The Equilibrium Effects of Floodplain Regulation on Housing Markets

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Abstract

High-risk floodplains cover a substantial and increasing share of the US housing market, and are heavily regulated to mitigate damages. We investigate the effect of floodplain regulation on the location of development, housing prices, estimated flood damages, and consumer welfare in the state of Florida. Using a spatial regression discontinuity design, we find that just inside the regulated floodplain, land is 9% less-developed and house prices are 6% higher. Though the regulations yield benefits — we estimate that the mandatory flood-safe building codes reduce damages and insurance premiums by at least 3.2% of house value — consumers are not willing to pay more for elevated houses. We use the boundary discontinuity to estimate the effect of floodplain regulation in an equilibrium model of residential choices and housing supply. Relative to an unregulated benchmark, existing floodplain regulation reduces new development in regulated floodplains by 16%. This relocation reduces expected future damages by 9%, or $950 per newly-developed house. The building code averts an additional 30% of damages, for a total reduction of $2.6 billion per county. Despite the scope to correct inefficiently-high flood risk, the policy reduces welfare: averted damages do not exceed consumer surplus losses from higher prices and relocation to less-desirable places.

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1 Introduction

Floods cause an estimated $32 billion in damages per year, making them the costliest form of natural disaster in the United States. These losses are expected to double by 2050, reflecting both an increase in flood hazard due to climate change and a concentration of population growth in risky locations (Wing et al., 2022). Despite these risks, demand for flood insurance is low (Kriesel and Landry, 2004; Kousky, 2018), plausibly because of moral hazard from government aid (Kydland and Prescott, 1977; Kousky et al., 2018b) or because of risk misperception (Bakkensen and Barrage, 2021; Gallagher, 2014; Wagner, 2021; Mulder, 2021). These same frictions may also reduce consumers' willingness to pay for safer houses, generating inefficiently-high development in risky areas. In the US, policymakers have responded to the threat of floodplain overdevelopment by demarcating "Special Flood Hazard Areas" (SFHAs) as especially risky parts of the floodplain. In the SFHA, homeowners face a flood insurance mandate and higher flood insurance prices, and developers must comply with building and permitting rules.[1]

This paper investigates the impact of floodplain regulation on the location of development, housing prices, estimated flood damages, and consumer welfare. While floodplain regulation is the primary policy instrument to regulate flood risk exposure and is a prevalent form of zoning regulation nationwide,[2] there is limited evidence on its costs or benefits. The policy could reduce damages by both shifting development to safer areas and requiring adaptation in high-risk locations. However, these adaptations also incur costs: homeowners move to less-preferred neighborhoods and developers expend labor and materials to meet higher building standards. Understanding the relative magnitudes of the benefits and costs of this policy is critical for determining if and how policymakers should intervene further in the face of

[1]The policy instrument of creating a binary distinction of "floodplain" or not and imposing both insurance and building requirements is not unique to the United States. EU countries and Australia also manage flood risk via the creation of flood maps that influence both flood insurance and building codes (de Moel et al., 2009; Golnaraghi et al., 2020).

[2]6% of properties are in an area designated as high-risk (First Street Foundation, 2020).

increasing disaster risks.

We begin by studying how land use and house prices differ discontinuously across SFHA boundaries in flood maps first drawn in the 1970s and early 1980s. The spatial regression discontinuity design addresses omitted variables bias by leveraging the coarse classification of the early SFHA and the assumption that unobservable characteristics of the land evolve smoothly through the SFHA boundary. The historic maps also address concerns about reverse causality, which arise over time from homeowner behavior in response to the flood maps. We construct a rich dataset that pairs newly-digitized archival scans of Florida flood maps with granular satellite data on land use. We further add spatially-linked administrative data on parcel characteristics and transaction prices. We document that new development just inside the floodplain is 9% lower than outside, while house prices are 6% higher. We see no evidence of a discontinuity in development at the boundary in the 1970s-1980s, suggesting that neither omitted variables bias nor endogenous regulation drives these results. While the negative effect on development is consistent with SFHA designation suppressing either supply or demand, the non-negative effect on prices indicates a meaningful role for the supply side. Policy and research discussions of floodplain regulations have largely ignored the costs they impose on developers, but our results indicate that these costs are first-order.

To estimate the scope for policy to reduce distortions in location and adaptation decisions, we next study how flood costs and house prices respond to mandatory floodplain adaptation. To avoid omitted variable bias, we exploit the discrete introduction of flood-safety building standards in the year that floodplain regulations first applied to an area in an event study design. We leverage National Flood Insurance Program (NFIP) policy and claims data to measure elevation status, insurance payouts, and policy costs. We pair this with our rich spatially-linked parcel data on house sales prices. We find that the regulation increases the share of houses that are elevated by 27 percentage points, reduces net present value of insurance payouts by an amount equal to 4.2% of the average house value, and reduces net present value of insurance premiums by an amount equal to 3.2% of average house value.[3] Despite this greater safety, elevation has no statistically-significant effect on house prices. If elevation only affects consumer valuation via the channel of reduced risk, these results indicate that consumers fully fail to internalize the value of their own expected insurance premiums. This behavior is consistent with a range of plausible frictions, including moral hazard due to future investments (Kydland and Prescott, 1977) or disaster aid (Kousky et al., 2018b; Baylis and Boomhower, 2019), or behavioral frictions such as misperceptions of risk (Kunreuther, 1996; Gallagher, 2014; Bakkensen and Barrage, 2021). These frictions plausibly distort consumer demand for both adapted houses and low-risk locations. Therefore, our results indicate considerable scope for policy to improve welfare by mandating adaptation and discouraging development in risky areas.

Although we find that floodplain regulations suppress development in floodplains, our reduced-form results alone cannot tell us to what extent the policy reduced flood risk or what its overall welfare impacts might be. First, equilibrium quantities and prices on either side of the boundary are the result of households choosing among alternative locations, and developers choosing whether or not to develop. Applying floodplain regulation in one area impacts demand and therefore prices and development in another. To interpret our reduced-form results, we must explicitly account for the residential choices and housing supply curves that yield these equilibrium outcomes. Second, the reduced-form estimates capture outcomes in a narrow slice of land, while the spatial equilibrium resolves prices and quantities across the entire market. We must account for market-wide characteristics such as the joint distribution of amenities and flood risks when estimating the policy's effects.

In the second part of the paper, we specify a model of consumer location choices and housing supply. In the demand model, individuals maximize utility when choosing census-tract-by-flood-zone locations, as a function of prices, floodplain status, and unobserved amenities. On the supply side, landowners build houses when doing so is more profitable than their

[3] Throughout the paper, net present value calculations use a discount rate of 5%.

outside option of land use; housing profits depend on housing prices, tract-zone-specific construction costs, and any costs of required elevation. We calibrate supply and demand price elasticities to estimates from the literature (Baum-Snow and Han, 2019; Calder-Wang, 2021; Song, 2021). We then estimate the model's key parameters — demand and supply costs of floodplain designation — by matching the cross-border differences we find in our reduced-form spatial regression discontinuity analysis. We estimate the parameters only on land that is located 100 feet or less from an SFHA boundary, but conduct counterfactuals using all land in a given market.

Our model allows us to translate our regression discontinuity estimates into a regulatory cost to consumers of 23% of home value and a cost to developers of 25% of home value. These large costs of floodplain regulation underscore the power of this policy to affect house location. Using our estimated parameters, we find that relative to an unregulated benchmark, floodplain regulation reduces new development in the regulated area by 16%, or approximately 118,000 houses in our 10-county sample. Under current flood risk levels, this relocation reduces the net present value of expected damages by 9%, or $950 per newly-developed house. The building code averts an additional 30% of damages, for a total reduction of $2.6 billion per county. However, the regulation achieves this risk reduction at a considerable cost, raising prices by 1.7% market-wide. Prices fall by 3.8% inside the regulated SFHA and increase by 4.3% outside the regulated area, illustrating how equilibrium effects can offset the direct effects of the regulation. We compute the policy's effect on welfare, defined to capture both consumer surplus and any damages that consumers do not consider. We find that the policy reduces welfare by at least $2 billion and potentially more than $34 billion. A small share of these welfare losses come from direct adaptation costs. The bulk comes from pecuniary externalities, as higher demand outside of the SFHA increases prices for all houses, and from consumers shifting to lower-amenity areas. Non-financial costs of the regulation further exacerbate the welfare losses.

Our paper relates to several strands of the literature. Most narrowly, it expands upon previous work studying floodplain regulations. While others have studied the effect of regulations on adaptation (Kousky et al., 2018b; Wagner, 2021), we build on this literature by inquiring about damages additionally avoided through the location channel.[4] Another strand of the literature has also studied the floodplain discount on house prices (Harrison et al., 2001; Bin and Landry, 2013; Indaco et al., 2018; Gibson and Mullins, 2020; Hino and Burke, 2021)); these efforts have typically relied on cross-sectional or panel regressions. We build on this work by credibly addressing empirical challenges related to both omitted variable bias and endogenous designation of floodplain status on already-developed land. Finally, to our knowledge we are the first to attempt to use reduced-form estimates of the policy's effects to quantify the policy's costs on both the adaptation and location margins.

Beyond the specific context of flood risk, this paper connects to a larger literature documenting frictions in mitigating or adapting to climate risk (Baylis and Boomhower, 2019; Bakkensen and Ma, 2019; Kousky et al., 2018b; Wagner, 2021). Recent work studying mandatory adaptation in the context of wildfires finds that adaptation cuts damages in half (Baylis and Boomhower, 2021). We build on this work in the context of flooding by studying both supply-side and demand-side treatments, disentangling them with a structural model of the housing market. Moreover, in addition to estimating welfare effects of adaptation-in-place, we also account for welfare impacts that accrue via the relocation channel.

Our paper also speaks to the importance of accounting for equilibrium effects when estimating the costs and benefits of a policy. In the specific context of zoning regulations, this analysis builds on recent work that combines boundary discontinuities with models of consumer choice and housing supply to estimate policy impacts (Anagol et al., 2021; Song,

[4] We are not the first to study the effect of floodplain regulation on location. Browne et al. (2019) find that participating in the National Flood Insurance Program (NFIP) shifted housing development from coastal to non-coastal areas in Florida, while Peralta and Scott (2019) find that across all early-joining counties, NFIP participation reduced population outflows. We build on this work by analyzing the effect of risk designation on development *within* a community rather than across communities, holding constant the availability of flood insurance.

2021). Here, we apply this approach to a setting with a clear and measurable benefit of regulation: reducing flood damages. More broadly, we build on a body of work studying how attribute-based supply-side regulation affects the equilibrium provision of goods (Ito and Sallee, 2018; Germeshausen, 2018; Kellogg, 2020). For example, Ito and Sallee (2018) consider how the US regulation of fuel mileage, based on the secondary attribute of vehicle size, shifted the market towards larger vehicles. Our analysis describes a similar phenomenon, where regulations based on floodplain status shift development out of the floodplain. On the demand side, another strand of literature exemplified by Barahona et al. (2020) explores how mandatory labeling of goods with hidden costs alters the equilibrium characteristics of those goods. In our context, mandatory labeling of risky houses is one important channel through which floodplain policy reduces equilibrium flood risk.

The next section describes the institutional details of the National Flood Insurance Program, including the regulations imposed inside the SFHA and the process of generating flood maps that distinguish between SFHA and non-SFHA land. The following section describes our setting — the state of Florida — and data. In Section 4, we present reduced-form evidence of decreases in modern development and increases in price just inside the historical SFHA. Section 5 presents evidence that consumers fail to internalize flood risk when purchasing a house. We specify our equilibrium model of the housing market in Section 6 and discuss its estimation in Section 7. Section 8 simulates counterfactual distributions of development and prices under an alternative policy regime, and Section 9 discusses welfare implications. We conclude by considering directions for future work and implications for policy.

2 Institutional Background

2.1 The National Flood Insurance Program (NFIP) and Special Flood Hazard Areas (SFHAs)

Congress established the National Flood Insurance Program (NFIP) in 1968 in response to high flood losses and a perception that lackluster local regulation permitted excessive development in high-risk areas (Burby, 2001). Today, the NFIP, administered by the Federal Emergency Management Authority (FEMA), remains the primary provider of flood risk protection and regulator of floodplain development in the United States. The NFIP underwrites over 90% of flood insurance policies, creates the most widely-used measures of flood risk through its flood mapping process, and sets construction standards for buildings in areas mapped as high risk (Kousky et al., 2018a).

The most important distinction for both insurance policies and floodplain regulation is between areas that are determined to be high-risk, known as Special Flood Hazard Areas (SFHAs), and those that are not. In the SFHA, some homeowners face a flood insurance mandate and higher flood insurance prices for otherwise-equivalent houses.[5] Though the insurance mandate is loosely enforced, approximately 50% of homeowners in SFHAs hold a flood insurance policy, compared to 2% outside of SFHAs (Bradt et al., 2021). In the counties we study, annual flood insurance premiums cost on average \$740 (125%) more for homes located inside the SFHA than for homes outside the SFHA.

All new construction and substantial home improvements in SFHAs must comply with building regulations. The most important regulation is that a home's lowest floor must lie above the Base Flood Elevation (BFE).[6] This elevation requirement is well-founded; building above

[5] Homeowners with federally-backed mortgages are legally required to purchase flood insurance.

[6] While popular images of elevated houses commonly show those on posts or piles, this adaptation tends to appear only in close proximity to the coast, where wave action can destroy walls. In the mostly inland areas

the BFE reduces flood damages by at least 30% (Wagner, 2021). Other costs imposed on development in SFHAs include substantial permitting requirements and the production of elevation certificates.[7]

Throughout the United States, 10% of land[8] and 6% of properties are in an SFHA (First Street Foundation, 2020) . Due to both climate change and population growth, the share of the US population at a level of risk that triggers SFHA classification is expected to rise from 13% to 15% by 2050 (Wing et al., 2018). This makes SFHA-induced building requirements one of the most common forms of zoning regulation in the U.S., comparable to minimum lot area requirements, which apply to an estimated 16% of single-family homes (Song, 2021).

2.2 The Flood Mapping Process

FEMA produces flood maps that distinguish between SFHA and non-SFHA locations. These flood maps are generated by combining hydrological models with historical flood data and elevation surveys to determine floodwater depths. Areas with at least a 1% chance of flooding every year are designated as SFHAs. For inland areas, the models capture the estimated height of flooding from overflowing rivers and streams following an extreme precipitation event. Coastal areas incorporate additional features such as sea level, tides, storm surge, and wave effects (Hobbins et al., 2021). All studies take as inputs the topography of a region. In more populous regions, they also incorporate detailed waterway surveys to improve the accuracy of the maps (Maune, 2014).

Our spatial regression discontinuity approach relies on the assumption that SFHA delineation is a coarsening of a continuous measure of flood risk and does not follow the contours of true

we study, enclosed elevated foundations are more common. This approach allows garages and unfinished basements to be constructed at ground level. See Figure A.1 for an example of an adapted home in Naples, FL.

[7] Legislators designing the NFIP in the 1960s assumed that these costs would divert development away from high risk areas, further reducing flood damages (Burby, 2001).

[8] Authors' calculations using 2017 flood maps.

discontinuities in flood risk or other amenities. The validity of this assumption relies on the details of the mapping technology. In our specific context, there is substantial scope for imprecision in the historic boundaries we exploit.

The accuracy of a flood map depends on both the accuracy of the estimates of land elevation and the accuracy of the hydraulic model which simulates the amount of excess water in a flood event (National Research Council, 2009). Historically, engineers estimated land elevation based on US Geological Service contour lines, which suffer absolute elevation error on the order of meters.[9] After floodwater heights have been mapped, the floodplain is delineated by transforming vertical flood elevation profiles into horizontal floodplain boundaries. Because the same elevation of floodwaters yields a much wider floodplain in flat than steep areas, the floodplain boundary delineation is four to five times more uncertain in flat areas, such as Florida, compared to hillier areas (National Research Council, 2009). The floodplains of inland Florida are particularly uncertain since their drainage is dominated by shallow water flow, an atypical landscape for which FEMA does not specify hydrology and hydraulics guidelines.

During the 1970s and early 1980s, FEMA created initial flood maps for each participating community. FEMA is required to update these maps every 5 years to account for improved mapping technology and changes in development that may impact flood risk (National Research Council, 2009). In practice they are often updated much less frequently: as of 2017, more than 50% of maps were more than 5 years old (U. S. Office of Inspector General, 2017). In between official remapping cycles, property owners can request map amendments to correct inaccuracies (National Research Council, 2009) or petition for a fill-based carve-out if homeowners have elevated the land underneath their home. According to our conversations with floodplain managers, in the early years of the program the scale of paper maps meant that fill-based carve-outs of the SFHA had to be at least 6 acres. Because of this requirement,

[9] Today, LiDAR technology has improved the accuracy of land elevation models. More-powerful computing has also improved the precision of hydraulic modeling over time.

most houses did not find it cost-effective to pursue a carve-out. More recently, the adoption of digital maps has enabled these carve-outs at a smaller scale, and they have subsequently become more common.

3 Setting and Data

Our empirical context is the state of Florida, one of the most flood-prone and populous states in the United States. This makes it an ideal setting to study how floodplain regulation impacts housing markets and disaster damages. Nearly 50% of land and 19% of homes in Florida are located in SFHAs. Florida alone accounts for 35% of the nation's NFIP policies.[10] In addition, Florida publishes detailed data on housing transactions and locations, enabling the granular analysis necessary for our empirical strategy.

We bring together three primary sources of data to conduct our analysis.

Digitized historic and current flood maps Our analysis is organized around archival scans of early flood maps that we digitized for parts of eleven counties.[11] We aimed to collect the first Flood Insurance Rate Maps ever drawn. In a few instances, constraints on the availability or formatting of these first maps made this impossible. In these cases, we were able to digitize maps that were drawn only a few years later. All but two of the 120 panels we digitized became effective between 1977 and 1984. Appendix A.2 describes the sample selection process in more detail.

Figure 1a presents an excerpt of these digitized flood maps. Figure 1b shows the geographic coverage of our digitized sample. While budget constraints prohibited digitizing the entire

[10] See https://nfipservices.floodsmart.gov//reports-flood-insurance-data for details.

[11] These archival scans were downloaded from FEMA's Map Service Center. See https://msc.fema.gov/portal/advanceSearch for details. In order to maximize power, we prioritized areas with substantial development over the last 40 years. Our estimates on development when expressed in levels may therefore generalize less well to other settings, but this choice will not affect results expressed as a percentage of new development.

state of Florida, we are able to obtain good coverage of most major population centers. Table 1 illustrates that our sample covers 10.5% of the land mass in Florida, but 14% of all homes, reflecting the fact that our digitized areas are more developed and populous than average.

We pair our newly-digitized historic flood maps with snapshots of flood maps for the whole state from 1996 and 2017. Statewide, almost half of all land was classified as an SFHA in 2017, underscoring the relevance of this form of regulation for real estate development across the state. In our digitized counties, 30% of land is in an SFHA.

Satellite-Derived Land Use Data Figure 1a demonstrates that the floodplain distinctions are detailed, necessitating spatially granular data on land use to study outcomes on either side of the boundary. We use two datasets to measure land use at two points in time. The first is US Geological Survey data on land use patterns contemporaneous to the time the original maps were drawn. This dataset consists of a 30x30 meter raster describing land use and land cover as belonging to one of nine mutually exclusive meta-categories, including urban/built-up land, agriculture, wetland, and water.[12] The categories were determined based on high-altitude photographs taken between 1971 and 1982 (1976 is the median and mode image date). We define "developed" land in this data as land falling into the "urban/built-up" category, which includes land used for residential, commercial, industrial, or transportation purposes. For current land use, we employ the National Land Cover Database (NLCD) from 2016, which classifies Landsat remote sensing imagery into similar categories of land cover, also in a 30x30 meter grid. Our main category of interest, "developed", indicates land that is covered by a mixture of constructed materials and mostly-lawn-grass vegetation.

Table 1 panel A presents land use summary statistics for the state of Florida and our digitized subsample. Commensurate with Florida's population boom between 1980 and 2020,[13] Table

[12]Across Florida, the median number of raster grid cells per census tract is 4865.

[13]Between 1980 and 2020, Florida's population more than doubled from 9.75 million to 21.5 million (US

1 illustrates that development increased substantially both statewide (2.6x) and in our sample of interest (2.4x).

Parcel Characteristics Data from the Florida Department of Revenue property tax records from 2005 to 2020 provide detailed information about structures, including sales prices. We precisely geolocate each parcel's exact building location using Microsoft's open-source building footprints dataset.[14] Across Florida, each census tract — the geographic unit of analysis in our model of the housing market — has a median of 1873 parcels, 1680 of which are residential and 994 of which are single-family houses. Table 1 panel C summarizes average home prices statewide and in our sample of interest.

Other Sources of Data We supplement the three sources of data described above with a few other auxiliary datasets. We obtain historical data on home prices from the 1980 Census (Manson et al., 2021). For homes insured through the NFIP, we obtain data on flood insurance policies and claims from 2009 to 2019 from FEMA.

To assess the risk profile of development across policy regimes, we draw on spatially-granular estimates of flood risk from a third-party hydrological model. This model is produced by the First Street Foundation, a nonprofit organization devoted to quantifying and communicating climate risks. First Street aims to improve on government-issued risk assessments, which have been criticized for being out-of-date and inaccurate (Keller et al., 2017; Brannon and Blask, 2017; Frank, 2020b; Wing et al., 2022). First Street takes into account sources of flooding that NFIP maps ignore (e.g. rainfall), provides estimates for areas that FEMA had not been able to survey, and accounts for sea level rise due to climate change (First Street Foundation, 2020). Although First Street's model does not employ the "gold standard" of surveying that FEMA uses in the highest-risk locations, their validation exercises have

Census Bureau).

[14]This dataset is also derived from satellite imagery, mostly captured in 2019. See https://github.com/microsoft/USBuildingFootprints for more details.

achieved 80-90% flood extent similarity with historical observations and they are considered to “fus[e] the accuracy of local studies with the spatial continuity of large-scale models” (Wing et al., 2022). Nationally, First Street’s model estimates that NFIP flood maps identify only 60% of areas that face a 1% chance of flooding every year (First Street Foundation, 2020). In Florida this discrepancy is smaller: only about 10% more houses face substantial risk under the First Street model than the current flood maps indicate. However, First Street and FEMA disagree about the exact location of this risk. Appendix Table A.1 tabulates the discrepancies between FEMA’s flood maps and the First Street model in our sample, showing that more than one-fifth of parcels are categorized differently by FEMA and First Street.

4 Equilibrium Development and Price Effects of Floodplain Designation

4.1 Empirical Strategy

Our principal goal is to estimate the impact of SFHA designation and regulation on housing market outcomes — specifically, the location of real estate development and prices. This presents two challenges. First, SFHA designation is likely correlated with other amenities like coastal access. Second, SFHA designation may be endogenous to development, as the mapping process allows homeowners to petition for map corrections or adapt their homes to “remove” themselves from the SFHA.

In practice, we found that both omitted variables and reverse causality are relevant threats to credibly isolating the effect of floodplain regulation. Table 1 indicates that land in SFHAs is more likely to be water or wetlands and is closer to the coast. Moreover, development status likely directly affects SFHA status. Inside the SFHA, homeowners who are correctly mapped have an incentive to elevate their house to “escape” the SFHA. Homeowners who were incorrectly mapped into an SFHA would benefit from petitioning FEMA to correct the mistake. Meanwhile, nobody faces an incentive to elevate or correct mapping errors on undeveloped land. Indeed, Appendix Table A.1 shows direct evidence of such reverse causality: land that was developed as of 2004 is more likely to be remapped out of a floodplain in the next map revision than land that was undeveloped. This dynamic could lead to a mechanical relationship between development status and SFHA status that is unrelated to the causal effect of interest.

We address these two challenges with a spatial regression discontinuity design that leverages the first flood maps drawn in the late 1970s and early 1980s. The historic maps address concerns about reverse causality, which arise over time from homeowner behavior in response to the flood maps. The regression discontinuity addresses omitted variables bias by leveraging the coarse classification of the SFHA and the assumption that unobservable characteristics of the land evolve smoothly through the SFHA boundary. We probe this identifying assumption by examining land use outcomes before or contemporaneous to the drawing of these initial flood maps.

We estimate our boundary discontinuity design by examining how outcomes at each 30x30m pixel vary as a function of the distance to the SFHA boundary. Formally, we estimate the following model:

$$y_i = \beta 1\{d_i > 0\} + f(d_i) + \gamma_{j(i)} + \epsilon_i \tag{1}$$

where y_i is a characteristic of pixel i and d_i is the perpendicular distance from each pixel i to the nearest SFHA boundary, with positive values indicating that the pixel is located inside the SFHA. Our coefficient of interest is β, the magnitude of the discontinuity at the boundary. We allow characteristics y_i to vary flexibly on either side of the boundary.

In our baseline specification, we specify $f(d_i)$ as a fourth-order polynomial that is allowed to differ on either side of the boundary. Results are similar under alternative local linear specifications. Finally, we include census tract fixed effects $\gamma_{j(i)}$ as a substitute for boundary fixed effects. We use census tract fixed effects instead of the more common boundary fixed effects as our boundaries do not have natural segments.

We estimate equation 1 on land within 2,000 feet (0.38 miles) of an SFHA boundary. Following previous work, we exclude boundaries that trace a body of water (Dell, 2010). Table 1 presents summary statistics for our boundary estimation sample: land close to a boundary is more developed and has slightly higher home prices than areas further from the boundary. Appendix Figure A.2 plots a histogram of the number of pixels in our estimation sample across distance-to-boundary bins. We cluster our standard errors at the census tract level to allow for spatial correlation in the error term.

4.2 Results

We present our main results in Figures 2, 3, and 4 by plotting binned outcomes as a function of the distance to the SFHA boundary, residualized of census tract fixed effects. We report coefficient estimates of $\hat{\beta}$ in Table 2. Column (1) presents the variable average outside the SFHA. Our baseline coefficient estimates are in column (2); columns (3) through (5) present results from alternative specifications.

Persistence and exogeneity of boundaries Figure 2 first documents that the historic boundaries persist: the boundary of the SFHA from flood maps drawn in the late 1970s and early 1980s predicts the location of the boundaries in flood maps in 1996 and 2017. The relationship between historic and current maps is not exact and is diminishing over time as maps evolve. Between the time of the first maps and 1996, the first stage fell from 100 to 43 percentage points (Figure 2a) and it further eroded to 29 percentage points as of 2017

(Figure 2b). Therefore, we discuss our results in the context of an *intent-to-treat* framework: the treatment of interest is the initial designation, which may evolve over time. Alternatively, we could estimate the treatment effect of regulating land as an SFHA by instrumenting for SFHA status with the historic boundaries.[15]

Next, we turn to our historic land use data to probe our identifying assumptions. These tests amount to checking for smoothness in land use before SFHA designation.[16] If preexisting amenities differed discontinuously across the boundary, or if boundaries were drawn around the contours of existing development, we would observe discontinuous patterns in development around the SFHA boundary. Figure 3a shows that pre-period development is smooth across the boundary, supporting our hypothesis that the institutional details of the mapping process in the 1970s and 1980s provide a compelling setting to conduct a boundary discontinuity analysis. The magnitude of the discontinuity in pre-period development, reported in Table 2, is economically small and statistically insignificant in our baseline specification.[17]

Appendix Figure A.3 and Table A.2 demonstrate smoothness in other pre-period land use outcomes.

Development falls in the SFHA By 2016, we observe a clear discontinuity in development. Figure 3b shows that land just inside the SFHA is 4.2 percentage points less likely to be developed than land just outside the SFHA.[18] This represents an 18% reduction as a

[15]For outcomes measured as a stock, like development, this would require integrating over all first stages between the initial map and today. In practice, due to limited data on historical maps, one would interpolate over the magnitude of the first stage in 1996 and 2017.

[16]Our land use data was collected via aerial photographs between 1971 and 1982, while the flood maps were drawn between 1977 and 1984. Most aerial photographs were taken during or before 1976, before any of the maps were drawn. While it is possible that some aerial photographs were taken after the maps had been drawn, we will interpret these land use outcomes as a pre-period. We do not think this is particularly problematic, as land use evolves slowly and the worst-case scenario is that the photographs were taken five years after the drawing of the map.

[17]In our local-linear specification, the coefficient continues to be economically small but does become statistically significant.

[18]Figure 3b documents a striking pattern of land use on either side of the SFHA boundary. Moving away from the boundary further into the SFHA, development decreases sharply for about 400 feet before leveling out. Because this pattern is asymmetric, we do not believe it is driven by measurement error. Instead,

share of total new development occurring between 1980 and 2016 just outside the SFHA. It is comparable to the difference in development share between the 50th and 60th percentiles of the tract-level distribution of share developed. Table 2 Panel C shows that these results are robust to alternative definitions of development, including the share of land covered by a building footprint. We interpret this finding as indicating that SFHA regulations have a substantial impact on the location of real estate development. While the direction of the development effect is theoretically unambiguous — SFHA designation is undesirable for households and costly to developers — the large magnitude of the policy's negative effect on development is notable.

Table 2 illustrates that the effect is driven by single family homes, which make up the majority of residences (86%) in our sample. Marginal land developed just outside of the SFHA is converted from wetlands, consistent with the fact that high-flood-risk areas tend to have substantial wetlands.

Prices increase in the SFHA In Figure 4, we examine how the sale prices of houses vary across the SFHA boundary. While the theoretical effect of the policy on quantities was *ex-ante* unambiguous, whether prices fall or rise depends on whether the effect of the policy on demand or supply dominates. Figure 4a illustrates that prices are approximately 6% higher inside the SFHA, indicating that the construction costs imposed in the floodplain dominate any negative demand effect driven by mandatory flood insurance, higher flood insurance prices, or any salience or risk perception effects of living in an SFHA. These impacts are again driven by single family residential homes (Figure 4b). This finding contrasts with recent work that has found floodplain designation *decreases* house prices by 1-2% (Hino and Burke, 2021). This work, however, exploits map updates to achieve identification, and therefore primarily captures short-term demand effects. Supply responses, which would

it may be driven by a higher prevalence of wetlands deep inside the SFHA, which is also apparent in the pre-period land use (see Appendix Figure A.3).

reduce the quantity of houses built in the floodplain, could counteract the demand-driven price decrease in the long run.

We see no evidence that observable differences in structures across the boundary explain the positive price effect: in Appendix Table A.3, we report estimates on sales prices residualized of a rich set of observable characteristics of each house. Though these results are noisier, the positive effects persist. We also see similar effects on prices for homes built before and after the introduction of the SFHA regulations. This suggests that the positive price effect is driven by restrictions in supply and not a greater willingness-to-pay for SFHA-regulated structures.[19]

In contrast to Figures 4a and 4b, Figure 4c shows that vacant land prices decrease by 12% across the SFHA boundary, consistent with the interpretation that the SFHA imposes both a negative demand shock and a negative supply shock.

The positive price effects on built structures inside the SFHA indicate that the regulatory costs imposed by the SFHA are significant, though these costs have been largely overlooked by research and policy discussion on the NFIP. We explore the magnitude of these costs and their role in mediating the effect of floodplain regulation on housing market outcomes in more detail with our equilibrium model in Sections 6, 7, and 8.

Robustness Columns (2), (3), and (4) of Table 2 illustrate the robustness of our results to alternative specifications to ensure that our results are not driven by specific choices in our estimation framework. Appendix Figure A.4 shows corresponding figures. We observe similar effect sizes on both quantities developed and prices if we estimate a linear function of the running variable estimated on a narrower bandwidth (500 feet) or if we estimate a local linear regression instead of a fourth order polynomial. Our results are also robust to excluding areas close to the coast (column 4), providing additional evidence that unobserved

[19] We investigate this finding in more detail in Section 5.

amenities that could change discretely at the boundary (like coastal views) are not biasing our results.

5 Consumer Willingness to Pay for Lower Risk

The preceding section shows that floodplain regulations shift development out of regulated, higher-risk areas. The normative implications of this relocation depend on the extent to which it corrects frictions in the consumer location decision. If consumers fully internalize flood risk, floodplain regulations would unambiguously reduce welfare by distorting their efficient location decisions. If, however, consumers fail to internalize some portion of flood risk, then floodplain regulations could recover the efficient spatial allocation of development. Therefore, in the following section we study whether consumers are willing to pay for the full value of averted flood damages when buying lower-risk houses.

5.1 Empirical Strategy

Because differently-risky locations also exhibit different amenities, to avoid omitted variables bias we study whether consumers are willing to pay for safety on the adaptation margin. We estimate the effect of adaptation on flood damages (measured using insurance payouts) and insurance premiums. Then, we calculate the effect of adaptation on house prices. This price effect will indicate the value consumers place on adaptation. Under the assumption that adaptation only affects consumer utility through its effect on flood risk, the price effect captures consumers' attitudes towards this risk. This assumption would be violated if homeowners disliked the aesthetics or mechanics of elevated houses. However, we view this as unlikely since elevated houses typically look similar to non-elevated houses (see Appendix Figure A.1) and the additional stairs do not usually exceed what would normally be found inside any multi-story house. We therefore interpret the adaptation effect on prices as the consumer's willingness-to-pay for reducing flood risk.

To evaluate the extent to which consumers internalize risk, we compare the adaptation price effect to the effect of adaptation on damages and premiums. If consumers fully internalized all flood risk, as the social planner would, adaptation would increase house prices by the entire amount it reduces damages. However, since premiums are more-subsidized for non-adapted houses, adaptation might only increase prices by the amount it decreases *premiums*. This scenario would indicate the potential for subsidized premiums to induce a form of moral hazard: a consumer's internally-optimal level of risk would be higher than the social planner's. Finally, prices might change by only a fraction of the adaptation effect on insurance premiums. Consumers might not value premium reductions if they did not plan to purchase insurance — for example, if they trusted that government aid would be sufficient to make them whole following a disaster. However, insurance take-up inside the floodplain is approximately 50% (Bradt et al., 2021), so this would only partially explain such a phenomenon. This pattern could also be explained by behavioral frictions such as risk misperception or myopia.

Following Wagner (2021), to estimate the effect of adaptation we exploit the fact that a community must adopt flood-safe building standards at the time it enrolls in the NFIP. Newly-built houses were required to be elevated to the level of the 100-year-flood just after a community enrolled in the NFIP — but not before. This suggests an event-study design in which we regress our outcomes of interest against the year a house was built relative to enrollment, within the SFHA:

$$y_i = \Sigma_r \beta_r 1\{r_i = r\} + \gamma_{j(i)} + \varepsilon_i \tag{2}$$

where y_i is an outcome of interest (elevation, insurance payouts, premiums, or sale price);

r_i indicates the construction year relative to house i's year of enrollment; and $\gamma_{j(i)}$ is a set of census tract fixed effects.[20] In practice, we estimate this model at the census-tract-by-flood-zone-by-relative-year-built level. We cluster standard errors at the census tract level. In order to increase power, we also estimate a binned specification: we restrict to houses built within 10 years of the date a community joined the NFIP and estimate the following model, again clustering standard errors at the census tract level:

$$y_i = \alpha + \beta Post_i + \nu r_i + \eta r_i Post + \gamma_{j(i)} + \varepsilon_i \tag{3}$$

where $Post_i$ indicates being in or after the year of NFIP enrollment ($r_i \geq 0$). Under the assumption that the year of construction was not manipulated, β indicates the causal effect of building code regulations on our outcomes of interest.[21]

We estimate these models on two datasets. The first, derived from NFIP claims and policy data, describes elevation status, insurance payouts, and policy cost in all Florida counties between 2010 and 2018. The second dataset describes house prices by census tract, flood zone, and relative year built. Appendix A.4.1 describes the construction of these datasets in more detail. Appendix Table A.4 presents summary statistics for the datasets used in this analysis.

5.2 Results

In Figure 5 we present the coefficient estimates on relative year from the event study specification (Equation 2). Variable means and regression coefficients from the binned specification

[20] Census tracts are smaller than communities.

[21] Our results indicate smooth trends in adaptation prior to the year of construction. We do not find jumps in the year just before NFIP enrollment, indicating that developer manipulation of construction year is not a concern.

are presented in Table 3. Table 3 also shows the results from a difference-in-difference specification, described in Appendix A.4.3 and graphed in Appendix Figure A.5. Results are similar in this alternative specification.

Figure 5a shows that for houses constructed just prior to NFIP enrollment, the average share elevated in the SFHA is about 60%. This value jumps to about 90% after NFIP enrollment. The large share of houses elevated prior to NFIP enrollment could be due to preexisting building codes that mandated elevation in some locations. It could also reflect low costs or high demand for elevation in some areas. As we discuss shortly, we find evidence of low demand for elevation. However, we cannot distinguish between preexisting building codes or heterogeneity in elevation cost. In the years following NFIP enrollment, we would expect 100% of houses inside the SFHA to be elevated. That we observe only 90% suggests either measurement error or imperfect developer compliance with building codes. Either seems plausible in this sample. Nevertheless, the sharp jump in elevation indicates that NFIP enrollment indeed caused approximately one-third of newly-built houses to be elevated.

Figure 5b shows that after NFIP enrollment, insurance payouts fall by about 55%, or \$1.60 per \$1000 of coverage. At the average coverage amount of \$252,000, this translates to a difference of \$400 less in damages per policy-year. With a discount rate of 5%, the NPV of these savings is 4.2% of the average house value. Partially reflecting this difference, premiums for adapted houses are cheaper by on average \$1.21 per \$1000 of coverage, or \$300 per year. The NPV of these savings is 3.2% of the average house value.[22]

Despite this higher value to both the government and the consumer, the prices of elevated houses are not significantly different from those of non-elevated houses. The point estimate of the increase in house price for elevated houses is -0.7%, or -\$1400. Appendix A.4.4 shows that

[22] Although in this dataset, the cost of an insurance policy exceeds payouts, in general policies are not more expensive than expected damages. Damages are noisy and average damages will depend on how many particularly large events occurred in the window that is considered. Wagner (2021), who estimates payouts by year using a dataset that includes Hurricane Katrina, finds damages in the SFHA for pre-enrollment houses to be around \$6 per \$1000 of coverage. She finds that insurance payouts drop by about \$2/\$1000 in the year of NFIP enrollment, and premiums by about \$1.5/\$1000.

in a stylized model of a housing market, a zero effect on price implies zero willingness-to-pay for reduced risk. Thus, not only do we find that consumers are unwilling to pay for the total value of a safer house, we find that they are unwilling to pay to reduce *any* of the flood costs they personally face.[23] As discussed previously, this indicates behavioral frictions such as risk misperception or myopia, and potentially also moral hazard from consumer expectations of government aid in case of disaster. Separating these two alternatives is challenging and outside the scope of this paper. However, in either case the effect would be the construction of inefficiently-risky houses.

We cannot reject that consumers are not willing to pay anything for elevated houses. Under the assumption that consumers treat safety improvements on the adaptation and relocation margins similarly, we can also interpret the above results as indicating that consumers have no willingness-to-pay for houses in safer locations. This assumption seems unlikely to be violated in the case of the moral hazard friction: if consumers believe the government will provide aid after flooding in high-risk areas, this reduces both the incentive to buy an elevated house and the incentive to buy in a safer area. This assumption could be violated if willingness-to-pay for adaptation were low because consumers are ill-informed about the difference in risk between elevated and non-elevated houses, while they are perfectly informed about the difference between high- and low-risk areas. Because elevation differences are more obvious to the naked eye than differences in risk across locations, however, this scenario is implausible. In general, behavioral frictions seem like they would be at least as strong for location-based risk as elevation-based risk. Therefore, we assume in the next section that consumers also have zero willingness-to-pay to reduce their risk on the location margin.

[23] Even if we considered the upper end of the 95% confidence interval – a price increase of 1.06% — it would translate to only 25% of the NPV of insurance payout savings and 33% of the NPV of insurance premium savings. A discount rate of 16% would be required to rationalize this price increase based on the reduction in insurance premiums.

6 An Equilibrium Model of the Housing Market

The results in Section 5 indicate that floodplain regulation could improve welfare by shifting consumers away from inefficiently-risky locations and requiring adaptations that they fail to demand. Indeed, Figure 5a shows that the regulation induces houses to elevate that otherwise would not. Moreover, the results in Section 4 indicate that the regulation shifts development out of risky locations. However, without information on the costs of these risk reductions, we cannot say whether the policy improves welfare overall. Furthermore, while the results in Section 5 enable us to quantify the benefit of elevating a house, the spatial discontinuity analysis does not do the same on the margin of relocation.

The reduced-form spatial RD results reflect equilibrium differences across regulated and unregulated regions, not the direct effect of the policy. Quantities and prices on either side of the boundary are the result of households choosing among alternative locations, and developers choosing whether or not to develop. Applying floodplain regulation in one area impacts demand and therefore prices and development in another. This violation of the stable unit treatment value assumption precludes estimating a well-defined treatment effect of the regulation from the magnitudes of the discontinuities alone. To interpret our spatial RD results, we must explicitly account for the household choices and housing supply curves that yield these equilibrium outcomes. Therefore, to recover the effect of the policy we need to explicitly model residential choices and development decisions.

In addition, the spatial equilibrium resolves prices and quantities across the entire market, not just the slice of land on which we estimated the spatial regression discontinuity. The policy's effect will depend on the joint distribution of amenities and flood risk across locations. If highly-risky areas are also pleasant places to live, shifting development to safer locations will incur a high amenity cost. And because high-risk areas make up a large share of the housing market — nearly one-third of all land in our sample is currently regulated as an SFHA — regulating them will have large market-wide effects.

Therefore, to facilitate interpretation of our reduced-form boundary analysis, evaluate the market-wide effects of floodplain regulation, and investigate the impact of counterfactual policies, we develop an equilibrium model of residential housing demand and supply. The model explicitly accounts for the fact that SFHA status may impact both residential choice and housing supply.

Our model's key parameters of interest describe how the SFHA designation shifts the demand and supply curves inwards. These parameters capture the "direct effect" of the SFHA on demand and supply. We estimate the key parameters in a narrow band around the SFHA boundary, within which we assume SFHA status is uncorrelated with unobservable land features and impose that average equilibrium differences match our spatial RD estimates. Using these estimated parameters, we then simulate the housing market under factual and counterfactual policies to quantify the "overall effect" of the SFHA. The market-wide simulations allow us to account for equilibrium price effects and estimate the magnitude of the policy's impact across each county in our study.

6.1 Residential choice

Because consumers appear indifferent to adaptation, we model them as choosing between locations, where houses are uniform within each location. Thus, each individual i makes a discrete choice of where to live within market m, which we take to be a county.[24] Locations are differentiated goods characterized by tract j and SFHA designation status z. Following the standard discrete choice framework of Berry et al. (1995), the indirect utility of individual i living in location jz is given by:

$$u_{ijz} = \alpha^D \ln(P_{jz} + \gamma_z D_{jz}) + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz}$$

[24] In Florida, counties are large but tend to only contain one major city and commuting zone.

where P_{jz} is the price of housing in location jz, $\gamma_z D_{jz}$ indicates the net present value of the expected cost to the consumer of flood risk in location jz, $\phi_1 C_z$ indicates non-financial costs of living in an area designated as a floodplain, ξ_{jz} are unobserved amenities, and ε_{ijz} is an i.i.d. preference shock. We express the consumer's expected cost of flood risk as a share γ_z of total expected damages D_{jz} to reflect the possibility that subsidized flood insurance or the expectation of future governmental assistance causes consumers to discount their true damages.[25] Non-financial costs of living in a floodplain $\phi_1 C_z$ can include the hassle cost of regulations, such as the requirement to document flood insurance purchase when receiving a mortgage, or anxiety around future flood risks that is introduced by the high-risk label.

The evidence presented in Section 5 suggests that consumers purchasing a house may face information frictions, for example underestimating a house's flood risk. At the same time, floodplain designation may (at least partially) debias homebuyers. Following Allcott and Taubinsky (2015), we model this bias correction as affecting decision utility without affecting experienced utility. Therefore, we model consumers as purchasing houses following a decision utility of

$$u_{ijz} = \alpha^D \ln(P_{jz} + \rho\gamma_z D_{jz}) + \phi_1 C_z + \phi_2 B_z + \xi_{jz} + \varepsilon_{ijz}$$

where we have allowed consumers to misperceive actual damages by a factor of ρ and introduced $\phi_2 B_z$ as a potential channel for floodplain designation to affect beliefs about flood risk. Modeling the SFHA as an on-or-off label rather than a modulator of expected damages captures the fact that flood maps do not give any indication of the expected flood damages of a particular location; they simply indicate a coarse level of risk.

We cannot separately identify the effects on demand of perceived damages D_{jz}, non-financial

[25]Theoretically, the cost to consumer of flood risk could include both insurance premiums and exposure to uninsured risk. In practice, the average claim amount of \$60,000 is well below the coverage cap of \$250,000 (Wagner, 2021) so we conceptualize flood risk to the consumer entirely in terms of flood insurance costs.

costs C_z, and debiasing labels B_z, since all three vary with floodplain status. The evidence presented in Section 5 indicates that consumers are insensitive to risk on the location margin, so we assume $\rho\gamma_z = 0$ and omit the damages term from our model of demand. We then combine the non-financial cost term C_z and debiasing effect term B_z into an overall "floodplain effect" term $\phi SFHA_z$. To the extent that we are incorrect in assuming that consumers are insensitive to flood damages, their reactions to larger expected damages in the floodplain will load on to this combined term.[26]

The model of consumer decision utility we take to the data is therefore

$$u_{ijz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz} + \varepsilon_{ijz} \tag{4}$$

where p_{jz} is the log price of housing in location jz,[27] $SFHA_z$ indicates whether z is regulated as a floodplain, ξ_{jz} are unobserved amenities, and ε_{ijz} is an i.i.d. preference shock. We allow for the possibility that amenities ξ_{jz} are correlated with both price p_{jz} and floodplain status $SFHA_z$. The correlation between ξ_{jz} and p_{jz} arises in equilibrium as higher-amenity locations command a higher price. The correlation between ξ_{jz} and $SFHA_z$ occurs because SFHA status may be correlated with other amenities, like coastal access, or disamenities, like abundant insects. We assume ε_{ijz} is distributed according to an Type 1 Extreme Value Distribution.

Individuals choose the location jz that maximizes their idiosyncratic decision utility within the locations in market m. Because ε_{ijz} is distributed EVT1, the fraction of individuals in a county choosing to live in location jz is:

$$s_{jz} = \frac{\exp(\delta_{jz})}{\Sigma_{j' \in J_m, z' \in \{0,1\}} \exp(\delta_{j'z'})} \tag{5}$$

[26]We will discuss the implications of this aggregation for our descriptive analysis in Section 7 and for welfare in Section 9.

[27]Note that the price p_{jz} is for the bundle of housing that a consumer purchases, which includes both the structure and the land on which the structure is built.

where $\delta_{jz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz}$ indicates the mean (non-idiosyncratic) utility for each tract-zone pair.

6.2 Housing supply

To flexibly capture heterogeneity in supply elasticities across locations and within locations across time, we model development in each location jz as the result of decisions of granular, heterogeneous landowners. Each tract-zone pair is composed of L_{jz} plots, each of which could either be developed into a house or used for some outside option (e.g. agriculture). The value of the outside option for plot g is denoted c_g (for opportunity cost) and distributed Normally with a mean and standard deviation that varies by tract: $c_g \sim N(\mu_j, \sigma_j^2)$. Developers make static decisions about whether to develop at two points in time: before the regulations are imposed ($t = 0$) and after they are imposed ($t = 1$).

The value of developing a house in period t depends on the (log) price p_{jz}^t for which it could sell, which varies by location and time, and the cost to build the house η_{jz}^t, which also varies by location and time and increases by a constant amount ψ when the house is elevated. Whether a house is elevated is determined exogenously and varies by tract and time period.

The development decision for an undeveloped plot g in time period t is given by

$$D_g^t = \begin{cases} 0 & c_g > p_{jz}^t - \psi E_{jz}^t - \eta_{jz}^t \\ 1 & c_g < p_{jz}^t - \psi E_{jz}^t - \eta_{jz}^t \end{cases}$$

where $D_g^t = 1$ indicates that a plot of land is developed and E_{jz}^t indicates whether or not houses built in location jz are elevated in period t. Thus, the share of land that is developed in the pre-period is given by $\Phi\left(\frac{p_{jz}^0 - \psi E_{jz}^0 - \mu_j - \eta_{jz}^0}{\sigma_j}\right)$ where Φ denotes the CDF of the $N(0,1)$ distribution. The total share developed at the end of time $t = 1$ is

$$\Phi\left(\frac{p^1_{jz} - \psi E^1_{jz} - \mu_j - \eta^1_{jz}}{\sigma_j}\right)$$

6.3 Equilibrium

In equilibrium, house prices and location decisions adjust so that the quantity of housing supplied in each location equals the number of individuals choosing to live there. Specifically: $L_{jz}\Phi\left(\frac{p^1_{jz}-\psi E^1_{jz}-\mu_j-\eta^1_{jz}}{\sigma_j}\right) = Q_{jz} = N_m s_{jz}$ where L_{jz} is the total amount of land in location jz, Q_{jz} is the quantity of developed land in jz, and N_m is the number of households in the market. Expanded, this equilibrium condition reads:

$$L_{jz}\Phi\left(\frac{p^1_{jz} - \psi E^1_{jz} - \mu_j - \eta^1_{jz}}{\sigma_j}\right) = N_m \frac{\exp(\alpha^D p_{jz} + \phi SFHA_z + \xi_{jz})}{\Sigma_{j'\in J_m, z'\in\{0,1\}} \exp(\alpha^D p_{j'z'} + \phi SFHA_{z'} + \xi_{j'z'})} \tag{6}$$

7 Estimation and Results

The model has two key parameters — the direct effects of floodplain designation on demand (ϕ) and supply (ψ) — in addition to demand elasticity α^D and tract-level supply parameters μ_j and σ_j.[28] In similar settings, authors estimating within-city location substitution elasticities have found values corresponding to our α^D in the range of -0.4 to -3.3 (Calder-Wang, 2021; Song, 2021).[29] The model in Song (2021) is of the most similar granularity to ours and estimates a value for α^D ranging from -0.9 to -0.99, so we calibrate α^D to be -1.[30] We calibrate the tract-specific supply parameters μ_j and σ_j to estimates of tract-level housing

[28] Given the way that we specified preferences (over log prices), own-price elasticities for each location are equal to $\alpha^D(1 - s_j)$. Because locations are small and $1 - s_j \approx 1$, we interpret α^D as a demand elasticity.

[29] To impute elasticities from values presented in Calder-Wang (2021), we use an average NYC apartment rental price of \$1615 and neighborhood-specific average rental prices from 2019 from furmancenter.org.

[30] In Appendix Table A.6 we present a set of results in which we estimate α^D (without instrumenting), which yields a value of approximately -0.8.

supply elasticities from Baum-Snow and Han (2019). We then estimate SFHA costs ψ and ϕ in our setting, using a variation of our spatial discontinuity assumption to account for the endogeneity between SFHA status and unobserved amenities (ξ_{jz}) and construction costs (ζ_{jz}). We describe the estimation procedure in more detail below.

7.1 Demand-side identification and moments

We use the standard Berry inversion to obtain δ_{jz} from observed market shares:

$$ln(s_{jz}) - ln(s_{0m}) = \delta_{jz} \tag{7}$$

where s_{0m} indicates the market share of the arbitrary geography within each market that we have normalized to be utility 0. We construct the empirical market shares using

$$\hat{s}_{jz} = \frac{Q_{jz}^{2016}}{\sum_{j' \in J_m, z' \in \{0,1\}} Q_{j'z'}^{2016}} \tag{8}$$

where Q_{jz}^{2016} is the total amount of developed land in geography jz. We then calculate unobserved amenities as:

$$\xi_{jz} = \delta_{jz} - \alpha^D p_{jz}^{2016} - \phi SFHA_z \tag{9}$$

Recall that in Section 4, the SFHA effect was causally identified under the assumption that unobserved amenities are identical in the limit on the left and right sides of the floodplain boundary. However, in our structural model, after aggregating to the tract-zone level, amenities ξ_{jz} may be correlated with floodplain status $SFHA_z$. Therefore, we identify the floodplain effect on demand ϕ by separating the amenity term into components that are and are not correlated with floodplain status. We then simulate the average price difference across zones after subtracting the correlated component. We require this price difference to match the price difference estimated in the spatial RD, denoted $\beta^{p,2016}$. We lay out our moment conditions more fully in Appendix A.6.

To operationalize the assumption that these moment conditions hold in a narrow band around the boundary, in practice we subdivide the tract-zone pairs into tract-zone-band observations, where band $b \in \{close, far\}$ indicates whether an observation is within 100 feet from a floodplain boundary. We then impose our moment conditions only on observations in the "close" band.

7.1.1 Discussion and Implications

This estimation strategy identifies the floodplain effect on demand ϕ close to the floodplain boundary, but assumes it is identical across all floodplain designations. The floodplain boundary tends to have lower risk than areas deeper inside the floodplain. Thus, to the extent that the true floodplain effect on demand is larger in higher-risk areas (e.g. coastal areas subject to storm surge) ours will be an underestimate.

We earlier imposed the assumption that consumers do not respond to expected flood costs. As mentioned previously, if this assumption is incorrect any increases in flood expenses to which consumers *do* react will load onto the SFHA demand parameter ϕ. Since policies cost more inside the SFHA, the SFHA demand parameter may include some (perhaps attenuated) consumer reaction to these larger costs. As above, insurance costs are larger for higher-risk areas deeper inside the floodplain so we may again underestimate the true floodplain effect.

7.2 Supply-side calibration and moments

We begin by calibrating μ_j and σ_j using estimates of tract-level housing supply elasticities from Baum-Snow and Han (2019). Appendix A.5 discusses this calibration in detail.

We then estimate the effect of the SFHA on the supply function. We assume that the SFHA only affects supply by requiring houses to be elevated. Therefore, we identify the elevation cost term ψ using the floodplain boundaries. This identification is again complicated by the possibility that unobserved construction costs η_{jz}^t could be correlated with floodplain status. As on the demand side, we separate the unobserved construction costs into components that are and are not correlated with floodplain status. This time, we match the spatial RD estimates of the SFHA effect on share developed. That is, we simulate the average quantity difference across zones after subtracting the correlated component and require it to match the quantity difference estimated in the spatial RD, in both the pre-period ($\beta^{q,1980}$), and the post-period ($\beta^{q,2016}$). Again, details on the exact moment conditions we use can be found in Appendix A.6.

As before, to operationalize the assumption that these moment conditions hold in a narrow band around the boundary, in practice we impose the moment conditions only on observations in the "close" band (within 100 feet from the boundary).

7.3 Estimation

After calibrating α^D, μ_j, and σ_j, we estimate ϕ and ψ jointly via the Generalized Method of Moments. We measure today's sales price p_{jz}^{2016} with the log of the median sales price for single-family homes from 2014-2019 based on the location of the building footprint. We measure sale price in the pre-period p_{jz}^{1980} as the log of the median value of owner-occupied non-condominium housing units from the 1980 Census. These data are not available at the SFHA level. Because floodplains did not exist in the pre-period we assume that the price does not differ between floodplains within a Census tract. We measure quantity of developed land in 1980 and 2016 as the number of gridcells that are categorized as developed in the 1980 and 2016 land-use datasets.[31] We measure elevation from NFIP policy data, as discussed

[31] To account for the fact that some locations jz have no developed land in 1980, we calculate share developed $q_{jz} = (Q_{jz} + 1)/L_{jz}$ in those locations. Also, in the spirit of Burchfield et al. (2006) we correct

in Appendix A.4.1. We define a tract as elevated if more than 50% of insured houses in that tract are elevated. Where we do not observe elevation (including all non-SFHA tracts), we assume it only occurred when required by regulation. When historic SFHA status is unavailable because of the limited reach of our digitized maps, we use the 1996 SFHA status to calculate mean utility δ_{jz}, but we restrict to historic boundaries in our estimation of the main parameters.

Appendix Table A.5 presents summary statistics for the model estimation sample. A larger share of our estimation sample is developed than the sample used in Section 4,but house prices and flood risk look similar in the two samples.

7.4 Results

Model fit Figure 6 evaluates the fit of the supply model. Following Anagol et al. (2021), we plot observed price in 1980 and 2016 against the prices that rationalize observed market shares in each year using our estimated supply parameters. This exercise does not use our estimated demand model at all; it assesses model fit based only on our estimated supply parameters and our method of simulating an equilibrium. We see a strong correlation between model-generated and observed prices, indicating our supply curve is reasonable.

Parameter estimates Table 4 column (1) presents the parameter estimates for our baseline specification.[32] As expected, SFHA status is disliked by consumers (negative ϕ) and imposes costs on developers (positive ψ). On the demand side, the magnitude of the SFHA cost implies that consumers are willing to pay 23% more to avoid living in a floodplain. As discussed above, under the assumption that consumers are insensitive to flood insurance

for potentially mismeasured growth by measuring the number of developed cells in 1980 as the minimum of the observed number of developed cells in the location in 1980 and the number measured in 2016.

[32] Appendix Table A.6 presents parameter estimates under alternative calibrations of demand elasticity α^D.

payments, this term captures both non-financial costs and debiasing effects of floodplain designation.

The result that consumers are willing to pay 23% more to avoid living in a floodplain contrasts sharply with recent work that has argued that consumers are mostly unresponsive to floodplain designation (Hino and Burke, 2021). This difference could be attributed to our setting: residents in flood-prone Florida may be particularly sensitive to signals of risk. Also, our work uses modern sales prices (2014-2019) rather than the older years (1998-2013) studied elsewhere, and a growing awareness of climate change could have strengthened the effect of floodplain designation. Nevertheless, a 23% premium on avoiding the floodplain far exceeds the actual financial benefits of relocation: Appendix Table A.5 indicates that in our sample, being in a floodplain increases expected damages by about 0.64% of house price per year, which translates to an NPV of 12.6% of house price — just over half of the estimated demand effect. This large discount may arise from consumers' strong dislike of the bureaucratic burdens of complying with floodplain regulations. Alternatively, the floodplain designation may cause consumers to over-update their beliefs about risk.

Turning to supply, we estimate that regulations produce a shift in the supply curve that corresponds to a 25% increase in construction costs in the SFHA.[33] The magnitude of this effect is large, but within the plausible (albeit wide) range of estimates of building codes and zoning regulations on construction costs: from 5% (Listokin and Hattis, 2005) to 24% (Emrath, 2021) to 42% (Song, 2021). The wide variation reflects both differences in strategies to estimate regulatory costs and variation in the types of regulations imposed. A 25% increase in costs appears plausible based on our informal interviews with Certified Floodplain Managers and Florida contractors. These practitioners argued that SFHA building codes were a highly relevant factor in influencing the location of development. This perspective aligns with our results. However, as described in Section 5, elevation reduces damages by about 5% of house price — far less than the estimated cost of elevating.

[33]Construction costs include both the land on which a building is built and labor and materials costs.

Taking our results together, we conclude that the SFHA regulations shift both demand and supply curves inwards by more than 20%. In the following sections, we use these estimates to study the effect of the policy on descriptive outcomes and social welfare.

8 Counterfactuals

We use our model to investigate the impact of floodplain regulation by simulating counterfactual outcomes in the absence of the policy. We compute counterfactual outcomes by taking the distribution of development and prices and quantities in 1980 as given and searching for a price vector $\tilde{p}_{jz}^{2016}$ that equates:

$$Q_{jz}^{1980} + L_{jz}\Phi\left(\frac{p_{jz}^{2016} - \hat{\psi}E_{jz}^{2016} - \mu_j - \eta_{jz}^{2016}}{\sigma_j}\right) = \tag{10}$$

$$N_m \frac{\exp(\alpha^D \tilde{p}_{jz}^{2016} + \hat{\phi} SFHA_z + \xi_{jz})}{\Sigma_{j' \in J_m, z' \in \{0,1\}} \exp(\alpha^D \tilde{p}_{j'z'}^{2016} + \hat{\phi} SFHA_{z'} + \xi_{j'z'})}$$

given our calibrated and estimated parameters $(\alpha^D, \mu_j, \sigma_j, \hat{\phi}, \hat{\psi})$ and our recovered values of ξ_{jz} and η_{jz}. This approach generates counterfactual prices and quantities of development in each of the locations jz. Our counterfactuals assume a closed city: we assume the 2016 population in each of our ten counties remain constant across counterfactual scenarios.[34]For each counterfactual, we report expected damages and home prices based on the location of development. In this section, we discuss these outcomes in a positive sense. Section 9 will then discuss normative implications.

To assess how floodplain regulation impacts development in high risk areas, we use external estimates of flood risk generated by a state-of-the-art hydrological model produced by the

[34]We estimate the parameters on 11 counties but encountered difficulties estimating the equilibrium in the no-SFHA counterfactual for Miami-Dade county. Therefore, we omit it in these counterfactuals.

First Street Foundation.[35] We use this external measure of flood risk rather than the FEMA flood maps for a few reasons. First, the FEMA flood maps have received extensive criticism for being out-of-date and backwards-looking (Keller et al., 2017; Brannon and Blask, 2017; Frank, 2020b; Wing et al., 2022) and for failing to include certain important components of flood risk, e.g. pluvial (rainfall) risk. The First Street model incorporates climate change predictions as well as all major flood drivers in a novel peer-reviewed approach. Second, the First Street flood risk estimates provide granular estimates of the average annual loss for each parcel, which allows us to estimate damages in a way that FEMA maps do not.

We focus on three main outcomes: number of houses in the regulated area (observed SFHAs); expected damages, both overall and attributable to relocation or adaptation; and house prices inside and outside the regulated area. The number of houses in the regulated area directly follows from share of land developed.[36] Appendix A.7 describes how we use the First Street data to calculate expected damages. House prices are also directly simulated in our model, and we take the average across areas, weighting by developed area.

We estimate the effect of floodplain regulation by setting $SFHA_z = 0$ to simulate outcomes in the absence of any SFHA policy. On the supply side, this amounts to eliminating all SFHA-imposed building regulations: there is no requirement to build above the BFE and no additional permitting requirements. On the demand side, we think of eliminating the SFHA as a policy regime in which consumers receive no signals indicating higher risk and have no mandate to purchase insurance. Under this policy regime, there are no differences in insurance prices across areas.

Table 5 presents the results. Relative to our no-SFHA counterfactual, floodplain regulation reduces the number of homes built in regulated areas with a substantial risk of flooding by

[35]First Street's estimates are increasingly used in economics research as an independent assessment of flood risk (Bradt et al., 2021; Mulder, 2021; Sastry, 2021).

[36]We use the approximation that one house equals 900 square meters of developed land. Each grid cell in our simulation is 900 square meters. In recent years, the size of a new lot in Florida has been about one-quarter acre, or 1,000 square meters (Morrell, 2018).

almost 16%, reallocating over 117,000 houses. This represents approximately 2.6% of all new development in our sample. Notably, this figure exceeds the total number of houses removed from risky areas by the NFIP's home buyouts program over its lifetime (Frank, 2020a).

This relocation effect reduces the net present value of damages by $949 per newly-developed house. This amounts to approximately 10% of expected damages. The requirement to adapt houses that remain in the floodplain further reduces net present value of damages by another 30%. Overall, the policy reduces net present damages by $5,877 per newly-developed house. This amounts to an overall reduction in net-present-value damages by $26 billion.

The policy also increases house prices market-wide by 1.7%, or more than $3000 per house. However, this overall effect masks heterogeneity across regulated zones: inside the SFHA, prices fall by 3.8% under the regulation, while outside the SFHA, prices rise by 4.3%. This divergence is driven by the policy shifting demand from the regulated to the unregulated areas. It illustrates how equilibrium effects can offset the direct effects of the regulation and underscores the importance of taking an equilibrium approach in interpreting our reduced-form spatial discontinuity results.

The above findings suggest that floodplain regulation generates societal benefits, in the form of substantially-reduced damages, but also generates costs to consumers when higher construction costs are passed on to them. Consumers can mitigate these costs by substituting away from regulated areas, but this incurs additional costs in the form of lower-amenity locations. We attempt to quantify these costs, and balance them against benefits, in the following section.

9 Welfare Implications

Our preceding results formed a descriptive analysis of the impact of floodplain regulation on house location, house prices, and expected damages. We now describe a welfare framework in which to evaluate the normative implications of the regulation. As discussed in Section 6, the experienced utility of a consumer choosing housing in location jz is given by:

$$u_{ijz} = \alpha^D \ln(P_{jz} + \gamma_z D_{jz}) + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz}$$

where the factor γ_z accounts for insurance subsidies. While the consumer does not experience the remaining $(1 - \gamma_z)D_{jz}$ as a cost, the social planner incurs these costs in the form of insurance payouts that exceed premiums on average. Therefore, the social welfare function is given by

$$\Sigma_{jz} N_{jz} \left(\alpha^D \ln(P_{jz} + D_{jz}) + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz} \right). \tag{11}$$

As discussed in Section 6, we are unable to separate non-financial costs ϕ_1 from the estimate of the overall floodplain effect on demand ϕ. The makeup of ϕ is welfare-relevant, however: the component of ϕ that is made up of non-financial costs ϕ_1 has real costs to the social planner. However, the component of ϕ which is a debiasing effect ϕ_2 does not affect experienced utility and so does not impact social welfare. We therefore consider the spectrum of possible welfare effects by calculating welfare under two polar cases: (a) $\phi_1 = \phi$ (ϕ is 100% non-financial costs,) or (b) $\phi_1 = 0$ (ϕ contains only terms that do not affect welfare).

Since we do not observe ε_{ijz}, we cannot directly calculate Equation 11. Instead, we compute per-person consumer surplus in each market using the estimated demand function, multiply by the amount of new housing, and subtract total damages. Under the assumption that consumers do not factor damages into their demand, this is equivalent to computing welfare using Equation 11. As previously discussed, if this assumption is incorrect, some portion of the ϕ term will reflect this consumer valuation of damages. We would then double-count damages and overstate the cost. We account for this by considering the range of welfare outcomes between $\phi_1 = \phi$ and $\phi_1 = 0$.

Following Train (2009), we compute per-person consumer surplus in each market as $CS_i = \frac{-\bar{P}}{\alpha^D} \ln \Sigma_{j,z} \exp(\delta_{jz})$, where $\bar{P}$ is the average house price in levels. In the case where $\phi_1 = \phi$ we take $\delta_{jz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz}$; when $\phi_1 = 0$ we take $\delta_{jz} = \alpha^D p_{jz} + \xi_{jz}$.

We report results of these welfare calculations in Table 6. We report differences in consumer surplus under the observed regulation relative to the unregulated benchmark. We see that if we assume the floodplain effect on demand ϕ entirely reflects non-financial costs of regulation, the policy reduces consumer surplus by \$61 billion. If, alternatively, the floodplain effect ϕ entirely captures a debiasing channel, consumer surplus is only reduced by \$29 billion. Even at the low end, these costs exceed the reduction in NPV damages, which is just over \$26 billion.

A small share of the consumer surplus loss is attributable to the increased cost of providing adapted houses. Our parameter estimates indicate that the cost to elevate a house is equivalent to about 25% of the average house price, or \$43,800 per house. Under the observed policy, 640,000 new houses are built in the SFHA. Assuming based on Figure 5a that the regulation causes 30% of these to elevate, this implies an elevation cost of \$2.8 billion – less than 10% of the overall consumer surplus loss. An additional decrease in consumer surplus is due to the average \$3,000 increase in the cost of housing, which when multiplied over the population of newly-developed homes yields a total increase in cost of \$13.8 billion. Finally, consumer surplus also decreases because consumers substitute to lower-amenity locations, and — depending on the assumption we make — also incur non-financial costs of regulation.

The above calculations necessarily embed a number of additional assumptions, made for the purposes of obtaining a bottom-line welfare estimate, but which should be considered carefully. We focused on fiscal costs of flooding and ignored any emotional costs, for example the hassle cost of coordinating contractors to perform repairs and the pain of losing objects of sentimental value. According to NFIP data, each year a claim is filed by about 1.5% of pre-enrollment houses inside the SFHA, 1% of post-enrollment houses inside the SFHA, and

0.75% of houses outside the SFHA. A back-of-the-envelope calculation indicates this policy eliminates 1256 flood events per year in our sample. Emotional costs of flooding would thus have to exceed $111,000 per claim to change the sign of the welfare effect.

Additionally, when calculating benefits of the policy we have focused solely on damages that are induced or averted through the housing channel. In reality, when consumers locate in a risky area, they also induce public goods to be located there — for example schools, fire departments, electric lines, and roads. Flooding requires repairs to all these public goods, but the cost of these repairs is typically borne by the federal government in disaster aid. These costs are not incorporated into consumers' property taxes or insurance premiums, forming another channel of moral hazard. The Congressional Budget Office estimates that for every $34 billion in residential damages caused by hurricanes, the public sector incurs $12 billion of damages (U.S. Congressional Budget Office, 2019). If we, accordingly, inflated our estimated damages averted on the relocation margin by 35%, our estimate of the total welfare effect of the policy would be between $-33 billion and $-1.5 billion. Thus, our conclusions about the welfare impacts of the policy remain unchanged.

10 Conclusion

For over 40 years, federal floodplain regulations have influenced housing markets, with little evidence on either their costs or potential benefits. This paper combines a spatial regression discontinuity analysis of floodplain boundaries, an event study of the introduction of flood-safe building codes, and a model of the housing market to investigate the policy's equilibrium effects on development location, housing prices, expected damages, and welfare. We find that local to the SFHA boundary, floodplain regulation decreases development and increases prices, highlighting the importance of a previously unstudied element of the NFIP: construction costs imposed by building regulations. We also find that although flood-safe building codes reduce damages by at least 3.2% of housing value, consumers are not willing to pay for any of the benefit when purchasing a house. Using our model to interpret our reduced-form results, we find that floodplain regulation in Florida reduces new development in floodplains by 16%, or approximately 118,000 houses in the ten counties we study. This impact is large, exceeding the effect of a country-wide home-buyout program and comprising almost one-fifth of the policy's total risk reductions. The net present value of damages averted by the program exceeds $26 billion. However, the regulation achieves these reductions in risk at a considerable cost, raising prices by 1.7% market-wide and relocating consumers to less-preferred areas. Consumer surplus falls by between $29 billion and $61 billion, yielding a net negative effect on welfare of $2 billion to $34 billion.

Our findings have important implications for policies to promote the resilience of cities in the face of sea level rise and other climate-change-induced increases in flooding. First, our results indicate that relocations generated more welfare loss than benefits. This suggests that policy efforts that prioritize adaptation-in-place can be better for welfare than strategies of managed retreat, despite the apparent futility of living in high-risk areas. This paper also highlights the challenges to simultaneously increasing resiliency to natural hazards and maintaining housing affordability: a major driver of the welfare loss from this policy was due to increases in house prices. Finally, our work emphasizes the importance of considering equilibrium effects when regulating land use.

We briefly identify a few caveats. First, we model flood risk as static. In reality, flood risk is increasing over time. Because of the persistence of built structures, regulations that treat flood risk as static may under-incentivize relocation relative to adaptation. Second, although we provide suggestive evidence that consumers are not willing to pay to reduce their flood risk, we are unable to determine the underlying reason for this behavior. We leave the challenge of carefully identifying the causes and extent of overdevelopment in at-risk areas to future work.

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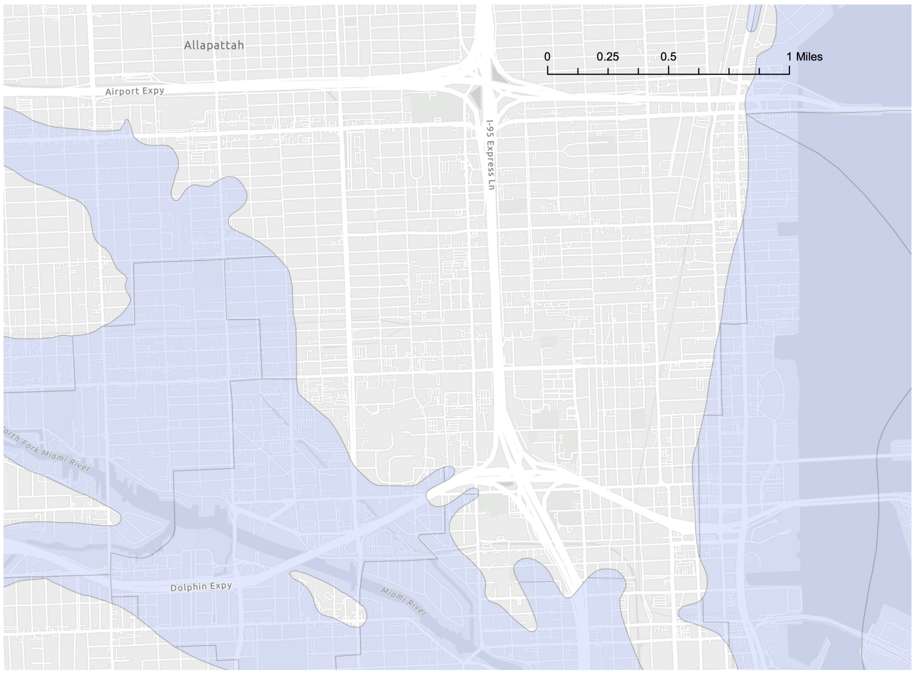
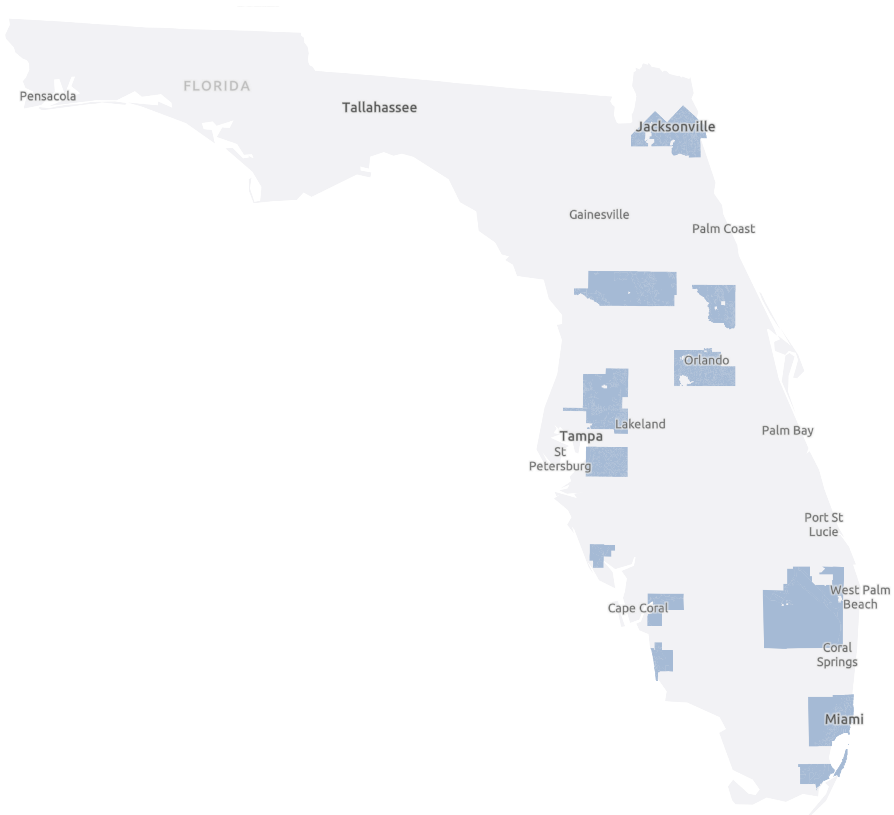
Tables and Figures

Figure 1: Digitized Flood Maps

(a) Digitized Map of Miami (1978)



(b) Coverage of Digitized Maps

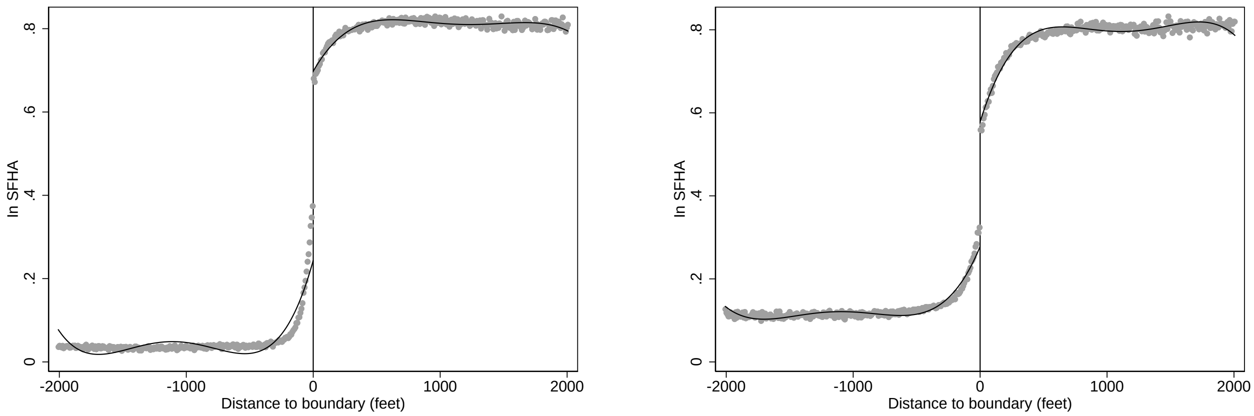


Notes: Figure (a) shows an example of a historical flood map in Miami. SFHAs are depicted in blue. Figure (b) displays the coverage of our digitized sample.

Figure 2: RD Estimates: Current SFHA Status

(a) Share of Land in 1996 SFHA

(b) Share of Land in 2017 SFHA

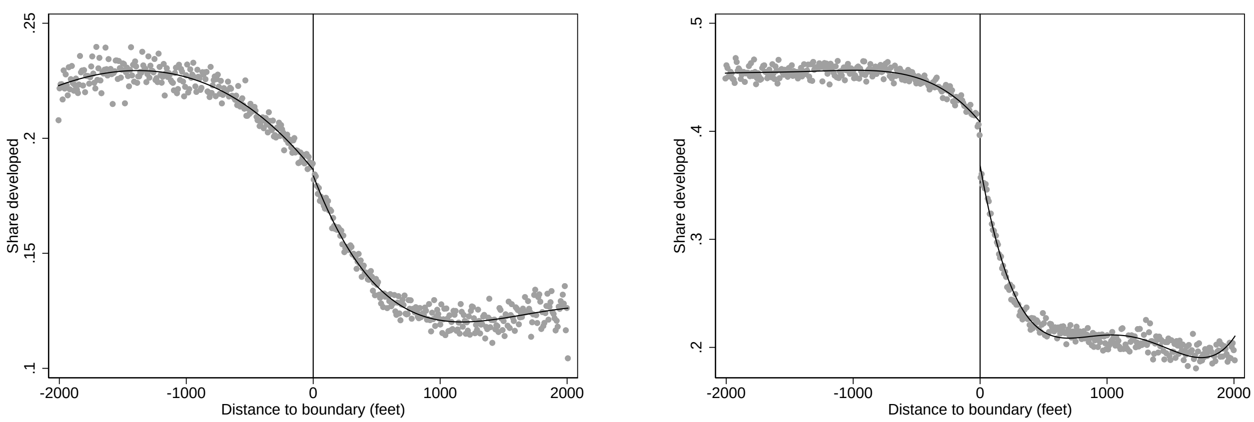


Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the SFHA. Sub-figure (a) plots the share of land in the 1996 SFHA, and sub-figure (b) plots the share of land in the 2017 SFHA. Estimates are residualized of census tract fixed effects.

Figure 3: RD Estimates: Development

(a) Pre-Period Share Developed (1980)

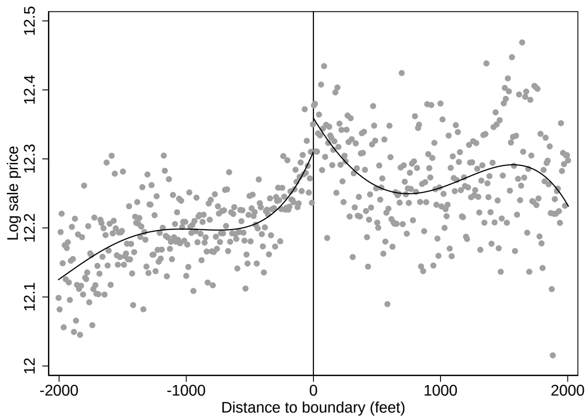
(b) Share Developed in 2016



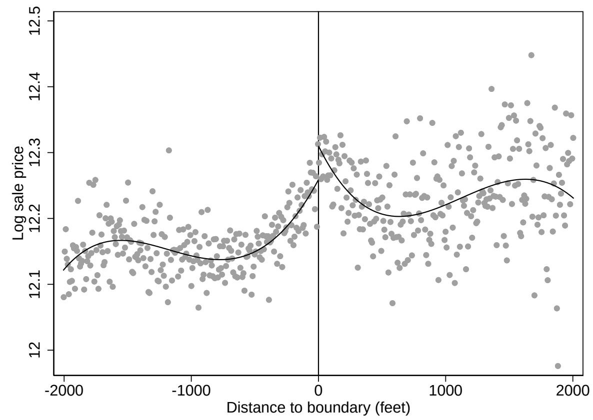
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary measured in feet, with positive distance indicating being inside the SFHA. Sub-figure (a) plots the share of land that was developed as of the late 1970s and early 1980s, and sub-figure (b) plots the share of land developed as of 2016. Estimates are residualized of census tract fixed effects.

Figure 4: RD Estimates: Prices

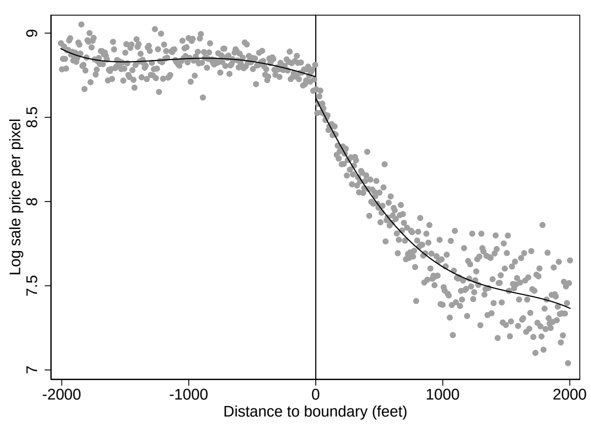
(a) Log Sale Price (2005-2020)



(b) Log Single Family Sale Price (2005-2020)



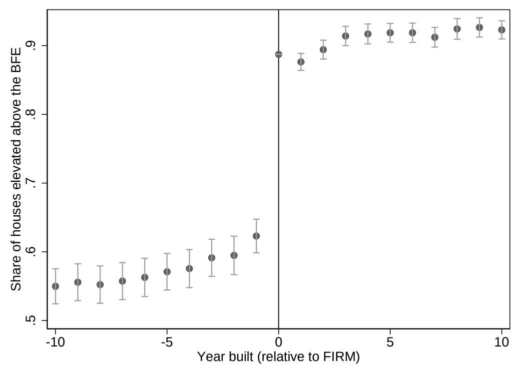
(c) Log Vacant Land Sale Price (2005-2020)



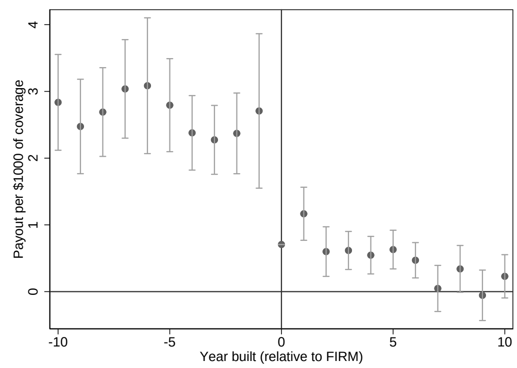
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary measured in feet, with positive distance indicating being inside the SFHA. Sub-figure (a) plots log sales prices of arms-length sales for homes with structures that sold between 2005 and 2020, sub-figure (b) replicates (a) restricting to single family homes, and subfigure (c) plots sales prices for vacant land, with prices normalized by the size of the lot (results are presented as the sale price per 30x30m pixel). Estimates are residualized of census tract fixed effects.

Figure 5: NFIP Enrollment Year Event Study Estimates

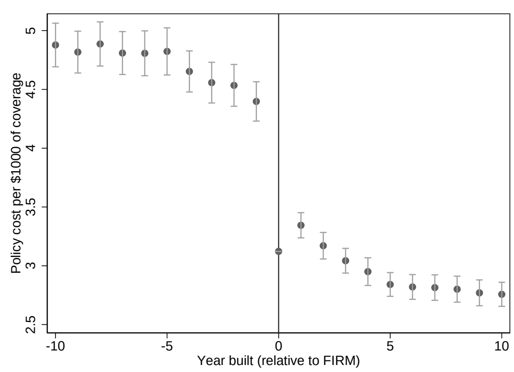
(a) Share Adapted



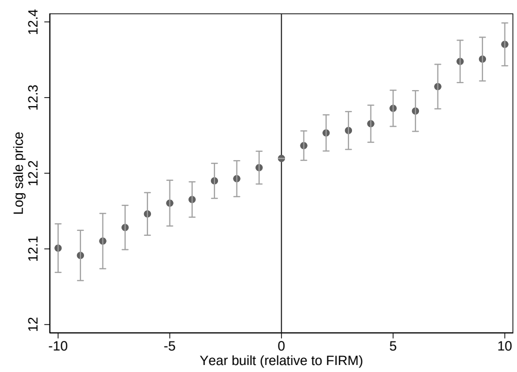
(b) Damages



(c) Policy Cost



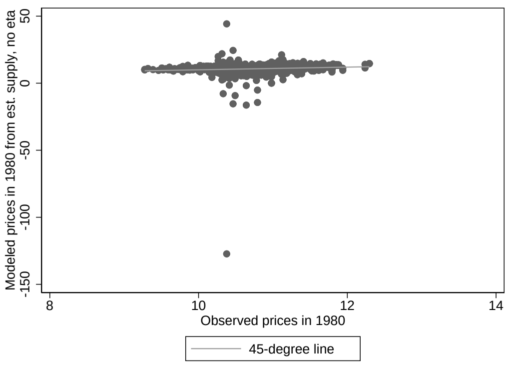
(d) Log Sale Price



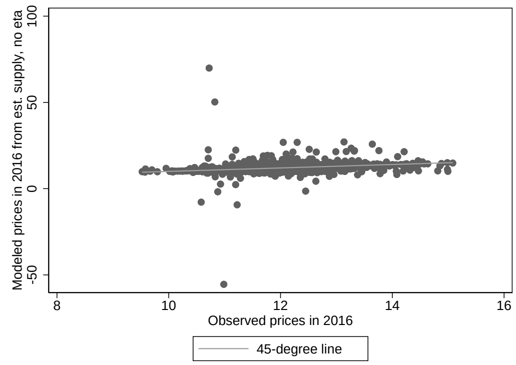
Notes: Figures present coefficients on bins of year built relative to FIRM year. Sample is restricted to single-family residences inside SFHA flood zones.

Figure 6: Supply Model Fit

(a) Prices to rationalize 1980 development using supply curve, omitting idiosyncratic costs



(b) Prices to rationalize 2016 development using supply curve, omitting idiosyncratic costs



Notes: Figures present prices that rationalize observed development using just the supply and just the demand curves. Sub-figures (a) and (b) present the prices in 1980 and 2016 that would rationalize the observed quantities of development, using the estimated parameters (μ_j, σ_j, ψ) and observed elevation decisions, if the model omitted structural supply costs.

Table 1: Summary Statistics of the Spatial RD Sample

	Florida	Digitized map sample		Boundary sample	
		Outside historic SFHA	Inside historic SFHA	Outside historic SFHA	Inside historic SFHA
	(1)	(2)	(3)	(4)	(5)
Panel A. Development					
Share developed in 1980	0.056	0.116	0.124	0.243	0.178
Share developed in 2016	0.145	0.313	0.243	0.473	0.313
Single family homes	5,175,979	552,230	191,507	159,607	80,218
Single family share of structures	0.662	0.872	0.869	0.861	0.869
Share post-FIRM	0.808	0.777	0.648	0.676	0.618
Panel B. Other characteristics					
Share wetlands	0.348	0.205	0.449	0.101	0.343
Share water	0.069	0.015	0.163	0.011	0.098
Distance to coast (miles)	10.7	7.9	6.4	7.7	7.9
Panel C. Prices					
Median house price 1980	47,459	48,198		46,912	
Median house price (2005-2020)	167,233	177,673	274,065	188,148	252,935
Median single family house price (2005-2020)	182,452	179,419	298,716	190,204	277,492
Panel D. Risk					
FEMA flood maps					
Land share in SFHA as of 1996	0.379	0.031	0.798	0.046	0.723
Land share in SFHA as of 2017	0.450	0.106	0.720	0.130	0.664
First Street risk measures					
Land share with $\geq$ 1% chance of flooding	0.447	0.425	0.582	0.240	0.573
Land share with substantial flood risk	0.153	0.065	0.225	0.061	0.223
Total area (square miles)	58,257	4,169	1,978	746	553

Notes: Table displays summary statistics for the entire state of Florida, the geographic area covered by the digitized flood maps, and a sample restricted to 2,000 feet on either side of the SFHA boundary. Median house price in 1980 is a population-weighted census tract average of 1980 Census estimates of the average value of owner-occupied single family housing, tabulated in 1980 dollars. Median house price (2005-2020) tabulates the median sales price, in 2010 dollars, of houses sold between 2005 and 2020. For houses sold multiple times, we take the average transaction price across sales. House prices from 2005 to 2020 are derived from administrative sales records from the state of Florida and are restricted to arms length sales. Substantial flood risk is defined as areas with an estimated flood depth above two feet.

Table 2: Regression Discontinuity Estimates

	Outside SFHA mean	Polynomial	Local linear regression Rectangular kernel, constant band-width	Triangular kernel, optimal band-width	Polynomial excl. coastal areas
	(1)	(2)	(3)	(4)	(5)
Panel A. Current SFHA status					
SFHA as of 1996	0.060	0.430	0.405	0.268	0.454
		(0.020)	(0.019)	(0.014)	(0.024)
SFHA as of 2017	0.130	0.292	0.283	0.207	0.300
		(0.019)	(0.017)	(0.013)	(0.023)
Panel B. Historical land use					
Share of land developed in 1980	0.235	-0.003	-0.005	-0.006	-0.003
		(0.002)	(0.002)	(0.002)	(0.002)
Panel C. Modern land use					
Share of land developed in 2016	0.470	-0.042	-0.044	-0.037	-0.040
		(0.005)	(0.005)	(0.004)	(0.006)
Share of land covered by a building footprint	0.264	-0.029	-0.026	-0.025	-0.027
		(0.004)	(0.004)	(0.004)	(0.004)
Share of land covered by a single family home	0.101	-0.015	-0.013	-0.010	-0.013
		(0.003)	(0.002)	(0.003)	(0.003)
Share of land covered by wetlands	0.098	0.110	0.096	0.071	0.113
		(0.007)	(0.007)	(0.006)	(0.009)
Panel D. Prices					
Log house price	12.151	0.065	0.063	0.066	0.049
		(0.022)	(0.021)	(0.019)	(0.026)
Log single-family house price	12.073	0.056	0.060	0.064	0.051
		(0.018)	(0.018)	(0.015)	(0.021)
Log vacant land price (per 30X30m pixel)	8.806	-0.102	-0.062	-0.070	-0.126
		(0.048)	(0.032)	(0.030)	(0.056)

Notes: Table displays estimates of equation (1). Outside of SFHA means are calculated within 50 feet of the boundary. The polynomial specification estimates a fourth order polynomial separately on either side of the boundary, restricted to a window of 2,000 feet on either side of the boundary. Column (3) estimates linear regressions separately on either side of the cutoff, with each point equally weighted within 250 feet of the boundary. Column (4) estimates the MSE-optimal RD bandwidth from Calonico et al. (2014) and fits a local linear regression within that bandwidth using a triangular kernel. Column (5) replicates Column (2), but excluding land less than one mile from the coast. All discontinuities are estimated on the historic boundaries and exclude boundaries that trace a body of water. Robust standard errors are clustered at the census tract level.

Table 3: NFIP Enrollment Regression Coefficients

Variable	(1) Mean in SFHA, Pre-FIRM	(2) Event study	(3) DD Spec
Share elevated	0.57	0.271	0.335
		(0.013)	(0.019)
Insurance payouts (per $1000 of coverage)	$2.94	$-1.61	$-2.84
		(0.40)	(0.96)
Policy cost (per $1000 of coverage)	$4.80	$-1.21	$-1.78
		(0.075)	(0.127)
Log house price (sold 2005-2020, in 2010 $USD)	12.21	-0.007	0.004
		(0.009)	(0.011)

Notes: Table presents variable means and coefficient estimates from the event-study and difference-in-difference analyses of NFIP enrollment on share elevated, insurance payouts, policy cost, and house price. Elevation, payout, and cost data come from residential NFIP policies from 2010-2018. Price data come from residential sales prices in 2005-2020. We use all single-family residences in Florida. Standard errors are clustered at the census tract level.

Table 4: Parameter Estimates

Supply cost of SFHA (ψ)	0.246
	(0.296)
Demand cost of SFHA ($-\phi$)	0.230
	(0.491)
Demand elasticity (α^D)	-1.0
Consumer WTP to avoid SFHA (ϕ/α^D)	0.230
Estimating α^D?	N

Notes: Table presents estimates of the coefficients on SFHA in the GMM estimation of household preferences and housing supply. Standard errors (in parentheses) were generated from bootstrapping (100 iterations).

Table 5: Counterfactual Outcomes

Outcome	Observed SFHAs Modeled (1)	No SFHA Modeled (2)	Change relative to No-SFHA Counterfactual Current Policy (3)
New dev on land designated as risky in current policy			
Approximate N Houses	640,676	758,567	-117,891
			-15.5%
New dev on all land			
Approximate N Houses	4,472,641	4,472,641	0
			0%
Per-house NPV of location-based damages for new dev.	$9,756	$10,705	$-949
(i.e. current avg annual loss, counterf. locations)			-8.9%
Per-house NPV of adaptation-based damages for new dev.	$9,756	$13,748	$-3,992
(i.e. current locations, counterf. avg annual loss)			-29.0%
Per-house NPV of all damages for new dev.	$9,756	$15,633	$-5,877
(i.e. counterf. locations, counterf. avg annual loss)			-37.6%
Price			
Inside observed SFHA	$250,865	$260,893	$-10,028
			-3.8%
Outside observed SFHA	$174,505	$167,283	$7,221
			4.3%
Overall	$185,670	$182,572	$3,098
			1.7%

Notes: Table presents estimates of counterfactual outcomes using the baseline parameters. N houses is defined by assuming that each developed pixel is equivalent to one house. Prices are weighted by developed area. The No SFHA counterfactual sets $SFHA_z = 0$ everywhere.

Table 6: Counterfactuals: Welfare-Relevant Components

Outcome (Millions of $)	Level	Differences from No-SFHA	
	(1) No SFHAs	(2) Observed SFHAs, $\phi_1 = \phi$	(3) Observed SFHAs, $\phi_1 = 0$
Consumer Surplus		-60,929	-29,225
Damages	69,922	-26,286	-26,286
Total Welfare (CS - Net Damages)		-34,643	-2,939

Notes: Table presents estimates of counterfactual outcomes using the baseline parameters. Outcomes are in millions of $.

A Appendix

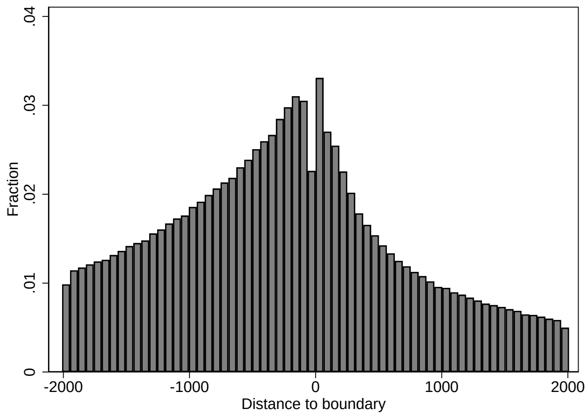
A.1 Appendix Tables and Figures

Figure A.1: Building Above the BFE in Naples, FL

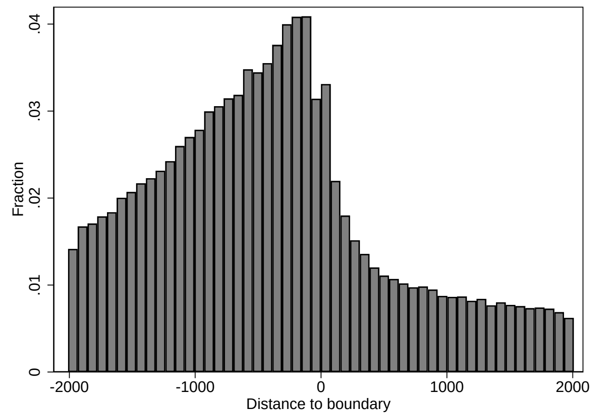
Notes: Figure shows an elevated house in Collier County, Florida. At this location, SFHA regulations require the bottom of the lowest (non-basement) floor to be elevated to 10 feet above ground level.

Figure A.2: Histogram of Distance to SFHA Boundary

(a) 30x30m pixels

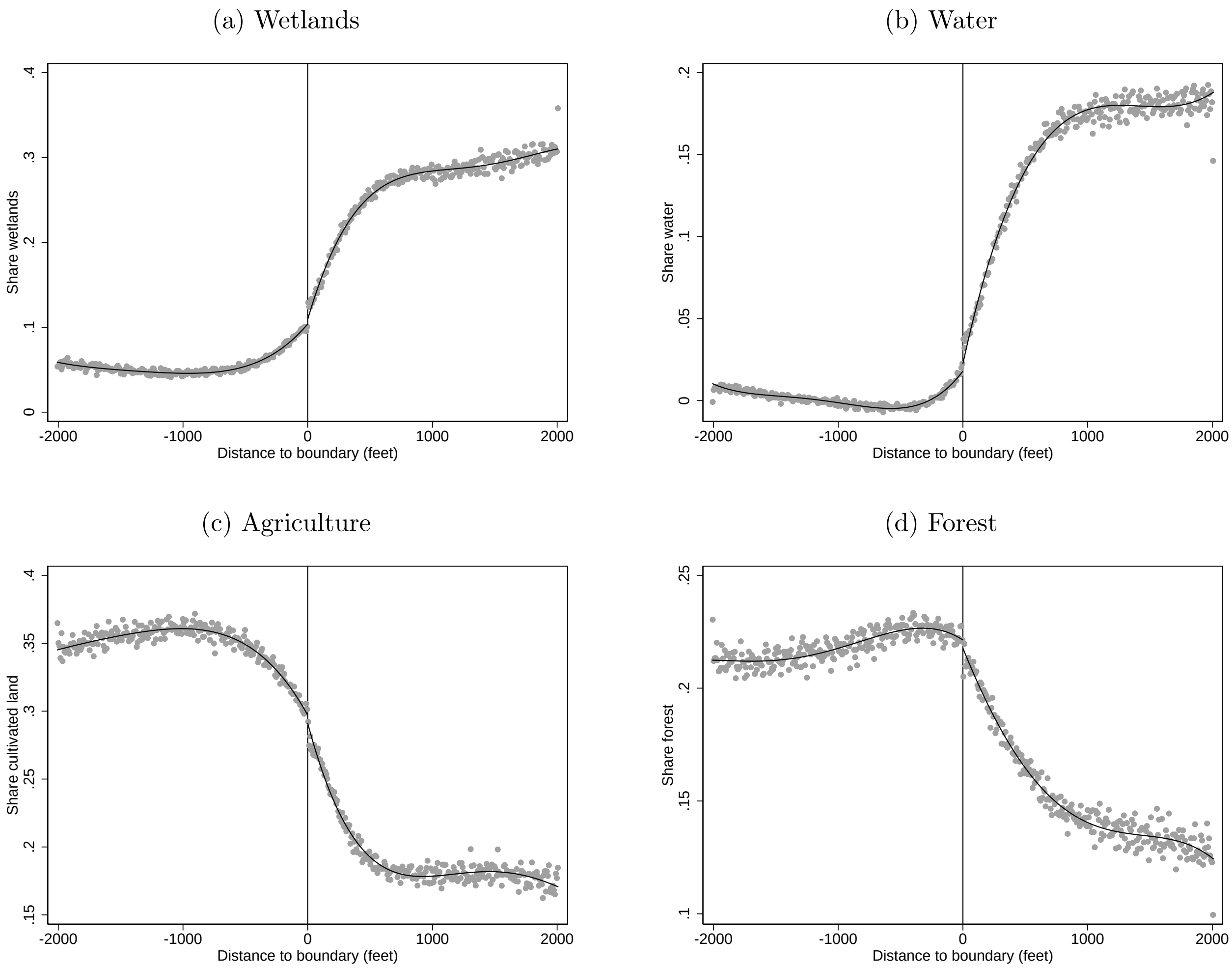


(b) Sales



Notes: Figure presents histograms of distance to boundary for land (a) and sales (b). Distance to boundary is in feet, with positive distance indicating being inside the SFHA. Excludes boundaries that trace a body of water and pixels that overlap with the boundary.

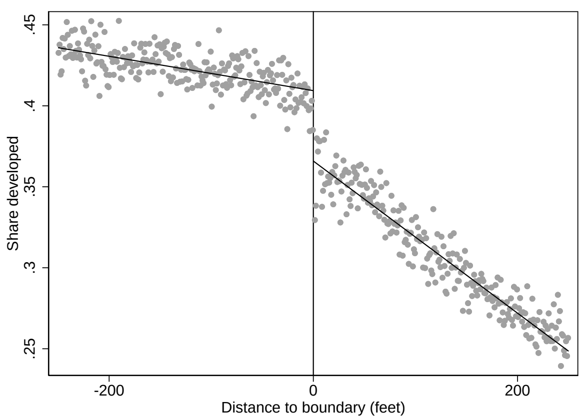
Figure A.3: RD Estimates: Other Pre-Period Land Use



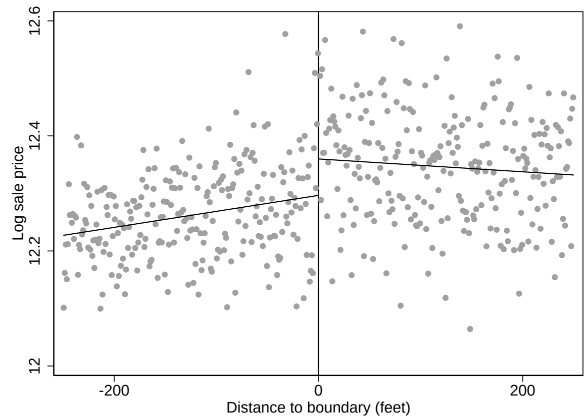
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the SFHA. All land use outcomes are measured as of the late 1970s and early 1980s. Estimates are residualized of census tract fixed effects.

Figure A.4: RD Figures: Local Linear Specification

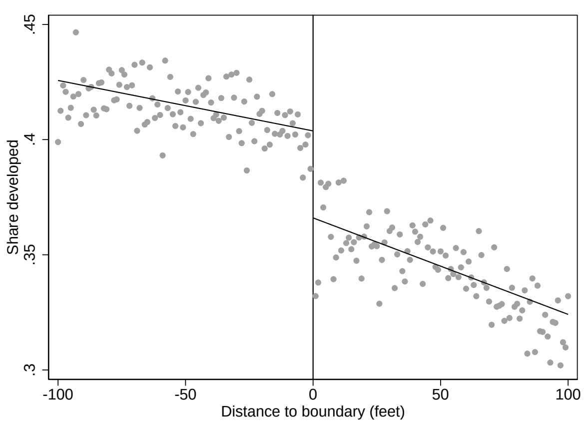
(a) Share Developed in 2016, Rectangular Kernel



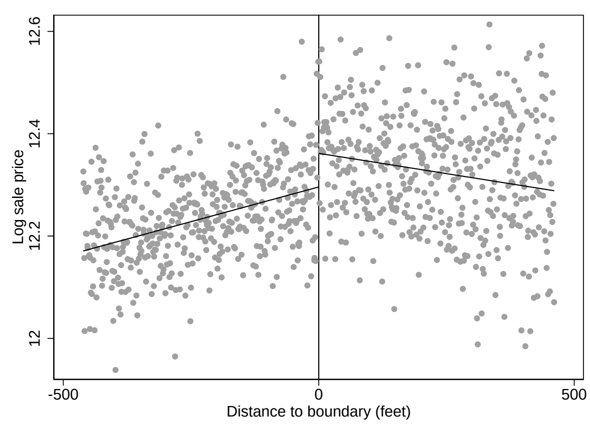
(b) Log Sales Price, Rectangular Kernel



(c) Share Developed in 2016, Triangular Kernel



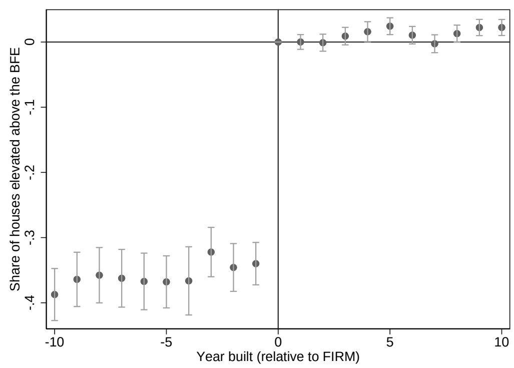
(d) Log Sales Price, Triangular Kernel



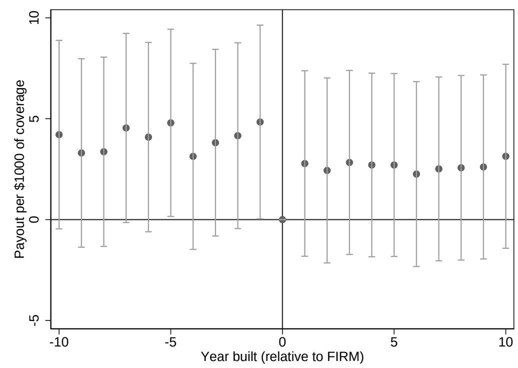
Notes: Figures present RD plots for alternative specifications. Sub-figures (a) and (b) present results using a local linear regression with a rectangular kernel on a fixed 250 foot bandwidth. Subfigures (c) and (d) present results using a local linear regression with a rectangular kernel on the MSE-optimal bandwidth following Calonico et al. (2014). Estimates are residualized of census tract fixed effects.

Figure A.5: NFIP Enrollment Year Difference-in-Difference Estimates

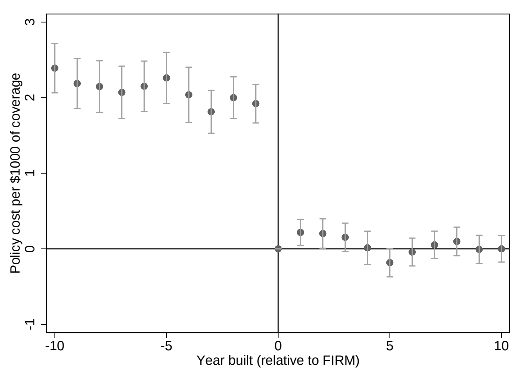
(a) Share Adapted



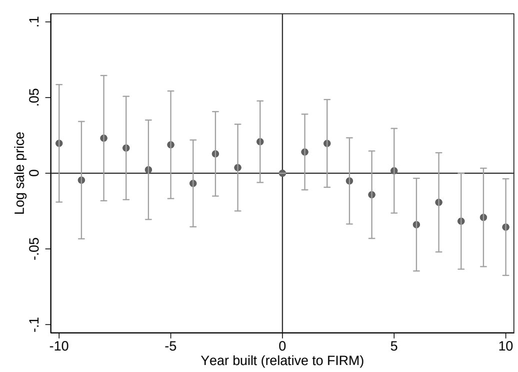
(b) Damages



(c) Policy Cost

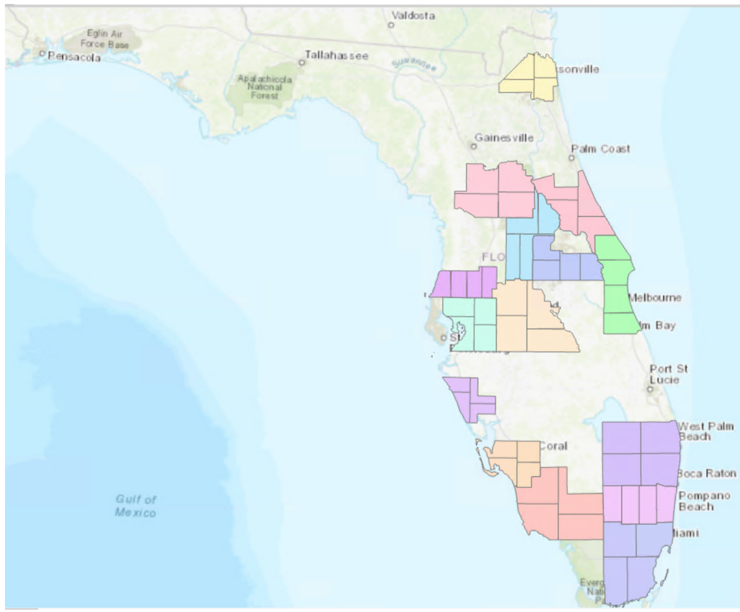


(d) Log Sale Price



Notes: Figures present coefficients on bins of year built relative to FIRM year. Sample is restricted to single-family residences.

Figure A.6: Quadrants



Notes: Figure depicts the top 16 counties with most development in Florida, divided into equal-area quadrants.

Table A.1: Tabulating discrepancies between SFHA status and the First Street Model

	Equal weight to each developed pixel		Weighted by number of parcels	
	Inside SFHA (1)	Outside SFHA (2)	Inside SFHA (3)	Outside SFHA (4)
Inside First Street 100 year floodplain	0.188	0.136	0.277	0.104
Outside First Street 100 year floodplain	0.093	0.583	0.136	0.484

Notes: Table tabulates the share of all buildings in our eleven counties of interest that fall into each of the four mutually exclusive categories of SFHA status by First Street floodplain status. SFHA status designates areas that FEMA has determined is in a 100 year floodplain (has a greater than 1 percent chance of flooding per year). Columns (1) and (2) tabulate the share of pixels covered by a building footprint that are in each category. Columns (3) and (4) tabulate the number of parcels (accounting for multiple parcels on the same pixel).

Table A.2: RD Analysis Results: Other Land Use Outcomes

	Pre-period (1)	Current (2)
Wetlands	0.005	0.110
	(0.019)	(0.071)
Water	0.004	0.007
	(0.003)	(0.006)
Agriculture	0.005	-0.020
	(0.012)	(0.001)
Forest	0.003	0.004
	(0.002)	(0.001)

Notes: Table displays estimates of equation (1) from a fourth order polynomial, fit separately on either side of the boundary, restricted to a window of 2,000 feet on either side of the boundary. All discontinuities are estimated on the historic boundaries and exclude boundaries that trace a body of water. Robust standard errors are clustered at the census tract level.

Table A.3: RD Estimates: Other Sale Price Estimates and Compositional Differences

	Log sale price				Log square footage
	Baseline	Residualized of characteristics	Homes built pre regulations (pre-FIRM)	Homes built post regulations (post-FIRM)	Baseline
	(1)	(2)	(3)	(4)	(5)
All houses	0.065	0.057	0.070	0.048	0.057
	(0.022)	(0.067)	(0.030)	(0.021)	(0.021)
Single-family houses	0.056	0.093	0.103	0.038	0.024
	(0.018)	(0.066)	(0.039)	(0.025)	(0.010)

Notes: Table displays estimates of equation (1) using a fourth order polynomial, fit separately on either side of the boundary, restricted to a window of 2,000 feet on either side of the boundary. Column (2) presents results using the residuals of a regression of log sales price on polynomials in land square footage and interior square footage, sale date (month and year) by county fixed effects, and year built fixed effects. Columns (3) and (4) present results for homes that were built pre vs. post the introduction of the NFIP and the enforcement of building regulations. Column (5) runs the RD regression with the outcome of log square footage of the home conditional on being built. All discontinuities are estimated on the historic boundaries and exclude boundaries that trace a body of water. Robust standard errors are clustered at the census tract level.

Table A.4: Summary Statistics of Construction Year RD Sample

Variable	Inside SFHA (1) Pre-Enrollment	Inside SFHA (2) Post-Enrollment	Outside SFHA (3) Pre-Enrollment	Outside SFHA (4) Post-Enrollment
Share elevated	0.562	0.927		
Building coverage	$188,970	$203,384	$200,232	$203,867
Contents coverage	$42,963	$51,360	$78,908	$80,500
Policy cost	$1,058	$658	$438	$433
Payout	$444	$187	$240	$222
N policy-years	598,961	618,265	236,628	391,829
House price	$263,253	$307,565	$166,752	$188,521
N house sales (2010 $USD)	69,976	103,620	147,663	304,083

Notes: Table presents variable means in the estimation sample for the analysis of the effect of building codes on elevation, insurance payouts, premiums, and house prices. Elevation, payout, and cost data come from residential NFIP policies from 2010-2018. Price data come from residential sales prices in 2005-2020. We use all single-family residences in Florida. Sample is restricted to houses constructed +/- 10 years around NFIP enrollment.

Table A.5: Summary Statistics of the Model Estimation Sample

	All Census Tracts		Balanced Boundary Sample (100 Foot Window)	
	(1)	(2)	(3)	(4)
	Outside SFHA	Inside SFHA	Outside SFHA	Inside SFHA
Share developed, 1980	0.65	0.49	0.56	0.50
N developed gridcells, 1980	2,117	884	94	74
Share developed, 2016	0.84	0.68	0.81	0.73
N developed gridcells, 2016	5,820	1,960	240	179
Share elevated pre-regulation	0.00	0.39	0.00	0.45
Share elevated post-regulation	0.00	1.00	0.00	1.00
Home price, 1980	\$46,405	\$51,510	\$49,785	\$49,785
Home price, 2017	\$207,555	\$302,264	\$238,150	\$282,524
Average Annual Loss (expected annual damages in 2021, middle scenario)	0.0031	0.0095	0.0048	0.0084
N gridcells	20,254	11,231	564	527
N observations	1,045	805	254	254

Notes: Table presents summary statistics of the aggregated sample at the tract-zone-boundary proximity level. Columns (1) and (2) describe the whole sample, used for counterfactuals. Columns (3) and (4) describe the subset of the sample used for estimating the coefficients of interest. This subset is restricted to paired SFHA/non-SFHA observations that are within 100 feet of a boundary. Each observation has the same weight regardless of share developed.

Table A.6: Estimated Parameters, Alternative Specifications

	Specification			
	(1)	(2)	(3)	(4)
Supply cost of SFHA (ψ)	0.246	0.246	0.246	0.246
	(0.296)	(0.322)	(0.286)	(1.603)
Demand cost of SFHA ($-\phi$)	0.230	0.269	0.083	0.247
	(0.491)	(0.572)	(0.159)	(0.813)
Demand elasticity (α^D)	-1.0	-0.4	-3.3	-0.7
				(1.041)
Consumer WTP to avoid SFHA (ϕ/α^D)	0.230	0.672	0.025	0.333
Estimating α^D?	N	N	N	Y

Notes: Table presents parameter estimates from our baseline specification. Column (1) uses a demand elasticity calibrated to -1 based on Song (2021). Columns (2) and (3) use demand elasticities at either end of the range imputed from estimates in Calder-Wang (2021). Column (4) estimates the demand elasticity in-sample. Standard errors (in parentheses) were generated from bootstrapping (100 iterations).

A.2 Data Processing

A.2.1 Algorithm for Selecting Counties to Digitize

The early maps are available online in a series of scanned images. These maps are organized first by county and then by "community," which can be as small as a village or as large as all unincorporated areas of a county. Each community is mapped in a series of tiles. Tiles vary in size and amount of land covered. Because we faced a fixed per-tile digitization cost, and we had a limited budget, our goal was to select the fewest number of tiles that give the most useful variation. In particular, we wanted to ensure we digitized tiles that saw substantial development between the 1980s and present day, but focused on sufficiently large areas to avoid any concerns about selecting on an endogenous outcome.

Our process for selecting maps was as follows:

Step 1. Select the top 15 counties with the largest quantity of newly-developed land, according to our digitized land use data.[37]

Step 2. Divide each county into equal-area quadrants. An illustration of the quadrants is shown in Figure A.6.

Step 3. For each quadrant, compute total area of new development.

Step 4. For each quadrant, count the number of tiles that overlap it.

Step 5. For each quadrant, compute the total area of new development per tiles that would need to be mapped.

Step 6. Sort quadrants by area of new development per tile and drop quadrants with the lowest value until budget constraint is met.

We ended up with 119 tiles from 11 different counties (21 quadrants). An alternative procedure, in which we first dropped all quadrants with more than ten tiles, and then selected the quadrants with largest total area of new development, yielded a very similar set of quadrants.

A.3 Computing Distance to Boundaries

We compute the distance from each grid cell to the closest point on an SFHA boundary that is not within 100 feet of the border of a body of water. We also compute the distance to county boundaries. We drop grid cells that fall within 2 miles of county boundaries to avoid accidentally including SFHA boundaries that overlap county boundaries.

[37]We restricted to the top 15 counties because each county requires substantial manpower to evaluate (we had to manually determine the location of each tile in order to assign it to a quadrant).

A.4 NFIP Enrollment Year Analysis: Additional Material

A.4.1 Data Restrictions

We restrict to residential policies on single-family homes and drop any policies whose coverage exceeds statutory caps. We measure payouts as the total claims paid out for building, contents, and total cost of compliance insurance.[38] We measure policy cost as the total of the premium and other fees. We measure elevation using an indicator of whether a house's elevation exceeds the base flood elevation (BFE).[39] By definition, this variable is not available outside the floodplain since these areas are assumed to be above the base flood elevation. Inside the floodplain, it is available in about 53% of pre-period (unregulated) houses and 98% of post-period (regulated) houses. We measure a house as elevated if the measured difference between lowest floor elevation and BFE is greater than or equal to 0. Pre-period houses can get cheaper policies if they can show they are elevated to the BFE, providing an incentive to report only if they are adapted. Because of this, we assume that if the elevation is missing, the house is not adapted.

To construct the dataset on house prices, we match building footprints to their respective parcels in the Florida parcel data. The parcel data records the effective year built of each house. We then match the centroid of each building footprint to the NLCD gridcell into which it falls. We assign the building footprint the census tract and SFHA status of that NLCD gridcell. We measure sales price as the average arms-length transaction price for single family residences between 2005 and 2020. The next section describes how we measure the year of NFIP enrollment for each census tract.

[38]Total cost of compliance pays for rebuilding that is required when a house is more than 50% damaged and is required to meet a higher building standard when it is rebuilt.

[39]The Base Flood Elevation measures the height of the flood that has a greater than 1% chance of happening every year.

A.4.2 Determining NFIP Enrollment Year

We define the year of each community's NFIP enrollment, and therefore the year in which floodplain regulations were imposed, using data on NFIP policies from 2009 to 2019. We use the fact that policies have an indicator for whether the house was built post-FIRM to construct the year of NFIP enrollment at the census tract level. Because enrollment occurred at the community level, and a community is generally larger than a census tract, characterizing year of enrollment at the census tract level is unlikely to introduce substantial inaccuracies. We define the year of enrollment as the first year within a census tract in which over 50% of homes are coded as post-FIRM. We restrict to census tracts with at least 25 distinct years of construction to avoid classifying the enrollment year based on noise. The year that regulations were imposed was then calculated as the later of the enrollment year or 1975, following NFIP policy.

A.4.3 Difference-in-Difference Specification

We expand on our event-study strategy in Section 5 by also using the fact that building standards were imposed for houses built inside the SFHA but not for houses built outside of it. We estimate the following difference-in-difference models for houses in the SFHA:

$$y_i = \Sigma_r \beta_r 1\{r_i = r\} SFHA_i + \Sigma_r \gamma_r 1\{r_i = r\} + \delta SFHA_i + \gamma_{j(i)} + \varepsilon_i \tag{12}$$

$$y_i = \alpha + \beta Post_i SFHA_i + \gamma Post_i + \delta SFHA_i + \nu r_i + \eta r_i Post + \phi r_i SFHA_i + \psi r_i Post_i SFHA_i + \gamma_{j(i)} + \varepsilon_i \tag{13}$$

Here, $\gamma_{j(i)}$ indicates census tract-by-flood-zone fixed effects. We again cluster standard errors at the census tract level. In all claims and policy regressions, we weight each observation by the number of policies it represents.

The patterns observed in Section 5 are largely unchanged in the difference-in-difference specification. Here, we find that the regulation reduces insurance payouts by \$2.84 per \$1000 of coverage. At the average coverage amount of \$252,000, this translates to a difference of \$715 per year. With a discount rate of 5%, the NPV of these savings is 7.5% of the average house value. Similarly, the difference-in-difference specification finds that the regulation reduces policy premiums by about \$1.78 per \$1000 of coverage, or \$450 per year. The NPV of these savings is 4.7% of the average house value.

A.4.4 Stylized Model of WTP for Adaptation

Suppose that houses are either adapted (A) or non-adapted (B), with a fixed supply of each. Denote c as the (total lifetime) savings from living in an adapted house ($c < 0$ means less money is spent on premiums) and ρ as the share of savings that are internalized by the home-buyer. Let p_A and p_B be the respective house prices (in levels), α be the demand elasticity for house price, and assume supply of houses is fixed.

Suppose $u_{iA} = \alpha(p_A + \rho c) + \varepsilon_{iA}$ and $u_{iB} = \alpha p_B$ where ε_{iA} is distributed i.i.d Type 1 Extreme Value. This specification embeds the assumption that consumers only care about adaptation through its effects on risk. The distribution of ε_{iA} yields that the share of adapted houses $s_A = \frac{\exp(\alpha(p_A - p_B + \rho c))}{1+\exp(\alpha(p_A - p_B + \rho c))}$. Algebraic manipulation reveals that given the prices, the elasticity of demand, and the shares, we can compute the share of internalized savings as

$$\rho = \frac{1}{c}\left(\frac{1}{\alpha}\ln\left(\frac{s_A}{1-s_A}\right) - (p_A - p_B)\right)$$

We assume that $\alpha = -1$ and take $s_A = 0.8$ based on the market share of post-FIRM houses in 2016, and $c = -6403$ based on our estimates. An estimate of no price difference between adapted and non-adapted houses yields an internalized share of $\rho = 0.0002$: effectively zero.

The upper end of the 95% confidence interval is a price increase of 1.06%. This would translate to 25% of the NPV of insurance payout savings and 33% of the NPV of insurance premium savings. If we instead apply this upper-bound estimate we find that $\rho = 0.36$. That is, consumers only internalize a maximum of 36% of the NPV of the savings that would accrue to them from an adapted house.

A.5 Calibration of Supply Elasticities

We use as our starting point estimates produced in Baum-Snow and Han (2019) (BSH) for land development elasticities at the census tract level, estimated between 2001 and 2011. Specifically, we take estimates of the elasticities as of 2001 from the IV specification. We replace any negative elasticities with the smallest nonnegative elasticity in our eleven-county sample. We denote these elasticities as α_j^S.

For each tract, we average the 2016 price and quantity within all observations in the tract, weighting by number of grid cells. We then compute the decrease in share developed that would be implied by the BSH elasticities for a decrease of 10% in the price from the observed 2016 price: $\tilde{q}_j = q_j^{2016} - \alpha_j^S(0.1p_j^{2016})$.[40] We then use our postulated relationship that

$$q_j^{2016} = \frac{Q_j^{2016}}{L_j} = \Phi\left(\frac{p_j^{2016} - \psi E_j^{2016} - \mu_j - \eta_j}{\sigma_j}\right)$$

to recover μ_j and σ_j under the assumption that $\eta_j = 0$ and E_j^{2016} is the elevation status in the tract in 2016. We compute σ_j and μ_j as

[40]We use 2016 price and quantity as our baseline because the BSH estimates used data from 2000-2010.

$$\sigma_j = \frac{-0.1p_j^{2016}}{\Phi^{-1}(\tilde{q}_j) - \Phi^{-1}(q_j^{2016})} \tag{14}$$

$$\mu_j = p_j^{2016} - \psi E_j^{2016} - \Phi^{-1}(q_j^{2016})\sigma_j \tag{15}$$

Since ψ enters the calculation of μ_j and σ_j , we follow a two-step procedure where we first calculate μ_j and σ_j with the assumption that $\psi = 0$, then we re-estimate using the estimated value of ψ.

A.6 Moment Condition Details

A.6.1 Demand-side identification and moments

We assume that in a narrow band around the floodplain boundary, some portion of unobserved amenities $\Delta^D SFHA_z$ is a function of SFHA status, but the remainder $\xi_{jz} - \Delta^D SFHA_z = \tilde{\xi_{jz}}$ is uncorrelated with SFHA. For a given value of Δ^D, we compute the "de-biased" price $\tilde{p}_{jz} = \delta_{jz} - \alpha^D p_{jz}^{2016} - \phi SFHA_z - \tilde{\xi}_{jz}$. We then compute the difference in "de-biased" prices at the boundary as:

$$\begin{aligned}
\tilde{p}_{j1} - \tilde{p}_{j0} &= \frac{1}{\alpha^D}\left(\delta_{j1} - \delta_{j0} - \phi - (\tilde{\xi}_{j1} - \tilde{\xi}_{j0})\right) \\
&= \frac{1}{\alpha^D}\left(\delta_{j1} - \delta_{j0} - \phi - (\xi_{j1} - \Delta^D - \xi_{j0})\right)
\end{aligned}$$

and we set the expected difference in price across the boundary equal to $\beta^{p,2016}$. This yields the moment:

$$E\left[\frac{1}{\alpha^D}\left(\delta_{j1}-\delta_{j0}-\phi-(\xi_{j1}-\Delta^D-\xi_{j0})\right)-\beta^{p,2016}\right]=0 \tag{16}$$

The non-correlation between $\tilde{\xi_{jz}}$ and SFHA yields the following moments, where we introduce μ_D as a constant baseline unobserved amenity level:

$$E[(\tilde{\xi}_{jz}-\mu_D)SFHA_z]=0 \tag{17}$$

$$E[(\tilde{\xi}_{jz}-\mu_D)]=0 \tag{18}$$

A.6.2 Supply-side calibration and moments

We assume that in a narrow band around the floodplain boundary, some portion of unobserved construction costs $\eta^t_{jz}=\Delta^{S,t}SFHA_z$ are a function of floodplain status, while the remainder $\tilde{\eta}^t_{jz}=\eta^t_{jz}-\Delta^{S,t}SFHA_z$ are uncorrelated with floodplain status. In each period, for a given value of $\Delta^{S,t}$, we compute the "de-biased" share developed $\Phi\left(\frac{p^t_{j1}-\psi E^t_{j1}-\mu_j-\tilde{\eta}^t_{j1}}{\sigma_j}\right)$. We then require the difference in "de-biased" share developed at the boundary to match the RD quantity coefficients $\beta^{q,1980}$ and $\beta^{q,2016}$. The moments that identify $\Delta^{S,1980}$ and $\Delta^{S,2016}$ are:

$$E\left[\Phi\left(\frac{p^{1980}_{j1}-\psi E^{1980}_{j1}-\mu_j-\tilde{\eta}^{1980}_{j1}}{\sigma_j}\right)-\Phi\left(\frac{p^{1980}_{j0}-\psi E^{1980}_{j0}-\mu_j-\tilde{\eta}^{1980}_{j0}}{\sigma_j}\right)-\beta^{q,1980}\right]=0 \tag{19}$$

$$E\left[\Phi\left(\frac{p_{j1}^{2016}-\psi E_{j1}^{2016}-\mu_j-\tilde{\eta}_{j1}^{2016}}{\sigma_j}\right)-\Phi\left(\frac{p_{j0}^{2016}-\psi E_{j0}^{2016}-\mu_j-\tilde{\eta}_{j0}^{2016}}{\sigma_j}\right)-\beta^{q,2016}\right]=0 \tag{20}$$

We then recover the supply cost of regulation ψ and the baseline supply cost terms $\mu_{s,1980}$ and $\mu_{s,2016}$ with the moments:

$$E[(\tilde{\eta}_{jzb}^{1980}-\mu_{s,1980})SFHA_z 1\{b=close\}]=0 \tag{21}$$

$$E[(\tilde{\eta}_{jzb}^{2016}-\mu_{s,2016})SFHA_z 1\{b=close\}]=0 \tag{22}$$

$$E[(\tilde{\eta}_{jzb}^{1980}-\mu_{s,1980})1\{b=close\}]=0 \tag{23}$$

$$E[(\tilde{\eta}_{jzb}^{2016}-\mu_{s,2016})1\{b=close\}]=0 \tag{24}$$

A.7 Calculation of Expected Damages

We define flood risk using data from the First Street Flood Lab estimates of Average Annual Loss (AAL). AAL expresses expected annual damages as a share of house price. These data come from parcel specific-estimates (as opposed to the raw hazard layer) that combine the raw hazard layer (which generates the parcel-specific inundation depth) with the output of an engineering damage model. The damage model takes as inputs a number of features of the structure, including its market value, number of stories and units, and foundation type, and calculates damages using the HAZUS-MH methodology. The HAZUS-MH methodology was developed for FEMA to calculate estimated damages from natural disasters and is based on a set of depth-damage curves collected from FEMA's Federal Insurance and Mitigation Administration (FIMA) and the USACE Institute for Water Resources (USACE-IWR).[41]

Expected damages are computed as the product of number of newly-developed gridcells by the NPV of expected damage under a given counterfactual. The expected damage is computed as $0.7 \times P_{jz}^{CF} \times AAL_{jz} + AD_{jz}^{CF}(E_{jz}^{2016} - E_{jz}^{1980})$, where P_{jz}^{CF} is the (level) price of a house under counterfactual CF and AAL_{jz} is the average annual loss.[42]The term AD_{jz}^{CF} describes additional damages which are attributable to a lack of elevation. This is equal to 0 in the observed counterfactual. It is also equal to 0 if an observation was elevated in the pre-period (since we assume it would therefore also be elevated in the post-period.) In the no-SFHA counterfactual, reflecting our results from the FIRM RD, for houses that did not previously elevate, we add annual damages of $\frac{1}{.3} \times \$2.84 \times \frac{P_{jz}^{Obs}}{1000}$.

We compute three measures of damages:

$D^{All} = \frac{1.05}{.05} N^{CF} D^{CF}$

$D^{Reloc} = \frac{1.05}{.05} N^{CF} D^{Obs}$

$D^{Adapt} = \frac{1.05}{.05} N^{Obs} D^{CF}$

where N^{CF} denotes the number of newly-developed gridcells under counterfactual CF and D^{CF} denotes the NPV of expected damage under counterfactual CF. The first measure ("all damages") measures total expected damages by multiplying the counterfactual number of newly-developed houses in each area by the expected damages in that counterfactual. The second measure ("relocation-based damages") holds the NPV of expected damage constant at the observed level and only changes the number of houses in each location. This captures damages attributable to the location of houses only. The third measure ("adaptation-based

[41]See https://assets.firststreet.org/uploads/2021/02/The_Cost_of_Climate_FSF20210219-1.pdf for more details.

[42]This 70% factor was recommended by First Street, who provided the underlying AAL data.

damages") holds the number of houses in each location constant at the observed level and only changes the expected damage in each location. We then compute per-house damages by dividing the total expected damages of each type by the number of newly-developed houses (which is constant across counterfactuals).

A.8 Welfare Calculation Details

We compute consumer surplus differences in each counterfactual scenario relative to the unregulated benchmark. Following Small and Rosen (1981), we calculate per-person consumer surplus in each market m as:

$$CS_i = \frac{-1}{\alpha^D} \ln \Sigma_{j \in J_m, z \in \{0,1\}} \exp(\alpha^D p_{jz} + \phi SFHA_{jz} + \xi_{jz}) \tag{25}$$

where j denotes census tract and z indicates SFHA status. For each market, we compute the change in level price required to make per-person consumer surplus in the counterfactual equivalent to that of the same market in the unregulated benchmark. That is, we solve for ΔP_m^{CF} such that

$$\Sigma_{j \in J_m, z \in \{0,1\}} \exp(\alpha^D p_{jz}^{NoSFHA} + \xi_{jz}) = \Sigma_{j,z} \exp(\alpha^D \ln(P_{jz}^{CF} + \Delta P_m^{CF}) + \phi SFHA_{jz} + \xi_{jz}) \tag{26}$$

where P_{jz}^{CF} is the house price in levels in the counterfactual of interest, and p_{jz}^{NoSFHA} is the house price in logs in the unregulated benchmark. We then compute differential consumer surplus of new development as the sum of price-equivalents multiplied by the number of new houses in each county N_m: $\Delta CS^{CF} = \Sigma_m N_m \Delta P_m^{CF}$.