precision vary substantially. The estimated change is -0.137 log points within 150 meters and -0.089 within 250 meters, both statistically significant at the five-percent level. At 350 meters, the estimate is smaller, at -0.023 log points, and is not statistically distinguishable from zero. The 50-meter specification produces an estimate of -0.191 with a standard error of 0.227, leaving the immediate-boundary contrast particularly imprecisely estimated.

Taken together, these estimates provide directionally consistent but bandwidth-sensitive evidence of a reduction in the SFHA boundary price gap. They do not establish a precise, bandwidth-invariant cutoff effect. Expanding the window changes the observations used to fit the discontinuity; it does not estimate treatment effects at progressively greater distances from the boundary. The results therefore complement the near-boundary DiD estimates without implying that the disclosure effect necessarily declines with distance.

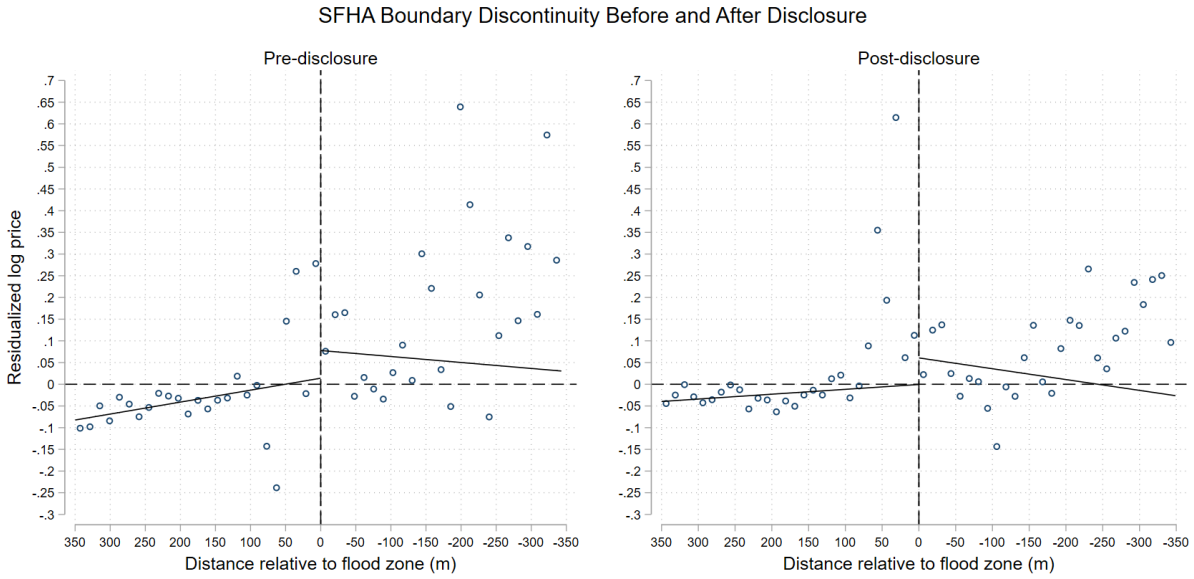
Table 7: Difference-in-Discontinuities Estimates at the SFHA Boundary

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth (meters)	50	100	150	200	250	300	350
SFHA × Post	−0.191	−0.327*	−0.137***	−0.086	−0.089**	−0.037*	−0.023
	(0.227)	(0.177)	(0.041)	(0.075)	(0.032)	(0.021)	(0.014)
Observations	11,078	15,706	22,174	30,209	35,085	42,676	48,227
Property controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is log transaction price. Each column reports a difference-in-discontinuities estimate from a pooled local-linear regression using transactions within the indicated distance on either side of the SFHA boundary. The coefficient on SFHA × Post measures the post-minus-pre change in the fitted inside-minus-outside log price discontinuity at the cutoff. All specifications include signed distance and its interactions with SFHA status and the post-disclosure indicator, allowing separate linear distance slopes on each side of the boundary in each period. All specifications include property controls and County by Month-Year fixed effects. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



(a)



(b)

Figure 10: This panel plots the housing price patterns near the SFHA boundary before and after disclosure. Panel (a) plots transactions within 150 meters of the boundary, while Panel (b) uses a 350 meter window. Negative distances indicate properties inside the SFHA, and positive distances indicate properties outside. Circles show unweighted mean residualized log transaction prices within approximately 14-meter wide bins constructed separately for each period and on each side of the boundary. Solid lines show separate linear regressions estimated using equal observation weights over the displayed distance window. The pre-period includes transactions before January 2013, and the post-period includes transactions after July 2014.

6.3 Donut Specifications and Boundary Classification Error

Small discrepancies between recorded property coordinates and mapped flood-zone boundaries can place a property on the wrong side of the cutoff. If observations immediately around the boundary account for the estimated response, excluding them could materially change the results. To address boundary misclassification arising from GPS imprecision or flood-map digitization error, I implement donut specifications that exclude properties within 25 or 50 meters of the SFHA boundary, nested within the 750- and 500-meter bandwidth samples. This procedure mitigates potential bias from boundary misclassification, endogenous sorting at the cutoff, and localized spillovers, comparing immediate spatial neighbors who differ discretely in flood zone designation while excluding the properties most likely to be misclassified.

Table 8: Donut Difference-in-Differences Estimates Near the SFHA Boundary

	(1) Full Sample	(2) 750m/50m	(3) 750m/25m	(4) 500m/50m	(5) 500m/25m
SFHA	0.227*** (0.008)	0.140*** (0.011)	0.159*** (0.009)	0.121*** (0.011)	0.144*** (0.010)
SFHA × Post	-0.073*** (0.010)	-0.042*** (0.013)	-0.050*** (0.011)	-0.044*** (0.013)	-0.052*** (0.011)
Observations	463,510	88,426	99,412	47,890	52,873
Adjusted R^2	0.633	0.688	0.680	0.690	0.678
Property Controls	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes

Notes: This table reports donut specifications that exclude properties located within 50 or 25 meters of the SFHA boundary, nested within the 750-meter and 500-meter bandwidth samples from Table 6, to address potential bias from GPS measurement error and flood-map digitization imprecision near the boundary. Column (1) reports the full-sample estimate; Columns (2)–(5) report estimates excluding the nearest 50 and 25 meters within the 750-meter and 500-meter bands. Similar magnitudes across donut widths indicate that the baseline effect is not driven by properties immediately at the boundary that may be misclassified. All models include property-level controls and County by Month-Year fixed effects. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 presents these results. The estimated post-disclosure interaction remains negative and statistically significant in every specification, ranging from -0.042 to -0.052. Within the 750-meter band, the estimates are -0.050 when excluding the nearest 25 meters and -0.042 when excluding the nearest 50 meters. The corresponding estimates within the 500-meter band are -0.052 and -0.044. This indicates that the baseline effect is not driven by properties immediately at the boundary that may be misclassified, and that the disclosure effect is detectable even when comparing spatial near-neighbors on either side of the flood zone cutoff.

6.4 Summary of Spatial Robustness

Figure 11 plots the bandwidth-restricted estimates from Table 6 and the donut estimates from Table 8 on a common scale. All specifications shown in the figure yield negative and

statistically significant relative price changes after disclosure. The estimates range from -0.037 to -0.062 log points across the bandwidth-restricted samples and from -0.042 to -0.052 across the donut samples, compared with -0.073 statewide. Negative relative price changes therefore persist when comparisons are restricted to near-boundary properties and when observations closest to the boundary are excluded.

The estimated magnitude nevertheless varies with sample construction. In particular, the 250-meter DiD estimate is smaller than the statewide estimate. This difference may reflect variation in flood exposure, disclosed information, property composition, or counterfactual price trends. Reduced precision alone does not explain the smaller point estimate.

The difference-in-discontinuities analysis in Table 7 addresses a separate question: whether the fitted price gap exactly at the boundary changed after disclosure. These estimates are negative across all reported windows, but their magnitude and statistical significance depend on the bandwidth, and the narrowest specification is particularly imprecise. Overall, the spatial evidence supports negative relative price changes across near-boundary samples more clearly than it establishes the magnitude of a change exactly at the cutoff.

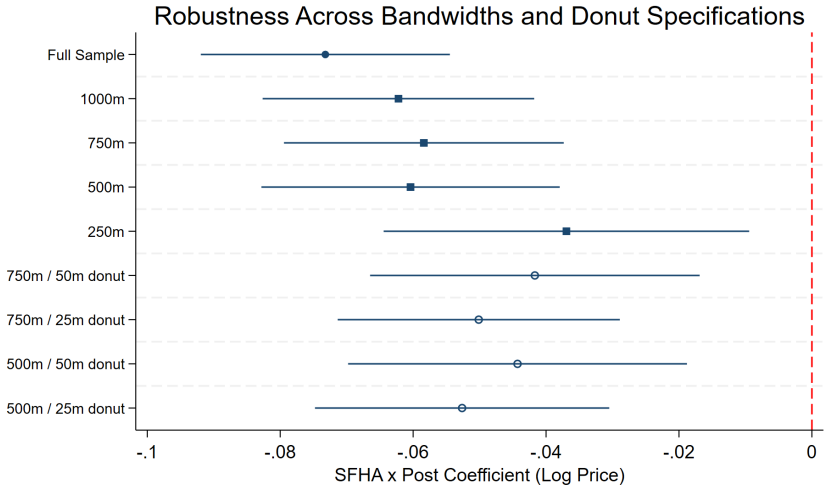


Figure 11: This figure plots the SFHA × Post coefficient (log price) from Tables 6 and 8, along with 95% confidence intervals, across the full sample, four narrowing bandwidths around the SFHA boundary (1000m, 750m, 500m, 250m), and four donut specifications that additionally exclude properties nearest the boundary (750m/50m, 750m/25m, 500m/50m, 500m/25m). The filled circle marks the full-sample estimate; filled squares mark the bandwidth-restricted estimates; open circles mark the donut specifications. The dashed red line marks zero. Standard errors are clustered at the county level.

7 Conclusion

This paper studies whether mandatory, pre-offer disclosure of flood risk changes how that risk is priced in residential housing markets. Exploiting South Carolina's 2013 disclosure reform as a natural experiment, I find consistent evidence that moving standardized flood informa-

tion earlier in the transaction timeline increased the capitalization of flood risk into home prices. The baseline difference-in-differences estimates indicate that properties in Special Flood Hazard Areas experienced an additional 7 percent price decline relative to comparable non-SFHA properties following the mandate, with the preferred county-by-month specification implying a decline of approximately 5 to 7 percent. The risk-interacted difference-in-differences specification shows that this effect was not confined to the average floodplain discount: disclosure also steepened the price–risk gradient within the floodplain, with a one standard deviation increase in continuous flood risk associated with an additional 4 percent discount post-mandate. Benchmarking South Carolina against a neighboring state in a formal triple-difference (DDD) design confirms that both the level effect and the risk-gradient steepening are attributable to the state disclosure mandate rather than to the contemporaneous phase-in and rollback of Biggert-Waters rate changes. The results are robust to narrowing geographic bandwidths around the SFHA boundary, to donut specifications that exclude properties closest to the cutoff, and to a repeat-sales design that absorbs unobserved, time-invariant property characteristics. The difference-in-discontinuities estimates also point toward a reduction in the price gap at the SFHA boundary, but are sensitive to the estimation bandwidth. The evidence is therefore stronger for relative repricing across near-boundary samples than for a precisely estimated change exactly at the cutoff.

Taken together, these findings support the interpretation developed in the conceptual framework: pre-mandate prices reflected incomplete capitalization of flood risk driven by information frictions rather than by fully informed preferences over risk and amenities. Had buyers already possessed and fully weighted flood risk information prior to the reform, the disclosure mandate should have had no detectable effect on either the level or slope of the price-risk relationship. Instead, the data show a sharpening of both margins, consistent with a model in which disclosure raises the salience of expected flood damages at the moment offers are formed. The heterogeneity documented between coastal and inland markets, where the coastal discount is more than twice the inland discount, further suggests that disclosure had the largest effect where the gap between amenity value and hazard exposure was largest. The income heterogeneity results point to an additional margin of variation: disclosure effects are concentrated outside the highest-income census tracts, with the largest estimated discounts in middle-income neighborhoods and little detectable response in the highest-income tracts. This pattern suggests that disclosure was less binding in high-income markets, either because buyers in those neighborhoods were already better informed, because they were less sensitive to flood-related ownership costs, or because amenity values more strongly offset disclosed flood risk. By contrast, in middle- and lower-income tracts, where information frictions and flood-related costs may be more consequential for household budgets, pre-offer disclosure appears to have induced larger price adjustments.

These results carry several implications. For housing market efficiency, the findings suggest that simple, low-cost information interventions can meaningfully improve price discovery in a market where climate risk has historically been underpriced, without requiring changes to insurance subsidies or underlying hazard levels. At the same time, the income hetero-

geneity results show that the benefits and incidence of disclosure are unlikely to be uniform across market segments. Disclosure appears most consequential in middle- and lower-income neighborhoods, where flood-related costs may represent a larger share of household resources and where information frictions may be more binding. This suggests that disclosure policy should be understood not only as a tool for improving aggregate price efficiency, but also as a consumer-protection policy whose effects may be concentrated among buyers with fewer resources to absorb unexpected flood costs.

This study has several limitations that suggest directions for future research. First, while the cross-state triple-difference design and event-study estimates help rule out contemporaneous federal insurance reform and disaster-driven salience as alternative explanations, I cannot fully rule out other unobserved state-specific shocks coinciding with the reform's timing. Second, the analysis focuses on transaction prices and does not directly examine whether disclosure changed insurance take-up, mortgage default rates, or the composition of buyers sorting into flood-prone areas. This limitation is particularly relevant for interpreting the income heterogeneity results, since the data do not observe buyer income, liquid wealth, insurance coverage decisions, risk beliefs, or capacity to self-insure. As a result, the weaker response in high-income tracts cannot be uniquely attributed to better prior information; it may also reflect lower sensitivity to disclosed ownership costs or stronger amenity preferences in high-income floodplain markets. Third, the South Carolina reform bundled several types of information (SFHA status, prior flood damage, and insurance history) into a single mandate, so this paper cannot separately identify which component of disclosure is driving the observed price response. Future work using states that adopted narrower disclosure requirements, or that phased in different disclosure components at different times, could help disentangle these channels. Finally, as more states consider adopting or strengthening flood disclosure laws, extending this design to a broader panel of state-level reforms would help establish whether the magnitude of the effect documented here generalizes beyond South Carolina's particular housing market and flood geography.

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A Appendix

A.1 Time to Sale

As discussed in Section 5, I test whether the disclosure mandate affected not only transaction prices but also the speed of sale for SFHA properties. Table A1 reports difference-in-differences estimates of the effect of the disclosure mandate on days-on-market, using both a log-transformed outcome (Column 1) and the raw number of days from listing to closing (Column 2). Both specifications include the same property-level controls and county × year fixed effects as the baseline price specification in Table 1.

Table A1: Effect of Flood Disclosure Mandate on Time to Sale

	(1) Log Days on Market	(2) Days on Market
SFHA	0.009 (0.023)	5.181** (2.382)
SFHA X Post	0.136*** (0.027)	13.528*** (2.866)
Observations	293,304	293,304
Adjusted R^2	0.428	0.496
Property Controls	Yes	Yes
County × Month-Year FE	Yes	Yes

Notes: This table reports difference-in-differences estimates of the effect of the 2013 disclosure mandate on time-to-sale, measured as days from initial listing to closing. Column (1) uses the natural log of days-on-market as the outcome; Column (2) uses the raw number of days. Both specifications include property-level controls and County by Month-Year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results indicate that the disclosure mandate significantly increased time-to-sale for SFHA properties. The interaction coefficient in Column (1) suggests that SFHA properties experienced a 13.6 percent increase in days-on-market following the mandate (significant at the 1% level). In levels, Column (2) shows that SFHA properties took approximately 13.5 additional days to sell after the disclosure requirement took effect, relative to non-SFHA properties.

These findings are consistent with a model in which flood risk disclosure increases search frictions or reduces the pool of willing buyers for flood-prone properties. The extended marketing time may reflect buyers' need for additional time to process flood risk information, obtain flood insurance quotes, or negotiate over risk-related contingencies. Alternatively, sellers of SFHA properties may face a thinner market post-disclosure as some buyers exit the market or search for lower-risk alternatives.

The estimated effect on time-to-sale is economically meaningful. The baseline average days-on-market for SFHA properties in the pre-period is approximately 95 days (see Table 2). An additional 13.5 days represents a 14.2 percent increase, imposing additional holding

costs on sellers and potentially affecting their reservation prices. This increase in marketing time may partially explain the negative price effects documented in Table 1, as longer time-on-market is typically associated with lower transaction prices in the housing literature.