The Effects of Floodplain Regulation on Housing Markets

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Abstract

We investigate the effects of housing regulations designed to correct a wedge between privately- and socially-optimal construction in areas at risk of flooding in Florida. Using a spatial regression discontinuity around regulatory boundaries and an event study around the policy's introduction, we document that floodplain regulation reduces new construction in high-risk areas and increases the share of newly-built houses that are elevated. Embedding these effects in a model of residential choices with elastic housing supply, we find that the policy reduces expected flood damages by 60%. One-quarter of this reduction is driven by relocation of new construction to lower-risk areas, and three-quarters is driven by elevation of houses remaining in risky areas. However, this second-best policy achieves at best about one-tenth of possible welfare gains because of poor targeting. It overcorrects in many areas, inducing more consumers to elevate and relocate than is socially-optimal, while still allowing inefficiently-high construction in the riskiest places. By contrast, a flexible corrective tax on flood risk would achieve substantial welfare gains of more than $2,700 per newly-developed house.

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1 Introduction

Floods cause an estimated $32 billion in damages per year, making them the costliest form of natural disaster in the United States (Wing et al., 2022). These losses are expected to grow by 25% by 2050, reflecting both an increase in flood hazard due to climate change and a concentration of population growth in risky locations (Wing et al., 2022). A central concern among policymakers and economists is that these trends are driven by a wedge between social and private values for flood safety (Kydland and Prescott, 1977; Coate, 1995; Ben-Shahar and Logue, 2016). Indeed, a large body of work has documented that private incentives to reduce flood risk are muted by both misperceptions of that risk and expectations of government aid (Gallagher, 2014; Kousky et al., 2018b; Davlasheridze and Miao, 2019; Bakkensen and Barrage, 2021; Wagner, 2021; Mulder, 2021; Landry et al., 2021). To correct these market frictions, policymakers demarcate especially risky locations as "Special Flood Hazard Areas" (SFHAs) and regulate them more strictly. Inside the SFHA, developers are required to build elevated homes, and homeowners face a flood insurance purchase mandate and high flood insurance prices.[1]

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

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

extrapolate effects away from the boundary, estimate the costs of regulation, and investigate the welfare effects of current and counterfactual policies.

To conduct our analysis, we assemble a new comprehensive and spatially-granular dataset to describe regulation, real estate development, and flood risk in Florida over a 40-year time horizon. Florida is both populous and highly flood-prone, with 45% of land currently designated as a high-risk flood zone. Our dataset combines maps of historic flood zone extents, granular remote-sensing-based measures of historic and current development, administrative data describing house prices and attributes, and the flood risk profile of current and counterfactual development, generated from a state-of-the-art hydrological model. The scope and detail of our dataset are both critical to our analysis. Because our dataset extends to the first maps delineating regulated areas — which we digitized from archival scans for this project — we are able to study the policy's effect on long-run development and confirm the validity of our core empirical strategy. And because our dataset details both elevation and location decisions, as well as how flood damages vary along these margins, we are able to comprehensively measure the policy's effect on flood risk.

We use two complementary empirical strategies — a spatial regression discontinuity and an event study around the regulation's implementation — to characterize the policy's risk reduction effects along two margins: reduced construction and mandatory elevation in risky areas. Our spatial regression discontinuity design compares current development and house prices on either side of the regulatory boundary delineated at the time of the policy's introduction in the 1970s and 1980s. Our analysis relies on the assumption that flood risk and other amenities are smooth through these initial regulatory boundaries. While unlikely to hold in modern maps, this assumption is reasonable for the original maps because mapping technologies were rudimentary and homeowners lacked the ability to influence the initial regulatory boundaries.[2] Importantly, we validate this assumption by examining smoothness in

[2]After the initial maps were drawn, landowners could deregulate developed parts of their properties either by petitioning to correct a mistake or physically elevating land to reduce its risk. Since maps are updated

pre-period land use through the historic boundaries. We document that modern-day development is 9% lower just inside the regulated area, highlighting the potential for the policy to reduce damages by shifting development out of risky areas. This decrease in development is not accompanied by a reduction in prices – if anything, prices are slightly higher just inside the regulated area – indicating that floodplain regulation imposes costs on developers, which they at least partially pass through to consumers.

We also document reduced damages on the intensive margin, through elevation of houses that are built in the floodplain. Building on Wagner (2021), we exploit the sharp timing of the policy's introduction to examine the effect of building standards on flood risk in an event study design. We find that regulation increases the share of elevated homes by 27 percentage points and reduces flood risk by 3% of average home value. Though mandatory elevation generates social value via reduced damages, we show that homeowners do not privately value this reduction in flood risk. This result is consistent with prior work documenting a large wedge between social and private valuations of flood risk, implying scope for welfare-improving intervention. It also suggests that the increase in price across the boundary is attributable to construction costs, not differences in willingness-to-pay for elevated homes.

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

over time, this behavior produces a negative correlation between development and floodplain designation in modern flood maps.

which could mitigate the incentive to relocate and impose pecuniary externalities on unregulated consumers. Moreover, a full welfare analysis of the policy requires us to quantify the regulation's costs to developers and consumers.

We therefore specify and estimate a model of residential choice and real estate development. In our model, individuals maximize utility when choosing census-tract-by-flood-zone locations, as a function of prices, floodplain status, and unobserved amenities. 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 elevation. Because our event study estimates imply that homeowners are unwilling to pay for elevation, we simplify the model by differentiating houses only by location — not risk — and assuming that suppliers do not endogenously provide elevated houses absent policy intervention. We estimate the model's key parameters — demand and supply costs of floodplain designation — by matching the cross-border differences we find in our boundary discontinuity analysis. We calibrate supply and demand price elasticities to estimates from the literature (Calder-Wang, 2021; Song, 2021; Baum-Snow and Han, 2022).

We first use the model to quantify the policy's impact on expected flood damages. We find that the policy reduces expected flood damages by 60%, or approximately \$3.5 billion per county. Both the extensive-margin location channel and intensive-margin elevation channel are quantitatively important in this setting. We estimate that one-quarter of the reduction in damages is driven by the relocation of new construction to lower-risk locations. The remaining three-quarters are due to elevation mandates for the houses that remain in risky locations. However, we find that these risk reductions are driven by large costs imposed by the regulation on both consumers and producers of housing. Our parameter estimates suggest that mandatory elevation of a house increases construction costs by 25%. Furthermore, consumers are willing to pay 23% more for an equivalent house to avoid living in a regulated area.

We conclude by developing a normative framework that allows us to estimate and compare welfare under current and counterfactual policies. We define social welfare to capture both producer and consumer surplus and any socially-costly damages that consumers do not value, whether due to internalities or externalities. Because the wedge between private and social valuation of flood risk could be due to misperceptions, our welfare framework allows consumers' decision and experienced utility to diverge (Allcott and Taubinsky, 2015). We contrast the *status quo* policy regime against both an unregulated benchmark and a first-best corrective tax in the spirit of Pigou (1920) equal to the social cost of flooding, which varies by location and elevation status. This corrective tax is not only a useful theoretical benchmark, but a plausible extension of recent policy changes which have introduced more property-specific granularity in flood insurance premiums (Flavelle, 2021).

Though the current policy achieves approximately the socially-efficient degree of damage reductions, we estimate that it achieves at best about 10% of the possible social welfare gains from regulation. The policy performs poorly despite correcting market frictions because the uniform delineation of high-risk zones is poorly-targeted. In some regulated but relatively safe areas, the current policy is over-corrective: the elevation and relocation it induces is costlier than the benefits it generates, and this adaptation would not occur under the first-best regulation. Meanwhile, in the riskiest areas, the current policy should be even stronger: expected damages are so large that reducing risk is worth more than even the large costs imposed by current regulation. These findings underscore the gains to transitioning from a blunt attribute-based regulation to a more flexible policy instrument, such as a tax or an actuarially-fair insurance mandate, that can efficiently incentivize both elevation and relocation where appropriate.

Our work builds on and contributes to a number of distinct literatures. Most narrowly, we contribute to a body of work analyzing various aspects of floodplain regulation, including its effect on house prices (Harrison et al., 2001; Bin and Landry, 2013; Indaco et al., 2018;

Gibson and Mullins, 2020; Hino and Burke, 2021; Lee, 2022), in-place adaptation (Wagner, 2021; Mulder, 2021), and population (Lee, 2022; Peralta and Scott, 2019). We build on this literature by developing an equilibrium framework that allows us to simultaneously analyze and quantify both the extensive and intensive channels for averting damages, and, importantly, to trade them off against regulatory costs.

We also contribute to a literature documenting frictions in mitigating or adapting to climate risk (Annan and Schlenker, 2015; Deryugina and Kirwan, 2018; Kousky et al., 2018b; Balboni, 2019; Baylis and Boomhower, 2019; Bakkensen and Barrage, 2021; Wagner, 2021; Mulder, 2021). In particular, we investigate the extent to which a second-best corrective policy can reduce the welfare losses from these frictions. In doing so, our paper relates to a body of work examining second-best corrective instruments to reduce welfare losses from externalities or internalities (Ito and Sallee, 2018; Germeshausen, 2018; Barahona et al., 2020; Kellogg, 2020).

Methodologically, we draw on work in urban economics that embeds boundary discontinuity designs in discrete choice frameworks (Bayer et al., 2007; Turner et al., 2014; Anagol et al., 2021; Song, 2021; Almagro et al., 2023). We build on these models by incorporating recent estimates of spatially-granular supply elasticities from Baum-Snow and Han (2022) to more-accurately characterize equilibrium changes in housing supply across markets.

The next section describes the institutional details of the National Flood Insurance Program, including the regulations imposed inside the SFHA and the process of generating flood maps that distinguish between SFHA and non-SFHA land. The following section describes our setting — the state of Florida — and data. In Section 4, we present reduced-form evidence of the causal effects of SFHA designation. We specify and estimate our equilibrium model of the housing market in Section 5. Section 6 simulates distributions of development and prices and discusses welfare under factual and counterfactual policies. We conclude by considering directions for future work and implications for policy.

2 Institutional Background

2.1 The National Flood Insurance Program and Special Flood Hazard Areas

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

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

In both the initial FIRM and subsequent, updated flood maps, the most important distinction for both insurance policies and floodplain regulation is between areas that are determined to be high-risk, known as Special Flood Hazard Areas (SFHAs), and those that are not. In this paper, we will refer to the SFHA as the "flood zone".[4] All new construction and substantial home improvements in the flood zone must comply with building regulations that require

[3]Communities are geographic units specific to the NFIP. They are generally municipalities or unincorporated areas of a county.

[4]In the parlance of the NFIP, a "flood zone" denotes a category of flood risk that determines insurance premium price (e.g. V, A, X, etc.). With apologies to FEMA, we reclaim this phrase with the hope of improving readability for a general audience.

that a home's lowest floor lie above the Base Flood Elevation (BFE).[5] In the flood zone, some homeowners face a flood insurance mandate and all homeowners face higher flood insurance prices for otherwise-equivalent houses.[6] Though the insurance mandate is loosely enforced, approximately 50% of homeowners in the flood zone hold a flood insurance policy, compared to 2% outside of the flood zone (Bradt et al., 2021). In Florida, annual flood insurance premiums inside the flood zone cost twice as much as outside the flood zone: $855 compared to $435.

Throughout the United States, 10% of land[7] and 6% of properties are in the flood zone (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 flood-zone-induced building requirements one of the most common forms of zoning regulation in the U.S., comparable to minimum lot area requirements, which apply to an estimated 16% of single-family homes (Song, 2021).

2.2 The Flood Mapping Process

Our spatial regression discontinuity approach relies on the assumption that flood zone delineation is a coarsening of a continuous measure of flood risk and does not follow the contours of true discontinuities in flood risk or other amenities. The validity of this assumption relies on the details of the mapping technology. In our specific context, there is substantial scope for imprecision in the historic boundaries we exploit.

[5] While popular images of elevated houses commonly show those on posts or piles, this adaptation tends to appear only in close proximity to the coast, where wave action can destroy walls. In the mostly inland areas we study, enclosed elevated foundations are more common. This approach allows garages and unfinished basements to be constructed at ground level. See Figure A.1 for an example of a house with an elevated foundation.

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

[7] Authors' calculations using 2017 flood maps.

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.[8] After floodwater heights have been mapped, the floodplain is delineated by transforming vertical flood elevation profiles into horizontal floodplain boundaries. Because the same elevation of floodwaters yields a much wider floodplain in flat than steep areas, the floodplain boundary delineation is four to five times more uncertain in flat areas, such as Florida, compared to hillier areas (National Research Council, 2009). The floodplains of inland Florida are particularly uncertain since their drainage is dominated by shallow water flow, an atypical landscape for which FEMA does not specify hydrology and hydraulics guidelines.

After the construction of the initial flood map, 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 map carve-out if homeowners have physically changed the land elevation (e.g. by adding dirt, called "fill"). According to our conversation with a floodplain manager, in the early years of the program the scale of paper maps meant that fill-based carve-outs of the flood zone had to be at least 6 acres. Because of this requirement, most houses did not find it cost-effective to pursue a carve-out. More recently, the adoption of digital maps has enabled these carve-outs at a smaller scale, and they have subsequently become more common.

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

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 the flood zone, underscoring the relevance of this form of regulation for real estate development across the state. (First Street Foundation, 2020). Florida alone accounts for 35% of the nation's NFIP policies (Lingle and Kousky, 2018).[9] We bring together four primary sources of data to conduct our analysis.

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

Figure 1a presents an excerpt of these digitized flood maps. Figure 1b shows the geographic coverage of our digitized sample. While budget constraints prohibited digitizing the entire state of Florida, we are able to obtain good coverage of most major population centers. Table 1 illustrates that our sample covers 10.5% of the land mass in Florida, but 14% of all homes, reflecting the fact that our digitized areas are more developed and populous than average.

We pair our newly-digitized historic flood maps with snapshots of flood maps for the whole

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

[10] These archival scans were downloaded from FEMA's Map Service Center https://msc.fema.gov/portal/advanceSearch. In order to maximize power, we prioritized areas with substantial new 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 from 1996 and 2017. In our digitized counties, 32% of land is in the flood zone.

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.[11] 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,[12] Table 1 illustrates that development increased substantially both statewide (2.6x) and in our sample of interest (2.5x).

Parcel Characteristics Data from the Florida Department of Revenue property tax records from 2005 to 2020 provide detailed information about structures, including sales prices and parcel outline and geolocation. We precisely geolocate the exact location of any

[11] Across Florida, the median number of raster grid cells per census tract is about 4900.

[12] Between 1980 and 2020, Florida's population more than doubled from 9.75 million to 21.5 million (US Census Bureau, 2022).

buildings on each parcel using Microsoft's open-source building footprints dataset.[13] Table 1 panel C summarizes average home prices statewide and in our sample of interest.

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

Flood Risk Model To assess the risk profile of development across policy regimes, we draw on spatially-granular estimates of flood risk from a third-party hydrological model. This model is produced by the First Street Foundation, a nonprofit organization devoted to quantifying and communicating climate risks. First Street aims to improve on government-issued risk assessments, which have been criticized for being out-of-date and inaccurate (Keller et al., 2017; Brannon and Blask, 2017; Frank, 2020b; Wing et al., 2022). First Street takes into account sources of flooding that NFIP maps ignore (e.g. rainfall), provides estimates for areas that FEMA had not been able to survey, and accounts for sea level rise due to climate change (First Street Foundation, 2020). Although First Street's model does not employ the "gold standard" of surveying that FEMA uses in the highest-risk locations, their validation exercises have achieved 80-90% flood extent similarity with historical observations and they are considered to "fus[e] the accuracy of local studies with the spatial continuity of large-scale models" (Wing et al., 2022). Nationally, First Street's model estimates that NFIP flood maps identify only 60% of areas that face a 1% chance of flooding every year (First Street Foundation, 2020). In Florida this discrepancy is smaller: only about 10% 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

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

Street model in our sample, showing that more than one-fifth of parcels are categorized differently by FEMA and First Street.

4 Causal Evidence on the Effects of Floodplain Designation

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

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

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

4.1.1 Empirical Strategy

Estimating the effect of flood zone designation on new construction presents two challenges. First, flood zone designation could be correlated with unobserved amenities, such as coastal access or views. Indeed, Table 1 indicates that land inside the flood zone is more likely to be water or wetlands and is closer to the coast. Second, flood zone designation may be endogenous to real estate construction, as the mapping process allows homeowners to deregulate parts of their properties by petitioning for map corrections or "filling" in dirt to elevate the land. Inside the flood zone, homeowners who are correctly mapped have an incentive to elevate their house to "escape" the flood zone. Homeowners who were incorrectly mapped have an incentive to petition FEMA to correct a mistake that overstates a home's risk. Meanwhile, owners of undeveloped land face no such incentives. Appendix Table A.2 shows direct evidence of such reverse causality: land that was developed as of 2004 is more likely to be remapped out of a floodplain in the next map revision than land that was undeveloped. This endogenous amendment process would lead to a mechanical negative correlation between development status and flood zone status that is unrelated to the causal effect of interest.

We address these two challenges with a spatial regression discontinuity design that leverages the first flood maps drawn in the late 1970s and early 1980s. The historic maps address concerns about reverse causality, since homeowner petitions and reactive adaptations were not reflected in the original maps: amendments and endogenous adaptation happen only after the maps are drawn. The regression discontinuity addresses omitted variables bias by leveraging the coarse classification of the flood zone and the assumption that unobservable characteristics of the land evolve smoothly through the historical flood zone 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 flood zone boundary. Formally, we estimate the following model:

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

where y_i is a characteristic of pixel i and d_i is the perpendicular distance from each pixel i to the nearest flood zone boundary, with positive values indicating that the pixel is located inside the flood zone. Our coefficient of interest is β, the magnitude of the discontinuity at the boundary. We allow characteristics y_i to vary flexibly on either side of the boundary. In our baseline specification, we specify $f(d_i)$ as a fourth-order polynomial that is allowed to differ on either side of the boundary. Results are similar under alternative local linear specifications. We cluster our standard errors at the census tract level to allow for spatial correlation in the error term. Finally, since our boundaries do not have natural segments, we include census tract fixed effects $\gamma_{j(i)}$ as a substitute for boundary fixed effects.

We estimate equation 1 on land within 2,000 feet (0.38 miles) of a flood zone boundary. Following previous work, we exclude boundaries that trace a body of water (Dell, 2010). Columns 4 and 5 of Table 1 present 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.

4.1.2 Results

We discuss our results in the context of an *intent-to-treat* framework: the treatment of interest is the initial flood zone designation, which may evolve over time. Appendix Figure A.3 documents the evolution of the relationship between initial designation and floodplain status over time.

Exogeneity of boundaries To validate our identifying assumptions, we check for smoothness in land use through the regulatory boundaries prior to flood zone designation.[14] 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 flood zone boundary. We test this by estimating Equation 1 with y_i equal to pre-period development. Figure 2a shows that pre-period development is smooth across the boundary, and the estimated coefficient, reported in Table 2, is economically small and statistically insignificant.[15] This test supports our hypothesis that the institutional details of the initial mapping process provide a compelling setting to conduct a boundary discontinuity analysis.

Development falls in the Flood Zone By 2016, we see a sharp discontinuity in development at the SFHA boundary, as shown in Figure 2b. Table 2 reports the estimated level shift at the boundary ($\hat{\beta}$), which indicates that land just inside the SFHA is 4.2 percentage points less likely to be developed than land just outside the SFHA.[16] This effect is substantial: it is 9% of the outside-SFHA mean level of development and it represents an 18% reduction as a share of total new development occurring between 1980 and 2016 just outside the SFHA.[17] The effect is driven by single family homes, which make up the majority of residences (87%) in our sample. The magnitude of the policy's negative effect indicates

[14]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.

[15]Appendix Figure A.4 and Table A.3 demonstrate smoothness in other pre-period land use outcomes. Note that in our alternative local-linear specification, the coefficient continues to be economically small but does become statistically significant.

[16]Figure 2b documents a striking pattern of land use on either side of the SFHA boundary. Moving away from the boundary further into the SFHA, development decreases sharply for about 400 feet before leveling out. Because this pattern is asymmetric, we do not believe it is driven by measurement error. Instead, it may be driven by a higher prevalence of wetlands deep inside the SFHA, which is also apparent in the pre-period land use (see Appendix Figure A.4).

[17]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.

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

Prices increase in the Flood Zone The effects of floodplain regulation on house price are *ex-ante* ambiguous. On the one hand, by suppressing demand for regulated houses, flood zone designation could push prices down. On the other hand, by requiring suppliers to employ costlier construction methods, flood zone designation could push prices up. Figure 3, which plots the estimated coefficients for the sales price outcome, indicates that supply must shift inward substantially as a result of the policy: though we observe a large decrease in development, prices are non-decreasing through the boundary. In fact, our point estimates suggest that prices are 6% higher inside the flood zone, 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 a flood zone. These impacts are again driven by single family residential homes (Figure 3b).[18]

Our evidence suggests that this positive price effect should indeed be interpreted as an inward shift in supply. We rule out compositional changes in housing characteristics, including house elevation, as drivers of our result. In Appendix Table A.4, we report estimates using sales prices residualized of a rich set of observable characteristics of each house and show that the positive price effects persist. We also see similar effects on prices for homes built before and after the introduction of the flood zone regulations. We provide additional evidence to rule out changes in amenities driving a positive price effect: our results are robust to excluding areas close to the coast (column 5 of Table 2). Figure 3c shows that vacant land prices

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

decrease by 10% across the flood zone boundary, a pattern that would not be consistent with increases in amenities.

Robustness Columns 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.5 shows corresponding figures. We observe similar effect sizes on both quantities developed and prices if we estimate a linear function of the running variable estimated on a narrower bandwidth (500 feet) or if we estimate a local linear regression instead of a fourth order polynomial using the MSE-optimal bandwidth proposed by Calonico et al. (2014).

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

The preceding results indicate that regulations impose costs on developers that shift supply inward. In this section, we investigate whether these regulations also reduce damages on the intensive margin of elevation.

4.2.1 Empirical Strategy

Following Wagner (2021), to estimate the effect of elevation 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 flood zone:

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

where y_i is an outcome of interest (elevation, insurance payouts, 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.[19] We scale payouts by amount of purchased insurance coverage; since individual claims cannot be linked to individual policies we therefore estimate this model at the census-tract-by-flood-zone-by-relative-year-built level.[20] The sample for this analysis includes all counties in Florida. We cluster standard errors at the census tract level. In order to increase power, we also estimate a binned specification: we restrict to houses built within 10 years of the date a community joined the NFIP and estimate the following model, again clustering standard errors at the census tract level:

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

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

4.2.2 Results

In Figure 4 we present the coefficient estimates on relative year from the event study specification (Equation 2). Variable means and regression coefficients from the binned specification

[19] Census tracts are smaller than communities.

[20] We estimate these models on two datasets. The first, derived from NFIP claims and policy data, describes elevation status, insurance payouts, and policy cost between 2010 and 2018. The second dataset describes house sale 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.5 presents summary statistics for the datasets used in this analysis.

[21] Our results indicate smooth trends in elevation 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. Appendix Figure A.6 shows that these results are robust to a difference-in-differences specification which adds a comparison to houses outside the flood zone.

Building regulations increase elevation and reduce flood damages Figure 4a shows that building regulations increase the share of homes built above the modern-day Base Flood Elevation (BFE) from 60% to 90%. We draw two conclusions: first, regulations increase elevation substantially, and second, a large share of homes were already built above the BFE. Our results suggest our measurement of pre-period elevation is most likely driven by areas where the BFE is sufficiently low that homes are built above it in standard construction practice. We arrive at this conclusion using two pieces of evidence. As we document below, consumers are unwilling to pay for elevation, meaning that costly elevation will not be provided absent regulation. Second, we find that the pre-period elevated homes are concentrated in areas with low flood risk, and hence low Base Flood Elevations (Appendix Figure A.8).

Figure 4b shows that the introduction of building regulations causes insurance payouts to fall by \$1.60 per \$1000 of coverage. At average coverage levels, this indicates that the increase in elevation mandated by the regulations does indeed generate social value in damage reduction across all houses in the flood zone of about 3% of the average home value (using a 5% discount rate).

House prices are unchanged despite substantial changes in damages Despite their higher social value, the prices of elevated houses are not significantly different from those of non-elevated houses. The point estimate of the increase in house price for elevated houses is -0.7%, or -\$1400. Appendix Figure A.7 shows that this result is robust to residualizing the sales price against fourth-degree polynomials in parcel size and total living area, and fixed effects for county-by-month-of-sale and year of construction. Appendix A.4.4 shows in a stylized model that a zero effect on price implies zero willingness-to-pay for reduced risk if all other consumer-welfare-relevant aspects of houses remain constant. The null effect we find on price is not driven by a failure of insurance prices to reflect changes in risk, as premiums for elevated homes incorporate 75% of the reduction in risk (Figure 4c).

This result confirms existing work documenting a wedge between social and private valuations of flood risk in our setting (Gallagher, 2014; Kousky et al., 2018b; Bakkensen and Barrage, 2021; Wagner, 2021; Mulder, 2021). The fact that safe houses provide no private value to consumers indicates the presence of frictions, including behavioral frictions such as risk misperception or myopia, or moral hazard from consumer expectations of government aid in case of disaster (or both). Regardless of the source, this wedge between private and social value will yield construction of inefficiently-risky houses, absent regulation.

5 An Equilibrium Model of the Housing Market

Our reduced-form results inform us about the policy's effect on both flood damages and regulatory costs, and show the potential for the policy to improve welfare. Floodplain regulation suppresses construction and increases elevation in high-risk areas. The policy therefore potentially decreases damages on both extensive and intensive margins of construction. However, our results also indicate that the costs of mandatory elevation are substantial, since flood zone designation substantially decreases new construction without a commensurate fall in price. Finally, although elevation reduces damages, consumers are not willing to pay more for elevated houses, suggesting a role for policy to restore efficient risk allocations.

While informative, these results alone are insufficient to quantify the total effect of the policy on either damages or overall welfare. The policy's effects depend critically on the location of counterfactual construction: if the regulation shifts construction to equally-risky areas, damages will not fall. Counterfactual risk will also depend on the joint distribution of risk and amenities, since more-desirable locations will attract more counterfactual construction. Additionally, the large size of the regulated area — nearly one-third of all land in our digitized sample is currently designated as a flood zone — may introduce spillover effects on the unregulated locations. Higher demand and consequently higher prices outside the flood zone could mitigate the incentive to relocate and impose pecuniary externalities on unregulated consumers. Moreover, while our empirical results indicate that floodplain regulation shifts supply, we require a model to quantify the regulation's costs to developers and consumers. To account for counterfactual location and spillover effects, quantify the regulation's costs, and compare welfare across current and counterfactual policies, we specify and estimate a model of residential choice and real estate development.

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

5.1 Residential choice

Motivated by our result in Section 4.2 that consumers are indifferent between elevated and non-elevated homes, we model consumers as choosing among uniform homes across differentiated locations. We further assume that consumers do not privately value differences in flood risk when deciding between houses, consistent with prior work and with our results in Section 4.2. Each individual i makes a discrete choice of where to live within market m, which we take to be a county.[22] Locations are differentiated goods characterized by tract j and flood zone designation status z. Census tracts are small geographic units of analysis: the average county in Florida has 63 census tracts, each containing roughly 1,600 residential structures. Following the standard discrete choice framework of Berry et al. (1995), the indirect utility of individual i living in location jz is given by:

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

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

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

$$u_{ijz} = \alpha^D p_{jz} + \phi_C SFHA_z + \phi_B SFHA_z + \xi_{jz} + \varepsilon_{ijz} \tag{5}$$

where p_{jz} is the log price of housing and we have introduced $\phi_B SFHA_z$ as a potential channel for flood zone designation to affect beliefs about flood risk. Modeling the debiasing effect

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

of the flood zone as an on-or-off label captures the fact that flood maps do not give any indication of the expected flood damages of a particular location; they simply indicate a coarse level of risk.

We cannot separately identify real costs ϕ_C and debiasing labels ϕ_B since both vary with floodplain status. We therefore combine the real cost term $\phi_C SFHA_Z$ and debiasing effect term $\phi_B SFHA_Z$ into an overall "floodplain effect" term $\phi SFHA_z$.

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

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

where p_{jz} is the log price of housing in location jz,[23] $SFHA_z$ indicates whether z is regulated as a floodplain, ξ_{jz} are unobserved amenities, and ε_{ijz} is an i.i.d. preference shock. We assume ε_{ijz} is distributed according to an Type 1 Extreme Value Distribution.

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

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

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

[23]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.

5.2 Housing supply

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

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

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

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

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

5.3 Equilibrium

In equilibrium, house prices and location decisions adjust so that the quantity of housing supplied in each location equals the number of individuals choosing to live there. Specifically:

$L_{jz}\Phi\left(\frac{p^1_{jz}-\psi E^1_{jz}-\mu_j-\eta^1_{jz}}{\sigma_j}\right) = Q_{jz} = N_m s_{jz}$ where L_{jz} is the total amount of land in location jz, Q_{jz} is the quantity of developed land in jz, and N_m is the number of households in the market.

5.4 Estimation and Results

A key challenge for estimation arises from the fact that amenities ξ_{jz} and construction costs η_{jz} may be correlated with floodplain status $SFHA_z$.[24] The correlation between $SFHA_z$ and ξ_{jz} and η_{jz} occurs because flood zone status may be correlated with other amenities, like coastal access, or challenges to construction, like wetlands. We use a variation of our spatial discontinuity assumption to account for the endogeneity between flood zone status and unobserved amenities ξ_{jz} and construction costs η_{jz} to estimate regulatory costs ψ and ϕ. We describe the estimation procedure in more detail below.

5.4.1 Demand-side identification

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

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

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

$$\hat{s}_{jz} = \frac{Q^{2016}_{jz}}{\sum_{j' \in J_m, z' \in \{0,1\}} Q^{2016}_{j'z'}} \tag{10}$$

[24] Another important source of endogeneity is the correlation between p_{jz} and ξ_{jz} and η_{jz}, as locations with higher amenities and higher construction costs have higher prices in equilibrium. However, because we calibrate supply and demand elasticities rather than estimate them in-sample, we avoid this additional estimation challenge.

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

Simply estimating Equation 9 via OLS would yield a biased estimate of the regulatory demand cost ϕ because of the correlation between $SFHA_z$ and amenities ξ_{jz}. We address this challenge in our boundary discontinuity analysis by identifying the effect of flood zone designation in the limit on either side of the boundary. We therefore estimate our model of housing choice by matching the cross-boundary differences we document in the descriptive analysis. First, we restrict to a narrow window around the boundary (100 feet). Because even this narrow window does not capture differences at the limit, we then separate the unobserved amenity term ξ_{jz} into a nuisance term, which can be correlated with flood zone status, and a component of ξ_{jz} that is uncorrelated with flood zone status. We require that the price difference across boundaries net of the nuisance term match the price difference estimated in our spatial RD, denoted $\beta^{p,2016}$. Appendix A.6 specifies our moment conditions in more detail.

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

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

[25]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.

[26]In Appendix Table A.7 we present a set of results in which we estimate α^D using variation in price in our sample, which yields a value of approximately -0.8.

We have imposed the assumption that consumers do not respond to expected flood damages. If this assumption is incorrect, any increases in flood expenses to which consumers *do* react will load onto the flood zone term if they change discretely at the boundary (e.g. through insurance premiums) or onto the unobserved amenity terms ξ_{jz} if they do not change discretely at the boundary. Underestimating consumer responsiveness to risk would lead us to overstate the gains from regulation in our normative analysis, but would not impact any conclusions about the positive effects of the policy on construction, damages, or house prices.

5.4.2 Supply-side identification

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

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

5.4.3 Results

After calibrating α^D and matching μ_j and σ_j to estimates from Baum-Snow and Han (2022), we estimate the flood zone cost terms ϕ and ψ jointly via the Generalized Method of Moments using the boundary discontinuity moments described above and detailed in Appendix A.6. Appendix A.7 includes more details about the data restrictions used for estimation.

Model fit While our structural error terms allow our model to achieve a perfect fit to the observed data, we can assess the model by investigating the extent to which we rely on structural error terms to achieve that fit. In Figure 5 we plot observed price in 1980 and 2016 against the modeled prices that would rationalize observed market shares in each year, *if we omitted the structural error terms.* This exercise does not use our estimated demand model at all; it assesses the role of supply structural error terms based only on our estimated supply parameters and our method of simulating an equilibrium. We see a strong correlation between model-generated and observed prices, indicating our supply curve is reasonable.

Parameter estimates Table 4 presents the parameter estimates for our baseline specification.[27] Standard errors from bootstrapping with 100 replications are given in parentheses. Large standard errors are due to small sample size, a result of the cuts made when restricting to census tracts with sufficient data. We focus on the point estimates. As expected, flood zone status is disliked by consumers (negative ϕ) and imposes costs on developers (positive ψ). On the demand side, the magnitude of the flood zone cost implies that consumers are willing to pay 23% more to avoid living in a floodplain and it costs developers 25% more in construction costs to build a compliant home. We arrive at these numbers by rationalizing the changes in quantity and price around the regulatory boundary with estimates of how

[27] Appendix Table A.7 presents parameter estimates under alternative calibrations of demand elasticity α^D.

consumers trade off home prices with other attributes (our calibrated α^D) and the elasticity of housing supply (which is determined by μ_j and σ_j).

The result that consumers are willing to pay 23% more to avoid floodplain regulations is at the high end of a range of recent estimates, which find floodplain discounts ranging from 1 to 28% (Indaco et al., 2019; Gibson and Mullins, 2020; Hino and Burke, 2021; Lee, 2022). This difference could be attributed to our time period and setting: residents living in flood-prone Florida today may be particularly sensitive to signals of risk as awareness of climate change grows (Bernstein et al., 2019). Nevertheless, a 23% premium on avoiding the floodplain far exceeds the risk-reducing benefits of relocation: the risk difference across boundaries is equivalent to just over 11% of house price, just half of the estimated demand effect. This large discount may arise from consumers' strong dislike of the bureaucratic burdens of complying with floodplain regulations, which are substantial.[28] Alternatively, the flood zone designation may cause consumers to over-update their beliefs about risk, as flood maps do not communicate risks in terms of expected damages.

Turning to supply, we estimate that regulations produce a shift in the supply curve that corresponds to a 25% increase in construction costs in the flood zone.[29] The magnitude of this effect is large, but within the plausible (albeit wide) range of estimates of the effect of building codes and zoning regulations on construction costs: from 5% to 42% (Listokin and Hattis, 2005; Emrath, 2021; Song, 2021). The wide variation reflects both differences in strategies to estimate regulatory costs and variation in the types of regulations imposed. Yet, our informal interviews indicated that a 25% increase in costs is reasonable. For example, the elevation requirement may necessitate a stem wall, which can add $100,000 to the cost of a new build. The results in Section 4.2, combined with estimates of average risk levels from our hydrological model, indicate that elevation reduces expected flood damages by 55%

[28]In particular, non-elevated 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.

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

among compliers. It follows that elevating an average home in a flood zone reduces damages by approximately 11% of house value, so on average, the benefits of mandatory elevation are less than half the costs. However, this comparison masks substantial heterogeneity across locations within the flood zone: elevating homes in the highest-risk areas will generate social value, while elevating relatively-safe homes will incur net social costs.

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

6 Model-Informed Estimates of The Effects of Floodplain Regulation

We use our model to investigate the impact of floodplain regulation by comparing observed outcomes to counterfactual outcomes simulating the absence of the policy. We also compare the effect of the observed policy to the effect of a counterfactual first-best policy that imposes corrective taxes in the spirit of Pigou (1920) on consumers that are equal to the present discounted value of expected damages in each tract-by-flood-zone.[30]

We compute counterfactual outcomes by searching for a price vector $\tilde{P}_{jz}^{2016}$ that equates

[30]This policy could potentially be implemented as mandatory flood insurance with actuarially-fair rates at the property level. Recent changes to the National Flood Insurance Program moved the program in this direction — from a very coarse to a more granular pricing scheme — though the prices likely will not grow to reflect the level of damages estimated by First Street Foundation (Flavelle, 2021).

housing supply and demand:

$$L_{jz}\Phi\left(\frac{\ln\left(\tilde{P}_{jz}^{2016}\right)-\hat{\psi}E_{jz}^{2016}-\mu_j-\eta_{jz}^{2016}}{\sigma_j}\right)= \tag{11}$$

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

given our calibrated and estimated parameters $(\alpha^D,\hat{\mu}_j,\hat{\sigma}_j,\hat{\phi},\hat{\psi})$, our recovered values of ξ_{jz} and η_{jz}, and counterfactual values for the flood zone designation $SFHA_z$ and taxes τ_{jz}.[31] Under the taxation policy, taxes vary with both location and elevation status. In this scenario, to capture the fact that consumers would demand and developers would endogenously supply elevated houses in some locations, we impose that locations elevate when it is socially-efficient to do so. That is, we assume that houses are elevated when the gains from lower tax rates on an elevated house exceed the elevation costs. This approach generates counterfactual prices and quantities of development in each of the locations jz. Our counterfactuals assume a closed city: we assume the 2016 population in each of our ten counties remains constant across counterfactual scenarios.[32]

First, we quantify the magnitude of flood-risk reduction each policy achieves – the intended goal. We also decompose the risk reduction into components due to relocation to safer areas versus elevation in risky areas. In addition to our model of housing supply and demand, this risk quantification exercise crucially relies on our hydrological-model-based estimates of flood risk by location and our estimates from Section 4.2 on the risk-reducing benefits of elevation.[33] Appendix A.8 describes how we compute expected damages in further detail.

[31] Under the observed policy, we set τ_{jz} equal to 0 to reflect the fact that the main driver of insurance premiums is flood zone designation, so consumers face no other prices that vary with flood risk.

[32] We estimate the parameters on 11 counties but encountered difficulties resolving the counterfactual equilibria for Miami-Dade county. Therefore, we omit it in these counterfactuals.

[33] 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

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

6.1 Policy Effects on Flood Damages

Our quantification of each policy's effect on flood damages focuses on two main outcomes. First, we study the approximate number of houses in the regulated area (observed flood zones).[34] This analysis extrapolates the effect of our regression discontinuity away from the boundary to investigate the extent to which floodplain regulation reduces building in regulated areas market-wide. Because regulated areas are on average riskier than unregulated areas, shifting development from regulated to unregulated areas will likely reduce damages, but the magnitude is uncertain and depends on the joint distribution of amenities and flood risk. Our second primary outcome therefore quantifies the reduction in damages directly, including both relocation and elevation. To complete our analysis of the positive effects of each policy on housing markets, we also report total number of elevated houses and average home prices inside and outside of regulated areas.

We estimate the effect of floodplain regulation by setting $SFHA_z = 0$ and $\tau_{jz} = 0$ to simulate outcomes in the absence of any floodplain regulation. On the supply side, this amounts to eliminating all flood-zone-imposed building regulations: there is no requirement to elevate to a minimum level. On the demand side, we think of eliminating the existing flood zone

important components of flood risk, e.g. pluvial (rainfall) risk. The First Street model incorporates climate change predictions as well as all major flood drivers in a novel peer-reviewed approach. Second, the First Street flood risk estimates provide granular estimates of the average annual loss for each parcel. First Street's estimates are increasingly used in economics research as an independent assessment of flood risk (Bradt et al., 2021; Mulder, 2021; Sastry, 2021).

[34] 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).

regulations as a policy regime in which consumers receive no signals indicating higher risk and have no mandate to purchase insurance or other hassles of living in the flood zone. Under this policy regime, there are no differences in insurance prices across areas. We also estimate the effect of a counterfactual policy that both eliminates all flood zone regulations and also imposes corrective taxes equal to the expected damages by location and elevation status. In our model, this counterfactual yields socially-optimal development decisions.

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

The policy achieves its goal of reducing flood damages via both relocation and elevation. Overall, the policy reduces the present discounted value of damages by about $7,800 per newly-developed house, which amounts to a 60% reduction in damages. Relocation contributes about one-quarter of that effect, while the requirement to elevate houses contributes the remainder.[35] The policy's benefits in terms of reduced damages are substantial, both in absolute numbers (a decrease in $3.5 billion of expected damages per county), and in comparison to alternative policy instruments: the number of homes relocated exceeds the total

[35]We calculate our decomposition based on percent rather than level reductions in damages, since changing either elevation or location behavior first dramatically changes the base damages from which we compute the effect of the other behavior in levels.

number of houses removed from risky areas by the NFIP's home buyouts program across the entire nation over its lifetime (Frank, 2020a).

The *status quo* policy reduces damages by approximately the same amount as the corrective tax policy. However, the corrective tax policy achieves these same reductions in damages with fewer distortions to housing decisions. Figure 6 illustrates the mechanism driving these differences. Relative to the *status quo* policy, the tax policy allows more construction and damages in moderately-risky areas while reducing construction and damages in the risky tail. It achieves this result by imposing regulatory costs that are proportional to flood risk, rather than imposing regulations uniformly across the designated flood zone. These findings suggest that although the current floodplain regulation policy generates its intended social benefits in the form of substantially-reduced damages — and even achieves the socially-optimal level of those reductions — those same benefits could be achieved with smaller distortions under a more-targeted policy. These distortions may be significant. Thus, a key question is to what extent the costs of these housing market distortions offset the policy's benefits.

6.2 Policy Effects on Welfare

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

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

Though government policies such as subsidized insurance premiums may shield consumers from some flood risk, the social planner values all flood costs. We therefore define the social welfare function for consumers — that is, consumer surplus augmented by the social value of externalities and internalities and any government revenue — as:

$$\Sigma_{jz} N_{jz} \left(\alpha^D \ln(P_{jz} + D_{jz} + R) + \phi_C SFHA_z + \xi_{jz} + \varepsilon_{ijz}\right) \tag{12}$$

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

Following Train (2009), we compute per-person consumer surplus in each market as $CS_i = \frac{-\bar{P}}{\alpha^D} \ln \Sigma_{j,z} \exp(\delta_{jz})$, where $\bar{P}$ is the average house price in levels. In the fully-welfare-relevant case we take $\delta_{jz} = \alpha^D p_{jz} + \phi SFHA_z + \xi_{jz}$; in the debiasing-plus-premiums case we take $\delta_{jz} = \alpha^D p_{jz} + \phi_F SFHA_z + \xi_{jz}$, where ϕ_F reflects our estimate that the PDV of insurance premiums amounts to about 5% of average house price.

We account for the impact of each policy on producer surplus by computing the change in producer surplus relative to the unregulated baseline. Specifically, for each observation we compute $Q\Delta P + \frac{\Delta Q \Delta P}{2}$, where Q is the number of newly-constructed houses and ΔP is the price change relative to no-flood-zone in levels, plus, in the case where elevation changes as a result of the policy, the cost of elevation in dollars (approximated as ψP_{jz}).[36] We then define total social welfare as social consumer welfare plus producer surplus.

[36] We take this approach rather than calculating producer surplus directly from the model in order to avoid complications introduced when converting between log dollars and level dollars. This approximation does not impact our overall welfare or policy conclusions.

We report results of these welfare calculations in Table 6 by comparing total social welfare under the observed regulation and the counterfactual corrective tax policy to the unregulated benchmark. Even if the floodplain demand cost ϕ mostly captures a debiasing channel rather than imposing costs on consumers, we estimate that the policy does not substantially improve social welfare: at most it increases welfare by \$140 million per county. This is just over 10% of the benefits that could be generated under the corrective tax policy. The poor performance of the *status quo* policy is further reinforced if the policy introduces non-financial costs to consumers rather than debiasing them. We ignore any potential effects of each policy on net government revenue through the channel of subsidized premiums or disaster aid; accounting for the cost of public funds would further strengthen the performance of the corrective tax policy relative to the *status quo*.

Our results indicate that current policy achieves at best a small fraction of the maximum possible benefits of regulation. Because of the wedge between the private and social value of flood safety, risk would be inefficiently-high in the absence of any policy intervention. Indeed, Table 6 shows that a well-targeted policy intervention can substantially improve welfare, by more than \$2,700 per newly-developed house. However, because existing policy applies strong incentives equally to more- and less-risky areas, the policy may yield *too little* construction in moderately-risky areas, while still permitting too much construction in the riskiest locations. A corrective tax policy could improve upon the existing policy by targeting strong elevation and relocation incentives to the riskiest areas and foregoing onerous regulation in safer areas.

Our results highlight an important policy lesson. Some proposals (e.g. home buyouts (Frank, 2020a)) focus purely on the savings that could be achieved by reducing risk. However, equivalent risk reductions can entail substantially different costs. A strong but poorly-targeted, undifferentiated policy may overcorrect risk in some areas, imposing costs on developers and consumers that are not warranted by the benefits. Our results underscore the gains to transitioning from a blunt attribute-based regulation to a more flexible policy instrument. While the NFIP is indeed transitioning to a more flexible pricing system (Risk Rating 2.0), consumers may be insensitive to premium increases without further efforts to increase salience. NFIP premiums will also need to grow substantially to accurately reflect risk: expected damages are currently estimated to exceed current premiums in Florida by a factor of four (First Street Foundation, 2021). Moreover, the costly elevation mandate is still in place for all areas where FEMA calculates there is a more than 1% chance of flooding each year. In many less-risky parts of these areas, our estimates indicate elevation is not cost-effective and should not be required. With appropriate changes, however, we find that floodplain regulation may be able to achieve substantial welfare gains.

7 Conclusion

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

We find that local to the regulatory boundary, floodplain regulation decreases construction and increases house prices. We also find that flood-safe building codes reduce flood damages among all houses by about 3% of house value, but these reductions are not privately valued by consumers.[37] Using our model to interpret our reduced-form results, we find that floodplain regulation in Florida reduces damages by 60%, with one-quarter of the effect accruing through relocation and the remaining three-quarters through elevation. Though the policy

[37]Elevation reduces damages for a given elevated house inside the flood zone by about 11% of house value.

achieves substantial benefits in terms of flood damage reductions, it is poorly-targeted: in some areas, flood zone designation induces responses whose costs exceed their benefits, and in others, it does not go far enough. Because of this, at best the policy only achieves about one-tenth of the possible regulatory gains, and at worst may actually harm welfare.

Our findings have important implications for policies to promote the resilience of cities in the face of sea level rise and other climate-change-induced increases in flooding. They suggest that current policy instruments may perform substantially worse than flexible instruments that allow consumers to live in risky areas if they are willing to pay the full social cost of doing so.

We leave a few areas open for future work. Though we model flood risk as static after 2050, in reality flood risk could increase further over time. Regulating durable construction with evolving flood risk may raise interesting dynamic considerations. Furthermore, although we provide suggestive evidence of frictions in the market for flood safety, we leave the challenge of identifying the welfare costs of each separate friction to future work.

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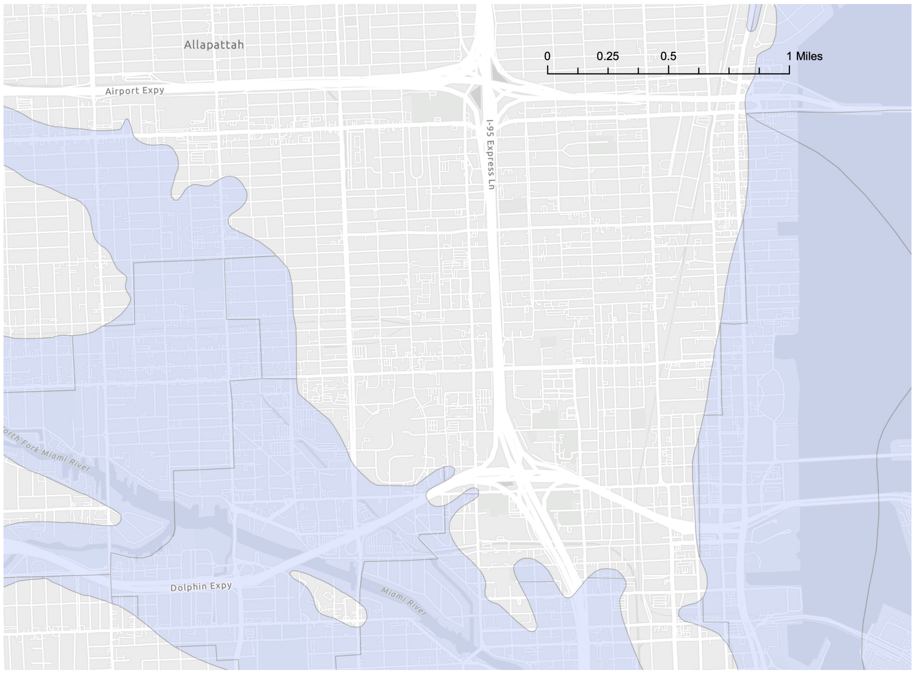
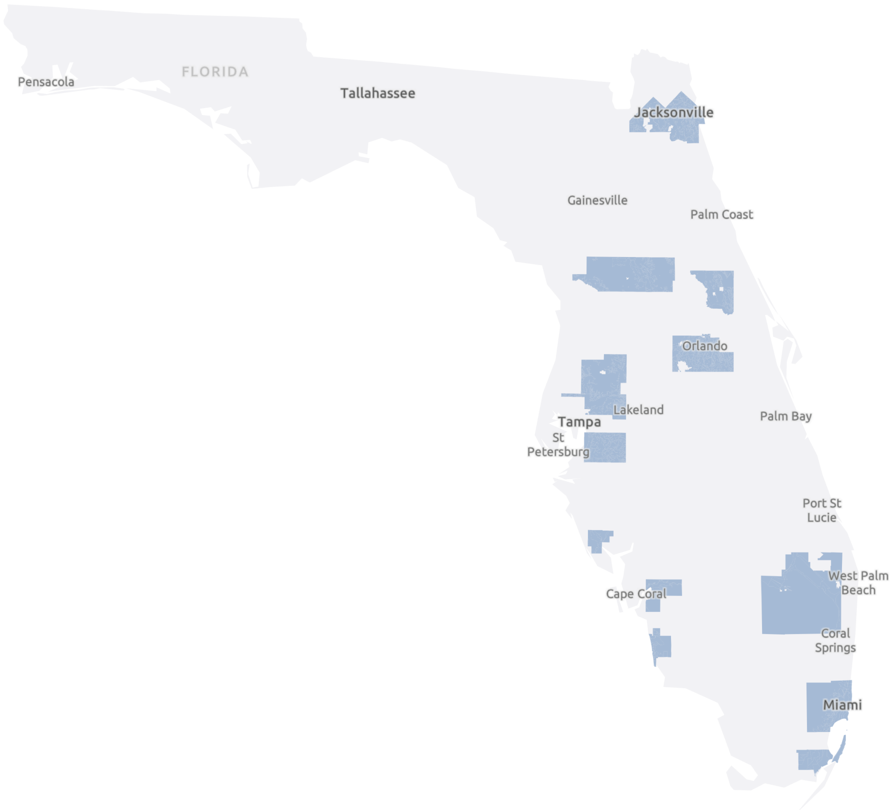
Tables and Figures

Figure 1: Digitized Flood Maps

(a) Digitized Map of Miami (1978)



(b) Coverage of Digitized Maps

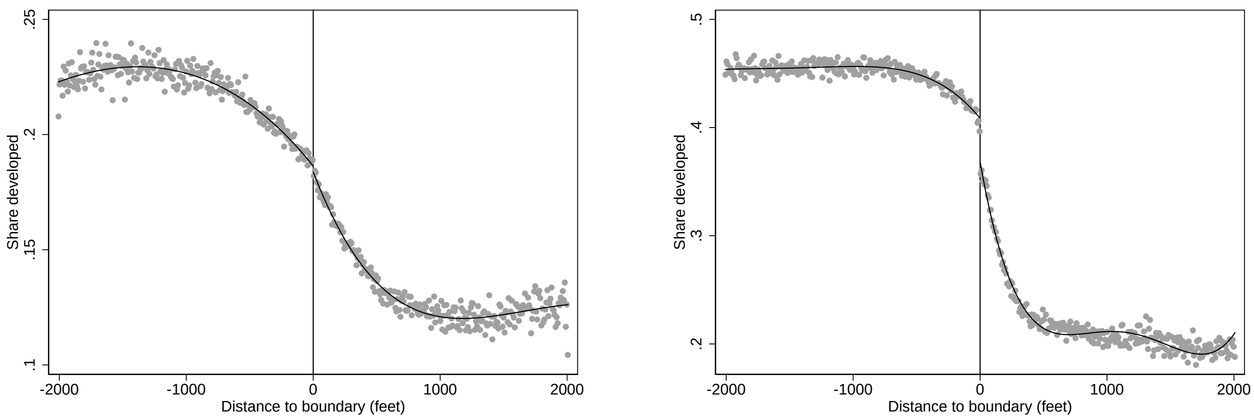


Notes: Figure (a) shows an example of a historical flood map in Miami. flood zones are depicted in blue. Figure (b) displays the coverage of our digitized sample.

Figure 2: RD Estimates: Development

(a) Pre-Period Share Developed (about 1980)

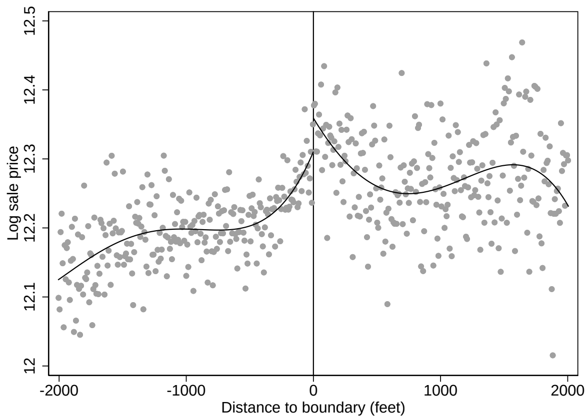
(b) Share Developed in 2016



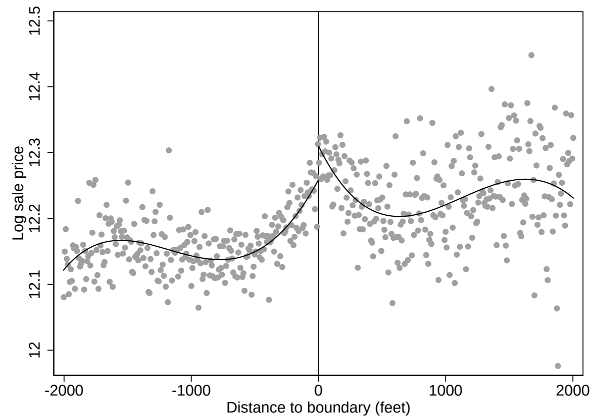
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the flood zone boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Sub-figure (a) plots the share of land that was developed as of the late 1970s and early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

Figure 3: RD Estimates: Prices

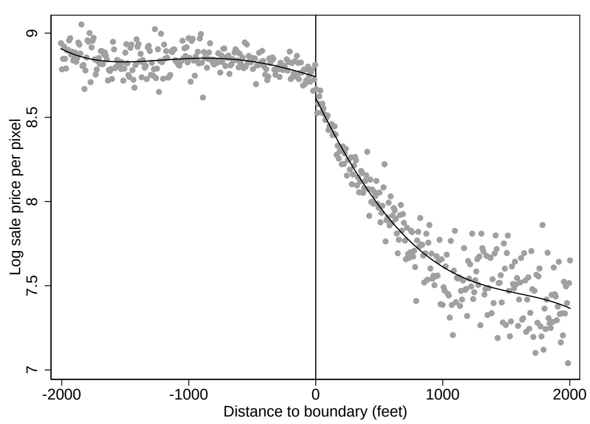
(a) Log Sale Price (2005-2020)



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

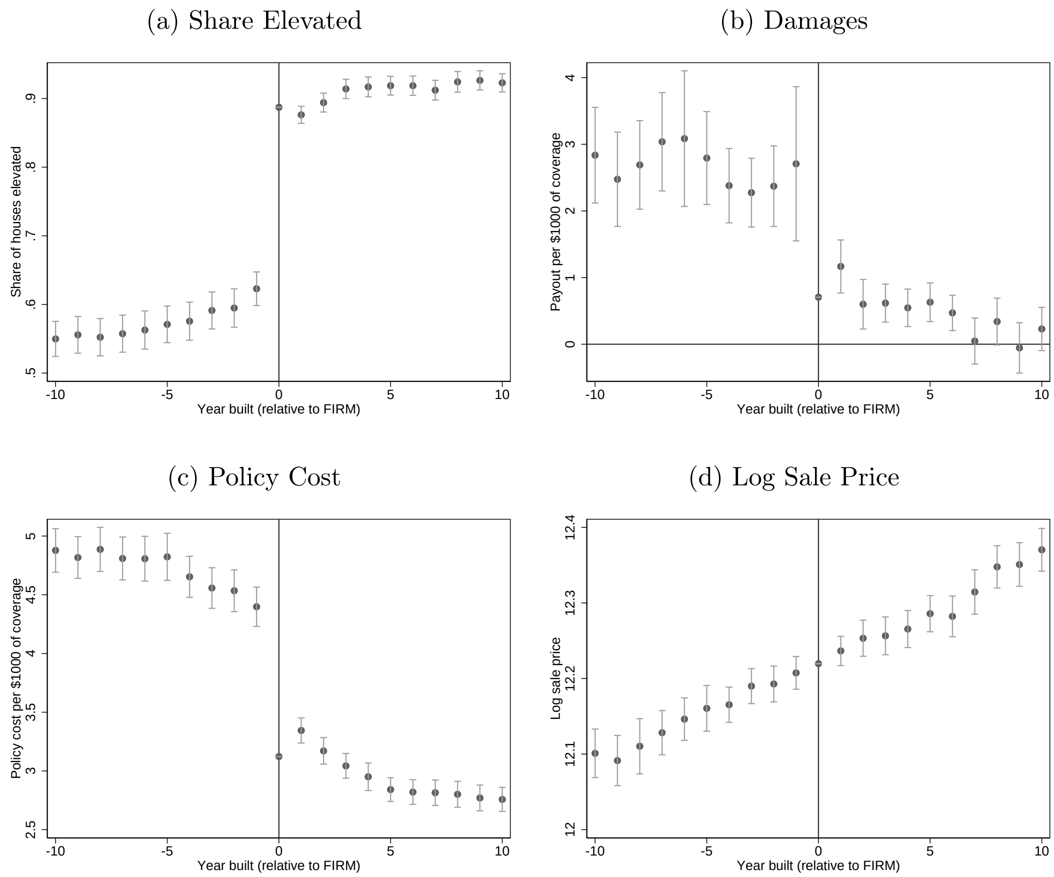


(c) Log Vacant Land Sale Price (2005-2020)



Notes: Figures present RD plots with a fourth order polynomial fit on either side of the flood zone boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. 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.

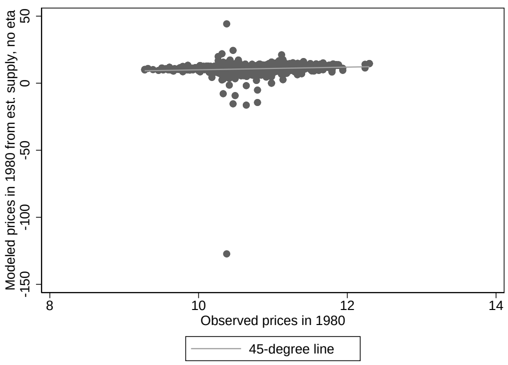
Figure 4: NFIP Enrollment Year Event Study Estimates



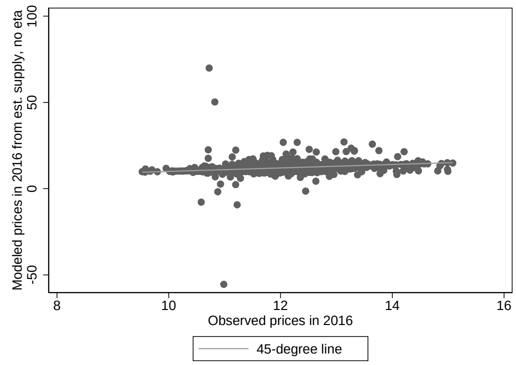
Notes: Figures present coefficients on bins of year built relative to the FIRM year (the year of enrollment in the National Flood Insurance Program, at which time building codes began to be imposed on newly-constructed housing). Sample is restricted to single-family residences inside flood zone flood zones. Sub-figure (a) shows share of NFIP policies that indicate elevation above the modern-day Base Flood Elevation. Sub-figure (b) shows insurance payouts from 2010 to 2018 in each census tract as a share of total dollars of coverage across the time period in that census tract. Sub-figure (c) shows total policy costs to consumers from 2010 to 2018 in each census tract as a share of total dollars of coverage across the time period in that census tract. Sub-figure (d) shows the log sale price in 2010 dollars between 2005 and 2020 in each census tract.

Figure 5: Supply Model Reliance on Structural Error Terms

(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 estimated supply curve, without estimated idiosyncratic construction costs. Sub-figures (a) and (b) present the prices in 1980 and 2016 that would rationalize the observed quantities of development, using the estimated parameters (μ_j, σ_j, ψ) and observed elevation decisions, if the model omitted structural supply costs.

Figure 6: Expected new annual damages by risk bin: *status quo* vs. first-best corrective tax policy

../external/estimates/new_dev_damages_obs_v_fb_ventiles_top_all.pdf

Notes: Figure plots expected annual damages on newly-constructed houses by risk bin under the observed and counterfactual tax policies. Sample includes both flood zone and non-flood-zone land.

Table 1: Summary Statistics of the Spatial RD Sample

	Florida	Digitized map sample		Boundary sample	
		Outside historic flood zone	Inside historic flood zone	Outside historic flood zone	Inside historic flood zone
	(1)	(2)	(3)	(4)	(5)
Panel A. Development					
Share developed in 1980	0.056	0.116	0.124	0.243	0.178
Share developed in 2016	0.145	0.313	0.243	0.473	0.313
Single family homes	5,175,979	552,230	191,507	159,607	80,218
Single family share of structures	0.662	0.872	0.869	0.861	0.869
Share post-FIRM	0.808	0.777	0.648	0.676	0.618
Panel B. Other characteristics					
Share wetlands	0.348	0.205	0.449	0.101	0.343
Share water	0.069	0.015	0.163	0.011	0.098
Distance to coast (miles)	10.7	7.9	6.4	7.7	7.9
Panel C. Prices					
Median house price 1980	47,459	48,198		46,912	
Median house price (2005-2020)	167,233	177,673	274,065	188,148	252,935
Median single family house price (2005-2020)	182,452	179,419	298,716	190,204	277,492
Panel D. Risk					
FEMA flood maps					
Land share in flood zone as of 1996	0.379	0.031	0.798	0.046	0.723
Land share in flood zone 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 flood zone 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 flood zone mean	Polynomial	Local linear regression Rectangular kernel, constant band-width	Local linear regression Triangular kernel, optimal band-width	Polynomial excl. coastal areas
	(1)	(2)	(3)	(4)	(5)
Panel A. Current flood zone status					
In flood zone as of 1996	0.060	0.430	0.405	0.268	0.454
		(0.020)	(0.019)	(0.014)	(0.024)
In flood zone as of 2017	0.130	0.292	0.283	0.207	0.300
		(0.019)	(0.017)	(0.013)	(0.023)
Panel B. Historical land use					
Share of land developed in 1980	0.235	-0.003	-0.005	-0.006	-0.003
		(0.002)	(0.002)	(0.002)	(0.002)
Panel C. Modern land use					
Share of land developed in 2016	0.470	-0.042	-0.044	-0.037	-0.040
		(0.005)	(0.005)	(0.004)	(0.006)
Share of land covered by a building footprint	0.264	-0.029	-0.026	-0.025	-0.027
		(0.004)	(0.004)	(0.004)	(0.004)
Share of land covered by a single family home	0.101	-0.015	-0.013	-0.010	-0.013
		(0.003)	(0.002)	(0.003)	(0.003)
Share of land covered by wetlands	0.098	0.110	0.096	0.071	0.113
		(0.007)	(0.007)	(0.006)	(0.009)
Panel D. Prices					
Log house price	12.151	0.065	0.063	0.066	0.049
		(0.022)	(0.021)	(0.019)	(0.026)
Log single-family house price	12.073	0.056	0.060	0.064	0.051
		(0.018)	(0.018)	(0.015)	(0.021)
Log vacant land price (per 30X30m pixel)	8.806	-0.102	-0.062	-0.070	-0.126
		(0.048)	(0.032)	(0.030)	(0.056)

Notes: Table displays estimates of equation (1). Outside of flood zone 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 flood zone, Pre-FIRM	(2) Event study
Share elevated	0.57	0.271
		(0.013)
Insurance payouts (per $1000 of coverage)	$2.94	$-1.61
		(0.40)
Policy cost (per $1000 of coverage)	$4.80	$-1.21
		(0.075)
Log house price (sold 2005-2020, in 2010 $USD)	12.21	-0.007
		(0.009)

Notes: Table presents variable means and coefficient estimates on Equation 3 from the event-study analysis 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. Sample includes all single-family residences in Florida. Standard errors are clustered at the census tract level.

Table 4: Parameter Estimates

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

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

Table 5: Counterfactual Outcomes

	Modeled Outcomes			Change relative to Unregulated Counterfactual	
	Unregulated	Current Policy	Corrective Tax	Current Policy	Corrective Tax
	(1)	(2)	(3)	(4)	(5)
New constr. on land in observed floodplain					
Approximate N Houses	757,814	640,676	698,942	-117,138	-58,872
				-15.5%	-7.8%
New constr. on all land					
Approximate N Houses		4,472,641		0%	0%
Per-house PDV damages for new construction					
Location-based	$6,245	$5,207	$4,865	$-1,037	$-1,379
(i.e. obs. elevation, counterf. locations)				-16.6%	-22.1%
Elevation-based	$10,770	$5,207	$5,394	$-5,563	$-5,376
(i.e. obs. locations, counterf. elevation)				-51.7%	-49.9%
All	$13,064	$5,207	$5,085	$-7,857	$-7,979
(i.e. counterf. locations & elevation)				-60.1%	-61.1%
Number of Elevated Houses	288,002	640,676	482,901	352,674	194,899
				122%	68%
Price					
Inside observed floodplain	$260,578	$250,865	$251,596	$-9,713	$-8,982
				-3.7%	-3.4%
Outside observed floodplain	$166,954	$174,505	$170,170	$7,550	$3,216
				4.5%	1.9%
Overall	$182,235	$185,670	$182,765	$3,435	$530
				1.9%	0.3%

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. "Observed floodplain" is the area designated as the flood zone in the original flood maps. Areas in our sample counties without digitized historic flood maps are assigned their flood zone status as of 1996, which we estimate overlaps with historic flood zone status in over 90% of cases. Prices are weighted by total developed area. The unregulated counterfactual sets $SFHA_z = 0$ everywhere. The corrective tax counterfactual sets $SFHA_z = 0$ everywhere and imposes taxes equal to expected flood damages, conditional on socially-optimal elevation choices.

Table 6: Counterfactuals: Welfare-Relevant Components

Outcome (Millions of \$)	Level	Differences from Unregulated		
	(1) Unregulated	(2) Current Policy, $\phi_1 = \phi$	(3) Current Policy, $\phi_1 = 0$	(4) Corrective Tax
Producer Surplus		-3,095	-3,095	-2,188
Consumer Surplus		-62,377	-39,192	-41,462
Damages	58,431	-35,140	-35,140	-35,687
Government Revenue	3,225	8,524	8,524	20,117
Total Social Welfare		-21,809	1,377	12,154

Notes: Table presents estimates of counterfactual outcomes using the baseline parameters. Outcomes are in millions of \$. The unregulated counterfactual sets $SFHA_z = 0$ everywhere. Column (2) computes consumer surplus under the current policy, assuming that the entire flood zone demand cost affects experienced utility. Column (3) replicates column (2), but allows most of the flood zone demand cost to reflect debiasing rather than experienced utility costs. Column (4) describes outcomes in the corrective tax counterfactual, which sets $SFHA_z = 0$ everywhere and imposes taxes equal to expected flood damages, conditional on socially-optimal elevation choices. Government revenue indicates revenue from insurance premiums in columns 1-3, and revenue from the corrective tax in column 4.

A Appendix

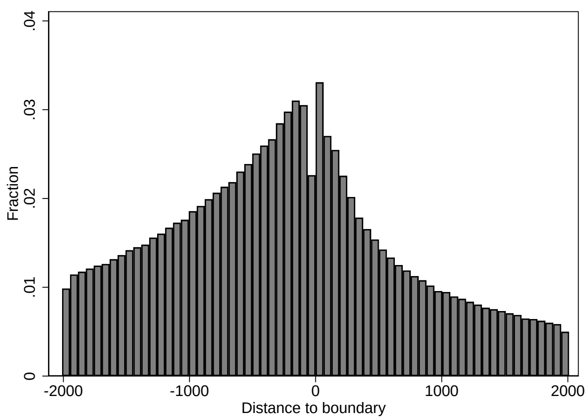
A.1 Appendix Tables and Figures

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

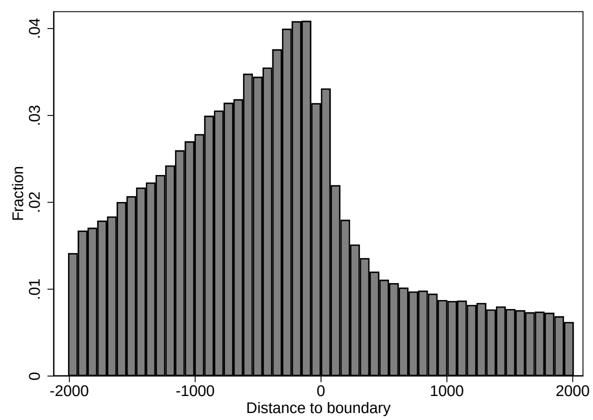
Notes: Figure shows an elevated house in Collier County, Florida. At this location, flood zone regulations require the bottom of the lowest (non-basement) floor to be elevated to 10 feet above sea level.

Figure A.2: Histogram of Distance to Flood Zone Boundary

(a) 30x30m pixels



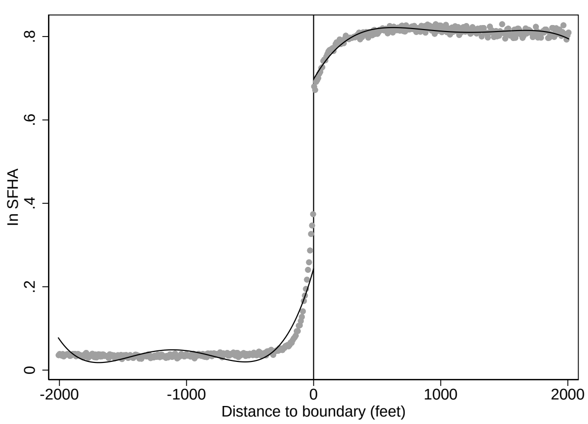
(b) Sales



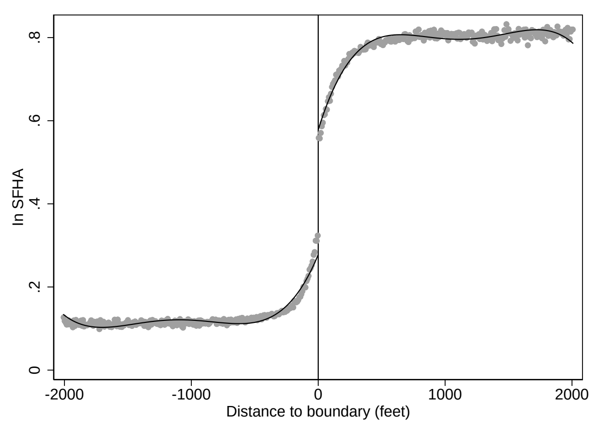
Notes: Figure presents histograms of distance to boundary for land (a) and sales (b). Distance to boundary is in feet, with positive distance indicating being inside the flood zone. Excludes boundaries that trace a body of water and pixels that overlap with the boundary.

Figure A.3: RD Estimates: Current Flood Zone Status

(a) Share of Land in 1996 Flood Zone

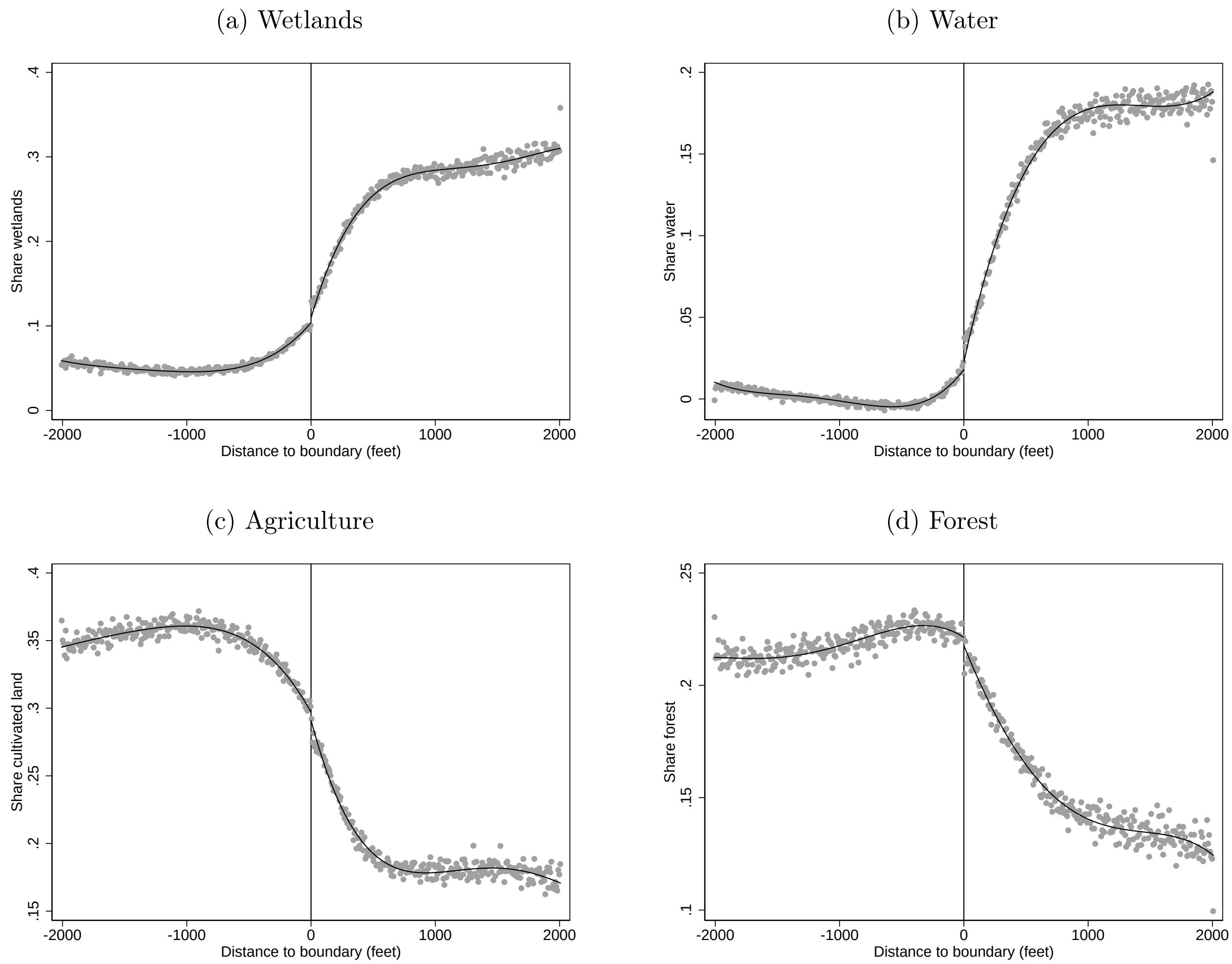


(b) Share of Land in 2017 Flood Zone



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

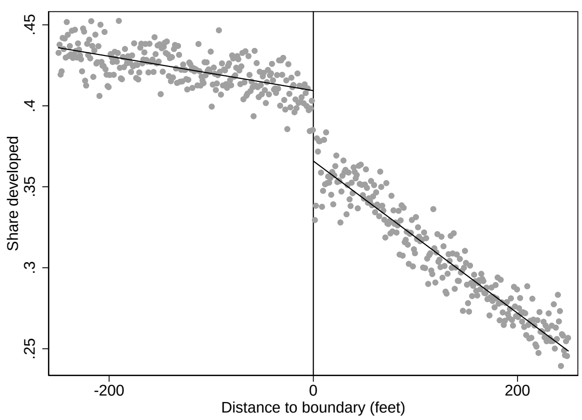
Figure A.4: RD Estimates: Other Pre-Period Land Use



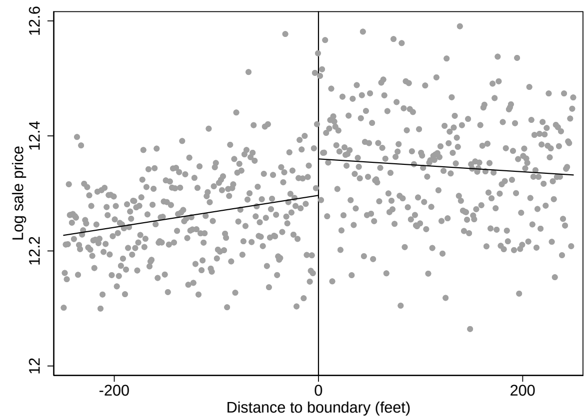
Notes: Figures present RD plots with a fourth order polynomial fit on either side of the flood zone boundary. Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. All land use outcomes are measured as of the late 1970s and early 1980s. Estimates are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

Figure A.5: RD Figures: Local Linear Specification

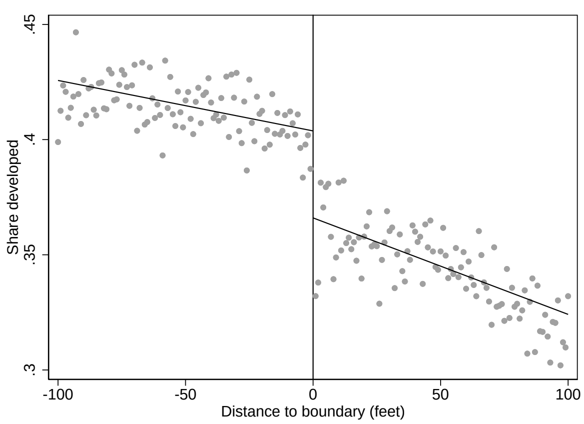
(a) Share Developed in 2016, Rectangular Kernel



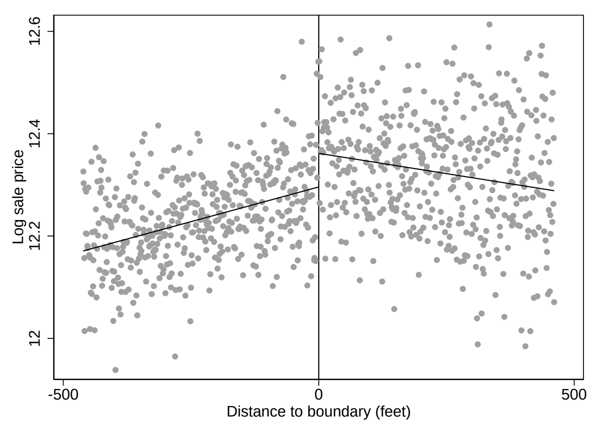
(b) Log Sales Price, Rectangular Kernel



(c) Share Developed in 2016, Triangular Kernel



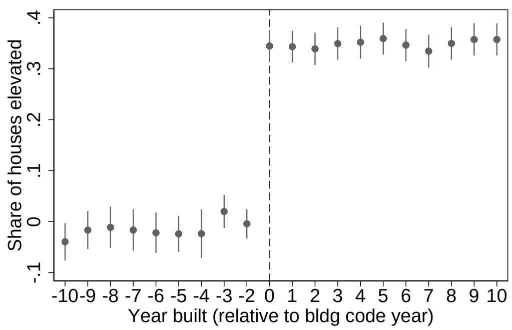
(d) Log Sales Price, Triangular Kernel



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.

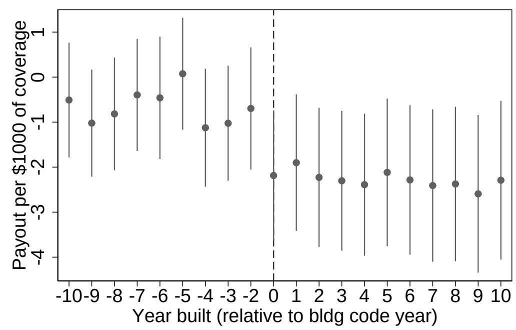
Figure A.6: NFIP Enrollment Year Difference-in-Difference Estimates

(a) Share Elevated



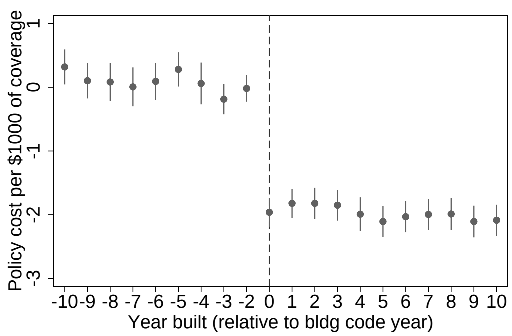
Level shift in 5-year window, no time trend: 0.356 (0.015)

(b) Damages



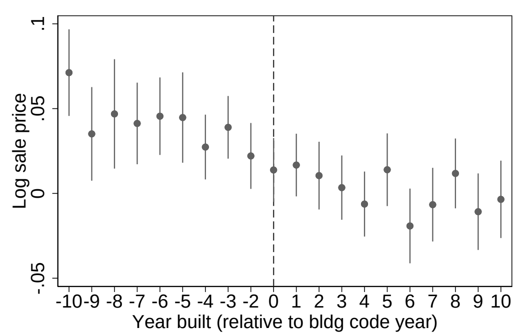
Level shift in 5-year window, no time trend: -1.555 (0.278)

(c) Policy Cost



Level shift in 5-year window, no time trend: -1.935 (0.107)

(d) Log Sale Price

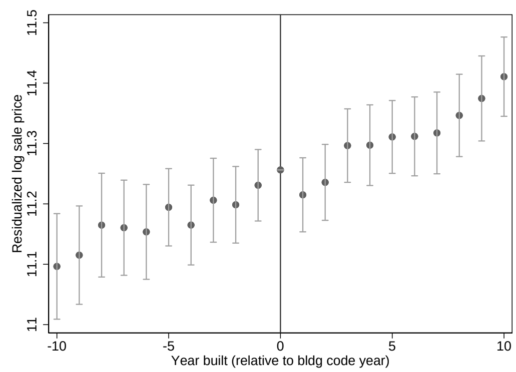


Level shift in 5-year window, no time trend: -0.025 (0.009)

Notes: Figures present coefficients from the difference-in-difference specification comparing houses built before and after the year of building code introduction, inside and outside of the flood zone. The Sun-Abraham eventstudyinteract package is used to account for potential heterogeneity in treatment effects across cohorts (Sun, 2021). The effect of regulation is estimated as the average of the year-specific coefficient estimates from years 0-5, less the average of the year-specific estimates from years -6 to -2. Sample is restricted to single-family residences and standard errors are clustered at the census tract level. Sub-figure (a) shows share of NFIP policies that indicate elevation above the modern-day Base Flood Elevation. Sub-figure (b) shows insurance payouts from 2010 to 2018 in each census tract as a share of total dollars of coverage across the time period in that census tract. Sub-figure (c) shows total policy costs to consumers from 2010 to 2018 in each census tract as a share of total dollars of coverage across the time period in that census tract. Sub-figure (d) shows the log sale price in 2010 dollars between 2005 and 2020 in each census tract for single-family residences.

Figure A.7: NFIP Enrollment Year Event Study Estimates of Residualized Sales Price

(a) Log Sale Price



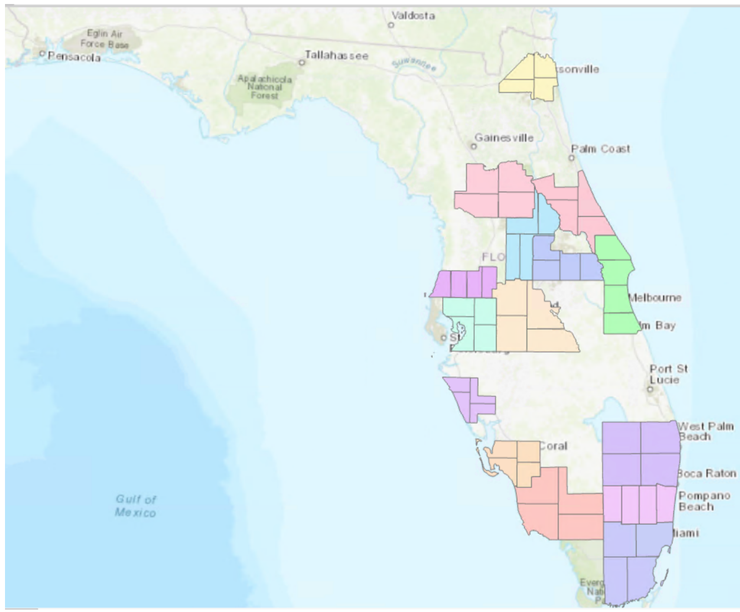
Notes: Figures present coefficients on bins of year built relative to FIRM year (the year of enrollment in the National Flood Insurance Program, at which time building codes began to be imposed on newly-constructed housing). Outcome is sales price, residualized of fourth-degree polynomials in parcel size and total living area, and county-by-sale-month and year built fixed effects. Sample is restricted to properties inside the flood zone.

Figure A.8: Tract Share of Pre-Regulation Houses Elevated, by Flood Risk

../external/estimates/sh_elv_pre_aal_2021.pdf

Notes: Figure plots a binned scatter plot of the share of houses that were built before building code regulations in each tract, against that tract's average annual loss in 2021. Sample is restricted to houses inside the flood zone.

Figure A.9: Quadrants



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

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

	Equal weight to each developed pixel		Weighted by number of parcels	
	Inside flood zone	Outside flood zone	Inside flood zone	Outside flood zone
	(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 flood zone status by First Street floodplain status. Flood zone status designates areas that FEMA has determined are in a 100 year floodplain (i.e. they have 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: Share Mapped Out of Floodplain by Land Use

	Inside flood zone in 2004	
	Land Share of Flood Zone (2004)	Share Mapped Out of Flood Zone in 2008/2009
	(1)	(2)
Any Developed	0.148	0.230
Developed - Open	0.045	0.160
Developed - Low	0.056	0.224
Developed - Mid	0.032	0.282
Developed - High	0.015	0.351
Wetlands	0.621	0.004
Water	0.064	0.003
Cultivated	0.076	0.068
Other Land Use	0.092	0.059

Notes: Table presents the share of the land inside the flood zone in 2004 that is remapped out of the flood zone in the next remapping, by land use category in 2004. Sample is restricted to Marion and Dade Counties, because these were the two counties in our sample of interest that experienced zero remappings between 1996 and 2004 and one remapping between 2004 and 2016. These restrictions were made so that (1) we could be confident that the flood zone status in 2004 was reflected in the digitized flood maps from 1996 (our next-most-recent set of flood maps) and (2) we could observe a change in flood zone status between 2004 and our modern-day flood maps in 2017. These two counties had maps drawn in 1995 and then 2008 or 2009. We measure land use in 2004 from the NLCD. We compute the share of land inside the flood zone in 2004 that was remapped outside of the flood zone by 2017, splitting by land use category in 2004.

Table A.3: 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	0.110	0.019	0.071
	(0.004)	(0.007)	(0.003)	(0.006)
Water	0.005	-0.020	0.012	0.001
	(0.003)	(0.004)	(0.002)	(0.001)
Agriculture	-0.006	-0.029	-0.015	-0.021
	(0.004)	(0.007)	(0.003)	(0.005)
Forest	-0.004	-0.009	-0.006	0.001
	(0.003)	(0.006)	(0.003)	(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.4: RD Estimates: Other Sale Price Estimates and Compositional Differences

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

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

Table A.5: Summary Statistics of Construction Year RD Sample

	Inside flood zone		Outside flood zone	
Variable	(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 (2010 $USD)	$263,253	$307,565	$166,752	$188,521
N house sales	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.6: Summary Statistics of the Model Estimation Sample

	Whole Sample		Balanced Boundary Sample (est. samp.)	
	(1)	(2)	(3)	(4)
	Outside flood zone	Inside flood zone	Outside flood zone	Inside flood zone
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
First Street AAL (expected annual damages in 2021, middle scenario)	0.0005	0.0042	0.0007	0.0019
First Street AAL (expected annual damages in 2051, middle scenario)	0.0013	0.0084	0.0019	0.0043
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 inside/outside flood zone observations that are within 100 feet of a boundary. Each observation has the same weight regardless of share developed.

Table A.7: Estimated Parameters, Alternative Specifications

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

Notes: Table presents parameter estimates from our baseline specification. Column (1) uses a demand elasticity calibrated to -1 based on Song (2021). Columns (2) and (3) use demand elasticities at either end of the range imputed from estimates in Calder-Wang (2021). Column (4) estimates the demand elasticity in-sample using residual variation (i.e. not accounting for potential endogeneity between price and unobserved quality). Standard errors (in parentheses) were generated from bootstrapping (100 iterations).

A.2 Data Processing

A.2.1 Algorithm for Selecting Counties to Digitize

The historic flood 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.[38]

Step 2. Divide each county into equal-area quadrants. An illustration of the quadrants is shown in Figure A.9.

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 120 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 a flood zone 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 flood zone boundaries that overlap county boundaries.

[38]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.[39] 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).[40] 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 half of pre-period (unregulated) houses and almost 100% 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 elevated. Because of this, we assume that if the elevation is missing, the house is not elevated.

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 flood zone 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.

[39]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.

[40]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.

A.4.3 Difference-in-Difference Specification

We expand on our event-study strategy in Section 4.2 by also using the fact that building standards were imposed for houses built inside the flood zone but not for houses built outside of it. We estimate the following difference-in-difference model using the Sun-Abraham eventstudyinteract package (Sun and Abraham, 2021; Sun, 2021):

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

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 4.2 are largely unchanged in the difference-in-difference specification. Here, we find that the regulation reduces insurance payouts by $1.55 per $1000 of coverage. At the average coverage amount of $252,000, this translates to a difference of

\$391 per year. With a discount rate of 5%, the PDV of these savings is 4% of the average house value. This difference-in-difference specification also finds that the regulation reduces policy premiums by about \$1.94 per \$1000 of coverage, or \$490 per year. The PDV of these savings is about 5% of the average house value.

A.4.4 Stylized Model of WTP for Adaptation

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

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

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

We assume that $\alpha = -1$ and take $s_A = 0.8$ based on the market share of post-FIRM houses in 2016, and $c = -6398$ based on our estimates. An estimate of no price difference between adapted and non-adapted houses yields an internalized share $\rho$of approximately zero.

The upper end of the 95% confidence interval is a price increase of 1.06%. This would translate to 35% of the PDV of insurance payout savings and 46% of the PDV of insurance premium savings. If we instead apply this upper-bound estimate we find that $\rho = 0.36$.

That is, consumers only internalize a maximum of 36% of the PDV of the savings that would accrue to them from an adapted house.

A.5 Calibration of Supply Elasticities

We use as our starting point estimates produced in Baum-Snow and Han (2022) (BSH) for land development elasticities at the census tract level, estimated between 2001 and 2011. Specifically, we take estimates of the elasticities as of 2001 from the IV specification. We replace any negative elasticities with the smallest nonnegative elasticity in our eleven-county sample. We denote these elasticities as α_j^S.

For each tract, we average the 2016 price and quantity within all observations in the tract, weighting by number of grid cells. We then compute the decrease in share developed that would be implied by the BSH elasticities for a decrease of 10% in the price from the observed 2016 price: $\tilde{q}_j = q_j^{2016} - \alpha_j^S(0.1 p_j^{2016})$.[41] We then use our postulated relationship that

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

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

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

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

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

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

A.6 Moment Condition Details

A.6.1 Demand-side identification and moments

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

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

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

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

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

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

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

A.6.2 Supply-side calibration and moments

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

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

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

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

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

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

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

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

A.7 Details on Data for Model Estimation

We measure today's sales price p^{2016}_{jz} 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^{1980}_{jz} 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 flood zone 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.[42] We measure elevation from NFIP policy data, as discussed in Appendix A.4.1. We define a tract as elevated if more than 50% of insured houses in that tract are elevated. Where we do not observe elevation (including all non-flood-zone tracts), we assume it only occurred when required by regulation. When historic flood zone status is unavailable because of the limited reach of our digitized maps, we use the 1996 flood zone status to calculate mean utility δ_{jz}, but we restrict to historic boundaries in our estimation of the main parameters.

[42] To account for the fact that some locations jz have no developed land in 1980, we calculate share developed $q_{jz} = (Q_{jz} + 1)/L_{jz}$ in those locations. Also, in the spirit of Burchfield et al. (2006) we correct for potentially mismeasured growth by measuring the number of developed cells in 1980 as the minimum of the observed number of developed cells in the location in 1980 and the number measured in 2016.

Appendix Table A.6 presents summary statistics for the model estimation sample. A larger share of our estimation sample is developed than the sample used in Section 4,but house prices and flood risk look similar in the two samples.

A.8 Calculation of Expected Damages

We define flood risk using data from the First Street Flood Lab estimates of Average Annual Loss (AAL). AAL expresses expected annual damages as a share of house price. These data come from parcel-specific estimates (as opposed to the raw hazard layer) that combine the raw hazard layer (which generates the parcel-specific inundation depth) with the output of an engineering damage model. The damage model takes as inputs a number of features of the structure, including its market value, number of stories and units, and foundation type, and calculates damages using the HAZUS-MH methodology. The HAZUS-MH methodology was developed for FEMA to calculate estimated damages from natural disasters and is based on a set of depth-damage curves collected from FEMA's Federal Insurance and Mitigation Administration (FIMA) and the USACE Institute for Water Resources (USACE-IWR).[43] Average annual loss is expected to grow over time; we assume risk increases linearly from the estimated 2021 risk to the estimated 2051 risk and then stays constant at the 2051 risk for all future years. Wherever First Street did not provide an AAL estimate but did provide a Flood Factor (another measure of risk), we assumed the AAL was 0.

Expected damages are computed as the product of number of newly-developed gridcells and the PDV of expected damage under a given counterfactual. The expected damage is computed as $0.7 \times P_{jz}^{Obs} \times AAL_{jz} \times M_{jz}^{CF}(E_{jz}^{2016})$, where P_{jz}^{Obs} is the observed (level) price of a house and AAL_{jz} is the observed average annual loss.[44] The term M_{jz}^{CF} is a multiplier that accounts for differences in elevation in each counterfactual. We assume that if an observation

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

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

was elevated in the pre-period it will be elevated in the post-period for all counterfactuals. Otherwise, houses that are not observed to be elevated in the post-period but are elevated in a counterfactual will experience 55% lower damages in that counterfactual; a similar calculation applies to houses that are observed to be elevated but are counterfactually non-elevated.

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

A.9 Welfare Calculation Details

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

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

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

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

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

We compute government revenue from the tax policy by adding up all taxes levied on newly-developed houses. We compute government revenue under the flood zone policy by estimating the total amount of insurance premiums. Using our flood insurance policy data, we assume that policies inside the flood zone cost \$1484 per year and policies outside the flood zone cost \$572 per year. Applying back-of-envelope calculations to recent estimates of take-up in Florida (Lingle and Kousky, 2018) and inside and outside the floodplain nationally (Bradt et al., 2021), we assume take-up is 45% inside the flood zone, 6% outside the flood zone in high-risk areas (areas with positive probability of flooding more than 2 feet in the 100-year flood), and 0% outside the flood zone in low-risk areas. As with the tax revenue, we calculate premium revenue only for new development.