The Effects of Floodplain Regulation on Housing Markets

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Abstract

We investigate the effects of regulation designed to align 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 construction in high-risk areas and mitigates damages of at-risk homes. Embedding these effects in a model of the housing market, we find the policy reduces damages to the socially-efficient level, but incurs higher costs than a first-best corrective tax. Improved targeting of the existing policy achieves 79% of first-best welfare gains, or $7,041 per newly-constructed 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., 2018a; Davlasheridze and Miao, 2019; Landry et al., 2021; Bakkensen and Barrage, 2022; Wagner, 2022; Mulder, 2022; Hsiao, 2023). 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 higher 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. We study the extent to which floodplain regulation reduces flood damages via both the location and flood-safe adaptation of construction, and we weigh these benefits against the costs of that regulation. We do so by studying effects around the boundaries of the regulated area and around the date of the policy's introduction. We then embed these empirical results in a model of residential choices and construction to estimate the market-wide effects of floodplain regulation and investigate the welfare impacts of current and counterfactual policies.

To conduct our analysis, we assemble a new dataset to describe regulation, real estate development, and flood risk in Florida over a 40-year time horizon. Florida is both populous and flood-prone; 45% of land is a regulated floodplain. Our dataset combines maps of historic regulated 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 hydrological model. Importantly,

[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).

this dataset extends to the first maps delineating regulated areas, which we digitized from archival scans. This allows us to study the policy's effect on long-run development and confirm the validity of our empirical approach. And because our dataset details both adaptation status and location decisions, as well as how flood damages vary along these margins, we are able to comprehensively measure the policy's effect on flood risk.

We use two complementary empirical strategies — a spatial regression discontinuity and an event study around the regulation's implementation — to characterize the policy's risk reduction effects along two margins: reduced construction and mandatory adaptation in risky areas. Our spatial regression discontinuity design compares current development and house prices on either side of the regulatory boundary delineated at the time of the policy's introduction in the 1970s and 1980s. Our analysis relies on the assumption that flood risk and other amenities are smooth through these initial regulatory boundaries. While unlikely to hold in modern maps, this assumption is reasonable for the original maps because mapping technologies were rudimentary and homeowners lacked the ability to influence the initial regulatory boundaries.[2] Importantly, we validate this assumption by demonstrating smoothness in pre-period land use through the historic boundaries. We document that the modern-day share of developed land is 9% lower just inside the regulated area, highlighting the potential for the policy to reduce damages by shifting construction out of risky areas. This decrease in new construction 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 via building standards that impose mandatory adaptation of houses built in the floodplain. Building on Wagner (2022), we exploit the sharp timing of the policy's introduction in an event study design. We document that building standards reduce average flood damages by 55%, or 3% of average home value. Though building standards generate social value via reduced damages, we also 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 regulatory boundary is attributable to construction costs, not differences in willingness-to-pay for adapted homes.

Together, our empirical results yield four facts. First, floodplain regulation suppresses con-

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

struction in high-risk areas. Second, the policy's adaptation mandate binds, reducing flood risk. Third, the cost of mandating adaptation is large enough that the price effect of the resulting inward shift of housing supply 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 social welfare. However, these results alone are insufficient to quantify the total effect of the policy on either damages or social welfare. The policy's effects depend on the location of counterfactual development: both risk and amenities vary across space. And due to the large size of the regulated area, house prices in unregulated locations may be affected. These spillover effects shift relative incentives to develop across space, thereby impacting housing prices and consumer welfare market-wide. A complete welfare analysis of *status quo* or alternative policy designs requires quantifying both benefits via market-wide reductions in flood risk and costs via distortions in the housing market.

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 location characteristics including unobserved amenities. Developers build houses when doing so is more profitable than the outside option of land use; housing profits depend on housing prices and construction costs, which include a cost of compliance with flood-safe building codes. Our quasi-experimental results inform our model and estimation strategy. Because the event study estimates imply that homeowners are unwilling to pay for flood safety, developers of housing do not expend costs to adapt absent policy intervention. We estimate effects of floodplain designation on consumer choices and construction costs by matching the spatial discontinuity estimates around the regulatory boundaries.

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 62%, or approximately $3.8 billion per county. Both the extensive-margin location channel and intensive-margin adaptation channel are quantitatively important. The reduction in expected flood damages from mandatory flood-safe construction in regulated areas account for the majority, or 84%. The incentive to build new houses in safer areas contributes the remaining 16%. Risk reductions are driven by effects on both the demand for and supply of housing. Our parameter estimates imply that building standards increase construction costs by 24%. Consumers are willing to pay 27% 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 social welfare under current and counterfactual policies. We define social welfare as the sum of consumer surplus, producer surplus, government revenue from each policy, and uninternalized flood damages, due either 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 design 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 adaptation 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.[3]

The current policy achieves approximately the socially-efficient degree of damage reductions and improves social welfare substantially relative to an unregulated benchmark, an increase of $2,975 per newly-developed house. However, these social welfare gains are only one-third of those under a first-best corrective tax. The current policy imposes distortions in the housing market it attempts to correct, relying more on adaptation and less on relocation than is socially-optimal. In some regulated but relatively safe areas, mandating adaptation is costlier than its benefits, while in the riskiest locations, development is still inefficiently high despite strong disincentives to build. Motivated by this shortcoming, we propose a simple change to the current policy: improved targeting of regulated areas. By imposing regulation only in locations where the benefits of mandating adaptation exceed costs, fewer locations are regulated and fewer homes are adapted, but flood risk levels remain similar to under the *status quo*. When well-targeted, the simple binary regulation can achieve 79% of first-best social welfare gains, or $7,041 per newly-developed house.

Our work contributes to several literatures. Most directly, we contribute to a literature analyzing regulations designed to reduce damages from floods and other natural disasters, including effects on house prices (e.g. Hino and Burke (2021); see Beltran et al. (2018) for a survey) and in-place adaptation (Baylis and Boomhower, 2021; Wagner, 2022; Mulder, 2022). We build on this literature by jointly studying the impacts of floodplain regulation on both the location and type of construction, which together determine flood damages.[4] Furthermore, we contribute by developing an equilibrium framework to trade off the damage-reducing benefits and regulatory costs of current and counterfactual policy designs.

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., 2018a;

[3] See The Congressional Research Service for an overview of this policy change and The National Flood Insurance Program for more details.

[4] This complements work studying the effects on population flows of related policies, including the introduction of the National Flood Insurance Program (NFIP) (Peralta and Scott, 2024) and Home Seller Disclosure Requirements (Lee, 2023).

Bakkensen and Barrage, 2022; Wagner, 2022; Mulder, 2022; Baylis and Boomhower, 2023; Hsiao, 2023; Balboni, 2024). Beyond documenting frictions, we investigate the extent to which *status quo* second-best corrective policy can reduce resulting social welfare losses. In doing so, we relate to other work studying such policies, addressing internalities or externalities in other settings with coarse labels (e.g. Barahona et al. (2023)) and standards and attribute-based regulation (e.g. Ito and Sallee (2018); Kellogg (2020); Jacobsen et al. (2020, 2023)).

Methodologically, we relate to other work embedding boundary discontinuity designs in welfare frameworks in urban economics (Bayer et al., 2007; Turner et al., 2014; Anagol et al., 2023). Our model also incorporates recent estimates of location-specific supply elasticities from Baum-Snow and Han (2024) to characterize equilibrium changes in housing supply.

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. Section 3 describes our setting — the state of Florida — and data. In Section 4, we present quasi-experimental 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. In Section 7, we conclude.

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., 2018b).

After the NFIP was established in 1968, the program was rolled out to communities throughout the country in the late 1970s and 1980s.[5] 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, developers of new buildings in specific areas had to comply with flood safety regulations, and flood insurance became available to homeowners.

In both the initial FIRM and subsequent, updated flood maps, an 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." All new construction and substantial home improvements in the flood zone must comply with building regulations that require that a home's lowest floor lie above the Base Flood Elevation (BFE).[6] In the flood zone, some homeowners face a flood insurance mandate and all homeowners face higher flood insurance prices for otherwise-equivalent houses.[7] 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: $850 compared to $435.

Throughout the United States, 10% of land[8] 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).

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.

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

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

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

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

[8] Authors' calculations using 2017 flood maps.

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

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.[10]

3 Setting and Data

Our empirical context is Florida, a flood-prone and populous state. This makes it an ideal setting to study how floodplain regulation impacts housing markets and disaster damages. Nearly 50% of land and 20% 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 (Table 1 and First Street Foundation, 2020). Florida alone accounts for 35% of the nation's NFIP policies.[11] 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.[12] We aimed to collect

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

[10]According to a floodplain manager in Florida, in the early years of the program the scale of paper maps meant that such carve-outs had to be at least 6 acres. Because of this requirement, it was infrequently 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.

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

[12]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

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 B 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 historic flood maps with statewide snapshots in 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. 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, Table 1 illustrates that development increased substantially both statewide (2.6x) and in our sample of interest (2.5x).

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.

Parcel Characteristics and NFIP Policies and Claims Data from the Florida Department of Revenue property tax records from 2005 to 2020 provide detailed information about structures, including sales prices and parcel outlines and location. We geolocate the exact location of any buildings on each parcel using Microsoft's open-source building footprints dataset.[13] In Table 1 Panel C we summarize average home prices statewide and in our sample of interest. Much of our analysis focuses on single-family homes, which make up a large majority of structures in our sample (Table 1 Panel A). We obtain historical data on home prices from the 1980 Census (Manson et al., 2021). We also use NFIP claims and policies from 2010 to 2018 to provide information about flood damages for insured structures.

Flood Risk Model To assess the risk profile of development across policy regimes, we use spatially-granular estimates of flood risk from a third-party hydrological model 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 (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 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, but First Street and FEMA disagree about the exact location of risk. Appendix Table A.1 tabulates the discrepancies between FEMA's flood maps and the First Street model in our sample, showing that more than one-fifth of parcels are categorized differently by FEMA and First Street.

4 Quasi-Experimental Evidence

We begin our analysis by describing the effects of floodplain regulation on flood risk, which could occur both by shifting construction away from high risk areas and by mandating adaptation through building standards. In this section, we employ a spatial regression disconti-

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

nuity 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 flood damages among built structures.

4.1 Spatial RD Around the Boundaries of Regulation

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 at the boundary – will later allow us to estimate the effects of floodplain regulations on consumers' residential choices and developers' construction costs.

4.1.1 Empirical Strategy

Estimating the effect of floodplain regulations 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 (SFHA) 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.

The boundary discontinuity design examines how outcomes at each 30x30m pixel vary as a function of the distance to the flood zone boundary. Specifically, we estimate

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

where y_i is a characteristic of pixel i and d_i is the perpendicular distance from each pixel i to the nearest 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. In our baseline specification, $f(d_i)$ are local linear functions allowed to differ on either side of the boundary.[14] Finally, since boundaries in this setting do not have natural segments, we include census tract fixed effects $\gamma_{j(i)}$ as a substitute for boundary fixed effects. We cluster standard errors at the census tract level to allow for spatial correlation in the error term.

For each outcome, we compute the MSE-optimal bandwidth proposed by Calonico et al. (2014) and estimate equation 1 on land within that distance 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 than areas further from the boundary. Appendix Figure A.2 plots a histogram of the number of pixels (or grid cells) 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. This is motivated by our focus on the effects of floodplain regulation on long-run adaptation to flood risk. Appendix Figure A.3 documents the evolution of the relationship between initial designation and floodplain status over time.

[14] Columns 3 and 4 of Table 2 present robustness to alternative specifications of $f(d_i)$.

Exogeneity of Boundaries To validate our identifying assumptions, we check for smoothness in land use through the regulatory boundaries prior to flood zone designation.[15] 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 small.[16] This test supports our hypothesis that the institutional details of the initial mapping process provide a compelling setting in which to conduct a boundary discontinuity analysis.

Development Falls in the Flood Zone By 2016, we see a sharp discontinuity in development at the flood zone 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 flood zone is 3.8 percentage points less likely to be developed than land just outside. This effect is substantial: it is 9% of the outside-flood-zone mean level of development and it represents a 17% reduction as a share of total new development occurring between 1980 and 2016 just outside the flood zone.[17] This effect indicates both the potential for substantial reductions in flood damages via reduced building in risky areas and the possibility of costs imposed on developers or consumers that yield this behavioral response.

Prices Increase in the Flood Zone The effects of floodplain regulation on house prices 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 developers to employ costlier construction methods, flood zone designation could push prices up. Figure 3 shows that although we observe a large decrease in development, prices are non-decreasing through the boundary. In fact, our point estimates suggest that house prices are 6.7% higher inside the flood zone.[18]

Our evidence suggests that this positive price effect should be interpreted as an inward shift in

[15] This 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. In some locations, the images are approximately contemporaneous to the maps.

[16] Appendix Figure A.4 and Table A.3 examine smoothness in other pre-period land use outcomes.

[17] Table 2 Panel B shows that these results are robust to alternative definitions of development, including the share of land covered by a building footprint.

[18] This finding contrasts with recent work that has found flood zone designation *decreases* house prices Hino and Burke (2021). That work exploits map updates, capturing short-term effects, while we study a 40-year horizon. This long-run setting allows supply to respond to mandatory building codes, which could offsets short-run reductions in price from reductions in demand alone.

supply, or that the construction costs imposed in the floodplain dominate any negative effects on price driven by demand, e.g. mandatory flood insurance, higher flood insurance prices, or any salience or risk perception effects of living in a flood zone. We rule out other competing explanations: changes in willingness to pay for housing characteristics – including house adaptation — and changes in amenities. In Appendix Table A.4 Panel A and Figure A.5, we demonstrate that prices increase for houses built both before and after the introduction of the regulation, and price increases persist when we control for compositional changes via polynomials in square footage and lot area and indicators for county-by-sale-month and year built. Furthermore, Panel B of Appendix Table A.4 and Appendix Figure A.6 show that single-family housing square footage, the main housing quality measure we observe in the property tax records, exhibits only a marginal increase across the boundary in houses built after the regulation.[19] To rule out changes in amenities across the flood zone boundary, column 5 of Table 2 shows that our results are robust to excluding areas close to the coast, complementing the pre-period smoothness test in Figure 2a.

Testing for Heterogeneity Across Risk Levels We observe our baseline pattern — sharp reductions in development without commensurate decreases in prices at the flood zone boundary — across levels of flood risk. We leverage the fact that the SFHA boundaries cut across a distribution of flood risk levels, measured using estimates from the First Street hydrological model. Appendix Figure A.7 replicates our main regression discontinuity plots, split by land in census tracts with below- *versus* above-median flood risk.[20] Beyond qualitatively similar patterns, the magnitude of the reduction as a share of new development is quantitatively similar across these two samples: 18% in lower-risk tracts *versus* 16% in higher-risk tracts. Price effects in both samples are noisy, but positive. These results are consistent with the policy's design as a coarse, binary instrument.

4.2 Event Study Around the Introduction of Building Standards

The preceding results indicate that regulations impose a cost on new construction. In this section, we investigate whether these regulations also reduce damages via impacts on built homes, and if so, whether consumers are willing to pay for this attribute.

[19] We note that there is a positive, albeit not statistically-significant, point estimate on the outcome of log square footage in post-regulation houses. However, because controlling for square footage does not eliminate (or reduce) the point estimates of SFHA designation on sales price, we remain comfortable interpreting our results as an inward shift in supply. Ruling out compositional changes in housing characteristics is a simplification to focus on the primary housing attribute of interest, adaptation status.

[20] Flood risk is measured as the depth of the 100-year flood in a grid cell's 1980-era census tract. In our analysis sample, the median depth is 1.05 feet.

4.2.1 Empirical Strategy

Following Wagner (2022), we exploit the fact that a community had to adopt flood-safe building standards at the time it enrolled in the National Flood Insurance Program, 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 community enrollment within the flood zone. We estimate the following specifications for all counties in Florida:

$$
\begin{aligned}
m_{jbt} &= \Sigma_r \beta_r \mathbb{1}\left\{r = b - e_j\right\} + \gamma_j + \varepsilon_{jbt} \\
p_i &= \Sigma_r \beta_r \mathbb{1}\left\{r = b_i - e_{j(i)}\right\} + \gamma_{j(i)} + \varepsilon_i
\end{aligned} \tag{2}
$$

where m_{jbt} measures insurance payouts per dollar of coverage in policy year t, for homes in census tract j, built in year b, p_i is the log sales price of house i, r is the construction year relative to the year of NFIP enrollment of the census tract (e_j), and γ_j are census tract fixed effects.[21] The model is estimated at the census tract j by house construction year b by policy year t level for insurance payouts (line 1) and at the house i level for house prices (line 2).[22]

Our baseline specification in equation 2 relies on the sharp change in the year the building standards are introduced (similar in spirit to an RD). Appendix Figure A.8 replicates the analysis and finds similar results in a difference-in-differences specification that includes controls for construction year. In both specifications, the event time is the construction year b, around which building standards were introduced in the late 1970s and early 1980s. Current-day outcomes m_{jbt} and p_i are measured in recent time periods (between 2005 and 2020).

We also estimate the following pooled specification, among houses built in a ten-year window around NFIP enrollment:

$$
\begin{aligned}
m_{jbt} &= \alpha + \beta Post_{jb} + \nu r_{jb} + \eta r_{jb} Post_{jb} + \gamma_j + \varepsilon_{jbt} \\
p_i &= \alpha + \beta Post_i + \nu r_i + \eta r_i Post_i + \gamma_{j(i)} + \varepsilon_i
\end{aligned} \tag{3}
$$

[21] Census tracts are smaller than communities.

[22] Because individual claims cannot be linked to individual policies, we aggregate from the house to the census-tract-by-flood-zone-by-relative-year-built level for outcomes related to insurance payouts or policies. For houses sold multiple times, we take the log of the average transaction price across sales. Appendix C.1 describes the construction of the datasets used in this analysis in more detail and Appendix Table A.5 presents summary statistics.

where *Post* indicates houses constructed in or after the year of the community's NFIP enrollment when flood-safe building standards are imposed ($r \geq 0$). Under the assumption that the year of construction was not manipulated, β indicates the causal effect of building standards.[23] We cluster standard errors in all specifications at the census tract level.

4.2.2 Results

Figure 4 presents the coefficient estimates on relative year from the event study specification (equation 2). Variable means and regression coefficients from the pooled specification (equation 3) are presented in Table 3.

Building Standards Reduce Flood Damages Figure 4a shows that the introduction of building standards causes insurance payouts to fall by $1.60 per $1000 of coverage (Table 3). This accompanies a sharp increase in reported house elevation in flood insurance policies, as mandated by the regulation (Appendix Figure A.9a). The $1.60 (per $1000 of coverage) reduction represents a 55% decrease in expected flood damages (on a pre-period mean of $2.93), highlighting that building standards generate substantial social benefits in expected damage reduction. At average coverage levels, this reduction in expected flood damages is equal to 3% of the average home value (using a 5% discount rate).

The reductions in damages following the introduction of building standards are largest in high-risk locations. We conduct the event study analysis separately by the flood risk of the census tract, either measured via the First Street hydrological model or via flood insurance payouts. Appendix Table A.6 shows that locations with high baseline risk experience larger reductions in damages (in levels), consistent with findings in Hovekamp and Wagner (2023).

House Prices are Unchanged Despite Lower Damages Despite their higher social value, houses that are compliant with the building standards do not command a higher price than non-adapted houses (Figure 4b). The point estimate on log prices is -0.007 (Table 3), and the upper bound of the 95% confidence interval reflects only one third of the reduction in expected damages. This result implies that homeowners are unwilling to pay for reductions in risk, as long as all other house attributes remain constant. This is plausible in our setting, as homes are typically adapted by adding fill below the house while keeping the structure constant.

[23] Appendix Figure A.10 shows no bunching of house construction in the years prior to NFIP enrollment, ruling out such manipulation.

We conduct a series of empirical tests to probe the conclusion that consumers are unwilling to pay for flood safety. First, we confirm that the null house price effect does not mask price declines generated by changes in other house attributes. We show in Appendix Figure A.11 that the null effect on price is robust to controlling flexibly for observable housing characteristics, including polynomials in parcel size and total living area and fixed effects for county-by-month-of-sale and year of construction. We also investigate the possibility that the existence of stairs on adapted houses may be a confounding disamenity by testing whether the observed result is concentrated among consumers with a strong distaste for stairs. We proxy for this heterogeneity by splitting the sample across census tracts with an above- *versus* below-median elderly population. We find no evidence of this confounding disamenity (see Appendix Table A.7). Second, we show that the null effect on price is not driven by a failure of insurance prices to reflect changes in risk, as premiums for adapted homes incorporate at least 75% of the reduction in risk (Appendix Figure A.9b). Finally, we show similar effects on price across locations with varying flood risk (Appendix Table A.6).

This result therefore confirms existing work documenting a wedge between social and private valuations of flood risk (Gallagher, 2014; Kousky et al., 2018a; Bakkensen and Barrage, 2022; Wagner, 2022; Mulder, 2022). The fact that safe houses provide no private value to consumers indicates the presence of frictions, including behavioral frictions such as risk misperception 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 — both in their type (adapted *versus* not) and location — absent regulation.

5 An Equilibrium Model of the Housing Market

The quasi-experimental results indicate that the current policy affects both flood damages and regulatory costs, and may improve social welfare. Floodplain regulation suppresses construction and requires flood-safe building 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 building standards are substantial, since flood zone designation decreases new construction without a commensurate fall in price. Finally, consumers are not willing to pay more for houses with lower flood risk, suggesting a role for policy to correct inefficient risk exposure.

While informative, these results alone are insufficient to quantify the total effect of the policy on either damages or social welfare. The policy's effects depend on the location of counterfactual construction: if the regulation shifts construction to equally-risky areas, damages will not fall. Counterfactual risk depends 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 sample is currently designated as a flood zone — may lead to equilibrium price effects in unregulated locations. Higher demand and consequently higher prices outside the flood zone could then reduce the relative incentive to build in safe, unregulated areas and also reduce consumer surplus market-wide. Finally, 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 flood zone designation shifts the demand for and supply of housing in each location, capturing how consumers and developers trade off home prices and living or building in a regulated area. Both of our empirical exercises inform our model. We use our event study estimates that consumers are unwilling to pay for adaptation to simplify the model: consumers choose only among differentiated locations and developers do not endogenously supply adapted homes. We use our cross-boundary changes in prices and quantities from our RD analysis as moments to estimate the effect of flood zone designation on consumer choices and construction costs.

5.1 Residential Choice

Motivated by the result in Section 4.2 that consumers are indifferent between adapted (post-standards) and non-adapted (pre-standards) 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 the 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.[24] 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.

Following the standard discrete choice framework of Berry et al. (1995), we write the indirect utility of individual i living in location jz as:

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

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

where p_{jz} is the log price of housing in location jz,[25] $\phi SFHA_z$ indicates flood zone status, X_{jz} is a vector of observed housing characteristics,[26] ξ_{jz} are unobserved amenities, and ε_{ijz} is an i.i.d. preference shock, distributed according to a Type 1 Extreme Value Distribution. Modeling the effect of the flood zone as an indicator captures the binary nature of the regulation — in *versus* out of the flood zone — and is consistent with our evidence documenting similar effects across locations with heterogeneous levels of risk.

A large body of work suggests that misperceptions likely drive consumers' failure to internalize flood risk (Bakkensen and Barrage, 2022; Wagner, 2022; Mulder, 2022). This complicates interpretation of $\phi SFHA_z$ if it debiases consumers, as SFHA status may influence choices without imposing a utility cost. Following Allcott and Taubinsky (2015), we will refer to equation 4 as a consumer's *decision utility,* which is not necessarily her *experienced utility.* While equation 4 alone is sufficient for estimation and to characterize the effects of the policy on housing market outcomes, normative evaluation requires an assumption about the welfare-relevance of $\phi SFHA_z$. We return to this issue in Section 6.

Individuals choose the location jz that maximizes their decision utility within a market m. The fraction of individuals choosing to live in location jz is:

$$s_{jz} = \frac{\exp(\alpha^D p_{jz} + \phi SFHA_z + X_{jz}\beta + \xi_{jz})}{\Sigma_{j' \in J_m,\, z' \in \{0,1\}} \exp(\alpha^D p_{j'z'} + \phi SFHA_{z'} + X_{j'z'}\beta + \xi_{j'z'})}. \tag{5}$$

5.2 Housing Supply

Our model of housing supply is designed to simply and flexibly capture heterogeneity in housing supply elasticities across locations. 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 is 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 adapted. A

[25] The price p_{jz} is for the bundle of housing, including both structure and land.

[26] Observed housing characteristics include the share of residences that are single-family houses, the average age of residential buildings, the average square feet of land and living area for residential parcels, the share of buildings ranked "average," "high," or "superior" quality, and the share of parcels that are residential, commercial, industrial, agricultural, or open space.

house's adaptation status is determined exogenously by building standards and varies by location (SFHA *versus* not) and time (pre- *versus* post-regulations).[27]

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

$$D_g^t = \mathbb{1}\left\{p_{jz}^t \geq c_g + \psi E_{jz}^t + \eta_{jz}^t\right\} \tag{6}$$

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 adapted in period t. Let $\Phi\left(\right)$ denote the Normal Cumulative Distribution Function. The share of land developed at the end of time $t = 1$ is

$$\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right). \tag{7}$$

5.3 Equilibrium

In equilibrium, the quantity of housing supplied in each location equals the number of individuals choosing to live there:

$$L_{jz}\Phi\left(\frac{p_{jz}^1 - \psi E_{jz}^1 - \mu_j - \eta_{jz}^1}{\sigma_j}\right) = Q_{jz} = N_m s_{jz} \tag{8}$$

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

5.4.1 Demand

We use the standard inversion to estimate equation 4 from observed market shares:[28]

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

where s_{0m} is the market share of an arbitrary geography within each market that we have normalized to be utility 0.

[27] In some locations where the minimum required elevation (Base Flood Elevation, or BFE) is very low, we observe houses recorded as built above the BFE in the flood zone before the introduction of building standards. While technically "adapted," these homes were likely built under standard construction practices. To be conservative, we assume houses in these locations are adapted across counterfactuals.

[28] We construct the empirical market shares as $\hat{s}_{jz} = \frac{Q_{jz}^{2016}}{\sum_{j' \in J_m,\ z' \in \{0,1\}} Q_{j'z'}^{2016}}$, where Q_{jz}^{2016} is the total amount of developed land in geography jz measured in the satellite-derived land use data.

Because amenities ξ_{jz} may be correlated with both $SFHA_z$ and prices p_{jz}, simply estimating equation 9 via OLS would yield biased estimates. These correlations arise because flood zone status may coincide with other amenities, like coastal access (biasing ϕ). In equilibrium, higher-amenity locations will command higher prices (biasing α^D).

We address the possible correlation between amenities ξ_{jz} and $SFHA_z$ using our boundary discontinuity design, which isolates the causal effect of SFHA designation. In the boundary discontinuity analysis, we assume that as distance to the boundary approaches zero, amenities ξ_{jz} are constant on either side of the boundary, and thus independent of SFHA status. We therefore construct moments to match the cross-boundary differences we document in the RD analysis.

To address the possible correlation between amenities ξ_{jz} and prices p_{jz}, we construct instruments for p_{jz} that exploit variation in the characteristics of *other* locations within the market, excluding areas surrounding each location jz. The characteristics of distant houses in the same housing market influence prices in location jz in equilibrium, but do not directly affect the utility provided by a house in location jz.[29] Our instruments include averages of observable characteristics X_{jz} of locations in the same housing market that are located more than 3 miles away from geography jz, weighted by land area. Following Bayer et al. (2007), we also construct a price vector $\tilde{p}_{jz}$ that rationalizes market shares under no unobserved amenities (i.e., setting $\xi_{jz} = 0$), using the equilibrium conditions to capture the price impact of the observable attribute space.

Discussion We estimate the floodplain effect on consumer choices (ϕ) using observations close to the boundary. We assume that ϕ is constant across regulated areas. The most salient information provided (in a flood zone *versus* not) and regulations imposed (insurance mandate) are consistent across regulated areas. Although the exact insurance premium may vary, SFHA status is an important determinant of premiums and the most readily available information to consumers (Mulder and Kousky, 2023). Empirically, we observe qualitatively similar patterns across boundaries in locations with heterogeneous risk (Appendix Figure A.7). However, to the extent that the true flood zone effect on demand is larger in higher-risk areas than at the flood zone boundary, ours will be an underestimate.

We have imposed the assumption that consumers do not respond to expected flood damages. If this assumption is incorrect, any increases in flood expenses to which consumers *do* react will load onto the flood zone term (if they change discretely at the boundary, e.g. through

[29] Because each location is a small share of the market, we assume that prices in location jz do not in turn affect characteristics of houses constructed in other locations.

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 conclusions about effects on construction, damages, or house prices.

5.4.2 Supply

We assume that floodplain regulation affects supply only via building standards mandating adaptation. However, estimating ψ is still challenging because flood zone designation $SFHA_z$ may be correlated with construction costs η_{jz}, like the presence of wetlands. As with demand, we address this challenge by constructing moments to match the cross-boundary differences in the regression discontinuity analysis.

To capture heterogeneity in housing supply elasticities across tracts, we match estimates of tract-specific supply elasticities from Baum-Snow and Han (2024), which are estimated in the same locations as our setting. 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 more inelastic than inland, suburban or rural areas.

5.4.3 Estimation Details

Estimation proceeds in two steps. First, in a preliminary step we calibrate μ_j and σ_j based on estimates from Baum-Snow and Han (2024). Then, we estimate the remaining parameters via the Generalized Method of Moments (GMM) using two sets of moment conditions. The first set are regulatory boundary moments. These moments require that cross-boundary differences in price ($\beta^{p,2016}$) and pre-period ($\beta^{q,1980}$) and post-period ($\beta^{q,2016}$) development shares at the boundaries match the coefficient estimates in the spatial regression discontinuity analysis. We operationalize this requirement by constructing moments restricted to a window around the regulatory boundary (100 feet). Even within this narrow window, average amenities and construction costs across the boundary may diverge from the differences at the limit. Because of this, we construct moment conditions that directly target the regression discontinuity estimates from Section 4.1 while allowing for mean differences across the boundary in the 100-foot window. The second set of moments are based on the exogeneity of instruments $\tilde{p}_{jz}$, average characteristics $X_{j'z'}$ of distant locations in the same market, and own-location characteristics X_{jz}. Appendix D.2 specifies these moment conditions, provides more details about the estimation procedure, discusses the data used for estimation, and assesses model fit.

5.4.4 Parameter Estimates

Table 4 presents select parameter estimates with standard errors in parentheses.[30] We estimate that α^D (price elasticity of demand) is approximately -1.3.[31] Flood zone status is disliked by consumers (negative ϕ) and imposes costs on developers (positive ψ): consumers are willing to pay 27% more to avoid living in a floodplain, and it costs developers 24% more to build a compliant home. Intuitively, we arrive at these numbers by rationalizing the changes in quantity and price around the regulatory boundary with estimates of how consumers trade off home prices with other attributes and housing supply elasticities.

The result that consumers are willing to pay 27% 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, 2023).[32] This difference could be attributed to our setting: residents living in flood-prone Florida may be particularly sensitive to signals of risk as awareness of climate change grows. Nevertheless, a 27% premium on avoiding the floodplain exceeds the risk difference between the floodplain and unregulated areas, equal to 11% of average house price. Flood zone designation may cause consumers to over-update beliefs about risk, as the policy does not communicate risks in terms of expected damages. There may also be hassle costs associated with complying with floodplain regulations that could contribute to this estimated effect.

The estimated 24% increase in construction costs is also large, but within the plausible (albeit wide) range of estimates of the effect of building codes and zoning regulations on construction costs (Listokin and Hattis, 2005; Emrath, 2021). Survey evidence on regulatory costs (Emrath, 2021) and our own interviews indicate that our cost estimate of approximately \$40,000 is reasonable.[33] The results in Section 4.2, combined with estimates of average forward-looking risk levels from our hydrological model, indicate that mandating adaptation reduces expected flood damages by 5% of home value. On average, the benefits of mandatory adaptation are less than costs. However, this comparison not only holds the regulation's effects on new development constant, but also masks substantial heterogeneity across locations within the flood zone. Mandating adaptation in the highest-risk areas will generate social value, but in safer ones, will incur social costs (see Appendix Table A.6).

[30] Appendix Table A.9 presents parameter estimates under alternative assumptions about supply elasticities and alternate values of boundary discontinuity estimates.

[31] The magnitude of this parameter is in line with existing estimates of choice in urban settings among geographies of similar granularity (Calder-Wang, 2021).

[32] A meta-analysis (Beltran et al., 2018) finds an even wider range of a 75.5% discount to a 61% premium.

[33] Emrath (2021) surveys land developers and single-family builders and finds that costs incurred during development and construction due to housing regulations total between \$65,224 (2011 survey) and \$93,870 (2021 survey).

6 Model-Informed Estimates

We use our estimates to quantify the total benefits and costs of *status quo* regulation and alternative policy designs. First, we investigate the impact of floodplain regulation by simulating an equilibrium with *versus* without the *status quo* policy. Then, we compare the performance of the observed policy to a counterfactual first-best that imposes corrective taxes in the spirit of Pigou (1920), equal to the present discounted value of expected damages in each tract-by-flood-zone.[34] Finally, we propose and evaluate a change in the targeting of the *status quo* policy, holding the binary policy design constant while changing the location of the regulated areas. Motivated by our estimates that imposing adaptation may only be socially-optimal in the highest-risk locations, this counterfactual restricts regulation to where the risk-reducing benefits of mandating adaptation exceed costs.

For each factual and counterfactual policy — determined by flood zone designation $SFHA_z$ and taxes τ_{jz} — we search for a price vector P_{jz}^{cf} and adaptation status E_{jz}^{2016} that equates housing supply and demand in each location:

$$L_{jz}\Phi\left(\frac{\ln\left(P_{jz}^{cf}\right)-\psi E_{jz}^{2016}-\mu_j-\eta_{jz}^{2016}}{\sigma_j}\right)=$$

$$N_m\frac{\exp\left(\alpha^D\ln\left(P_{jz}^{cf}+\tau_{jz}\right)+\phi SFHA_z+X_{jz}\beta+\xi_{jz}\right)}{\Sigma_{j'\in J_m,\,z'\in\{0,1\}}\exp\left(\alpha^D\ln\left(P_{j'z'}^{cf}+\tau_{j'z'}\right)+\phi SFHA_{z'}+X_{j'z'}\beta+\xi_{j'z'}\right)} \tag{10}$$

given our estimated parameters and values of ξ_{jz} and η_{jz}.[35] Under the *status quo* policy, τ_{jz} is equal to 0 and houses are adapted when it is mandated in the flood zone. Under the unregulated benchmark (no policy), $SFHA_z = 0$ and $\tau_{jz} = 0$. In this scenario, there exists no requirement to adapt to a minimum standard, and consumers face no disincentives to locate in risky areas (e.g. an insurance mandate or higher insurance premiums). Under the corrective taxation policy, $SFHA_z = 0$ and taxes vary with both location and adaptation status. Homes are endogenously adapted in locations where it is socially-efficient to do so, that is, where the gains from lower tax rates on an adapted house exceed the difference in construction costs. Solving equation 10 generates counterfactual prices and quantities of

[34] This policy could be implemented as mandatory flood insurance with actuarially fair premiums. In 2021, NFIP pricing was indeed reformed from a very coarse to a more granular scheme. However, even under this recent change, premiums are constrained, adaptation continues to be mandated in all regulated areas, and flood zone status is still the primary information available to consumers at the time of purchase (Mulder and Kousky, 2023).

[35] We take the housing stock as of 1980 as given and hold it constant across counterfactuals.

development in each of the locations jz. Counterfactuals assume a closed city, or that the 2016 population in each county is held constant at observed levels.

We quantify the magnitude of flood-risk reduction across counterfactuals — the policy's intended goal. We also decompose the risk reduction due to building in safer areas *versus* mandated adaptation through building standards. This risk quantification exercise relies on both hydrological-model-based estimates of flood risk by location and our estimates from Section 4.2 on the risk-reducing benefits of the imposed building standards.[36] Consistent with results in Appendix Table A.6 showing larger damage reductions in higher-risk areas, we assume that the effect of adaptation on flood damages is proportional to the level of flood risk of each location. Appendix E provides more details on damage calculations.

Quantifying the risk-reducing benefits of each policy does not require a normative welfare framework. In Section 6.2, we add further assumptions in order to calculate the social welfare impacts of observed and counterfactual policies.

6.1 Effects on Flood Damages

Table 5 presents results. Relative to a counterfactual without the policy, *status quo* floodplain regulation (column 2) increases the number of new flood-adapted houses by 563,489 (12% of all new development) and reallocates more than 180,000 houses (4% of all new development) out of the regulated floodplain. This is a 17% reduction in construction in regulated areas, equal to the 17% reduction estimated in the boundary discontinuity design.[37]

The policy achieves its goal of reducing flood damages, cutting them by \$8,865 per newly-developed house, for a 62% reduction in damages. The gains from adapted construction mandated via building standards account for 84% of this reduction, while incentivizing construction in safer areas contributes the remaining 16%. These damage reductions are substantial, both in absolute numbers (a decrease in \$3.8 billion of expected damages per county), and in comparison to alternative policy instruments: the number of homes relocated in just these eleven counties exceeds the total number of houses removed from risky areas under FEMA-financed home buyouts across the entire nation.[38]

[36]We use this external measure of flood risk rather than the FEMA flood maps because the hydrological-model-based estimates are more accurate, granular, and forward-looking, as they incorporate projections of sea level rise.

[37]While the average reduction in new construction in the regulated area matches the boundary discontinuity estimate, this was not a targeted moment. We match the price and quantity differences at the boundary; aggregate reductions could differ from differences at the boundary either due to amenities or construction costs that are correlated with regulation status or equilibrium effects on prices.

[38]Authors' calculations using the Hazard Mitigation Assistance Mitigated Properties OpenFEMA dataset.

The damage reductions due to the *status quo* policy are approximately equal to those under a first-best corrective tax on flood risk (-64%, in column 3). However, the corrective tax achieves these damage reductions with fewer distortions in the housing market: 43% as many houses are relocated out of the designated floodplain and 36% as many houses are adapted. Though it incentivizes less adaptation, the tax provides stronger incentives to suppress development in high-risk locations.

To illustrate how impacts vary by risk level, Figure 5 plots the effects of each policy on new development and damages by flood risk ventile. Both the *status quo* and the corrective tax policy reallocate new construction away from the riskiest 20% of land to lower-risk locations. However, the corrective tax concentrates almost all of its curtailments in the top decile of risk, with particularly strong reductions in the top ventile. By contrast, the effects of the *status quo* policy are more diffuse. Crucially, as shown in Figure 5b, the top decile — and in particular, the top ventile — of risk has a much highest potential to avert damages. Thus, compared to the *status quo*, the corrective tax leverages more reallocation from the highest-risk areas to achieve greater total damage reductions with less reallocation overall.

A simple change in the targeting of the *status quo* policy (column 4) also achieves similar damage reductions (-61%) with less adaptation, while regulating only half the area as the *status quo* policy. This occurs by two channels. First, by restricting regulation to the locations where the risk-reducing benefits of mandating adaptation exceed costs, mandatory adaptation is relaxed in lower-risk areas where effects on damages are small. Second, restricting the regulated area provides stronger disincentives to build in the highest-risk locations, relative to the more expansive *status quo*. Figure 5 demonstrates that — like the corrective tax — the targeted policy also leads to larger reductions in new construction on land in the top 5% of flood risk, relative to the *status quo*.

These findings suggest that *status quo* regulations generate the intended benefits of substantial reductions in expected flood damages. However, an important question is the extent to which the costs of housing market distortions offset the policy's benefits and whether these benefits could be achieved with smaller costs.

6.2 Effects on Social 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. Based on results — in both our setting and others — that consumers do not internalize flood risk in their housing decisions, we define social welfare as the sum of consumer surplus, producer surplus, government revenue raised, and uninternalized expected flood damages. Expected flood damages are included in the calculation of social welfare despite the fact that consumers do not appear to value them because they impose social costs, either onto the government (externalities) or onto the consumers themselves (internalities).

To calculate consumer surplus, we must make an assumption about the normative interpretation of the effect of floodplain status on consumer choices. Does $\phi SFHA_z$ represent real costs or simply a change in expectations about flood risk? A large body of work suggests that misperceptions likely drive consumers' failure to internalize flood risk (Gallagher, 2014; Bakkensen and Barrage, 2022; Wagner, 2022; Mulder, 2022).[39] Therefore, we assume in our baseline calculations that the magnitude of ϕ in excess of the financial costs of living in a regulated area (via increased insurance prices) is *not* welfare-relevant, and is instead de-biasing as in Allcott and Taubinsky (2015). This would be incorrect if flood zone status imposed substantial hassle or psychic costs on consumers. While we think that the magnitude of these costs is likely to be small, we also report results under the assumption that ϕ is fully welfare-relevant and represents costs imposed upon consumers.

We report results in Table 6, comparing social welfare under the observed regulation and counterfactual policies to the unregulated benchmark.[40] In our baseline specification, *status quo* floodplain regulation achieves over \$14 billion of social welfare gains in our setting, or \$2,975 per newly-developed house.[41] Because of the wedge between the private and social value of flood risk, risk would be inefficiently-high in the absence of any policy intervention. However, the social welfare effects of the *status quo* policy are theoretically ambiguous, as the risk-reducing benefits need not exceed the costs imposed via distortions in the housing market. We estimate positive effects on social welfare as the policy corrects inefficient exposure to flood risk. The policy would need to impose substantial deadweight loss via the demand term ϕ — imposing hassles approximately equal to the magnitude of the entire financial costs of living in a flood zone — to overturn the result that the *status quo* policy leads to a net increase in social welfare.

[39]See Wagner (2022) for a discussion about the likely contribution of misperceptions relative to other frictions in explaining observed consumer choices in the face of flood risk.

[40]We compute per-person consumer surplus in each market as $-\frac{1}{\alpha^D}\ln \Sigma_{j,z}\exp(\alpha^D p_{jz} + \phi_F SFHA_z + X_{jz}\beta + \xi_{jz})$, where ϕ_F is set to equal the estimated present discounted value of insurance premiums. We define producer surplus as price minus costs. We compute equivalent variation, relative to the unregulated benchmark, for both producer and consumer surplus to account for the fact that prices are modeled in logs. See Appendix F for more details.

[41]We calculate social welfare per newly-developed house because the *status quo* and counterfactual policies have no effect on social welfare for existing development. They do, however have distributional implications. Relative to the unregulated benchmark, the *status quo* policy leads to a large transfer from consumers to property owners and the government.

Despite achieving the socially-optimal degree of damage reduction, the *status quo* policy introduces distortions that reduce its social welfare gains, which equal only one-third of the first-best gains implemented by the corrective tax. This is due to two features of the *status quo* policy. First, it is over-corrective: on average across regulated areas, the costs of mandated adaptation exceed risk-reducing benefits. Therefore, holding the location of new construction constant, mandatory adaptation leads to a net social welfare loss. The *status quo* policy delivers social welfare gains only because it suppresses development in regulated, high-risk locations.[42] Second, using a binary policy to correct continuous flood risk is inefficient; it still permits *too much* construction in the riskiest locations (Figure 5).

While the corrective tax implements the first-best, it involves substantial changes to policy design that may be challenging to implement in practice (see Mulder and Kousky (2023) for a discussion). We conclude by assessing whether there are simple changes that can improve upon the *status quo*. Table 5 (column 4) and Figure 5 highlight the possibility of social welfare improvements through changes in the targeting of regulated areas; columns 4 and 6 of Table 6 tabulate them. By imposing regulations only in areas where the social benefits from adaptation exceed costs, improved targeting of the *status quo* regulation mitigates over-corrective mandatory adaptation and cuts development in the highest-risk locations to be closer to socially-optimal levels. This leads to substantial social welfare gains, relative to the *status quo*, yet entails only a "remapping" of regulated areas. When well-targeted, the simple policy design can achieve over three quarters (79%) of first-best social welfare gains, or $7,041 per newly-developed house.

7 Conclusion

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

Local to the regulatory boundary, floodplain regulation decreases construction and increases house prices. Flood-safe building standards reduce flood damages, but these reductions are not privately valued by consumers. Using a model to interpret these results, we find that

[42] We obtain this decomposition by calculating social welfare under counterfactual locations and prices, holding adaptation constant at the unregulated benchmark. This yields greater social welfare gains than the *status quo* policy.

floodplain regulation in Florida reduces damages by 62% and increases social welfare.

Our results stand in contrast to common critiques of the National Flood Insurance Program, which argue that it promotes inefficiently high development in risky areas (e.g. Ben-Shahar and Logue (2016)). We find instead that the *status quo* policy leads to *approximately efficient* levels of flood risk and social welfare gains. However, there is still scope for substantial improvement. Much of it can be achieved via simple remappings of regulated areas, which implement almost 80% of first-best social welfare gains.

Our results come with caveats and directions for future work. Our model is static, and regulating durable construction in the face of evolving flood risk may involve additional unmodeled considerations. And while we provide evidence suggesting that consumers do not internalize flood risk, more work is needed to precisely identify the source of this friction.

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. Policy proposals (e.g. for home buyouts) focus largely on the benefit of reduced risk. However, as we highlight, equivalent risk reductions can entail substantially different costs. When designed to balance these benefits and costs, even simple, existing policy instruments can promote efficient adaptation to climate change.

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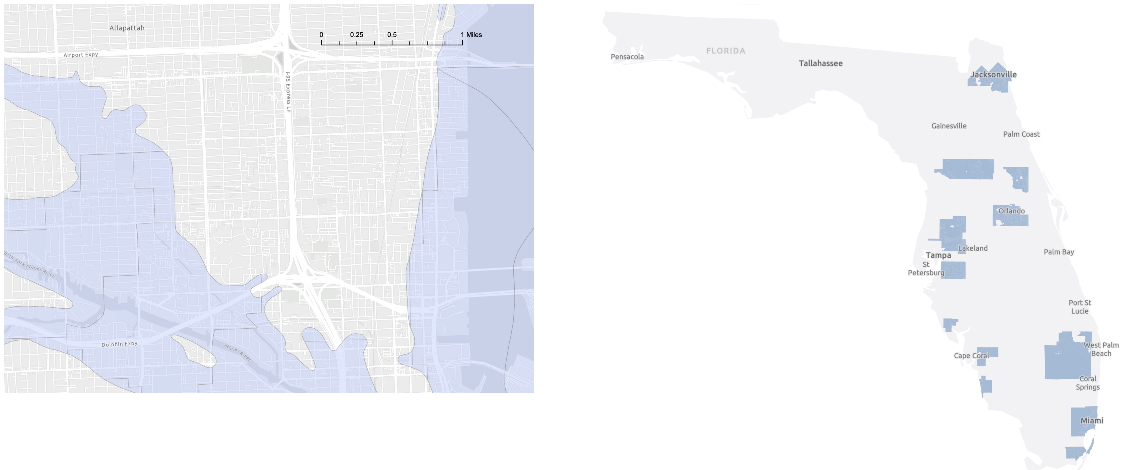
Tables and Figures

Figure 1: Digitized Flood Maps

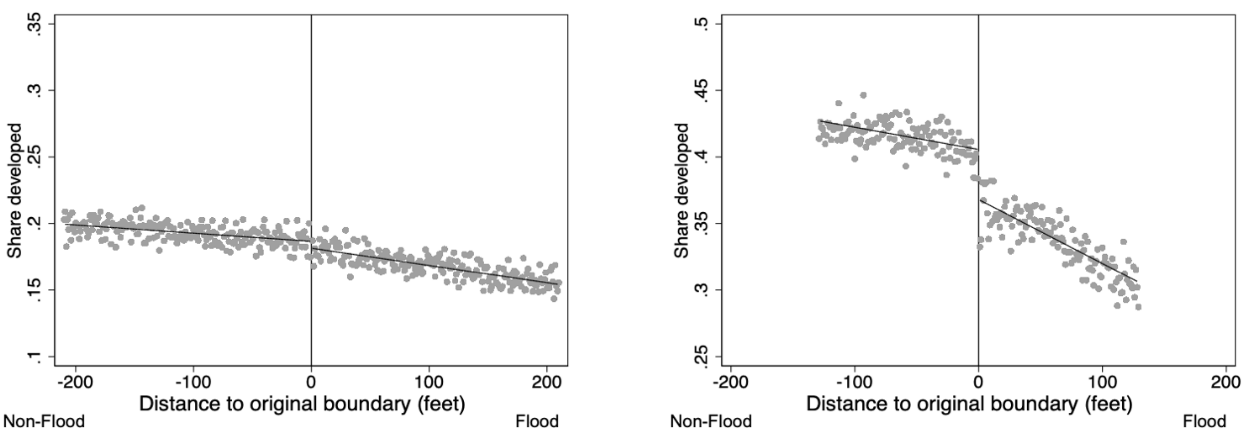
(a) Digitized Map of Miami (1978) (b) Coverage of Digitized Maps



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

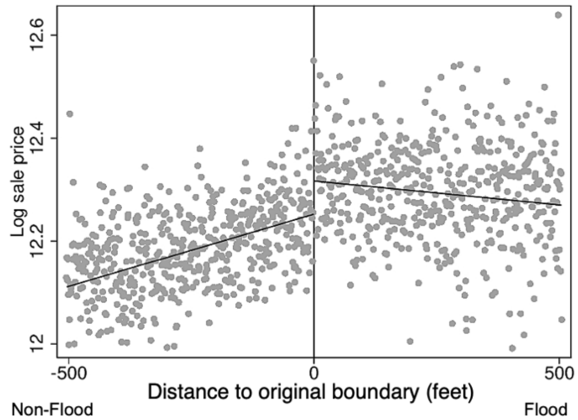
Figure 2: Spatial Regression Discontinuity Estimates: Development

(a) Pre-Period Share Developed (about 1980) (b) Share Developed in 2016



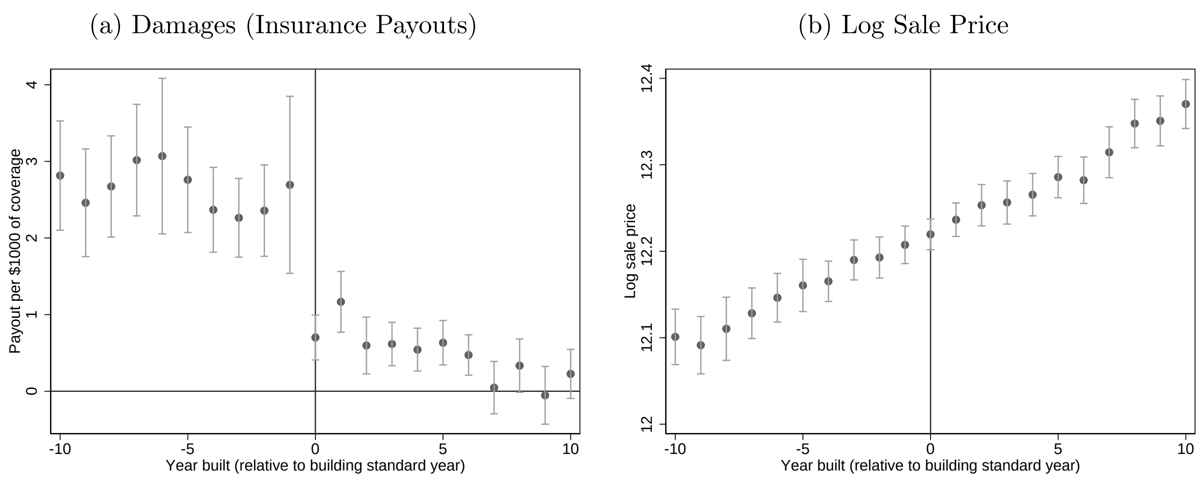
Notes: Figures present spatial regression discontinuity plots with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Sub-figure (a) plots the share of land that was developed as of the late 1970s and early 1980s, and sub-figure (b) plots the share of land developed as of 2016. Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure 3: Spatial Regression Discontinuity Estimates: Log Single-Family Sale Price



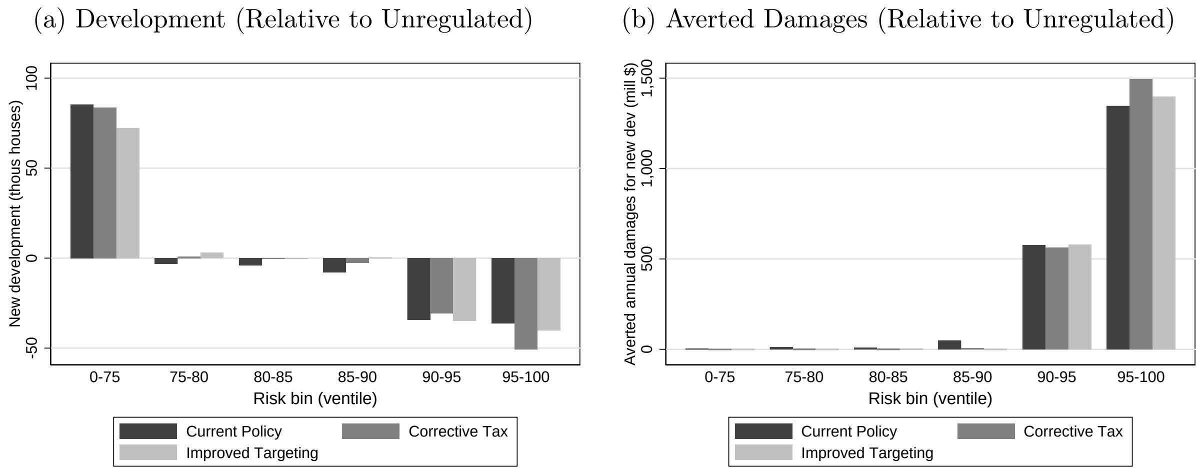
Notes: Figure presents spatial regression discontinuity plot with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Outcome is log sales prices of arms-length sales for single-family homes that sold between 2005 and 2020. Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure 4: Event Study Estimates: The Effect of Building Standards



Notes: Figures present coefficients on bins of year built relative to the year of a community's enrollment in the National Flood Insurance Program (at which time building standards began to be imposed on newly-constructed housing), from equation 2. Sample is restricted to single-family residences inside the flood zone. Sub-figure (a) shows insurance payouts from 2010 to 2018 as a share of total dollars of coverage (estimated using the specification in line 1 of equation 2). Sub-figure (b) shows the log sale price in 2010 dollars between 2005 and 2020 (estimated using the specification in line 2 of equation 2). Coefficients in sub-figure (a) are estimated at the policy-year by census-tract by year-built level, weighted by the number of policies. Coefficients in sub-figure (b) are estimated at the house level, unweighted. For houses sold multiple times, we take the log of the average transaction price across sales. Standard errors are clustered at the census tract level.

Figure 5: New Development and Averted Annual Damages by Flood Risk of Location



Notes: Figure plots the number of new houses constructed (sub-figure (a)) and expected annual damages averted among newly-constructed houses (sub-figure (b)), by ventile of estimated flood risk under the observed policy, the corrective tax policy, and the improved targeting policy, relative to the unregulated benchmark. Ventiles representing land up to the 75th percentile of flood risk are pooled. Sample includes both flood zone and non-flood-zone land. Risk is measured as Average Annual Loss (as computed by First Street Foundation). New houses developed are measured in thousands of houses. Annual averted damages are measured in millions of dollars per year.

Table 1: Summary Statistics

	Florida	Digitized map sample		Boundary sample	
		Outside historic SFHA	Inside historic SFHA (the flood zone)	Outside historic SFHA	Inside historic SFHA (the 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	0.749	0.739	0.734	0.747
Share post-FIRM	0.616	0.606	0.520	0.478	0.429
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. House Prices (Median)					
Price (1980)	47,459	48,198		46,912	
Price (2005-2020)	167,233	177,673	274,065	188,148	252,935
Price (single-family, 2005-2020)	182,452	179,419	298,716	190,204	277,492
Panel D. Risk					
FEMA flood maps					
Land share in SFHA as of 1996	0.345	0.040	0.739	0.061	0.707
Land share in SFHA as of 2017	0.450	0.106	0.720	0.130	0.664
First Street risk measures					
Land share w. $\geq$ 1% risk of flooding	0.447	0.425	0.582	0.240	0.573
Land share w. 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 100-year flood depth above two feet.

Table 2: Spatial Regression Discontinuity Estimates

	Outside flood zone mean	Local linear: Triangular kernel, optimal bandwidth	Local linear: Rectangular kernel, constant bandwidth	Fourth-order polynomial	Local linear: Excl. coastal areas
	(1)	(2)	(3)	(4)	(5)
Panel A. Historical land use					
Share (of land) developed in 1980	0.189	-0.005	-0.004	-0.003	-0.005
		(0.002)	(0.002)	(0.002)	(0.002)
		542,302	642,720	3,027,326	543,594
Panel B. Modern land use					
Share (of land) developed in 2016	0.411	-0.038	-0.044	-0.042	-0.035
		(0.005)	(0.005)	(0.005)	(0.005)
		323,362	642,720	3,027,326	277,662
Share covered by a building footprint	0.238	-0.025	-0.026	-0.029	-0.023
		(0.004)	(0.004)	(0.004)	(0.004)
		382,470	642,720	3,027,326	332,234
Share covered by a single-family home	0.090	-0.010	-0.013	-0.015	-0.009
		(0.003)	(0.002)	(0.003)	(0.003)
		371,202	642,720	3,027,326	309,866
Share covered by wetlands	0.145	0.072	0.096	0.110	0.075
		(0.006)	(0.007)	(0.007)	(0.007)
		221,336	642,720	3,027,326	187,032
Panel C. Prices					
Log house price	12.278	0.065	0.063	0.064	0.052
		(0.019)	(0.021)	(0.022)	(0.023)
		59,341	35,493	186,171	44,298
Log single-family house price	12.241	0.064	0.060	0.055	0.058
		(0.015)	(0.019)	(0.018)	(0.019)
		52,605	27,984	146,989	28,263

Notes: Table displays estimates of equation 1. Outside-of-flood-zone means are calculated within 50 feet of the boundary. Column 2 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 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 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 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. Observations are grid cells. Robust standard errors (in parentheses) are clustered at the census tract level. Sample sizes are included below each standard error.

Table 3: The Effect of Building Standards: Event Study Coefficient Estimates

	Mean in flood zone, Pre-FIRM (1)	Event study (2)
Variable		
Insurance payouts (per $1000 of coverage)	$2.93	$-1.60
		(0.40)
		225,998
Log house price (sold 2005-2020, in 2010 $)	12.21	-0.007
		(0.009)
		173,458

Notes: Table presents variable means and coefficient estimates from equation 3 on insurance payouts and log house prices. Insurance payout data come from residential National Flood Insurance Program policies from 2010-2018. Price data come from residential sales prices in 2005-2020. Coefficients with insurance payouts as the outcome are estimated at the policy-year by census-tract by year-built level, weighted by the number of policies. Coefficients with log house price as the outcome are estimated at the house level. For houses sold multiple times, we take the log of the average transaction price across sales. Sample includes all single-family residences in Florida. Standard errors (in parentheses) are clustered at the census tract level. Sample size of each regression is listed below standard errors.

Table 4: Selected Parameter Estimates

Supply	
Cost of adaptation (ψ)	0.243
	(0.037)
Demand	
Coefficient on SFHA (ϕ)	-0.367
	(0.097)
Coefficient on log price (α^D)	-1.344
	(0.222)
WTP to avoid SFHA (ϕ/α^D)	0.273

Notes: Table presents coefficient estimates for household preferences and housing supply, estimated via two-step GMM (see Appendix D.2 for more details). Standard errors are in parentheses.

Table 5: Counterfactuals: Housing Market Outcomes

Outcome	Level	Change Relative to Unregulated Counterfactual		
	Unregulated	Current Policy	Corrective Tax	Improved Targeting
	(1)	(2)	(3)	(4)
New dev on land regulated in current policy				
Approximate N Houses	1,047,291	-181,453	-77,301	-107,415
		-17.3%	-7.4%	-10.3%
New dev on all land				
Approximate N Houses	4,736,719	0	0	0
Number of Adapted Houses	302,349	563,489	200,156	167,564
		186%	66%	55%
Per-house NPV of adaptation-based damages	$14,343	$-7,426	$-7,129	$-7,129
(locations held at unreg. benchmark)		-51.8%	-49.7%	-49.7%
Per-house NPV of all damages	$14,343	$-8,865	$-9,147	$-8,758
(incorporate counterf. locations)		-61.8%	-63.8%	-61.1%
Price				
Inside observed SFHA	$255,002	$7,112	$-8,962	$-12,042
		2.8%	-3.5%	-4.7%
Outside observed SFHA	$166,522	$12,251	$3,534	$6,106
		7.4%	2.1%	3.7%
Overall	$186,841	$9,097	$-103	$952
		4.9%	-0.1%	0.5%

Notes: Table presents estimates of counterfactual outcomes using baseline parameter estimates. Outcomes for the observed policy, corrective tax, and improved targeting counterfactuals are presented in differences from the modeled outcomes under the unregulated benchmark. Percentage differences from the unregulated benchmark are presented below the level differences. N houses is defined by assuming that each developed pixel is equivalent to one house. "Land regulated in current policy" is the area designated as the flood zone in the original flood maps. "Per-house NPV of damages" is the net present value of damages for newly-built houses divided by the number of all newly-built houses. "Adaptation-based damages" computes expected damages under counterfactual adaptation decisions, holding location constant at location choice as modeled under the unregulated benchmark. "All damages" computes damages under counterfactual adaptation and location decisions. 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 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 adaptation choices. The improved targeting counterfactual sets $SFHA_z = 1$ only in the locations regulated under the status quo where it is socially-optimal to adapt.

Table 6: Counterfactuals: Social Welfare

Outcome (Billions of $)	Change Relative to Unregulated Counterfactual				
	Non-financial demand cost not welfare-relevant			Non-financial demand cost welfare-relevant	
	Current Policy (1)	Corrective Tax (2)	Improved Targeting (3)	Current Policy (4)	Improved Targeting (5)
Producer Surplus	7.6	4.4	6.8	7.6	6.8
Consumer Surplus	-51.7	-35.4	-23.9	-89.3	-43.3
Expected Flood Damages	-42.0	-43.3	-41.5	-42.0	-41.5
Government Revenue	16.1	29.9	8.9	16.1	8.9
Social Welfare	14.1	42.2	33.4	-23.6	13.9

Notes: Table presents estimates of counterfactual outcomes using baseline parameter estimates. Outcomes are in billions of 2010 dollars. The unregulated counterfactual sets $SFHA_z = 0$ everywhere. Columns 1-3 present results under the assumption that the magnitude of ϕ in excess of the financial costs of SFHA designation (via increased premiums) is not welfare-relevant (baseline). Columns 4-5 present results under the alternative assumption that it is. Columns 1 and 4 present the social welfare effects of the observed policy. Column 2 presents the social welfare effects of the first-best corrective tax counterfactual, which sets $SFHA_z = 0$ everywhere and imposes taxes equal to expected flood damages; homes are adapted when it is socially-optimal to do so. Columns 3 and 5 report effects under the improved targeting counterfactual, which sets $SFHA_z = 1$ only in the locations regulated under the status quo where it is socially-optimal to adapt. Government revenue indicates revenue from insurance premiums and taxes.

Supplemental Appendix for

The Effects of Floodplain Regulation on Housing Markets

Abigail Ostriker and Anna Russo

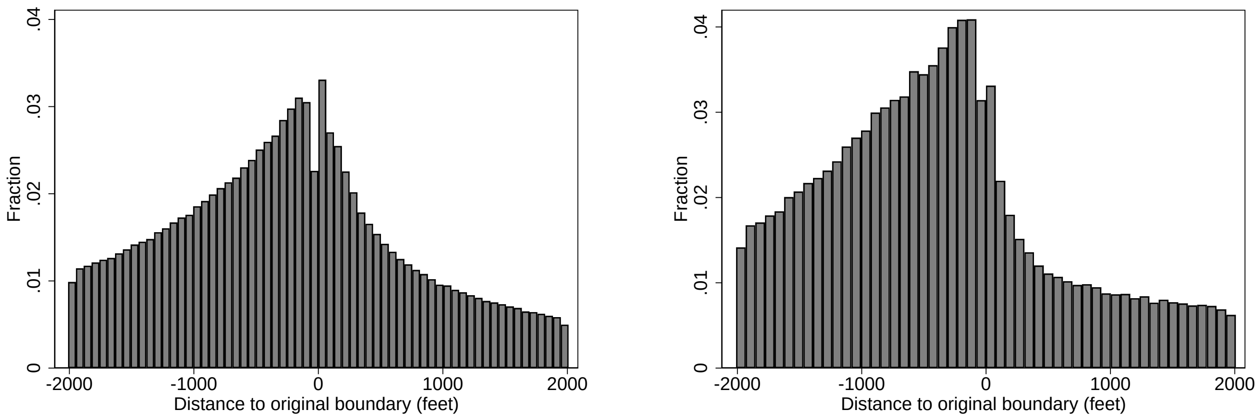
A Appendix Tables and Figures

Figure A.1: Building Satisfying Adaptation Requirements in Naples, FL

Notes: Figure shows a flood-safe 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

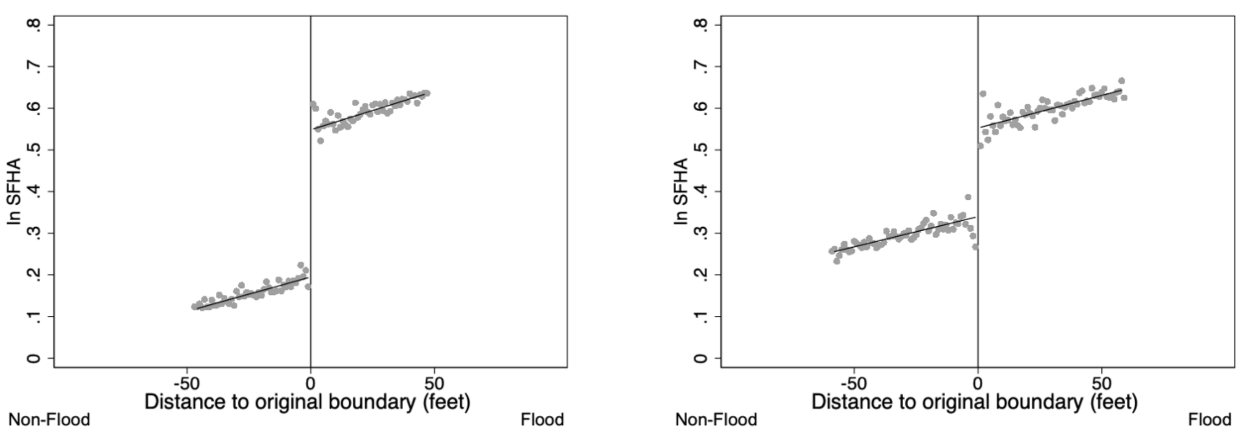
(a) Land (30x30m grid-cells) (b) Sales



Notes: Figure presents histograms of distance to boundary. Sub-figure (a) shows land observations, at the grid-cell level, and sub-figure (b) shows home sales observations. Distance to boundary is in feet, with positive distance indicating being inside the flood zone. Excludes boundaries that trace a body of water and grid-cells that overlap with the boundary (leading to the dip at zero).

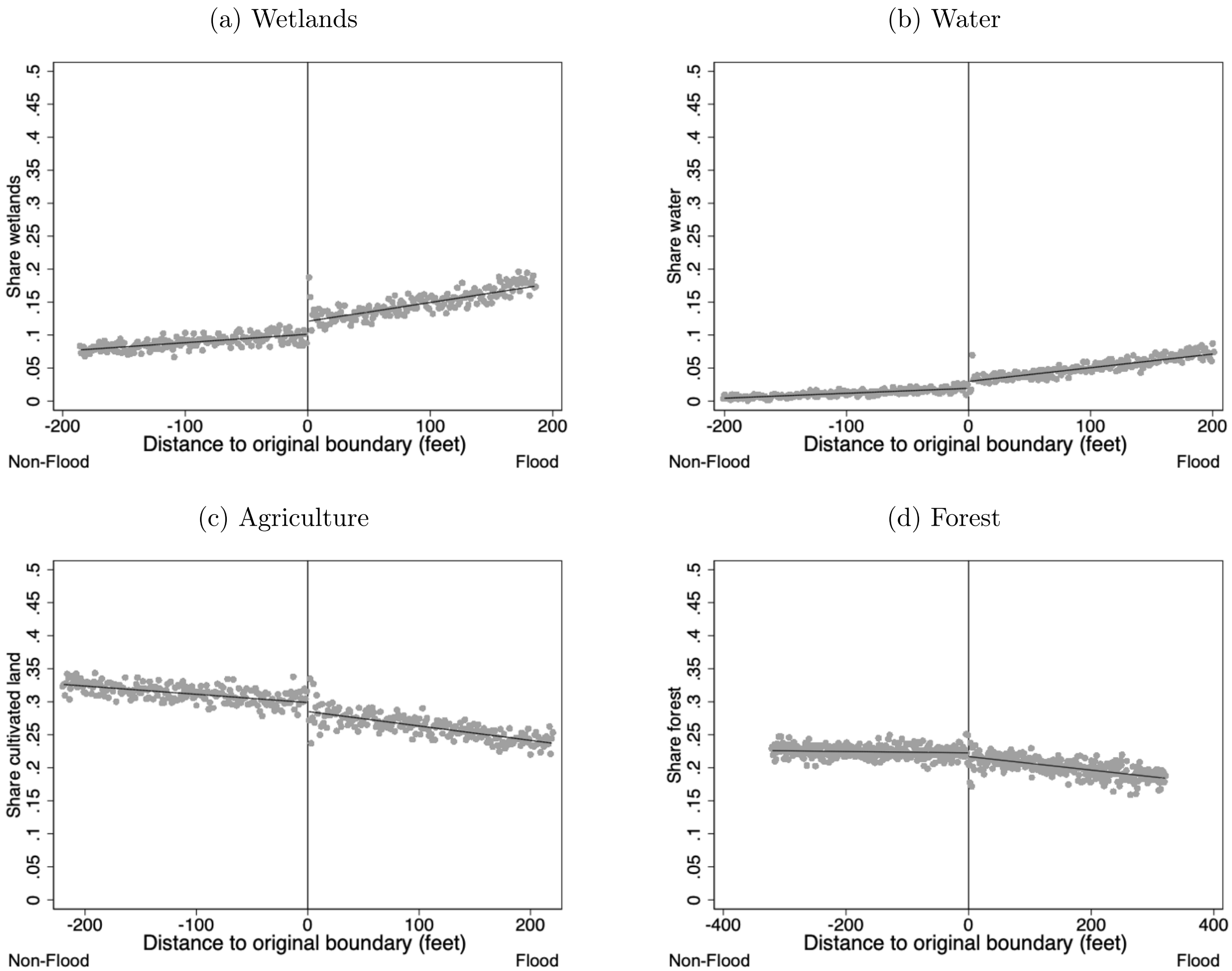
Figure A.3: Spatial Regression Discontinuity Estimates: Current Flood Zone Status

(a) Share of Land in 1996 Flood Zone (b) Share of Land in 2017 Flood Zone



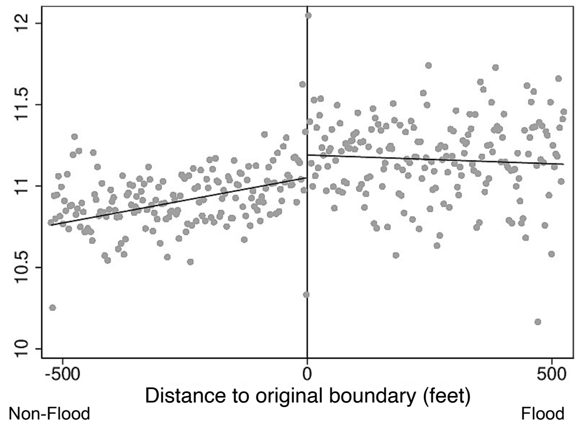
Notes: Figure presents spatial regression discontinuity plots with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Sub-figure (a) plots the share of land in the 1996 flood zone, and sub-figure (b) plots the share of land in the 2017 flood zone. Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure A.4: Spatial Regression Discontinuity Estimates: Other Pre-Period Land Use



Notes: Figure presents spatial regression discontinuity plots with a local linear fit on either side of the flood zone boundary (equation 1). 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. Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure A.5: Spatial Regression Discontinuity Estimates: House Price Net of Attributes

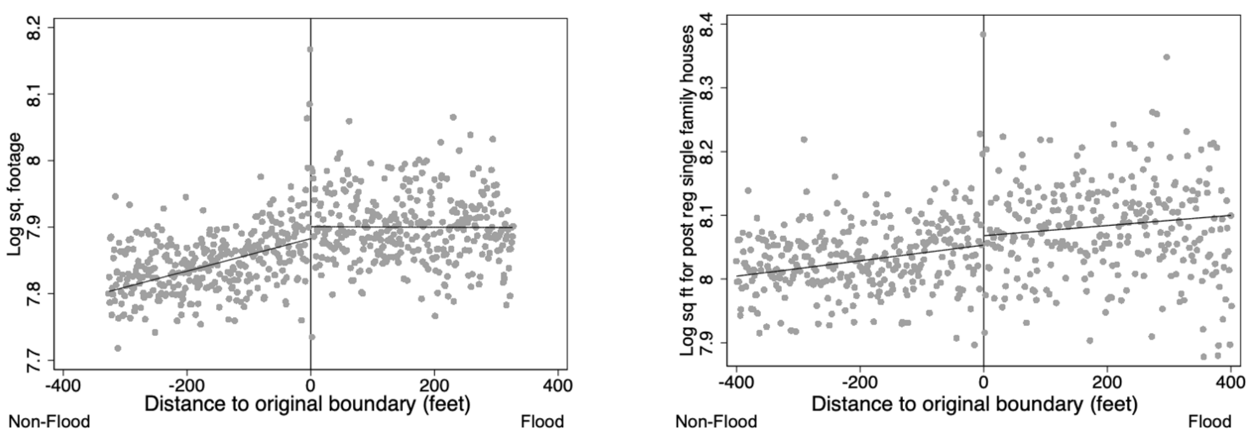


Notes: Figure presents spatial regression discontinuity plot with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Outcome is the residual from a regression of log sales prices of residential properties on polynomials in lot size and living area, indicators for construction year, and indicators for county-by-sale-month. Plotted points are binned averages of grid-cell-level observations. Estimates are also residualized of census tract fixed effects.

Figure A.6: Spatial Regression Discontinuity Estimates: Log Sq. Footage

(a) All single-family residential parcels

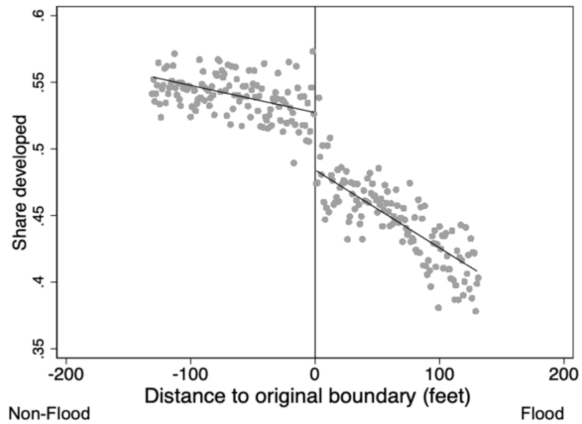
(b) Post-FIRM single-family residential parcels



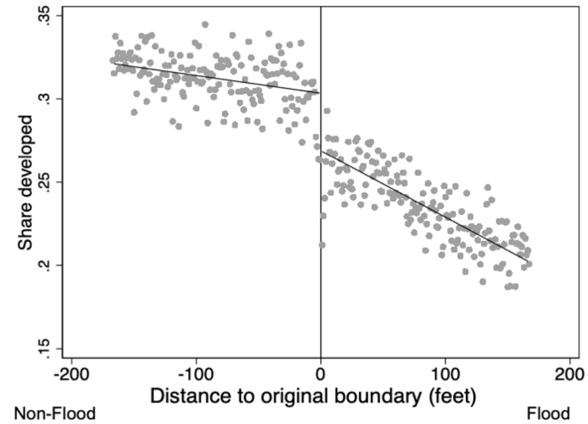
Notes: Figure presents spatial regression discontinuity plots with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Outcome is log square footage of homes in two samples: (a) single-family houses and (b) and single-family houses built after the introduction of building standards (post-FIRM). Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure A.7: Spatial Regression Discontinuity Estimates: Effects By Flood Risk Level

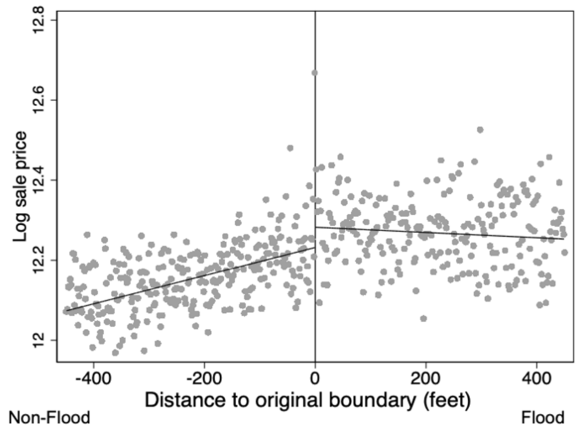
(a) Development in 2016, Low Risk



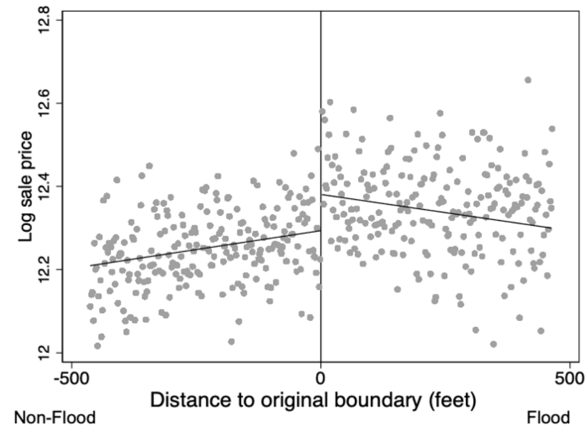
(b) Development in 2016, High Risk



(c) Log Single-Family House Price, Low Risk



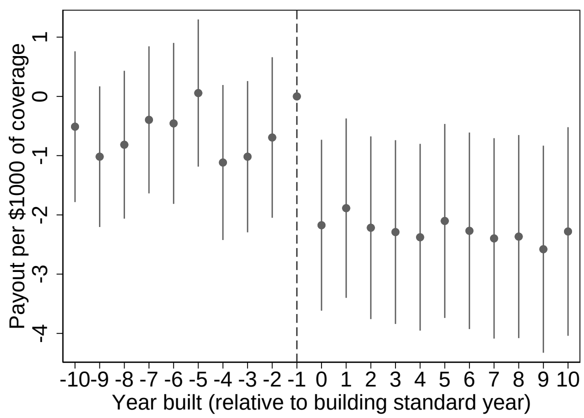
(d) Log Single-Family House Price, High Risk



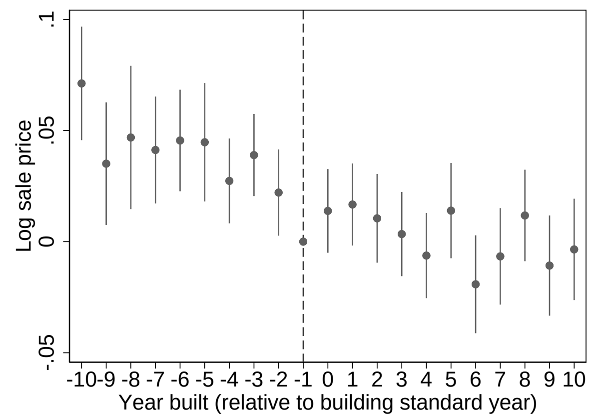
Notes: Figure presents spatial regression discontinuity plots with a local linear fit on either side of the flood zone boundary (equation 1). Distance to boundary is measured in feet, with positive distance indicating being inside the flood zone. Sub-figures (a) and (b) plot the share of land developed as of 2016. Sub-figures (c) and (d) plot log sales prices of arms-length sales for single-family homes with structures that sold between 2005 and 2020. Flood risk is calculated at the (1980) census tract level based on the First Street Foundation hydrological model. “High risk” denotes an above-median census tract and “low risk” denotes a below-median census tract in our sample. Plotted points are binned averages of grid-cell-level observations. Estimates are residualized of census tract fixed effects.

Figure A.8: Introduction of Building Standards: Difference-in-Differences Estimates

(a) Damages (Insurance Payouts)



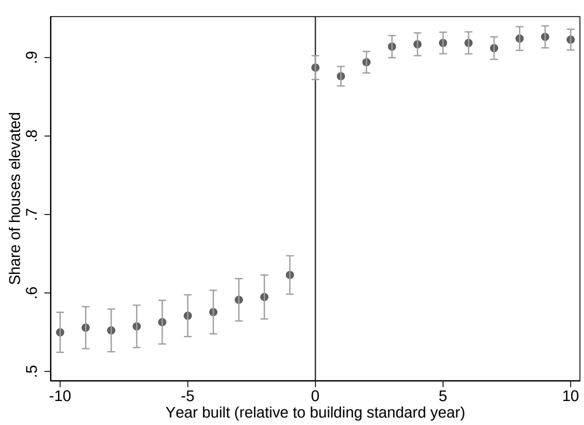
(b) Log Sale Price



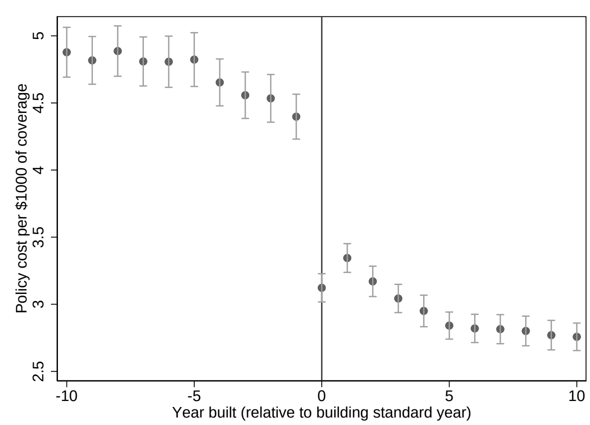
Notes: Figures present coefficients from the difference-in-difference specification comparing houses built before and after the year of building standards introduction, inside and outside of the flood zone (equation 12). The Sun-Abraham eventstudyinteract package is used to account for potential heterogeneity in treatment effects across cohorts (Sun, 2021). Sample is restricted to single-family homes. Sub-figure (a) shows insurance payouts from 2010 to 2018 as a share of total dollars of coverage, aggregated to the policy-year by census-tract by year-built level, and weighted by number of policies. Sub-figure (b) shows the log sale price in 2010 dollars between 2005 and 2020 in each census tract, estimated at the house level. Standard errors are clustered at the census tract level. 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 and is equal to -1.54 (standard error = 0.28) in (a) and -0.025 (standard error = 0.009) in (b).

Figure A.9: Introduction of Building Standards: Other Outcomes

(a) Share Elevated to Minimum Required Level

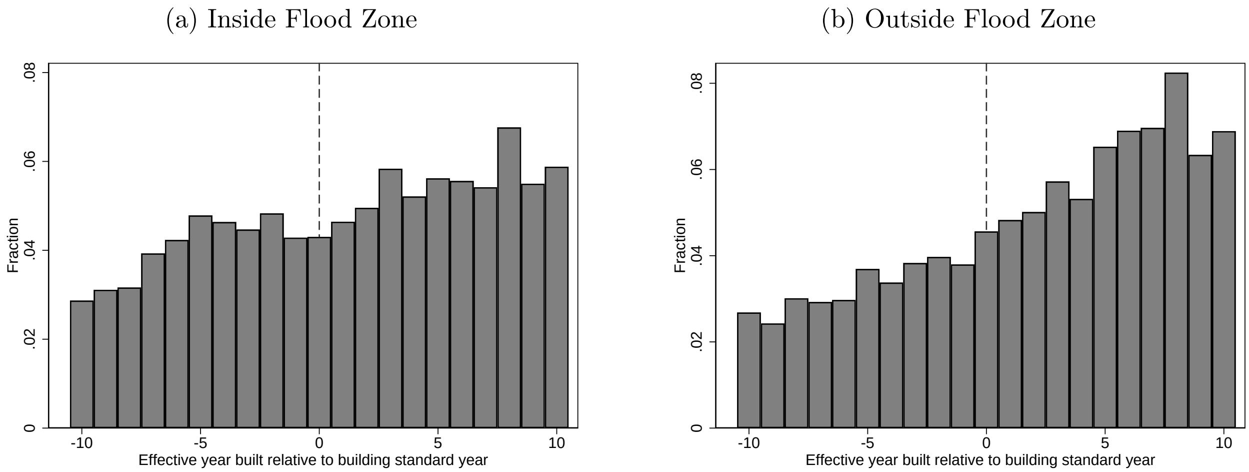


(b) Insurance Policy Cost



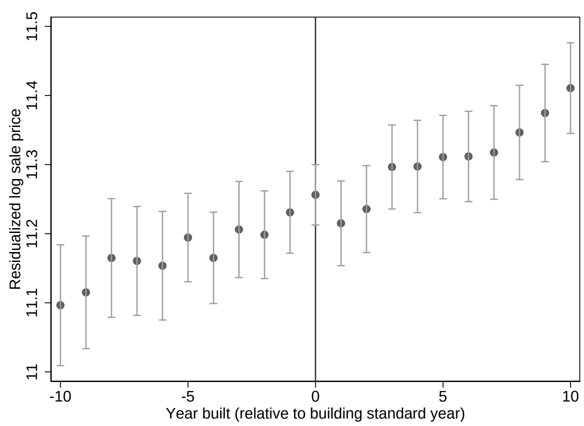
Notes: Figures present coefficients on bins of year built relative to the year of a community's enrollment in the National Flood Insurance Program (at which time building standards began to be imposed on newly-constructed housing) from equation 2. Sample is restricted to single-family residences inside the flood zone. Sub-figure (a) shows elevation status reported in National Flood Insurance Policies policies from 2010 to 2018, and sub-figure (b) shows policy cost from 2010 to 2018 as a share of total dollars of coverage (both estimated using the specification in line 1 of equation 2). Estimates are estimated at the policy-year by census-tract by year-built level, weighted by the number of policies. Standard errors are clustered at the census tract level.

Figure A.10: Histogram: Year Built Relative to Introduction of Building Standards



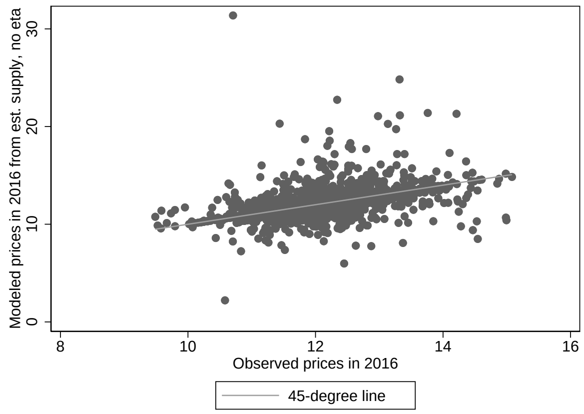
Notes: Figures present histograms of the year houses were built relative to the year building codes were introduced, both inside and outside the flood zone.

Figure A.11: Introduction of Building Standards: Residualized Log Sales Price



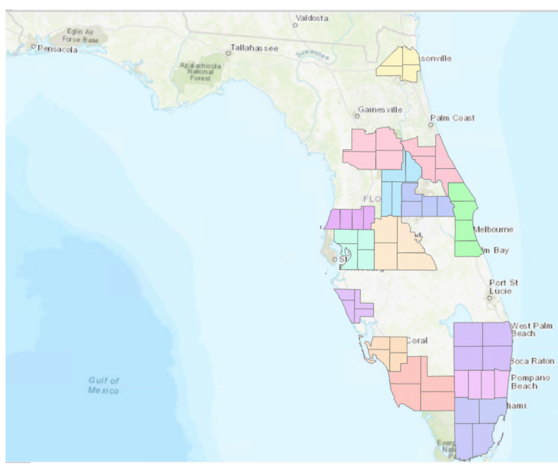
Notes: Figure presents coefficients on bins of year built relative to the year of a community's enrollment in the National Flood Insurance Program (at which time building standards began to be imposed on newly-constructed housing), estimated from equation 2 (line 2). Outcome is log sales price, residualized 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. Coefficient estimate (and standard error): -0.010 (0.023).

Figure A.12: Model Fit: Housing Supply Model Reliance on Structural Error Terms



Notes: Figure presents prices that rationalize observed development shares using just the estimated supply curve, without estimated idiosyncratic construction costs, i.e. the prices in 2016 that would rationalize the observed quantities of development, using the estimated parameters (μ_j, σ_j, ψ) and observed adaptation decisions, if the model omitted η^t_{jz}.

Figure A.13: Areas Considered for Digitization



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

Table A.1: Discrepancies Between Flood Zone Status and the First Street Model

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

Notes: Table calculates the share of all buildings in our eleven counties of interest that fall into each of the four mutually exclusive categories of National Flood Insurance Program flood zone status by modern-day (2017) First Street floodplain status, in our 11 counties of interest. Both "flood zone" categories designate areas that have been determined to be in a 100 year floodplain (i.e. they have a greater than 1 percent chance of flooding per year) by either First Street or the National Flood Insurance Program. 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 Flood Zone by Land Use

	Share of initial flood zone land that is mapped out
Any Developed	0.230
Developed - Open	0.160
Developed - Low	0.224
Developed - Mid	0.282
Developed - High	0.351
Wetlands	0.004
Water	0.003
Cultivated	0.068
Other Land Use	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, split by land use category in 2004. Sample is restricted to Marion and Dade Counties, because these were the two counties in our sample that experienced zero remappings between 1996 and 2004 and one remapping between 2004 and 2016. This restriction is motivated by data availability for both map updates and land use outcomes. Land use in 2004 is from the NLCD.

Table A.3: Spatial Regression Discontinuity Estimates: Other Land Use Outcomes (1980)

		Local linear		Fourth-order polynomial	Local linear
	Outside flood zone mean	Triangular kernel, optimal bandwidth	Rectangular kernel, constant bandwidth		Excl. coastal areas
	(1)	(2)	(3)	(4)	(5)
Wetlands	0.098	0.020	0.017	0.006	0.021
		(0.003)	(0.003)	(0.004)	(0.004)
		479,659	642,720	3,027,326	436,080
Water	0.017	0.011	0.010	0.004	0.010
		(0.002)	(0.002)	(0.003)	(0.002)
		518,899	642,720	3,027,326	446,653
Agriculture	0.301	-0.014	-0.014	-0.006	-0.016
		(0.003)	(0.003)	(0.004)	(0.004)
		567,993	642,720	3,027,326	429,803
Forest	0.223	-0.006	-0.006	-0.003	-0.007
		(0.003)	(0.002)	(0.003)	(0.003)
		809,732	642,720	3,027,326	656,567

Notes: Table displays estimates of equation 1. Outside-of-flood-zone means are calculated within 50 feet of the boundary. Column 2 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 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 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 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. Observations are grid cells. Robust standard errors (in parentheses) are clustered at the census tract level. Sample sizes are included below each standard error.

Table A.4: Spatial Regression Discontinuity Estimates: Other Sale Price Estimates

		Local linear		Fourth-order polynomial	Local linear
	Outside flood zone mean	Triangular kernel, optimal bandwidth	Rectangular kernel, constant bandwidth		Excl. coastal areas
	(1)	(2)	(3)	(4)	(5)
Panel A. Log sale price of single-family homes					
Baseline	12.2	0.064	0.060	0.055	0.058
		(0.015)	(0.019)	(0.018)	(0.019)
		52,605	27,984	146,989	28,263
Residualized of characteristics	11.1	0.139	0.108	0.092	0.163
		(0.054)	(0.069)	(0.066)	(0.061)
		17,228	9,095	44,006	10,621
Built pre-regulations (pre-FIRM)	12.0	0.093	0.091	0.064	0.083
		(0.027)	(0.031)	(0.030)	(0.036)
		17,671	10,178	57,037	9,046
Built post-regulations (post-FIRM)	12.5	0.040	0.043	0.035	0.050
		(0.020)	(0.025)	(0.024)	(0.023)
		28,735	13,147	64,723	17,275
Panel B. Log square footage					
Single-family homes	7.9	0.018	0.019	0.024	0.014
		(0.009)	(0.009)	(0.010)	(0.012)
		64,134	49,876	263,945	38,592
Single-family homes built post-regulations	8.1	0.015	0.010	0.023	0.008
(post-FIRM)		(0.012)	(0.014)	(0.013)	(0.014)
		32,176	21,306	104,509	23,285

Notes: Table displays estimates of equation 1. Outside-of-flood-zone means are calculated within 50 feet of the boundary. Column 2 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 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 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 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. Observations are grid cells. Robust standard errors (in parentheses) are clustered at the census tract level. Sample sizes are included below each standard error.

Table A.5: Summary Statistics: Building Standards Event Study Sample

Variable	Inside flood zone Pre-Standards (1)	Inside flood zone Post-Standards (2)	Outside flood zone Pre-Standards (3)	Outside flood zone Post-Standards (4)
Share elevated to minimum level	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 $)	$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: Effects of Building Standards: Heterogeneity by Flood Risk

	Low-risk			High-risk		
	Mean in flood zone, Pre-FIRM	Event study	Difference-in-Differences	Mean in flood zone, Pre-FIRM	Event study	Difference-in-Differences
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Risk Measured by First Street						
Variable						
Damages (insurance payouts per $1000 of coverage)	$0.74	$-0.55	$-0.66	$3.36	$-1.54	$-1.33
		(0.22)	(0.18)		(1.11)	(0.45)
N		54,237	117,302		64,569	111,251
Log house price (sold 2005-2020, 2010 $)	12.09	-0.021	-0.045	12.28	-0.010	-0.005
		(0.022)	(0.017)		(0.017)	(0.016)
N		34,832	205,023		53,902	123,547
Panel B. Risk Measured by Insurance Payouts						
Variable						
Damages (insurance payouts per $1000 of coverage)	$0.10	$-0.05	$-0.03	$7.62	$-4.62	$-3.95
		(0.02)	(0.02)		(1.09)	(0.75)
N		92,237	170,537		89,494	168,763
Log house price (sold 2005-2020, 2010 $)	12.29	-0.019	-0.047	12.21	-0.009	0.009
		(0.013)	(0.013)		(0.017)	(0.001)
N		94,052	224,630		54,869	218,414

Notes: Table presents variable means and coefficient estimates from equations 3 and 12 on insurance payouts and log house price, split by the risk level of the census tract. Insurance payout data come from residential National Flood Insurance Program policies from 2010-2018. Price data come from residential sales prices in 2005-2020. Coefficients with insurance payouts as the outcome are estimated at the policy-year by census-tract by year built level, weighted by the number of policies. Coefficients with log house price as the outcome are estimated at the house level. The difference-in-difference estimates are estimated as the average of the year-specific coefficient estimates of equation 12 from years 0-5, less the average of the year-specific estimates from years -6 to -2. Sample includes all single-family residences in Florida. Standard errors (in parentheses) are clustered at the census tract level. In Panel A, census tracts are categorized by whether the census tract's estimated 100-year-flood depth based on the First Street hydrological model falls below (low-risk) or above (high-risk) the median in our sample. In Panel B, census tracts are categorized by whether census tracts have below (low-risk) or above (high-risk) median levels of insurance payouts, conditional on the census tract having any payout event.

Table A.7: Effects of Building Standards: Interacted with Share Elderly

	Threshold for "elderly":		
Outcome: Log house price	65+	75+	85+
	(1)	(2)	(3)
Post-FIRM	-0.025	-0.027	-0.015
	(0.015)	(0.014)	(0.015)
Post-FIRM x Above Median Share Elderly	0.028	0.030	0.011
	(0.019)	(0.019)	(0.019)
Difference	0.053	0.057	0.026
	(0.032)	(0.031)	(0.032)

Notes: Table presents coefficients estimated from equation 11. Price data come from residential sales prices in 2005-2020. Indicator for whether a census tract exceeds the overall median share elderly uses data from the 2014-2018 American Community Survey. Sample includes all single-family residences in Florida, restricted to the SFHA. Standard errors are clustered at the census tract level.

Table A.8: Summary Statistics: Model Estimation Sample

	Whole Sample		Balanced Boundary Sample	
	Outside Flood Zone	Inside Flood Zone	Outside Flood Zone	Inside Flood Zone
	(1)	(2)	(3)	(4)
Share developed, 1980	0.65	0.49	0.57	0.50
N developed grid-cells, 1980	2,120	886	93	74
Share developed, 2016	0.84	0.68	0.81	0.73
N developed grid-cells, 2016	5,831	1,964	239	178
Share adapted pre-regulation	0.00	0.39	0.00	0.45
Share adapted post-regulation	0.00	1.00	0.00	1.00
Home price, 1980	$46,428	$51,389	$49,757	$49,757
Home price, 2017	$206,984	$300,546	$238,114	$283,941
First Street Flood Risk	0.08	0.34	0.09	0.25
First Street AAL (2021)	0.0005	0.0042	0.0007	0.0020
First Street AAL (2051)	0.0013	0.0083	0.0019	0.0046
First-Street future-adjusted AAL	0.0009	0.0063	0.0013	0.0033
N grid-cells	20,293	11,251	538	503
N observations	1,043	803	255	255

Notes: Table presents summary statistics of the aggregated sample at the tract-zone-boundary proximity level, used for model estimation and counterfactuals. Columns 1 and 2 describe the whole sample. Columns 3 and 4 describe the subset of the sample used for constructing the boundary regression discontinuity moments. 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. First Street Flood Risk is the share of observations with an annual risk of flooding more than 2 feet greater than 1%. First Street AAL is the expected Average Annual Losses (as a share of parcel value), calculated using the First Street Foundation model under the "middle" scenario for future flood risk projections.

Table A.9: Estimated Parameters, Alternative Specifications

	Baseline	0 price effect	0 price effect, 1/2 quantity effect	Supply Elast + 1 SD
	(1)	(2)	(3)	(4)
Supply				
Cost of adaptation (ψ)	0.243	0.242	0.141	0.197
	(0.037)	(0.037)	(0.037)	(0.027)
Demand				
Coefficient on SFHA (ϕ)	-0.367	-0.454	-0.430	-0.374
	(0.097)	(0.095)	(0.095)	(0.096)
Coefficient on log price (α^D)	-1.344	-1.378	-1.405	-1.337
	(0.222)	(0.220)	(0.220)	(0.222)
WTP to avoid SFHA (ϕ/α^D)	0.273	0.330	0.306	0.280

Notes: Table presents parameter estimates of our main parameters under alternate assumptions. Column 1 is the baseline (reported in Table 4). Column 2 imposes no price effect instead of the price increase estimate used in the baseline. Column 3 additionally assesses the sensitivity to cutting our quantity estimate in half. Column 4 calibrates supply parameters using the baseline estimates of census-tract level supply elasticities plus one standard deviation of the estimates in Baum-Snow and Han (2024). Standard errors (in parentheses) generated from the analytic GMM formula.

B Data Appendix

B.1 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 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.[43]

[43] We restricted to the top 15 counties because each county requires substantial effort to evaluate (manually determining the location of each tile in order to assign it to a quadrant).

Step 2. Divide each county into equal-area quadrants. An illustration of the quadrants is shown in Figure A.13.

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.

The final sample included 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.

B.2 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 drop grid cells that fall within 2 miles of county boundaries in the RD analysis to avoid including flood zone boundaries that overlap county boundaries.

C NFIP Enrollment Event Study: Additional Material

C.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 and contents insurance. 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).[44] 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

[44]The Base Flood Elevation measures the height of the flood that has a greater than 1% chance of happening every year. For communities participating in the NFIP, houses built in the floodplain are required to be elevated to at least this level.

lowest floor elevation and BFE is greater than or equal to 0. We assume that if the elevation is missing, the house is not elevated.

C.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 2010 to 2018. 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.

C.3 Testing for Confounding Disamenities

Section 4.2 finds that despite reduced flood risk, flood-safe houses are not more expensive than comparable non-adapted houses. One possible explanation for this result is that adaptation, specifically elevating to a minimum level, may introduce stairs, which could be a disamenity for homeowners. To test this, we note that elderly homeowners are most likely to find stairs a disamenity. We therefore split the analysis into tracts with above- and below-median share of elderly adults.[45] We estimate:

$$\begin{aligned} y_{jbt} = {} & \beta_1 Post_{jb} + \beta_2 Post_{jb} \times Elderly_j + \nu_1 r_{jb} + \nu_2 r_{jb} \times Elderly_j \\ & + \eta_1 r_{jb} Post_{jb} + \eta_2 r_{jb} Post_{jb} \times Elderly_j + \gamma_j + \varepsilon_{jbt} \end{aligned} \tag{11}$$

which replicates equation 3, but introduces interaction terms between all relative construction year terms and $Elderly_j$ (an indicator for above-median-elderly-share in each census tract).

Results are displayed in Table A.7. If anything, the effect of building code regulation on house prices is *higher* for more-elderly tracts, rather than lower, but the differences across age groups are not statistically-significant. We view this as suggestive that a dislike for stairs is not confounding our interpretation of our baseline event study estimates.

[45]Using data from the 2014-2018 ACS, we compute the share of population in each census tract that is above 65, 75, or 85. We then define a tract as above-median for each age cutoff if it exceeds the median census tract share above that cutoff across the state.

C.4 Difference-in-Difference Specification

We expand on our event-study strategy in Section 4.2 using the fact that building standards were imposed for houses built inside the flood zone but not for houses built outside of it.[46] Building on equation 2, we estimate the following specification:

$$m_{jzbt} = \Sigma_r \beta_r \mathbb{1}\{r = b - e_j\} \times SFHA_z + \Sigma_r \gamma_r \mathbb{1}\{r = b - e_j\} + \lambda_b + \gamma_{jz} + \varepsilon_{jzbt} \tag{12}$$
$$p_i = \Sigma_r \beta_r \mathbb{1}\{r = b_i - e_i\} \times SFHA_{z(i)} + \Sigma_r \gamma_r \mathbb{1}\{r = b_i - e_i\} + \lambda_{b(i)} + \gamma_{j(i)z(i)} + \varepsilon_i$$

where now z indicates flood-zone, γ_{jz} are tract-by-flood-zone fixed effects and $\lambda_{b(i)}$ indicates year-of-construction fixed effects. We cluster standard errors at the census tract level. For outcomes related to insurance claims and policies, we weight each location by the number of policies. Figure A.8 presents results. Results are qualitatively and quantitatively similar to the analysis in Section 4.2 of the main text.

D Model Estimation Details

D.1 Calibration of Supply Elasticities

We use as our starting point estimates produced by Baum-Snow and Han (2024) (BSH) for land development elasticities at the census tract level, estimated between 2001 and 2011. The supply elasticities BSH estimate are derived from the following relationship

$$\alpha_j^S = \beta_0 + \beta_1 ShareDev_j + \beta_2 DistCBD_j + \beta_3 Flat\%_j \tag{13}$$

which relates the supply elasticity to the share of the census tract that is developed, the distance from the census tract to the center business district (CBD), and the share of the census tract that is not steep-sloped. The share of land in the census tract that is developed ($ShareDev_j$) is measured in 2001.

The fact that elasticities are estimated from 2001-2010 means that these supply elasticities will likely underestimate supply elasticities in our sample, and differentially so for census tracts that became more developed between 1980 and 2001 as more developed areas have fewer attractive plots on which to build.

[46] We use the never-treated units outside of the flood zone to avoid bias in two-way fixed effects estimators, following Sun and Abraham (2021).

We therefore adjust our estimates of α_j^S to accord with 1980s development shares as follows:

1. Estimate the relationship in equation 13 with 2004 development shares, as well as the same $DistCBD_j$ and $Flat\%_j$ used in the original BSH estimates.

2. Predict $\hat{\alpha}_j^S$ using the estimated coefficients from step 1 and our measures of 1980s development shares.

3. Replace any negative elasticities with the smallest nonnegative elasticity in our eleven-county sample.

We use the approach above instead of directly using BSH's estimates of β_1, β_2, and β_3 because we measure development slightly differently than BSH.

We then translate our adjusted measures of α_j^S into an implied tract-level μ_j and σ_j for each census tract. Again, to be consistent with the BSH estimates, we estimate μ_j and σ_j treating census tracts as uniform, i.e. not differentiating across flood zones, as this is the level at which the supply elasticities are estimated. We first compute the decrease in share developed in each census tract that would be implied by the BSH elasticities for a price decrease of 10%: $\tilde{q}_j = q_j^{2016} - \alpha_j^S(0.1p_j^{2016})$, and use:

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

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

to obtain census-tract level estimates of μ_j and σ_j (note that because we are matching μ_j and σ_j to the BHS estimates at the census tract level, we ignore η_{jz} and E_{jz}).

Then, we build on this matching exercise by allowing the supply of housing to differ by (1) adaptation status (ψE_{jz}), detailed in the following section, and (2) by a shifter η_{jz}, which we obtain, given estimates of ϕ, as $\eta_{jz} = p_{jz}^{2016} - \psi E_{jz} - \mu_j - \Phi^{-1}(q_{jz}^{2016})\sigma_j$.

D.2 Moment Condition Details

Moments Based on the Exogeneity of Building and Land Characteristics The moments based on the exogeneity of building and land characteristics are as follows:[47]

[47] We include a constant term in X_{jz}.

$$\mathbb{E}\left[\xi_{jz}X_{jz}\right]=0 \qquad \mathbb{E}\left[\xi_{jz}\tilde{X}_{jz}\right]=0 \qquad \mathbb{E}\left[\xi_{jz}\tilde{p}_{jz}\right]=0 \tag{16}$$

for a vector $\tilde{X}_{jz}$ that averages the observable characteristics X_{jz} of locations in the same housing market that are located more than 3 miles away from geography jz, weighted by land area, and a price vector $\tilde{p}_{jz}$ that rationalizes market shares under no unobserved amenities (i.e., setting $\xi_{jz}=0$).

Boundary Moments: Demand We construct moments to match our spatial regression discontinuity (RD) analysis around the regulatory boundaries. To operationalize this, we first subdivide the tract-zone pairs into tract-zone-band observations, where band $b \in \{close, far\}$ indicates whether an observation is within K feet from a floodplain boundary. Ideally, we would like to construct moments $E[\xi_{jzb}SFHA_z \mathbb{1}\{b=close\}]=0$, taking $K\to 0$. This captures the RD assumption that as the boundary is approached, amenities become uncorrelated with flood zone status. However, taking $K \to 0$ has practical challenges, namely a lack of sufficient observations.

We address this challenge by constructing moments that match our RD estimates in Section 4.1 directly. First, we define $K=100$ feet. Then, in order to capture the fact that within a 100-foot boundary, mean amenities may still differ inside and outside the flood zone, we add a boundary-SFHA fixed effect, $\Delta^D SFHA_{zb}$. We redefine unobserved amenities as $\tilde{\xi}_{jzb}=\delta_{jzb}-\left(\alpha^D p_{jzb}+\phi SFHA_z+X_{jzb}\beta+\Delta^D SFHA_{zb}\right)$, where $\phi SFHA_z$ is the causal effect of regulation on choices and $\Delta^D SFHA_{zb}$ is the average difference in amenities between SFHA and non-SFHA locations within 100 feet of the flood zone boundary. Then, we define the following moments to exactly match the RD estimates:

$$\mathbb{E}\left[\left(\frac{1}{\alpha^D}\left(\delta_{j1b}-\delta_{j0b}-\phi-\Delta^D\right)-\beta^{p,2016}\right)\mathbb{1}\left\{b=close\right\}\right]=0 \tag{17}$$

$$\mathbb{E}\left[\tilde{\xi}_{jzb}SFHA_z\mathbb{1}\left\{b=close\right\}\right]=0 \tag{18}$$

$$\mathbb{E}\left[\tilde{\xi}_{jzb}\mathbb{1}\left\{b=close\right\}\right]=0 \tag{19}$$

where $\beta^{p,2016}$ is the RD effect on price. These moments are only calculated for observations close to the flood zone boundary where there is a balanced pair (i.e. an observation both inside and outside the SFHA in the same tract).[48]

Boundary Moments: Supply We follow a similar approach for supply-side moments, constructing moments that match our RD estimates in Section 4.1 directly. In order to capture the fact that within a 100-foot boundary, mean construction costs may still differ in the SFHA, versus not, we add a boundary-SFHA fixed effect, $\Delta^{S,t} SFHA_{zb}$. We redefine unobserved construction costs as $\tilde{\eta}^t_{jzb} = p^t_{j1b} - \psi E^t_{j1} - \mu_j - \Delta^{S,t} SFHA_{zb} - \mu^t_b$, where ψE^t_{j1} is the causal effect of regulation on construction costs, $\Delta^{S,t} SFHA_{zb}$ is the average difference in construction costs between SFHA and non-SFHA locations within 100 feet of the flood zone boundary, and μ^t_b is a mean shifter in the boundary sample. Then, we define the following moments to exactly match the RD estimates:

$$\mathbb{E}\left[\Phi\left(\frac{p^{1980}_{j1} - \psi E^{1980}_{j1} - \mu_j - \tilde{\eta}^{1980}_{j1} - \mu^{1980}_b}{\sigma_j}\right) - \right. \\ \left. \Phi\left(\frac{p^{1980}_{j0} - \psi E^{1980}_{j0} - \mu_j - \tilde{\eta}^{1980}_{j0} - \mu^{1980}_b}{\sigma_j}\right) - \beta^{q,1980}\right] = 0 \tag{20}$$

$$\mathbb{E}\left[\Phi\left(\frac{p^{2016}_{j1} - \psi E^{2016}_{j1} - \mu_j - \tilde{\eta}^{2016}_{j1} - \mu^{2016}_b}{\sigma_j}\right) - \right. \\ \left. \Phi\left(\frac{p^{2016}_{j0} - \psi E^{2016}_{j0} - \mu_j - \tilde{\eta}^{2016}_{j0} - \mu^{2016}_b}{\sigma_j}\right) - \beta^{q,2016}\right] = 0 \tag{21}$$

$$\mathbb{E}\left[\tilde{\eta}^{1980}_{jzb} SFHA_z \mathbb{1}\left\{b = close\right\}\right] = 0 \tag{22}$$

$$\mathbb{E}\left[\tilde{\eta}^{2016}_{jzb} SFHA_z \mathbb{1}\left\{b = close\right\}\right] = 0 \tag{23}$$

$$\mathbb{E}\left[\tilde{\eta}^{1980}_{jzb} \mathbb{1}\left\{b = close\right\}\right] = 0 \tag{24}$$

$$\mathbb{E}\left[\tilde{\eta}^{2016}_{jzb} \mathbb{1}\left\{b = close\right\} = 0\right] \tag{25}$$

[48] We also include a boundary sample fixed effect in X_{jzb}.

for estimated RD effects on the share of land that is developed in the pre-period, $\beta^{q,1980}$, and the post-period, $\beta^{q,2016}$. All moments are calculated for observations close to the flood zone boundary where there is a balanced pair (i.e. an observation both inside and outside the SFHA in the same tract).

Estimation and Data Details Using the moments specified above, we obtain parameter estimates with two-step optimal GMM and calculate standard errors analytically.

We measure p_{jz}^{2016} as the log of the median sales price for single-family homes from 2014-2019 based on the location of the building footprint. We measure p_{jz}^{1980} as the log of the median value of owner-occupied non-condominium housing units from the 1980 Census. House prices from 1980 are not available at the flood zone level. Because flood zones did not exist in the pre-period we assume that the price does not differ between flood zones within a Census tract. We measure quantity of developed land in 1980 and 2016 as the number of grid-cells that are categorized as developed in the 1980 and 2016 land-use datasets.[49] We base our adaptation indicator on a measure of elevation from NFIP policy data, as discussed in Appendix C.1. We define a tract as adapted 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 adaptation 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 market shares, but we restrict to historic maps for the boundary moments.

Appendix Table A.8 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 are similar.

Model Fit The structural error terms $\left(\xi_{jz}, \eta_{jz}^t\right)$ allow our model to achieve a perfect fit to the observed data. However, given that we calibrate μ_j and σ_j in our model of housing supply from external estimates, we would like to assess fit in our context. To do so, we investigate the extent to which we rely on these structural error terms for model fit. We conduct this exercise in Figure A.12, where we plot observed price in 2016 against the modeled prices that would rationalize observed market shares in each year, *if we omitted the structural error terms.* This exercise isolates the fit of our model of housing supply, as we calculate

[49] 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.

model-implied prices using our estimated supply parameters. Figure A.12 demonstrates a strong correlation between model-generated and observed prices, indicating our supply curve is reasonable. Of course, the inclusion of the structural error terms mechanically improves the model fit.

E Expected Damages Calculations

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).[50] 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 grid-cells 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.[51] The term M_{jz}^{CF} is a multiplier that accounts for differences in adaptation in each counterfactual. We assume that if an observation was adapted in the pre-period it will be adapted in the post-period for all counterfactuals. Otherwise, houses that are not observed to be adapted in the post-period but are adapted in a counterfactual will experience 55% lower damages in that counterfactual. This is based on our estimates of the average effect of building code adoption on damages in our event study analysis in Table 3 and our heterogeneity analysis in Appendix Table A.6 that indicates that damage reductions are proportional to baseline risk. A similar calculation applies to houses that are observed to be adapted but are counterfactually non-adapted.

We compute two measures of damages:

[50] See First Street Foundation (2021) for more details.

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

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

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

where N^{CF} denotes the number of newly-developed grid-cells under counterfactual CF , N^{UR} denotes the number of newly-developed grid-cells under the unregulated benchmark, 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 ("adaptation-based damages") holds the number of houses in each location constant at the unregulated benchmark 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).

F 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:

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

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 unregulated benchmark equivalent to that of the same market in a counterfactual CF. That is, we solve for ΔP_m^{CF} such that

$$\begin{aligned} &\ln\left(\Sigma_{j \in J_m,\ z \in \{0,1\}} \exp(\alpha^D \ln\left(\exp(p_{jz}^{NoSFHA}) - \Delta P_m^{CF}\right) + X_{jz}\beta + \xi_{jz})\right) = \\ &\quad \ln\left(\Sigma_{j,z} \exp(\alpha^D \ln(\exp\left(p_{jz}^{CF}\right) + \tau_{jz}^{CF}) + \phi SFHA_{jz}^{CF} + X_{jz}\beta + \xi_{jz})\right) \end{aligned} \tag{27}$$

where p_{jz}^{NoSFHA} is the log house price in in the unregulated benchmark, p_{jz}^{CF} is the log house price in the counterfactual of interest, τ_{jz}^{CF} is the tax in levels, and $SFHA_{jz}^{CF}$ indicates the SFHA designation in the counterfactual of interest. The total consumer surplus associated with new development is then $\Delta CS^{CF} = \Sigma_m N_m \Delta P_m^{CF}$, where N_m denotes the number of new houses in each county m.

We compute producer surplus differences using equivalent variation (relative to the unregulated benchmark), analogous to the consumer surplus calculation. We solve for a market-wide

ΔP_m^{CF} such that

$$\Sigma_{jz\in M} L_{jz} \int_{q_{jz}^0}^{q_{jz}^{1,NoSFHA}} \left(\ln\left(\exp(p_{jz}^{NoSFHA}) + \Delta P_m^{CF}\right) - \left(\sigma * \Phi^{-1}(\tilde{q}) + \psi E_{jz}^0 + \eta_{jz}^1\right)\right) d\tilde{q}$$

$$= \Sigma_{jz\in M} L_{jz} \left(\int_{q_{jz}^0}^{q_{jz}^{1,CF}} \left(p_{jz}^{CF} - (\sigma * \Phi^{-1}(\tilde{q}) + \psi E_{jz}^{CF} + \eta_{jz}^1)\right) d\tilde{q} \right) \tag{28}$$

where p_{jz}^{CF} is the log house price in the counterfactual of interest, E_{jz}^{CF} indicates whether the house is adapted under the counterfactual, E_{jz}^0 indicates whether the house is adapted in the absence of regulation, p_{jz}^{NoSFHA} is the house price in logs in the unregulated benchmark, q_{jz}^0 is the share of plots developed as of the end of the pre-period in location jz, and $q_{jz}^{1,CF}$ is the share of plots developed as of the end of the post-period in location jz in counterfactual CF. Analogous to our consumer surplus calculation, we computer producer surplus for new development, holding 1980 development fixed, as there is no aggregate social welfare impact of the policy on existing development.

We compute government revenue from each policy by comparing consumer surplus under each policy to the consumer surplus with insurance premiums and taxes set to 0. We multiply the difference in consumer surplus by number of newly-developed houses to yield total revenue for each policy.