2023-09

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Journal of Housing Economics 61 (2023) 101956

Contents lists available at ScienceDirect

Journal of Housing Economics

journal homepage: www.elsevier.com/locate/jhec

Potential Benefits in Remapping the Special Flood Hazard Area: Evidence from the U.S. Housing Market

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ARTICLE INFO

JEL:
Q54
R31
G52

Keywords:
Flood risk
Hedonic prices
Spatially restricted triple-difference
Risk premiums
Risk Rating 2.0

ABSTRACT

A typical U.S. homebuyer's understanding of whether a property faces flood risk is based on whether the property is located inside the National Flood Insurance Program's (NFIP) Special Flood Hazard Area (SFHA). The SFHA boundary, however, may bias homebuyers' perceptions of flood risk relative to unobserved true risk because the SFHA is an incomplete, and sometimes inaccurate, representation of flood hazards. Using a national dataset of property transactions, flood hazard data from the First Street Foundation, state-level measures of flood disclosure laws, and a spatially restricted triple-difference design, we distinguish capitalization effects of policy-driven (SFHA) versus quasi-objective (First Street) indicators of flood hazard. We identify these effects by assessing thousands of local spatial interactions between property SFHA designations, measures of quasi-objective flood hazard, and being in a state that mandates flood history disclosures. Being inside the SFHA generates a risk discount, but the signal is muted relative to underlying hazard exposure. Further, the SFHA signal can result in inefficient discounts for properties erroneously mapped in the SFHA. Disclosure requirements about flood history accentuate price effects for hazardous properties and result in more adequate risk internalization. In the absence of disclosures, however, we find either no or a weak risk signal for houses outside the SFHA that face flood hazard. Our results highlight potential benefits of updating SFHA boundaries to include all houses that may experience flooding, and argue in favor of requiring flood-related disclosures from sellers to improve the market's ability to internalize flood risk.

1. Introduction

The extent to which property markets price flood risk is a crucial determinant of the economic costs of flooding. Since 2017, the costs of flood-related events have exceeded $650 billion in the U.S. (NCEI, 2020). Although $160 billion in supplemental funding was appropriated over the same period for recovery from all disaster types (RSFLG, 2020), these sums do not fully compensate affected households because they leave a large gap in terms of uninsured losses (Kousky, 2018; Kousky & Shabman, 2012). This state of affairs highlights the importance of incorporating risk in the prices of properties affected by hazards. In addition, it reveals the challenge of constructing a public insurance program that: (1) is capable of covering the spectrum of flood risks experienced by households, (2) is actuarially fair, and (3) sends accurate price signals to prospective house buyers (Bin & Landry, 2013; Gourevitch et al., 2023; Hino & Burke, 2021; Kousky et al., 2020).

The U.S. National Flood Insurance Program (NFIP) provides over 90% of all flood insurance policies nationwide (Kousky, 2018). Several features of the NFIP's design and implementation suggest that it does not provide adequate risk signals to prospective buyers. First, the actuarial methods for determining NFIP premium rates are often based on outdated data or methods, and rates are subsidized (Bin & Landry, 2013; Czajkowski et al., 2013; Hennighausen et al., 2023; Kousky, 2018) thus reducing incentives to invest in defense expenditure to reduce exposure to flooding (Kousky, 2018). Second, although the NFIP mandates the purchase of flood insurance for houses with mortgages from federally backed lenders located in Special Flood Hazard Areas (SFHAs or flood zones), fewer than 50% of properties in SFHAs hold NFIP insurance (Bradt et al., 2021). And finally, the SFHA boundary is, in many places, failing to reflect the true underlying risk faced by households.

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https://doi.org/10.1016/j.jhe.2023.101956
Received 4 November 2022; Received in revised form 6 July 2023; Accepted 20 July 2023
Available online 26 July 2023

Specifically, the SFHA is intended to designate areas where there is an equal or greater chance of experiencing a 1-in-100-year flood event, but research suggests that in many places the SFHA over- or underestimates the underlying risk (Czajkowski et al., 2013; FEMA, 2015; Kousky et al., 2020; Wing et al., 2017). In fact, many properties located outside SFHAs have experienced substantial damages from flooding (Cooper et al., 2022; Dixon et al., 2013; McCoy & Zhao, 2018; Smiley, 2020; Wing et al., 2018). However, NFIP insurance purchase mandates and building code standards do not apply in these areas, and this leaves houses that are erroneously mapped outside the SFHA potentially more vulnerable to flood damage and uninsured losses (Bradt et al., 2021; Michel-Kerjan & Kunreuther, 2011; Mulder & Kousky, 2023; Thomson et al., 2023). Moreover, updates to SFHA designations that incorporate advances in flood hazard modeling have resulted in the re-zoning of hundreds of thousands of properties from inside to outside the SFHA and vice versa (Weill, 2022), raising the question of the extent to which buyers incorporate flood risks to the prices of potentially flood-affected properties (Kousky et al., 2020).[1]

A large hedonic literature finds that houses within SFHAs sell at a discount relative to houses outside flood zones suggesting that the SFHA conveys a risk signal that is capitalized (Atreya et al., 2013; Atreya & Ferreira, 2015; Beltrán et al., 2018; Bin et al., 2008; Bin & Kruse, 2006; Gourevitch et al., 2023; Hino & Burke, 2021; Johnston & Moeltner, 2019; Kousky, 2010; Pope, 2008). Recent comparisons between this implicit valuation of the cost of flood risk and expected losses conclude that property markets undervalue flood risk on the order of tens to hundreds of billions of dollars (Gourevitch et al., 2023; Hino & Burke, 2021), a gap that constitutes substantial risks to households, communities, governments, and financial institutions (Bakkensen & Barrage, 2022; Bernstein et al., 2019; Gourevitch et al., 2023; Hino & Burke, 2021; Ouazad & Kahn, 2022; Shi & Varuzzo, 2020).

One caveat to these studies, however, is that they implicitly assume that the presence of a house in the SFHA is the only mechanism through which flood hazards are capitalized into housing prices. While the maps that delineate SFHA boundaries are historically the main source of information to prospective buyers about flood hazards (Cooper et al., 2022; Kousky, 2010), there is emerging evidence that additional factors may influence risk perceptions and willingness to pay to avoid risk (Atreya & Czajkowski, 2019; Atreya & Ferreira, 2015; Beltrán et al., 2019; Bernstein et al., 2019; Davlasheridze & Fan, 2019a; Ortega & Taşpınar, 2018; Pollack & Kaufmann, 2022). One of these factors are public disclosure laws requiring sellers to disclose SFHA status or flooding history, such as past damages, flood-related insurance claims, or knowledge of local flooding (FEMA, 2022b; NRDC, 2018). Pope (2008), for example, shows that information about the SFHA is capitalized only after seller disclosures about SFHA status and the associated insurance purchase mandate are required, suggesting asymmetric information and attenuation bias. More recent research over larger scales reinforces the result that SFHA risk signals are strongest in states with these kinds of seller disclosures (Gourevitch et al., 2023; Hino & Burke, 2021).[2]

Evidence that aims to disentangle the relative importance of additional factors to the commonly studied SFHA risk signal is challenging to reconcile. Studies that find risk signals in hazardous areas outside the SFHA tend to do so following historically large and damaging floods—as opposed to prospective risk—over a geographically restricted domain, and none control for seller disclosures (Atreya et al., 2013; Atreya & Ferreira, 2015; Davlasheridze & Fan, 2019a; Ortega & Taşpınar, 2018; Pollack & Kaufmann, 2022). Similarly, few studies simultaneously control for SFHA status and measures of hazard exposure or characteristics of structures that might defend against them (Atreya & Ferreira, 2015; Davlasheridze & Fan, 2019; Pollack & Kaufmann, 2022). Varying locations, major events, and research designs across these studies may explain their seemingly conflicting results. Atreya and Ferreira (2015) find that price discounts are attributable to direct inundation from a flood, not SFHA status. Conversely, neither Davlasheridze and Fan (2019) or Pollack and Kaufmann (2022) find that inundation directly decreases prices. Davlasheridze and Fan (2019) find that structural defenses that reduce vulnerability to hazard increase prices, but only in the SFHA, while Pollack and Kaufmann (2022) find that similar defenses increase prices irrespective of SFHA status. Other studies that control for direct flood hazard exposure do not control for SFHA status, so while these other studies often suggest capitalization of hazard exposure, they do not reconcile the question of whether the hazard exposure effects are distinct from SFHA effects (Beltrán et al., 2019; Bernstein et al., 2019; Ortega & Taşpınar, 2018).

Reconciling the relative importance of different risk pricing factors across geographic contexts and housing markets is important to inform potential policy reforms, but there are major challenges involved. First, because flood hazards are spatially heterogeneous on scales as small as tens of meters (Cooper et al., 2022; Wing et al., 2017), there are major challenges in developing hazard models over a broad geographic domain. Such models are only recently becoming available (Bates et al., 2021), and their accuracy has not been extensively assessed (Wing et al., 2021) making them potentially subject to a range of uncertainties that lead to unknown measurement error (Alipour et al., 2022; Pappenberger et al., 2008). Second, there is a threat of confounding. Research suggests that flood hazard exposure, whether proxied for by SFHA boundaries or quasi-objective flood hazard layers, often correlates strongly with unobservable water-related amenities – e.g., accessibility to rivers, lakes, or beach recreational opportunities and/or viewsheds (Beltrán et al., 2018; Bin et al., 2008). And finally, newly constructed houses in the SFHA are often subject to stricter building codes and are more likely to have structural defenses (Pollack & Kaufmann, 2022). We expect that without careful research design, these hard-to-observe variables will lead to positive biases in treatment effects related to flood hazard exposure, especially in housing markets near the coast where amenities are large and true counterfactuals are hard to find.[3]

To learn about the varying mechanisms of flood risk pricing across the U.S. and their implications for flood risk internalization and potential policy reform, we employ a nationwide Spatially Restricted Triple Difference (SRTD) identification strategy for estimating the interaction of flood hazard exposure, SFHA boundary, and flood history disclosure price effects. In our SRTD design, motivated by a Geographic Regression Discontinuity (GRD) model, we assume that in tight spatial buffers surrounding individual segments of individual SFHA boundaries (Bishop et al., 2020; Black, 1999; Dell, 2010; Keele & Titiunik, 2015; Pope, 2008), the impact of the SFHA on housing prices is identical regardless of a house's quasi-objective flood hazard status. We interact a house's SFHA and quasi-objective flood hazard status with an indicator for whether states require flood history disclosures from sellers. We conduct our analysis in market segments that limit confounding to understand the settings in which treatment effects are identifiable such as inland and coastal segments and houses built before the SFHA boundaries were determined for a community.

Our nationwide sales data comes from Zillow's ZTRAX database and is filtered, geo-located, and enriched with property information by the

[1] Although FEMA is currently updating rates based on the new Risk Rating 2.0 (RR2.0) methodology (FEMA, 2020), RR 2.0 does not address issues such as low adherence to the insurance purchase mandate and the fact that the mandate only applies to houses in the SFHA.

[2] Relatedly, studies show that risk signals can be strong in hazardous areas outside the SFHA that experience flooding in the wake of historically large and damaging floods (Atreya et al., 2013; Atreya & Ferreira, 2015; Davlasheridze & Fan, 2019a; Ortega & Taşpınar, 2018; Pollack & Kaufmann, 2022).

[3] Special care is needed in estimating treatment effects to avoid misattributing a hazard-based information effect to capitalization of damages from past floods (Beltrán et al., 2018, 2019; Ortega & Taşpınar, 2018).

PLACES database according to good data practices for large-scale hedonic analyses (Nolte et al., 2021; Zillow, 2019). We use property-level flood inundation estimates from the First Street Foundation (Bates et al., 2021) as a quasi-objective indicator of whether a house is exposed to flood depths at the 1%-per-year level. This allows us to capture the impact of quasi-objective flood hazard on houses on both sides of the SFHA boundary which is a policy-driven floodplain that aims to capture 1%-per-year extents of hazard exposure. If the property market in the U. S. is efficient, the implied price discount from a property's SFHA presence and flood hazard exposure would equal or exceed the net present value of flood losses for that property (Bin & Landry, 2013). We evaluate this condition using discounted property level estimates of expected losses to structures for all houses in our estimation samples. Nationwide property level flood loss estimates are extracted from the PLACES database as described in Pollack et al., (2022). These estimates were also used to estimate the extent of risk internalization in Gourevitch et al., (2023).

Our findings contribute to the extensive literature on the effect of the SFHA on house prices. We find that in inland market segments, the policy effect of the SFHA plays a prominent role in hazard capitalization.[4] Specifically, we find that being in the SFHA reduces prices by 2.3% to 3.1%, which corresponds closely with previous hedonic estimates.[5] Despite our different quasi-experimental design, our findings for SFHA presence in disclosure and non-disclosure states — 4.2% and 1.9%, respectively, for samples restricted to houses within 1,000 feet of the SFHA boundary segment — are similar to findings in Hino and Burke (2021) (a 4.1% discount in disclosure states) and Gourevitch et al. (2023) (discounts from 4.5% to 9.7%).

Expanding on previous studies, we find that the policy effect appears heterogeneous with respect to the local spatial interaction of SFHA boundaries, quasi-objective flood hazard exposure, and flood history disclosures. The combination of SFHA presence and flood history disclosures translates into steep discounts for flood-exposed houses. Outside the SFHA, or in states without flood history disclosures, discounts for properties in the quasi-objective floodplain are smaller or not found. Importantly, the SFHA price effect extends to houses not exposed to flooding at the 1% level, indicating that poor information signals about underlying hazard can negatively impact some homeowners whose properties are erroneously mapped in the SFHA. Translating these price effects into estimates of risk internalization, we find that estimates of unpriced risk are sensitive to whether hazard-based heterogeneity in the SFHA price effect is employed. For the sample of properties in the quasi-objective floodplain that we evaluate risk internalization for, we find that 3.5% to 6.5% of risk is internalized when implied risk discounts are based on just SFHA and disclosure treatment effects, depending on the damage function and discount rate. Accounting for direct flood hazard leads to estimates of 22.2% to 42.1% of internalized risk, depending on the damage function and discount rate. These results support previous conclusions that risk signals from the SFHA are insufficient for property markets to adequately price expected flood risks but call for more research on identifying heterogeneity and uncertainty in such estimates (Gourevitch et al., 2023; Hino & Burke, 2021).

Our results contribute to the debate surrounding two related policy reforms. First, the Technical Mapping Advisory Council and previous academic research have called for FEMA to remap SFHA boundaries to include any property that may experience flooding (FEMA, 2015; Kousky et al., 2020). Our results suggest that FEMA's new rate setting procedure, Risk Rating 2.0 (RR 2.0), could lead to more internalization of risk in the housing market because it is the combination of SFHA and flood hazard exposure in disclosure states that lead to the greatest price discounts. However, because the SFHA boundaries are unaffected from this reform it may not meet its full potential (Mulder & Kousky, 2023). We show that flood-exposed houses outside the SFHA in disclosure states have small or nonexistent risk signals. Second, our results support FEMA's proposal to Congress to amend the National Flood Insurance Act to explicitly require states to have flood disclosure laws (FEMA, 2022b). We show that disclosures about flood history serve as an important price signal that contribute to more accurate risk internalization. In a hypothetical illustration of a reform based on remapped SFHA boundaries and expanded disclosures, we find that risk internalization in our sample could increase to between 66% to 125% depending on the damage function and discount rate. Therefore, policy reforms that increase the availability of flood hazard information in the property market have the potential to substantially reduce socialized costs of risk. These socialized costs go beyond NFIP debt and have the potential to increase financial risks to municipalities and government-sponsored entities who securitize trillions in mortgage debts without pricing flood risks (Gourevitch et al., 2023; Ouazad & Kahn, 2022). Nevertheless, we caution that additional research is needed to understand the potential consequences of abrupt changes in information provision about hazard exposure and risk on property markets to fully elaborate and assess the associated benefits and costs (Gourevitch et al., 2023; Hennighausen et al., 2023).

2. Data

Our empirical analysis is based on the Private-Land Conservation Evidence System (PLACES) database (Nolte, 2020). PLACES standardizes the ZTRAX nationwide tax assessor and transaction database for data consistency and accuracy according to good data practices for hedonic and geospatial analyses (Nolte et al., 2023; Zillow, 2019). Crucially, PLACES vastly improves the geographic accuracy of coordinates linked to transactions and building attributes needed for geolocation and distance calculations.

We use the ZTRAX database for the estimation of our hedonic models and to produce flood-loss estimates to compare with the capitalization results from the models. For both, we limit our sample to single-family houses as indicated by ZTRAX building codes RR000, RR101, RR102, and RR999 and to parcels with at least one, and no more than two, building footprints. This allows for houses with sheds or garages but does not allow for properties without a building footprint, which can occur in cases with tree or forest cover when assessment data indicates a residence. This is important in the context of geolocating building footprints to determine SFHA status in our empirical models. If any portion of a building footprint is within the SFHA boundary, the property is considered inside the SFHA. Parcel-based geolocation, which would be available for properties without a building footprint, can lead to substantial estimation biases of SFHA treatment effects (Netusil et al.,

[4] We do not identify a statistically significant SFHA discount for houses within 1 mile from the coast. However, as distance to a specific segment of a SFHA boundary is restricted to 100 feet and houses closer to the shore are dropped from the sample, we obtain a point estimate of -0.020 (SE=.026), which corresponds to a 2.0% discount based on 7,696 sales. This reflects a potential bias-variance tradeoff for identification in coastal housing markets when specifying treatment variables that invariably correspond to unobserved, spatially divergent coastal amenities. Controlling for this omitted variable substantially reduces sample size and statistical power. For example, even when the sample is restricted to houses within 250 feet, the sample is 45,808 observations but the point estimate is 0.0.

[5] Beltrán et al. (2018), in meta-analysis of the larger literature, find an average estimated discount of 2.9% for inland properties; Hino & Burke (2021), using national data and policy changes surrounding the location of the SFHA, find an estimate of 2.1%; and Gourevitch et al. (2023), using similar nationwide data, find an estimate of 2.8%.

2019).[6]

To operationalize our empirical models, we require information on whether a property is in a flood zone at the time of sale and how far away from a SFHA boundary the house is located. To obtain flood zone status, we download the National Flood Hazard Layer database from FEMA for the U.S. and join the centroid of the largest building footprint for each parcel to this layer to produce an indicator for SFHA status (FEMA, 2022a). Next, we use the building footprint centroid and calculate the distance to the nearest SFHA boundary for each parcel to create a distance variable. This distance variable serves several purposes. First, in our empirical models we use it to create increasingly restrictive buffers around the SFHA border to limit our estimation samples as a means of controlling for spatially correlated unobservables (Black, 1999; Dell, 2010; Keele & Titiunik, 2015). Second, we use it to create a running variable necessary for estimating our spatial regression discontinuity models. And finally, we use it, combined with a one-kilometer discretization of individual SFHA borders, to create a series boundary fixed effects to further control for spatial unobservables at a fine spatial scale.

Our measure of quasi-objective flood hazard comes from the First Street Foundation's flood hazard model (FSM). The FSM includes modeled inundation depths, at 30-m horizontal resolution, relative to grade, for pluvial, fluvial, and coastal flood processes for three climate change scenarios at five-year intervals from 2020 to 2050 (Bates et al., 2021; First Street Foundation, 2020). We assign the value of the closest FSM pixel to each house in our dataset. We assume that when the pixel has a non-zero inundation depth for FSM representation of a 1%-per--year event, then treatment is positive for quasi-objective flood hazard (*Haz*).[7]

An important question is whether the FSM data is the best measure of quasi-objective flood hazard. Bates et al. (2021) perform an analysis on behalf of First Street to determine how well their model reproduces floodplain extents from high-resolution local models. They find that the underlying model captures the 1%-per-year floodplain based on comparisons with high-resolution local models. However, they do not appear to assess how well inundation depths are modeled relative to local benchmarks or observations of floods. For example, Table A4 shows that the max inundation relative to grade in the 1%-per-year floodplain is 33.75 meters, or roughly 109 feet, which is an obvious error. Wing et al. (2021) evaluate the underlying hydrodynamic model's ability to reproduce inundation of simulated historic events, but the event set is small, largely concentrated in inland areas, and inundation accuracy is represented at an aggregate level instead of at the property-level resolution at which FSM data is provided. For this reason, we use the FSM only to delineate which houses are exposed to hazard. Lacking other high-resolution, large-scale representations of quasi-objective hazard, we view the FSM as a good first step in obtaining estimates of the potential heterogeneity in flood risk pricing across the U.S.

Our final data source comes from the National Resource Defense Council (NRDC) database on state-level flood disclosures, which we use to identify which states require flood history disclosures from sellers (FEMA, 2022b; Gourevitch et al., 2023; NRDC, 2018). States with flood history disclosures appear to also require SFHA disclosures, but the opposite is not always the case. We limit out disclosure indicator to states requiring strong flood history disclosures according to the NRDC data (see Fig. A1 in the Appendix). We use the NRDC data for our specification because it has been used in previous nationwide hedonic studies and provides guidance on the strength of disclosure requirements across states (Gourevitch et al., 2023; Hino & Burke, 2021).

Table A3 in the Appendix shows summary statistics for the variables in our final data sample. The data include 31,662,320 transactions for 17,760,575 properties from 1999 to 2019 in 1,882 counties for which we can identify flood zones, distances to nearest SFHA boundaries, and for which we are able to link to FSM depths.[8]

3. Methods

The SFHA is designed to indicate areas that are subject to a 1% or greater chance of flooding each year. It is well known, however, that in many places the SFHA boundary is based on outdated data and models leading to a misclassification of risk (Czajkowski et al., 2013; FEMA, 2015; Kousky et al., 2020; Wing et al., 2017). To the extent that households only respond to information provided by the SFHA, there is the potential for increased future losses from flooding when actual risks differ from those indicated by the SFHA. Here, we use data on housing prices, information on quasi-objective flood hazard based on FSM, and information on SHFA boundaries to test the hypothesis that households respond to flood hazards apart from those provided by the SFHA.

A large literature has employed hedonic price theory to estimate impact of increased flood hazard on housing prices (Beltran et al. 2018). The standard approach in hedonic models is to regress housing prices, P_i, on housing characteristics, fine-scaled fixed effects, and a treatment variable indicating whether a house is inside or outside of the SFHA. One challenge, however, is that houses cannot be observed in both low- and high-hazard states of the world simultaneously (P_i^0 and P_i^1). To remedy this, researchers have applied difference-in-differences (DD) methods exploiting exogenous changes in risk saliency driven by extreme weather events or changes in policy. The basic setup in these models is as follows:

$$p_{it} = \alpha_0 + \sum_{j=1} \alpha_j Z_{ij} + \beta r_i + \gamma SFHA_i + \delta Flood_t + \theta(Flood_t * SFHA_i) + \varepsilon_{it}, \quad (1)$$

where p_{it} indicates the natural log of the sale price of house i at time t, $SFHA_i$ is a dummy variable for whether the house is in the FEMA flood zone, and $Flood_t$ is a dummy variable indicating that the house sold after a flood event or policy change with the interaction between the flood-zone variable and the variable indicating the change in risk saliency providing treatment effect estimates (average treatment effect on the treated) in the DD model. This specification includes a full set of property characteristics, Z_{ij}, and site attributes related to flood risks, r_i, such as elevation.

We extend the DD model in Eq. (1) and estimate a so-called Spatially Restricted Triple Difference (SRTD) model. Our model combines the differencing logic of the DD setup above with spatial identification logic embedded in Geographic Regression Discontinuity (GRD) models (Bishop et al., 2020; Black, 1999; Dell, 2010; Keele & Titiunik, 2015; Pope, 2008).

Our primary specification is as follows:

[6] The source of data harmonized in PLACES and their application – hedonic analysis and flood loss estimation – is summarized in Table A1. We also outline decisions made in the processing of ZTRAX data for the hedonic analysis (Table A2) and report summary statistics for the variables used in our hedonic analysis (Table A3).

[7] Table A4 in the Appendix shows descriptive statistics for the FSM 1%-per-year data underlying *Haz* for different samples.

[8] Using the ZTRAX transactions in the PLACES database, we also drop observations with prices above the 98th percentile and below the 2nd percentile of the distribution of nominal prices for each county to remove outliers in price. The only other observations we drop are those with no information on build year, effective build year, or remodeled year as we need these values to determine the pre- or post-FIRM status of each house as well as calculate the home's age.

$$p_{ikst} = \alpha_0 + \sum_{j=1}\alpha_j Z_{ij} + \gamma_1 SFHA_i Haz_i Disc_s + \gamma_2 SFHA_i Haz_i + \gamma_3 SFHA_i Disc_s + \gamma_4 Haz_i Disc_s + \gamma_5 SFHA_i + \gamma_6 Haz_i + \delta SFHADist_i + \eta_k + \zeta_{st} + \varepsilon_{ikst}, \quad (2)$$

The outcome variable, p_{ikst}, is the natural log of price for house i, in location k, in state s, in year t. We regress our price variable on indicators for whether a house in located inside or outside of the SFHA ($SFHA_i$), whether it is inside or outside of a high-risk area as defined by the FSM (Haz_i), and whether it is inside or outside of a state that mandates flood risk disclosure ($Disc_s$). The triple difference aspect of the model is produced by creating a full set of interactions between these indicator variables. As opposed to Eq. (1), r_i is not included because the quasi-objective flood hazard boundary represented by *Haz* directly specifies the relationship of site attributes to flood hazard over space.

The other variables in the model include a running variable for the distance from each house to the SFHA boundary, $SFHADist_i$, a set of state-by-year fixed effects, ζ_{st}, and a set of spatial fixed effects to capture local unobservables, η_k. Traditionally, spatial fixed effects in hedonic models are specified based on the census tract or census block group in which the house is located. In our model, however, we use a boundary fixed effects based on the discretization of the SFHA boundary. In the PLACES database, flood zones are given unique identifying information so that properties near the same SFHA boundary can be identified. We split SFHA boundaries of individual SFHA polygons into one-kilometer segments (or three segments, whichever is larger), take the centroid of these segments, and assign unique identifiers to these centroids so that properties which are close to the same segment can be grouped together. Houses on either side of the SFHA boundary share a common fixed effect, and different fixed effects are used for different groups of houses. This process is akin to the boundary fixed effects defined in other GRD models and follows guidance on how to specify running variables for these settings (Bishop et al., 2020; Black, 1999; Dell, 2010; Gelman & Imbens, 2019; Keele & Titiunik, 2015; Pope, 2008).

The vector of structure characteristics, Z_{ij}, consists of a continuous building square footage variable and dummy variables for the age of the house, the number of bedrooms (including levels for 'na' and greater than or equal to 10), the number of bathrooms (including levels for 'na' and greater than or equal to 10), missing building square footage information (interacted with observed building square footage to avoid collinearity), distance within 50-meters to a coastline (only used in coastal segment regressions), lake frontage, and river frontage (Table A2, Table A3).

The base case in our model is for houses located outside of the SFHA, in non-disclosure states, and without hazard exposure as defined by the FSM. The model estimates six ($\gamma_1 \ldots \gamma_6$) interaction-based treatment effects.[9] We expect all coefficients to be negative. However, γ_6 may have no effect because in the absence of flood history disclosures outside the SFHA it is unclear what specific information a prospective buyer can access about flood hazard exposure for a house. Some studies suggest this may be due to information channels like media coverage (Ortega & Taşpınar, 2018) or local learning about hazardous areas after events (Pollack & Kaufmann, 2022), but due to obstacles in processing such data on a national scale we cannot attribute a possible negative effect to a specific information channel (Beltrán et al., 2019).

While our triple difference estimation strategy and the inclusion of boundary fixed effects likely does a good job at identifying our parameters, it is still possible that there exist additional unobservables, in space and time, that may bias our results. To further limit the impact of spatially varying unobservables, we follow previous GRD models and create spatial buffers around the SFHA boundary restricting our data based on increasingly tighter buffers around the border. Our primary specification uses 500- and 1000-foot buffers following Pope (2008). In terms of temporal unobservables, one issue that may arise relates to the relationship between the year a house is built and how that relates to the timing of the mapping of the SFHA for each NFIP community. Houses in the SFHA built after a community joins the NFIP are subject to stricter building codes than houses outside the SFHA. To address this, we estimate our main models using only data on pre-FIRM houses – those houses built before the SFHA was mapped – to improve identification.[10] We test the robustness of our results to these spatial and temporal restrictions later.

To provide intuition for our model, Fig. 1 Panel A shows a conceptual model of a spatial double difference model, where we consider four groups of houses: (1) those in the SFHA and in a hypothetical true floodplain (SFHA = 1, Haz = 1), (2) those inside the SFHA and outside the true floodplain (SFHA = 1, Haz = 0), (3) those outside the SFHA and inside the true floodplain (SFHA = 0, Haz = 1), and (4) those outside the SFHA and outside the true floodplain (SFHA = 0, Haz = 0). The SFHA boundary intersects the stationary true hazard boundary, which follows a gradient that intensifies as you go deeper into the SFHA (right and down from the true hazard boundary in the figure). Traditional approaches to identify the price effect of flood hazard exposure estimate a treatment effect only for houses in the SFHA and consider all houses outside the SFHA even if they are very far from the border and potentially different from closer properties in unobservable ways. Here, we assume that within a buffer around the SFHA border properties within the same distance of either side share common unobservables and thus the only thing impacting price is the SFHA designation. Our actual estimation method expands this conceptual model to include variation based on whether the house in located in or out of a disclosure state.

While the conceptual model in Panel A appears very precise in terms of the boundary cutoffs, the actual data, in many cases, is far less precise. Specifically, Panel B of Fig. 1 illustrates that because the SFHA boundary is highly irregular in many locations, and the quasi-objective representation of property-level flood hazard exposure is highly heterogeneous and discontinuous, the hypothesized treatment groups do not always fall on opposite sides of a single border. Instead, properties from each of the *SFHA- x -Haz* groups can fall next to each other in a manner inconsistent with the conceptual model in Panel A. Still, we argue that our SRTD model allows for identification of the treatment effects of interest in a similar manner to the conceptual model in Panel A. Like that model, our approach controls for omitted variable bias by limiting control and treatment groups to houses that are near the same segment of the same SFHA boundary (Bishop et al., 2020; Black, 1999; Dell, 2010; Keele & Titiunik, 2015; Pope, 2008). Moreover, highly localized fixed effects, based on individual segments of SFHA boundaries, and a local linear running variable of distance to that segment enhances identifiability of treatment effects by controlling for unobserved price generating characteristics that may smoothly vary on either side of the boundary (Dell, 2010; Keele & Titiunik, 2015). We provide further discussion of this in the results section.

4. Results

4.1. Identification

Before presenting our main findings, we address several remaining identification issues related to the construction of our final estimation sample.

The first issue relates to the risk signal conveyed for houses located near the coast versus those located further inland. Previous meta-

[9] The 7th parameter, on the variable for public disclosure ($Disc_s$), which is standard in DDD models, is not identified with the inclusion of the state-by-year fixed effects.

[10] All regressions were fit using the 'fixest' function from the *feols* package in R (Bergé, 2018).

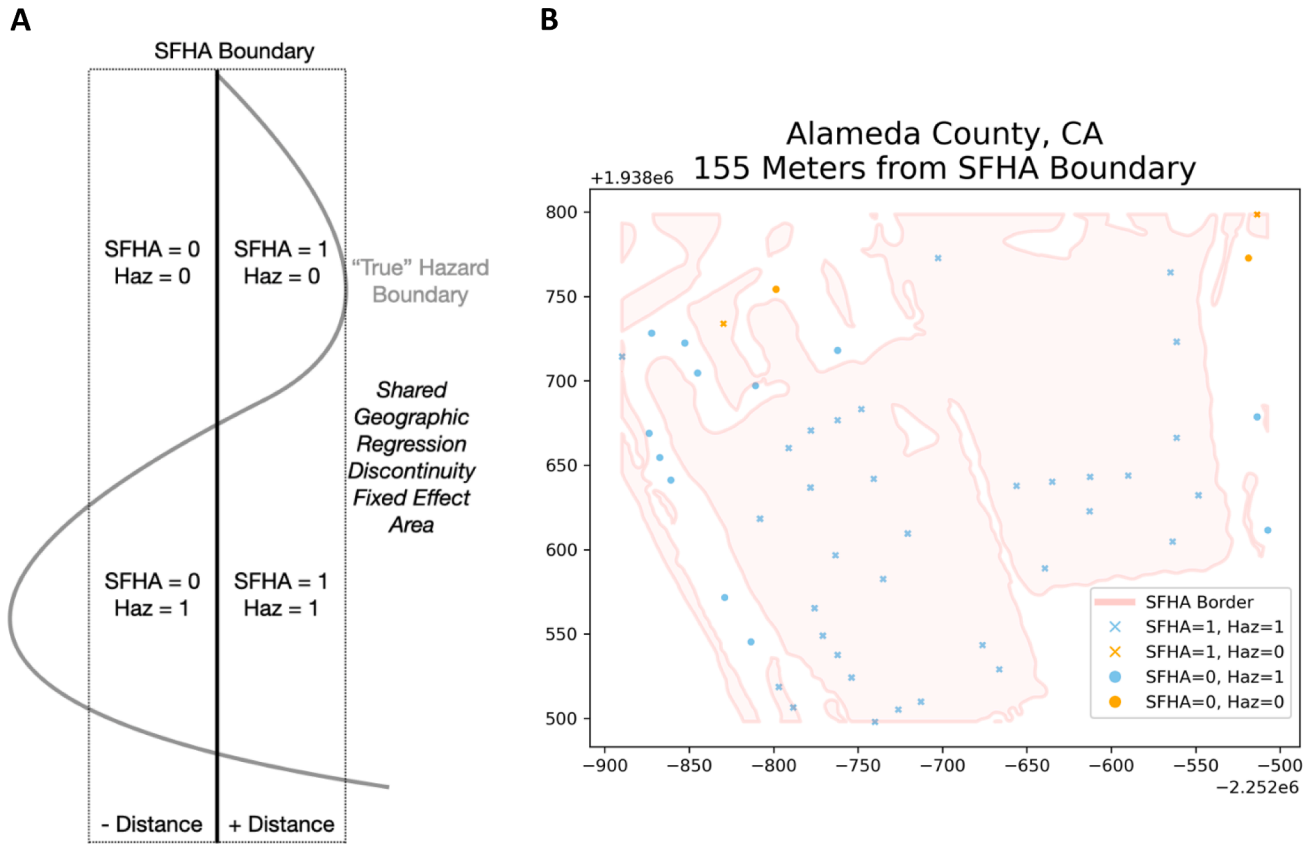

Fig. 1. Panel A: Conceptual model of a geographic regression discontinuity (GRD) model. The Special Flood Hazard Area (SFHA) boundary (black, solid) is intersected by a true hazard boundary (gray, curved). Within a tight distance buffer from the SFHA boundary, a shared (GRD) fixed effect captures the sales generating process for houses in this area. The remaining contribution to prices is explained by treatment differentials, houses outside the true floodplain and outside the SFHA, houses outside the tree floodplain and inside the SFHA, houses in the true floodplain and outside the SFHA, and houses in the true floodplain and inside the SFHA. Panel B: An example of the spatial distribution of SFHA boundaries and a flood hazard exposure representation from the First Street Foundation for a single segment of a SFHA flood zone in Alameda County, CA. Houses in the SFHA are delineated by "x" marks and houses exposed to hazard are marked in blue. Houses outside the SFHA are delinetaed by "o" marks and houses not exposed to hazard are marked in orange.

analysis of the hedonic literature on flood risk has shown consistent, negative treatment effects from being inside the SFHA for houses located in inland flood zones, but has found more mixed, and often positive, results for houses located closer to the coast (Beltrán et al. 2018). The most likely reasons for these mixed results for coastal models are: (1) the inability to control for all water-related amenities and (2) the inability to find proper counterfactuals for coastal risk properties – i.e., those that are both on the coast, but not subject to the hazard of interest.

To address these issues in our data, we split our full sample of data into three subsamples based on a house's location relative to the coast and estimate hedonic models testing for treatment effects associated with being in and out of the SFHA for each subsample of data (Beltrán et al., 2018; Johnston & Moeltner, 2019). The results from these models are presented in Table 1. In Panels A and B, we present results for samples of inland homes located more than one mile and more than five miles from the coast, respectively. Conversely, in Panel C we present results using a sample of homes located one mile or less from the coast. Columns (1) and (2) present results using the full subsample of data – pre-FIRM housing sales subject to each distance cutoff above each column – with census tract and census block group fixed effects. Columns (3)-(6) start with the full subsamples, but present results using our SRTD method of restricting samples based on buffers around the SFHA boundary. We present results for buffers of 1,000, 500, 250, and 100 feet. The buffer models all include SFHA boundary fixed effects, and all models include controls for housing characteristics with standard errors clustered at the county level.

For the non-coastal sample, we find that the SFHA conveys a strong and negative risk signal, which is consistent with expectations that being in a flood zone is a disamenity, all else equal. In the sample for houses located more than one mile from the coast, which contains 92% of all pre-FIRM transactions in our full data, the SFHA treatment effect is -5.4% for the full sample with tract fixed effects and -4.4% with block group fixed effects. For the models using the buffered samples and SFHA fixed effects (columns 3-6 in Panel A), we find roughly similar results with estimates ranging from -2.3% to -3.1%. After increasing the distance from the coast to five miles, a sample that still contains 83% of original pre-FIRM sample, we find that the estimates become even more negative with a roughly similar pattern to the one-mile, inland sample. This change is roughly consistent with samples further from the coast being less impacted by the impacts of coastal amenities.

The results for the coastal sample, however, are less consistent with theory. Estimates for this market segment suggest a strong bias-variance tradeoff. Using the full coastal subsample with tract and block group fixed effects, we find positive and significant coefficients on the SFHA treatment variables – i.e., homeowners are willing to pay a premium for increased exposure to flood hazard. These positive results, however, are likely driven by the two issues we highlighted above. To address these issues, in columns (3)-(6) we present results where we restrict the coastal estimation samples using increasingly restrictive buffers around the SFHA boundary. From the logic of the GRD model, this provides better assurances that houses on either side of the border are comparable in both observable and unobservable ways not captured by the vector of structure characteristics Z. After using these restrictive samples, we find that the positive results disappear. In fact, in the model using a 100-foot

Table 1
Price effects for in SFHA model.

A: > 1 Mile From Coast	(1)	(2)	(3)	(4)	(5)	(6)
SFHA	-0.055***	-0.045***	-0.031***	-0.023***	-0.024***	-0.031**
	(0.006)	(0.005)	(0.004)	(0.003)	(0.004)	(0.012)
Buffer	Full	Full	1000	500	250	100
Num Obs	16223435	16223435	3224818	1009434	263611	39475
FE: Tract	X					
FE: Block Group		X				
FE: SFHA Boundary			X	X	X	X
SFHADist			X	X	X	X
B: > 5 Miles From Coast	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
SFHA	-0.067***	-0.054***	-0.037***	-0.028***	-0.028***	-0.038**
	(0.006)	(0.005)	(0.004)	(0.003)	(0.005)	(0.015)
Buffer	Full	Full	1000	500	250	100
Num Obs	13404702	13404702	2642452	808823	206066	30599
FE: Tract	X					
FE: Block Group		X				
FE: SFHA Boundary			X	X	X	X
SFHADist			X	X	X	X
C: <= 1 Mile From Coast	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
SFHA	0.043*	0.033*	0.011	0.004	0.000	-0.020
	(0.0017)	(0.016)	(0.008)	(0.006)	(0.007)	(0.026)
Buffer	Full	Full	1000	500	250	100
Num. Obs.	1457611	1457611	431896	159096	45808	7696
FE: Tract	X					
FE: Block Group		X				
FE: SFHA Boundary			X	X	X	X
SFHADist			X	X	X	X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
This table presents results for the risk signal conveyed by the SFHA for three different data samples. Panels A and B present results for inland (non-coastal) houses with A showing results for houses located more than one mile from the coast, and B showing results for houses located more than five miles from the coast. Panel C shows results for houses located a mile of less from the coast. Columns (1) and (2) show results for the full sample with census tract and census block group fixed effects. Columns (3)-(6) shows results for samples using increasingly restrictive buffers around the SFHA boundary. The models in columns (3)-(6) include SFHA boundary fixed effects and a running variable for distance to the boundary. All models include full control for housing characteristics, and the standard errors are clustered at the county level.

buffer (column 6), our point estimate turns negative. However, the estimate is statistically indistinguishable from zero most likely driven by the small number of observations remaining in the sample.

The results in Table 1 suggest that the underlying market mechanisms and capitalization process for inland and coastal regions is quite different with the coastal sample showing results, at least in the full sample, inconsistent with theory. While we were able to partially address the issues in the coastal sample using increasingly restrictive buffers, the smaller sample sizes remaining after using increasingly restrictive buffers leaves us limited power with which to identify the treatment effect; this fact will almost certainly be worse in a model with increasingly large numbers of interactions – i.e., the SRTD modeling Eq. (2). Thus, in our main models we use the subsample from Panel A, which consists of pre-FIRM houses located more than one mile from the nearest coastline. We include estimates from running Eq. (2) on the coastal subsample in the Appendix.

4.2. Identification of Treatment Effects with the SRTD Approach

Identification of treatment effects with the SRTD approach requires several conditions to be met. First, there must be sufficient identifying variation in treatment groups after SFHA boundary buffer restrictions to the data. Second, the SRTD approach assumes that all price generating factors, apart from treatment, smoothly vary at the SFHA boundary. Data inspection checks described below suggest our implementation appears appropriate with respect to both conditions for the subsamples of data used in our models.

Our SRTD implementation maintains many fixed effect areas with identifying variation when distance from the nearest SFHA is restricted. Table A5 shows that when our main estimation sample is restricted to 1,000 feet from SFHA boundary, most localized fixed effect areas drop out – i.e, 191,817 of 297,854 individual SFHA areas fall out. However, the remaining fixed effect areas contain roughly 65% of the original observations (2,087,909 out of 3,224,818), and these observations allow for identification of treatment effects. To provide context for this, Table A6 in the Appendix shows how these identifying observations are distributed across control and treatment groups at different distances from the SFHA boundary in both the inland and coastal segments. In the inland segment, going from no SRTD to 1,000 feet from a SFHA boundary reduces the number of control observations from 23,839,430 to 1,534,713. In contrast, this restricted sample retains 121,493 out of 334,260 observations inside both the SFHA and the quasi-objective floodplain and 271,968 out of 1,583,724 observations outside the SFHA and inside the quasi-objective floodplain. While the SRTD approach leads to a more limited sample of properties at risk of flooding than those in the full sample, the estimation sample is still relatively large in the 1000-foot buffer SFHA sample. For the 500-foot buffer, the pattern of a greater portion of control observations dropping than identifying variation holds, but at more restrictive distances the portion of identifying variation dropping is relatively high. Therefore, we examine the 500-foot buffer sample along with the 1,000-foot buffer sample in our main results. We use the other highly restrictive samples as robustness checks but note the bias-variance tradeoff issues inherent in increasingly restrictive samples.

Covariate balance checks generally suggest there are no large jumps in observable dimensions for structural and environmental characteristics across the SFHA boundary (Figs A2-A3 in the Appendix). The variables that appear to have the largest jumps immediately across the SFHA boundary are the proportion of properties with lake presence and age of house. In the first case, inland properties outside the SFHA very close to the boundary have twice the mean proportion of observations on a lakefront than those just inside the SFHA boundary. In the second case,

inland properties just outside the SFHA very close to the boundary tend to be younger on average than their counterparts just inside the SFHA. However, these differences are not large. It is important to note that these mean values should be interpreted with caution because a market segment mean may not represent the local degree of covariate balance. However, it is challenging to assess the level of local covariate balance across all localized fixed effect areas. Nevertheless, we design our estimation approach to help control for any small covariate imbalance (Keele & Titiunik, 2015). First, we include observable dimensions to enhance covariate balance in the estimation of treatment effects. Second, a local linear running variable helps control for some of these differences that may affect price (Dell, 2010; Gelman & Imbens, 2019). The results shown in Table 1 on SFHA signals for the inland market segments are like SFHA price effects found in previous nationwide studies, which increases our confidence in the identification strategy using the pre-FIRM, inland data sample (Beltrán et al., 2018; Gourevitch et al., 2023; Hino & Burke, 2021).

Lastly, it is important to emphasize that we expect that price effects for *Haz* to reflect salience about whether houses are exposed to hazard, which our indicator represents, as opposed to differences in intensity about hazard, which our indicator aggregates. To test this expectation, we inspect how hazard exposure is distributed on either side of the SFHA boundary based on different distance cutoffs. If the intensity – i.e., the depth – of the hazard exposure is substantially greater within, as opposed to outside, of the SFHA, then the price effects of the *Haz* variable may be steeper inside the SFHA because there is more associated flood risk. In contrast, if the gradient of hazard exposure is insignificant across the SFHA boundary, then if price effects are steeper in the SFHA it more likely reflects salience about the fact that hazard is present rather than the fact hazard is more intense. Table A4 in the Appendix suggests that the distribution of inundation for exposed houses is slightly more intense inside the SFHA than outside. However, the differences are not large, and even outside the SFHA there are locations with very intense hazard exposure. The differences also appear attenuated when observations are restricted to properties closer to the SFHA boundary. Therefore, we do not expect slight differences in the distribution of hazard intensity to be salient to buyers, especially in the absence of disclosures.

4.3. Main Results

Having addressed our various identification issues, we now present our main results based on our sample of pre-firm homes located more than one mile from the coast. Table 2 shows results based on our SRTD model using a 1000-foot (columns 1-3) and 500-foot buffer (columns 4-6). All models include SFHA boundary fixed effects with standard errors clustered at the county level.

As with previous work, we find that information provided by the SFHA boundary negatively impacts housing prices; we also find that disclosing this information to buyers increases this impact. In the sample using the 1000-foot buffer, we find an overall discount of 3.1% for the SFHA. Separating this effect based on whether a states requires disclosure reveals of a 4.2% discount for states with disclosure and a 1.9% discount for those without it. In contrast, we find a 2.8% discount for quasi-objective hazard exposed houses in disclosure states and a 1.1% discount in non-disclosure states, irrespective of SFHA status. This suggests that buyers have a hard time distinguishing between hazard exposure of houses even with disclosure requirements, and that the SFHA is a prominent risk signal despite its inaccuracies.

Importantly, we find that the local spatial interaction of SFHA status

Table 2
Price effects for SFHA x Disc, Haz x Disc, and full interaction models.

	(1)	(2)	(3)	(4)	(5)	(6)
SFHA, Disc	-0.043***			-0.032***		
	(0.005)			(0.004)		
SFHA, Disc=0	-0.019***			-0.015***		
	(0.006)			(0.004)		
Haz, Disc		-0.028***			-0.017***	
		(0.004)			(0.004)	
Haz, Disc=0		-0.010+			-0.007	
		(0.005)			(0.004)	
SFHA, Haz, Disc			-0.065***			-0.050***
			(0.006)			(0.006)
SFHA, Haz, Disc=0			-0.024*			-0.022***
			(0.010)			(0.006)
SFHA, Haz=0, Disc			-0.029***			-0.019***
			(0.005)			(0.005)
SFHA, Haz=0, Disc=0			-0.018***			-0.012*
			(0.005)			(0.005)
SFHA=0, Haz, Disc			-0.017***			-0.006
			(0.004)			(0.003)
SFHA=0, Haz, Disc=0			-0.007+			-0.003
			(0.004)			(0.005)
Buffer	1000	1000	1000	500	500	500
Num. Obs.	3224818	3224818	3224818	1009434	1009434	1009434
R2 Adj.	0.794	0.794	0.794	0.810	0.810	0.810
R2 Within Adj.	0.005	0.005	0.005	0.026	0.026	0.026
RMSE	0.39	0.39	0.39	0.37	0.37	0.37
Std. Errors	by:County	by:County	by:County	by:County	by:County	by:County
FE: SFHA Boundary	X	X	X	X	X	X
SFHADist	X	X	X	X	X	X
Controls	X	X	X	X	X	X

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents results for the risk signals conveyed by the local spatial interaction of the SFHA, a quasi-objective representation of 1%-per-year exposure to flooding, and knowledge of flood history through disclosures. Column (1) corresponds to Eq. (2) with SFHA x Disc interaction treatment groups and column (2) corresponds to Eq. (2) with Haz x Disc interaction treatment groups. Column (3) corresponds to Eq. (2) in full. Columns (1)-(3) are estimated with the sample of pre-FIRM houses restricted to 1,000 feet from the nearest SFHA boundary. Columns (4)-(6) are defined analogously but run with the sample restricted by a 500-foot buffer. All models include SFHA boundary fixed effects, a running variable for distance to the boundary, full control for housing characteristics, and the standard errors are clustered at the county level.

and disclosure requirements accentuate the flood hazard exposure price signal. Houses inside the SFHA which face flood exposure and in disclosure states sell at a 6.3% discount, but the same kinds of houses in non-disclosure states sell at a 2.4% discount. These houses have similar hazard intensity estimates (see Table A7 in the Appendix), so we attribute the difference to the fact that disclosures are required. Even houses not exposed to hazard in disclosure states sell at a 2.9% discount if they are in the SFHA. In non-disclosure states, these kinds of houses sell for a 1.8% discount. Importantly, outside the SFHA, houses exposed to hazard sell at a 1.7% discount with flood history disclosures, but a 0.7% discount without disclosures. At 500 feet from the SFHA boundary, there is no discount for houses exposed to hazard outside the SFHA. Other price effects remain significant, but smaller, which may reflect the loss in identifying variation.

Capturing the full local spatial interaction of SFHA status, flood hazard exposure, and disclosure requirements is crucial to capture heterogeneity in flood risk pricing. Table 3 shows that inside the SFHA in disclosure states, the risk signal is significantly different depending on quasi-objective hazard exposure. However, in non-disclosure states there is no statistical difference between the risk signals for presence in the SFHA and the interaction of presence in the SFHA for exposed and non-exposed houses. This distinction is important because even in disclosure states the SFHA-based risk signal appears to underestimate the discount for exposed houses and overestimates the discount for non-exposed houses.

4.4. Robustness Checks

Sensitivity analyses suggests that our key findings about the trends of treatment effect estimates are robust. Specifically, the local interaction of the SFHA boundary and flood hazard exposure generates heterogeneous price effects based on the presence of flood history disclosure requirements. The weakest price signals are for houses with hazard exposure outside of the SFHA, and the strongest price signal is for houses exposed to flooding inside the SFHA and subject to seller disclosure. These trends are reinforced when restricting the inland estimation sample to properties further from the coast (Table A8). In addition, estimates in the 250-foot restricted sample for houses greater than one mile from the coast show the same trends as Table 2, but discounts are less steep (Table A9). However, in the 100-foot restricted sample the trends from Table 2 are not upheld (Table A9). This is likely because the sample is small (39,475 overall observations) and lacks identifying variation in treatment groups (Table A6).

Table A10 shows that treatment effect estimate trends are robust to when the sample is limited to properties that sell after their NFIP community's last update of flood insurance rate maps (FIRM) that delineate flood zones. One limitation with the National Flood Hazard Layer is that it only allows analysts to identify the flood zone boundary since the most recent update. This means that properties which sell before the most recent update may have been in a different flood zone at the time of the sale (Weill, 2022). In this robustness check, where we are more confident that properties are in the flood zone the digital maps imply at the time of sale, estimated treatment effects generally imply steeper discounts than our main results. This suggests that our main treatment effect estimates could suffer from slight attenuation bias because some of the control and treatment houses could be classified in the wrong SFHA group. However, the sample of current FIRM houses is one-third the size of our main sample. We prefer the main sample definition because rezoning into/out the SFHA is expected to affect a relatively small portion of our sample (Weill, 2022). An important extension of the present work is to incorporate the time varying SFHA status of houses to retain a large sample and obtain better treatment effect estimates.

Robustness checks also suggest that limiting the main estimation sample to pre-FIRM houses is important to control for unobserved confounding of structural defense differences across the SFHA boundary that the SRTD approach cannot control for. Table A11 shows treatment effect estimates for all houses greater than one mile from the coast and within 1000 feet of the nearest SFHA boundary, but not limited to pre-FIRM houses. Compared to Table 2, price effects are attenuated, but the direction and magnitude of the results are similar. For example, our preferred specification, based on pre-FIRM houses, yields an SFHA discount in disclosure states of 4.2% and a 1.9% discount in non-disclosure states. Alternatively, the full sample yields estimated discounts of 3.7% and 1.5%, respectively. The difference is important inside the SFHA and for properties exposed to hazard since houses at risk are also more likely to have structural defenses. Compared to discounts of 2.4% in non-disclosure states and 6.3% in disclosure states using pre-FIRM data, the full sample yields estimated discounts of 1.8% and 5.6%. However, the 1.8% estimate is only significant at the 10% level. The tendency to underestimate discounts due to structural defenses is especially important in the context of estimating how much risk is internalized in the housing stock.

With respect to the potential concern that hazard exposure treatment effects are influenced by the occurrence of recent flood events, we create an indicator for *PostFlood* based on monthly simulated runoff percentiles from the United States Geological Survey (USGS) WaterWatch (USGS, 2020) for HUC8 subbasins over a 100-year time period to identify months in which runoff percentiles suggest extreme flooding (99^{th} percentile or more). When this is interacted with our variable for quasi-objective flood hazard, *Haz*, it serves as a proxy for experiencing inundation equal to or greater than a 1-in-100-year event. Controlling for extreme flood events allows us to absorb capitalization in post-event periods that is challenging to attribute to information or damage effects. Our main treatment effects in the 1000-foot data samples are the same after controlling for recent, large flood events (see Table A12 in the Appendix). Results from these models show that there is a positive price effect in the year following a large flood event in areas that experience flooding, thus indicating a shift in the general market equilibrium.

Finally, robustness checks offer insights about the sensitivity of treatment effect estimates to the SRTD design as opposed to a more conventional repeated cross section. As we discussed above, the repeated cross section design produces a spurious result in the coastal market segment. Although the SRTD design avoids this outcome, it loses statistical power to identify a statistically significant effect and lacks observations to promote generalizable insights. Table A13 in the Appendix shows the SRTD design does not allow us to identify higher order treatment effects through Eq. (2). These results show that estimating our model without *SFHADist* in a repeated cross section again leads to biased treatment effects. However, doing the same for the inland market segment and using census block fixed effects results in a steeper discount. Notwithstanding, the model results from the repeated cross section for the inland segment reinforce our main results.

Table 3
Tests of statistical differences between SFHA x Disc, and full interaction model coefficients.

SFHADist	SFHA x Disc Group	SFHA x Haz x Disc Group	Z Test Statistic
1,000	SFHA, Disc	SFHA, Haz, Disc	2.82**
	SFHA, Disc	SFHA, Haz=0, Disc	-1.98*
	SFHA, Disc=0	SFHA, Haz, Disc=0	.43
	SFHA, Disc=0	SFHA, Haz=0, Disc=0	-.13
500	SFHA, Disc	SFHA, Haz, Disc	2.50*
	SFHA, Disc	SFHA, Haz=0, Disc	-2.03*
	SFHA, Disc=0	SFHA, Haz, Disc=0	.97
	SFHA, Disc=0	SFHA, Haz=0, Disc=0	-.47

+ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$
This table presents statistical comparisons of coefficient estimates for SFHA x Disc and SFHA x Haz x Disc model runs for the 1,000- and 500-foot buffers for the greater than one mile from coast market subsample.

5. Discussion

5.1. The importance of treatment effect heterogeneity on capitalization estimates and implications

Our econometric findings illustrate substantial heterogeneity in flood hazard capitalization that is often overlooked in previous studies. This heterogeneity has important implications for previous research findings about how well property markets internalize exposure to flood risk. Based on the procedure for estimating housing market internalization of flood risk in Gourevitch et al. (2023), with our implementation described in detail in Appendix B (Kousky and Walls, 2014; Pollack et al., 2022; Tate et al., 2016; FEMA 2009; FEMA 2011; OMB 2022; Reclamation Bureau 2022), we estimate that the net present value of expected annual flood damages outweighs the extent of flood hazard capitalization in the residential housing segment we examine (Fig. 2). We also find that a hypothetical policy reform that remaps SFHA boundaries to all houses with modeled flood hazard exposure in the quasi-objective 1% per year floodplain and enforces seller disclosures for flood history greatly reduces the margin of unpriced flood risk. We estimate that this reform could lead to risk internalization of 59.7% to 111.5% (Table 4, Fig. 2). The most dramatic increases in risk internalization occur outside the SFHA for properties at risk, underscoring the importance of the SFHA as a risk signal. In fact, an important consequence of the SFHA as a risk signal is that properties outside the quasi-objective floodplain erroneously mapped inside the SFHA sell at an inefficient discount. Improved risk signals could avoid this negative distributional effect.

An important implication of this illustration is that previous nationwide risk internalization estimates likely overestimate the degree of property overvaluation because they underestimate flood hazard capitalization for risk-prone properties in flood history disclosure states. Treatment effects based only on SFHA and disclosure status, which reflect the approaches of Hino and Burke (2021) and Gourevitch et al. (2023), suggest only 7.4% to 13.9% of risk is internalized (Table 4). In contrast, accounting for the full local spatial interaction of the SFHA, flood hazard exposure, and disclosures translates to 28.5% to 53.3% of risk being internalized (Table 4). Importantly, our results reinforce the conclusions from these studies that present hazard capitalization is lower than expected flood risks, but the degree of property overvaluation is subject to larger uncertainty than reported due to heterogeneity in flood hazard capitalization.

Another important implication from our results is that studies which purport to identify treatment effects related to present or future flood hazard exposure without controlling for current presence in the SFHA are possibly biased because the SFHA boundaries are the predominant risk signal in the housing market. For example, Bernstein et al., (2019) aimed to identify a sea-level rise exposure effect for properties within ¼ mile of the coast but include no explicit controls or matching for properties based on their presence in or outside the SFHA, and do not include disclosure requirements in their specification. The likelihood that sea-level rise treatment is correlated with both SFHA presence and current hazard raises questions about what their estimates identify. We don't identify any discount in the coastal market segment. One potential explanation for these discrepancies is that they find non-owner-occupied buyers drive the sea-level rise discount they identify. Considering our results, it's possible that these buyers, who Bernstein et al., (2019) call sophisticated, seek information about factors like SFHA status and therefore their estimated price discounts reflect SFHA status. We may not find such effects on the coast because we don't have access to an owner-occupied indicator in our dataset. An important expansion of the present study is to examine heterogeneity beyond our estimates based on owner-occupied status of buyers and sellers.

5.2. The potential impacts and extensions of Risk Rating 2.0 on the U.S. housing market

Our results have important implications for the ongoing implementation of FEMA's Risk Rating 2.0 (RR 2.0) reform. FEMA promotes RR 2.0 based on state-of-the-art flood hazard modeling tools and flood

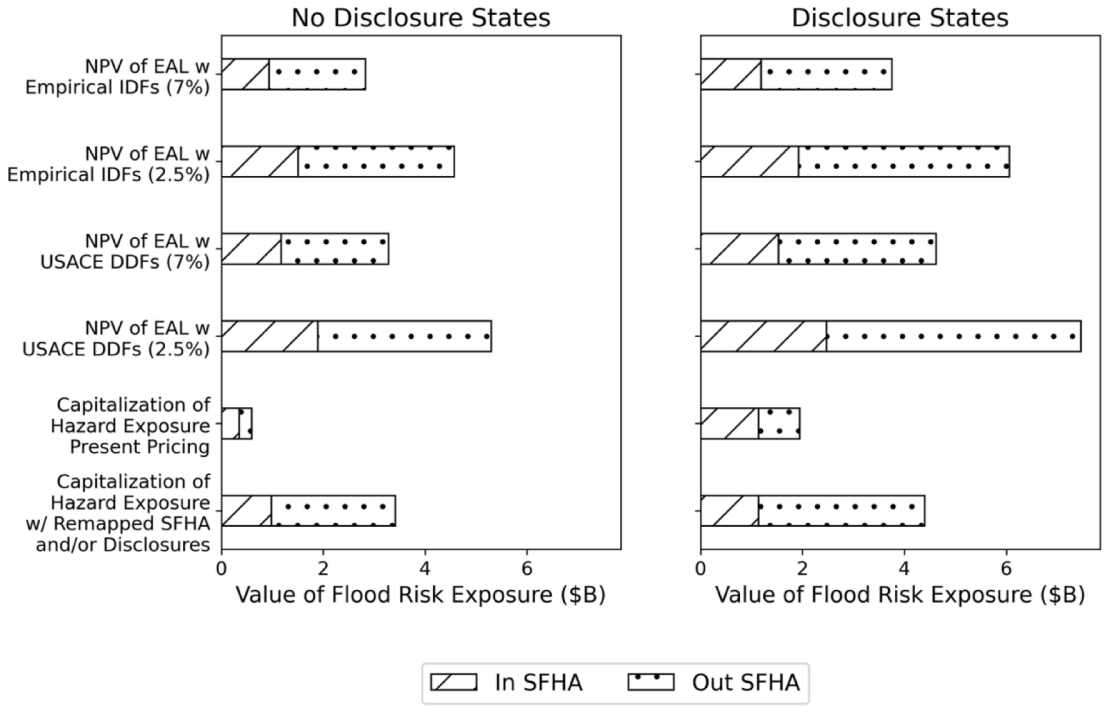

Fig. 2. The net present value of expected annual losses from damages to structures estimated using different damage functions and discount rates is compared to the implied monetary capitalization of hazard exposure in disclosure and non-disclosure states. Capitalization is estimated based on the results in column 3 of Table 2. Totals are delineated by striped bars for estimates in the SFHA and dotted bars for estimates outside the SFHA. The effect of a hypothetical reform where all properties are subject to flood history disclosures and the SFHA boundary is remapped to correspond to the quasi-objective 1% per year floodplain.

Table 4
Market value, net present value of expected annual losses, and risk internalization for pre-FIRM 1,000 feet from SFHA boundary sample. All cells are in terms of 2019 $B. NPV of EAL in Haz=0 groups occurs because some properties only face flood hazard outside the 1% per year quasi-objective floodplain. Capitalization is always 0 for control groups.

Group	Market Value	NPV EAL (Upper)	NPV EAL (Lower)	Capitalization w/ SFHA & Disc.	Capitalization w/ All Price Effects	Capitalization Under Reforms
SFHA Haz Disc	13.89	1.86	0.92	0.24	0.97	0.97
SFHA Haz Disc=0	11.99	1.5	0.76	0.08	0.29	0.83
SFHA Haz=0 Disc	17.22	0.03	0.02	0.3	0.51	0
SFHA Haz=0 Disc=0	19.15	0.27	0.18	0.13	0.35	0
SFHA=0 Haz Disc	35.29	3.49	1.82	0	0.61	2.45
SFHA=0 Haz Disc=0	26.69	2.4	1.38	0	0.19	1.86
SFHA=0 Haz=0 Disc	174.29	0.12	0.08	0	0	0
SFHA=0 Haz=0 Disc=0	158.92	0.56	0.31	0	0	0
Total	457.44	10.23	5.48	.76	2.92	6.11

loss estimation techniques to deliver rates that are "actuarily sound, equitable, easier to understand and better reflect a property's flood risk" (FEMA, 2020). However, despite RR 2.0's advances in hazard modeling and representing flood risk relative to the historic SFHA boundaries, FEMA still administers the insurance purchase requirement based off current SFHA delineations. This policy undercuts the potential of RR 2.0 to impact risk management in the U.S. and send strong risk signals to property markets (Mulder & Kousky, 2023). While the ongoing implementation of RR 2.0 is likely to have important reverberations throughout the U.S. property market through the more accurate price signal it may convey about risk (Hennighausen et al., 2023), this may only occur inside the SFHA or in disclosure states where buyers are aware of these changes and which properties they affect. Because the SFHA border is an important contribution to the risk signal, remapping it to the extent of hazard exposure implied by RR 2.0 is an important policy lever for FEMA to consider. In addition to promoting better risk internalization, our results suggest that updating maps to better reflect true hazard exposure could avoid negative distributional effects that affect properties potentially not at risk but priced at a discount due to presence in the current SFHA borders. However, we also caution that more research on potentially unintended consequences of such actions, such as rapid and widespread losses in property valuation and knock-on consequences, is needed (Gourevitch et al., 2023). Notwithstanding these concerns, our results support emerging evidence that risk rating without information provision underachieves on stated policy goals (Mulder & Kousky, 2023).

In addition to remapping the SFHA border, our results join previous studies in illustrating the importance of information about flood hazard conveyed through legally mandated seller disclosures (Gourevitch et al., 2023; Hino & Burke, 2021). Expanding on previous studies, we illustrate the impact on risk internalization due to disclosures in more detail. We show that estimates based on just SFHA presence may substantially overestimate property overvaluation, especially because seller disclosures appear effective in conveying information about flood hazard that SFHA boundaries miss. We then show that implementing reforms which incorporate SFHA border remapping and expand disclosures can substantially reduce the degree to which risk-prone properties are overvalued. Our results can serve as a useful evidence point for FEMA which has recently begun exploring its ability to expand flood disclosure requirements to better manage the costs of flooding (FEMA, 2022b).

5.3. The value of large-scale property assessment datasets for policy-relevant hedonic analyses

A final implication of our results is that large-scale data like ZTRAX is useful to assess important research questions on environmental valuation in a nationwide policy context (Zillow, 2019), but are also limited in what they can identify. In the case of flood hazard capitalization, we respond to the challenge of few relevant observable variables available at the scale of our policy and research questions by implementing a SRTD approach. However, on its own this approach doesn't address the increased likelihood of structural defenses in houses in the SFHA or challenging identification issues in the coastal market segment. Fortunately (in the limited perspective of econometric identification goals) the use of pre-FIRM houses allows us to reduce this structural defense confounding in the inland segment and this sample consistently results in steeper discounts in the SFHA than the full sample of houses. The flip side of this result is that there are many sources of heterogeneity that large-scale analyses are unlikely to identify, and this heterogeneity can have important implications for research or policy questions. For example, the amenity value of proximity to hazardous water features or the presence of structural defenses, or information on timing of state-level disclosures, have been employed in only a few county-level studies (Pope, 2008; Bin et al., 2008; Pollack and Kaufmann, 2022). Our estimation approach absorbs these effects in the localized fixed effect areas.

Our analysis suggests that results from large-scale efforts can inform research directions for local case-studies, and vice versa, to advance scientific understanding. For example, our study is motivated by detailed county-level case studies (Atreya & Czajkowski, 2019; Atreya & Ferreira, 2015; Davlasheridze & Fan, 2019b; Pollack & Kaufmann, 2022) that address sources of confounding previously overlooked in hedonic models for flood hazard capitalization. Because large-scale studies about risk internalization or flood hazard capitalization had

not incorporated research insights about structural defense confounding and the overlap of direct hazard exposure on both sides of SFHA boundaries, there was an important research gap to fill. Our study is just a first step in filling this research gap and highlights many potential avenues for county-level or more local case studies to elucidate our findings in more detail. Specific extensions include incorporating detailed data on structural defense, adopting methods in calculating coastal viewsheds and representing coastal amenity, and enriching state-level disclosure data with time of implementation and more specific requirements (Pope, 2008; Bin et al., 2008; Pollack and Kaufmann, 2022). Further, turning attention to local case studies presents the opportunity for analysts to identify whether results are consistent when more accurate local flood hazard models, flood event occurrences, and other hazard representations are used to specify quasi-objective floodplain boundaries and flood indicators (Hawker et al., 2020; Sanders et al., 2023). Similarly, flood risk estimation is very uncertain and these qualities are currently challenging to estimate rigorously in large-scale settings (Apel et al., 2004, 2008; De Moel et al., 2014; de Moel & Aerts, 2011; Figueiredo et al., 2018; Merz et al., 2004; Pollack, Sue Wing, & Nolte, 2022; Rözer et al., 2019; Tate et al., 2015; Wagenaar et al., 2016; Wing et al., 2022; Zarekarizi et al., 2020). Scrutinizing our results in settings where estimating uncertainty in flood risk faces less computational challenges is an important extension or the present study. These directions, only a subset of worthwhile endeavors to improve identification and uncover sources of heterogeneity, exemplify that insights from large-scale and detailed case-studies can inform priorities and improvements for each other in turn to advance knowledge and tackle vexing problems.

6. Conclusion

In this study, we implemented a nationwide SRTD to learn about the drivers of flood risk capitalization and the implications for flood risk internalization in the U.S. single family house property market. We found that the local spatial interaction between the SFHA, houses exposed to flood hazard, and state disclosure laws about flood history produce heterogeneity in risk signals. We showed that current estimates of risk internalization may underestimate internalization because they don't account for this heterogeneity. Nevertheless, our results support previous conclusions that flood risk is undervalued. Building on this result, we provide evidence that expanding the SFHA to the extent of hazard exposure truly faced by properties, and expanding flood history disclosure requirements, can substantially close the gap between expected flood risk and property valuation of exposure to flooding. Previous studies have made similar recommendations (FEMA, 2015, 2022b; Kousky et al., 2020) and FEMA may have the tools at its disposal to implement them. First, the hazard exposure reflected by the efforts underlying RR 2.0 could be used beyond its current scope of increasing the accuracy of insurance rates to also delineate a new policy-based quasi-objective floodplain boundary. Second, the evidence presented here and in other related studies (Gourevitch et al., 2023; Hino & Burke, 2021) broadly supports efforts to require nationwide disclosure of flood history and SFHA status (Department of Homeland Security, 2022).

In addition to these broad theoretical insights and policy implications, our analysis highlights limitations, and extensions for future research. Beyond the research questions we pursued, we emphasized the importance of large-scale transaction data like ZTRAX to enable the kinds of feedbacks between general large-scale and detailed case-study research to provide decision-makers with an improved understanding of how the effect of various policy levers reverberate in property markets.

Funding

We acknowledge funding from Boston University, U.S. Department of Energy, Office of Science, Biological and Environmental Research Program, Earth and Environmental Systems Modeling, MultiSector Dynamics, Grant/Award Numbers: DE-SC0016162, DE-SC0022141

CRediT authorship contribution statement

Adam B Pollack: Conceptualization, Data curation, Formal analysis, Methodology, Writing – original draft, Writing – review & editing, Software. **Douglas H Wrenn:** Conceptualization, Methodology, Writing – review & editing. **Christoph Nolte:** Data curation, Funding acquisition, Writing – review & editing, Software. **Ian Sue Wing:** Conceptualization, Funding acquisition, Methodology, Writing – review & editing.

Data availability

The authors do not have permission to share data.

Acknowledgements

The authors thank participants in the ZTRAX Special Issue Workshop. We thank Jeff Zabel and anonymous reviewer for thoughtful and helpful comments on an early version of this manuscript. Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group. Parcel data for approximately two thirds of U.S. counties was provided by Regrid through its "Data with Purpose" program (https://regrid.com/purpose).

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhe.2023.101956.

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