The Price of Risk: Flood Insurance Premium Reform and Local Development

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Abstract

The climate is changing rapidly. Globally, trillions of dollars of housing wealth is at risk of flood damage, putting billions of people in harm's way. In many countries, federally administered flood insurance programs are the primary tool that policy-makers employ to influence coastal development and mitigate flood damages. I study the design of federal flood insurance programs in the United States, using data on the universe of tax parcels in Florida and North Carolina. For identification, I leverage a 2021 reform to the National Flood Insurance Program's rate calculation methodology that more tightly linked prices to actual underlying flood risk. I find that increasing flood insurance premiums causes declines in new development. My findings suggest the existence of substantial moral hazard prior to the reform, and emphasizes the importance of accurately pricing risk for avoiding the worst economic - and human - costs of climate change.

1 Introduction

Over the past two decades, nearly 2.1 million acres of flood-prone land was developed, including 844,000 residential properties (Agopian et al., 2024). Concurrently, there has been a dramatic rise in the number of billion-dollar natural disaster events, with estimates of average annual flood damages in the US to be $32.1 billion (Wing et al., 2022). Flood risk is projected to keep increasing due to climate change, causing more frequent and more intense storms. Continued development in flood-prone areas will place more people and more infrastructure in harm's way.

The National Flood Insurance Program (NFIP) offers a solution to counter the increasing threat of flooding in the United States. Founded in 1968 with the goals of reducing future flood damage and protecting property owners with affordable insurance, the NFIP has a responsibility to accurately price flood risk to discourage development in especially risky areas. As the dominant (public) firm in the flood insurance market, NFIP boasts about 5 million policy holders nationwide, providing nearly $1.3 trillion in coverage amounts (National Flood Insurance Program, 2024). However, the NFIP has been criticized for much of its existence due to its offering of excessive subsidies, out-dated flood risk methodology and estimates, and coarse risk-price menu. For that reason, there have been many concerns that the NFIP's operations have contributed to increased development in high-risk flood-prone areas.

In this paper, I study a significant reform in the NFIP's price-setting methodology that allows me to investigate the effect of flood risk price changes on local development decisions, holding regulatory boundaries fixed. The new premiums utilize property-specific characteristics to determine flood risk as opposed to the prior system that used coarse flood zone boundaries. This results in a more actuarially fair price and distribution of flood risk. The new reform, titled *Risk Rating 2.0* (RR2), went into effect in October 2021 for all new underwriting business. The previous pricing system, which I will call *Risk Rating 1.0* (RR1) for convenience, utilized Flood Insurance Rate Maps (FIRMs) in order to provide flood risk information and set premiums. These maps are provided at the NFIP community level and delineate the boundaries of the 100-year floodplain, also known as the Special Flood Hazard Area (SFHA), where the annual probability of flood event exceeds 1%.[1] Outside of the SFHA lies the 500-year floodplain, where the annual probability of flooding is between 0.2% and 1%, and all other areas are classified as minimal risk. Under current law, homeowners are required to purchase a flood insurance policy if their property lies in a SFHA and they have a federally-backed mortgage.

Under RR1, variation in premiums was mainly driven by flood zone status (in SFHA or not), housing characteristics (basement type, floor number, building type), and policy attributes (coverage amounts and deductibles). Additionally, elevation is considered for policyholders inside the SFHA. However, conditional on these characteristics, there was no variation in premiums across geography and very little variation within flood zone. For example, a single family home with no basement and $200,000 of coverage would pay the exact same premium in Rhode Island and Florida, despite flood risk being drastically higher in Florida. Further, within a flood zone, that same single-family home would pay the same premium regardless if it was 10 feet from the coastline or 100 feet from the coastline. RR2 introduced significant amounts of variation in premium values both within and across flood zones, reflecting updated methodologies and the addition of many property-specific characteristics, including new elevation and hydrological measures related to flood risk.

I exploit this newly introduced variation in premiums to study the effect on local development decisions within Florida and North Carolina, two states with significant flood risks from hurricanes and tropical storms, but with greatly different exposure intensities. Florida has the most policies in force in the NFIP with 1.7 million policies (17.4% of state-wide housing units), while North Carolina only has about 130,000 (2.8% of state-wide housing units) policies.[2] Further, of the residential floodplain development that has occurred over the last two decades, Florida accounted for nearly half, building 398,000 new residential units from 2001 to 2019, which accounts for 21% of total new housing built over that period. Conversely, while North Carolina has some of the most developable land in its floodplain compared to other states (nearly 16%), it only has 3.7% of its new housing built in the floodplain (Agopian et al., 2024). Thus, these two states should provide a sufficiently representative setting to study the effect of RR2 and test its external validity.

In order to study the effect of RR2, I construct a novel dataset using numerous sources of granular geo-spatial data. Using the universe of tax parcels in both Florida and North Carolina, administrative data from the NFIP, and hyrdrological and topographical data from the United States Geological Survey (USGS), I construct hypothetical premiums faced by each tax parcel under RR1 and RR2, based on available flood insurance and technical manuals. This gives me a continuous measure of premium changes for every parcel in both states. Due to the sparse nature of new development as a measurable variable

[1] NFIP communities can be either a town, city or county, but must have authority to adopt and enforce floodplain ordinances for the area under its jurisdiction.

[2] Based on measures from the 2020 decennial census and rolling policies-in-force (PIF) from the NFIP as of August 2024. Florida has 1,718,625 PIF and 9,865,350 housing units, while North Carolina has 129,878 PIF and 4,708,710 housing units.

over my sample period, I aggregate my data from the parcel level to the block group-floodplain level.[3] In order to causally identify an effect of premium changes on housing development, I utilize a conditional difference-in-differences (DID) design with a two-way fixed effect (TWFE) specification. Identification relies on the standard assumption that treated observations within the conditional fixed effect groupings would follow a parallel trend to control observations in the absence of the implementation of RR2. I validate this assumption with visual tests in the form of event studies and statistical tests of joint coefficient nullity. Additionally, I utilize newer methods from Callaway et al. (2024a) that allow me to exploit a continuous treatment in a DID framework.

This strategy will allow me to address two important questions related to the price reform: 1.) Did RR2 change new development on average? and 2.) Which premium change levels have larger causal effects? Question 1 can provide necessary evidence on moral hazard as a result of the NFIP's outdated policies and subsidized rates. Significant decreases in development in response to the reform would suggest the prior pricing scheme encouraged riskier location choices. Question 2 can provide crucial information related to climate adaptation policy. If premium increases are too small, or negative for a given level of true risk, development may respond positively to effects of the reform, negating the overall risk reductions. Conversely, if premium increases are too large, development might be too constricted, which could produce unnecessary economic losses. Thus, the causal estimates of development's responsiveness to premium change levels would be needed to improve the pricing menu, one of the NFIP's key policy levers for reducing future flood risk.

To start my analysis, I employ a binary version of my continuous premium change measure that defines treated observations as units that experienced an "economically meaningful" change in their potential flood insurance premium. This setup ignores any potential treatment intensity or heterogeneity, and includes both positive and negative premium changes. I find that the introduction of RR2 led to a 27% decrease in the share of new development in a block group-floodplain in North Carolina and a 10% decrease in Florida, relative to the control mean. These large and economically significant estimates suggest the NFIP's prior policy may have been encouraging moral hazard. Next, in order to examine casual response effects among the various levels of the premium change, I increasingly discretize my treatment until I reach the continuous premium change measure. Using separate treatment indicators for increases and decreases, I find that increased premiums in North Carolina led to a 36% decrease in the share of new development, while in Florida, increased premiums led to a 14% decrease. Decreased premiums in North Carolina had no effect on development. Conversely, decreased premiums in Florida led to 13% increase in development, an effect of nearly the same magnitude as the premium increases, which highlights the need for more flexible heterogeneity analysis using a continuous measure.

The continuous measures of the TWFE specification imply negative effects of the reform on development overall. However, recent work by Callaway et al. (2024a) shows that there are undesirable consequences (e.g. selection bias and weighting problems) of using a TWFE estimator with continuous treatment, confounding interpretation of any parameters of interest. While I still report results from the continuous TWFE specification, I follow Callaway et al. (2024a) and produce estimates for the average treatment effect on the treated for the entire distribution of premium changes. I find that estimates are negative for positive premium change levels, and zero or slightly positive estimates for negative changes, implying that only significant increases in premiums will have an effect on development decisions. The

[3] My sample period is 2016-2023.

asymmetric response across the premium distribution has significant implications for policy prescriptions and understanding of this market.

The rest of the paper proceeds as follows. Section 2 outlines the existing literature and my contributions to each strand. Section 3 covers the relevant institutional background of the National Flood Insurance Program and the way they calculate premiums. Section 4 describes my setting and data sources. Section 5 introduces my empirical strategy and my econometric framework. Section 6 presents my main empirical results, Section 7 discusses heterogeneity of my results by covariates and premium levels, and Section 8 shows they are robust to a number of alternative assumptions and specifications. Section 9 illustrated back-of-the-envelope estimates of my main results. Section 10 concludes.

2 Related Literature

I contribute to a very robust literature in economics, urban planning and insurance that has studied the National Flood Insurance Program extensively. First, I extend the literature on moral hazard (Conte and Kelly, 2018; Hudson et al., 2017; Peralta and Scott, 2024) and adverse selection (Bradt et al., 2021; Wagner, 2022) in the NFIP, focusing on a different type of moral hazard than what has been studied previously. Moral hazard in the current literature addresses risky decision making after a household acquires flood insurance, particularly investigating the extent of their disaster-mitigation activities once they are insured (Hudson et al., 2017) or the evolution of population sizes in communities (Peralta and Scott, 2024). Instead, I provide evidence about moral hazard related to development decisions, showing that distorted flood insurance prices have led to riskier development that would not have otherwise occurred if the risk was correctly priced. Notably, this is different from prior research, as I can isolate the effects of flood insurance price changes alone, as access to insurance was the same throughout my sample.

Additionally, my paper is related to Ortega and Petkov (2024) and Mulder and Kousky (2023), who study the effect of Risk Rating 2.0 on entry and exit of policyholders in the NFIP and the impact of the reforms on premiums broadly. Ortega and Petkov (2024) find that RR2 increased exit and reduced entry in both the floodplain and surrounding areas. My findings complement this analysis by providing causal estimates of development decisions in response to the price reform, of which the significant decreases likely contributed to decreases in entry of the program, as less development would lead to less potential NFIP customers. Mulder and Kousky (2023) find that Risk Rating 2.0 led to higher premiums for more than two-thirds of policyholders, using data on renewing policies from the NFIP. I am able to extend their findings by calculating hypothetical premium changes for *all* areas using the technical documents released by the consulting firm hired by FEMA to construct the new rating methodology. This data-work allows my analysis to consider effects on agents outside of the NFIP, which the prior studies are unable to do with their current measures.

Next, I contribute to a literature that documents effects of NFIP regulations and program participation on housing prices and development (Ostriker and Russo, 2024; Georgic and Klaiber, 2022; Indaco et al., 2019; Browne et al., 2018; Hino et al., 2023; Agopian et al., 2024; Shi et al., 2023; Noonan and Liu, 2019, 2022; Noonan and Sadiq, 2019). Many of these papers exploit community entrance into the NFIP as well as regulatory floodzone boundaries to identify causal effects on housing prices and development, in both the short and long-run. In my setting however, I abstract from any differences in regulation between the floodplain and non-floodplain as Risk Rating 2.0 does not change any regulations, it purely changes the price of flood insurance.[4] This unique feature of the policy allows me to isolate the effect of flood insurance prices on their own, and examine potential agent responses who are not already in the NFIP, a context that is rare for the literature as prior price reforms mainly affected existing policy-holders. This allows me to provide novel estimates on the effect of flood insurance prices on development decisions which build on the results from Ostriker and Russo (2024) and Browne et al. (2018), who provide evidence that NFIP program participation affected development in the long-run and short-run, respectively. My results provide evidence that in addition to the regulatory effects, the price of flood insurance can have important impacts on development decisions in the short-run, expanding the scope for how policy-makers think about climate adaptation.

Finally, I also contribute to a rich literature on flood risk information frictions (Bakkensen and Barrage, 2022; Gallagher, 2014; Mulder, 2024; Kousky, 2010; Weill, 2023; Petrolia et al., 2013; Fairweather et al., 2023; Chivers and Flores, 2002). Much of this literature has found that agents greatly underestimate their true flood risk and are not able to learn effectively from new information on flooding. My results extend much of the findings from these papers by showing that the price of flood insurance can be a very effective and strong signal of underlying flood risk, even in areas where there is no mandatory purchase requirement. These results are in line with how FEMA itself views the benefits of Risk Rating 2.0, as they argue the new prices lead to "better risk communication" (Horn, 2022).

3 Institutional Background

3.a National Flood Insurance Program (NFIP)

The NFIP was founded in 1968 with two primary goals: 1.) To constrict development in land exposed to flood hazards and guide development away from such locations, and 2.) to provide insurance for at-risk property owners. At the beginning of the 20th century, flood insurance was offered by private insurers, but the losses, damage and displacement incurred by the 1927 Mississippi River floods permanently altered that market, and led most insurers to stop providing flood policies. With no active flood insurance market left, post-flood financial relief only came through federal disaster funds. From this period up until the passage of the National Flood Insurance Act of 1968, the federal government provided funds for victims of flooding (National Research Council, 2015). The NFIP served as the government's solution to decrease taxpayer burden by substituting insurance payouts for federal aid, thus shifting the risk to homeowners who decide to locate in areas with high flood risk.

Communities voluntarily participate in the NFIP in order to unlock flood insurance access for their residents. Acceptance is tied to each community's adoption and enforcement of floodplain management standards, which ensure the NFIP's policy goal of limiting development in the floodplain. In order to determine a community's flood risk, FEMA is responsible for completing Flood Insurance Studies (FISs), in coordination with communities, in order to locate flood sources, areas of risk, and designations of flood zones (Horn and Webel, 2024). These FISs are then used to create flood maps known as Flood Insurance Rate Maps (FIRMs), which outline the official boundaries of a community's flood zones. Under RR1, FIRMs were used to determine premiums and whether a property falls within a SFHA. Under RR2,

[4] Additionally, I control explicitly for differences in the two areas within my model specification.

FIRMs are only used for property location purposes to determine mandatory purchase requirements.[5]

At the beginning of the NFIP's inception, many structures had been built in the floodplain with little knowledge or care of flood risk, due to both a lack of information from the federal government and lack of zoning or other land use restrictions by local governments. As a result, these properties would face exorbitantly high risk-based premiums under the NFIP, which led to Congress establishing two rating systems for premium setting (National Research Council, 2015). Structures built in the floodplain after a FIRM had been issued would pay risk-based rates, and structures built in the floodplain prior to a FIRM would pay a subsidized rate, commonly referred to as a "pre-FIRM" rate. The expectation was that these properties were high risk, and would eventually be lost to flooding or storms, and thus this would be a temporary subsidy. That expectation proved to be quite wrong. As of October 2023, approximately 15% of NFIP policies still received a pre-FIRM subsidy according to FEMA (Horn and Webel, 2024).[6]

3.b Risk-Rating 1.0 (Pre-October 2021)

Prior to implementation of *Risk Rating 2.0*, premiums were determined based on several basic characteristics that classified properties according to flood risk: Flood zone, occupancy type[7], and elevation of the structure relative to the Base Flood Elevation (BFE), which is equivalent to the level of the 1%-annual chance flood.[8] FEMA would then calculate average expected losses for groups of structures that are similar in flood risk and structural features, and assign the same rate to all policies in that group (Horn, 2022). These groups were constructed across many different geographies and topographies, resulting in coarse premium assignment as mentioned above. Two different properties with the same calculated measures of risk (i.e. single family homes, with a basement, 3 feet above the BFE) would receive the same rate despite being located in two different states, with different topography and hydrology. Further, two identical properties in the same flood zone would be charged the same rate regardless of their location within the zone.

Under RR1, FEMA only modeled the potential for coastal storm surge and fluvial (river) flooding risk. Models of physical processes that focus on flooding sources are combined with weather models, structure vulnerability and flood protection measures (e.g. levees) to compute flood depths, velocities and their probability of occurring. These predictions are then used to determine the expected economic loss due to flooding and the probability of that loss. The final insurance rate is then determined from this loss after adjusting for expenses, deductibles and under-insurance rates (Horn, 2022). These rates are then made available to insurance agents in table-format as shown in Appendix Figure A.1.

Despite all of these price distortions, under RR1, FEMA considered these to be "full-risk" rates that were reflective of the true underlying flood risk that faced the policyholder. However, as noted prior, the NFIP has also been subsidizing rates for large portions of its customer base since its inception (i.e. pre-FIRM subsidies) and offers even more subsidies applying to different groups and contexts. The NFIP previously offered subsidies for properties that experienced changes in their flood zone status or their flood zone requirements. "Grandfather" subsidies applied to properties that were mapped

[5] An example of a FIRM (using Brown University as the central location) can be found in Appendix Figure A.2

[6] Under current law, mainly the Homeowner Flood Insurance Affordability Act of 2014 (HFIAA) and Risk-Rating 2.0, these subsidies are being gradually phased out, subject to annual increase caps. See Table 4 in Horn and Webel (2024) for more information.

[7] Occupancy types include single family, 2-4 family, other residential, non-residential business or other non-residential. Most of these categories carry over into RR2, with some extensions and modifications.

[8] BFE is only used for full-risk (i.e. non-Pre-FIRM) rates in the SFHA.

between different flood zone types (e.g. Zone A vs. Zone V), allowing policyholders to maintain the lower rate based on the prior zone.[9] "Newly Mapped" subsidies applied to policyholders that were mapped into an SFHA after a FIRM update.[10] Finally, the Community Rating System (CRS), which is still in place, allows communities to accrue additional discounts for their residents by undertaking additional floodplain mitigation measures above what is required by normal program participation. These discounts can range from 5-45% depending on the level of measures enacted. Thus, on top of existing price distortions underlying the "full-risk" premiums, additional distortions are compounded with the existence of subsidies.

3.c Risk-Rating 2.0 (October 2021 - Present Day)

Risk Rating 2.0 corrects many of the price distortions that existed in RR1 and gradually eliminates subsidies for existing policyholders. These reforms bring the "full-risk" premiums offered by the NFIP closer to actuarily fair measures of true flood risk. Under the new pricing system, rates will now be tied to the geographic and structural features of each individual property as opposed to the broader groups defined under RR1.

RR2 incorporates a much broader range of flood sources and frequencies into its risk modeling process, including flooding due to rainfall (i.e. pluvial flooding), tsunamis, Great Lakes, coastal erosion and flooding in leveed areas. To account for these new sources, RR2 uses three commercial catastrophe models to estimate future loss potential, a major improvement over RR1 which relied on a single model with only two flood sources. In order to tie premium calculations to new flood sources, many more geographic variables needed for each structure when producing a premium for a policy. These include distance to water sources (e.g. rivers, lakes, or coast), the drainage area of the nearest flow-line source (usually a river), elevation of the structure relative to the flooding source, elevation of the structure compared to its relative area, and whether the structure is on a barrier island or behind a levee (Milliman, 2022). Individual structure characteristics considered now include the foundation type, the height of the lowest floor relative to the BFE, number of floors, presence of machinery and equipment on the ground floor, and the building replacement cost value.

Under RR1, rates were based on the amount of insurance purchased for a structure, rather than the replacement cost value. This resulted in major price distortions fueled by inequity in housing values. For example, if a million-dollar single-family home was built next to a $100,000 single-family home, and both purchased $50,000 in coverage (and were at the same elevation), they would pay the same amount for their annual premiums, despite the fact that if they both experienced the same flood, the damages of the million dollar home would result in much higher claims. RR2 corrects this inequity with the addition of replacement cost values into the rate calculation methodology.

FEMA argues that the implementation of RR2 is a more transparent and accurate measurement of flood risk pricing that will lead to "better risk communication and an increase in flood insurance take-up rates" (Horn, 2022). Critics of RR1 would likely agree that RR2 provides more accurate risk measurements, based on all of the changes I have outlined above. Transitioning from broad risk groups to granular individual ratings is sure to provide more accurate pricing. However, transparency of the

[9] Flood zone types can be generally defined by three categories: A Zones are SFHA areas with flooding risk from riverine and lake sources. V Zones are SFHA areas with flooding risk from coastal storm surges and other wave-related flooding sources. X Zones are non-SFHA areas with minimal flooding risk.

[10] These were available only for policies underwritten on or after April 1, 2015.

rate calculation process seems to have gotten worse under RR2. Since premiums are based on individual property characteristics, flood zones and rate tables are no longer used to determine premium amounts. Flood zones are still used to determine which properties are subject to the mandatory purchase requirement however. Under RR1, insurance agents relied on FIRMs and rate tables to manually calculate premiums and show customers visually where the dollar amounts are originating. Now, under RR2, insurance agents rely on a "black box" algorithm that returns a premium calculation based on inputs (mainly an address), making it much harder to explain what is driving premium costs.[11] While information on the premium calculations exist and is publicly available, it requires a rather esoteric knowledge to interpret.

4 Setting and Data

I focus on North Carolina and Florida for my empirical context, as both states experience significant flood risks but at different intensities, as mentioned above. Both states have ample experience with hurricanes and flooding, and large proportions of their area covered by floodplains (although at much different rates). Thus, they should be excellent candidates to test whether flood insurance price reform has an effect on development decisions. I combine two rich sources of granular data to construct my sample and run my analysis.

4.a Tax Parcels

My analysis relies primarily on administrative tax parcel data from North Carolina and Florida. Both datasets are obtained through state-run sources and provide the geo-located boundaries of each tax parcel in the state. For each parcel, the year of construction for the primary structure is reported, which I will use as my main measurement of development, which will be my primary outcome. For Florida, I also utilize other parcel-level assessment valuations, sales price records, and measurements of living area and the number of units, as alternative outcomes. In North Carolina, there are data reporting issues that only allow me to utilize 47 out of the 100 total counties in the state.[12] However, as you can see in Figure 1, the missing counties are not concentrated in any specific part of the state, leaving relatively uniform coverage.

4.b Flood Zones

Data on flood zones comes from the National Flood Hazard Layer (NFHL), which is a geospatial database that contains current effective flood hazard data. This includes the boundaries of each flood zone across both states, as well as measurements of the base flood elevation (BFE). I overlay the NFHL with the parcel data to determine what percentage of each parcel lies within the floodplain. The BFE measurements are then used to inform rating calculations under RR1 and flood zone overlap areas are used when aggregating to the block group-floodplain level. Distance to flood zone boundaries are also calculated

[11] In insurance manuals under RR1, there were numerous rating examples at the back of the manual that could be easily followed by inputs from tables as in Appendix Figure A.1. Now, in the rating manuals under RR2, the examples include a footnote saying "All components of the total amount due will be calculated by the FEMA system."

[12] The statewide parcel database for North Carolina is compiled by collecting tax parcel data from each county individually. However, there is significant variation in the processes each county uses to collect and report their specific tax parcel data. Unfortunately, a large portion of counties do not report primary structure construction years for their tax parcels, causing me to drop them from the analysis.

using the parcel centroid as a reference point. These flood zone area shapes can be seen in Figures 1 and 2, highlighted in light blue.

4.c Topographical and Hydrological Measures

In order to calculate parcel-level premiums under RR2, I utilize data on elevation and hydrology from the United States Geological Survey (USGS). The 1/3 arc-second Digital Elevation Model (DEM) provided through USGS provides 10x10 meter grids of elevation measures for each state that I can then map to each parcel. Additionally, data from the National Hydrography Dataset (NHD) and the National Watershed Boundary Dataset (WBD) provide geo-located measures of rivers, lakes and drainage areas. Using these two hydrological datasets, I can construct measures such as distance to flooding sources and nearest drainage amount.

4.d Leveed Areas and Barrier Islands

Following conventions outlined in Milliman (2022), I manually construct measurements of barrier islands by drawing polygons around selected island areas that are completely detached from the mainland.[13] I then map these new barrier island polygons back to the parcel data to identify parcels that are on barrier islands. Data on leveed areas comes from the National Levee Database, which is administered by the U.S. Army Corps of Engineers. This provides polygons of areas protected by levees as well as information regarding the specific characteristics of each levee.

4.e Other Data

I utilize a variety of other data sources to explore robustness and heterogeneity of my main results. First, I have data on individual NFIP claims (from 1978-2023) and policies (from 2009-2023) that provide information on claim amounts, flood damages, and program participation for policyholders. Next, I use data from the 5-year American Community Surveys (ACS) for measurements related to demographics, income, and housing at the block-group level. I use data from USGS stream gauge monitors to measure stream-flow flooding conditions, annual average rainfall measurements from DAYMET, and geo-located hurricane storm paths from the NOAA HURDAT Best Track data to measure weather conditions over my sample period. Finally, I use data from the USGS Protected Area Database to identify protected federal lands and data from the US Forest Services Land Fire program to measure elevation slope.

4.f Premium Calculations

In order to construct my main treatment measure, I calculate premiums at the parcel level under RR1 and RR2. For each parcel, I utilize the block group-floodplain averages from the NFIP policy data to assign measures of building replacement cost values and coverage amounts. Using the insurance rating manual from April 2021, I construct the hypothetical premium of building a single family home on each parcel, using the mapped coverage amounts, flood zone assignment, and elevation relative to the BFE.[14] Next, following the materials in Milliman (2022), I do the same exercise. For these calculations, I use parcel-level measures of distance to river, distance to coast, distance to ocean, elevation, relative elevation

[13]These drawings are done in QGIS. See the appendix for more details.
[14]I assume the single family home has no basement and only one floor.

(within 500 meters), barrier island status, leveed area status, total flow-line drainage, in addition to the mapped coverage amounts and replacement cost values. Each measure is then averaged at the block group-floodplain level, so I can construct my final treatment measure:

$$D_i = log(\overline{Premium_i}^{RR2}) - log(\overline{Premium_i}^{RR1}) \tag{1}$$

where i is a block group-floodplain unit of observation and $\overline{Premium_i}$ is the block group-floodplain average of parcel-level premium calculations. D_i is the log difference of the average hypothetical premium under each price regime, which will give me the approximate percentage change of flood insurance premiums faced by developers and potential new customers.[15]

Maps of D_i are constructed for each state in Figures 1 and 2. The red shading denotes areas that experience any positive increase in their premium, even as small as $1, while the orange denotes decreases. In both states, you can see significant variation in the treatment measure over the entirety of the state. Premium increases are not concentrated on the coast, and likewise, premium decreases are not concentrated in inland areas. However, in both states, one might notice that decrease areas are often following the shapes of river polygons, particularly inland. These patterns highlight the newly introduced variation in premium pricing across flood zones. As mentioned above, the transition from using flood zones to individual properties in order to determine premiums results in variation in premiums *across* flood zones of the same type. While many of these inland riverine areas still face flood risk, prices under RR1 in those areas were far too high for the true level of flood risk. This is precisely due to the fact that under RR1, all flood zones of the same type were treated as equals. Now, the pricing scheme can recognize that it doesn't make sense to charge an inland river area the same as a coastal river area, as they likely face very different flooding conditions and risks. Appendix Figures A.4 and A.8 illustrate these decrease patterns more clearly.

Figure 3 plots the distribution of premium changes for both states. In both North Carolina and Florida, the distribution follows a similar pattern, with the largest mass being centered around zero or moderately positive increases, and long tails of decreases and extreme increases. Dissecting these changes further in Figure 4 shows that nearly all of the significant decreases in premiums are occurring in the floodplain, for the same reason discussed above about the prior treatment of all floodplains as equals. Further, these distributions show that Florida is experiencing much higher premium increases compared to North Carolina, with maximum increase amounts about 5 times larger. This is reflective of the fact that Florida has a much higher average flood risk than North Carolina and therefore, should pay more in total flood insurance amounts. Before, under RR1, the price of risk was unevenly distributed across states, resulting in places with lesser risk (like North Carolina), subsidizing the true cost of flood risk for more dangerous states such as Florida. FEMA sees this more accurate and equitable distribution of premiums and risk to be one of the major benefits of RR2 (NFIP, 2023).

4.g Analysis Sample

I construct two separate analysis samples for both North Carolina and Florida, and calculate my outcome and treatment measures at the parcel level. These measurements are then averaged over the block group-

[15]I use the log difference instead of the percentage change to account for any asymmetry around zero, as percentage decreases in premiums would be bounded by -1.

floodplain unit. I separate block groups by their floodplain status to account for regulatory differences between the floodplain and non-floodplain areas of each state. As mentioned prior, floodplains require stricter building standards and alternative regulations compared to their non-floodplain counterparts in the same community, which could introduce bias into my estimates. Block groups can either be fully in a floodplain, fully outside a floodplain, or have a portion dedicated to both. Table 1 reports summary statistics by state, highlighting key differences in floodplain area distribution. In North Carolina, a limited amount of block groups are fully within a floodplain, while in Florida there are considerably more, reflecting the larger prevalence of floodplain development and land availability across the state.

A key challenge when creating my analysis sample is to drop observations that do not contribute to my identifying variation without introducing selection bias. Thus, each sample restriction needs to be carefully chosen in order to cut out parcels that cannot credibly be developed, or are so far removed from flood risk that the reform will have no effect on their development status. This leads me to restrict my samples to all "potentially developable" parcels that are undeveloped in 2015, the year before my sample period begins. I drop parcels that are government-protected or publicly-owned lands as well as parcels that are described as schools, institutions, agriculture and churches. These parcels will likely face a different development decision when compared to a residential or commercially-owned parcel. Further, I restrict to all parcels that are within a mile of a flood zone to ensure there is some measure of salience for the price reform.[16] Finally, I drop parcels that have an elevation slope of 25% or greater, as development on these parcels is much more difficult and faces different regulations.[17] After dropping all of these parcels, I then aggregate my data up to the block group-floodplain level for my analysis.

My main outcome variable is the percentage of newly developed parcels within a block group-floodplain. I utilize an average share as opposed to counts so that I can compare more easily between block groups with different numbers of available vacant parcels.[18] In order to better reflect the decision perspective of the developer, I update the number of available vacant parcels each year to account for the newly developed parcels, resulting in the measurement of my outcome as such:

$$y_{it} = \frac{NewDev_{it}}{\sum_t^T AllPars_{it} - NewDev_{it-1}} \tag{2}$$

where $NewDev_{it}$ is the number of newly developed parcels and $AllPars_{it}$ is the number of vacant parcels in block group-floodplain i in year t. The denominator reflects the updating count of vacant parcels as development occurs over the sample. I prefer this measurement to a static denominator as it better reflects the development decision environment a developer will face in each given year.[19]

Table 1 shows that the mean development share is roughly similar between the two states within my unit of observation: 5% for North Carolina and 6% for Florida. However, we can see that over the sample period, North Carolina actually develops more parcels than Florida, which might be surprising considering Florida is larger and has more parcels and available land. This is likely due to the fact that in the Florida data, I can more accurately identify vacant parcels that are suitable for development as there is a standardized land use code that is consistent over time and counties. For North Carolina, I have to rely on string descriptions that likely increases measurement error and over-inflates the parcels

[16]In practice, this is not too extreme of a restriction as most parcels are within a mile of a flood zone. This just ensures that parcels that have close to zero flood risk are excluded from my sample.

[17]This is standard practice within the literature.

[18]However, results are also robust to including measures of total parcel counts in my model specification.

[19]I show robustness to my outcome measurement in Table X, where I impose a fixed denominator for the number of available parcels.

I end up including in my sample. Including extraneous parcels with no likelihood of development likely biases my results downward for North Carolina.

5 Empirical Strategy

The core of my identification strategy relies on a difference-in-differences (DID) framework where I compare units with significantly large premium changes to units with little-to-no premium changes. In an ideal setting, I would use all of the continuous variation in my treatment variable, D_i, in a standard two-way fixed effects (TWFE) specification as was standard in the prior literature:

$$y_{it} = \alpha_i + \gamma_{tf} + \beta \cdot (POST_t \cdot D_i) + \epsilon_{it} \tag{3}$$

where y_{it} is the share of new development in block group-floodplain i in year t, α_i is a block group-floodplain fixed effect, γ_{tf} is a year by floodplain status ($f \in \{\text{Floodplain}, \text{Non-Floodplain}\}$) fixed effect, $POST_t$ is a indicator equal to 1 in years 2021-2023, D_i is my continuous measure of calculated average premium changes, and ϵ_{it} is an error term capturing any unobserved omitted influences. However, recent work by Callaway et al. (2024a) show that there are considerable weighting and interpretation issues with β. In particular, they illustrate that in the past researchers have interpreted β as a level treatment effect, a scaled level treatment effect or even an elasticity-type marginal effect and show there are unique weighting issues with each interpretation. They propose both new assumptions and new estimators to address these shortfalls, which I employ later in the paper to explore heterogeneity in my ATTs across the premium change distribution.

To start, I employ binary and discrete versions of my treatment variable, D_i, to summarize average treatment effects on the treated (ATTs), and build intuition towards the new continuous estimator for my setting. First, I will define new treatment variables to fit the non-continuous framework. Let $median(D^f)$ be the median value of D_i within floodplain type f. Then, the binary version of my treatment variable will be:

$$d_i = \begin{cases} 1, & \text{if } D_i^f > |median(D^f)| \\ 0, & \text{otherwise} \end{cases} \tag{4}$$

where $|median(D^f)|$ is the absolute value of the f-specific median D_i.[20] This defines the treatment group as block group-floodplains that receive *any* significant changes in their premium as a result of Risk Rating 2.0. Replacing D_i for d_i yields a slightly different specification and interpretation of β:

$$y_{it} = \alpha_i + \gamma_{tf} + \beta \cdot (POST_t \cdot d_i) + \epsilon_{it} \tag{5}$$

I view this as a test of the price reform overall, as it will include the effects of both increases and decreases in premiums on new development. Assuming symmetric responses to the pure price effect of the reform, one would expect positive increases in premiums to lead to decreases in development and decreases to lead to increases in development. Thus, β in Equation 5 will be the effect of transitioning flood insurance prices from their distorted version to actuarily correct values. This will provide a strong

[20] Median values of D_i for North Carolina are 0.60 in the floodplain and 0.28 outside the floodplain. Median values of D_i for Florida are 0.72 in the floodplain and 0.40 outside the floodplain

test of whether the NFIP was contributing to moral hazard within development decisions. If β is negative and significant, accurate pricing of risk led to less development overall, strongly suggesting that the prior pricing system distorted development incentives through the under-estimation of true flood risk on average. If β is positive and significant, this would suggest that true flood risk was over-estimated on average and the correction led to more development. In this specification, β is identified under a standard parallel trends assumption: In the absence of Risk Rating 2.0, treated units' average new development share would have evolved in the same way as the observed path of the untreated units.

Recognizing that Equation 5 could be too limiting, I allow for treatment heterogeneity by positive and negative premium changes to investigate the symmetry of responses and test whether there are significant effects on both sides of the premium change distribution. Now, treatment is defined:

$$d_i = \begin{cases} \text{Increase}, & \text{if } D_i^f > +median(D^f) \\ \text{No Change}, & \text{if } +median(D^f) <= D_i^f >= -median(D^f) \\ \text{Decrease}, & \text{if } D_i^f < -median(D^f) \end{cases} \tag{6}$$

with the corresponding specification:

$$y_{it} = \alpha_i + \gamma_{tf} + \sum_{j=1}^{J-1} \beta_j \cdot (POST_t \cdot \mathbb{1}\{D_i = d_j\}) + \epsilon_{it} \tag{7}$$

where $d_j \in \{\text{Increase}, \text{No Change}, \text{Decrease}\}$, $+median(D^f)$ is the positive value of the f-specific median D_i, and $-median(D^f)$ is the negative value of the f-specific median D_i.[21] The omitted category is the "No Change" group, leading to the interpretation of β_j to be the effect of each treatment group d_j relative to the group that received no significant change in their premiums. This specification allows me to test for treatment effect heterogeneity across different parts of the premium change distribution and test priors on symmetry in responses. In this specification, each β_j is identified under a standard parallel trends assumption: In the absence of Risk Rating 2.0, both "Increase" and "Decrease" units' average new development share would have evolved in the same way as the observed path of the "No Change" units.

In all specifications, to account for potential serial correlation of the observations from the same block group, I adjust standard errors by clustering at the block group level and allowing for an arbitrary variance-covariance matrix within each block group over time. I elect not to cluster at the block group-floodplain level (i.e. my unit of observation) as it is likely that there is within block group correlation of errors across the separate units.

6 Results

6.a New Development

Table 2 reports my main empirical results from equations 5 and 7. Columns 1 and 3 show the impact of Risk Rating 2.0 overall in both states. Each coefficient (β) is statistically significant and negative, implying that the total effect of the price reform led to less development. This is strong evidence that

[21] Concretely, if looking at the median value of D_i for the North Carolina floodplain, $+median(D^f) = 0.60$ and $-median(D^f) = -0.60$

the NFIP was indeed contributing to moral hazard in development decisions and fueling building in riskier areas. Both coefficients are of an economically meaningful magnitude as well. In North Carolina, Risk Rating 2.0 led to a decrease in the share of new development of about -0.015pp for the average block group-floodplain. Compared to the control mean of 0.055, this effect corresponds to a decrease in new development of 27%. In Florida, results are a bit more muted, but still large and impactful, as the -0.0046pp decrease over a control mean of 0.047 corresponds to a 10% decrease in development on average.

When I discretize my treatment further and explore heterogeneity by increases and decreases in premiums, the results tell a similar story. Columns 2 and 4 show that the overall price reform effects are being entirely driven by areas that experience an increase in their premiums. In North Carolina, increases in premiums led to a 36% decrease in the share of new development compared the control mean and in Florida, a 14% decrease. Further, in North Carolina, there is no meaningful response to premium decreases, as the β_{Dec} coefficient is quite small and statistically insignificant. In Florida on the other hand, there seems to be evidence of a symmetrical response, as β_{Dec} is positive and statistically significant and of a similar magnitude compared to β_{Inc}. The fact that $|\beta_{Inc}| > |\beta|$ and $\beta_{Dec} > 0$ for both states suggests that including the decreases in the treatment group in columns 1 and 3 is likely leading to attenuation in β.

Since D_i is not randomly assigned, it is important to verify that the fixed differences between treated and control units would not have led to a violation of my identifying parallel trends assumption. In order to test this, I can include pre-RR2 year indicators in my specifications to investigate whether there are differential trends prior to RR2. As an example, the specification for the binary case becomes:

$$y_{it} = \alpha_i + \gamma_{tf} + \beta \cdot (POST_t \cdot d_i) + \sum_{t<2020} \phi_t \cdot (k_t \cdot d_i) + \epsilon_{it} \tag{8}$$

where k_t is an indicator variable for year t. With this specification, I can then jointly test if all of the included pre-trend indicators (ϕ_t) are equal to zero. If I am unable to reject the result of this test, I can conclude that parallel trends is likely to hold. For each column in Table 2, I report the p-value of the joint F-Test that $\phi_t = 0 \quad \forall t < 2020$. In columns 1 and 3, I am unable to reject the test of joint coefficient nullity, suggesting that my identifying assumptions are justified. Further, I can show this visually in an event study plot. Altering the specification in Equation 5 slightly:

$$y_{it} = \alpha_i + \gamma_{tf} + \sum_{t \neq 2020}^{T} \beta_t \cdot (k_t \cdot d_i) + \epsilon_{it} \tag{9}$$

where k_t is an indicator variable for year t, and β_t is the year-specific coefficient of interest. The year 2020 (which is the year prior to RR2 implementation) is dropped and used as the reference period as is standard practice. Figure 5 displays the results of Equation 9. Visually, we can confirm the results of the F-tests as there is little evidence of any pre-trends in the years prior to RR2. Further, this plot allows us to examine the timing of the treatment across both states, of which there is some difference. In North Carolina, the plot suggests that the reform takes a bit of time to develop, as effects slowly increase in magnitude over the post-period, while in Florida, effects are rather immediate. This can likely be explained by the differences in each state's exposure to the NFIP and familiarity with flooding. Table 1 shows that the average county in Florida has much more of it's area covered by floodplains, along with significantly higher amounts of NFIP policies and claim amounts, and experience with hurricanes. Thus, one should expect Risk Rating 2.0 to be a much more salient issue in Florida than North Carolina, leading to more immediate results.

The p-value reported in column 2 shows that there is no evidence of pre-trends when splitting treatment into increase and decrease groups, but for Florida, column 4 says otherwise. I am able to reject the joint null of $\phi_t^{Inc} = \phi_t^{Dec} = 0 \quad \forall t < 2020$. Exploring these trends visually in Appendix Figure A.12, one can see this test failure is entirely driven by the decrease treatment group in the years 2016 and 2017. The increase pre-trend looks to be flat visually and is not reject-able when conducting a joint F test of $\phi_t^{Inc} = 0 \quad \forall t < 2020$. While concerning, I take this as evidence that arbitrary functional form specifications of my treatment (i.e. such as increase and decrease splits) might not be the most appropriate model, and defer to the tests proposed in Callaway et al. (2024a) for a continuous estimator to justify my assumptions.[22] They suggest that one can run a test of parallel trends for a continuous DID treatment by "comparing the average change in outcomes for all treated units to the average change in outcomes for untreated units. This can be estimated by running a binary DiD with a 'treatment dummy' equal to one for any units with positive doses." (Callaway et al., 2024a). Conveniently, this suggestion exactly corresponds to my specifications in Equations 5 and 9, which show little to no evidence of pre-trends.

As mentioned above, understanding which premium change levels led to the largest effects is of great policy interest and highly related to concerns regarding climate adaptation policy. Table 2 has already shown some evidence of important treatment effect heterogeneity across the premium distribution, showing evidence of potentially asymmetric responses in North Carolina and symmetric responses in Florida. To continue this exploration of my ATT heterogeneity, I split my treatment groups in two, creating "Big" and "Small" treatments within increases and decreases.[23] Table 3 shows the results of this further sub-dividing. Columns 1 and 3 show evidence of treatment effect heterogeneity across both the increase and decrease groups. In both states, treatment effects are larger for the "Big" treatment group compared to the "Small", for both increases and decreases in premiums. In Florida, column 3 shows that it is only large premium decreases that are driving the significance of the positive results, which also explains why these positive effects are not enough to outweigh the negative effects from the increase in totality. From a climate adaptation perspective, a policy-maker might be encouraged that both large and small changes in premium increases are leading to a shift away from riskier development, as opposed to only the largest increases. It suggests scope not only for price effects, but successful risk communication as well.

Finally, columns 2 and 4 show the results from Equation 3. Despite the many issues with this specification as outlined above, I elect to show these results for completeness and familiarity to prior methods in the literature, as well as to illustrate the potential issues with interpretation. The coefficients for both North Carolina and Florida are similar in magnitude and sign when compared to the binary versions reported in columns 1 and 3 of Table 2, although both are slightly larger. The fact that the estimates are close suggest they are highly comparable. The problem, as outlined in Callaway et al. (2024a), is the interpretation of β Equation 3. If we were to make the most direct comparison between Equation 3 and Equation 5, β would be interpreted as a level treatment effect. This interpretation suggests that continuous treatment (D_i) led to a -0.01784pp decrease in the share of new development

[22]Additionally, I have tested alternative specifications for Florida in column 4 that can produce better pre-trends, but I prefer the parsimonious model so I can ensure better comparability between North Carolina and Florida and thus argue more strongly for external validity of my results.

[23]"Big" and "Small" are defined as above median and below median, respectively.

on average in North Carolina. Compared to estimates in 2, this is quite similar. My binary treatment measure (d_i) led to a -0.01488pp decrease in the share of new development. However, is this the correct interpretation of β in our continuous version? Many times, researchers report a scaled level effect of the type I report in Table 3, which is calculated by multiplying β by the mean treatment level in the sample (i.e. for North Carolina, -.01784 $\times$ 0.114 = -0.002). This coefficient is much smaller than the binary version now, implying treatment led to a -.002pp decrease in the share of new development, which is only a 5% decrease over the control mean (compared to 27% in the binary treatment version). Further, many researchers often interpret β as a causal response parameter, suggesting a 1pp increase in premium change leads to a -0.01784pp decrease in the share of new development. Thus, this highlights the need for careful and purposeful interpretation of the causal effect of interest, which is why I employ binary measures of treatment for level treatment effects and then visual exploration of flexible treatment heterogeneity, as suggested by the methods in Callaway et al. (2024b,a).

6.b Alternative Housing Variables

The Florida tax data offers a much richer and more accurate source of parcel information over time, compared to its counterpart in North Carolina. This allows me to explore alternative outcomes related to the reform that could also be affected by the correction of flood insurance pricing. In particular, I am able to explore sales prices, the number of residential units and the total living area (in square feet) at the parcel level. Table 4 reports the results of these regressions using the specification in Equation 7. Column 1 shows that sales prices are quite responsive to changes in flood insurance prices. In areas with increases in premiums, sales prices decrease significantly by nearly 13%. Conversely, while insignificant, there is evidence that sales prices increase in response to premium decreases. This is consistent with existing research by Georgic and Klaiber (2022) who find that NFIP subsidies were capitalized into housing prices. Thus, it is unsurprising that the price reforms enacted through Risk Rating 2.0, which both eliminate subsidies and correct other price distortions, have the same effect. Further, this result can provide some suggestive, partial equilibrium evidence on how supply and demand could be shifting in response to flood insurance price changes. Risk Rating 2.0 provides an interesting setting in which one could argue it is a pure demand shifter, as it has no effect on housing supply outside of responses to demand shifts.[24] Developers should only be altering their supply decision based on expected demand, not on any change to their input costs or other supply shocks. Based on my main results in Table 2, RR2 should result in an inward shift of the demand curve, driving down prices and quantities of housing, either due to price effects or information signals about risk. If we assume the supply curve doesn't shift at all, an inward shift in demand would explain the decrease in sales prices found in Table 4. However, the fact that I find negative price effects suggests that even if the supply curve did shift inward due to developer expectations of demand, it must not be enough to overtake the inward shift of demand. This evidence suggests that the negative development effects from Risk Rating 2.0 are either entirely or mostly driven by an inward demand shift.

Columns 2 and 3 allow me to investigate intensive margin effects of the areas that do get developed over my sample period. Both the number of residential units and the total living area appear to increase in block group-floodplains that receive an increase in premiums. At first glance, this seems counter-

[24] If the policy changed building regulations for example, it could effect supply by increasing the cost to build in areas with premium increases. That is not the case however.

intuitive to my main findings of decreased development in these areas. However, it is entirely possible to have a decrease in the average number of *parcels* developed, while increasing units and living space. I hypothesize that while developers are building less at the parcel level, they might try to compensate by building up vertically. Adding an extra unit to a building or expanding the size of a building that is already set to be built, could be subtle ways to subvert the increased premium costs that would be accrued if building an entirely new structure on a new parcel. For multi-unit buildings, premiums are usually handled by the Homeowners Association or Co-op board, distributing costs or leaving it up to the unit-owner to purchase insurance. For renters, flood insurance is optional and would instead be the responsibility of the building owner. Further, if this is less price-driven and more risk-driven, there could be higher demand for high-floor apartments if people wish to avoid structures at ground-level. Future research into the intensive margin responses of development would be of great interest, particularly from a climate-adaptation perspective.

7 Heterogeneity

My results in Table 2 strongly suggest that the NFIP's prior pricing scheme was contributing to the moral hazard of development in flood-prone areas. Now, I explore the heterogeneity of my results by both covariate groups of interest and treatment intensity. This will help to illuminate the efficacy of this type of policy reform and evaluate its potential for climate adaptation.

7.a Heterogeneous Responses by Covariates

Ex ante, one would expect Risk Rating 2.0 to have a larger effect on floodplain development compared to non-floodplain development due to the mandatory purchase requirement. If prospective home-buyers have to purchase insurance they are likely going to be much more sensitive to the price than if it is merely an option. However, it could also be the case that the customer base for floodplain development has much lower information frictions than their counterparts purchasing land outside the floodplain. This sub-population could be quite willing to take on the risk at the new price. Separately, affordability of the new premiums under RR2 has been a significant concern. Demand in low-income floodplain areas could be relatively inelastic due to moving constraints. Thus, responses by different covariate groups are going to rely on empirical tests to clear up the ambiguity of their direction. Further, the response could also be dependent on the underlying conditions of each state.

Figure 6 plots the results of heterogeneity responses by covariate groups for both states. In North Carolina, the negative development effect is being entirely driven by responses outside the floodplain. Strikingly, there is an increase in development in the floodplain of seemingly similar magnitude. Distance to flood zone results confirm this split, as places farther from the flood zone boundary get developed much less. Together these results seem to suggest that RR2 had a significant and negative effect on development in the floodplain periphery, where purchase of insurance is entirely optional. This strongly suggests that the new prices are potentially signaling new information about risk to prospective property buyers. We see further evidence of this type of risk signaling when using proxy measures for flood knowledge via historical hurricane exposure and pervasiveness of NFIP policies. On both measures, I find that in areas with less hurricane exposure and less NFIP policies, development goes down significantly, and responds positively with high hurricane exposure and more NFIP policies, just like the floodplain results. I take this as confirming evidence that in North Carolina, due to the limited amount of floodplain area, RR2 led to significant decreases in places on the periphery of the flood zone that may have been unaware of their true flood risk. RR2 seems to have alleviated information frictions in these places through the updating of flood risk prices and prospective property buyers may now be unwilling to accept the level of risk of building in an area that might be near a flood zone, despite not ever having experienced a flood.

Additionally, heterogeneity results for North Carolina show that it is high income and high primary residence areas that are driving the negative responses to the reform.[25] This further adds to the evidence that the price reform is operating via risk signaling as opposed to a pure price effect in North Carolina, as concerns of affordability should be ameliorated in high income areas that have a higher percentage of permanent, primary residences.

In Florida, I find evidence to the contrary, as the effect on floodplain development appears to be much larger than non-floodplain development (although much noisier). The other heterogeneity results show that there are not clear differences in responses along income or proxies for flood knowledge as in North Carolina. The most significant difference occurs when looking at the presence of primary residences. Here, I find that areas with a lower percentage of primary residences (i.e. more secondary or vacation homes) are driving the negative development effects in Florida. Taken together with the fact that more of the effect is coming from the floodplain and there are no clear differences along flood knowledge lines, this suggests that the price reforms are much more indicative of moral hazard corrections as opposed to risk signaling as we saw in North Carolina. It appears that prospective property buyers likely knew the risk being faced in these areas, but now are unwilling to pay the cost. This would be consistent with Florida's comparatively larger floodplain land area and experience with flooding when compared to North Carolina.

7.b Heterogeneous Responses Across the Premium Distribution

Next, I turn to further heterogeneity analysis by exploring the effects across the premium change distribution. As stated prior, understanding which parts of the premium distribution are driving results is of key interest to climate adaptation potential, as we can test whether *all* units respond to premium changes, or if it is only among units that experience certain levels.

My prior results have explored this briefly by separating my treatment effects by increases and decreases, but now, I extend this analysis and specify a more flexible and discretized version of treatment. I create indicators for treatment bins based on the following quantile lists for increases and decreases (starting at the median value that defines treatment). Increase bins are defined by the 0, 15th, 30th, 45th, 60th, 75th, 90th, and 100th percentiles (7 bins). Decrease bins are defined by the 0, 33rd, 66th, and 100th percentiles (3 bins). Figure 7 plots the results of this exercise for both North Carolina and Florida.

In North Carolina, there is no evidence of premium decreases having any effect on development, while premium increases lead to larger and larger decreases in development as the premium change itself grows in magnitude. Premium increases start to produce significant effects around 40%, which is encouraging when thinking about climate adaptation potential. If development is also responding to lower levels of

[25] Primary residence is the place where a homeowner lives the majority of the year, unlike a secondary residence, which is most often a vacation home. Thus, places with low percentages of primary residences are much more likely to be vacation spots.

premium changes, prospective property buyers are likely inferring signals about risk through the price, which is consistent with the prior heterogeneity results by differing covariate groups. The fact that the effect seems to decrease linearly (to an approximation) however, also provides evidence that there is responsiveness to price, which would suggest a complementary moral hazard effect. Flood-prone areas have simply become too expensive for existing risk levels of property buyers.

A similar story exists in Florida, although there is continued evidence of the largest premium decreases driving increases in development. In regard to increases, there is less of a linear relationship present, but larger increases in premiums are still driving the negative development results. Further, there is some suggestive evidence that low levels of premium increases (around 40%, much like North Carolina) have some effectiveness in reducing development. While there was no evidence of this type of risk signaling in the other heterogeneity results, this could be suggestive of some risk communication occurring through the new premium prices. However, taken together, Florida still seems to be driven by a moral hazard story. The fact that we see responsiveness to premium changes at only considerably large levels (on both sides of the distribution) strongly suggests that the price changes have priced out prospective property buyers that are aware of the underlying risk but are not willing to pay the price.

Now, I follow the methods proposed in Callaway et al. (2024a,b), which allow me to construct the entire distribution of average treatment effects on the treated (ATTs) with respect to the whole premium change distribution. The flexible and non-parametric form of this method is preferred, as I do not have to make arbitrary functional form decisions when choosing how to bin and discretize treatment as above.

In short, this method involves calculating the ATTs manually using sample means and regressinng the changes in my outcome on a flexible functional form of my treatment D_i. In practice, I use a cubic spline with internal knots at the 25th, 50th and 75th percentiles of D_i. The resulting estimates from that regression can then be used to construct estimators of the ATT for each level of treatment in D_i. As a result, I can construct an entire function of ATTs over the premium distribution as displayed in Figure 8.

These figures tell a very similar story to the discrete version, but the flexible nature of the estimates allow me to not only compare the two states in an easier way, but understand how the treatment evolves smoothly with every level of premium change, not just the arbitrary bins which could also be hiding important heterogeneity. In North Carolina, significant effects start at lower levels of premium change increases (roughly 30%), confirming prior speculation that there is much more scope for information signaling of risk through the new prices as opposed to only a pure moral hazard effect. However, the fact that the ATTs decrease almost linearly with premium increases also suggests responsiveness to prices. Though I cannot disentangle the information signal from the price effect, the responsiveness at low and high levels of premium increases implies strong evidence of both effects.

In Florida, the continuous function shows a similar shape, but exhibits much more noise and a much flatter profile as premium changes increase. Significant negative effects on development only start around the 70-75% range, strongly suggesting that this is much less about signaling risk than it is about the higher prices. Further, this fact is bolstered by the relative flatness of the curve as premium changes increase. If developers are relatively uniform in their response to premium changes above a certain level, they are likely responding to demand decreases fueled by moral hazard responses, as prospective property buyers are now unwilling to locate in areas that have increased their price of risk at a relatively uniform rate above 70%. Again, this interpretation coincides with the history of flooding and disasters in Florida and their familiarity with the NFIP overall compared to North Carolina. Plus, additional heterogeneity analysis displayed in Appendix Figure**??**, confirm prior results that negative development effects are being driven by the floodplain and low primary residence areas. Similar to the main ATT function for Florida in Figure 8, the relative flatness of the low primary residence ATT curve strongly implies evidence of moral hazard as imposed to risk signaling. For every premium change level above 100%, there seems to be a uniform negative response in development, which I take as evidence of developers responding to uniform decreases in demand. Together, these results suggest a demand response fueled by people's unwillingness to buy secondary residences in flood-risk areas, a much clearer example of moral hazard than the North Carolina case.

8 Robustness

Tables 5 and 6 report robustness results from a variety of additional checks and alternate specifications. As stated prior, I trade-off parsimonious comparability between states with more accurate state-specific models in order to better argue for external validity. In most cases, robustness results can be strengthened by adding alternative controls that would better represent the state-specific setting. All robustness checks are completed using the specification in Equation 8.

My first robustness check investigates the concern that weather events over the sample period could be biasing results, as places with increased premiums are also places that will experience more extreme weather events. To account for this, I draw on a many rich sources of data to construct time-varying indicators that measure average annual rainfall, yearly exposure to hurricane-force winds, and average distance to flooding and stream-flow measures. Column 1 in both robustness tables shows my results are robust to these weather events.

In addition to the time-varying weather covariates, one also might be concerned that my results are driven by differences in income and house values, as places with increased premiums are also places of high wealth measures due to the structures being located in prime locations. Column 2 in both tables includes indicators for high income, high house values and high starting development share interacted with year-floodplain fixed effects (FE) in order to account for such differences in baseline wealth and development. Further, I include indicators for being with 400 meters of a water source, being a coastal county and in a place with high storm surge risk (also interacted with the year-floodplain FE). Column 2 shows my results survive, although the effect in Florida is slightly smaller in magnitude.

Next, one might be concerned that my premium calculations are biased due to the assignment of pre-period coverage amounts and building replacement cost values from the NFIP policy data. In column 3, I can use an alternate treatment measure that calculates the premium changes based on geographic features alone, assuming the same fixed values for insurance coverage and building replacement cost values. Column 3 shows my results are robust to this alternate treatment measure.

One might be further concerned that my results may be spuriously picking up underlying differential trends by block group-floodplains with different premium change levels. I add a linear trend ($d_i \cdot t$) to each specification to test this assumption in column 4. The results in Florida are robust to this trend, but less so in North Carolina. However, this is explicitly due to the timing of the effects in North Carolina, as the first two years in the post-period are relatively small and noisy compared to the very large significant drop in 2023. Despite this fact and the larger standard errors, the coefficient in column 4 is still negative and of an economically meaningful magnitude (6% decrease relative to the control mean).

A related concern to the linear trend above is one of mean reversion. If places with significant changes in their premiums are adjusting back to some equilibrium level, this could be picked up by my post-period treatment interaction. Column 5 investigates this potential issue by interacting the 2019 value of my main outcome (share of new development) for each observation with a full set of year dummies. This will flexibly control for any mean-reverting dynamics and potential differential trends that rely on block group-floodplain baseline characteristics. Both states' coefficients are robust to this exercise.

Next, those with knowledge of the NFIP might be concerned that I did not incorporate Community Rating System (CRS) discounts into the calculation of my hypothetical premiums, and my estimates are simply picking up artificially inflated premium differences. Column 6 applies CRS discounts to my premium measures and re-constructs the treatment variable. Both states are robust to this alternate treatment measure.

Building on the CRS discount concern, one might also be worried about savvy NFIP communities who will attempt to either increase their communities' CRS discount or update their flood maps around the timing of RR2's implementation in order to alleviate premium increases for their residents. Column 7 includes time-varying indicators for both CRS and flood map updates in each block group-floodplain to account for any changes over the sample period. Once again, results in both states are robust. Column 8 combines both the CRS discount treatment with the time-varying measures of CRS and flood map updates. The results remain robust.

Column 9 addresses the concern of the updating of vacant parcels over the sample period by fixing the denominator of my main outcome measure with the fixed baseline level of vacant parcels. Results in Florida survive with attenuation and significance, but results in North Carolina are attenuated and on the edge of significance. However, both coefficients are the correct sign and remain economically meaningful. I stress that without any additional controls for parcel counts or available land, this is likely far too strong a test. I find it encouraging that my results survive despite this fact.

Finally, column 10 addresses concerns with the definition of my control group. Instead of using the median values of treatment as defined by floodplain status, I arbitrarily set the control group bounds to be $+-0.30$. Both states' results are robust to this alternative treatment definition, and Florida's coefficient is even larger compared to baseline. North Carolina's coefficient is a bit smaller in magnitude compared to baseline, but still significant and meaningfully large.

I view the grand sum of all of these tests to be a powerful confirmation of my main results, especially when considering the parsimonious specification underpinning these regressions. The fact that both states can essentially survive the same 10 robustness checks provides a strong signal that my estimates are both valid and externally valid.

9 Back of the Envelope Impacts

In order to understand the potential impact of my estimates, I would like to benchmark my ATTs using average avoided development and average avoided disaster-related damage payouts. Using the estimates of my binary treatment specification in Equation 5, I add the ATTs in columns 1 and 3 of Table 2 back to the observed new development percentages for the treated block group-floodplains over the post-period (2021-2023). This should give me a proxy for the counterfactual new development percentage each treated block group-floodplain *would have* faced in the absence of Risk Rating 2.0. Fixing the availability of vacant parcels in 2021, I find that Risk Rating 2.0 led to 21,662 fewer parcels in North Carolina and 29,471 fewer parcels in Florida.[26] Using data on average claims and Individual Assistance (IA) federal funds distributed after a declared disaster, I can provide a rough estimate of avoided damages based on the avoided development under the counterfactual. In North Carolina, I find that the decrease in development led to a decrease in average claim amounts of $645,745 and decreases in IA amounts of $162,508. While not a huge amount, I will note that the number of flooding disasters in North Carolina over this time period was relatively sparse and not very widespread. Despite this fact, I can still find important reductions in damages and destruction as a result of the price fixes in Risk Rating 2.0. In Florida on the other hand, avoided development led to a reduction in average claims of nearly $2,440,900 and IA funds of nearly $144,228,127, a substantial sum. This is primarily due to the presence of Hurricane Ian in 2022, which resulted in nearly $110 billion in damages overall from just one storm event. While rough estimates, these amounts illustrate the enormous potential of the correction of risk pricing and how policy can be designed to guide development away from risky locations and reduce damage and destruction.

10 Conclusion

Flooding in the United States is a problem that is not going away. Increased flood risk as result of climate change stresses the importance of correct risk pricing to ensure agents can make informed and safe decisions about where to locate. This paper exploits a significant update in the price of flood risk within the National Flood Insurance Program to identify the causal effects of risk price updates on new development decisions. My results imply that the distorted prices of risk may lead to concerning moral hazard behaviors, confirming many of the critiques underpinning the NFIP. Further, the asymmetry in responses across the premium change distribution highlights a unique feature of this market and stresses unique policy considerations.

The negative development effects that I find in response to Risk Rating 2.0 show that while there are benefits to correcting the price of risk in terms of safety and destruction, there could be a significant trade-off with economic well-being of local communities. The asymmetry of my estimates suggests there could be serious downturns in development that could be having economic impacts on areas with premium changes that I have not explored, which I emphasize is an important area of future research. Further, the political economy decisions underpinning the interplay between local developers, local government leaders and community advocates is certainly an important aspect that can influence how risk price updates are distributed in practice. These relationships should certainly be studied to better understand the effects of flood insurance pricing reform.

Overall, my work highlights the potential benefits and limitations of using price instruments for climate adaptation policy. As more of the United States experiences floods, accurate risk pricing is a crucial first step in informing and protecting the public from the effects of climate change.

[26] The number of undeveloped parcels in North Carolina in 2021 is 335,752 and 393,060 in Florida.

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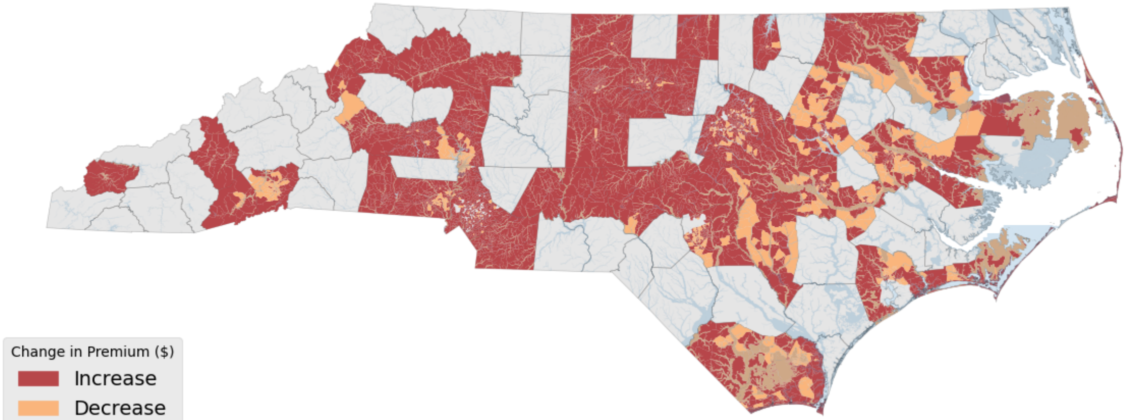
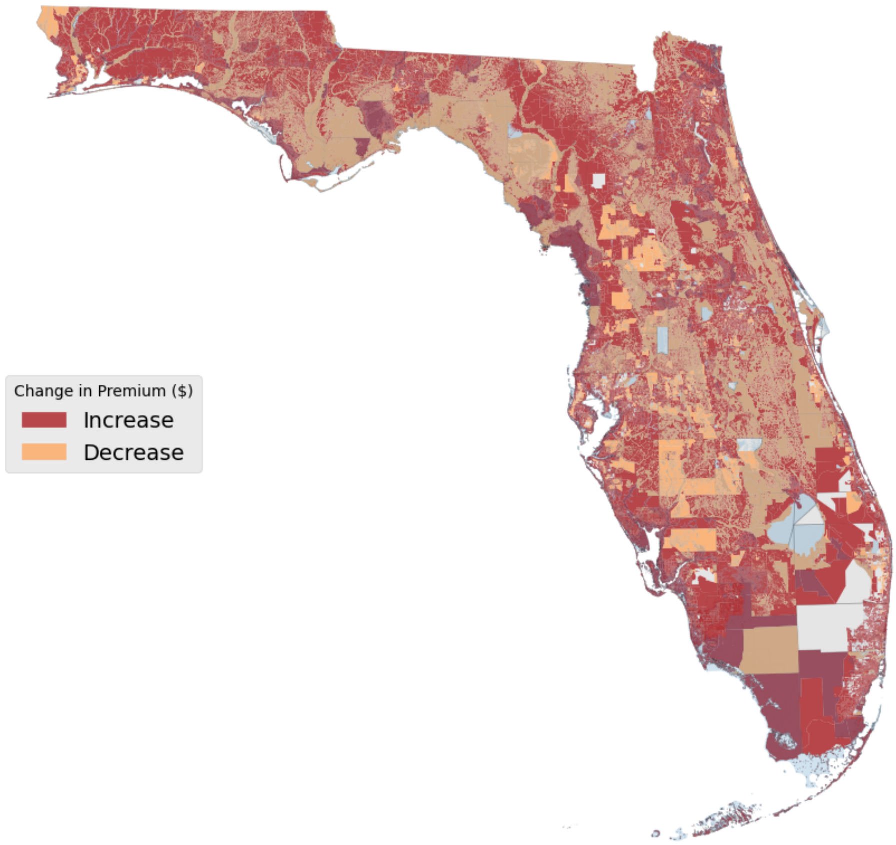
11 Figure List

Figure 1: North Carolina Treatment Map



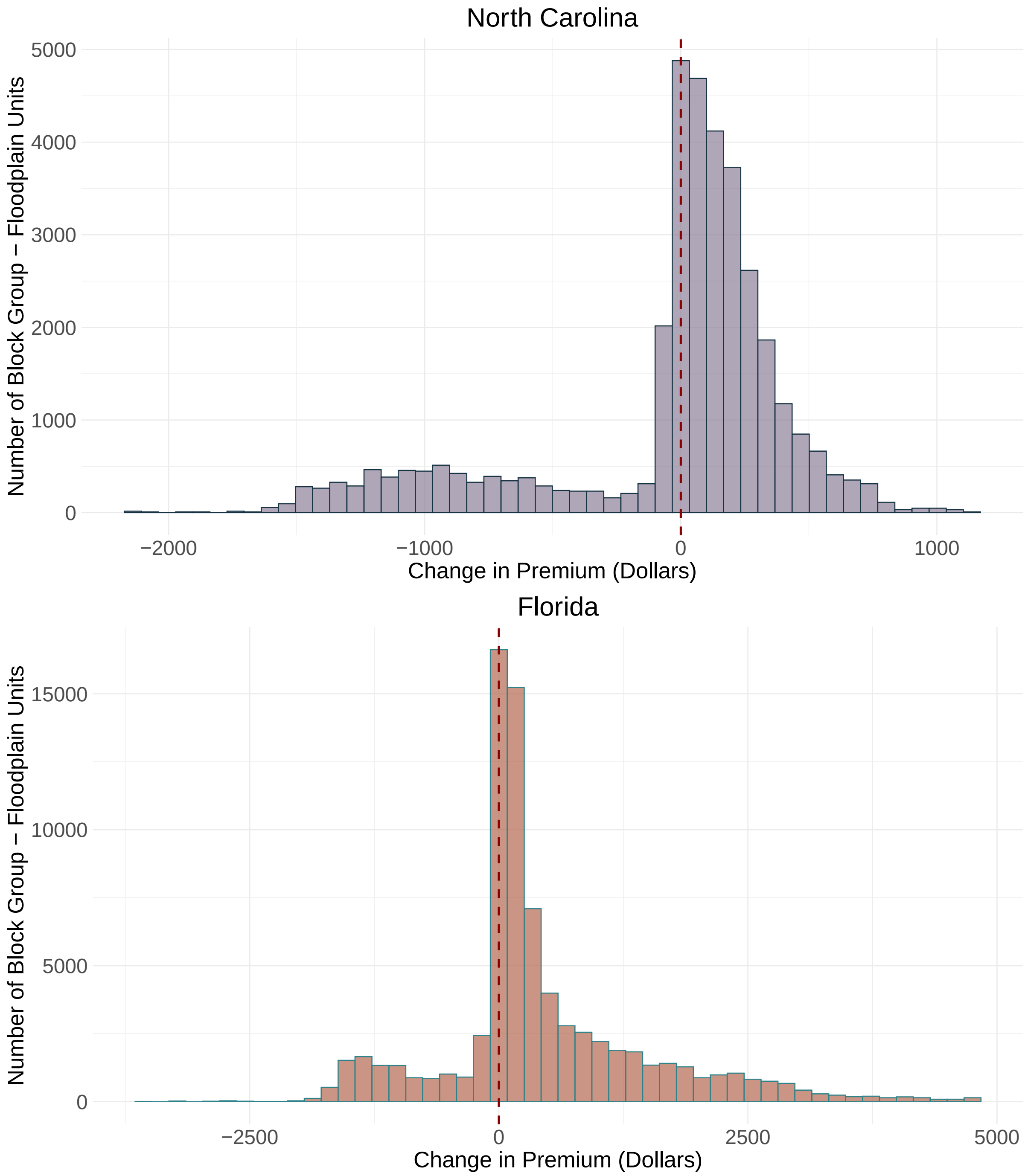
Each cell on the map is a block group - floodplain combination. Grey areas represent missing data. Blue areas are flood zones.

Figure 2: Florida Treatment Map



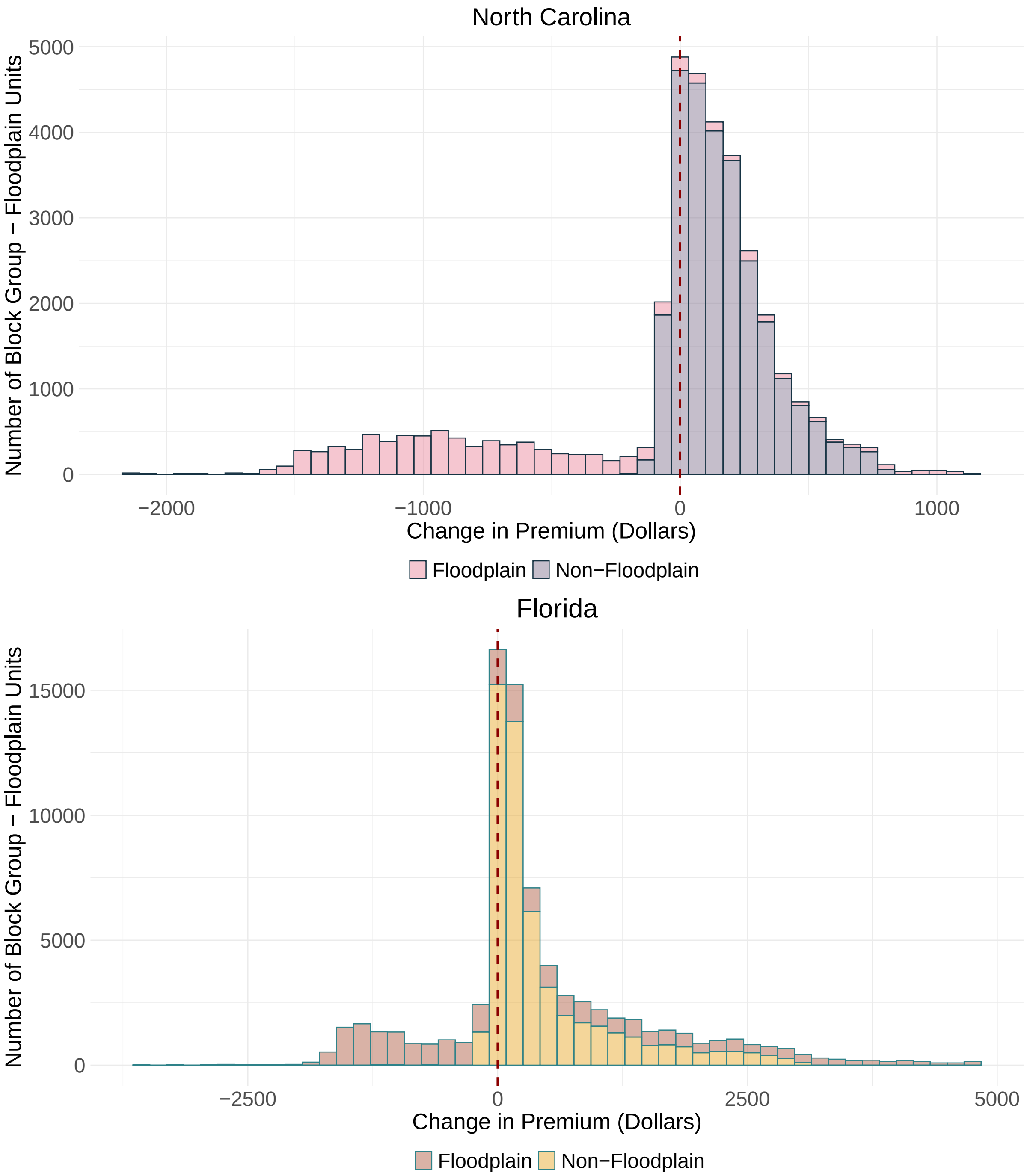
Each cell on the map is a block group - floodplain combination. Grey areas represent missing data. Blue areas are flood zones.

Figure 3: Premium Change Distributions



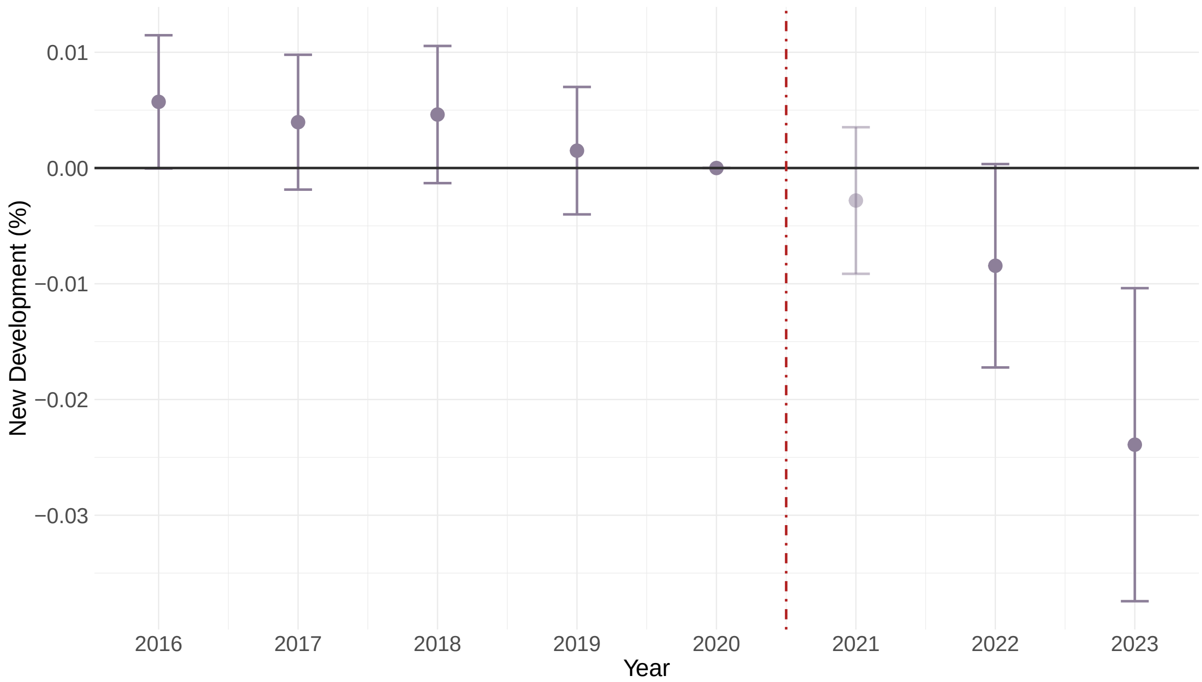
Change in premium is the dollar difference between a single-family home premium under RR2 compared to RR1.

Figure 4: Premium Change Distributions by Floodplain Status

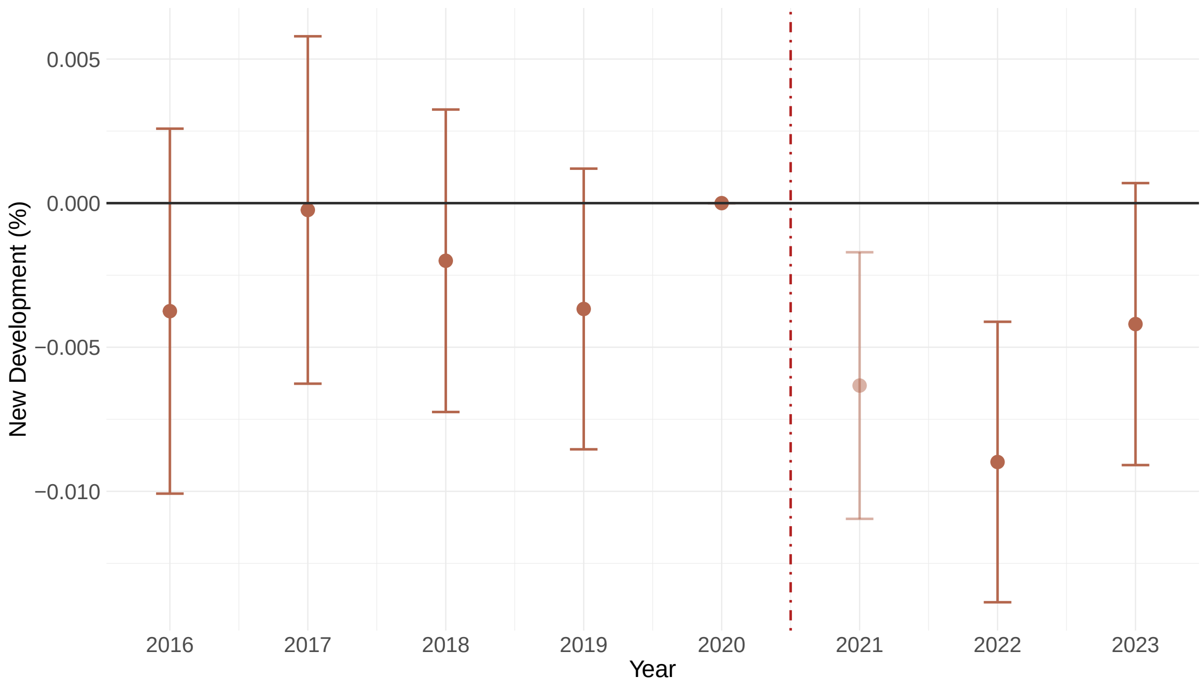


Change in premium is the dollar difference between a single-family home premium under RR2 compared to RR1.

Figure 5: Event Studies of Main Results

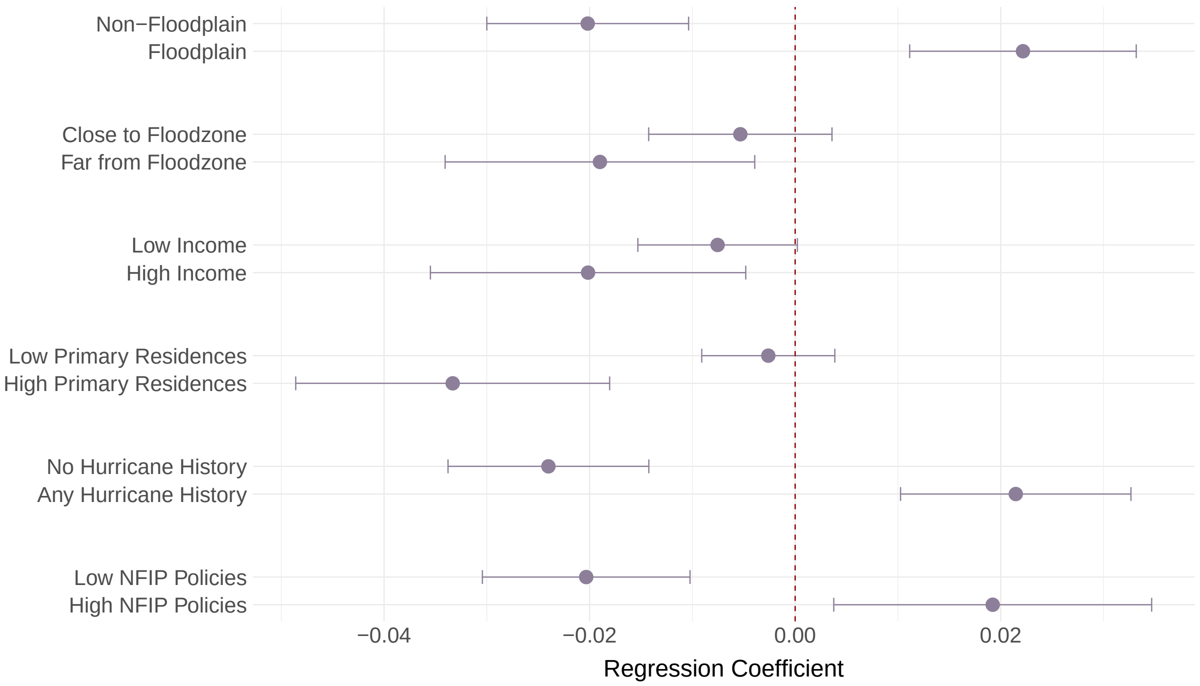


(a) North Carolina

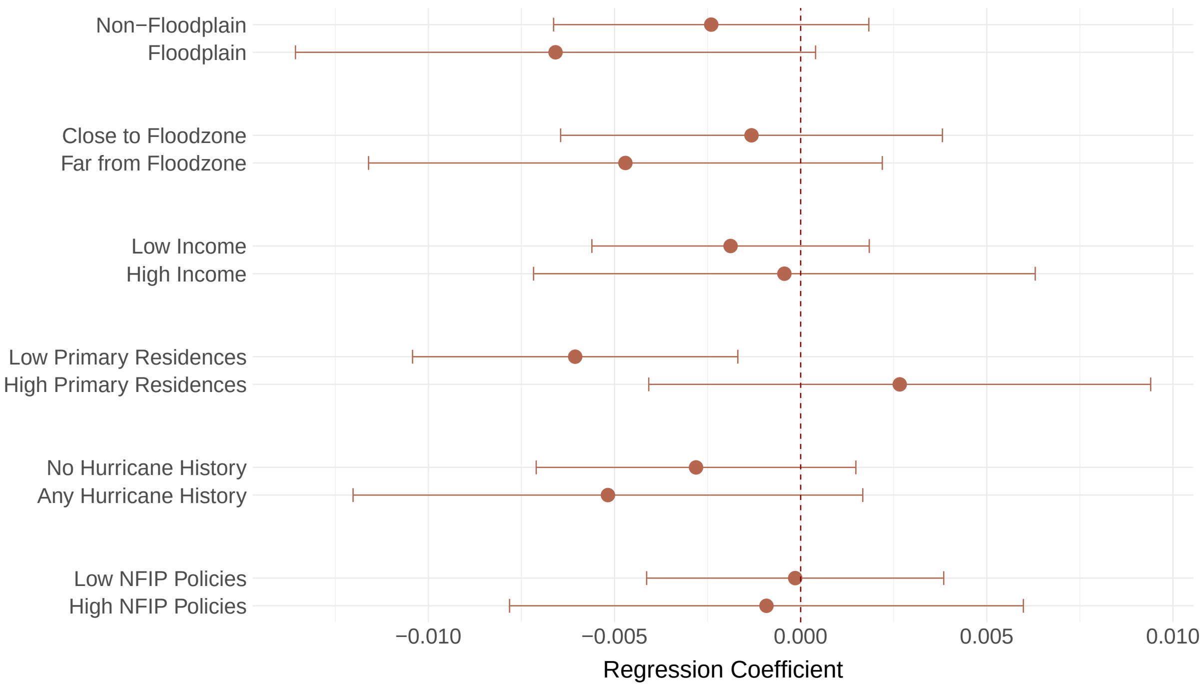


(b) Florida

Figure 6: Heterogeneity of Main Results

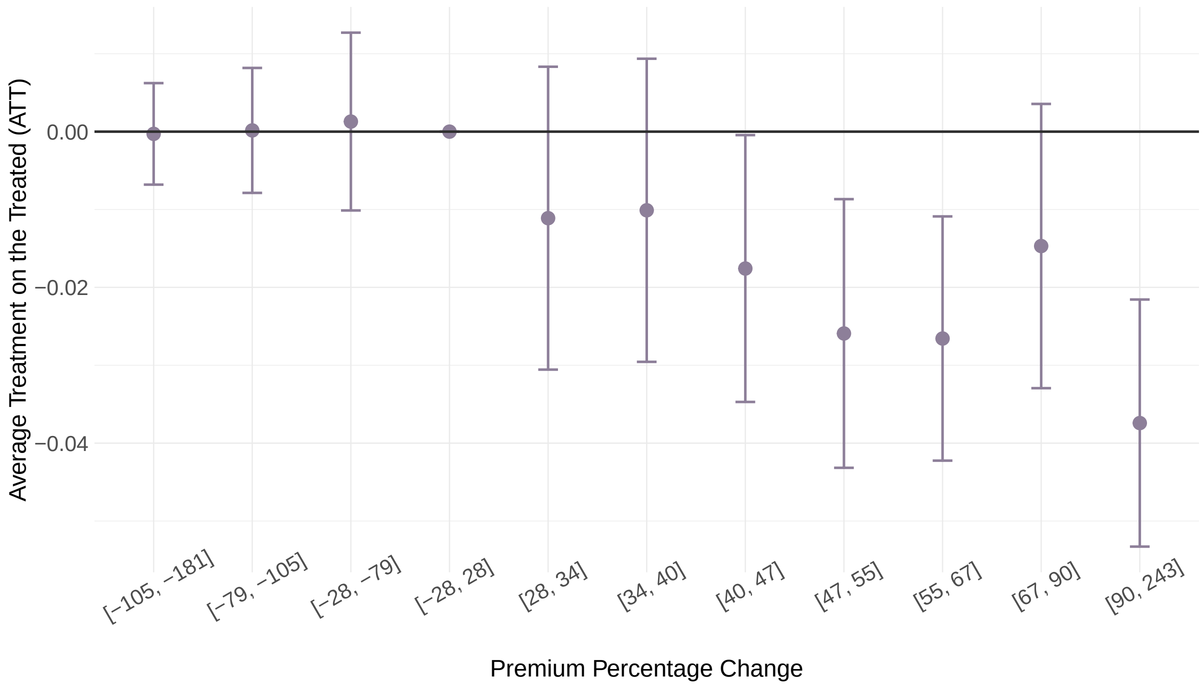


(a) North Carolina

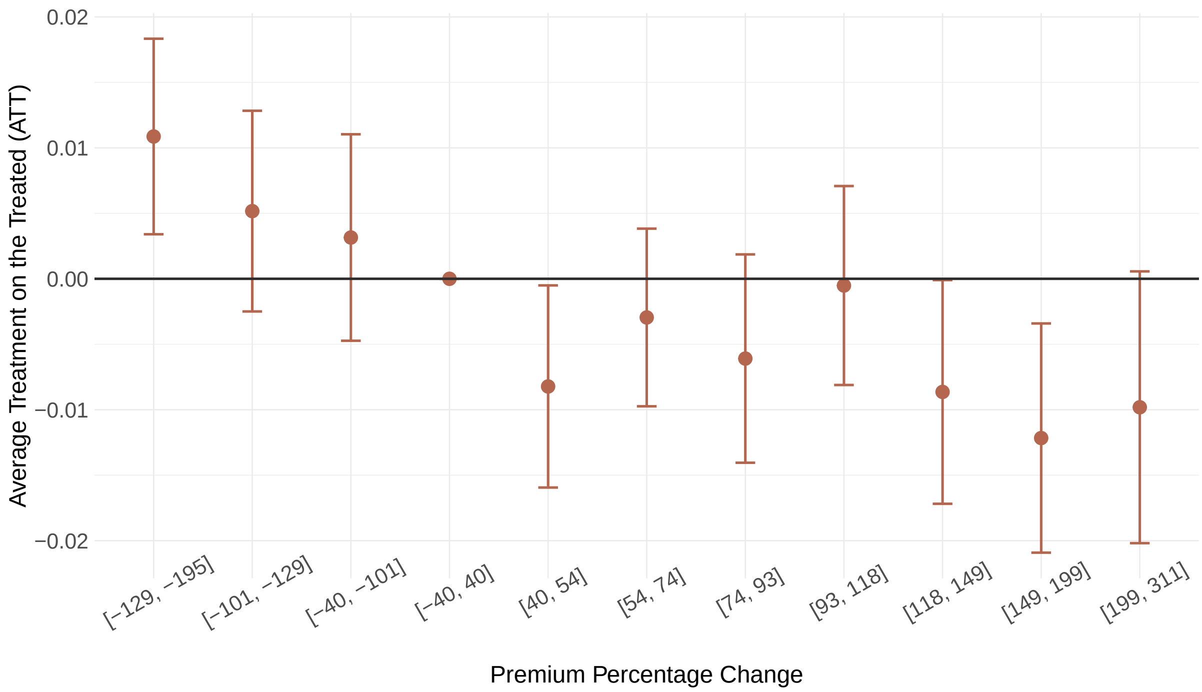


(b) Florida

Figure 7: Heterogeneity Across the Premium Change Distribution (Discrete)

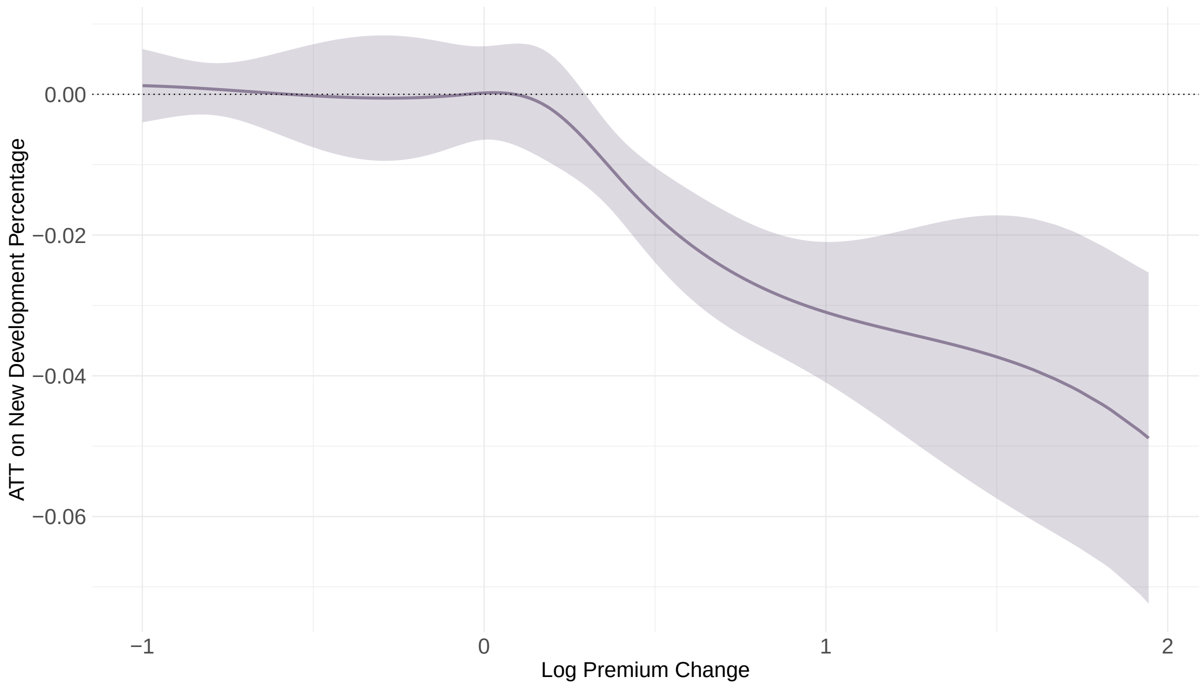


(a) North Carolina

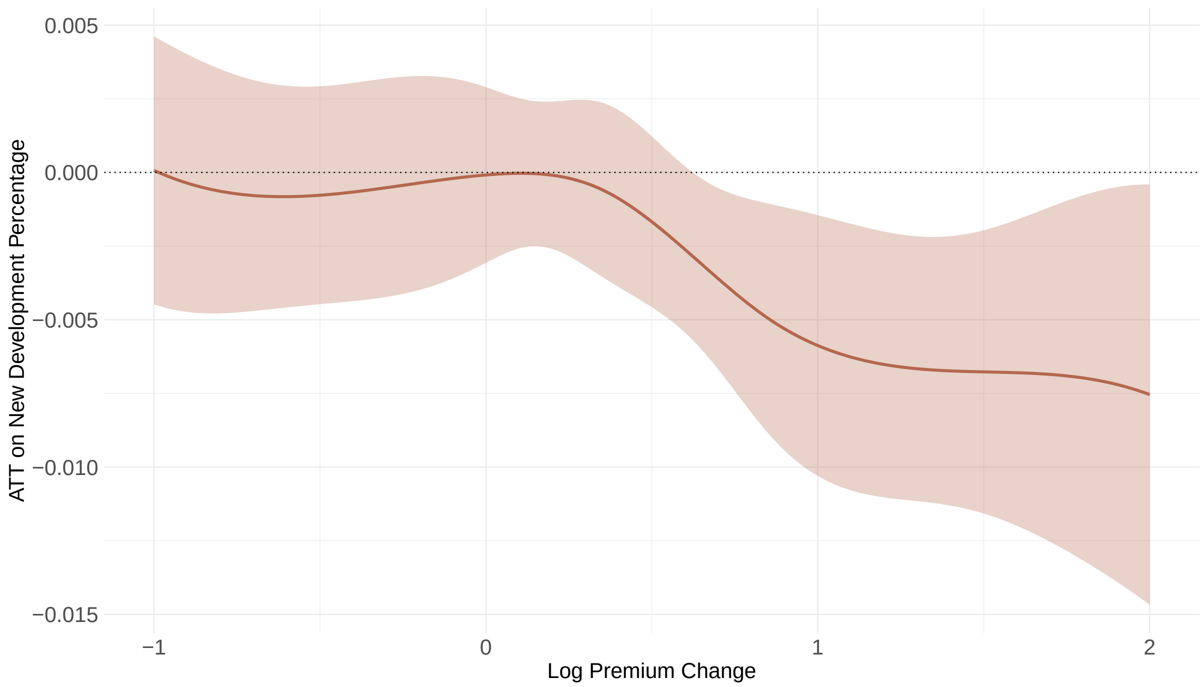


(b) Florida

Figure 8: Heterogeneity Across the Premium Change Distribution (Continuous)



(a) North Carolina



(b) Florida

12 Table List

Table 1: Summary Statistics

	North Carolina	Florida
	(1)	(2)
Statewide		
Number of Parcels	824,589	1,079,267
Number of Block Group-Floodplains	4,562	10,321
Premium Change ($)	5	666
Premium Change (Log Diff)	0.114	0.425
New Development	250,332	231,698
Floodplain Block Groups	12	872
Non-Floodplain Block Groups	2,390	4,391
Split Block Groups	2,160	5,058
Block Group - Floodplain Averages		
New Development Share	0.05	0.057
Parcels	181	105
Housing Units	752	856
Population	1,650	1,829
County Averages		
Total Area (km^2)	1,336,385	1,900,734
Floodplain Area	0.117	0.381
Non-Floodplain Area	0.883	0.619
Total NFIP Policies	1,032	9,173
Net NFIP Claims ($)	419,925	1,099,449
Hurricane History	0.298	0.398

Table 2: Main Results - Binary and Discrete Treatments

	North Carolina		Florida	
	(1)	(2)	(3)	(4)
Premium Change X Post-Period	-0.01488***		-0.00457***	
	(0.00386)		(0.00171)	
Increase X Post-Period		-0.01970***		-0.00677***
		(0.00500)		(0.00194)
Decrease X Post-Period		0.00048		0.00601**
		(0.00353)		(0.00265)
Pre-Trends F-Test	0.273	0.632	0.432	0.018
Dep. Var Means (Control)	0.055	0.055	0.047	0.047
N	36,496	36,496	82,568	82,568
R^2	0.401	0.401	0.345	0.346

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors clustered at the block group level.

Table 3: Main Results - Discrete and Continuous Treatments

	North Carolina		Florida	
	(1)	(2)	(3)	(4)
Big Premium Increase X Post-Period	-0.02683***		-0.00758***	
	(0.00559)		(0.00260)	
Small Premium Increase X Post-Period	-0.01247**		-0.00597***	
	(0.00623)		(0.00232)	
Big Premium Decrease X Post-Period	0.00143		0.00989***	
	(0.00348)		(0.00327)	
Small Premium Decrease X Post-Period	-0.00079		0.00216	
	(0.00462)		(0.00340)	
Continuous Premium Change X Post-Period		-0.01784***		-0.00535***
		(0.00407)		(0.00128)
Pre-Trends F-Test	0.22	0.182	0.001	0.359
Mean Treatment	0.114	0.114	0.425	0.425
Scaled Coefficient		-0.002		-0.0023
Dep. Var Means (Control)	0.055	0.055	0.047	0.047
N	36,496	36,496	82,568	82,568
R^2	0.401	0.4	0.346	0.346

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors clustered at the block group level.

Table 4: Main Results - Florida Alternative Outcomes

	Sales Price (Log)	Number Residential Units	Total Living Area
	(1)	(2)	(3)
Premium Increase X Post-Period	-0.13711***	0.50414***	788.08295***
	(0.03480)	(0.18193)	(232.18464)
Premium Decrease X Post-Period	0.10819	-0.23371	-164.78847
	(0.07424)	(0.16681)	(187.01172)
Dep. Var Means (Control)	11.094	1.693	2,992
Dep. Var Means (Control - No Logs)	400,870		
N	73,357	82,568	82,568
R^2	0.426	0.689	0.644

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors clustered at the block group level.

Table 5: Robustness Table - North Carolina

	Weather	Weather+Parcel Char.	Fixed Premium	Linear Trend	Mean Reversion
	(1)	(2)	(3)	(4)	(5)
Premium Change X Post-Period	-0.01182***	-0.01573***	-0.01806***	-0.00322	-0.01036***
	(0.00394)	(0.00410)	(0.00394)	(0.00343)	(0.00389)
Pre-Trends F-Test	0.249	0.383	0.009		0.356
Dep. Var Means (Control)	0.055	0.055	0.055	0.055	0.055
N	36,496	36,440	36,496	36,496	36,496
R^2	0.401	0.423	0.402	0.401	0.439
	CRS Premium	CRS + Map Updates	CRS Prem + Updates	Alt. New Dev	Alt. Treat
	(6)	(7)	(8)	(9)	(10)
Premium Change X Post-Period	-0.01172***	-0.01087***	-0.01087***	-0.00270	-0.00940**
	(0.00394)	(0.00384)	(0.00384)	(0.00175)	(0.00418)
Pre-Trends F-Test	0.273	0.31	0.31	0.619	0.03
Dep. Var Means (Control)	0.055	0.055	0.055	0.031	0.055
N	36,496	36,496	36,496	36,496	36,496
R^2	0.4	0.408	0.408	0.261	0.4

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors clustered at the block group level.

Table 6: Robustness Table - Florida

	Weather (1)	Weather+Parcel Char. (2)	Fixed Premium (3)	Linear Trend (4)	Mean Reversion (5)
Premium Change X Post-Period	-0.00639***	-0.00369*	-0.00446**	-0.00664**	-0.00420**
	(0.00205)	(0.00220)	(0.00206)	(0.00263)	(0.00199)
Pre-Trends F-Test	0.447	0.272	0.114		0
Dep. Var Means (Control)	0.047	0.047	0.047	0.047	0.047
N	82,568	80,784	82,568	82,568	82,568
R^2	0.345	0.357	0.345	0.345	0.4
	CRS Premium (6)	CRS + Map Updates (7)	CRS Prem + Updates (8)	Alt. New Dev (9)	Alt. Treat (10)
Premium Change X Post-Period	-0.00650***	-0.00611***	-0.00611***	-0.00203*	-0.00614***
	(0.00206)	(0.00204)	(0.00204)	(0.00113)	(0.00204)
Pre-Trends F-Test	0.432	0.475	0.475	0.498	0.231
Dep. Var Means (Control)	0.047	0.047	0.047	0.032	0.047
N	82,568	82,568	82,568	82,568	82,568
R^2	0.345	0.346	0.346	0.107	0.345

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors clustered at the block group level.

A Appendix

A.a Figures

Figure A.1: Risk Rating 1.0 Rate Table Example

RATE TABLE 3B. REGULAR PROGRAM – POST-FIRM CONSTRUCTION RATES[1,2]

ANNUAL RATES PER $100 OF COVERAGE (Basic/Additional)

FIRM ZONES AE, A1–A30 — BUILDING RATES

ELEVATION OF LOWEST FLOOR ABOVE OR BELOW THE BFE[3,4]	1 FLOOR No Basement/Enclosure/ Crawlspace[5, 6]: 1-4 Family	1 FLOOR No Basement/Enclosure/ Crawlspace[5, 6]: Other Residential, Non-Residential Business, Other Non-Residential[9]	MORE THAN 1 FLOOR No Basement/Enclosure/ Crawlspace[5, 6]: 1-4 Family	MORE THAN 1 FLOOR No Basement/Enclosure/ Crawlspace[5, 6]: Other Residential, Non-Residential Business, Other Non-Residential[9]	MORE THAN 1 FLOOR With Basement/Enclosure/ Crawlspace[5, 6]: 1-4 Family	MORE THAN 1 FLOOR With Basement/Enclosure/ Crawlspace[5, 6]: Other Residential, Non-Residential Business, Other Non-Residential[9]	MANUFACTURED (MOBILE) HOME[7, 8]: Single Family	MANUFACTURED (MOBILE) HOME[7, 8]: Non-Residential Business, Other Non-Residential[9]
+4	.31 / .09	.28 / .13	.27 / .08	.22 / .08	.24 / .08	.20 / .08	.32 / .16	.31 / .29
+3	.35 / .09	.32 / .15	.31 / .08	.25 / .08	.27 / .08	.23 / .09	.37 / .18	.35 / .33
+2	.51 / .11	.46 / .20	.44 / .08	.36 / .08	.32 / .08	.28 / .10	.54 / .24	.50 / .44
+1	.96 / .17	.84 / .31	.80 / .08	.66 / .09	.46 / .08	.36 / .12	1.02 / .40	.95 / .76
0	2.25 / .27	1.92 / .50	1.79 / .08	1.44 / .14	.68 / .08	.58 / .14	2.39 / .71	2.16 / 1.34
−1	5.47 / .36	4.58 / .69	4.40 / .08	3.54 / .15	1.17 / .08	.86 / .17	5.83 / 1.13	5.17 / 2.15
−2[8]	8.07 / .70	6.88 / 1.35	6.53 / .13	5.25 / .26	***	***	8.61 / 2.19	7.87 / 4.14
−3[8]	10.00 / 1.20	8.76 / 2.30	8.32 / .22	6.77 / .47	***	***	10.59 / 3.41	9.89 / 6.43
−4[8]	12.06 / 1.80	10.76 / 3.45	10.26 / .36	8.46 / .77	***	***	12.68 / 4.77	12.00 / 8.97
−5[8]	13.61 / 2.41	12.34 / 4.60	11.79 / .57	9.88 / 1.16	***	***	14.21 / 6.00	13.58 / 11.27
−6[8]	13.96 / 2.96	12.86 / 5.63	12.36 / .84	10.56 / 1.69	***	***	14.51 / 6.84	13.99 / 12.81
−7[8]	14.20 / 3.49	13.34 / 6.53	12.87 / 1.11	11.15 / 2.21	***	***	14.85 / 7.50	14.38 / 14.04
−8[8]	14.26 / 3.99	13.44 / 7.46	13.23 / 1.40	11.59 / 2.75	***	***	14.89 / 8.04	14.43 / 15.06
−9[8]	14.31 / 4.29	13.54 / 8.04	13.27 / 1.68	11.67 / 3.31	***	***	14.93 / 8.25	14.48 / 15.48
−10[8]	14.36 / 4.45	13.64 / 8.36	13.28 / 1.89	11.75 / 3.74	***	***	14.97 / 8.50	14.53 / 15.55

Figure A.2: FIRM Panel Example (Brown University Area)

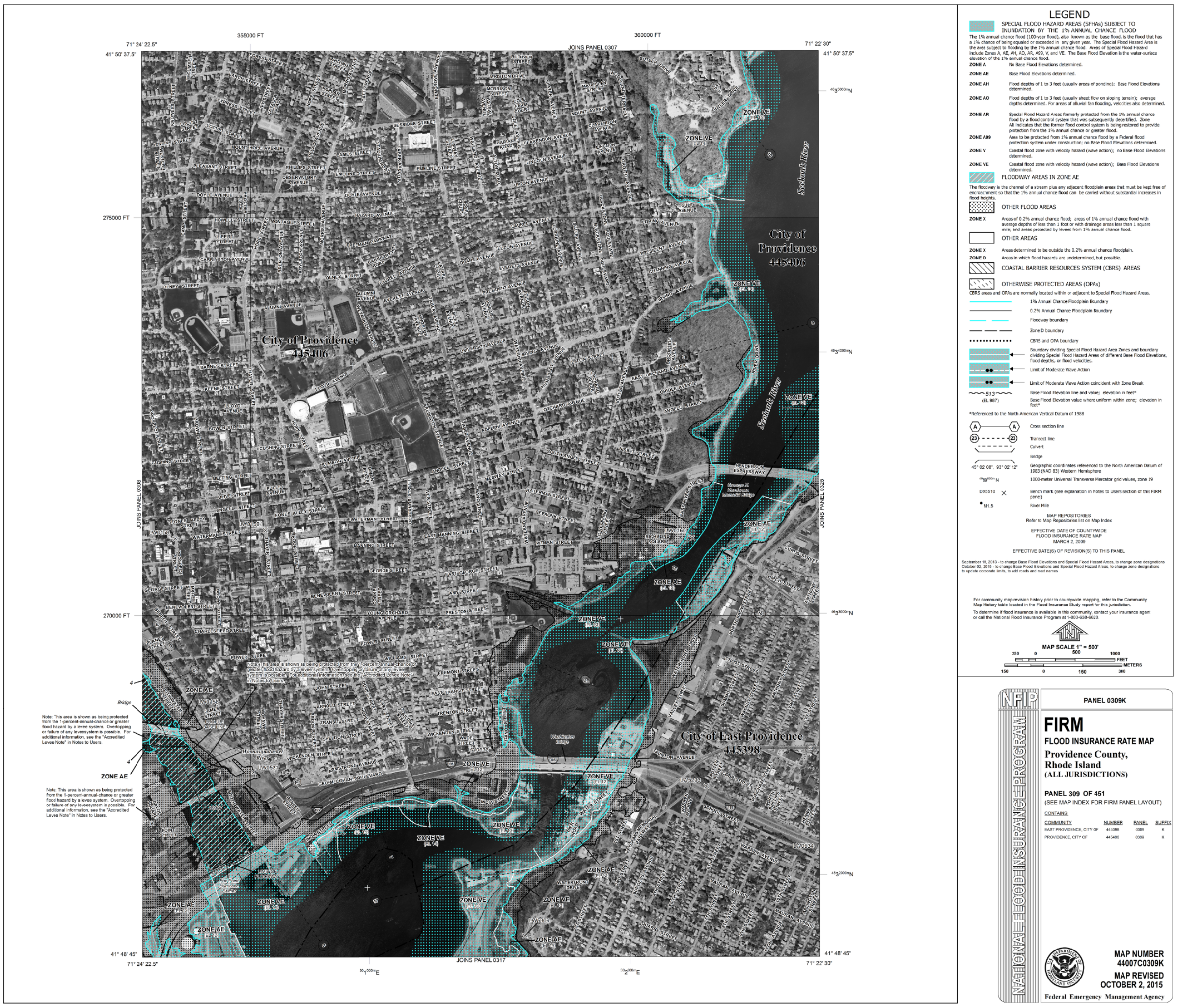


Figure A.3: North Carolina Treatment Map - Increases

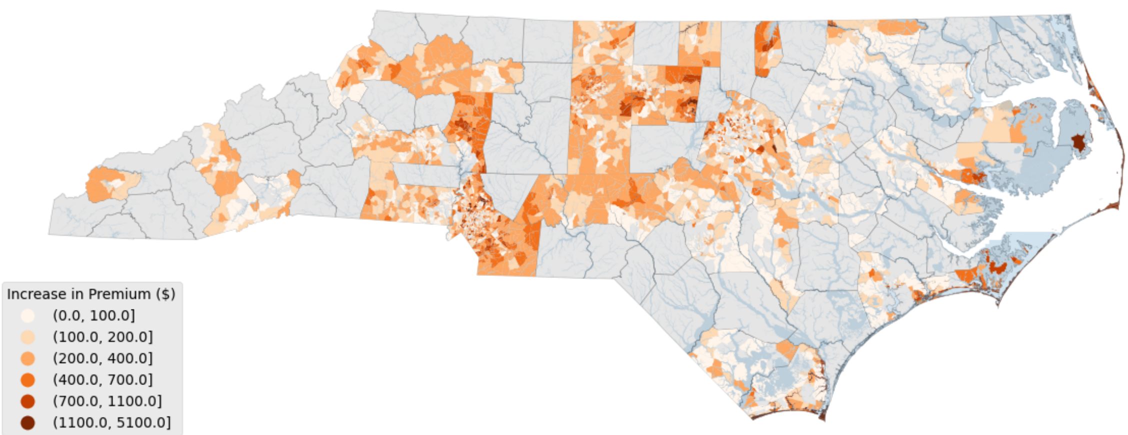


Figure A.4: North Carolina Treatment Map - Decreases

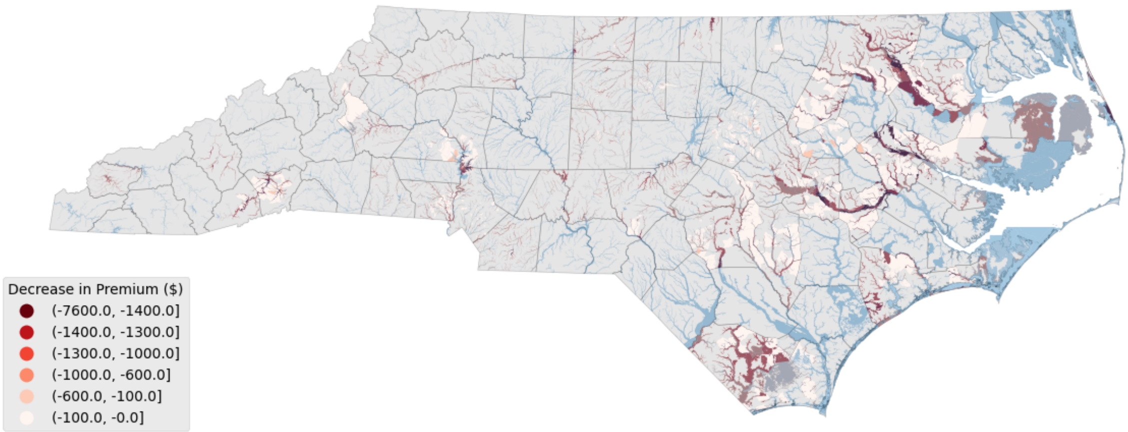


Figure A.5: North Carolina Treatment Map - New Development

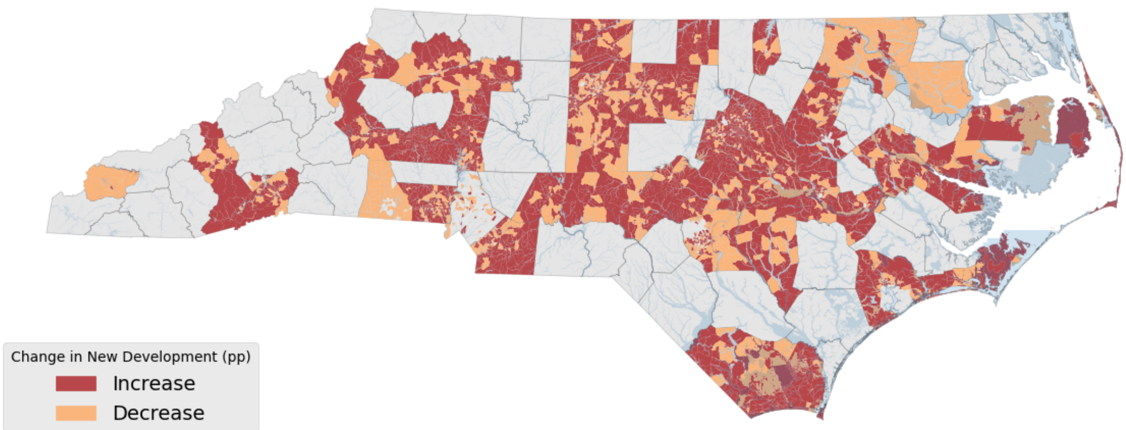


Figure A.6: North Carolina Treatment Map - New Development vs. Premium Changes

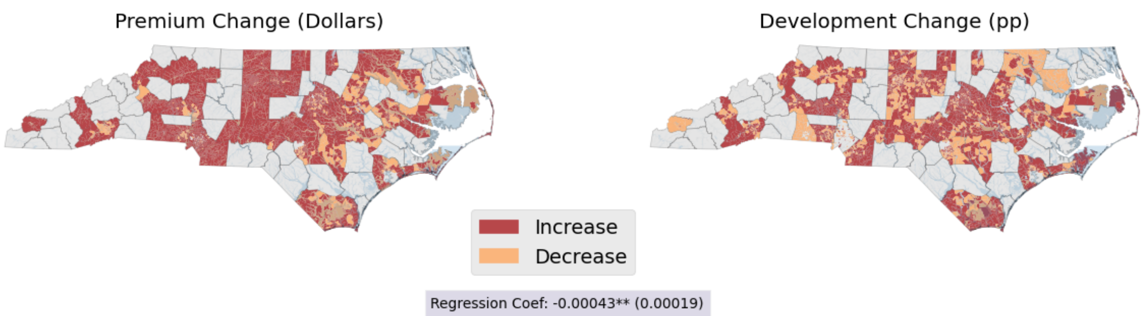


Figure A.7: Florida Treatment Map - Increases

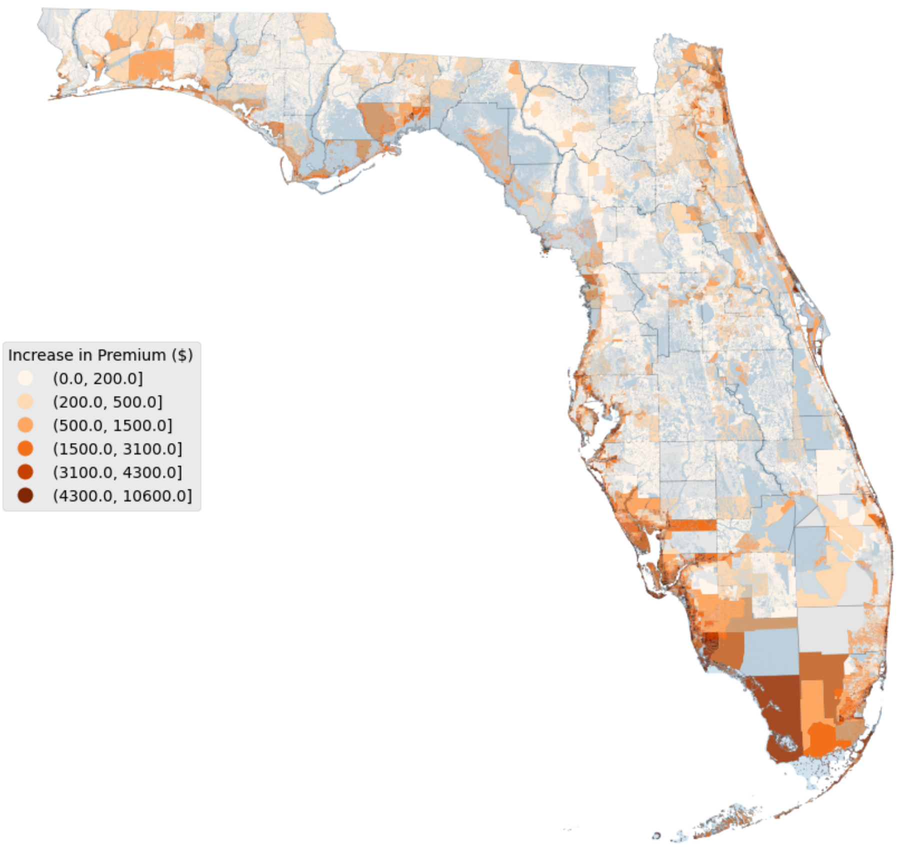


Figure A.8: Florida Treatment Map - Decreases

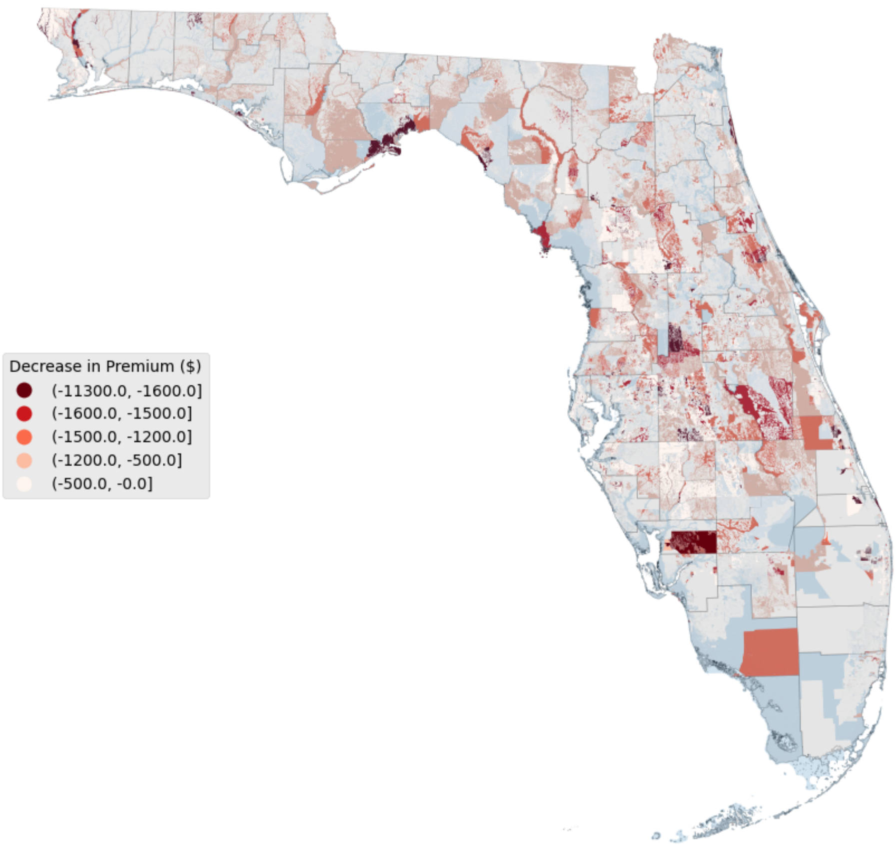


Figure A.9: Florida Treatment Map - New Development

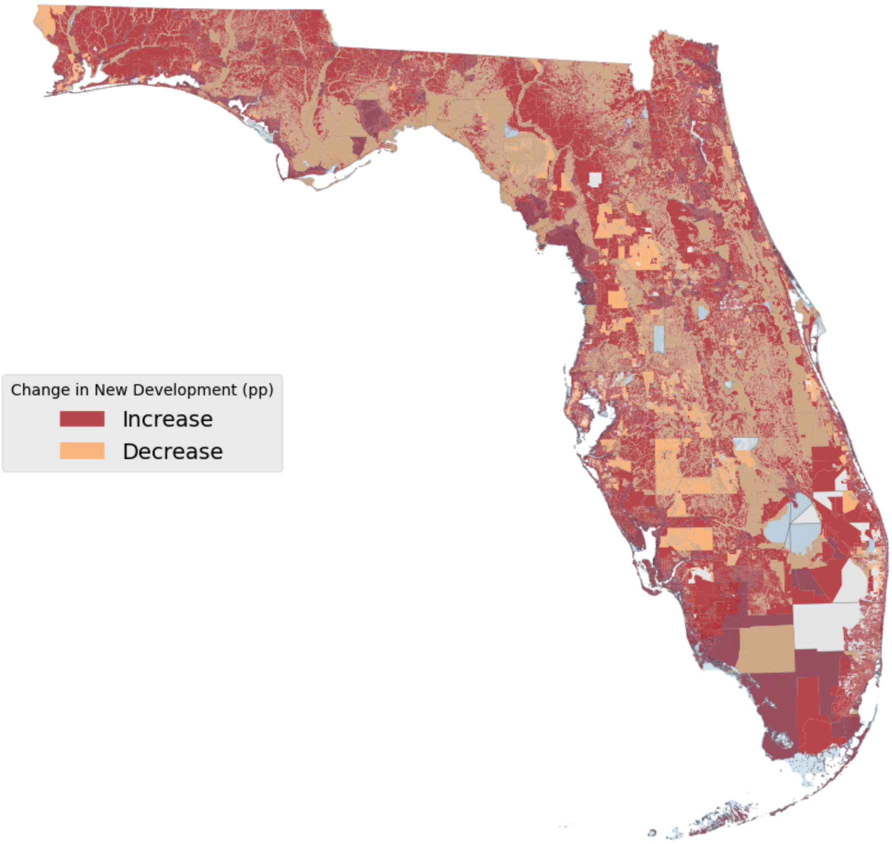


Figure A.10: Florida Treatment Map - New Development vs. Premium Changes

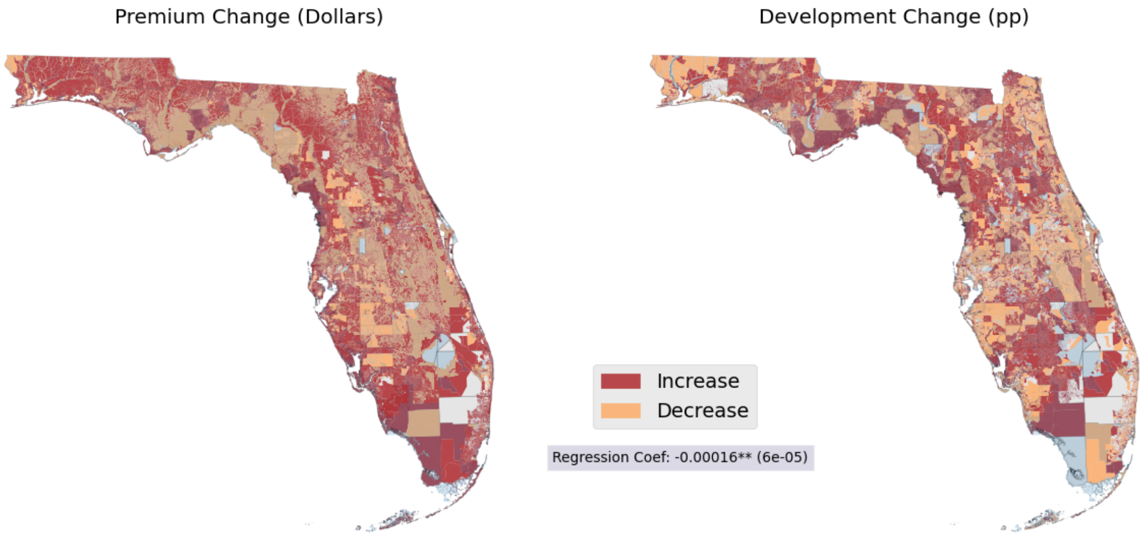


Figure A.11: Housing Trends by State

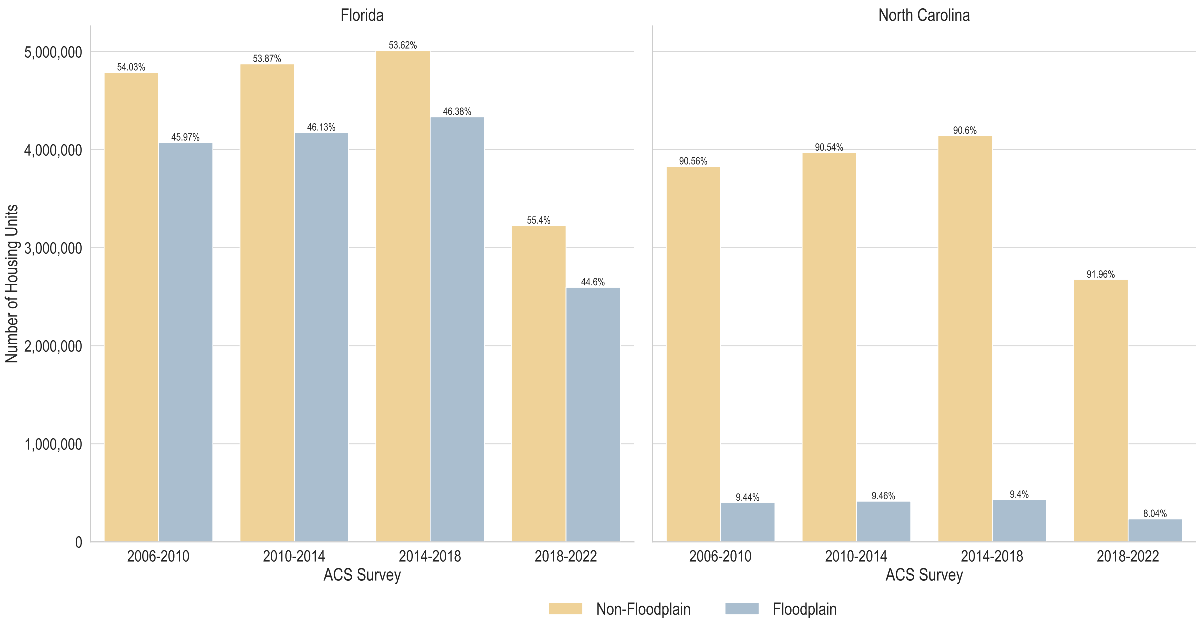
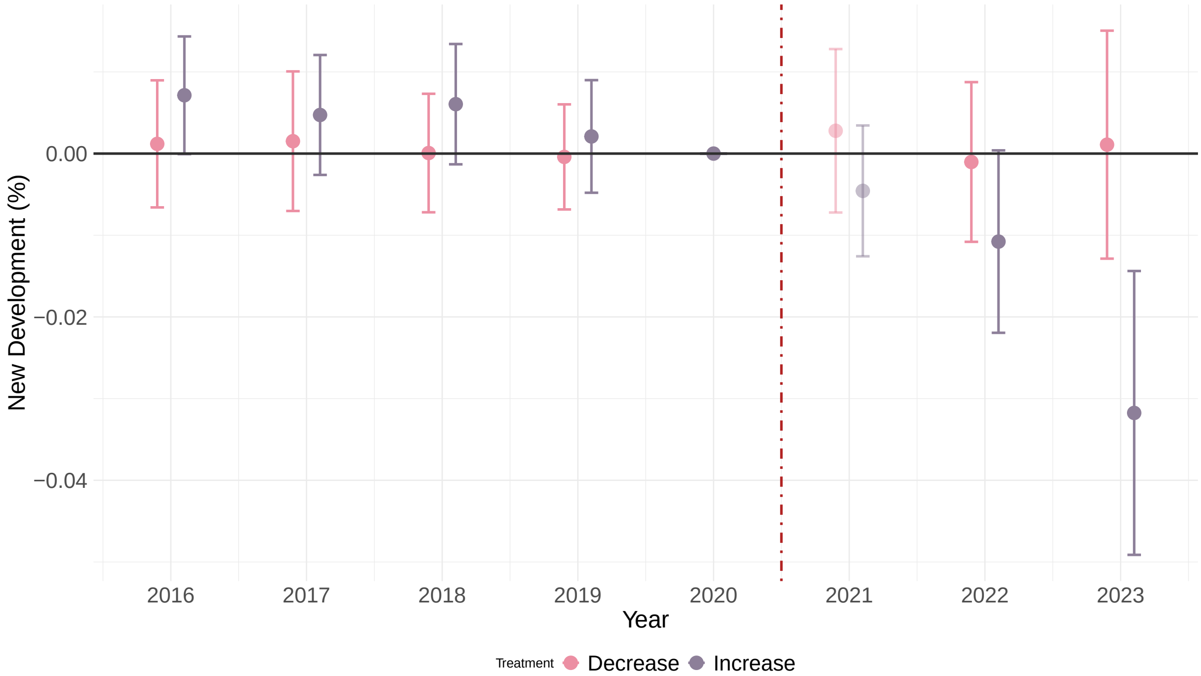
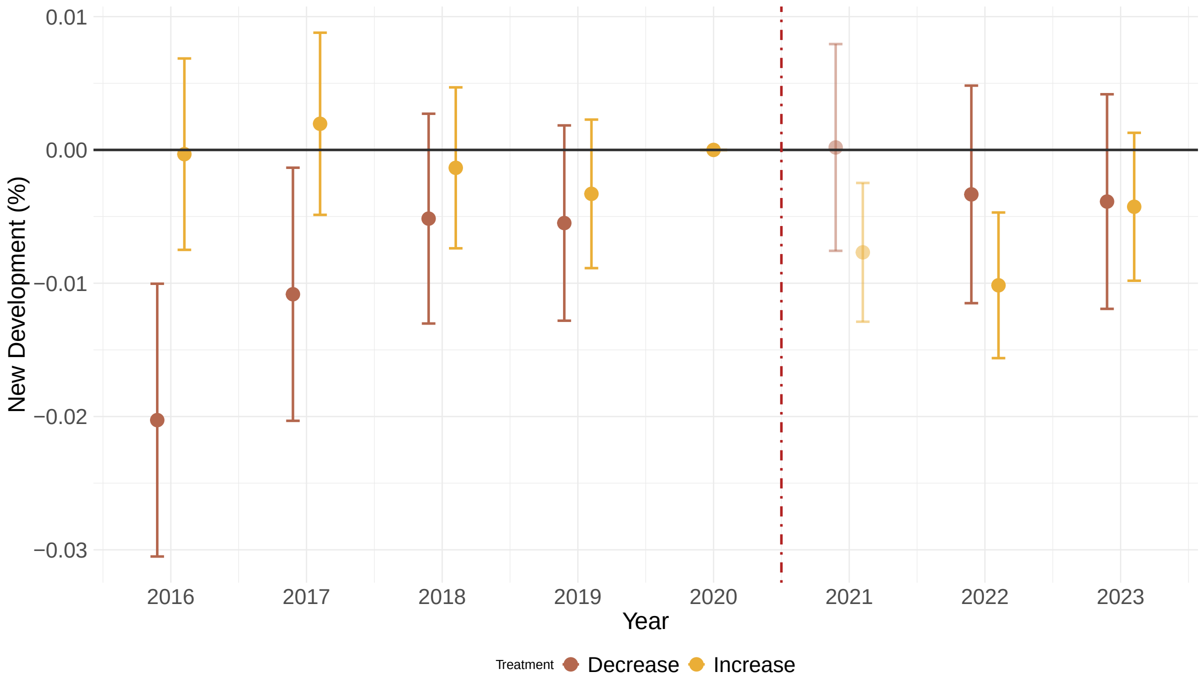


Figure A.12: Event Studies - Increase and Decrease Treatments

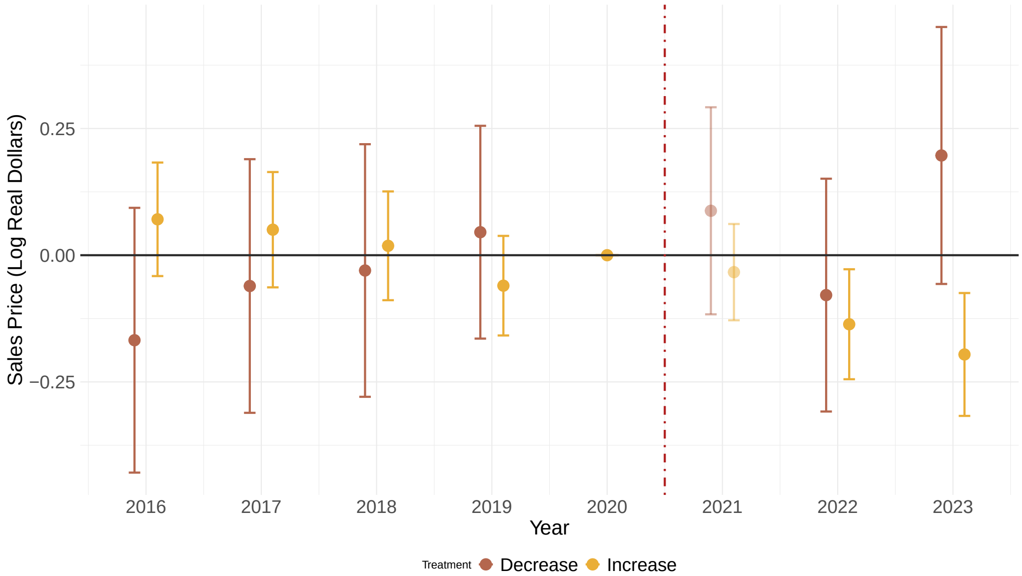


(a) North Carolina

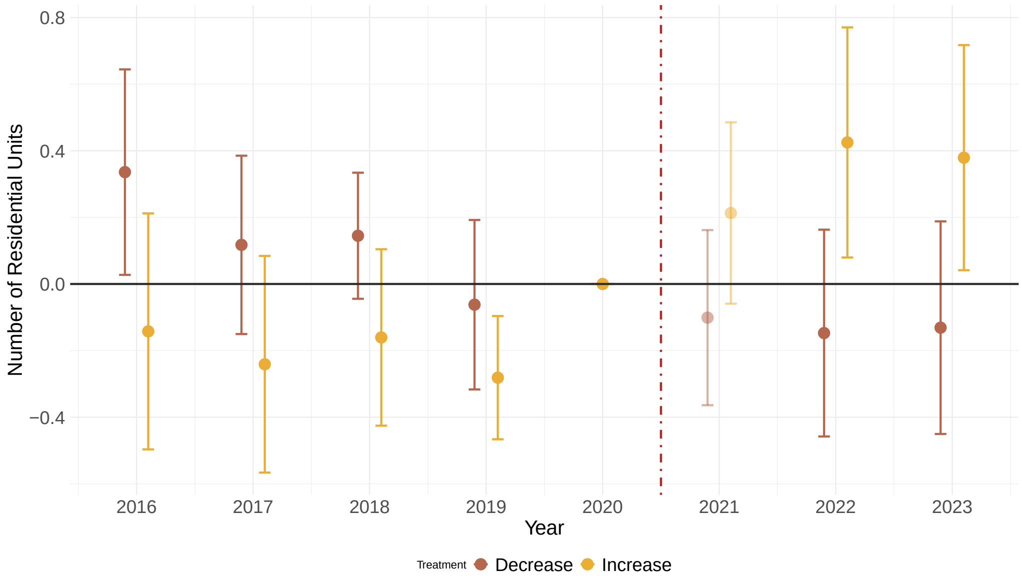


(b) Florida

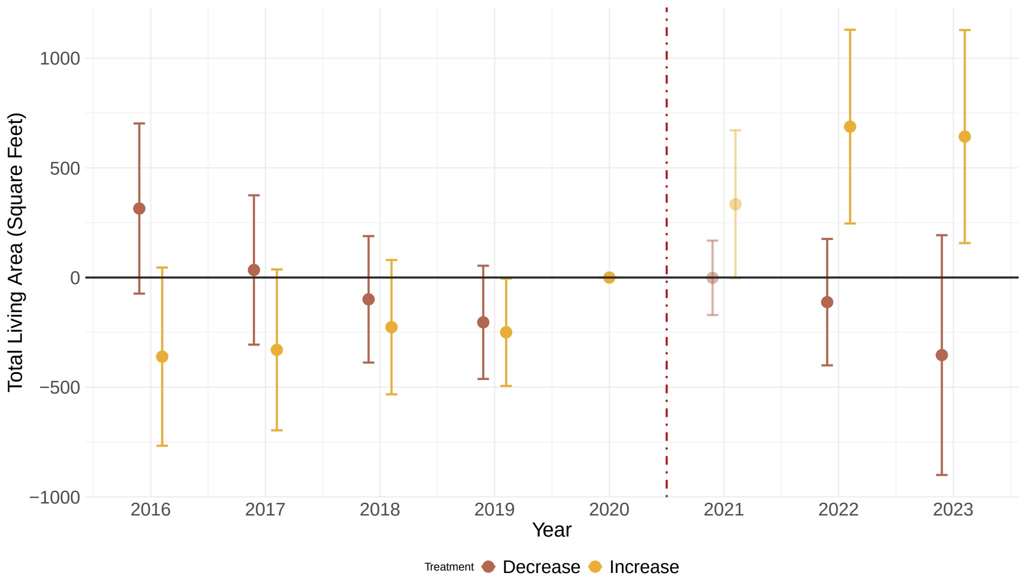
Figure A.13: Event Studies - Florida Outcomes



(a) Sales Prices (Log)

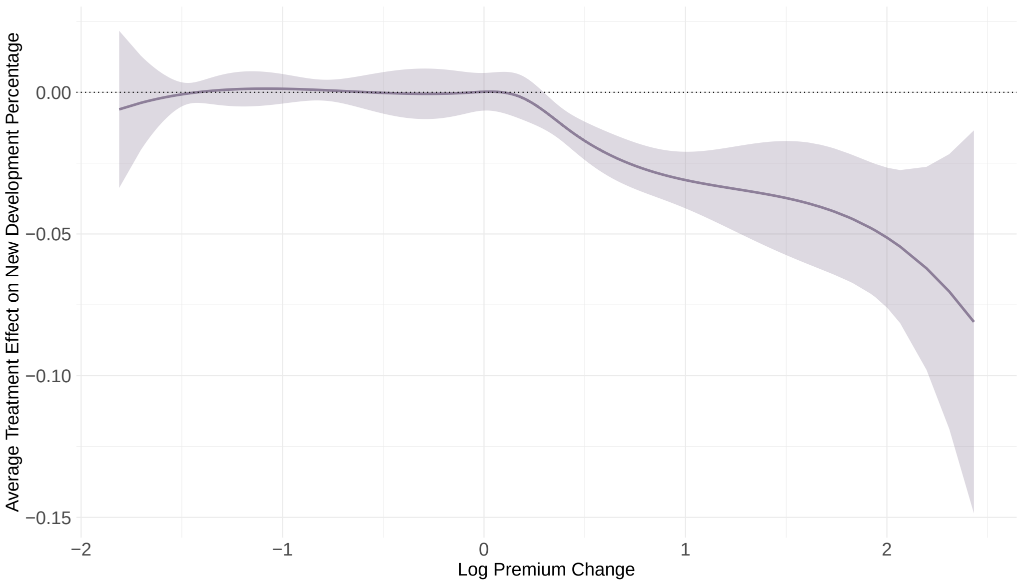


(b) Number of Residential Units

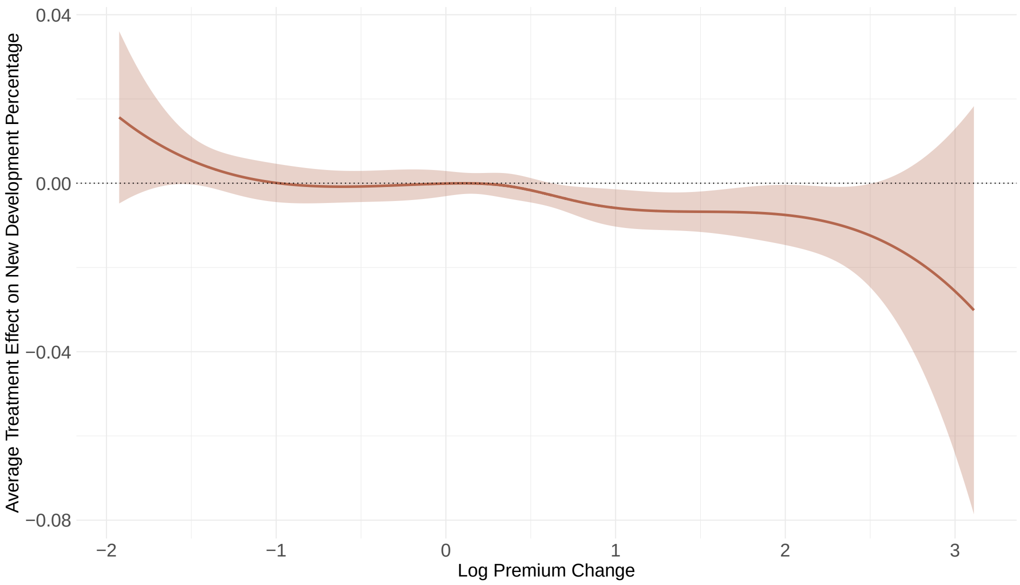


(c) Total Living Area (ft^2)

Figure A.14: Full Distributions of ATT Functions



(a) North Carolina



(b) Florida