

# The Equilibrium Effects of Floodplain Regulation on Housing Markets

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## Abstract

High-risk floodplains cover a substantial and increasing share of the US housing market, and are heavily regulated to mitigate damages. We investigate the effect of floodplain regulation on the location of new construction, housing prices, expected flood damages, and consumer welfare in the state of Florida. Using a spatial regression discontinuity and an event study around elevation mandates, we show that regulation relocates new construction to safer areas and elevates houses. Although elevation reduces both damages and insurance premiums, consumers are not willing to pay more for elevated houses. We interpret this as evidence of a market friction potentially justifying intervention, implying an uncertain sign of welfare impacts. We quantify the policy's effects by estimating an equilibrium model of residential choices and housing supply, explicitly accounting for market-wide spatial heterogeneity and substitution across locations. We find that the policy reduces new construction in regulated floodplains by 16%. This relocation reduces expected future damages by 9%, or \$950 per

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newly-constructed house. The building code averts an additional 20% of damages, for a total reduction of \$1.8 billion per county. Yet, the policy reduces welfare: averted damages do not exceed consumer surplus losses from higher prices and relocation to less-desired places.

## 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). Despite these risks, demand for flood insurance is low (Kriesel and Landry, 2004; Kousky, 2018), plausibly because of moral hazard from the expectation of future government aid (Kydland and Prescott, 1977; Kousky et al., 2018b) and because of risk misperception (Bakkensen and Barrage, 2021; Gallagher, 2014; Wagner, 2021; Mulder, 2021). These same frictions may also reduce consumers' willingness to pay for safer houses, generating inefficiently-high development in risky areas. In the US, policymakers have responded to the threat of floodplain overdevelopment by demarcating "Special Flood Hazard Areas" (SFHAs) as especially risky parts of the floodplain. Inside the SFHA, some homeowners face a flood insurance mandate, flood insurance prices are higher, and developers must comply with building and permitting rules.<sup>1</sup>

This paper investigates the impact of floodplain regulation on the location of development, housing prices, estimated flood damages, and consumer welfare. While floodplain regulation is the primary policy instrument to regulate flood risk exposure and is a prevalent form of zoning regulation nationwide,<sup>2</sup> there is limited evidence on its welfare impacts. The policy could reduce damages by both shifting development to safer areas and requiring adaptation

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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).

<sup>2</sup>6% of properties are in an area designated as high-risk (First Street Foundation, 2020).

in high-risk locations. However, these responses also incur costs: homeowners move to less-preferred neighborhoods and developers expend labor and materials to meet higher building standards. Understanding the relative magnitudes of the benefits and costs of this policy is critical for understanding the welfare impacts of current and counterfactual policies.

We explore the impacts of floodplain regulation in the context of Florida, one of the most flood-prone and populous states in the US. We construct a rich dataset that pairs newly-digitized archival scans of Florida flood maps with granular satellite data on land use. We further add spatially-linked administrative data on parcel characteristics and transaction prices from the Florida Department of Revenue. We leverage National Flood Insurance Program (NFIP) policy and claims data to measure elevation status, insurance payouts, and policy costs. We supplement these sources with spatially-granular data on flood risk, produced by the nonprofit First Street Foundation.

We use two different empirical strategies — a spatial regression discontinuity and an event study around elevation mandates — to describe the policy’s effects under minimal assumptions. The spatial regression discontinuity design isolates the effect of SFHA designation by exploiting boundaries in flood maps first drawn in the 1970s and early 1980s. This design addresses omitted variables bias by leveraging the coarse classification of the early flood maps and the assumption that unobservable characteristics of the land evolve smoothly through the SFHA boundary. Historic maps are also immune to bias that arises from the tendency of landowners to deregulate the developed parts of their properties. Since maps are updated over time, this behavior produces a negative correlation between development and floodplain designation in modern flood maps. Using historic maps ensures our estimate of the effect of floodplain designation on development is unbiased. Separately, the event study, following Wagner (2021), exploits the discrete introduction of flood-safety building standards in the year that floodplain regulations first applied to an area. This approach addresses omitted variables bias when estimating the effect of flood risk on house prices by decoupling risk

from unobserved amenities.

Our reduced-form analyses indicate that floodplain regulation improves safety but imposes costs on developers and consumers. On the extensive margin, we document that new development just inside the floodplain is 9% lower than outside. Since the floodplain on average has higher risk, these results indicate that the policy relocates development to safer areas. Simultaneously, the policy reduces damages via adaptation: the regulation increases the share of houses that are elevated by 27 percentage points, and reduces the present discounted value of insurance payouts by 4.2% of the average house value. However, house prices suggest that the policy also imposes costs on both consumers and developers. If the policy only reduced housing demand in the floodplain, without affecting supply, we would find lower prices just inside the floodplain boundary. Instead, we see that house prices just inside the floodplain are 6% higher than outside. This indicates that supply costs of floodplain regulations counterbalance any demand disutility effects. Furthermore, the fact that prices inside the floodplain are higher than outside indicates that consumers are not completely elastic to price: some consumers have a sufficiently high value for particular within-floodplain locations that they are willing to live there even when they could purchase cheaper housing just outside the floodplain. This inelasticity of demand to price enables suppliers to pass on some portion of the floodplain regulation costs to consumers.

The event study also indicates that consumers are not willing to pay for safer houses, implying that the policy could improve welfare. The regulation would be purely distortionary if consumers were already choosing risk levels optimally. However, in the presence of a market friction, consumers might choose inefficiently-high risk levels. In this case, the regulation could potentially correct the market friction by mandating adaptation and formal insurance. We assess the extent of market frictions by comparing consumers' willingness-to-pay for elevated houses to the value of that elevation. If consumers fully internalized all flood risk, as the social planner would, elevation would increase house prices by the entire amount

it reduces damages. If, alternatively, consumers only partially internalized flood risk — whether because of subsidized premiums, the Samaritan’s Dilemma, or information frictions — they would be willing to pay only a fraction of the value of elevation. We find that although elevation reduces damages, and reduces premiums by an amount equal to 3.2% of average house value, it has no statistically-significant effect on house prices. We conclude that consumers completely fail to internalize flood damages, and even fail to internalize any of their own premiums. This behavior is consistent with both information frictions and the Samaritan’s Dilemma. The Samaritan’s Dilemma potentially justifies insurance mandates (Coate, 1995), whether of formal (e.g. the National Flood Insurance Program) or informal (e.g. elevation) insurance. Information frictions also plausibly justify quantity regulation, such as restricting the construction of risky houses, particularly when consumers are inattentive to prices. Our evidence of market frictions thus indicates considerable scope for this policy to improve welfare.

Our descriptive results reveal a trade-off between regulatory costs and benefits, but without further structure they cannot tell us which one dominates. First, equilibrium quantities and prices on either side of the boundary are the result of households choosing among alternative locations, and developers choosing whether or not to develop. Applying floodplain regulation in one area impacts demand and therefore prices and development in another. To interpret our reduced-form results, we must explicitly account for the residential choices and housing supply curves that yield these equilibrium outcomes. Second, the reduced-form estimates capture outcomes in a narrow slice of land, while the spatial equilibrium resolves prices and quantities across the entire market. We must account for market-wide characteristics such as the joint distribution of amenities and flood risks when estimating the policy’s effects.

Thus, in the second part of the paper, we specify a model of consumer location choices and housing supply. In the demand model, individuals maximize utility when choosing census-tract-by-flood-zone locations, as a function of prices, floodplain status, and unob-

served 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 any elevation. We estimate the model’s key parameters — demand and supply costs of floodplain designation — by matching the cross-border differences we find in our reduced-form spatial regression discontinuity analysis. We calibrate supply and demand price elasticities to estimates from the literature (Baum-Snow and Han, 2019; Calder-Wang, 2021; Song, 2021).

Our model allows us to translate our regression discontinuity estimates into a regulatory cost to consumers of 23% of home value and a cost to developers of 25% of home value. These large costs of floodplain regulation substantially reduce flood damages, but not enough to justify the policy. Using our estimated parameters, we find that relative to an unregulated benchmark, floodplain regulation reduces new development in the regulated area by 16%, or approximately 118,000 houses in our 10-county sample. Under current flood risk levels, this relocation reduces the present discounted value (PDV) of expected damages by 9%, or \$950 per newly-developed house. The building code averts an additional 20% of damages, for a total reduction of \$1.8 billion per county. However, the regulation achieves this risk reduction at a considerable cost to consumers. We compute the policy’s effect on welfare, defined to capture both consumer surplus and any damages that consumers do not consider. We find that the policy reduces welfare by at least \$11 billion and potentially more than \$42 billion. These welfare losses are driven by two channels: mandatory elevation costs more than the value it creates, and the policy relocates some consumers who would have been willing to pay the full social cost of living in the floodplain. Non-financial costs of the regulation on consumers who remain in the floodplain further exacerbate the welfare losses.

Our paper relates to several strands of the literature. Most narrowly, it expands upon previous work studying floodplain regulations. While others have studied the effect of regulations on adaptation (Kousky et al., 2018b; Wagner, 2021; Mulder, 2021), we build on this litera-

ture by inquiring about damages additionally avoided through the location channel.<sup>3</sup> Like a recent analysis of the effect of floodplain disclosure laws on population (Lee, 2022), we use a spatial regression discontinuity at the SFHA boundary to avoid omitted variables bias. Since the development outcome is potentially endogenous to modern floodplain status, we expand on this approach by digitizing and studying *historical* SFHA boundaries. Another strand of the literature has also studied the floodplain discount on house prices (Harrison et al., 2001; Bin and Landry, 2013; Indaco et al., 2018; Gibson and Mullins, 2020; Hino and Burke, 2021; Lee, 2022); our spatial regression discontinuity on historical boundaries builds on this work by capturing the effect on price after allowing for supply to adjust over the long run. Finally, to our knowledge we are the first to use reduced-form estimates of the policy's effects to quantify the policy's costs on both the adaptation and location margins.

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

Our paper also speaks to the importance of accounting for equilibrium effects when estimating the costs and benefits of a policy. In the specific context of zoning regulations,

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<sup>3</sup>We are not the first to study the effect of floodplain regulation on location. Browne et al. (2019) find that participating in the National Flood Insurance Program (NFIP) shifted housing development from coastal to non-coastal areas in Florida, while Peralta and Scott (2019) find that across all early-joining counties, NFIP participation reduced population outflows. We build on this work by analyzing the effect of risk designation on development *within* a community rather than across communities, holding constant the availability of flood insurance.

this analysis builds on recent work that combines boundary discontinuities with models of consumer choice and housing supply to estimate policy impacts (Anagol et al., 2021; Song, 2021). Here, we apply this approach to a setting with a clear and measurable benefit of regulation: reducing flood damages. More broadly, we build on a body of work studying how attribute-based supply-side regulation affects the equilibrium provision of goods (Ito and Sallee, 2018; Germeshausen, 2018; Kellogg, 2020). For example, Ito and Sallee (2018) consider how the US regulation of fuel mileage, based on the secondary attribute of vehicle size, shifts the market towards larger vehicles. Our analysis describes a similar phenomenon, where regulations based on floodplain status shift development out of the floodplain. While attribute-basing fuel mileage standards undermines their effectiveness, we find that in this setting, attribute-basing floodplain regulations reinforces the intended effect. Similarly, on the demand side, Barahona et al. (2020) explore how mandatory labeling of goods with hidden costs alters the equilibrium characteristics of those goods. In our context, mandatory labeling of risky houses is one important channel through which floodplain policy reduces equilibrium flood risk.

The next section describes the institutional details of the National Flood Insurance Program, including the regulations imposed inside the SFHA and the process of generating flood maps that distinguish between SFHA and non-SFHA land. The following section describes our setting — the state of Florida — and data. In Section 4, we present reduced-form evidence of lower modern-day development and higher modern-day house prices just inside the boundary of the historical SFHA. Section 5 presents evidence that the policy increases adaptation and reduces damages, and that consumers are not willing to pay for the value of lower flood risk when purchasing safer houses. We specify our equilibrium model of the housing market in Section 6 and discuss its estimation in Section 7. Section 8 simulates counterfactual distributions of development and prices under an alternative policy regime, and Section 9 discusses welfare implications. We conclude by considering directions for future work and implications for policy.

## 2 Institutional Background

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

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

The most important distinction for both insurance policies and floodplain regulation is between areas that are determined to be high-risk, known as Special Flood Hazard Areas (SFHAs), and those that are not. In the SFHA, some homeowners face a flood insurance mandate and higher flood insurance prices for otherwise-equivalent houses.<sup>4</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.

All new construction and substantial home improvements in SFHAs must comply with building regulations. The most important regulation is that a home's lowest floor must lie above the Base Flood Elevation (BFE).<sup>5</sup> This elevation requirement is well-founded; building above

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<sup>4</sup>Homeowners with federally-backed mortgages are legally required to purchase flood insurance.

<sup>5</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

the BFE reduces flood damages by at least 30% (Wagner, 2021). Other costs imposed on development in SFHAs include substantial permitting requirements and the production of elevation certificates.<sup>6</sup>

Throughout the United States, 10% of land<sup>7</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

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

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

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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>6</sup>Legislators designing the NFIP in the 1960s assumed that these costs would divert development away from high risk areas, further reducing flood damages (Burbey, 2001).

<sup>7</sup>Authors' calculations using 2017 flood maps.

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

The accuracy of a flood map depends on both the accuracy of the estimates of land elevation and the accuracy of the hydraulic model which simulates the amount of excess water in a flood event (National Research Council, 2009). Historically, engineers estimated land elevation based on US Geological Service contour lines, which suffer absolute elevation error on the order of meters.<sup>8</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.

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

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<sup>8</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.

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

### 3 Setting and Data

Our empirical context is the state of Florida, one of the most flood-prone and populous states in the United States. This makes it an ideal setting to study how floodplain regulation impacts housing markets and disaster damages. Nearly 50% of land and 19% of homes in Florida are located in SFHAs. Florida alone accounts for 35% of the nation's NFIP policies.<sup>9</sup> In addition, Florida publishes detailed data on housing transactions and locations, enabling the granular analysis necessary for our empirical strategy.

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

**Digitized historic and current flood maps** Our analysis is organized around archival scans of early flood maps that we digitized for parts of eleven counties.<sup>10</sup> We aimed to collect the first Flood Insurance Rate Maps ever drawn. In a few instances, constraints on the availability or formatting of these first maps made this impossible. In these cases, we were able to digitize maps that were drawn only a few years later. All but two of the 120 panels we digitized became effective between 1977 and 1984. Appendix A.2 describes the sample selection process in more detail.

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

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<sup>9</sup>See <https://nfipservices.floodsmart.gov//reports-flood-insurance-data> for details.

<sup>10</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.

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

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

**Satellite-Derived Land Use Data** Figure 1a demonstrates that the floodplain distinctions are detailed, necessitating spatially granular data on land use to study outcomes on either side of the boundary. We use two datasets to measure land use at two points in time. The first is US Geological Survey data on land use patterns contemporaneous to the time the original maps were drawn. This dataset consists of a 30x30 meter raster describing land use and land cover as belonging to one of nine mutually exclusive meta-categories, including urban/built-up land, agriculture, wetland, and water.<sup>11</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>12</sup> Table

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<sup>11</sup> Across Florida, the median number of raster grid cells per census tract is 4865.

<sup>12</sup> Between 1980 and 2020, Florida’s population more than doubled from 9.75 million to 21.5 million (?).

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

**Parcel Characteristics** Data from the Florida Department of Revenue property tax records from 2005 to 2020 provide detailed information about structures, including sales prices. We precisely geolocate each parcel’s exact building location using Microsoft’s open-source building footprints dataset.<sup>13</sup> Across Florida, each census tract — the geographic unit of analysis in our model of the housing market — has a median of 1873 parcels, 1680 of which are residential and 994 of which are single-family houses. Table 1 panel C summarizes average home prices statewide and in our sample of interest.

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

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

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<sup>13</sup>This dataset is also derived from satellite imagery, mostly captured in 2019. See <https://github.com/microsoft/USBuildingFootprints> for more details.

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

## 4 Equilibrium Development and Price Effects of Floodplain Designation

### 4.1 Empirical Strategy

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

In practice, we found that both omitted variables and reverse causality are relevant threats to credibly isolating the effect of floodplain regulation. Table 1 indicates that land in SFHAs is more likely to be water or wetlands and is closer to the coast. Moreover, development status likely directly affects SFHA status. Inside the SFHA, homeowners who are correctly

mapped have an incentive to elevate their house to “escape” the SFHA. Homeowners who were incorrectly mapped into an SFHA would benefit from petitioning FEMA to correct the mistake. Meanwhile, nobody faces an incentive to elevate or correct mapping errors on undeveloped land. Indeed, Appendix Table A.1 shows direct evidence of such reverse causality: land that was developed as of 2004 is more likely to be remapped out of a floodplain in the next map revision than land that was undeveloped. This dynamic could lead to a mechanical relationship between development status and SFHA status that is unrelated to the causal effect of interest.

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

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

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

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.

## 4.2 Results

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

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

(Figure 2b). Therefore, we discuss our results in the context of an *intent-to-treat* framework: the treatment of interest is the initial designation, which may evolve over time. Alternatively, we could estimate the treatment effect of regulating land as an SFHA by instrumenting for SFHA status with the historic boundaries.<sup>14</sup>

Next, we turn to our historic land use data to probe our identifying assumptions. These tests amount to checking for smoothness in land use before SFHA designation.<sup>15</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. Figure 3a shows that pre-period development is smooth across the boundary, supporting our hypothesis that the institutional details of the mapping process in the 1970s and 1980s provide a compelling setting to conduct a boundary discontinuity analysis. The magnitude of the discontinuity in pre-period development, reported in Table 2, is economically small and statistically insignificant in our baseline specification.<sup>16</sup>

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

**Development falls in the SFHA** By 2016, we observe a clear discontinuity in development. Figure 3b shows that land just inside the SFHA is 4.2 percentage points less likely to be developed than land just outside the SFHA.<sup>17</sup> This represents an 18% reduction as a

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<sup>14</sup>For outcomes measured as a stock, like development, this would require integrating over all first stages between the initial map and today. In practice, due to limited data on historical maps, one would interpolate over the magnitude of the first stage in 1996 and 2017.

<sup>15</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>16</sup>In our local-linear specification, the coefficient continues to be economically small but does become statistically significant.

<sup>17</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,

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

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

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

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

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

We see no evidence that observable differences in structures across the boundary explain the positive price effect: in Appendix Table A.3, we report estimates on sales prices residualized of a rich set of observable characteristics of each house. Though these results are noisier, the positive effects persist. We also see similar effects on prices for homes built before and after the introduction of the SFHA regulations. This suggests that the positive price effect is driven by restrictions in supply and not a greater willingness-to-pay for SFHA-regulated structures.<sup>18</sup>

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

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

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

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<sup>18</sup>We investigate this finding in more detail in Section 5.

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

## 5 Consumer Willingness to Pay for Lower Risk

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

### 5.1 Empirical Strategy

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

inside any multi-story house. We therefore interpret the adaptation effect on prices as the consumer's willingness-to-pay for reducing flood risk.

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

Following Wagner (2021), to estimate the effect of adaptation we exploit the fact that a community 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, premiums, or sale price);

$r_i$  indicates the construction year relative to house  $i$ 's year of enrollment; and  $\gamma_{j(i)}$  is a set of census tract fixed effects.<sup>19</sup> In practice, we estimate this model at the census-tract-by-flood-zone-by-relative-year-built level. We cluster standard errors at the census tract level. In order to increase power, we also estimate a binned specification: we restrict to houses built within 10 years of the date a community joined the NFIP and estimate the following model, again clustering standard errors at the census tract level:

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

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>

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

## 5.2 Results

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

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<sup>19</sup>Census tracts are smaller than communities.

<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.

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

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

Figure 5b shows that after NFIP enrollment, insurance payouts fall by about 55%, or \$1.60 per \$1000 of coverage. At the average coverage amount of \$252,000, this translates to a difference of \$400 less in damages per policy-year. With a discount rate of 5%, the PDV of these savings is 4.2% of the average house value.<sup>21</sup> Partially reflecting this difference, premiums for adapted houses are cheaper by on average \$1.21 per \$1000 of coverage, or \$300 per year. The PDV of these savings is 3.2% of the average house value.<sup>22</sup>

Despite this higher value to both the government and the consumer, the prices of elevated

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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.

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

houses are not significantly different from those of non-elevated houses. The point estimate of the increase in house price for elevated houses is -0.7%, or -\$1400. Appendix A.4.4 shows that in a stylized model of a housing market, a zero effect on price implies zero willingness-to-pay for reduced risk. Thus, not only do we find that consumers are unwilling to pay for the total value of a safer house, we find that they are unwilling to pay to reduce *any* of the flood costs they personally face.<sup>23</sup> As discussed previously, this indicates behavioral frictions such as risk misperception or myopia, and potentially also moral hazard from consumer expectations of government aid in case of disaster. Separating these two alternatives is challenging and outside the scope of this paper. However, in either case the effect would be the construction of inefficiently-risky houses.

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

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

## 6 An Equilibrium Model of the Housing Market

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

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

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

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

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

## 6.1 Residential choice

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

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

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<sup>24</sup>In Florida, counties are large but tend to only contain one major city and commuting zone.

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

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

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

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

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

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

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

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

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

where  $p_{jz}$  is the log price of housing in location  $jz$ ,<sup>27</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 allow for the possibility that amenities  $\xi_{jz}$  are correlated with both price  $p_{jz}$  and floodplain status  $SFHA_z$ . The correlation between  $\xi_{jz}$  and  $p_{jz}$  arises in equilibrium as higher-amenity locations command a higher price. The correlation between  $\xi_{jz}$  and  $SFHA_z$  occurs because SFHA status may be correlated with other amenities, like coastal access, or disamenities, like abundant insects. 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:

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

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<sup>26</sup>We will discuss the implications of this aggregation for our descriptive analysis in Section 7 and for welfare in Section 9.

<sup>27</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.

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

## 6.2 Housing supply

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

The value of developing a house in period  $t$  depends on the (log) price  $p_{jz}^t$  for which it could sell, which varies by location and time, and the cost to build the house  $\eta_{jz}^t$ , 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

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

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

$$\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right)$$

### 6.3 Equilibrium

In equilibrium, house prices and location decisions adjust so that the quantity of housing supplied in each location equals the number of individuals choosing to live there. Specifically:  $L_{jz}\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right) = Q_{jz} = N_m s_{jz}$  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. Expanded, this equilibrium condition reads:

$$L_{jz}\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right) = N_m \frac{\exp(\alpha^D p_{jz} + \phi SFHA_z + \xi_{jz})}{\sum_{j' \in J_m, z' \in \{0,1\}} \exp(\alpha^D p_{j'z'} + \phi SFHA_{z'} + \xi_{j'z'})} \quad (6)$$

## 7 Estimation and Results

The model has two key parameters — the direct effects of floodplain designation on demand ( $\phi$ ) and supply ( $\psi$ ) — in addition to demand elasticity  $\alpha^D$  and tract-level supply parameters  $\mu_j$  and  $\sigma_j$ .<sup>28</sup> 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>29</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>30</sup> We calibrate the tract-specific supply parameters  $\mu_j$  and  $\sigma_j$  to estimates of tract-level housing

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

<sup>29</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>30</sup>In Appendix Table A.6 we present a set of results in which we estimate  $\alpha^D$  (without instrumenting), which yields a value of approximately -0.8.

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

## 7.1 Demand-side identification and moments

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

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

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

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

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

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

Recall that in Section 4, the SFHA effect was causally identified under the assumption that unobserved amenities are identical in the limit on the left and right sides of the floodplain boundary. However, in our structural model, after aggregating to the tract-zone level, amenities  $\xi_{jz}$  may be correlated with floodplain status  $SFHA_z$ . Therefore, we identify the floodplain effect on demand  $\phi$  by separating the amenity term into components that are and are not correlated with floodplain status. We then simulate the average price difference across zones after subtracting the correlated component. We require this price difference

to match the price difference estimated in the spatial RD, denoted  $\beta^{p,2016}$ . We lay out our moment conditions more fully in Appendix A.6.

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

### 7.1.1 Discussion and Implications

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

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

## 7.2 Supply-side calibration and moments

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

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

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

### 7.3 Estimation

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

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<sup>31</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

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.

## 7.4 Results

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

**Parameter estimates** Table 4 column (1) presents the parameter estimates for our baseline specification.<sup>32</sup> 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. As discussed above, under the assumption that consumers are insensitive to flood insurance

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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>32</sup>Appendix Table A.6 presents parameter estimates under alternative calibrations of demand elasticity  $\alpha^D$ .

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

The result that consumers are willing to pay 23% more to avoid living in a floodplain contrasts with recent work finding smaller floodplain discounts of 1-11% (Hino and Burke, 2021; Lee, 2022; Gibson and Mullins, 2020). This difference could be attributed to our setting: residents in flood-prone Florida may be particularly sensitive to signals of risk. Also, our work uses modern sales prices (2014-2019) rather than the older years (1992-2013) studied in papers that find smaller effects. A growing awareness of climate change could have strengthened the effect of floodplain designation (Bernstein et al., 2019). Nevertheless, a 23% premium on avoiding the floodplain far exceeds the actual financial benefits of relocation: Appendix Table A.5 indicates that in our sample, being in a floodplain increases expected damages by about 0.64% of house price per year, which translates to an PDV of 12.6% of house price — just over half of the estimated demand effect. This large discount may arise from consumers' strong dislike of the bureaucratic burdens of complying with floodplain regulations. 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. Alternatively, the floodplain designation may cause consumers to over-update their beliefs about risk.

Turning to supply, we estimate that regulations produce a shift in the supply curve that corresponds to a 25% increase in construction costs in the SFHA.<sup>33</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. The elevation

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<sup>33</sup>Construction costs include both the land on which a building is built and labor and materials costs.

requirement alone may necessitate a stem wall, which can add \$100,000 to the cost of a new build. The time spent on paperwork to comply with regulations is also costly. However, as described in Section 5, elevation reduces damages by about 16% of house price — somewhat less than the estimated cost of elevating.

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

## 8 Counterfactuals

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

$$L_{jz}\Phi\left(\frac{\tilde{p}_{jz}^{2016} - \hat{\psi}E_{jz}^{2016} - \mu_j - \eta_{jz}^{2016}}{\sigma_j}\right) = N_m \frac{\exp(\alpha^D \tilde{p}_{jz}^{2016} + \hat{\phi}SFHA_z + \xi_{jz})}{\sum_{j' \in J_m, z' \in \{0,1\}} \exp(\alpha^D \tilde{p}_{j'z'}^{2016} + \hat{\phi}SFHA_{z'} + \xi_{j'z'})} \quad (10)$$

given our calibrated and estimated parameters  $(\alpha^D, \mu_j, \sigma_j, \hat{\phi}, \hat{\psi})$  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 remain constant across counterfactual scenarios.<sup>34</sup> For each counterfactual, we report expected damages and home prices based on the location of development. In this section, we discuss these outcomes in a positive sense. Section 9 will then discuss normative implications.

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

To assess how floodplain regulation impacts development in high risk areas, we use external estimates of flood risk generated by a state-of-the-art hydrological model produced by the First Street Foundation.<sup>35</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, which allows us to estimate damages in a way that FEMA maps do not.

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

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

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<sup>35</sup>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).

<sup>36</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 the results. Relative to our no-SFHA counterfactual, floodplain regulation reduces the number of homes built in regulated areas with a substantial risk of flooding by almost 16%, reallocating over 117,000 houses. This represents approximately 2.6% of all new development in our sample. Notably, this figure exceeds the total number of houses removed from risky areas by the NFIP's home buyouts program over its lifetime (Frank, 2020a).

This relocation effect reduces the net present value of damages by \$949 per newly-developed house. This amounts to approximately 10% of expected damages. The requirement to adapt houses that remain in the floodplain further reduces net present value of damages by another 20%. Overall, the policy reduces net present damages by \$4,053 per newly-developed house. This amounts to an overall reduction in net-present-value damages by \$18 billion, or \$1.8 billion per county.

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

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

## 9 Welfare Implications

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

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

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

$$\sum_{jz} N_{jz} (\alpha^D \ln(P_{jz} + D_{jz}) + \phi_1 C_z + \xi_{jz} + \varepsilon_{ijz}). \quad (11)$$

As discussed in Section 6, we are unable to separate non-financial costs  $\phi_1$  from the estimate of the overall floodplain effect on demand  $\phi$ . The makeup of  $\phi$  is welfare-relevant, however: the component of  $\phi$  that is made up of non-financial costs  $\phi_1$  has real costs to the social planner. However, the component of  $\phi$  which is a debiasing effect  $\phi_2$  does not affect experienced utility and so does not impact social welfare. We therefore consider the spectrum of possible welfare effects by calculating welfare under two polar cases: (a)  $\phi_1 = \phi$  ( $\phi$  is 100% non-financial costs,) or (b)  $\phi_1 = 0$  ( $\phi$  contains only terms that do not affect welfare).

Since we do not observe  $\varepsilon_{ijz}$ , we cannot directly calculate Equation 11. Instead, we compute per-person consumer surplus in each market using the estimated demand function, multiply by the amount of new housing, and subtract total damages. Under the assumption that

consumers do not factor damages into their demand, this is equivalent to computing welfare using Equation 11. As previously discussed, if this assumption is incorrect, some portion of the  $\phi$  term will reflect this consumer valuation of damages. We would then double-count damages and overstate the cost. We account for this by considering the range of welfare outcomes between  $\phi_1 = \phi$  and  $\phi_1 = 0$ .

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

We report results of these welfare calculations in Table 6. We report differences in consumer surplus under the observed regulation relative to the unregulated benchmark. We see that if we assume the floodplain effect on demand  $\phi$  entirely reflects non-financial costs of regulation, the policy reduces consumer surplus by \$61 billion. If, alternatively, the floodplain effect  $\phi$  entirely captures a debiasing channel, consumer surplus is only reduced by \$29 billion. Even at the low end, these costs exceed the reduction in PDV damages, which is just over \$18 billion, or \$1.8 billion per county. Notably, this policy does not improve welfare even though we study the best-case scenario in which consumers completely fail to internalize any value of safer houses. If consumers were willing to pay for safer houses, the welfare impacts of this policy would be lower still.

The policy reduces welfare through both the adaptation and relocation channels because of the high supply costs and demand disamenities of regulation. We estimate that the policy increases the number of elevated houses by 352,000. The results in Section 5 show that elevating one house averts uninternalized damages worth about 15% of house price, on average. Thus, the elevation channel reduces damages by about \$13 billion in this sample. Meanwhile, our parameter estimates indicate that the cost to elevate a house is equivalent to about 25% of the average house price, or about \$63,000 per house inside the regulated floodplain. This yields a total elevation cost of about \$22 billion. The mandatory elevation

requirement therefore reduces welfare by approximately \$10 billion in our sample. Intuitively, the policy reduces welfare because it mandates a technology that is costlier than the value it provides to society.

The relocation channel likewise reduces consumer surplus, for even less gain. Consumers relocate because of the combination of passed-through elevation costs and regulation disamenities inside the floodplain. Relocating 2.6% of the population outside of the floodplain saves \$4 billion in uninternalized damages. It simultaneously raises prices outside the floodplain for *all* newly-developed houses, generating a pecuniary externality on non-movers.<sup>37</sup> Movers pay less on average for their new houses, but they also like their new locations less. We estimate the total welfare loss driven by these relocation-related factors is at least \$7 billion.<sup>38</sup> This amount substantially exceeds the welfare gains of relocation. Intuitively, this welfare loss is driven by the fact that the floodplain regulation demand disamenity of 23% of house value exceeds the average benefit of relocating a house outside of the floodplain, which amounts to approximately 18% of house value. The policy therefore pushes some consumers out of the floodplain even though they would have been willing to pay the full social cost of living there.

The above calculations necessarily embed a number of additional assumptions, made for the purposes of obtaining a bottom-line welfare estimate, but which should be considered carefully. We focused on fiscal costs of flooding and ignored any emotional costs, for example the hassle cost of coordinating contractors to perform repairs and the pain of losing objects of sentimental value. According to NFIP data, each year a claim is filed by about 1.5% of pre-enrollment houses inside the SFHA, 1% of post-enrollment houses inside the SFHA, and 0.75% of houses outside the SFHA. A back-of-the-envelope calculation using these values

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<sup>37</sup>Within-floodplain prices fall under the regulation, but the price effect overall is dominated by price increases.

<sup>38</sup>The overall consumer surplus loss of (at least) \$29 billion includes not only the above-mentioned relocation-based factors, but also passed-through elevation costs for non-movers, so we cannot attribute the entire consumer surplus loss to relocation. However, of the overall consumer surplus, a maximum of \$22 billion can be due to pass-through of elevation costs by developers. Thus, the relocation channel of lower amenities and equilibrium price changes causes at least \$7 billion of welfare loss.

indicates this policy eliminates 1256 flood claims per year in our sample. Emotional costs of flooding would thus have to exceed \$421,000 per claim to change the sign of the welfare effect.

Additionally, when calculating benefits of the policy we have focused solely on damages that are induced or averted through the housing channel. In reality, when consumers locate in a risky area, they also induce the government to locate public goods there — for example schools, fire departments, and roads. Flooding requires repairs to all these public goods, but the cost of these repairs is typically borne by the federal government in disaster aid. These costs are not incorporated into consumers' property taxes or insurance premiums, forming another channel of moral hazard. The Congressional Budget Office estimates that for every \$34 billion in residential damages caused by hurricanes, the public sector incurs \$12 billion of damages (U.S. Congressional Budget Office, 2019). If we, accordingly, inflated our estimated damages averted on the relocation margin by 35%, our estimate of the total welfare effect of the policy would be between \$-42 billion and \$-9 billion. Thus, our qualitative conclusions about the welfare impacts of the policy remain unchanged.

## 10 Conclusion

For over 40 years, federal floodplain regulations have influenced housing markets, with limited evidence on either their costs or potential benefits. This paper combines a spatial regression discontinuity analysis of floodplain boundaries, an event study of the introduction of flood-safe building codes, and a spatially-granular model of the housing market to investigate the policy's equilibrium effects on development location, housing prices, expected damages, and welfare.

We find that local to the regulatory boundary, floodplain regulation decreases development and increases house prices, highlighting the importance of a previously unstudied element of

the policy: construction costs imposed by stricter building codes. We also find that although flood-safe building codes reduce flood costs by at least 3.2% of housing value, consumers are not willing to pay for any of the savings when purchasing an elevated house. Using our model to interpret our reduced-form results, we find that floodplain regulation in Florida reduces new development in floodplains by 16%, or approximately 118,000 houses in the ten counties we study. This impact is large, exceeding the effect of a nation-wide home-buyout program and comprising about one-third of the policy's total risk reductions. The present discounted value of damages averted by the program exceeds \$1.8 billion per county. However, the regulation achieves these risk reductions at a considerable cost, raising prices by 1.7% market-wide and relocating consumers to less-preferred areas. Consumer surplus falls by between \$2.9 billion and \$6.1 billion per county, yielding a net negative effect on welfare of \$-1.1 billion to \$-4.3 billion per county. These results are driven by the fact that both corrective channels — relocation and elevation — impose costs exceeding their benefits. Notably, we find negative welfare impacts even though we assume consumers completely fail to internalize flood damages, which is the scenario in which the policy would have the largest benefits.

Our findings have important implications for policies to promote the resilience of cities in the face of sea level rise and other climate-change-induced increases in flooding. First, our results indicate that relocations generated more welfare loss than benefits. This suggests that policy efforts that prioritize adaptation-in-place can be better for welfare than strategies of managed retreat, despite the apparent futility of living in high-risk areas. At the same time, however, adaptation is not cost-effective in all places. Alternative policies could potentially improve welfare by restricting mandatory adaptation to those regions where flood risk is particularly high. Finally, our work emphasizes the importance of considering equilibrium effects when regulating land use. Spatial sorting across locations generates one-third of the estimated savings from this policy, but also generates pecuniary externalities on non-movers. Accounting for both of these effects is crucial for fully understanding the impacts

of this policy.

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

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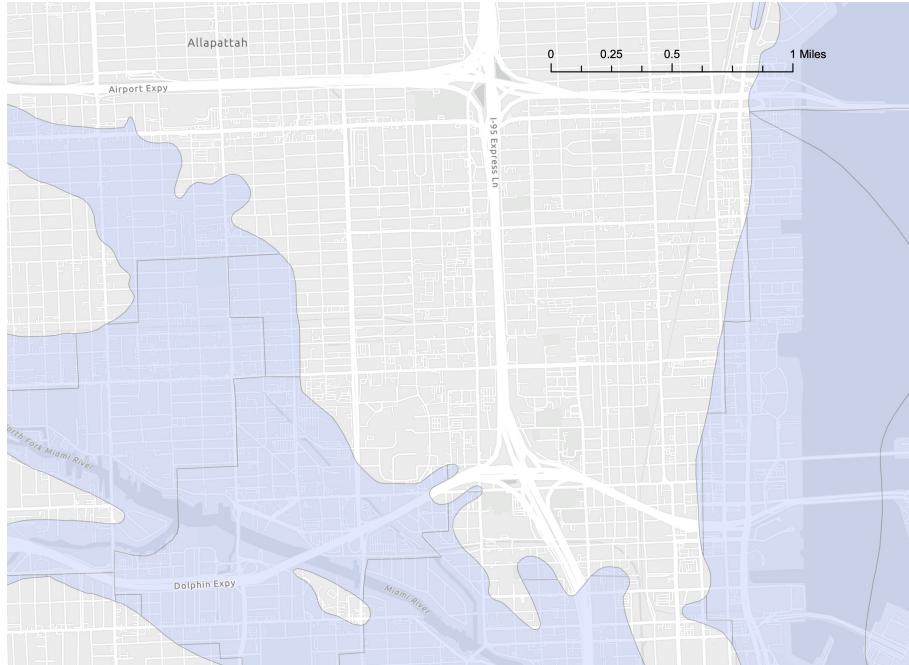
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## Tables and Figures

Figure 1: Digitized Flood Maps

(a) Digitized Map of Miami (1978)



(b) Coverage of Digitized Maps

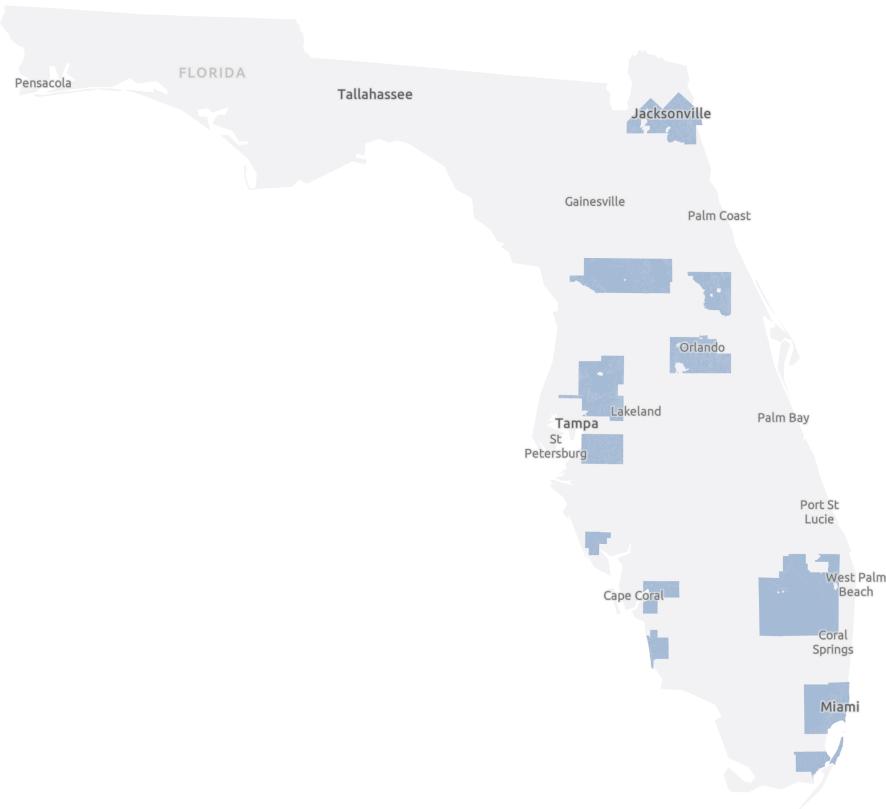
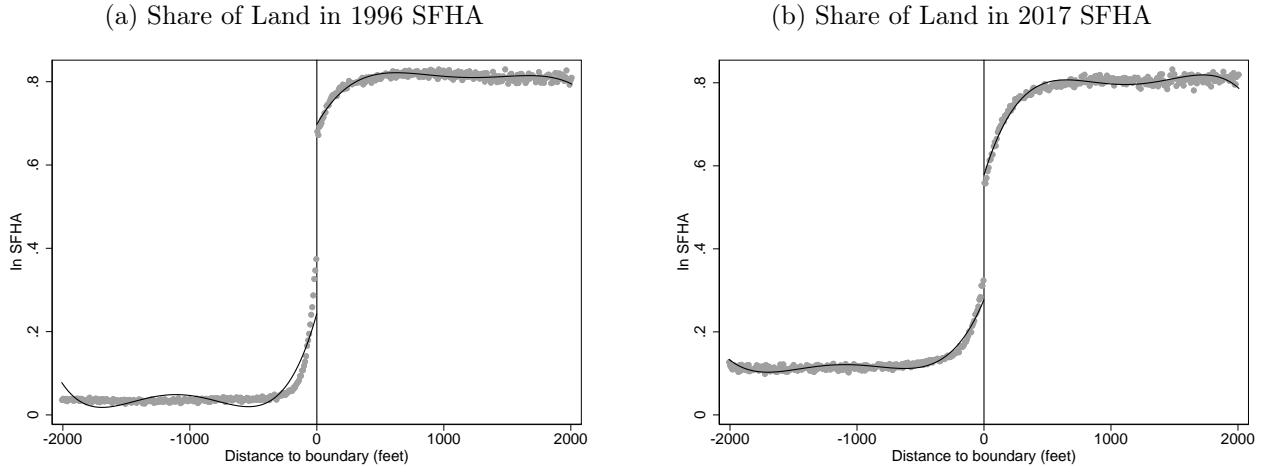
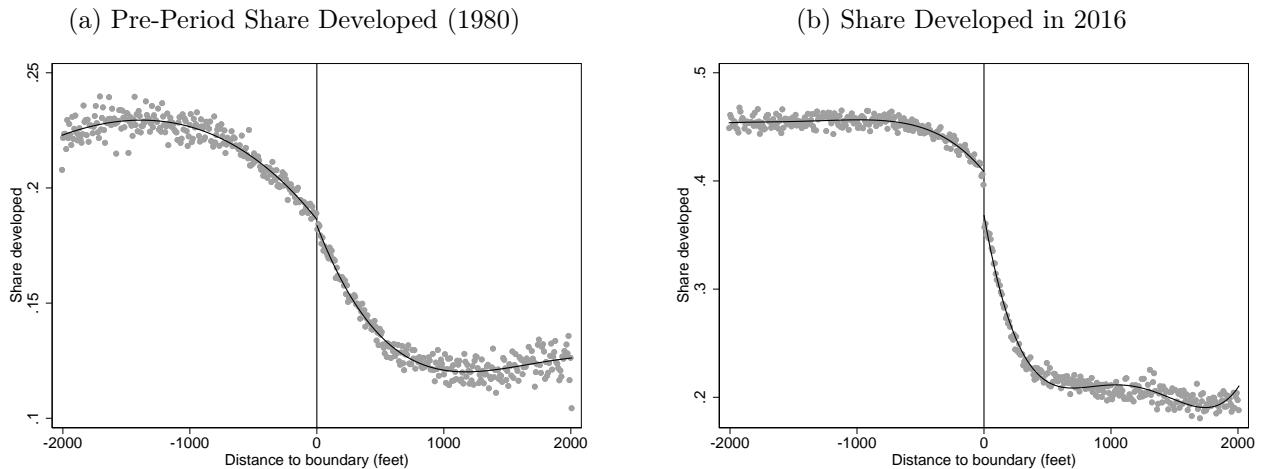


Figure 2: RD Estimates: Current SFHA Status



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



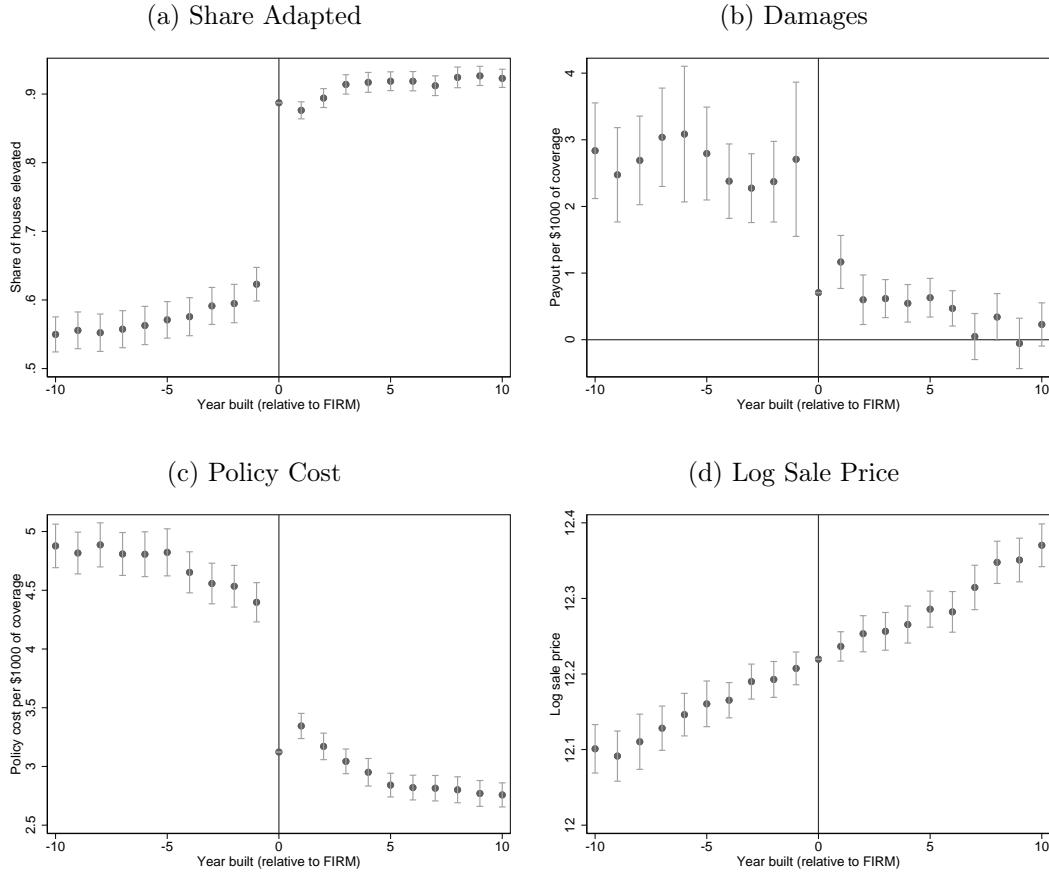
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary measured in feet, with positive distance indicating being inside the SFHA. Sub-figure (a) plots the share of land that was developed as of the late 1970s and early 1980s, and sub-figure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

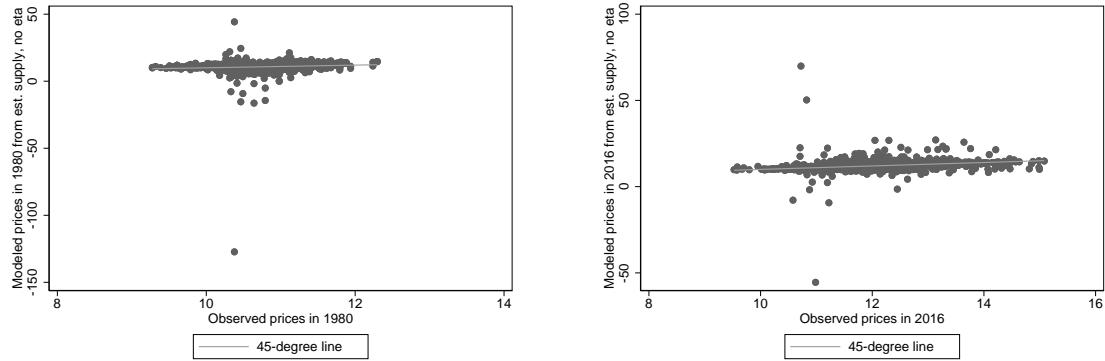
Figure 5: NFIP Enrollment Year Event Study Estimates



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

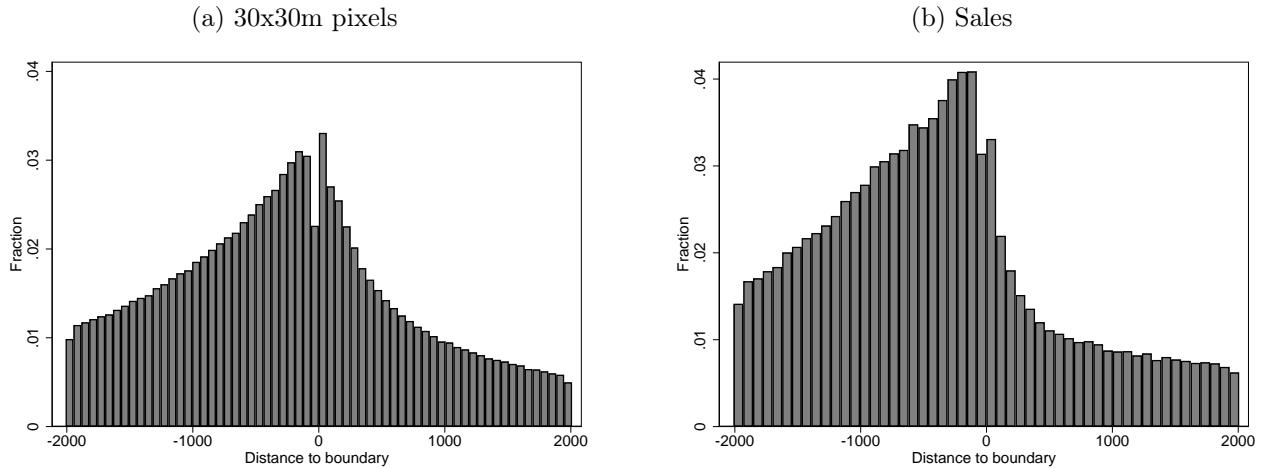
## A.1 Appendix Tables and Figures

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



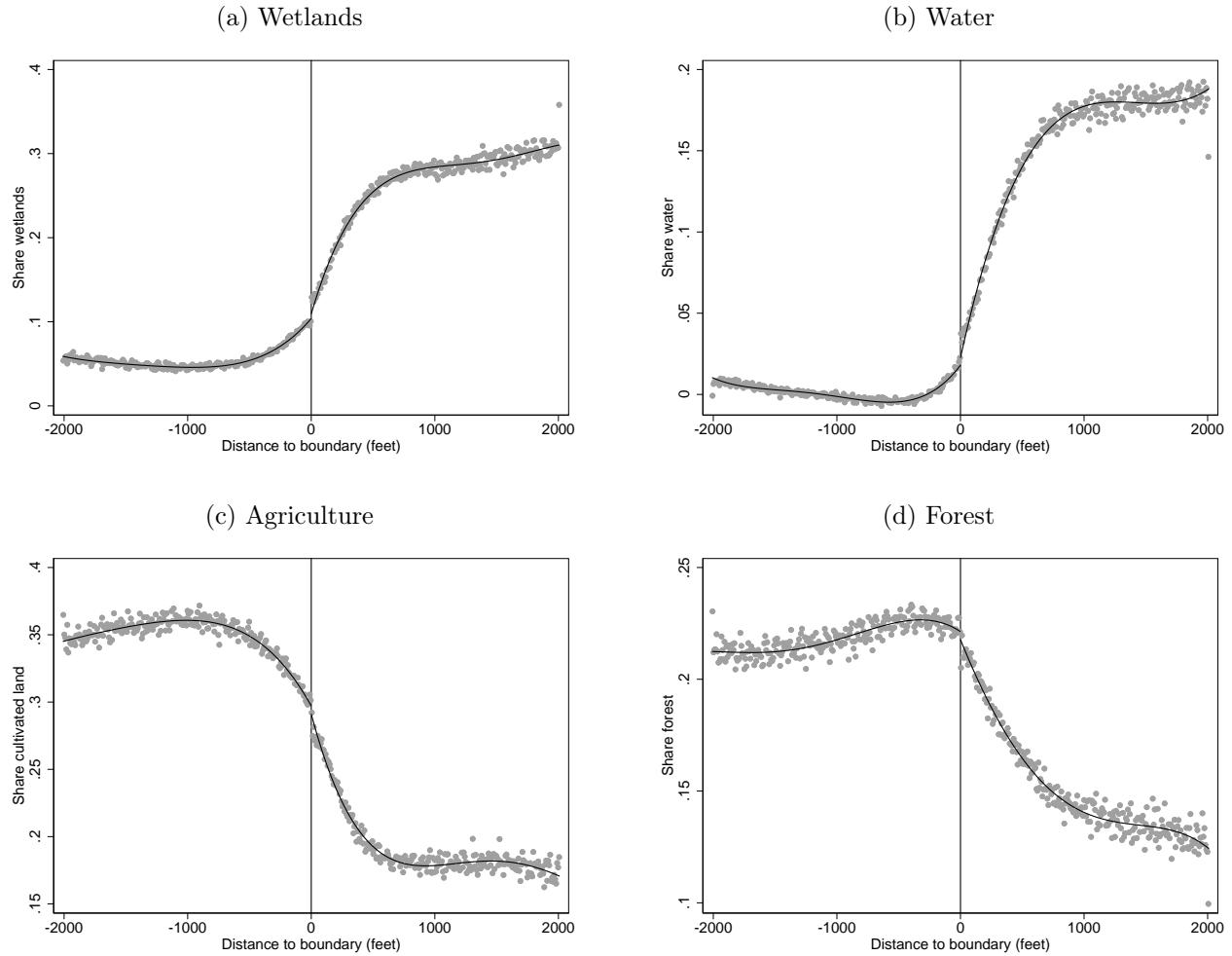
Notes: Figure shows an elevated house in Collier County, Florida. At this location, SFHA regulations require the bottom of the lowest (non-basement) floor to be elevated to 10 feet above ground level.

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.

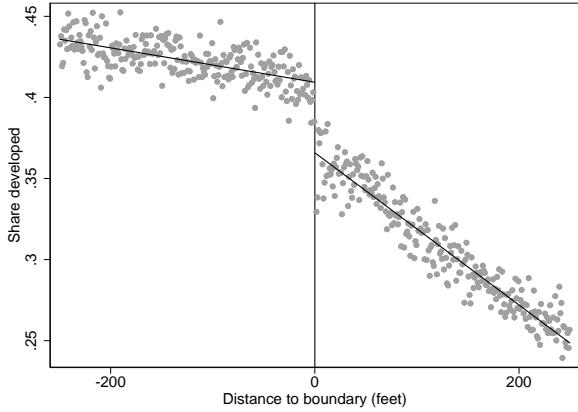
Figure A.3: RD Estimates: Other Pre-Period Land Use



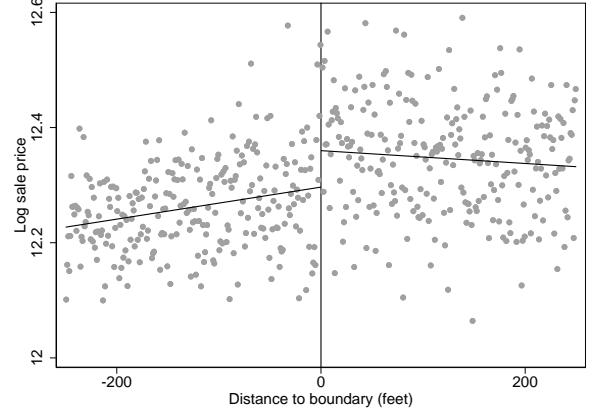
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the SFHA boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the SFHA. All land use outcomes are measured as of the late 1970s and early 1980s. Estimates are residualized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

Figure A.4: RD Figures: Local Linear Specification

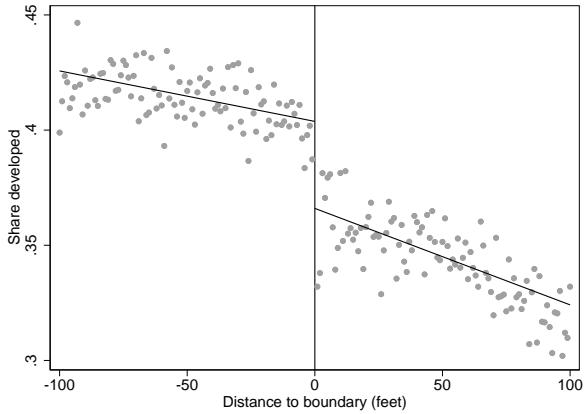
(a) Share Developed in 2016, Rectangular Kernel



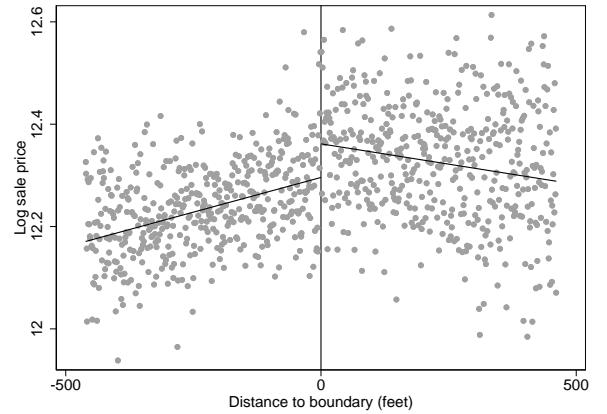
(b) Log Sales Price, Rectangular Kernel



(c) Share Developed in 2016, Triangular Kernel

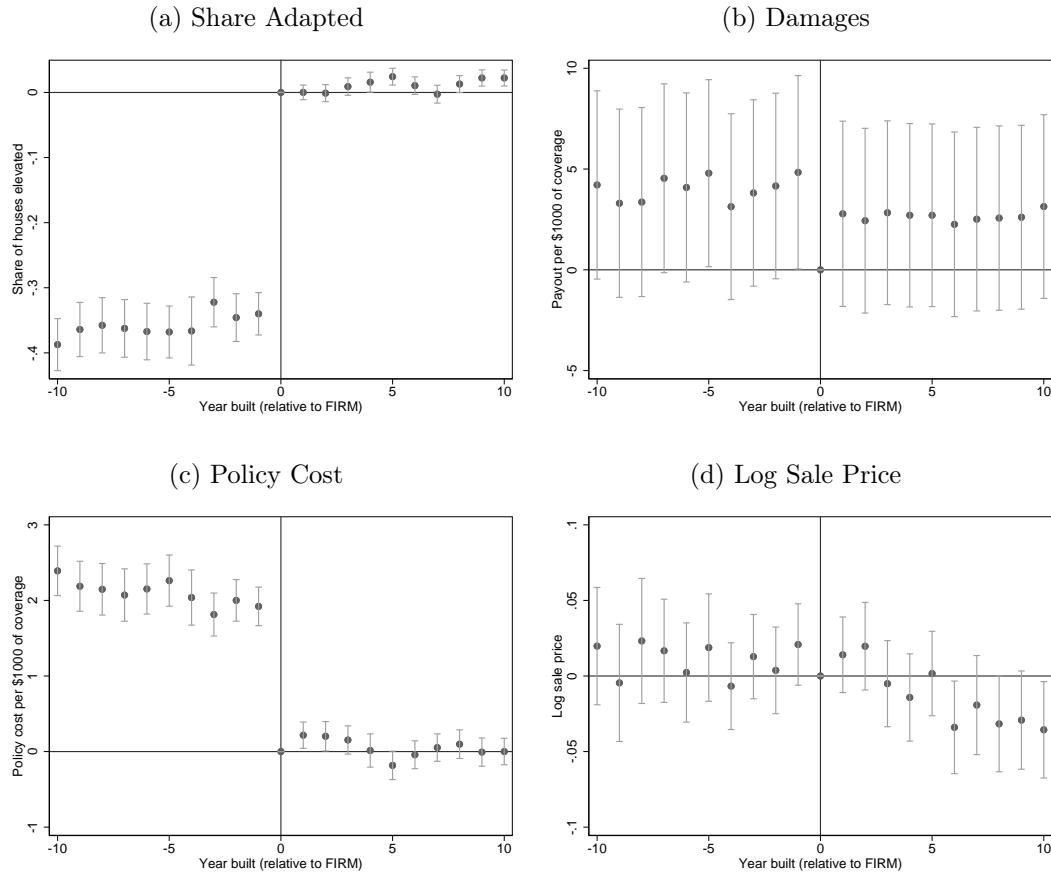


(d) Log Sales Price, Triangular Kernel



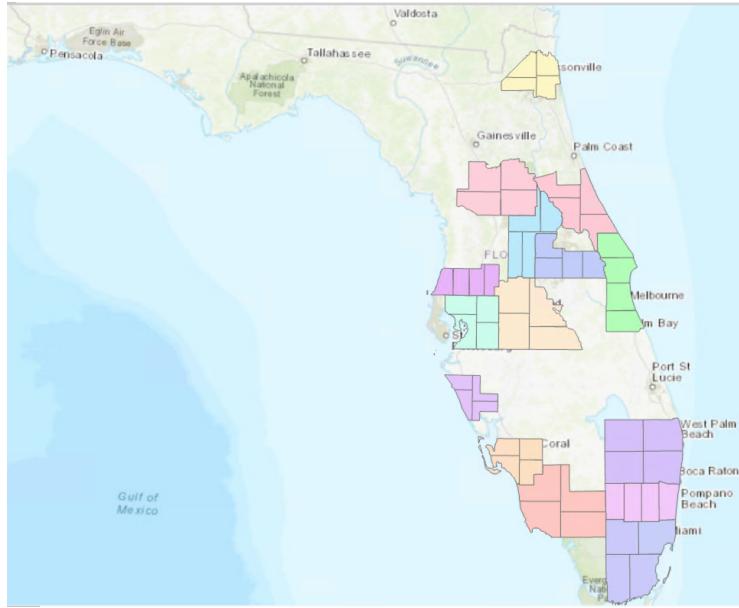
Notes: Figures present RD plots for alternative specifications. Sub-figures (a) and (b) present results using a local linear regression with a rectangular kernel on a fixed 250 foot bandwidth. Subfigures (c) and (d) present results using a local linear regression with a rectangular kernel on the MSE-optimal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

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



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

Figure A.6: Quadrants



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

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

	Equal weight to each developed pixel		Weighted by number of parcels	
	Inside SFHA	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)	(2)
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>39</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>39</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>40</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>41</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>40</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>41</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 5 by also using the fact that building standards were imposed for houses built inside the SFHA but not for houses built outside of it. We estimate the following difference-in-difference models for houses in the SFHA:

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

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

Here,  $\gamma_{j(i)}$  indicates census tract-by-flood-zone fixed effects. We again cluster standard errors

at the census tract level. In all claims and policy regressions, we weight each observation by the number of policies it represents.

The patterns observed in Section 5 are largely unchanged in the difference-in-difference specification. Here, we find that the regulation reduces insurance payouts by \$2.84 per \$1000 of coverage. At the average coverage amount of \$252,000, this translates to a difference of \$715 per year. With a discount rate of 5%, the 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 (2019) (BSH) for land development elasticities at the census tract level, estimated between 2001 and 2011. Specifically, we take estimates of the elasticities as of 2001 from the IV specification. We replace any negative elasticities with the smallest nonnegative elasticity in our eleven-county sample. We denote these elasticities as  $\alpha_j^S$ .

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>42</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>42</sup>We use 2016 price and quantity as our baseline because the BSH estimates used data from 2000-2010.

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

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

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:

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

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:

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

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

### A.6.2 Supply-side calibration and moments

We assume that in a narrow band around the floodplain boundary, some portion of unobserved construction costs  $\eta_{jz}^t = \Delta^{S,t} SFHA_z$  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:

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

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

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:

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

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

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

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

## A.7 Calculation of Expected Damages

We define flood risk using data from the First Street Flood Lab estimates of Average Annual Loss (AAL). AAL expresses expected annual damages as a share of house price. These data come from parcel specific-estimates (as opposed to the raw hazard layer) that combine the raw hazard layer (which generates the parcel-specific inundation depth) with the output of an engineering damage model. The damage model takes as inputs a number of features of the structure, including its market value, number of stories and units, and foundation type, and calculates damages using the HAZUS-MH methodology. The HAZUS-MH methodology

was developed for FEMA to calculate estimated damages from natural disasters and is based on a set of depth-damage curves collected from FEMA’s Federal Insurance and Mitigation Administration (FIMA) and the USACE Institute for Water Resources (USACE-IWR).<sup>43</sup>

Expected damages are computed as the product of number of newly-developed gridcells 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>44</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

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<sup>43</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.

<sup>44</sup>This 70% factor was recommended by First Street, who provided the underlying AAL data.

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

## A.8 Welfare Calculation Details

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

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

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

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

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