

# The Effects of Floodplain Regulation on Housing Markets

Abigail Ostriker and Anna Russo\*

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## Abstract

This paper investigates the effects of housing regulations designed to correct a wedge between privately- and socially-optimal construction in areas at risk of flooding. Using a spatial regression discontinuity around regulatory boundaries and an event study around the policy's introduction, we document that floodplain regulation reduces new construction in high-risk areas and increases the share of newly-built houses that are elevated. Embedding these effects in a model of residential choices with elastic housing supply, we find that the policy reduces expected flood damages by 29%. One-third of this reduction is driven by relocation of new construction to lower-risk areas, and two-thirds is driven by adaptation of houses remaining in risky areas. However, this second-best policy does not improve welfare: it overcorrects the market frictions, inducing more consumers to move away from high-risk areas than is socially-optimal.

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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 grow by 25% 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). A central concern among policymakers and economists is that these trends are driven by a wedge between social and private values for flood safety (Kydland and Prescott, 1977; Coate, 1995; Ben-Shahar and Logue, 2016). Indeed, a large body of work has documented that private incentives to reduce flood risk are muted by both misperceptions of that risk and expectations of government aid (Gallagher, 2014; Kousky et al., 2018b; Bakkensen and Barrage, 2021; Wagner, 2021; Mulder, 2021). To correct these market frictions, policymakers demarcate especially risky locations as “Special Flood Hazard Areas” (SFHAs) and regulate them more strictly. Inside the SFHA, developers are required to build flood-adapted homes, and homeowners face a flood insurance purchase mandate and high flood insurance prices.<sup>1</sup>

This paper investigates the impact of floodplain regulation on the location of new construction, housing prices, estimated flood damages, and consumer welfare. The effect of this coarse policy on welfare is ambiguous, as it need not reduce damages more than the costs it imposes via distortions in the housing market. In this paper, we study the extent to which floodplain regulation reduces flood damages, via both the location and elevation of construction, and we weigh these benefits against the regulation’s costs. We do so by studying effects around the boundaries of regulation and around the date of the regulation’s introduction. We then embed these empirical results in a model of residential choices and development to extrapolate effects away from the boundary, estimate the costs of regulation, and investigate the welfare effects of current and counterfactual policies.

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<sup>1</sup>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).

To conduct our analysis, we assemble a new comprehensive and spatially-granular dataset to describe regulation, real estate development, and flood risk in Florida over a 40-year time horizon. This dataset combines newly-digitized archival scans of maps of historic flood zone extents, granular remote-sensing-based measures of historic and current development, administrative data describing house prices and attributes, and the flood risk profile of current and counterfactual development, generated from a state-of-the-art hydrological model. The scope and detail of our dataset are both critical to our analysis. Because our dataset extends to the first maps delineating regulated areas – first digitized for this project – we are able to study the policy’s effect on long-run development and confirm the validity of our core empirical strategy. And because our dataset details both adaptation and location decisions, as well as how flood damages vary along these margins, we are able to comprehensively measure the policy’s effect on flood risk.

We use two complementary empirical strategies — a spatial regression discontinuity and an event study around the regulation’s implementation — to characterize the policy’s risk reduction effects along two margins: reduced construction and mandatory adaptation in risky areas. Our spatial regression discontinuity design compares current development and house prices on either side of the regulatory boundary delineated at the time of the policy’s introduction in the 1970s and 1980s. Our analysis relies on the assumption that flood risk and other amenities are smooth through these initial regulatory boundaries. While unlikely to hold in modern maps, this assumption is reasonable for the original maps because mapping technologies were rudimentary and homeowners lacked the ability to influence the initial regulatory boundaries.<sup>2</sup> Importantly, we validate this assumption by examining smoothness in pre-period land use through the historic boundaries. We document that development is 9% lower just inside the regulated area, highlighting the potential for the policy to reduce

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<sup>2</sup>After the initial maps were drawn, landowners could deregulate developed parts of their properties either by petitioning to correct a mistake or physically elevating land to reduce its risk. Since maps are updated over time, this behavior produces a negative correlation between development and floodplain designation in modern flood maps.

damages by shifting development out of risky areas. This decrease in development is not accompanied by a reduction in prices – if anything, prices are slightly higher just inside the regulated area – indicating that floodplain regulation imposes costs on developers, which they at least partially pass through to consumers.

Motivated by the result that regulations bind on the supply side, we turn to a second empirical strategy to investigate whether mandated adaptation generates social or private value. Building on Wagner (2021), we exploit the sharp timing of the policy’s introduction to examine the effect of building standards on flood risk in an event study design. We find that regulation increases the share of adapted homes by 27 percentage points and reduces flood risk by 4.2% of average home value. Though mandatory adaptation generates social value via reduced damages, we show that homeowners do not privately value this reduction in flood risk. This result is consistent with prior work documenting a large wedge between social and private valuations of flood risk, implying scope for welfare-improving intervention. It also highlights that the increase in price across the boundary is entirely attributable to construction costs, not differences in willingness-to-pay for elevated homes.

Together, our empirical results yield four facts. First, floodplain regulation suppresses construction in high-risk areas. Second, the policy’s elevation mandate binds, and reduces flood risk. Third, the cost of mandating elevation is large enough that the price effect of the inward supply shift dominates at the boundary. Fourth, our setting exhibits a wedge between private and social valuations for flood safety, yielding the potential for the policy to improve welfare. However, these results alone are insufficient to quantify the total effect of the policy on either damages or overall welfare. The policy’s effects depend critically on the location of counterfactual development: both risk and amenities vary across space. Additionally, the large size of the regulated area may introduce spillover effects on the unregulated locations, which could mitigate the incentive to relocate and impose pecuniary externalities on unregulated consumers. Moreover, while our empirical results indicate that floodplain

regulation shifts supply, we require a model to quantify the regulation’s costs to developers and consumers.

Therefore, to account for counterfactual location and spillover effects, quantify the regulation’s costs, and compare welfare across current and counterfactual policies, we specify and estimate a model of residential choice and real estate development. In our 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 outside option of land use; housing profits depend on housing prices, tract-zone-specific construction costs, and costs of adaptation. Because our event study estimates imply that homeowners are unwilling to pay for adaptation, we can simplify the model by differentiating houses only by location — not risk — and assuming that suppliers do not endogenously provide adapted houses absent policy intervention. We estimate the model’s key parameters — demand and supply costs of floodplain designation — by matching the cross-border differences we find in our boundary discontinuity analysis. We calibrate supply and demand price elasticities to estimates from the literature (Calder-Wang, 2021; Song, 2021; Baum-Snow and Han, 2022).

Using our model, we find that the policy achieves its intended effect: it reduces expected flood damages by 29%, or approximately \$1.8 billion per county. Both the extensive-margin location channel and intensive-margin adaptation channel are quantitatively important in this setting. One-third of the reduction in risk (9%) is driven by the relocation of new construction to lower-risk locations. The remaining two-thirds are due to elevation mandates for the houses that remain in risky locations. However, we find that these risk reductions are driven by large costs imposed by the regulation on both consumers and producers of housing. Our parameter estimates suggest that mandatory elevation of a house increases construction costs by 25%. Furthermore, consumers are willing to pay 23% more for a house to avoid living in a regulated area.

We conclude by developing a welfare framework to trade off these costs against the benefits from averted damages. We define welfare to capture both consumer surplus and any socially-costly damages that consumers do not value. Because the wedge between private and social valuation of flood risk could be due to misperceptions, our welfare framework allows consumers' decision and experienced utility to diverge (Allcott and Taubinsky, 2015). Our estimates suggest that even if the estimated costs to consumers do not affect experienced utility, floodplain regulations do not improve welfare, since the damage reduction is more than offset by regulatory costs. Any additional experienced costs of the regulation to consumers reinforce this conclusion.

We find that the policy does not improve welfare despite a setting which is maximally-favorable to intervention: consumers have zero private value for flood safety. If market frictions are present that the policy is designed to — and indeed does — correct, why does welfare not improve? The policy performs poorly because the blunt elevation mandate is overly-corrective. Building an elevated home is expensive, and mandating elevation increases costs and reduces development in high-risk areas more than a flexible policy instrument would. Although the policy removes socially-inefficient houses from the riskiest areas, it also removes some construction in risky areas that would have been welfare-improving. This finding underscores the gains to transitioning from a blunt attribute-based regulation to a more flexible policy instrument, such as a tax or an actuarially-fair insurance mandate.

Our principal contribution is to develop a framework for evaluating the net welfare impacts of a second-best policy aimed at correcting the wedge between private and social valuations of flood risk. In doing so, we build on and contribute to a number of literatures.

Most narrowly, we contribute to a body of work analyzing various aspects of floodplain regulation, including its effect on house prices (Harrison et al., 2001; Bin and Landry, 2013; Indaco et al., 2018; Gibson and Mullins, 2020; Hino and Burke, 2021; Lee, 2022), in-place adaptation (Wagner, 2021; Mulder, 2021), and population (Lee, 2022; Peralta and Scott,

2019). Our work builds on this literature by developing an equilibrium framework that allows us to analyze and quantify all of the channels for averting damages simultaneously, and, importantly, to trade them off against costs. In particular, we provide new evidence that floodplain regulation substantially reduces development in risky areas, and that this reallocation of development is quantitatively important to the policy’s overall effect on flood damages. In doing so, we also contribute by introducing a credible new empirical strategy in the form of our spatial regression discontinuity around historic regulatory boundaries.

We also contribute to a literature documenting frictions in mitigating or adapting to climate risk (Annan and Schlenker, 2015; Deryugina and Kirwan, 2018; Kousky et al., 2018b; Balboni, 2019; Baylis and Boomhower, 2019; Bakkensen and Barrage, 2021; Wagner, 2021; Mulder, 2021) by investigating whether a second-best corrective policy can reduce the welfare losses from these frictions. In doing so, our paper relates to a body of work examining second-best corrective instruments to reduce welfare losses from externalities or internalities (Ito and Sallee, 2018; Germeshausen, 2018; Barahona et al., 2020; Kellogg, 2020). Our results highlight that the existence of frictions need not imply that corrective policies are necessarily welfare-improving.

Methodologically, we draw on work in urban economics that embeds boundary discontinuity designs in discrete choice frameworks (Bayer et al., 2007; Anagol et al., 2021; Song, 2021). We build on these models by incorporating recent estimates of spatially-granular supply elasticities from Baum-Snow and Han (2022) to more-accurately characterize equilibrium changes in housing supply across markets. Moreover, the specific policy we study – floodplain regulation – is a particularly important setting, as it is estimated that by 2050, more than ten percent of all homes will be in an area at high risk of flooding.

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 the causal effects of SFHA designation. We specify and estimate our equilibrium model of the housing market in Section 5. Section 6 simulates distributions of development and prices and discusses welfare under factual and counterfactual policies. We conclude by considering directions for future work and implications for policy.

## 2 Institutional Background

### 2.1 The National Flood Insurance Program and Special Flood Hazard Areas

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).

After the NFIP was established in 1968, the program was rolled out to communities throughout the country in the late 1970s and 1980s.<sup>3</sup> When a community joined the NFIP, it obtained a Flood Insurance Rate Map (FIRM), produced by the NFIP using a hydrological study. After the FIRM was produced, buildings in specific areas had to comply with regulations, and flood insurance became available to homeowners.

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<sup>3</sup>Communities are geographic units specific to the NFIP. They are generally municipalities or unincorporated areas of a county.

In both the initial FIRM and subsequent, updated flood maps, 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. All new construction and substantial home improvements in SFHAs must comply with building regulations that require that a home's lowest floor lie above the Base Flood Elevation (BFE).<sup>4</sup> In the SFHA, some homeowners face a flood insurance mandate and higher flood insurance prices for otherwise-equivalent houses.<sup>5</sup> 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.

Throughout the United States, 10% of land<sup>6</sup> 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

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 discontinuities in flood risk or other amenities. The validity of this assumption relies on

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<sup>4</sup>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 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.

<sup>5</sup>Homeowners with federally-backed mortgages are legally required to purchase flood insurance.

<sup>6</sup>Authors' calculations using 2017 flood maps.

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.<sup>7</sup> 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.

After the construction of the initial FIRM, 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, 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

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<sup>7</sup>Today, LiDAR technology has improved the accuracy of land elevation models. More-powerful computing has also improved the precision of hydraulic modeling over time.

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.<sup>8</sup>

We bring together four 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.<sup>9</sup> We aimed to collect the first Flood Insurance Rate Maps (FIRMs) 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 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.

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<sup>8</sup>See <https://nfipservices.floodsmart.gov//reports-flood-insurance-data> for details.

<sup>9</sup>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.

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.<sup>10</sup> 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,<sup>11</sup> Table 1 illustrates that development increased substantially both statewide (2.6x) and in our sample of interest (2.4x).

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<sup>10</sup> Across Florida, the median number of raster grid cells per census tract is 4865.

<sup>11</sup> Between 1980 and 2020, Florida’s population more than doubled from 9.75 million to 21.5 million (US Census Bureau, 2022).

**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.<sup>12</sup> Table 1 panel C summarizes average home prices statewide and in our sample of interest.

We supplement our data from the Florida Department of Revenue with data from NFIP claims and policies (from 2010 to 2018) to provide information about adaptation, policy cost, and flood insurance damages for insured structures. We also obtain historical data on home prices from the 1980 Census (Manson et al., 2021).

**Flood Risk Model** 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 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%

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<sup>12</sup>This dataset is also derived from satellite imagery, mostly captured in 2019. See <https://github.com/microsoft/USBuildingFootprints> for more details.

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 Causal Evidence on the Effects of Floodplain Designation

We begin our analysis by describing the effects of SFHA designation on flood risk, which could occur both by shifting development away from high risk areas and by mandating adaptation in those risky areas. In this section, we employ a spatial regression discontinuity design around the regulatory boundary to study the policy’s impact on the location of new construction, and perform an event study around the introduction of the policy to study its effect on house elevation and the consequent effects on flood damages.

### 4.1 Boundary Analysis of the Effect of Floodplain Regulation on Construction and Prices

Our spatial regression discontinuity design compares current development and house prices on either side of the historic regulatory boundary to investigate the extent to which floodplain regulations reduce development in risky areas. We also use our boundary discontinuity to compare differences in home prices in regulated versus unregulated areas. In the context of our model, these two equilibrium points – prices and quantities inside and outside the regulated areas – will later allow us to estimate the costs of regulation by revealed preference.

#### 4.1.1 Empirical Strategy

Estimating the effect of SFHA designation on new construction presents two challenges. First, SFHA designation could be correlated with unobserved amenities, such as coastal access or views. Indeed, Table 1 indicates that land inside the SFHA is more likely to be water or wetlands and is closer to the coast. Second, SFHA designation may be endogenous to real estate development, as the mapping process allows homeowners to deregulate parts of their properties by petitioning for map corrections or “filling” in dirt to elevate the land. Inside the SFHA, homeowners who are correctly mapped have an incentive to elevate their house to “escape” the SFHA. Homeowners who were incorrectly mapped have an incentive to petition FEMA to correct a mistake that overstates a home’s risk. Meanwhile, owners of undeveloped land face no such incentives. 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 endogenous amendment process would lead to a mechanical negative correlation 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, since homeowner petitions and reactive adaptations were not reflected in the original maps: amendments and endogenous adaptation happen only after the maps are drawn. 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:

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

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  $\beta$ , 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.

#### 4.1.2 Results

We discuss our results in the context of an *intent-to-treat* framework: the treatment of interest is the initial floodplain designation, which may evolve over time. Appendix Figure 2 documents the evolution of the relationship between initial designation and floodplain status over time.

**Exogeneity of boundaries** To validate our identifying assumptions, we check for smoothness in land use through the regulatory boundaries prior to SFHA designation.<sup>13</sup> 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. We test this by estimating Equation 1 with  $y_i$  equal to pre-period development, measured with high resolution photographs in the late 1970s and early 1980s. Figure 3a shows that pre-period development is smooth across the boundary, and the estimated coefficient, reported in Table 2, is economically small and statistically insignificant.<sup>14</sup> This test supports our hypothesis that the institutional details of the initial mapping process provide a compelling setting to conduct a boundary discontinuity analysis.

**Development falls in the SFHA** By 2016, we see a sharp discontinuity in development at the SFHA boundary, as shown in Figure 3b. Table 2 reports the estimated level shift at the boundary ( $\hat{\beta}$ ), which indicates that land just inside the SFHA is 4.2 percentage points less likely to be developed than land just outside the SFHA.<sup>15</sup> This effect is substantial: it is 10% of the outside-SFHA mean level of development and it represents an 18% reduction as a share of total new development occurring between 1980 and 2016 just outside the SFHA.<sup>16</sup> The effect is driven by single family homes, which make up the majority of residences (86%) in our sample. The magnitude of the policy's negative effect indicates the potential for both

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<sup>13</sup>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.

<sup>14</sup>Appendix Figure A.3 and Table A.2 demonstrate smoothness in other pre-period land use outcomes. Note that in our alternative local-linear specification, the coefficient continues to be economically small but does become statistically significant.

<sup>15</sup>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, 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).

<sup>16</sup>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.

substantial reductions in flood damages via reduced building in risky areas and costs to either developers or consumers that yield this behavioral response.

**Prices increase in the SFHA** The effects of floodplain regulation on house price are *ex-ante* ambiguous. On the one hand, by suppressing demand for regulated houses, SFHA designation could push prices down. On the other hand, by requiring suppliers to employ costlier construction methods, SFHA designation could push prices up. Figure 4 indicates that supply must shift inward substantially as a result of the policy: though we observe a large decrease in development, prices are non-decreasing through the boundary. In fact, our point estimates suggest that prices are 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).<sup>17</sup>

Our evidence suggests that this positive price effect should indeed be interpreted as an inward shift in supply. We rule out compositional changes in housing characteristics, including house elevation, as drivers of our result. In Appendix Table A.3, we report estimates using sales prices residualized of a rich set of observable characteristics of each house and show that the positive price effects persist. We also see similar effects on prices for homes built before and after the introduction of the SFHA regulations. We provide additional evidence to rule out changes in amenities driving a positive price effect: our results are robust to excluding areas close to the coast (column (4)) of Table 2 ). Figure 4c shows that vacant land prices decrease by 10% across the SFHA boundary, a pattern that would not be consistent with

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<sup>17</sup>This finding contrasts with recent work that has found floodplain designation *decreases* house prices by 1-2% (Hino and Burke, 2021). That work exploits map updates to achieve identification, and therefore primarily captures short-term demand effects. In our context, we study the effect of floodplain designation over the course of 40 years. This long-run setting allows supply to respond to mandatory building codes. An inward shift of the supply curve would reduce the quantity of houses built in the floodplain over that time, and could counteract the demand-driven price decrease.

increases in amenities.

**Robustness** Columns (2) and (3) 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 using the MSE-optimal bandwidth proposed by Calonico et al. (2014).

## 4.2 Event Study of the Effect of Floodplain Regulation on Adaptation, Flood Damages, and Prices

The preceding results indicate that regulations impose costs on developers that shift supply inward. In this section, we investigate whether these regulations create social value, in terms of reduced damages, and whether consumers privately value those damage reductions.

### 4.2.1 Empirical Strategy

Following Wagner (2021), to estimate the effect of adaptation we exploit the fact that a community had to adopt flood-safe building standards at the time it enrolled 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:

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

where  $y_i$  is an outcome of interest (elevation, insurance payouts, 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.<sup>18</sup> We scale payouts by amount of purchased insurance coverage; since individual claims cannot be linked to individual policies we therefore estimate this model at the census-tract-by-flood-zone-by-relative-year-built level.<sup>19</sup> 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:

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

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,  $\beta$  indicates the causal effect of building code regulations on our outcomes of interest.<sup>20</sup>

#### 4.2.2 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 are presented in Table 3. Table 3 also shows the results from a difference-in-difference specification that compares trends in houses in the floodplain to houses outside the floodplain.

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<sup>18</sup>Census tracts are smaller than communities.

<sup>19</sup>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.

<sup>20</sup>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.

This specification, which is described in Appendix A.4.3 and graphed in Appendix Figure A.5, yields similar results.

**Building regulations increase adaptation and reduce flood damages** Figure 5a shows that building regulations increase the share of homes built above the Base Flood Elevation (BFE) from 60% to 90%. We draw two conclusions: first, regulations increase adaptation substantially, and second, a large share of homes were already built above the BFE. Our results suggest our measurement of pre-period adaptation is most likely driven by areas where the BFE is sufficiently low that homes are built above it in standard construction practice. We arrive at this conclusion using two pieces of evidence. As we document below, consumers are unwilling to pay for adaptation, meaning that costly adaptation will not be provided absent regulation. Second, we find that the pre-period adapted homes are concentrated in areas with low Base Flood Elevations.

Figure 5b shows that the introduction of building regulations causes insurance payouts to fall by about 55%, or \$1.60 per \$1000 of coverage. At average coverage levels, this indicates that the increase in adaptation mandated by the regulations does indeed generate social value in damage reduction of about 4.2% of the average home value (using a 5% discount rate).<sup>21</sup>

**House prices are unchanged despite substantial changes in damages** Despite their higher social value, 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. This result confirms existing work documenting a wedge between social and private valuations of flood risk in our setting. Appendix A.4.4 shows in a stylized model that a zero effect on price implies zero willingness-to-pay for reduced risk if all other consumer-welfare-relevant aspects of houses remain constant. The null effect we find on price is not

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<sup>21</sup>Using the elevation first stage estimate of 0.27, for a particular elevated house the PDV of savings is 15.6% of average house value.

driven by a failure of insurance prices to reflect changes in risk, as premiums for adapted homes incorporate 75% of the reduction in risk (Figure 5c).

The fact that safe houses provide no private value to consumers indicates the presence of frictions, including behavioral frictions such as risk misperception or myopia, or moral hazard from consumer expectations of government aid in case of disaster (or both). Regardless of the source, this wedge between private and social value would yield construction of inefficiently-risky houses, absent regulation.

## 5 An Equilibrium Model of the Housing Market

Our reduced-form results yield four facts. First, floodplain regulation suppresses construction in high-risk areas. Second, the policy’s elevation mandate reduces flood risk. Both of these margins indicate that floodplain regulations potentially decreased flood damages via both reallocating development and mandating safer building. Third, the costs of mandating adaptation are substantial, as new development decreases substantially inside regulated areas without a commensurate price decrease. Fourth, our setting exhibits a wedge between private and social valuations for flood safety, yielding the potential for the policy to improve welfare.

However, these results alone are insufficient to quantify the total effect of the policy on either damages or overall welfare. The policy’s effects depend critically on the location of counterfactual development: both risk and amenities vary across space. Additionally, the large size of the regulated area — nearly one-third of all land in our sample is currently regulated as an SFHA — may introduce spillover effects on the unregulated locations. Higher demand and consequently higher prices outside the SFHA could mitigate the incentive to relocate and impose pecuniary externalities on unregulated consumers. Moreover, while our empirical results indicate that floodplain regulation shifts supply, we require a model to

quantify the regulation’s costs to developers and consumers. To account for counterfactual location and spillover effects, quantify the regulation’s costs, and compare welfare across current and counterfactual policies, we specify and estimate a model of residential choice and real estate development.

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: they measure how consumers and developers trade off home prices and living or building in a regulated area. Both of our empirical exercises inform our model. We use our event study estimates that consumers are unwilling to pay for adaptation to simplify the model: consumers choose only among differentiated locations and developers do not endogenously supply adapted homes. We use our cross-boundary changes in prices and quantities from our boundary discontinuity analysis as moments to estimate the model’s key parameters. Using these estimated parameters, we then simulate the housing market to quantify the effect of factual and counterfactual policies on new construction, home prices, expected damages, and welfare.

## 5.1 Residential choice

Motivated by our result in Section 4.2 that consumers are indifferent between adapted and non-adapted homes, we model consumers as choosing among uniform homes across differentiated locations. We further assume that consumers do not privately value differences in flood risk, consistent with prior work and with our results in Section 4.2.

Each individual  $i$  makes a discrete choice of where to live within market  $m$ , which we take to be a county.<sup>22</sup> Locations are differentiated goods characterized by tract  $j$  and SFHA designation status  $z$ . Census tracts are small geographic units of analysis: the average

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<sup>22</sup>In Florida, counties are large but tend to only contain one major city and commuting zone.

county in Florida has 63 census tracts, each containing roughly 1,600 residential structures. Following the standard discrete choice framework of Berry et al. (1995), the indirect utility of individual  $i$  living in location  $jz$  is given by:

$$u_{ijz} = \alpha^D p_{jz} + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz} \quad (4)$$

where  $p_{jz}$  is the log price of housing in location  $jz$ ,  $\phi_1 C_z$  indicates financial and non-financial costs of living in an area designated as a floodplain,  $\xi_{jz}$  are unobserved amenities, and  $\varepsilon_{ijz}$  is an i.i.d. preference shock. Non-financial costs of living in a floodplain incorporated in  $\phi_1 C_z$  can include the hassle cost of regulations, such as the requirement to document flood insurance purchase when initiating a mortgage, or anxiety around future flood risks that is introduced by the high-risk label.

Because consumers purchasing a house may face information frictions, for example underestimating a house's flood risk, we allow for the possibility that SFHA designation may debias consumers. Following Allcott and Taubinsky (2015), we account for this by allowing SFHA designation to impact decision utility but not experienced utility. We model consumers as purchasing houses following a decision utility of

$$u_{ijz} = \alpha^D p_{jz} + \phi_1 C_z + \phi_2 B_z + \xi_{jz} + \varepsilon_{ijz} \quad (5)$$

where we have introduced  $\phi_2 B_z$  as a potential channel for floodplain designation to affect beliefs about flood risk. Modeling the debiasing effect of the SFHA as an on-or-off label 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 real costs  $C_z$  and debiasing labels  $B_z$  since both vary with floodplain status. We therefore combine the real cost term  $C_z$  and debiasing effect term  $B_z$

into an overall “floodplain effect” term  $SFHA_z$ .

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

$$u_{ijz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz} + \varepsilon_{ijz} \quad (6)$$

where  $p_{jz}$  is the log price of housing in location  $jz$ ,<sup>23</sup>  $SFHA_z$  indicates whether  $z$  is regulated as a floodplain,  $\xi_{jz}$  are unobserved amenities, and  $\varepsilon_{ijz}$  is an i.i.d. preference shock. We assume  $\varepsilon_{ijz}$  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  $\varepsilon_{ijz}$  is distributed EVT1, the fraction of individuals in a county choosing to live in location  $jz$  is:

$$s_{jz} = \frac{\exp(\delta_{jz})}{\sum_{j' \in J_m, z' \in \{0,1\}} \exp(\delta_{j'z'})} \quad (7)$$

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

## 5.2 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

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<sup>23</sup>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.

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  $\eta_{jz}^t$ , which also varies by location and time and increases by a constant amount  $\psi$  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

$$D_g^t = 1\{p_{jz}^t \geq c_g + \psi E_{jz}^t + \eta_{jz}^t\} \quad (8)$$

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  $t$ . The total share developed at the end of time  $t = 1$  is therefore  $\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right)$ .

### 5.3 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:  $L_{jz}\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right) = Q_{jz} = N_m s_{jz}$  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.

## 5.4 Estimation and Results

A key challenge for estimation arises from the fact that amenities  $\xi_{jz}$  and construction costs  $\eta_{jz}$  may be correlated with floodplain status  $SFHA_z$ .<sup>24</sup> The correlation between  $SFHA_z$  and  $\xi_{jz}$  and  $\eta_{jz}$  occurs because SFHA status may be correlated with other amenities, like coastal access, or challenges to construction, like wetlands. We use a variation of our spatial discontinuity assumption to account for the endogeneity between SFHA status and unobserved amenities ( $\xi_{jz}$ ) and construction costs ( $\eta_{jz}$ ) to estimate  $\psi$  and  $\phi$ . We describe the estimation procedure in more detail below.

### 5.4.1 Demand-side identification

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

$$\ln(s_{jz}) - \ln(s_{0m}) = \delta_{jz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz} \quad (9)$$

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

$$\hat{s}_{jz} = \frac{Q_{jz}^{2016}}{\sum_{j' \in J_m, z' \in \{0,1\}} Q_{j'z'}^{2016}} \quad (10)$$

where  $Q_{jz}^{2016}$  is the total amount of developed land in geography  $jz$ .

Simply estimating Equation 9 via OLS would yield a biased estimate of the regulatory demand cost  $\phi$  because of the correlation between  $SFHA_z$  and amenities  $\xi_{jz}$ . We address this

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<sup>24</sup>Another important source of endogeneity is the correlation between  $p_{jz}$  and  $\xi_{jz}$  and  $\eta_{jz}$ , as locations with higher amenities and higher construction costs have higher prices in equilibrium. However, because we calibrate supply and demand elasticities rather than estimate them in-sample, we avoid this additional estimation challenge.

challenge in our boundary discontinuity analysis by identifying the effect of SFHA designation in the limit on either side of the boundary. We therefore estimate our model of housing choice by matching the cross-boundary differences we document in the descriptive analysis. First, we restrict to a narrow window around the boundary (100 feet). Because even this narrow window does not capture differences at the limit, we then separate the unobserved amenity term  $\xi_{jz}$  into a nuisance term, which can be correlated with SFHA status, and a component of  $\xi_{jz}$  that is uncorrelated with SFHA status. We require that the price difference across boundaries net of the nuisance term match the price difference estimated in our spatial RD, denoted  $\beta^p$ .<sup>2016</sup> We specify our moment conditions in Appendix A.6.

We calibrate  $\alpha^D$  to values in the literature. In similar settings, authors estimating within-city location substitution elasticities have found values corresponding to our  $\alpha^D$  in the range of -0.4 to -3.3 (Calder-Wang, 2021; Song, 2021).<sup>25</sup> The model in Song (2021) is of the most similar granularity to ours and estimates a value for  $\alpha^D$  ranging from -0.9 to -0.99, so we calibrate  $\alpha^D$  to be -1.<sup>26</sup>

**Discussion** This estimation strategy identifies the floodplain effect on demand  $\phi$  close to the floodplain boundary, but assumes the cost of regulation is identical across all regulated areas. 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 have imposed the assumption that consumers do not respond to expected flood damages. If this assumption is incorrect, any increases in flood expenses to which consumers *do* react will load onto the SFHA term if they change discretely at the boundary (e.g. through insurance premiums) or onto the unobserved amenity terms  $\xi_{jz}$  if they do not change discretely at

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<sup>25</sup>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.

<sup>26</sup>In Appendix Table A.6 we present a set of results in which we estimate  $\alpha^D$  using variation in price in our sample, which yields a value of approximately -0.8.

the boundary. Underestimating consumer responsiveness to risk would lead us to overstate the gains from regulation in our normative analysis, but would not impact any conclusions about the positive effects of the policy on construction, damages, or house prices.

### 5.4.2 Supply-side identification

We assume that the SFHA only affects supply by requiring houses to be elevated, so we employ a similar strategy to identify the elevation cost term  $\psi$  as with the demand cost term  $\phi$ . We again restrict to land within 100 feet of the boundary and match the spatial RD estimates of the SFHA effect to the cross-boundary differences in share developed, net of the component of  $\eta_{jz}$  that is correlated with amenities. We match the cross-boundary difference in share developed in both the pre-period ( $\beta^{q,1980}$ ) and the post-period ( $\beta^{q,2016}$ ). Details on the moment conditions we use can be found in Appendix A.6.

We also construct moments to match recent estimates of tract-specific supply elasticities from Baum-Snow and Han (2022) to obtain estimates of supply parameters  $\mu_j$  and  $\sigma_j$ . These supply elasticities are estimated in the same locations as our setting, and we view them as accurately identifying both the magnitude of and spatial heterogeneity in the slopes of the housing supply curves in our sample. Using these estimates allows us to capture realistic patterns of within-market heterogeneity in supply elasticities, e.g. that housing supply in urban, coastal areas is likely much more inelastic than inland, suburban or rural areas.

### 5.4.3 Results

After calibrating  $\alpha^D$  and matching  $\mu_j$  and  $\sigma_j$  to estimates from Baum-Snow and Han (2022), we estimate the SFHA cost terms  $\phi$  and  $\psi$  jointly via the Generalized Method of Moments using the boundary discontinuity moments described above and detailed in Appendix A.6. Appendix A.7 includes more details about the data restrictions used for estimation.

**Model fit** While our structural error terms allow our model to achieve a perfect fit to the observed data, we can assess the model by investigating the extent to which we rely on structural error terms to achieve that fit. In Figure 6 we plot observed price in 1980 and 2016 against the modeled prices that would rationalize observed market shares in each year, *if we omitted the structural error terms*. This exercise does not use our estimated demand model at all; it assesses the role of supply structural error terms 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.<sup>27</sup> Standard errors from bootstrapping with 100 replications are given in parentheses. Large standard errors are due to small sample size, a result of the cuts made when restricting to census tracts with sufficient data. We focus on the point estimates. As expected, SFHA status is disliked by consumers (negative  $\phi$ ) and imposes costs on developers (positive  $\psi$ ). 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 and it costs developers 25% more in construction costs to build a compliant home. We arrive at these numbers by rationalizing the changes in quantity and price around the regulatory boundary with estimates of how consumers trade off home prices with other attributes (our calibrated  $\alpha^D$ ) and the elasticity of housing supply (which is determined by  $\mu_j$  and  $\sigma_j$ ).

The result that consumers are willing to pay 23% more to avoid floodplain regulations is at the high end of a range of recent estimates, which find floodplain discounts ranging from 0 to 28% (Indaco et al., 2019; Gibson and Mullins, 2020; Hino and Burke, 2021; Lee, 2022). This difference could be attributed to our time period and setting: residents living in flood-prone Florida today may be particularly sensitive to signals of risk as awareness of climate change

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<sup>27</sup>Appendix Table A.6 presents parameter estimates under alternative calibrations of demand elasticity  $\alpha^D$ .

grows (Bernstein et al., 2019). Nevertheless, a 23% premium on avoiding the floodplain far exceeds the risk-reducing benefits of relocation: Appendix Table A.5 indicates that the risk differences across boundaries are equivalent to 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, which are substantial.<sup>28</sup> Alternatively, the floodplain designation may cause consumers to over-update their beliefs about risk, as flood maps do not communicate risks in terms of expected damages.

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.<sup>29</sup> 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. Yet, our informal interviews indicated that a 25% increase in costs is reasonable. For example, the elevation requirement may necessitate a stem wall, which can add \$100,000 to the cost of a new build. However, as described in Section 4.2, elevation reduces damages by about 16% of house price, which is less than the estimated cost of elevating.

Taking our results together, we conclude that the SFHA regulations substantially shift both demand and supply curves inwards. The large costs of regulation to both consumers and developers implied by our boundary discontinuity results indicate that the policy imposes substantial burdens on consumers living in and developers building in flood zones. Back-of-the-envelope comparisons of estimated costs to risk-reducing benefits reveal that the costs may outweigh the benefits of the policy. We explore this possibility rigorously by quantifying the total benefits and costs of the regulation in the following section.

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<sup>28</sup>In particular, non-adapted houses inside the floodplain must be elevated if they undergo substantial renovation. In an interview, a Florida developer said this rule burdens floodplain homeowners, whose remodeling plans must either include unwanted and expensive elevation or be spread out over several years to avoid triggering the rule.

<sup>29</sup>Construction costs include both the land on which a building is built and labor and materials costs.

## 6 Model-Informed Estimates of The Effects of Floodplain Regulation

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 searching for a price vector  $\tilde{p}_{jz}^{2016}$  that equates:

$$L_{jz}\Phi\left(\frac{\tilde{p}_{jz}^{2016} - \hat{\psi}E_{jz}^{2016} - \mu_j - \eta_{jz}^{2016}}{\sigma_j}\right) = N_m \frac{\exp(\alpha^D \tilde{p}_{jz}^{2016} + \hat{\phi}SFHA_z + \xi_{jz})}{\sum_{j' \in J_m, z' \in \{0,1\}} \exp(\alpha^D \tilde{p}_{j'z'}^{2016} + \hat{\phi}SFHA_{z'} + \xi_{j'z'})} \quad (11)$$

given our calibrated and estimated parameters  $(\alpha^D, \hat{\mu}_j, \hat{\sigma}_j, \hat{\phi}, \hat{\psi})$  and our recovered values of  $\xi_{jz}$  and  $\eta_{jz}$ . 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 remains constant across counterfactual scenarios.<sup>30</sup>

First, we quantify the magnitude of flood-risk reduction the policy achieves – its intended goal. We also decompose the risk reduction into components due to relocation to safer areas versus adaptation in risky areas. In addition to our model of housing supply and demand, this risk quantification exercise crucially relies on our hydrological-model-based estimates of flood risk by location and our estimates from Section 4.2 on the risk-reducing benefits of adaptation.<sup>31</sup> Appendix A.8 describes how we compute expected damages in further detail.

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<sup>30</sup>We estimate the parameters on 11 counties but encountered difficulties resolving the equilibrium in the no-SFHA counterfactual for Miami-Dade county. Therefore, we omit it in these counterfactuals.

<sup>31</sup>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. 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). Appendix A.8 describes how we use the First Street data to calculate expected damages.

Quantifying the risk-reducing benefits of the policy does not require a normative welfare framework. In Section 6.2, we will use our model normatively to quantify the costs of the regulation to consumers and to calculate the welfare effects of regulation.

## 6.1 The Effect of Regulation on Flood Damages

Our quantification of the policy’s effect on flood damages focuses on two main outcomes. First, we study the approximate number of houses in the regulated area (observed SFHAs).<sup>32</sup> This analysis extrapolates the effect of our regression discontinuity away from the boundary to examine the extent to which floodplain regulation reduces building in regulated areas market-wide. Because regulated areas are on average riskier than unregulated areas, shifting development from regulated to unregulated areas will likely reduce damages, but the magnitude is uncertain and depends on the joint distribution of amenities and flood risk. Our second primary outcome therefore quantifies the reduction in damages directly, including both relocation and adaptation. To complete our analysis of the positive effects of the regulation on housing markets, we also report average home prices inside and outside of regulated areas with and without the policy.

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. 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 or other hassles of living in the flood zone. Under this policy regime, there are no differences in insurance prices across areas.

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<sup>32</sup>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).

Table 5 presents results. Relative to our no-SFHA counterfactual, floodplain regulation reduces the number of homes built in regulated areas by almost 16%, reallocating over 117,000 houses. Note that this effect (a 15.5% reduction in new development) is smaller than our corresponding boundary discontinuity estimate (an 18% reduction). This is due to two facts. First, our model accounts for the market-wide distribution of unobserved amenities. At the boundary, amenities are identical so substitution from the regulated to unregulated area is more likely. At the market level, amenities are correlated with regulation status so substitution will be less elastic, making the boundary discontinuity estimate an over-estimate of the total effect. Second, because such a large share of Florida is at risk of flooding, regulations have market-wide price effects that undermine the effect of the policy. As Table 5 illustrates, prices outside of the regulated area increase as consumers substitute to unregulated areas.

The policy achieves its goal of reducing flood damages via both relocation and adaptation. Overall, the policy reduces the present discounted value of damages by \$4,053 per newly-developed house, which amounts to a 29% reduction in damages. Relocation contributes one-third of that effect, or a 9% reduction in overall expected damages, while the requirement to adapt houses contributes an additional 20% reduction in damages. The policy's benefits in terms of reduced damages are substantial, both in absolute numbers (a decrease in \$1.8 billion of expected damages per county), and in comparison to alternative policy instruments: the number of homes relocated exceeds the total number of houses removed from risky areas by the NFIP's home buyouts program across the entire nation over its lifetime (Frank, 2020a).

The above findings suggest that floodplain regulation generates its intended social benefits in the form of substantially-reduced damages. But reduced damages should not be analyzed in isolation. Our empirical evidence indicates that the regulation also imposes costs on suppliers, through mandatory elevation, and consumers, through relocation to less-preferred areas. A key question is to what extent the costs of these housing market distortions offset

the policy's benefits.

## 6.2 The Effect of Floodplain Regulation on Welfare

In this section, we develop a welfare framework that allows us to quantify the policy's costs and compare these costs against the estimated benefits discussed in Section 6.1. Because it is possible that the SFHA both imposes real costs and debiases consumers, we must distinguish between decision and experienced utility when evaluating consumer welfare. As discussed in Section 5, the experienced utility of a consumer choosing housing in location  $jz$  is given by:

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

Though consumers do not incur private costs of flood risk, the social planner values these costs. We therefore define the social welfare function as:

$$\Sigma_{jz} N_{jz} (\alpha^D \ln(P_{jz} + D_{jz} + R) + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz}) \quad (12)$$

where  $P_{jz}$  is a location's price in levels,  $D_{jz}$  is the PDV of a location's expected damages, and  $R$  is any revenue generated by the policy that is lump-sum transferred back to all consumers. Recall that our estimated  $\phi = \phi_1 + \phi_2$ , i.e. it captures the combination of real costs imposed by the regulation ( $\phi_1$ ) and a debiasing effect ( $\phi_2$ ) that does not enter social welfare. Because we only estimate the combined effect  $\phi$ , we consider the spectrum of possible welfare effects by calculating welfare under two polar cases: (a)  $\phi_1 = \phi$  ( $\phi$  is 100% is experienced costs) or (b)  $\phi_1 = 0$  ( $\phi$  contains only terms that do not affect welfare).

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 by comparing consumer surplus and estimated damages under the observed regulation and the unregulated benchmark. Even if the floodplain demand cost  $\phi$  entirely captures a debiasing channel (i.e. the policy imposes no costs to consumers except for distorting their location choices), we estimate that the policy reduces consumer surplus by more than the social benefits it generates. This is further reinforced by additional deadweight loss induced by the non-financial costs of the policy if  $\phi$  is not entirely debiasing. Notably, we find that the policy does not improve welfare despite a setting which is maximally-favorable to intervention: consumers have no private value for flood safety.

Our results indicate that current regulations are over-corrective. Though risk would be inefficiently-high in the absence of any policy intervention because of the wedge between the private and social value of flood risk, overly-expensive mandated adaptation and a strong consumer response to the flood zone label yields *too little* construction in risky areas. Housing in risky areas that would have improved social welfare is not built because of mandatory costly house elevation.

Of course, our final welfare estimates necessarily embed a number of assumptions, though we do not believe these assumptions change our qualitative conclusions. Most importantly, we have assumed that the policy is fully corrective, i.e. consumers are unwilling to pay at all to reduce their flood risk. Relaxing this assumption will only reinforce our finding that the policy does not improve welfare. Second, 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. A back-of-the-envelope calculation suggests that emotional costs of flooding would have to exceed six times the financial losses of flooding to change the sign of the welfare effect. Our qualitative conclusions about the welfare impacts of the policy also remain unchanged if we incorporate additional flood damages to public goods (that are financed by the government, not the consumer).

Our results highlight an important policy lesson. Some policy proposals (e.g. those discussed by Frank (2020a)), focus purely on the savings that could be achieved by reducing risk. Our results highlight the economic costs of making policies with such a framework. Increasing barriers to building in risky areas may crowd too many consumers into unregulated parts of the market, eroding housing affordability and reducing efficiency. Our results underscore the gains to transitioning from a blunt attribute-based regulation to a more flexible policy instrument, such as a tax or an actuarially-fair insurance mandate. In fact, the NFIP is indeed transitioning to a more flexible pricing system on the demand side (Risk Rating 2.0), though the costly adaptation mandate will remain.

## 7 Conclusion

For over 40 years, federal floodplain regulations have influenced housing markets, with limited evidence on either their costs or 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 spatially-granular model of the housing market to investigate the policy’s equilibrium effects on the location of construction, housing prices, expected damages, and welfare.

We find that local to the regulatory boundary, floodplain regulation decreases development and increases house prices. We also find that flood-safe building codes reduce flood damages by about 4% of housing value, but these reductions are not privately valued by consumers. Using our model to interpret our reduced-form results, we find that floodplain regulation in Florida reduces damages by 30%, with one-third of the effect accruing through relocation and the remaining two-thirds through adaptation. Though the policy achieves substantial benefits in terms of flood damage reductions, it is over-corrective: both channels — relocation and elevation — impose costs exceeding their benefits. The regulations reduce development

in risky areas more than would be socially efficient, so much so that we estimate that the status quo policy reduces welfare relative to an unregulated benchmark.

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. They suggest that status quo policy instruments may perform substantially worse than flexible instruments that allow consumers who are willing to pay the full social cost of living in risky areas to do so.

We leave a few areas open for future work. Though we model flood risk as static, in reality flood risk is increasing over time. Regulating durable building with evolving flood risk may pose interesting dynamic considerations. Furthermore, although we provide suggestive evidence of frictions in the market for flood safety, we leave the challenge of identifying the welfare costs of each separate friction to future work.

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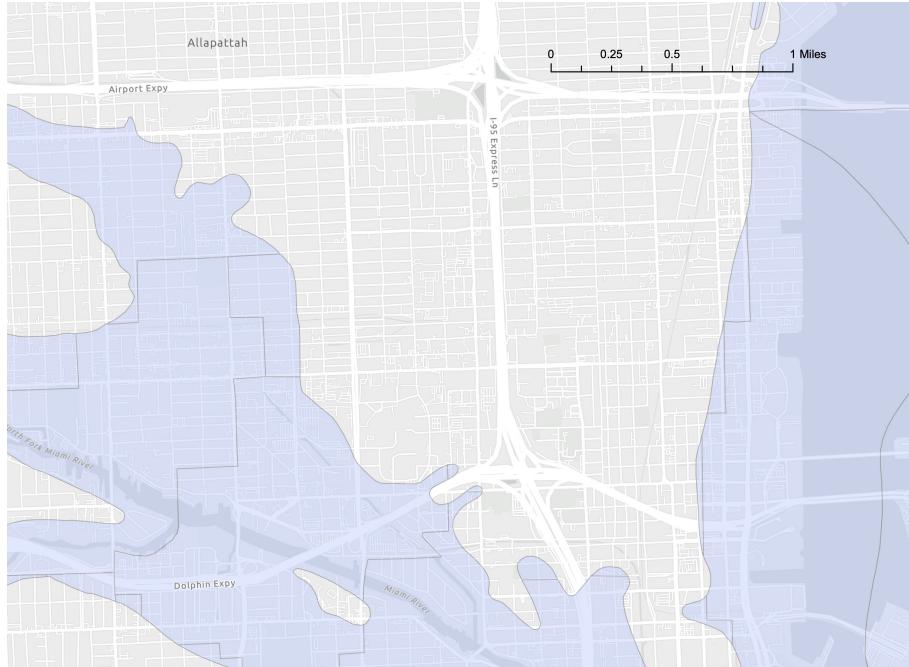
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## Tables and Figures

Figure 1: Digitized Flood Maps

(a) Digitized Map of Miami (1978)



(b) Coverage of Digitized Maps

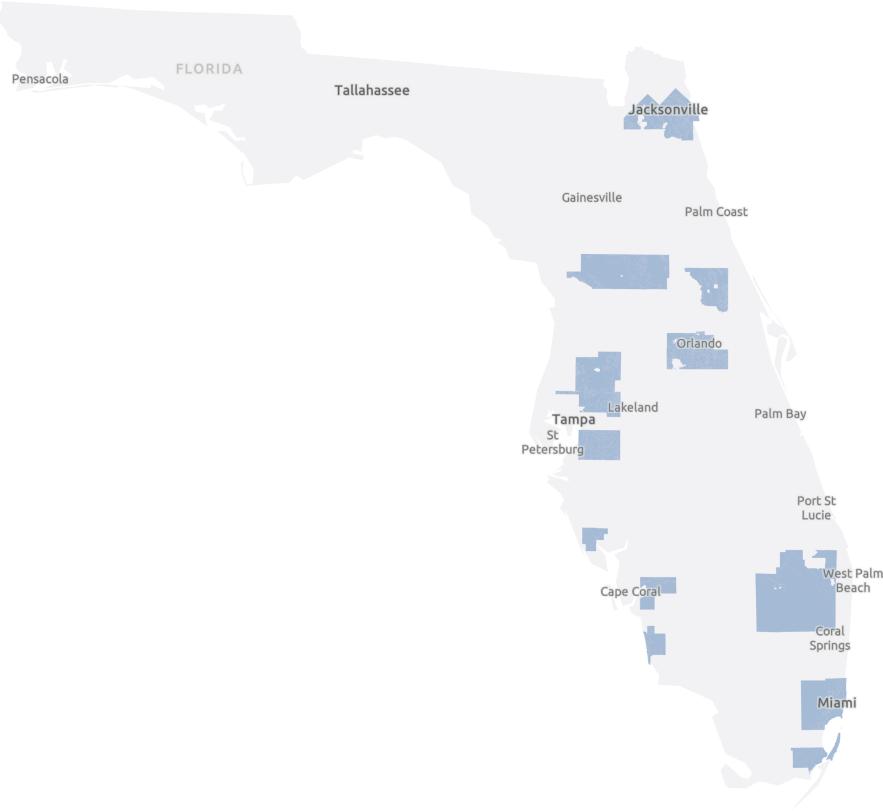
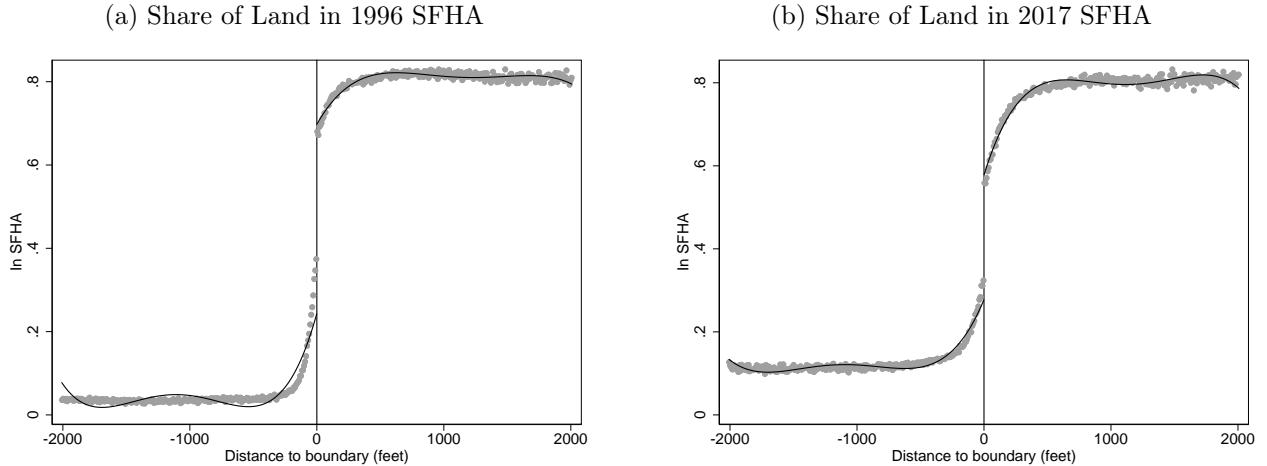
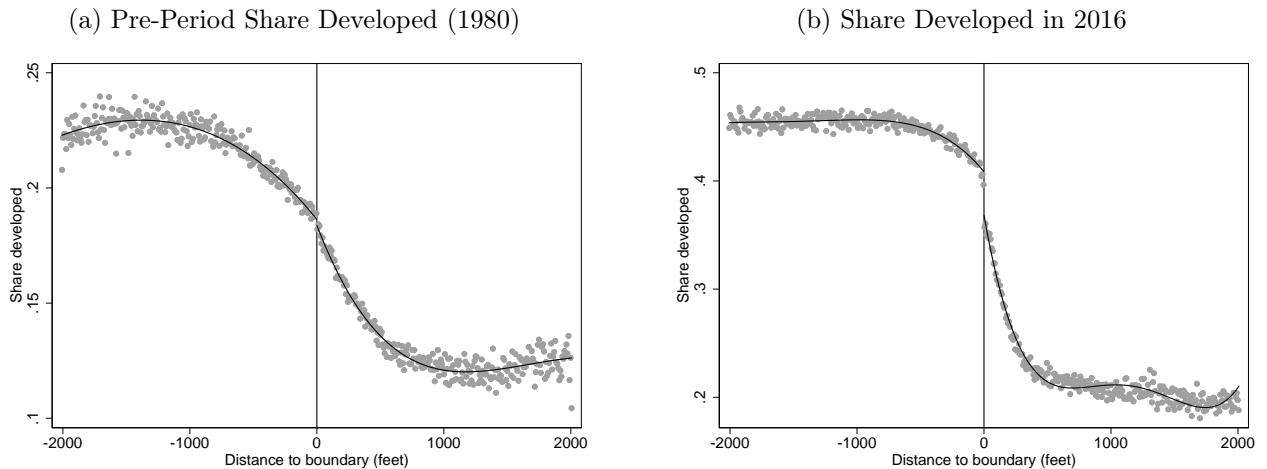


Figure 2: RD Estimates: Current SFHA Status



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



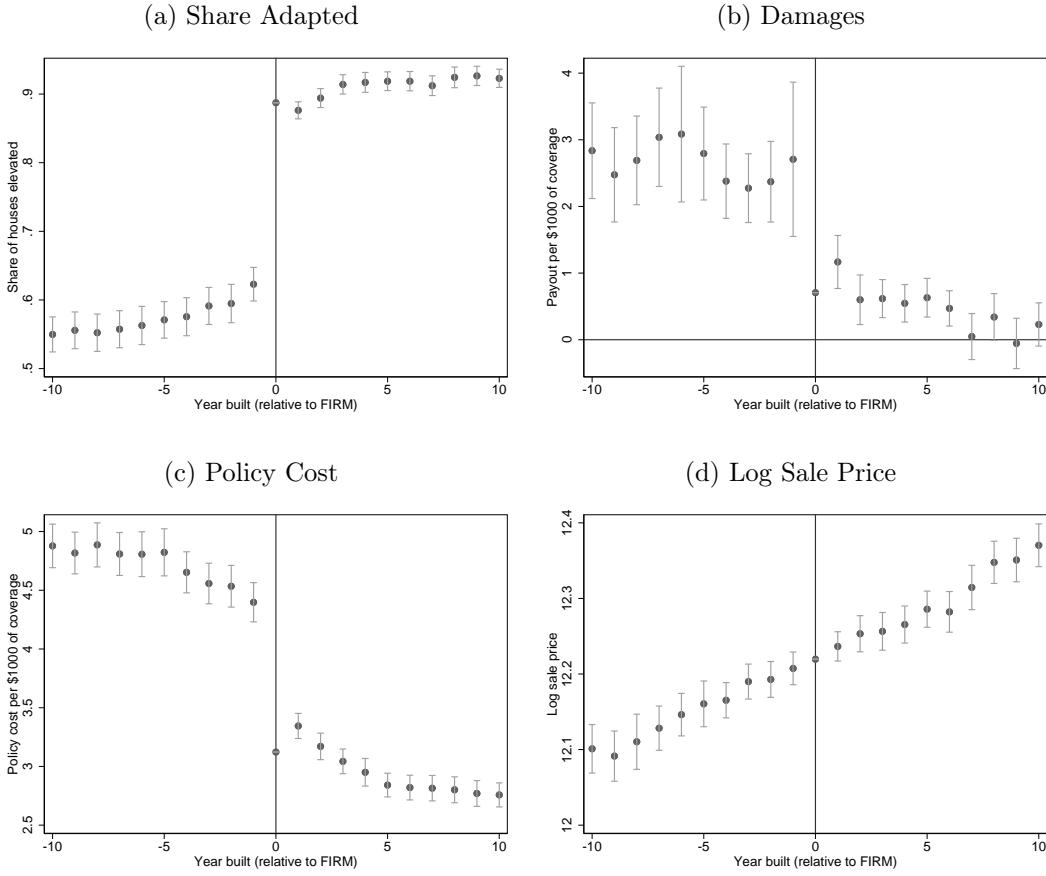
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary measured in feet, with positive distance indicating being inside the SFHA. Sub-figure (a) plots the share of land that was developed as of the late 1970s and early 1980s, and sub-figure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

Figure 4: RD Estimates: Prices



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.

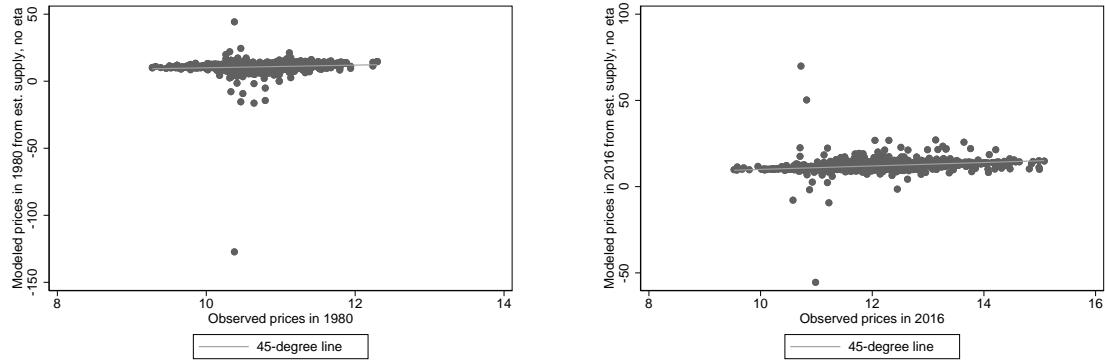
Figure 5: NFIP Enrollment Year Event Study Estimates



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  $(\mu_j, \sigma_j, \psi)$  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	Boundary sample		
	(1)	(2)	(3)	(4)	(5)
<b>Panel A. Development</b>					
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
<b>Panel B. Other characteristics</b>					
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
<b>Panel C. Prices</b>					
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
<b>Panel D. Risk</b>					
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 (1)	Rectangular kernel, constant band- width (2)	Triangular kernel, optimal band- width (3)	Local linear regression Polynomial excl. coastal areas (4)	Polynomial (5)
<b>Panel A. Current SFHA status</b>						
SFHA as of 1996	0.060	0.430 (0.020)	0.405 (0.019)	0.268 (0.014)	0.454 (0.024)	
SFHA as of 2017	0.130	0.292 (0.019)	0.283 (0.017)	0.207 (0.013)	0.300 (0.023)	
<b>Panel B. Historical land use</b>						
Share of land developed in 1980	0.235	-0.003 (0.002)	-0.005 (0.002)	-0.006 (0.002)	-0.003 (0.002)	
<b>Panel C. Modern land use</b>						
Share of land developed in 2016	0.470	-0.042 (0.005)	-0.044 (0.005)	-0.037 (0.004)	-0.040 (0.006)	
Share of land covered by a building footprint	0.264	-0.029 (0.004)	-0.026 (0.004)	-0.025 (0.004)	-0.027 (0.004)	
Share of land covered by a single family home	0.101	-0.015 (0.003)	-0.013 (0.002)	-0.010 (0.003)	-0.013 (0.003)	
Share of land covered by wetlands	0.098	0.110 (0.007)	0.096 (0.007)	0.071 (0.006)	0.113 (0.009)	
<b>Panel D. Prices</b>						
Log house price	12.151	0.065 (0.022)	0.063 (0.021)	0.066 (0.019)	0.049 (0.026)	
Log single-family house price	12.073	0.056 (0.018)	0.060 (0.018)	0.064 (0.015)	0.051 (0.021)	
Log vacant land price (per 30X30m pixel)	8.806	-0.102 (0.048)	-0.062 (0.032)	-0.070 (0.030)	-0.126 (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 (0.013)	0.271 (0.019)	0.335 (0.019)
Insurance payouts (per \$1000 of coverage)	\$2.94 (0.40)	\$-1.61 (0.96)	\$-2.84 (0.96)
Policy cost (per \$1000 of coverage)	\$4.80 (0.075)	\$-1.21 (0.127)	\$-1.78 (0.127)
Log house price (sold 2005-2020, in 2010 \$USD)	12.21 (0.009)	-0.007 (0.011)	0.004

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 ( $\psi$ )	0.246 (0.296)
Demand cost of SFHA ( $-\phi$ )	0.230 (0.491)
Demand elasticity ( $\alpha^D$ )	-1.0
Consumer WTP to avoid SFHA ( $\phi/\alpha^D$ )	0.230
Estimating $\alpha^D$ ?	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	No SFHA	Change relative to
	Modeled (1)	Modeled (2)	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 0.0%
Per-house PDV of location-based damages for new dev. (i.e. current avg annual loss, counterf. locations)	\$9,756	\$10,705	\$-949 -8.9%
Per-house PDV of adaptation-based damages for new dev. (i.e. current locations, counterf. avg annual loss)	\$9,756	\$12,270	\$-2,514 -20.5%
Per-house PDV of all damages for new dev. (i.e. counterf. locations, counterf. avg annual loss)	\$9,756	\$13,809	\$-4,053 -29.4%
Number of Elevated Houses	640,676	288,233	352,443 122%
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  $SFHAs_z = 0$  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		-60,934		-29,233
Damages	61,764	-18,129		-18,129
Total Welfare (CS - Damages)		-42,805		-11,105

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

# A Appendix

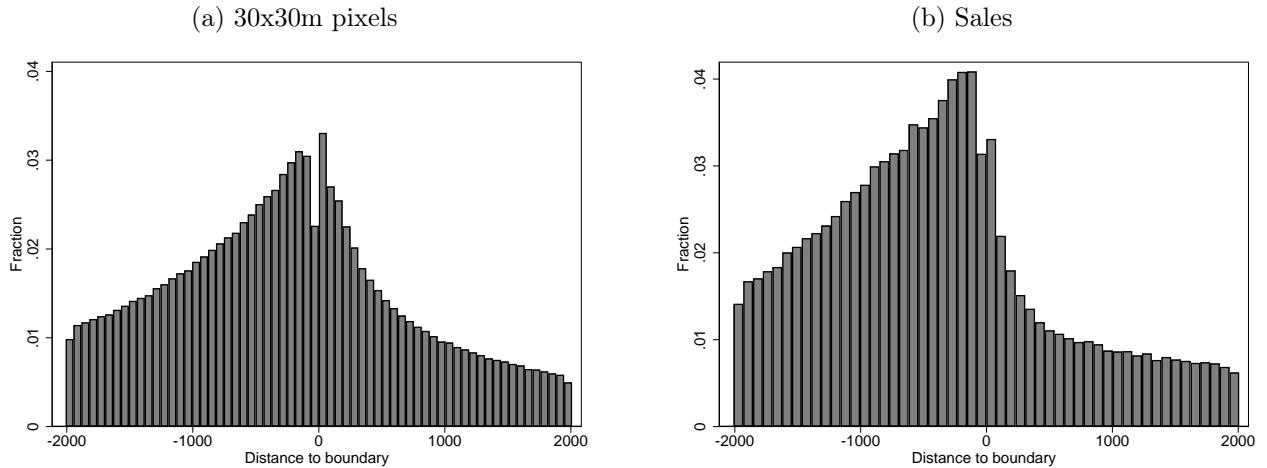
## A.1 Appendix Tables and Figures

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



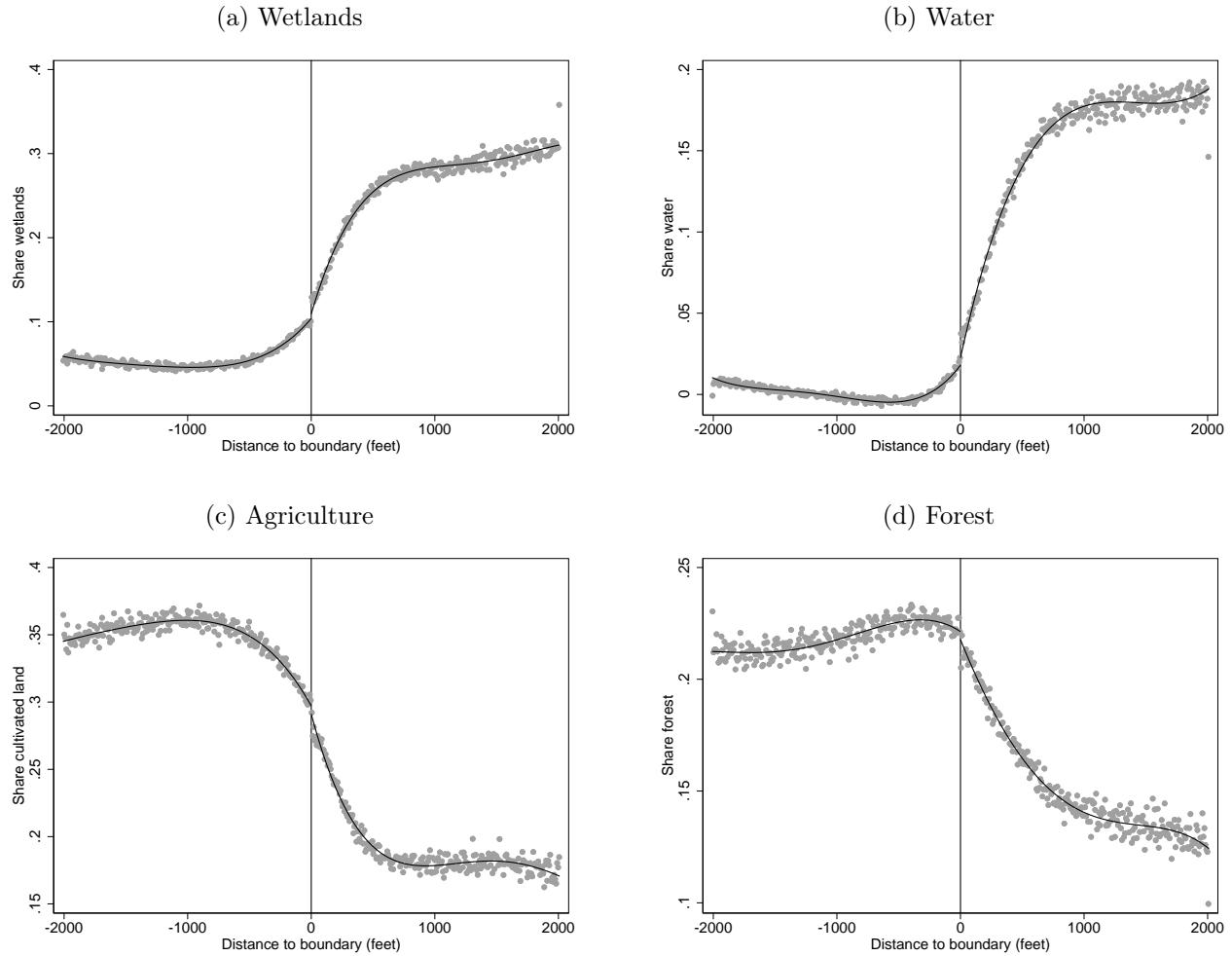
Notes: Figure shows an elevated house in Collier County, Florida. At this location, SFHA regulations require the bottom of the lowest (non-basement) floor to be elevated to 10 feet above ground level.

Figure A.2: Histogram of Distance to SFHA Boundary



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.

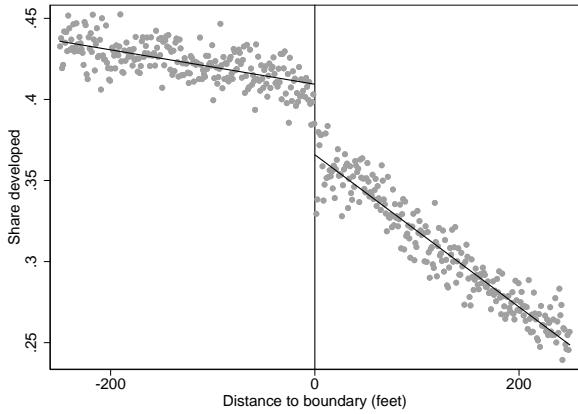
Figure A.3: RD Estimates: Other Pre-Period Land Use



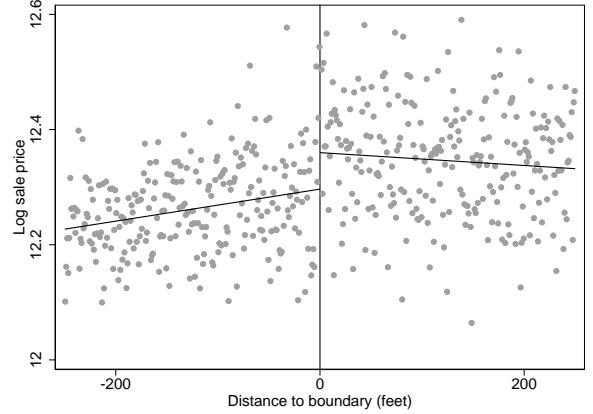
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the SFHA. All land use outcomes are measured as of the late 1970s and early 1980s. Estimates are residualized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

Figure A.4: RD Figures: Local Linear Specification

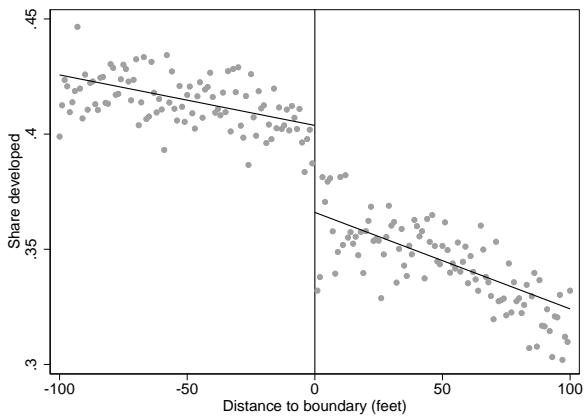
(a) Share Developed in 2016, Rectangular Kernel



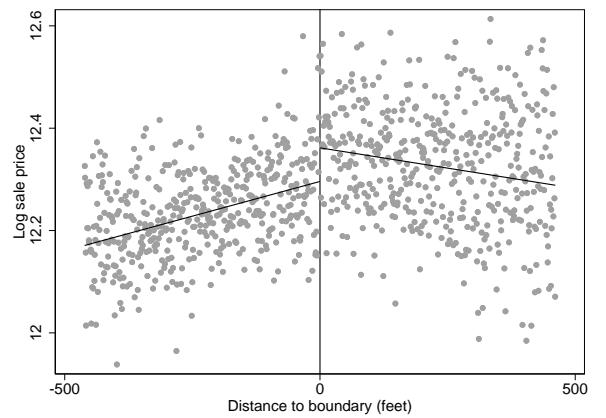
(b) Log Sales Price, Rectangular Kernel



(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

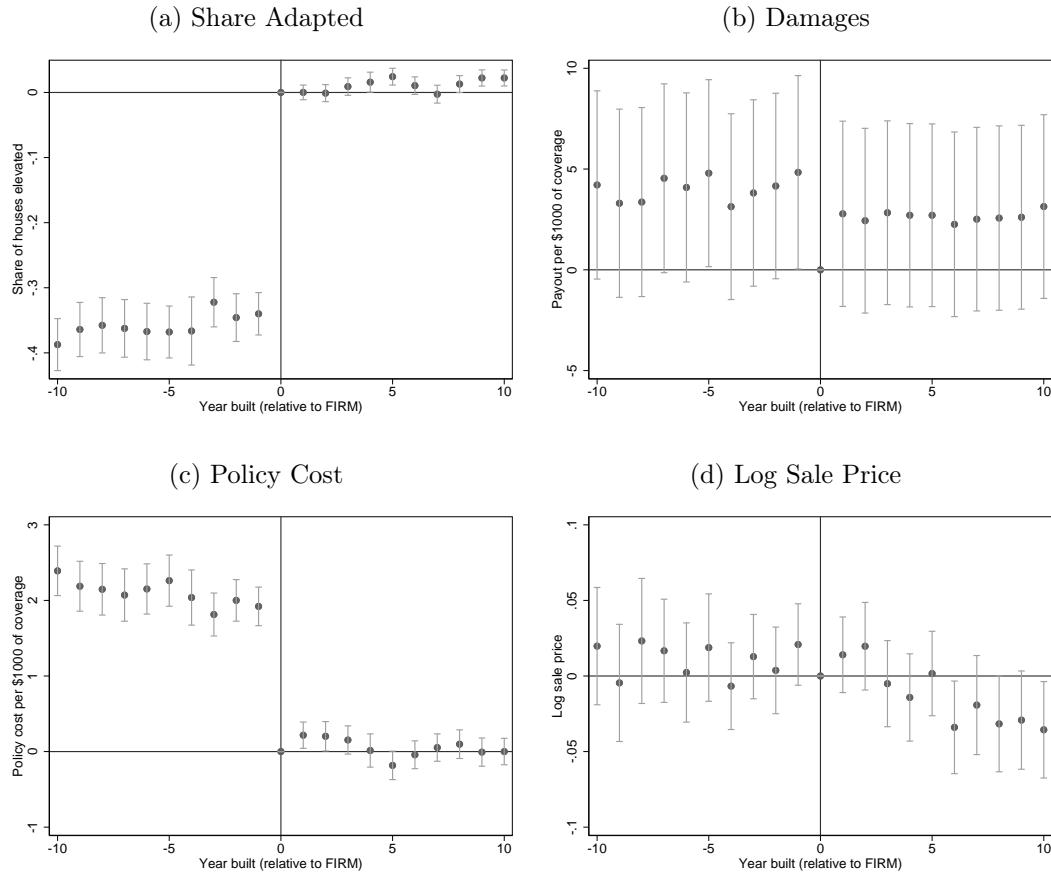
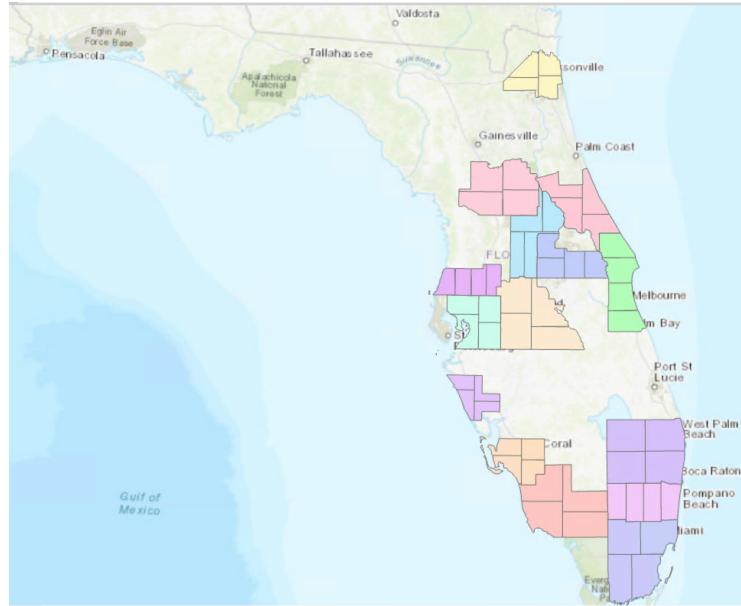


Figure A.6: Quadrants



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	Outside SFHA	Inside SFHA	Outside SFHA
	(1)	(2)	(3)	(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

	Polynomial		Local Linear (CCT bandwidth)	
	Pre-period	Current	Pre-period	Current
		(1)	(2)	(1)
Wetlands	0.005 (0.004)	0.110 (0.007)	0.019 (0.003)	0.071 (0.006)
Water	0.005 (0.003)	-0.020 (0.004)	0.012 (0.002)	0.001 (0.001)
Agriculture	-0.006 (0.004)	-0.029 (0.007)	-0.015 (0.003)	-0.021 (0.005)
Forest	-0.004 (0.003)	-0.009 (0.006)	-0.006 (0.003)	0.001 (0.004)

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
	Baseline	Residualized of character- istics	Homes built	Homes built	
			pre regulations (pre-FIRM)	post regulations (post-FIRM)	
	(1)	(2)	(3)	(4)	(5)
All houses	0.065 (0.022)	0.057 (0.067)	0.070 (0.030)	0.048 (0.021)	0.057 (0.021)
Single-family houses	0.056 (0.018)	0.093 (0.066)	0.103 (0.039)	0.038 (0.025)	0.024 (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		Outside SFHA	
	(1) Pre-Enrollment	(2) Post-Enrollment	(3) Pre-Enrollment	(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) Outside SFHA	(2) Inside SFHA	(3) Outside SFHA	(4) 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 ( $\psi$ )	0.246 (0.296)	0.246 (0.322)	0.246 (0.286)	0.246 (1.603)
Demand cost of SFHA ( $-\phi$ )	0.230 (0.491)	0.269 (0.572)	0.083 (0.159)	0.247 (0.813)
Demand elasticity ( $\alpha^D$ )	-1.0	-0.4	-3.3	-0.7 (1.041)
Consumer WTP to avoid SFHA ( $\phi/\alpha^D$ )	0.230	0.672	0.025	0.333
Estimating $\alpha^D$ ?	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.<sup>33</sup>

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.

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<sup>33</sup>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.<sup>34</sup> 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).<sup>35</sup> 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.

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<sup>34</sup>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.

<sup>35</sup>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 4.2 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:

$$y_i = \sum_r \beta_r 1\{r_i = r\} SFHA_i + \sum_r \gamma_r 1\{r_i = r\} + \delta SFHA_i + \gamma_{j(i)} + \varepsilon_i \quad (13)$$

$$y_i = \alpha + \beta Post_i SFHA_i + \gamma Post_i + \delta SFHA_i + \nu r_i + \eta r_i Post + \phi r_i SFHA_i + \psi r_i Post_i SFHA_i + \gamma_{j(i)} + \varepsilon_i \quad (14)$$

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 4.2 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 PDV 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 PDV 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  $\rho$  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),  $\alpha$  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  $\varepsilon_{iA}$  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  $\varepsilon_{iA}$  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 PDV of insurance payout savings and 33% of the PDV 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 PDV 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 (2022) (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  $\alpha_j^S$ .

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:  $\tilde{q}_j = q_j^{2016} - \alpha_j^S(0.1p_j^{2016})$ .<sup>36</sup> 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  $\mu_j$  and  $\sigma_j$  under the assumption that  $\eta_j = 0$  and  $E_j^{2016}$  is the elevation status in the tract in 2016. We compute  $\sigma_j$  and  $\mu_j$  as

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<sup>36</sup>We use 2016 price and quantity as our baseline because the BSH estimates used data from 2000-2010.

$$\sigma_j = \frac{-0.1 p_j^{2016}}{\Phi^{-1}(\tilde{q}_j) - \Phi^{-1}(q_j^{2016})} \quad (15)$$

$$\mu_j = p_j^{2016} - \psi E_j^{2016} - \Phi^{-1}(q_j^{2016})\sigma_j \quad (16)$$

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

## 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  $\Delta^D$ , 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:

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

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

$$E \left[ \frac{1}{\alpha^D} (\delta_{j1} - \delta_{j0} - \phi - (\xi_{j1} - \Delta^D - \xi_{j0})) - \beta^{p,2016} \right] = 0 \quad (17)$$

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

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

$$E[(\tilde{\xi}_{jz} - \mu_D)] = 0 \quad (19)$$

### 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  $\eta_{jz}^t = \Delta^{S,t} SFHA_z$  are a function of floodplain status, while the remainder  $\tilde{\eta}_{jz}^t = \eta_{jz}^t - \Delta^{S,t} SFHA_z$  are uncorrelated with floodplain status. In each period, for a given value of  $\Delta^{S,t}$ , we compute the “de-biased” share developed  $\Phi \left( \frac{p_{j1}^t - \psi E_{j1}^t - \mu_j - \tilde{\eta}_{j1}^t}{\sigma_j} \right)$ . 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:

$$E \left[ \Phi \left( \frac{p_{j1}^{1980} - \psi E_{j1}^{1980} - \mu_j - \tilde{\eta}_{j1}^{1980}}{\sigma_j} \right) - \Phi \left( \frac{p_{j0}^{1980} - \psi E_{j0}^{1980} - \mu_j - \tilde{\eta}_{j0}^{1980}}{\sigma_j} \right) - \beta^{q,1980} \right] = 0 \quad (20)$$

$$E \left[ \Phi \left( \frac{p_{j1}^{2016} - \psi E_{j1}^{2016} - \mu_j - \tilde{\eta}_{j1}^{2016}}{\sigma_j} \right) - \Phi \left( \frac{p_{j0}^{2016} - \psi E_{j0}^{2016} - \mu_j - \tilde{\eta}_{j0}^{2016}}{\sigma_j} \right) - \beta^{q,2016} \right] = 0 \quad (21)$$

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

$$E[(\tilde{\eta}_{jzb}^{1980} - \mu_{s,1980}) SFHA_z 1\{b = close\}] = 0 \quad (22)$$

$$E[(\tilde{\eta}_{jzb}^{2016} - \mu_{s,2016}) SFHA_z 1\{b = close\}] = 0 \quad (23)$$

$$E[(\tilde{\eta}_{jzb}^{1980} - \mu_{s,1980}) 1\{b = close\}] = 0 \quad (24)$$

$$E[(\tilde{\eta}_{jzb}^{2016} - \mu_{s,2016}) 1\{b = close\}] = 0 \quad (25)$$

## A.7 Details on Data for Model Estimation

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.<sup>37</sup> We measure elevation from NFIP policy data, as discussed 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  $\delta_{jz}$ , 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.

## A.8 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).<sup>38</sup>

Expected damages are computed as the product of number of newly-developed gridcells

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<sup>37</sup>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 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.

<sup>38</sup>See [https://assets.firststreet.org/uploads/2021/02/The\\_Cost\\_of\\_Climate\\_FSF20210219-1.pdf](https://assets.firststreet.org/uploads/2021/02/The_Cost_of_Climate_FSF20210219-1.pdf) for more details.

by the PDV 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.<sup>39</sup> 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  $\frac{1}{3} \times \$1.61 \times \frac{P_{jz}^{Obs}}{1000}$ .

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 PDV 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 PDV 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 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).

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<sup>39</sup>This 70% factor was recommended by First Street, who provided the underlying AAL data.

## A.9 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:

$$CS_i = \frac{-1}{\alpha^D} \ln \sum_{j \in J_m, z \in \{0,1\}} \exp(\alpha^D p_{jz} + \phi SFHA_{jz} + \xi_{jz}) \quad (26)$$

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  $\Delta P_m^{CF}$  such that

$$\sum_{j \in J_m, z \in \{0,1\}} \exp(\alpha^D p_{jz}^{NoSFHA} + \xi_{jz}) = \sum_{j,z} \exp(\alpha^D \ln(P_{jz}^{CF} + \Delta P_m^{CF}) + \phi SFHA_{jz} + \xi_{jz}) \quad (27)$$

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  $N_m$ :  $\Delta CS^{CF} = \sum_m N_m \Delta P_m^{CF}$ .