# *To Improve Is to Change:* The Effects of Risk Rating 2.0 on Flood Insurance Demand

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**Abstract**

We present a theory of the demand for flood insurance and empirically analyze the effects of the latest NFIP reform (known as *Risk Rating 2.0* or RR2) using individual insurance histories for the universe of all policies. The reform increased exit, both in the flood zone and its periphery, particularly among policyholders paying the highest premiums. The reform also reduced new entry, had highly heterogeneous effects on insurance costs and triggered adjustments in deductibles, generally shifting flood risk toward policyholders in exchange for lower premiums. While the reform may have improved the solvency of the NFIP, it has exacerbated the decades-long decline in enrollment.

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To improve is to change; to be perfect is to change often.

—*Winston Churchill*

# 1 Introduction

Severe climate events affect an increasingly large number of households in the United States and elsewhere. A growing body of evidence shows that insurance can play a key role in managing households' financial exposure to climate risk and speed up recovery, particularly when facing infrequent but severe climate events (Billings et al., 2022; del Valle et al., 2024; Collier and Kousky, Forthcoming), such as storm-related flooding.

Unlike for other climate hazards, the U.S. federal government provides flood insurance through the *National Flood Insurance Program* (NFIP). Generally speaking, the NFIP seeks three objectives: to reduce current exposure to flood risk by maximizing insurance take-up in flood-prone areas, to provide accurate estimates of flood risk that incentivize adaptation and lower future losses, and to be financially self-sustaining.

With almost 5 million policyholders, the NFIP has succeeded in attaining fairly high take-up rates in many flood-prone areas around the country, partly by offering highly subsidized insurance for most of its existence. However, critics of the program argue that it is also responsible for contributing to excessive agglomeration in high-risk areas due to the subsidies, the reliance on outdated flood risk estimates, and a coarse risk-pricing schedule. Moreover, the NFIP currently owes over $20 billion despite having received several multi-billion bailouts over the last 2 decades, and will soon require congressional reauthorization.

Largely to address these problems, FEMA rolled out a profound overhaul of the NFIP in 2021-2022, known as *Risk Rating 2.0* (RR2, for short). Essentially, this reform modernizes the estimation of property-level flood risk (based on a wide range of individual property characteristics) and implements a new pricing methodology that tailors premiums to each property's flood risk. The expectation is that the transition toward actuarially fair premiums will shore up the NFIP's finances. What is less clear is what will be the impact of the reform on enrollment in the program. The main goal of our paper is to estimate the effects of RR2 on entry, exit, and overall enrollment in the NFIP. Importantly, unlike car or health insurance, flood insurance remains primarily a voluntary decision and, therefore, the effects of the reform on premiums and affordability will play a key role in determining if the reform has helped the NFIP lower flood risk exposure on the floodplain.

Besides some recent evidence on the effects of the RR2 reform on average premiums (Mulder and Kousky (2023)), we are unaware of a comprehensive empirical analysis of the effects of the new pricing system on insurance take-up. Our paper presents a new theory of the demand for flood insurance where risk-averse homeowners (who differ by income and flood risk exposure) choose whether to buy insurance, and derives predictions for the effects of the reform. Our theory of insurance demand shares similarities with Wagner (2022), but it allows for general insurance pricing functions (rather than a single price for all homeowners) in order to analyze the effects of various reforms on premiums and take-up. Specifically, our setup allows for individual heterogeneity in flood risk beliefs (as documented by Johnston and Moeltner (2019) and Bakkensen and Barrage (2022)) and for reforms that flexibly alter the premiums charged to homes with different (objective) flood risk. We also emphasize the role of affordability as a key reason accounting for low insurance take-up, and provide extensions of the model with entry-exit flows and subjective risk beliefs (Bakkensen and Barrage, 2022; Wagner, 2022). Turning to the data, we build individual insurance histories for the universe of NFIP policies for the period 2019-2023, which allows us to distinguish new entry from policy renewals, and estimate the effects of RR2 along the extensive and intensive margins of the demand for insurance, including whether policyholders modified policy parameters (such as coverage and deductibles) in order to mitigate the effects of the reform on annual premiums.

According to our theory, the allocations of insurance purchases are symmetric in the 100-year flood zone (also known as *Special Flood Hazard Zone* or SFHA) and in its periphery, which overlaps with the 500-year flood zone. Under the old pricing function, purchase allocations are characterized by a threshold level of flood risk in each of the two risk zones. Homeowners with risk above the threshold buy insurance, provided they can afford the premiums. The resulting allocation entails *implicit* subsidies for some homeowners (paying premiums below the expected annual loss), *implicit* taxes for others, and a general pattern of adverse selection (Bradt et al., 2021). In the model, when RR2 is adopted, every homeowner can purchase insurance at actuarially fair prices, which eliminates the implicit taxes and subsidies. As a result, the model predicts that homeowners who can afford the premiums will purchase insurance, likely mitigating the correlation between flood risk and insured status. In the model, homeowners experience widely heterogeneous changes in the price of insurance, and this is expected to trigger entry (primarily among homeowners with relatively low flood risk who were previously unwilling to purchase insurance) and exit (largely among relatively high-risk homeowners who previously enjoyed an implicit subsidy), but the overall effect on program enrollment is theoretically unclear and becomes an empirical question. It is worth noting that, in the absence of affordability (income) constraints, our model would be unable to generate exit when RR2 is adopted (due to featuring risk-averse homeowners facing actuarially fair insurance prices).

Because the RR2 reform affected virtually everyone's premiums, there is no natural control group. As a result, our main research design to identify the effects of the reform is an event study coupled with pre-treatment trend projections. Importantly, we allow for differential effects in and out of the SFHA, as well as for policyholders with subsidized (pre-FIRM) rates who had been experiencing higher annual price increases due to previous reforms.[1] We estimate that the reform increased exit by almost 1 percentage point among SFHA policyholders paying full-risk rates and by over 2 percentage points in its periphery, relative to a no-reform counterfactual that accounts for pre-reform trends. However, the largest effect of RR2 on exit was felt by subsidized SFHA policyholders, whose probability of renewal fell by over 6 percentage points. Combining all three market segments, the reform pushed 53,000 homeowners out of the insurance market in the first year of implementation, growing to 83,000 exits if we also count the projected exit based on the pre-reform trend. Importantly, these findings are robust to an alternative identification strategy based on the regression discontinuity in time methodology (Hausman and Rapson, 2018).

We also document that homeowners choosing to discontinue flood insurance were paying substantially higher premiums (in the previous year) relative to policyholders in the same risk zone who chose to renew their policies. Bearing in mind that risk-averse or risk-neutral homeowners should be willing to purchase insurance at actuarially fair prices, our model rationalizes the pattern of *selective exit* as reflecting some policyholders' inability to afford higher premiums. Naturally, we do not observe the post-reform premiums that led policyholders to exit the insurance market. Further insights can be obtained by examining the changes in the cross-sectional price distribution for policies purchased in the years immediately before and after the implementation of RR2. Our second main result is that RR2 had highly heterogeneous effects on insurance prices at the individual level, with many policyholders experiencing premium reductions while others were hit by large increases. On average, the reform appears to have increased average insurance costs among periphery residents and, more intensely, among SFHA residents with subsidized (pre-FIRM) rates. This pattern is consistent with the larger increase in exit rates estimated for these market seg-

[1] The subsidies were applied to policies covering houses (in the flood zone) that were built prior to the release of flood maps in a community, commonly referred to as pre-FIRM constructions. Recognizing that subsidized (pre-FIRM) policies accounted for a disproportionate share of claims (Kousky and Michel-Kerjan, 2017), Congress enacted NFIP reforms in 2012 and 2014. The 2012 Biggert-Waters Act (BWA) introduced an ambitious schedule to eliminate subsidized rates by raising premiums by 20-25% annually until reaching full-risk rates. The resulting increases proved unaffordable for many homeowners and the 2014 Homeowners Flood Insurance Affordability Act (HFIAA) delayed the start of the premium increases (by 4 years) and capped them at 18% for most policies, while also introducing an annual surcharge to all policyholders. At the adoption of RR2, the full convergence of subsidized policies to full-risk rates was not yet completed, resulting in differential trends that can confound the estimation of the effects of RR2 in and out of the flood zone.

ments. Thus, our estimates underscore that renewal-exit decisions are highly responsive to changes in premiums. Naturally, these changes in insurance costs may capitalize into home prices accordingly (Georgic and Klaiber, 2022).

Third, when restricting the analysis to homeowners who continuously purchased insurance before *and after* the reform, we find that RR2 increased the average coverage-adjusted price in the periphery and among SFHA residents with full-risk rates by about 2% relative to the pre-reform trend. In stark contrast, the average *renewer* with (previously) subsidized premiums experienced a 33% *decline* in the price of flood insurance.[2] Thus, while some pre-FIRM policyholders were pushed out of the market due to large price increases, the reform was a boon to others who enjoyed large reductions. This finding, based on the universe of policies nationwide, provides confirmation for the survey evidence (for a single city) in Sherman and Kousky (2018).

Fourth, we also find evidence of adjustments along the intensive margin. Specifically, policyholders experiencing price increases mitigated the impact on their premiums by sharply raising deductibles (and slightly adjusting coverage). Our estimates indicate that RR2 led to a 12% average increase in the deductible chosen by renewing households outside of the flood zone. Flood-zone policyholders with full-risk rates, who also saw their premiums increase, also adjusted their deductible upward (by an average of 5%). In contrast, renewers with subsidized rates actually lowered deductibles by about 2%, consistent with the average price reduction they received. Pooling the three groups, RR2 led to an 8% increase in the deductible of the average policyholder (and a negligible effect on coverage). As a result, the reform had the unintended consequence of increasing the financial exposure to flood risk of *insured* homeowners, possibly increasing their incentives to take individual risk-mitigation actions.[3]

We also find that RR2 reduced entry into the insurance market, although the effects of the reform on entry had opposite signs in the periphery and SFHA. Relative to the trend-based counterfactual, the reform lowered entry by 26% in the periphery (where insurance is

[2] In 2020 roughly 15% of NFIP policies insured houses built before the release of flood maps, which were almost exclusively located in the 100-year flood zone. These (pre-FIRM) policies were priced favorably, with rates that were, on average, lower than those applied to houses built after the flood maps. However, *high-elevation*, pre-FIRM houses were believed to be paying premiums well *above* their expected loss. As we shall show, some pre-FIRM policies experienced large premium increases (and led to exit) while others enjoyed large discounts (and were renewed).

[3] The shift in risk sharing toward homeowners could be the financial counterpart of the simulation-based analysis in de Ruig et al. (2022), which shows that RR2 could incentivize risk-reduction investments. Their analysis does not use data on actual insurance purchases, relies on strong parametric assumptions on the geographic distribution of income, and assumes that households' choices of coverage and deductibles are fixed. Thus, our empirical findings will also help inform future simulation-based analysis of the long-term effects of the reform.

largely voluntary and the average premium increased) and increased it by 17% in the flood zone. In aggregate, though, the reform reduced annual entry flows by about 29,000 policies. Combining the effects on entry and exit, we conclude that the reform increased *net exit* from the insurance market by about 84,000 policyholders (or 2.3% of the baseline enrollment) during the first year of implementation. Additionally, our finding of selective exit (among high-price policyholders) suggests a reduction in annual program revenue, since the average probability of exit increased sharply with lagged premiums.

Our paper is closely related to Wagner (2022), which analyzes the effects of the 2012-2014 NFIP reforms on flood insurance demand within the 100-year flood zone and provides instrumental-variables estimates of the price-elasticity of demand and tests for adverse selection. The paper also documents that take-up is below 60 percent even though, on average, premiums are substantially lower than expected losses, and argues that this anomaly is likely due to widespread underestimation of actual flood risk by flood-zone residents (as in Bakkensen and Barrage (2022)). Our paper also builds on Mulder and Kousky (2023), who provided initial evidence of how the adoption of RR2 led to changes in average premiums, and documented that the new pricing schedule lowered average premiums in the flood zone but increased them elsewhere. Relative to these studies, our focus is on the empirical estimation of the effects of the reform on exit-entry dynamics in the flood insurance market and on policy parameters (such as coverage and deductibles), interpreted through the lens of a model that emphasizes the role of affordability in accounting for the effects of the reform on insurance take-up.

More generally, our paper contributes to the literature describing the various determinants of flood insurance demand, which is almost exclusively based on the old NFIP pricing methodology. Existing studies indicate that take-up is higher in areas with higher risk (Bradt et al., 2021; Kousky, 2011), for individuals with higher risk aversion (Petrolia et al., 2013), with higher incomes (Atreya et al., 2013; Netusil et al., 2021; Shao et al., 2017), and with more expensive residences (Brody et al., 2018). The impact of the mandatory purchase requirement for mortgaged properties in the floodplain is still subject to debate since some studies find that it increases take-up (Kriesel and Landry, 2004) whereas others fail to find an effect (Kousky, 2011; Landry and Jahan-Parvar, 2011). Compared to this literature, our analysis leverages individual insurance histories and emphasizes the study of renewal-exit decisions, which account for the bulk of annual policy purchases and are better identified empirically.

Our paper also adds to the understanding of the intensive margin of flood insurance demand. The work focusing on the choice of total coverage shows that it increases with policy subsidies (Landry and Jahan-Parvar, 2011) and objective measures of risk (Kousky, 2011). In the case of deductible choice, Dombrowski et al. (2020) argue that deductibles are sticky and are seldom adjusted, while Kousky (2011) compares policies across the flood zone boundary finding that deductibles decrease with risk. Petkov (2022) provides evidence that policyholders choose higher deductibles when they believe that disasters are unlikely. Our results indicate that deductibles are an important margin of adjustment when policyholders experience insurance price increases and imply that *insured* homeowners may end up effectively bearing more flood risk. It is also worth noting that increases in flood insurance costs may crowd out demand for *home* insurance. Thus, our findings are also relevant for analyses of the effect of climate risk on private insurance companies (Acharya et al., 2023; Jung et al., 2023).

Last, our paper also contributes to the literature that examines how direct disaster experience, or close proximity to losses, affects flood risk beliefs, which in turn affect flood insurance take-up (Gallagher, 2014; Kousky, 2017; Petkov and Ortega, 2023), housing values (Ortega and Taspinar, 2018), business establishment locations (Indaco et al., 2021), human capital investments (Gallagher et al., 2023), and the approval rates of mortgages that can be securitized (Ouazad and Kahn, 2021). This literature complements analyses of the post-disaster distribution of relief aid and its effects on residential choices (Billings et al., 2022; Pang and Sun, 2024).

The structure of our paper is as follows. Section 2 presents our theoretical model. Section 3 presents our data sources and describes the construction of individual insurance histories. Section 4 provides a descriptive analysis of the cross-sectional distributions of insurance prices before and after the RR2 reform. Section 5 presents the main analysis, which focuses on the identification of the effects of the reform on renewal-exit decisions. Section 6 analyzes the effects of the reform on prices and policy attributes for the subset of policyholders who continuously purchased flood insurance before and after RR2 was implemented. Section 7 turns to the estimation of the effects of the reform on entry into the flood insurance market and Section 8 concludes. The appendix contains proofs, extensions to the theoretical analysis, and auxiliary empirical analysis.

## 2 Theory

Next, we build a model for the demand for flood insurance where the schedule mapping flood risk into insurance prices is taken as given, though subject to change at the will of the government. The model focuses on the extensive margin of demand: homeowners either buy full coverage for their potential losses or do not buy any insurance. The main goal of the model is to characterize insurance purchase allocations under various changes to pricing schedules. We also present a number of extensions of our basic setup, where we analyze the effects of various pricing reforms on overall revenue and social welfare (consumer surplus), present a dynamic extension of the model, and consider situations with subjective flood risk beliefs.

## 2.1 Setting

Consider a population of heterogeneous homeowners (individuals) where each type is characterized by a pair $(r, y)$, where $r$ is the individual (objective) flood risk and $y$ is household income.[4] We assume that there exist independent and continuous probability density functions so that $r \sim h(r)$ and $y \sim g(y)$, and all homeowners live in a house of value $V$.

Due to flood risk, homeowner $i$ faces a lottery: with probability $r_i$, her house will flood and will only deliver $V - L$ units of service. Alternatively, with probability $1 - r_i$, she will enjoy the full value of her house, $V$. We assume that homeowners maximize expected utility over consumption bundles $(c_1, c_2)$, where $c_1$ and $c_2$ denote consumption in the no-flooding and flooding states of the world, respectively. We assume a well-behaved utility function $u(x)$ that is continuous, increasing and concave.[5]

Homeowner $i$ can buy insurance at price $0 < p < 1$ per dollar of coverage, which will be allowed to vary as a function of flood risk, and choose between no insurance at all or paying a premium $pL$ to fully insure against the loss associated to flooding. We also assume that homeowners face the following affordability constraint: $pL \leq y_i$. It is convenient to normalize income, the value of the house, and the price of insurance in loss units (L). As a result, the value of the house when flooding occurs and the premium become $V - 1$ and $p$, respectively.

Homeowners live inside the 100-year flood zone (denoted by indicator $z = A$) or in its periphery ($z = X$). Both risk zones differ in terms of (objective) flood risk. In particular, we partition flood risk as follows: $X = (0, \overline{r})$ and $A = [\overline{r}, 1)$. The distributional assumptions made earlier imply that the population of homeowners in each of the zones is given by $1 - H(\overline{r})$ in the flood zone and $H(\overline{r})$ in its periphery.

Our setting is similar to Wagner (2022), but we model both the flood zone and its periphery, and emphasize household heterogeneity in objective flood risk and household income.

[4]For simplicity, we think of income as net of all expenditures in goods and services except for flood insurance.

[5]We allow for linear utility to capture the risk neutrality case. Ocasionally, we specialize to the constant relative risk aversion (CRRA) family of utility functions: $u(c) = c^{1-\rho}/(1-\rho)$, with $\rho \geq 0$.

## 2.2 Utility maximization

Each homeowner compares two consumption bundles: $(V, V-1)$ if no insurance is purchased and $(V-p, V-p)$ if she chooses to buy insurance. Expected utility maximization implies that a homeowner with characteristics $(r, y)$ will purchase insurance if and only if

$$u(V-p) \geq (1-r)u(V) + ru(V-1) \tag{1}$$
$$p \leq y. \tag{2}$$

Ignoring for now the affordability constraint, it is helpful to define each individual's certainty equivalent to the no-insurance consumption vector. For a homeowner with flood risk $r$, the *no-insurance certainty equivalent* $x_r$ is defined by

$$u(x_r) = (1-r)u(V) + ru(V-1) \tag{3}$$
$$x_r = u^{-1}\left((1-r)u(V) + ru(V-1)\right). \tag{4}$$

Note also that under risk neutrality, $x_r = (1-r)V + r(V-1) = V - r$ is simply the expected value.[6] In other words, *risk neutral* homeowners will only purchase insurance at (or below) the actuarially fair price based on their individual flood risk, provided they can afford it. More generally, a homeowner with flood risk $r$ will only be willing to purchase insurance provided that it affords her a higher level of consumption than the no-insurance bundle: $V - p \geq x_r$.

It is well known from standard decision theory that the no-insurance certainty equivalent $x_r$ is a decreasing function of flood risk $r$, the probability of the low-consumption state of the world. Hence, facing equal premiums, homeowners with higher flood risk are more likely to purchase insurance than homeowners with lower flood risk. It is helpful to collect the following observations into a proposition:

**Proposition 1.** *When $p \leq 1$,*[7]

1. *The no-insurance certainty equivalent $x_r$ is a continuous and decreasing function in flood risk $r$ with slope $(u(V-1)-u(V))/u'(x_r))$. Furthermore, $x_0 = V$ and $x_1 = V-1$.*
2. *For a given value of $r$, $x_r$ is lower under risk aversion than under risk neutrality.*
3. *Given price $p$, there exists a threshold risk $r_p < 1$ such that the allocation of insurance*

[6] For CRRA, the no-insurance certainty equivalent simplifies to $x_r = \left((1-r)V^{1-\rho} + r(V-1)^{1-\rho}\right)^{1/(1-\rho)}$, where $\rho \geq 0$ is the coefficient of relative risk aversion.

[7] If $p > 1$, neither risk-neutral nor risk-averse individuals would be willing to purchase insurance.

*purchases is given by indicator function*

$$b(r,y) = \begin{cases} 0 & \textit{if } r < r_p \textit{ or } y < p \\ 1 & \textit{if } r \geq r_p \textit{ and } p \leq y, \end{cases} \tag{5}$$

*where*

$$r_p = \frac{u(V) - u(V-p)}{u(V) - u(V-1)}. \tag{6}$$

4. *Under risk neutrality, (i) the no-insurance certainty equivalent becomes the expected value of consumption* $x_r = r(V-1) + (1-r)V$*, and (ii) threshold* $r_p = p$.

*Proof.* See Appendix A. ■

Claim 3 in the proposition illustrates what Bradt et al. (2021) and Wagner (2022) refer to as *adverse selection* in the insurance market: faced with similar premiums, the buyers of insurance are more likely to have higher flood risk than the non-buyers.

## 2.3 Insurance demand prior to RR2

For convenience, we will refer to the old insurance pricing system as RR1, for the first risk rating system. Under this system, flood insurance premiums were substantially higher inside the 100-year flood zone than outside of it. Within each risk zone, premiums were somewhat tied to each property's individual risk (e.g. because of differences in elevation), but with a relatively low price-risk elasticity.

A simple functional form that captures the essence of the RR1 pricing schedule is the following linear spline function:

$$p_1(r) = \begin{cases} \alpha_X + \beta r & \text{if } r \in X = (0, \overline{r}) \\ \alpha_A + \beta r & \text{if } r \in A = [\overline{r}, 1), \end{cases} \tag{7}$$

where, for simplicity, the slope of the two linear parts is assumed to be the same. Furthermore, we assume $0 < \alpha_X \leq \alpha_A$ and $0 \leq \beta < 1$, where the latter assumption implies that premiums increase marginally less rapidly than flood risk.[8]

Figure 1 illustrates the price schedule and the relevant purchasing thresholds. To characterize the purchase allocations, it is helpful to first find the fixed points of the pricing

[8]In practice, prices within a given risk zone are a function of some structural characteristics of the house, such as whether it has a basement, and whether the first floor lies above or below the risk zone's base flood elevation (BFE), defined as the estimated height of waters in a 100-year flooding event (Kousky and Shabman, 2014). We will often interpret variation in flood risk $r$ as reflecting differences in elevation (relative to BFE).

function ($p_1(r) = r$) in each risk zone, denoted by $(r^A, r^X)$, which provide the indifference thresholds for purchasing insurance for *risk-neutral* homeowners. As stated in *Proposition 2* below, under risk aversion, the indifference thresholds become $(r_p^A, r_p^X)$ and it is easy to show that $r_p^Z < r^Z$ for $z = A, X$.

It is straightforward to prove the observations collected in the following proposition:

**Proposition 2.** *Assume premiums are given by the pricing schedule in Equation 7. Then*

1. *If* $\alpha_X < (1-\beta)\overline{r}$ *then there exists threshold* $r_p^X$ *such that* $0 < r_p^X < r^X < \overline{r}$ *(where* $r^X$ *is the threshold under risk neutrality) and insurance purchases in zone* $X$ *are given by indicator function*

$$b_1(r, y) = \begin{cases} 0 & \text{if } r < r_p^X \text{ or } y < p_1(r) \\ 1 & \text{if } r \geq r_p^X \text{ and } y \geq p_1(r). \end{cases} \tag{8}$$

2. *Similarly, if* $(1-\beta)\overline{r} < \alpha_A < 1-\beta$ *then there exists threshold* $r_p^A$ *such that* $\overline{r} \leq r_p^A < r^A < 1$ *(where* $r^A$ *is the threshold under risk neutrality) and insurance purchases in zone* $A$ *are given by indicator function*

$$b_1(r, y) = \begin{cases} 0 & \text{if } \overline{r} < r < r_p^A \text{ or } y < p_1(r) \\ 1 & \text{if } r \geq r_p^A \text{ and } y \geq p_1(r). \end{cases} \tag{9}$$

3. *The risk-neutral threshold,* $r^z = p_1(r^z) = \alpha_z/(1-\beta)$, *for* $z = A, X$. *Under risk neutrality, no homeowner voluntarily purchases insurance with a premium above her expected loss (*$p_1(r) > r$*).*

4. *The average premium paid by buyers in zone* $A$ *will be larger than the average premium paid in zone* $X$. *Moreover, in each risk zone* $z = A, X$, *insurance buyers with* $r_p^z \leq r < r^z$ *pay a premium above their expected loss:* $r < p_1(r)$. *Conversely, buyers with* $r > r^z$ *pay a premium below their expected loss:* $r > p_1(r)$.

5. *The mass of the set of buyers under the RR1 pricing schedule is*

$$\begin{aligned} Prob(B_1) &= \int_{r=r_p^X}^{r=\overline{r}} h(r)\left[1 - G(p_1(r))\right] dr + \int_{r=r_p^A}^{r=1} h(r)\left[1 - G(p_1(r))\right] dr && (10) \\ &= \left[H(\overline{r}) - H(r_p^X)\right] + \left[1 - H(r_p^A)\right], && (11) \end{aligned}$$

*where the later expression applies when all homeowners can afford insurance.*

*Proof.* See Appendix A. ■

## 2.4 Partial reform: elimination of implicit subsidies

As noted above, under RR1, some policyholders enjoyed implicit subsidies (i.e. were paying premiums below the annual expected loss) while others were implicitly taxed. Clearly, both situations are departures from actuarially fair pricing. Before examining the effects of RR2, it is helpful to consider a partial reform of the pricing schedule where implicit subsidies are eliminated, but implicit taxes remain.

Specifically, let us assume that premiums increase for policyholders with premiums below the annual expected loss ($p_1(r) < r$) while keeping premiums unchanged for all other policyholders ($p_1(r) \geq r$). The reformed price schedule can be described as

$$\widehat{p}_1(r) = \begin{cases} \alpha_X + \beta r & \text{if } r \in (0, r^X) \\ \widehat{\alpha}_X + \widehat{\beta} r & \text{if } r \in (r^X, \overline{r}) \\ \alpha_A + \beta r & \text{if } r \in [\overline{r}, r^A) \\ \widehat{\alpha}_A + \widehat{\beta} r & \text{if } r \in [r^A, 1), \end{cases} \tag{12}$$

where $0 < \beta < \widehat{\beta} \leq 1$ and $\widehat{\alpha}^z = \alpha^z - (\widehat{\beta} - \beta) r^z$.

Clearly, if $\widehat{p}_1(r) = r$ then the new premium equals the expected loss. The resulting utility maximizing purchase decisions are depicted in Figure 2 and described more formally below:

**Proposition 3**: *Under pricing function Equation 12, in each zone $z = X, A$,*

1. *The utility-maximizing choices are given by*

$$\widehat{b}_1(r, y) = \begin{cases} b_1(r) & \textit{if } r < r^z \\ 0 & \textit{if } r \geq r^z \textit{ and } y < \widehat{p}_1(r) \\ 1 & \textit{if } r \geq r^z \textit{ and } y \geq \widehat{p}_1(r). \end{cases} \tag{13}$$

2. *The switch from pricing function $p_1(r)$ to $\widehat{p}_1(r)$ triggers exit:*

$$Exit \;=\; \int_{r=r^X}^{r=\overline{r}} h(r)[G((\widehat{p}_1(r)) + G(p_1(r))]dr - \int_{r=r^A}^{r=1} h(r)[G((\widehat{p}_1(r)) - G(p_1(r))]dr > 0.$$

*Proof.* See Appendix A. ■

In words, below the risk-neutral threshold (where the premium equals the expected loss) in each zone, optimal choices are identical to those made under RR1 ($\widehat{b}_1(r, y) = b_1(r, y)$). However, homeowners with flood risk above the threshold experience a premium increase, resulting in exit from the insurance market by policyholders who can no longer afford insurance. Thus, the reform triggers selective exit and lowers overall take-up: low-income policyholders with high risk levels (relative to their zone) exit the market.

Clearly, social welfare falls because the reform leaves prices unchanged for some homeowners but raises them for others, forcing some of them to stop purchasing insurance because they can no longer afford it. However, the change in overall revenue is ambiguous since renewers (above the risk neutral threshold) pay higher premiums but take-up is now lower.

## 2.5 Risk Rating 2.0

The main tenets of RR2 are that premiums become individualized and actuarially fair, which entails the elimination of the implicit subsidies and taxes discussed above. Thus, as in Equation 12, policyholders with premiums below their individual expected loss should see premium increases, whereas the converse should occur for policyholders with premiums above expected loss. The resulting price function can be described as

$$p_2(r) = r. \tag{14}$$

It is straightforward to show that every homeowner with enough income will buy (full) insurance.[9]

**Proposition 4**: *Under pricing function Equation 14,*

1. *Regardless of risk, everyone who can afford the premiums buys insurance:*

$$b_2(r,y) = 1 \iff r \leq y.$$

2. *The mass of the set of buyers under the RR2 price schedule is*

$$Prob(B_2) \;=\; \int_{r=0}^{r=\bar{r}} h(r)\,[1 - G(r)]\,dr + \int_{r=\bar{r}}^{r=1} h(r)\,[1 - G(r)]\,dr, \tag{15}$$

*which equals to one if all homeowners can afford insurance.*

*Proof.* See Appendix A. ■

Let us now turn to the entry and exit generated by the switch from RR1 to RR2. As described in the following proposition (and illustrated in Figure 3), the qualitative pattern

[9] Observe that the price-risk gradient in $p_2(r)$ has a slope of 1, which is steeper than the price-risk gradient under RR1, the spline pricing function in Equation 7.

of entry and exit triggered by the new pricing system is the same in and out of the flood zone:

**Proposition 5**: *Assume* $0 < \alpha_X < (1-\beta)\overline{r} < \alpha_A < (1-\beta)$. *Switching from RR1 to RR2, in each zone* $z = A, X$, *there are 6 classes of homeowners:*

1. *There is new entry (*$b_1 = 0$ *and* $b_2 = 1$*) for all homeowners with relatively low flood risk* $r \leq r_p^z$ *that can afford the premiums* $y \geq r$. *These entrants pay lower premiums than renewers.*

2. *There is also new entry (*$b_1 = 0$ *and* $b_2 = 1$*) for homeowners with flood risk* $r_p^z < r < r^z$ *that were willing, but could not afford, to buy insurance under RR1 who now can afford it because* $p_2(r) = r < p_1(r)$.

3. *There is exit for high-risk homeowners* $r > r^z$ *who cannot afford the new premiums* $y < p_2(r) = r$. *The probability of exit increases in the individual flood risk and RR1 premium (*$p_1(r)$*) paid by each policyholder.*

4. *High-risk renewers* $r \geq r^z$ *experience an increase in premiums.*

5. *Low-risk renewers* $r_p^z < r < r^z$ *experience a reduction in premiums. Under risk neutrality, this set is empty (since* $r_p^z = r_z$*).*

6. *Last, there are homeowners that did not purchase insurance under either price system (*$b_1(r, y) = b_2(r, y) = 0$*) because they cannot afford it:* $y < min\{p_1(r), p_2(r)\}$.

*Proof.* See Appendix A. ■

Our analysis so far has focused on voluntary purchases of flood insurance. However, it is well known that homeowners in the 100-year flood zone ($z = A$) with a (publicly backed) mortgage are required to purchase insurance, regardless of their income. Moreover, there is recent evidence that lenders are increasingly requiring flood insurance as a condition for approval of mortgage requests, or some other adjustments in loan requirements, such as higher loan-to-value ratios (Sastry, 2023) or rationing (Blickle and Santos, 2022). Thus, involuntary buyers (due to the mandate) will increase take-up (primarily in zone $A$) relative to the scenario where all insurance purchases are voluntary, and some of these buyers may have risk below threshold $r_p^z$.

## 2.6 Extensions

### 2.6.1 Revenue and Social Welfare

As we have seen above, the set of homeowners who purchase insurance (and the premiums they pay) differs under RR1 and RR2. An important outcome to gauge if the switch to RR2 has improved the financial solvency of the NFIP is the change in the overall revenue collected through premiums, which reflects the convolution of entry, exit, and the composition of homeowners purchasing insurance.

Let us index the pricing functions for RR1 and RR2 by $j = 1, 2$, respectively. Overall revenue under pricing function $j$ is given by

$$R_j = \int_{r=0}^{r=1} \int_{y=0}^{y=\infty} b_j(r, y) p_j(r) h(r) g(y) dr dy, \tag{16}$$

where $b_j(r, y) = 1$ identifies the homeowners that choose to purchase insurance.[10] Specializing this expression to each pricing schedule delivers:

$$R_1 = \int_{r\in(0,r_p^X)\cup(r_p^A,1)} p_1(r)[1 - G(p_1(r))]h(r)dr \tag{17}$$

$$R_2 = \int_{r=0}^{r=1} p_2(r)[1 - G(p_2(r))]h(r)dr, \tag{18}$$

where the last expression makes clear that there is full take-up under RR2 among homeowners that can afford the premiums. The following proposition describes the change in *revenue* as we switch from RR1 to RR2:

**Proposition 6.** *The change in overall revenue (in units of the income numeraire) as we switch from RR1 to RR2 is given by*

$$\Delta R = Prob(Renew)E\left[p_2 - p_1|Renew\right] \tag{19}$$

$$+ \; Prob(Entry) \times E\left[p_2|Entry\right] \tag{20}$$

$$- \; Prob(Exit)E\left[p_1|Exit\right]. \tag{21}$$

*Proof.* See Appendix A. ■

[10] It is worth noting that, in our model, the population of the floodplain has been normalized to one and, therefore, $R_j$ measures per-capita revenue. To obtain the aggregate dollar amount, we need to multiply $R_j$ by the number of homeowners in the floodplain.

The proposition makes clear that the effects of the reform on overall revenue and, hence, the financial sustainability of the NFIP depend importantly on the entry and exit triggered by the new pricing schedule, which in turn depend on the parameters of the reform and the underlying risk and income distributions. Thus, whether the adoption of RR2 increased overall revenue is an empirical question. Importantly, our data allow us to estimate each of the terms in the equation above, which provides a decomposition of the changes in revenue due to changes in entry, exit and renewal decisions.

It is also interesting to analyze the changes in *social welfare* arising from changes in the pricing schedule, which can be found in Appendix B.[11] The main insights of the analysis are easy to convey. First of all, changes in the pricing schedule generically produce winners and losers. The winners are homeowners receiving *lower* premium quotes for flood insurance (i.e. for which the implicit tax is removed), which expands their budget sets and therefore leads to gains in expected utility (with a strict sign if they purchase insurance after the reform). Conversely, homeowners experiencing increases in premiums (due to the loss of implicit subsidies) will suffer a loss in expected utility (with a strict sign if they purchased insurance prior to the reform). In either case, the reform can lead to extensive margin effects (entry or exit) and to intensive margin effects in the form of changes in premiums (for homeowners who purchase insurance both before and after the reform). As was the case with revenue, whether a particular reform delivers a net increase or decrease in social welfare depends on the parameters of the reform and the underlying risk and income distributions.

Secondly, it is easy to see that entry and premium reductions increase social welfare, whereas exit and premium increases lower it. Additionally, our analysis shows that exit and entry may not have symmetric effects on social welfare. To the extent that the (consumer) surplus from purchasing insurance $(V - p(r) - x_r)$ increases in flood risk $r$, the volume of exit will have a larger weight than that of entry for reforms that disproportionately trigger exit of high-risk policyholders, as is the case with the reforms analyzed in subsection 2.4 and subsection 2.5.

#### 2.6.2 Dynamics

So far our model is purely static. As a result, provided that the pricing schedule remains unchanged, the model implies zero entry and exit, and all policyholders always renew their policies. Appendix C presents a simple overlapping generations extension of our current model that produces churning in the insurance market, perhaps driven by changes in property

[11] Our analysis assumes a utilitarian social welfare function where all homeowners are assigned equal weights. We do not account for the dynamic incentives in terms of risk-mitigation investments.

ownership due to home sales. More specifically, over periods of time when the pricing schedule remains unchanged, there is non-zero entry and exit in each period (and both offset each other) and, in addition, the renewal rate is strictly between zero and one.

More importantly, when the pricing schedule changes as in subsection 2.4 or subsection 2.5, entry, exit and renewal experience changes relative to their baseline values, and these changes correspond to the situations depicted in Figure 1-Figure 3. In particular, in our dynamic setting, the partial reform analyzed in subsection 2.4, where implicit subsidies are eliminated, leads to a reduction in *both* renewals and entry. More specifically, each shift of the pricing schedule in this direction leads to a one-time reduction in renewals among affected homeowners who cannot afford the higher premiums and a persistent reduction in entry affecting new homeowners who cannot afford the higher premiums. These groups are depicted in the pink triangles in Figure 2 and are characterized by high flood-risk levels (relative to their zone) and low household income.

Similarly, the adoption of RR2 (taking RR1 as the baseline) triggers exit and reduces entry for relatively high-risk homeowners, corresponding to the pink triangles in Figure 3. However, the reform is also predicted to incentivize entry among relatively low-risk homeowners who are now able to purchase low-premium policies aligned with their low levels of flood risk. As a result, RR2 may increase entry or lower it, relative to pre-reform levels.

### 2.6.3 Subsidized policies

Before the adoption of RR2, premiums for some properties built prior to the creation of flood maps were priced according to a more favorable pricing schedule. Insurance policies covering these properties are referred to as *pre-FIRM* or subsidized.

Essentially all *pre-FIRM* properties are located in the 100-year flood zone and their premiums did not depend on elevation because this information was not easy to establish in the absence of official flood maps. A simple way to think about the pricing schedule for these type of policies prior to RR2 is $p(r) = \alpha_A$ for $\overline{r} \leq r < 1$. Indeed, as one can deduce from Figure 1, some of the so-called 'subsidized' policies were receiving an implicit subsidy ($p(r) < \alpha_A$) whereas others were bearing an implicit tax ($p(r) > \alpha_A$). According to the figure, the former would have relatively high flood risk whereas the latter would have relatively low flood risk.

When RR2 was adopted, subsidized policies became subject to the same (full-risk) pricing schedule as all other properties. Hence, those enjoying an implicit subsidy (tax) are expected to experience a premium increase (reduction).

### 2.6.4 Subjective risk beliefs

Our analysis so far has assumed that homeowners flood risk beliefs reflect objective flood risk. However, several studies have empirically documented departures from this assumption. Bakkensen and Barrage (2022) document underestimation of flood risk beliefs in the 100-year flood zone arising from sorting of homeowners with downward biased subjective flood risk beliefs. Likewise, Weill (2022) argues that homeowners residing in the periphery of the floodplain underestimate their exposure to flood risk. A simple modification of our setting allows us to analyze the effect of these behavioral traits on flood insurance take-up. In particular, we allow for subjective flood risk by assuming that homeowners choose whether to buy insurance on the basis of a subjective probability of flooding given by $\pi(r) = \lambda r$ where $0 < \lambda \leq 1$, where lower values of $\lambda$ entail a larger degree of underestimation of the objective risk. For the sake of brevity, we refer interested readers to Appendix D for the formal analysis. The gist of this extension of the model is that there is a clash between flood risk skepticism (inversely related to $\lambda$) and risk aversion: for a given degree of risk aversion, insurance take-up falls in the discount placed on objective flood risk probabilities, as in the model in Wagner (2022).

# 3 Data: Sources and panel construction

## 3.1 NFIP policies

Our main data source is the *OpenFEMA Dataset* retrieved in January 2024.[12] The data contains the universe of flood insurance policies sold through the *National Flood Insurance Program* since 2014, even though our main focus is the period since 2019.

These data contain a rich set of attributes, as illustrated by Table 1. The table focuses on the approximately 17 million policies purchased in calendar years 2019-2023 for residential buildings with up to 4 residential units. Among these, 44% were in the 100-year flood zone (defined as properties with flood zone designations A or V) and 97% pertained to single-family houses. In addition, 82% of the policies covered primary residences, 7.4% of the policies were subsidized (due to pre-FIRM construction), and 15.2% of the policies were reported to be mandatorily purchased (mostly due to federally backed mortgages for buildings located in the 100-year flood zone). We also note that Florida and Texas account for a 46.5% market share.

[12]As required by FEMA, we acknowledge that this data product uses the FEMA OpenFEMA API, but is not endorsed by FEMA. The Federal Government or FEMA cannot vouch for the data or analyses derived from these data after the data have been retrieved from the Agency's website(s).

The average building replacement cost was around \$308,000, which is often used as a proxy for household wealth. The average total coverage was around \$280,000, with 76% of this amount insuring the building (and the rest providing coverage for contents). On average, the annual cost of flood insurance policies was \$941, consisting of a \$711 premium and \$230 fees. It will be helpful to define the *insurance price* for \$1,000 of coverage, which on average was \$4.5. Another important parameter affecting the price of insurance is the building deductible, which on average was \$2,049.

Importantly, these data have two important limitations for our purposes. First, FEMA does not release a policyholder identifier. In addition, due to privacy concerns, the data do not include detailed building coordinates (such as an address or precise latitude-longitude coordinates).

## 3.2 Individual Insurance Histories

Analyzing individual insurance histories longitudinally requires a method to link individual policies pertaining to a specific policyholder over time. To do so we build on Wagner (2022) and, particularly, the more recent algorithm proposed by Mulder and Kousky (2023), which we extend it in several ways. The goal in Mulder and Kousky (2023) was to provide a descriptive analysis of the effect of RR2 on annual premiums, based on NFIP policies (restricted to single-family homes) purchased in the month of April in years 2021 and 2022. Their main contribution is the use of the rich set of policy attributes in the data to link individual policies purchased by the *same policyholder* over time. Essentially, the authors identified a set of attributes that could plausibly be considered time-invariant and grouped policies with identical values for said vector. Using these data, they concluded that the adoption of RR2 led to a reduction in the average premium in the 100-year flood zone but an increase in the average premium paid by homeowners located in its periphery. Importantly, their estimates provide only a partial view of the effects of RR2 on the NFIP revenue. A more comprehensive analysis requires estimating the effects of the reform on entry and exit.

We will rely heavily on their algorithm to group policies, but extend it in several ways. Thus, we first describe the vector of attributes used by Mulder and Kousky (2023) and we will also evaluate the validity of their time-invariance assumptions. It is worth noting that our analysis will require linking policies over a longer (5-year) period of time (2019-2023), which requires adapting the algorithm used by Mulder and Kousky (2023) to link policies over a 2-year period.[13]

[13]Note that the unit of analysis is a homeowner. Analogous to a business establishment, it is a combination of a fixed physical location and the people who inhabit it.

### 3.2.1 Algorithm performance metrics

The linking algorithm proposed by Mulder and Kousky (2023) (MK for short) is based on the following 7 policy attributes: census tract, original construction date, original new business date, flood zone location, base flood elevation, lowest floor elevation, lowest adjacent grade, and (implicitly) month the policy becomes effective.[14]

Because of the high dimensionality of this vector, provided these attributes remain *invariant over time for a given policyholder*, it is extremely likely that policies with identical values for the vector of attributes were purchased by the same policyholder. However, if either attribute happens to vary over time, the MK algorithm will introduce an excessive fragmentation of individual insurance histories by creating two different identifiers when, in reality, all those policies belong to a single policyholder.

In order to examine the plausibility of the time-invariance assumptions and to gauge the relative contribution of each attribute, we will reconstruct the MK algorithm by adding attributes one at a time and examine the performance of the corresponding groupings (policyholder identifiers) along 6 metrics.[15] At each step, we evaluate the metrics on the basis of the groupings of policies resulting from the use of the attributes introduced up to that point:

*1. Duplicates.* This metric is based on the number of policies in the same group in a given year (minus one), normalized by the overall number of in-force policies in that year. The NFIP only allows a policyholder to have a single policy in-force for a specific residence at any point in time. Thus, observing multiple policies for a single ID-year points to an excessively coarse partition where some groups agglomerate policies that, in reality, belong to different policyholders.[16] Clearly, provided that all attributes used to define a group are indeed time-invariant, a more successful algorithm will exhibit fewer duplicates. We also note that, erroneously including a time-varying attribute in the attributes vector will *also* lower the number of duplicates, but result in an excessive fragmentation of individual insurance

[14] The original new business date records the first time (day, month and year) that a policyholder purchased insurance for a specific building. By restricting their sample to the month of April for 2021 and 2022, the MK algorithm effectively only links policies that came into effect in the same month (April). For exact variable definitions, see `https://www.fema.gov/openfema-data-page/fima-nfip-redacted-policies-v2`.

[15] Obviously, the contribution of each attribute depends on the specific ordering of the attributes. We introduce first the attributes that are more plausibly time-invariant.

[16] For instance, the first attribute we will use to create groups that partition the set of policies is the census tract (CT) reported in the policy. We will then compute the number of policies (referred to as copies) that belong to the same group and year. The number of duplicates is the number of copies minus one. Clearly, because most census tracts contain more than one policyholder, a partition based solely on census tract will exhibit many duplicates. Next, we will create a new partition where the groups are defined using two attributes: census tract (CT) and original construction date (OCD) and then recompute the number of duplicates, that is, the number of policies with the same values for census tract, OCD and year, minus one.

histories. As shown in the top panel of Table 2 (last row), the MK algorithm results in a duplicates rate of 1.26%.

*2. Average length of insurance spells.* For each group (ID), we compute the number of years between the first and last insurance policies and then average across all groups. To fix ideas, a 5-year insurance history $(b_1, b_2, b_3, b_4, b_5) = (1, 1, 1, 1, 1)$ has a spell of length 4, which is the maximum length in our 5-year dataset. Analogously, homeowners that only purchased one policy have a spell of zero length. Unfortunately, we do not know the actual average length of the individual insurance histories in our dataset, but it is well known that there is new entry every year and purchases are highly persistent. Hence, the true average length is likely to range between 2 and 4 years (for a 5-year data span). As noted earlier, erroneously relying on time-varying attributes will shorten insurance spells. Thus, this metric helps assess the validity of the time-invariance assumptions. The average length of the insurance histories obtained with the MK algorithm is 2.46 years.

*3. Coverage lapses* Typically, policyholders renew their policies on the same exact day (month-day) every year. However, unexpected (income or health) shocks can make a policyholder be late on a payment, creating a lapse in coverage. Our metric counts the number of groups (IDs) displaying a lapse in coverage, defined as a period of more than 12 months between two consecutive insurance purchases, normalized by the number of groups. Clearly, income and health shocks are a fact of life, so policy lapses surely take place, but are probably a rare occurrence. Thus, the true number of lapses is expected to be positive, but small.[17] The MK algorithm entails a minuscule frequency of policy lapses (0.08%).

*4. Changes in elevation variables.* Policyholders that provide an elevation certificate benefit from lower premiums. The elevation certificate reports information on the base flood elevation, elevation of the lowest floor, and lowest adjacent grade that is incorporated into the insurance policy. Prior to RR2, obtaining such a certificate was costly and a very infrequent event, but could be an endogenous response to a premium increase.[18] Specifically, our metric reports the number of groups exhibiting within-group variation in either of the three elevation variables, divided by the overall number of groups. We expect this value to be positive, but small. Because the MK algorithm uses the levels of the elevation variables as attributes to define groups, it mechanically implies a value of zero for this metric, which very likely underestimates the actual number of changes in elevation variables.

[17]The average forbearance rate for mortgages, defined by the share of consumers with mortgage balances who are more than 30 days late on their payments, was 2.2% in the period 2018-2023. It is likely that policyholders in forbearance who are due for renewal of their flood insurance policy will also let their coverage lapse. Among policyholders who do not have an outstanding mortgage, the probability of an insurance policy lapse is likely to be much lower.

[18]The new flood risk estimates used to price policies under RR2 incorporate property-level estimates for all buildings and adjust premiums accordingly (Mulder and Kousky, 2023).

*5. House sales with policy endorsement.* When a house is sold, the buyer often keeps the existing insurance policy (to benefit from the lower old rate). In this case, the insurance history continues and the purchase date is recorded as a policy attribute. Obviously, houses are sold every year, but this is an infrequent event for a given property.[19] We compute the rate of sales with a policy endorsement and then normalize by the number of groups. We expect this rate to be around 1.7%, under the assumption that flood insurance policies are always endorsed by the buyer of the house. The MK algorithm implies that 0.8% of policies were endorsed at the time a house was sold.

*6. Changes in flood zone status.* FEMA gradually updates flood maps and, as a result, some houses are moved into or out of the 100-year flood zone. This has been used in the literature to identify the effects of flood zone status on housing prices (Ortega and Taspinar, 2018; Indaco et al., 2019) and flood insurance take-up (Weill, 2022). We report the number of groups exhibiting a change in flood zone status within our period of analysis, normalized by the number of groups. The MK algorithm assumes that flood zone status is a time-invariant attribute and, hence, mechanically implies zero status changes.

### 3.2.2 The MK algorithm

Let us now examine the evolution of the performance metrics as we gradually add policy attributes to the MK algorithm. As shown in the top panel of Table 2, relying on census tract as the single attribute to identify policyholders is clearly not enough as it produces 71,156 groups (homeowner IDs) with an average insurance spell of 3.75 years (out of a maximum of 4 years) and 98% duplicates. In other words, it clearly generates too few groups with unrealistically long insurance spells and too many duplicates.

Adding the original construction date (OCD) as an attribute helps a lot: the number of groups increases to 2.44 million, but the rate of duplicates remains exceedingly high (45%).[20] Further adding the original new business date (ONBD) as an attribute practically doubles the number of groups (4.74 million) and brings down the rate of duplicates to 2.15% and the average spell to 2.52 years. Including the remaining 3 attributes (flood zone status, elevation variables, and month the policies become effective) has relatively small effects on the number of groups, rate of duplicates, and average spell, but let us examine the metrics in columns 5-7 in Table 2. The MK algorithm implies a very low frequency of policy lapses (0.08%)

[19] In the United States, 6.12 million homes were sold in 2021 out of a stock of 143.13 million housing units (*Statista.com*). Hence, a 0.43% annual rate of sales. Since we focus on a 5-year period, each house could conceivably be sold 4 times. Hence, the 4-year probability of sale is $4 \times 0.43\% = 1.72\%$.

[20] As far as we can tell, Wagner (2022) links policies on the basis of zip code, community id, flood zone and construction year, which probably generates a high rate of duplicates. Her algorithm also matches policies to individual houses and to claims data.

and assumes away the possibility of changes in elevation variables and flood zone status, both of which are known to happen occasionally. As a result, the rate of duplicates and the average insurance spell produced by the MK algorithm may be excessively low. It is also worth noting that the analysis in Mulder and Kousky (2023) entails only one year of data and, therefore, flood map revisions entailing changes in flood zone status and updates to elevation variables will affect very few policyholders. However, in other analyses (like ours) that require linking together policies for more years, these time-invariance assumptions will be less tenable.

### 3.2.3 The OP algorithm

On the basis of the previous discussion, we propose a new algorithm (named after ourselves) that (i) does not rely on some of the attributes that potentially vary over time for a given property (such as flood zone status, elevation variables and constant effective month), and (ii) introduces several new attributes that help refine the partition of policies (and are likely to be time-invariant). More specifically, our algorithm uses census block groups (rather than the broader census tract), latitude and longitude (only available with one decimal), flood map number, original construction date, number of floors, and new business date. Last, we also make use of a policy attribute indicating when a house was sold and the flood insurance policy endorsed by the new buyer. While this is not an intrinsically time-invariant attribute, it is convenient to break up insurance spells when this event happens because relevant characteristics of the policyholder may change, such as income, or whether the house is used as collateral for a mortgage, which could require mandatory purchases of flood insurance.

The performance of our algorithm is assessed in the bottom panel of Table 2. The first row creates groups using the most plausible time-invariant features of the house (census block group, latitude-longitude, flood map, original construction date and number of floors). These attributes lead to 3.56 million policies and a duplicates rate of 24.65%. When we add the original new business date, the number of groups grows to 4.82 million, the rate of duplicates falls to 1.49% and the average spell becomes 2.49 years. When we also require that insurance histories not contain a house sale, the number of groups (policyholder IDs) grows to 4.85 million groups, and the duplicates rate and average spell fall to 1.27% and 2.48 years, respectively. These values are very similar to those attained by the MK algorithm. However, the insurance histories in our dataset allow for more policy lapses (1.14% vs. 0.08%), updates in elevation variables (0.38% versus 0%), and changes in flood zone status (0.29% versus 0%) than the MK algorithm. At the end of the day, over short time periods, these discrepancies are unlikely to matter much, but our enhanced algorithm is more versatile and can be reliable applied to long periods of time.

# 4 Changes in price distributions around RR2

This section provides a descriptive analysis of flood insurance demand prior to the implementation of RR2 and a simple comparison of the changes in the distributions of insurance prices (by risk zone) around the reform. The insights from this analysis have been helpful in guiding our approach to identification of the effects of the reform described in the remainder of the paper.

## 4.1 Insurance demand prior to RR2

Let us summarize the key attributes of the policies purchased in the last year during which RR1 was in effect, namely, between 2021Q2 and 2022Q1 (both included), which we refer to as fiscal year 2021 (fy21). Specifically, Table 3 restricts the sample to 2-year insurance histories for policyholders who purchased insurance in fy20 and were up for renewal in fy21, which contains almost 3.57 million policyholders. The first row shows that 86% of the homeowners renewed insurance in fy21 (3.07 million). Almost all policies (97.3%) were for single-family homes, and slightly less than half of all policies (48.1%) were purchased by homeowners in the 100-year flood zone. The average total cost of the policies was $955, which can be broken down into a premium of $724 and $231 in fees. The average total insurance coverage was approximately $282,000 (with 76% corresponding to building coverage and the rest to contents). In turn, the average policy had a building deductible of $1,972. Last, 16.1% of the policies were subject to mandatory purchase and 10.7% benefitted from subsidized rates.

It is also instructive to compare the mean attributes of the policies purchased in the flood zone and the periphery. As shown in Table 4, the renewal rate was 4 percentage points higher in the flood zone than in the periphery (88.1% versus 84.1%). Put otherwise, the annual probability of exit was 11.9% in the flood zone and 15.9% in the periphery, partly reflecting the insurance purchase mandate affecting properties with mortgages in the flood zone. In addition, houses in the flood zone are on average about 7 years newer.[21] Not surprisingly, the annual insurance cost (including premiums and fees) is substantially higher in the flood zone by an average of $696 ($612 in the periphery vs. $1,308 in the flood zone). In turn, policyholders in the flood zone choose larger building deductibles ($2,693 versus $1,269 in the periphery) and lower coverage ($250,960 versus $312,604 in the periphery) in

[21] Rather than a feature of the stock of housing, we suspect that the house age difference is due to the insurance mandate. Newer houses are more likely to be collateral for a mortgage and thus subject to mandatory insurance purchase.

order to lower their annual premiums. There's also a large discrepancy in the prevalence of subsidized rates (mainly linked to pre-FIRM construction) and purchase mandates, both of which are heavily concentrated in the flood zone. In the flood zone, 21.7% of the policies enjoy discounts (compared to only 0.5% in the periphery) and 29% are bought *involuntarily* (compared to only 4% in the periphery).[22]

It is also interesting to compare the characteristics of renewers and quitters prior to RR2, conditional on both having purchased insurance in the previous year. We do this by restricting the sample to homeowners who purchased insurance in the second-to-last round of RR1 (fy20, 2020Q2-2021Q1) and estimate linear regression models for various lagged outcomes (such as log insurance price and log of the building replacement cost) where we include an *exit* indicator as a regressor, which takes a value of one for homeowners who did *not* purchase insurance in the last round of RR1, and zero if they did. As shown in Table 5 (columns 1-2), on average, homeowners who exited the insurance market were paying annually between 0.11 log points (in the periphery) and 0.31 log points (in the flood zone) more than renewers. Keeping in mind that RR2 offers actuarially fair premiums, which in our theoretical model implies that all homeowners are willing to purchase insurance, this finding suggests that affordability may have been the key factor in the decision to discontinue insurance coverage (Figure 2). Column 2 provides additional support for this interpretation: quitters lived in properties that had lower value (measured by replacement costs) by about 0.015 log points (similarly in the flood zone and its periphery). In addition, policies attached to a primary residence were significantly less likely to be discontinued.

## 4.2 Insurance price distributions before and after RR2

Let us begin by examining the cross-sectional distribution of insurance prices, separately by risk zone, in the last round of purchases under RR1 (fy21) and in the first round after RR2 (fy22), considering all in-effect policies (renewals and new entry). Clearly, these distributions will be affected both by entry and exit, as well as by within-individual changes in insurance prices.

Our measure of insurance prices is the annual premium (plus fees and surcharges) adjusted for (i.e. divided by) coverage in thousands of dollars.[23] As seen in Figure 4, the adoption of

[22]The purchase mandate linked to publicly backed mortgages only applies to properties located in the 100-year flood zone. The policies in the periphery reported to be subject to a mandate may reflect changes in the boundary of the flood zone. However, these policies could also be requirements imposed by lenders who increasingly require mortgage applicants in flood-prone areas to purchase flood insurance in order to approve their loan applications.

[23]In theory, the actuarially fair coverage-adjusted price for a property facing exactly a 1% annual probability of flooding should be $10.

RR2 significantly affected the distribution of insurance prices in the SFHA. Specifically, the price density for fy22 (orange line) has a lower probability mass of high-price policyholders (in the range of $10-20 per $1,000 of coverage) than the density for fy21 (blue line). Thus, either the reform led them to exit the insurance market, or they experienced premium reductions.

It is also interesting to examine the price distribution in the flood zone excluding homeowners with subsidized policies (about 20% of the flood zone). As seen in Figure 5, the pre-reform and post-reform price densities in the flood zone are now much more similar. In particular, the discrepancy between the right-tails of the price densities in Figure 4 has vanished, implying that those high-price policies belong to 'subsidized' policyholders (owning pre-FIRM constructions). Put differently, the largest price effects of RR2 among policyholders in the flood zone were borne by pre-FIRM policyholders, resulting both in exit and (as we shall show below) in substantial price reductions.

Let us now turn to the changes in insurance prices in the *periphery* of the flood zone. As seen clearly in Figure 6, the new pricing schedule essentially shifted the price density rightward. Under RR1, the bulk of the policyholders paid insurance prices ranging from $1.5-$2 per $1,000 of coverage. When RR2 was adopted, the price for the majority of policies shifted up to $1.8-2.3 per $1,000 of coverage. It is also worth noting that RR2 led to an increase in the density of homeowners in the periphery paying very low insurance prices (in the range of $1-1.4 per $1,000 of coverage), illustrating that some residents of the periphery also experienced premium reductions when RR2 was adopted.

These figures clearly illustrate that insurance price distributions changed substantially when RR2 was implemented. These changes suggest that the reform may have triggered extensive-margin changes (i.e. affected the decisions to renew coverage or to purchase it for the first time). Additionally, the figures also hint at highly heterogeneous effects of the reform within the SFHA between regular policies and those insuring pre-FIRM properties (which enjoy subsidized rates). These observations will crucially guide our analysis in the next section.

# 5 Effects of RR2 on Renewal and Exit

Our longitudinal dataset contains the universe of policies for the period 2019-2023, which allows us to reconstruct the flood insurance histories of all individuals that purchased insurance at least once during our period of study. Thus, we can decompose insurance purchases at each point in time into renewals and new entry, by conditioning on whether or not the homeowner purchased insurance in the previous period.

Specifically, let $b_{i,t}$ be an indicator for whether homeowner $i$ purchased insurance in year

$t = 2019, ..., 2023$. Our original longitudinal dataset does *not* contain homeowners that never bought insurance over our period of study, but we can expand our dataset by entering zero values ($b_{i,t} = 0$) in the years in which a homeowner did *not* purchase insurance (provided she had purchased at least one time in our period of study). As a result, we obtain a balanced panel $\{(i, t) : b_{i,t} = 0, 1\}$ for $t = 2019, ..., 2023$.

Next, we discuss our empirical approach to identify the effects of the reform along the extensive margin of insurance demand, with a focus on the renewal (exit) decision, allowing for differential effects in and out of the flood zone and for SFHA policyholders with formerly subsidized rates.

## 5.1 Empirical Model

Standard policy evaluation analyses compare the average changes in the outcome of interest (e.g. premiums or insurance purchases) for a treatment group relative to a control group, composed of similar individuals who were not affected by the policy. The RR2 reform completely replaced the previous pricing schedule, resulting in premium changes for the vast majority of homeowners exposed to flood risk. As a result, there is no control group that can be used to soak up the effects of confounding factors.[24]

We pursue an alternative identification strategy. In essence, for each of our groups of interest, we will construct a counterfactual based on pre-treatment trends and investigate if the deviations of the outcome of interest arising precisely *at the time* of the adoption of RR2 can be plausibly interpreted as a causal effect of the reform. We also complement the analysis with a regression discontinuity (in time) analysis investigating the changes in outcomes around a 10-day window before and after the implementation of the reform.

Our analysis of the effects of RR2 along the extensive margin of insurance demand focuses on the changes in the probability of renewal or, conversely, the probability of exiting the market, for individuals who had purchased insurance in the previous period. Considering individuals who purchased insurance one year ago ($b_{i,t-1} = 1$), we define *renewal* if they purchase again in the current year ($b_{i,t} = 1$) and *exit* if they do not ($b_{i,t} = 0$). It is worth noting that *exit* is the change in purchasing behavior between two consecutive years

[24] One possibility could be to use mortgage holders located in the SFHA as a control group, given that they are subject to the insurance purchase mandate. However, enforcement of the mandate is imperfect, which implies that the affected homeowners are still deciding whether to purchase insurance (in compliance with the mandate) or not. When RR2 was introduced, these homeowners also experienced changes in premiums, which may have affected their decision to comply with the mandate. Another possibility would be to identify policyholders who experienced very small premium changes when RR2 was implemented. However, this is only feasible conditional on renewal because we do not observe the post-reform premium for the non-renewers, which does not allow for the estimation of renewal probabilities.

(conversely, renewal is the non-change).[25]

Homeowners take renewal decisions once a year, when the previous (annual) policy is about to expire. However, renewals occur on a rolling basis throughout the year (peaking in the third quarter of every year), which allows us to examine renewal choices at a quarterly frequency (by policyholders facing expiration of their annual policy). Specifically, we will focus our analysis on individual histories spanning 3 years: 2 years prior to the implementation of RR2 (2020Q1-2022Q1) and 4 post-reform quarters (2022Q2-2023Q1).

It is worth pointing out that we could extend the post-reform analysis until the end of 2023. However, we are mostly interested in the policyholders' *first renewal choice* since the adoption of RR2 in order to investigate whether the reform triggered exit from the insurance market. To see why, consider a stylized scenario where premiums were constant in the quarters leading up to the reform. Assume also that insurance purchase decisions depend only on prices, insurance demand is downward-sloping, and homeowners are infinitely lived. In this setup, all policyholders renew their annual policies. Next, assume that a reform is adopted that introduces a one-time increase in the average premium and premiums remain constant (at the new level) from the on. As a result, we will observe a reduction in the number of insurance buyers, manifested in a reduction in the renewal rate (i.e. an increase in the exit rate) *at the first post-reform renewal decision*. Beyond that point, the new (smaller) set of insurance buyers goes back to a 100-percent renewal rate from then on. In reality, even with constant premiums, a minority of policyholders allow coverage lapse due to house sales or idiosyncratic shocks to household income (as in the dynamic model laid out in Appendix C).[26]

With slight abuse of notation, we redefine time index $t$ to denote *quarters*, with $t = -8, -7, ..., 0$ referring to quarters priced under RR1 and $t = 1, 2, ..., 4$ to quarters priced under RR2. In each quarter $t$, only homeowners whose policy is expiring choose whether to renew coverage or not.[27]

To fix ideas, let us focus first on the probability of renewal in a specific risk zone, say the SFHA. We define the probability of renewal in quarter $t$ (in the risk zone considered) by

[25]Hence, the dummy variable identifying exit (or renewal) is a *within-individual* first-difference of the purchasing dummy (conditional on having purchased insurance in the previous year). Consequently, individual, time-invariant factors are differenced out.

[26]Additionally, average premiums were increasing in the years leading up to the implementation of RR2 by virtue of the adjustments mandated by the 2012-2014 reforms.

[27]Quarterly insurance purchases display strong seasonality (as seen in Figure E.1 and Figure E.2), but this is not the case for the probability of renewal.

$q_{i,t} = Prob(b_{i,t} = 1 | b_{i,t-4} = 1)$. We assume the probability of renewal is given by

$$q_{i,t} = \alpha + (\beta t)D(t \leq 0) + \sum_{j=1}^{j=4} \gamma_j D(j = t) + u_{i,t}, \tag{22}$$

where $D(j = t)$ is an indicator variable, taking a value of one if condition $j = t$ holds and $u_{i,t}$ is a disturbance term. Observe that the probability of renewal in the last quarter in which policies were priced under RR1 is given by $q_{i,0} = \alpha$, which coincides with the aggregate renewal rate. In previous quarters ($t \leq 0$), the probability of renewal is $q_{i,t} = \alpha + \beta t$. In contrast, in post-reform quarters $t \geq 1$, we allow for fully flexible trends and $q_{i,t} = \alpha + \gamma_t$.[28]

It is also worth noting that Equation 22 nests more restricted models. For instance, we can also consider a version of the model with a linear pre-trend but a time-invariant post-reform effect ($\gamma_j = \gamma$ for $j = 1, ..., 4$), or an even simpler model where pre-reform values are also assumed to be constant ($\beta = 0$). In practice, we will estimate an extension of Equation 22 that allows for differential (linear) trends in and out of the SFHA.[29]

## 5.2 Counterfactuals

Naturally, isolating the causal effect of the reform requires specifying a counterfactual scenario regarding the evolution of renewal probabilities had the RR2 reform not been implemented. As discussed above, there is no obvious control group because the reform affected the premiums of virtually all homeowners on the floodplain. But we can envision two reasonable approaches to establish counterfactual renewal rates.

One approach is to assume that, in the absence of RR2, renewal rates would have remained at the values observed in the last quarter prior to the reform (2022Q1). This would correspond to a situation where the insurance price schedule is frozen in time, halting the annual changes in rates mandated by RR1, and no other determinants of insurance purchases change over the treatment period, such as household income or disaster-driven insurance

[28]It is straightforward to include a polynomial trend. But, as we shall see later, the linear trend is a good approximation to the data and provides a more straightforward counterfactual. It is also worth noting that the estimate for $\alpha$ will typically not match exactly the average renewal probability in period $t = 0$. Specifically, it will be equal to the predicted mean value (based on the linear trend) for the average renewal probability in period $t = 0$, namely, $\hat{q}_0 = \hat{\alpha} = \overline{q_t} - \hat{\beta}\bar{t} = \overline{q_t} - 4\hat{\beta}$, given that $t = -8, ..., 0$ in the pre-reform period.

[29]Our estimation sample will also include homeowners subject to the mandatory purchase requirement. While excluding these individuals would more clearly connect the predictions of the model with the data, there are practical difficulties in identifying these homeowners. In particular, we observe a sudden increase in missing values for the mandatory purchase indicator in the NFIP dataset at the adoption of RR2. At any rate, to the extent that the distribution of individuals (within the SFHA) subject to the requirement is independent from flood risk, our estimates of the price changes will not be biased by not excluding them from the sample.

demand surges. In this case, based on Equation 22, the counterfactual average renewal probability would be given by $E(\widetilde{q^1}_{i,t}) = \widehat{\alpha}$ for quarters $t \geq 1$ (where superindex 1 denotes counterfactual 1). As a result, the average treatment effect (on the treated) in quarter $t \geq 1$ can be estimated as the difference between the actual and the counterfactual values:

$$ATET_t^1 = E(q_{i,t}) - E(\widetilde{q^1}_{i,t}) = \widehat{\gamma}_t. \tag{23}$$

The model in Equation 22 can also be used to produce a more conservative counterfactual. Namely, one could assume that the renewal probabilities would have followed the pre-reform (linear) trend. In that case, the counterfactual average renewal probability would be given by $E(\widetilde{q^2}_{i,t}) = \widehat{\alpha}+\widehat{\beta}t$. This would be the case if, in the absence of RR2, we believe that the pricing scheduled would have continued evolving according to the dynamics of RR1 (combined with the pre-existing dynamics of any other relevant factors). This second approach (denoted by superindex 2) leads to the following estimator for the average treatment effect:

$$ATET_t^2 = E(q_{i,t}) - E(\widetilde{q^2}_{i,t}) = \widehat{\gamma}_t - \widehat{\beta}t. \tag{24}$$

In words, $ATET_t^2$ is given by the deviation relative to the continuation of the pre-existing linear trend in post-reform quarters $t \geq 1$. Clearly, if there is no pre-reform trend ($\hat{\beta} = 0$), both counterfactuals will deliver the same treatment effects.

Both counterfactual scenarios are meaningful, differing mostly in whether we envision a situation where the pricing schedule would have remained frozen in time or would have continued evolving as in the two years prior to RR2. But, as we shall see, the second approach delivers more conservative estimates of the effect of the reform on average renewal probabilities.

To conclude our discussion of the identification of the effects of the reform on renewal probabilities, we return to the limitations arising from lacking a control group. Mainly, we cannot rule out confounding effects of unobserved shocks, perhaps to household income or disaster-driven insurance demand, that could have coincided with the adoption of RR2 and generated deviations from pre-existing trends.

We partially address this challenge in two ways. First, in addition to estimates based on the nationwide sample, we will also provide estimates for several regional markets. Finding a similar evolution of post-reform renewal probabilities in all regional markets would provide evidence against the presence of *regional* confounding factors. Secondly, in the spirit of regression-discontinuity designs, we will also present estimates of the effects of the reform for a 10-day window around the implementation of RR2. The focus on such a narrow time window shrinks down the likelihood of coincidental shocks to factors that affect renewal decisions. In this setup, the natural counterfactual for the first 10 days of implementation of RR2 is that the renewal rates would have remained at the levels observed in the 10 days prior to the reform.[30]

### 5.3 Estimates for changes in renewal probabilities

We analyze the effects of RR2 on renewal probabilities using quarterly data spanning 3 years: the 8 quarters prior to the reform (fy20 and fy21) and the 4 quarters since RR2 was implemented (fy22).[31]

We estimate versions of the model in Equation 22 and report the estimates in Table 6. Column 1 estimates the simplest version of the model, which neither includes a pre-trend nor allows for time-varying post-reform effects. The model simply estimates the pre-reform and post-reform average renewal probabilities, allowing for differences within the SFHA and outside of it. The average renewal probability estimated for the 9 quarters prior to RR2 was 0.85 in the periphery (0.15 probability of exit) and 4 percentage-points higher in the SFHA, possibly due to the much higher prevalence of the insurance purchase mandate in the 100-year flood zone. When RR2 is adopted, we estimate large drops in the average renewal probabilities in both risk zones over the 4 post-reform quarters: a 4.7 percentage-point drop in the periphery and a 4 percentage-points drop in the SFHA. As discussed above, interpreting these estimates as causal requires strong assumptions, namely, a "flat" counterfactual (i.e. assuming the average renewal rate remains constant and equal to its value in the last quarter prior to the reform).

To address these shortcomings, column 2 extends the model by including a linear trend for the pre-reform period. As shown in the table, when we consider the pre-reform quarters, we find evidence of downward trends in renewal probabilities: both in the SFHA and its periphery, the average renewal rate fell by about 0.3 percentage points per quarter (with a slightly steeper slope in the periphery), implying a reduction of about 1.2 percentage points per year. In addition, in the last quarter in which premiums were based on RR1 pricing ($t = 0$), the average renewal probability was 0.83 in the periphery and almost 0.88 in the SFHA. When we consider the 4 quarters since RR2 was implemented, we estimate a 3.2 percentage-point reduction in the average renewal probability in the periphery and a 2.7 percentage-point reduction in the SFHA *relative to the last pre-reform quarter*. Importantly,

[30] Hausman and Rapson (2018) review the large number of studies based on the regression discontinuity in time methodology and discuss several implementation challenges, such as the lack of cross-sectional variation and too wide time windows. Our application of this methods seeks to mitigate these problems.

[31] RR2 was phased in gradually. Starting in October 2021, there was an interim period during which new policies (for entrants) were already priced under RR2 but renewals were priced under the old system. The full implementation of RR2 began in April 2022.

the reason these estimates of the effects of the reform are lower than in column 1 is because of the downward trends in renewal rates in the pre-reform period.

Column 3 further enriches the specification to allow for differential trends within the SFHA, separating policyholders with (pre-FIRM) subsidized rates and those with (post-FIRM) regular rates.[32] As before, we estimate downward linear pre-trends for the three groups: with quarterly reductions of 0.4 percentage points per quarter in the periphery and for SFHA policyholders with regular rates, and 0.2 percentage points for SFHA policyholders with subsidized rates. As was the case in column 2, the adoption of RR2 lowered the average renewal probability by 3.2 percentage points in the periphery. There were also reductions for the two groups of policyholders in the SFHA: 2.2 percentage-points for regular policyholders and 5.7 percentage-points for subsidized policyholders, suggesting that the latter experienced a much larger premium increase than the other two groups when RR2 was introduced.[33] Thus, as was the case with the 2012-2014 NFIP reforms, a major impact of RR2 is bringing the premiums for pre-FIRM constructions in line with the (revised) property-level flood risk estimates.

Column 4 allows us to test an implication of our theoretical model. As illustrated by the pink triangles in Figure 3, the adoption of RR2 is expected to trigger exit among policyholders with high risk levels (and thus high insurance prices) relative to others in their same zone. In the model, the policyholders who exit would like to continue purchasing insurance but cannot afford the increase in premiums. To test this prediction, we include in column 4 controls for lagged insurance prices (in logs) both before and after RR2, restricting the sample to full-risk rate policies (89% of the sample). The estimates are negative in both cases, implying that exit is systematically higher among policyholders paying relatively high premiums (vis-a-vis others in the same risk zone), consistent with the theoretical predictions (Figure 3). In other words, the reform induced *selective exit*.

Columns 5-7 report estimates for the three important regional flood insurance markets: the states of Florida, Texas and New York. In all three markets, we find downward-sloping pre-reform trends that are more pronounced in the periphery than in the SFHA. However, the estimates also reveal that the pre-reform quarterly reductions in average renewal rates were much larger in Florida and Texas than in New York.[34] When RR2 was implemented,

[32]As reported in Table 4, about 22% of the policies in the flood zone ($A$) were subsidized (mostly because of pre-FIRM construction) whereas the corresponding number for the periphery ($X$) was 0.5%. It is also worth noting that not all properties with pre-FIRM construction experienced a rate increase when RR2 was adopted. We show below that many experienced large reductions.

[33]In terms of renewal probabilities in the last quarter before RR2 came into effect, the average renewal probability in the periphery remains unchanged at 0.83. For policy holders in the SFHA with subsidized and regular and rates, the corresponding baseline values were 0.83 and nearly 0.89, respectively.

[34]Presumably, premiums and flood risk in 2020 were already more aligned in New York than in the other

average renewal probabilities fell in the three markets, but the magnitude of the changes varied across regions and segments of the market. The reduction in renewal probabilities was more muted in Florida than in the other two states: about 1.6 percentage points in the periphery and for regular policies in the SFHA in Florida compared to reductions more than twice as large in Texas and New York. In addition, the reductions in average renewal probabilities were substantially larger for subsidized SFHA policyholders (relative to the other segments of the market) in all three regions. In sum, the qualitative changes in average renewal probabilities were similar across the three regions, suggesting that these changes were driven by a nationwide shock, such as the premium changes ushered in by RR2.

Next, we turn to the estimation of the more general version of Equation 22, which includes flexible post-reform trends (through quarter dummy variables) that are allowed to vary for each of the three segments of the market, namely, policyholders in the periphery, in the SFHA facing regular rates, and those in the SFHA with subsidized rates. The estimates are presented in Table E.1 but also depicted in Figure 7 (for the nationwide sample). The top panel presents the estimates for the periphery. The blue stars trace the linear trend estimated on the basis of the pre-reform quarters and extended through the post-reform period to facilitate the visualization of the counterfactual no-reform scenario. The solid line plots the average renewal probability for this risk zone estimated at a quarterly frequency for the whole 3-year period (with the corresponding 95-percent confidence interval). Several points are worth noting. First, the linear trend is a reasonably good approximation to the evolution of the average renewal probabilities in the pre-reform period, despite small fluctuations around the trend. More importantly, at the time of the implementation of the reform ($t = 1$), the value of the average renewal probability falls sharply and continues its downward trend. Clearly, relative to either of the two counterfactuals discussed in subsection 5.2, the estimated average treatment effects (ATET) of the reform will be negative, as we discuss below.

Turning now to the middle panel in Figure 7, we also find that post-reform average renewal probabilities in the SFHA among regular (full-risk rated) SFHA homeowners fall below the counterfactual values (based on either of the two approaches discussed earlier). But, by far, the largest impact of the reform is found among SFHA homeowners with subsidized policies. As depicted in the bottom figure, despite a small initial drop at the implementation of the reform, the average renewal probability plummeted 3 quarters after the implementation by over 10 percentage points.

Using these estimates and the no-reform counterfactuals, we can estimate the average treatment effects (ATET) of the reform during the 4 quarters after its implementation. As

two markets, resulting in smaller annual increases in premiums.

reported in Table 7, the ATET for the first quarter after the implementation of RR2 ranges between a 3.5 and a 3.2 percentage-point drop in the average renewal probability in the periphery (top panel), according to counterfactuals 1 and 2, respectively. The effect of the reform is somewhat muted in quarters 2 and 3, but attains its higher value in quarter 4. Averaging the 4 quarters, we estimate an ATET ranging between a 2.3 and 3.3 percentage-point reduction in the average renewal probability in the periphery. Turning to regular (full-risk rated) SFHA homeowners, we estimate a lower ATET, ranging between a 0.8 and a 1.7 percentage-point reduction on average over the four post-reform quarters (middle panel). As expected on the basis of the estimates in Table 6, the highest ATET (in absolute value) is found among SFHA homeowners with subsidized-rate (pre-FIRM) policies, which ranges between a 6.2 and a 6.7 reduction in the average renewal probability, depending on which counterfactual is adopted.

As a robustness check on the trend-based approach to causality, next we adopt a different causal inference approach inspired in regression discontinuity designs. We restrict the analysis of renewal decisions to a narrow time window around the implementation of RR2, which took place on 4/1/2022. Specifically, we consider renewal decisions made in the 10 days immediately before or immediately following the implementation of the reform. The short time separating the two periods suggests that the causal effect of the reform will be identified by the change in the outcome of interest (subject to the caveats discussed in Hausman and Rapson (2018)).

As reported in Table E.2, periphery residents or SFHA residents with regular policies (i.e. paying full-risk rates) making renewal choices within the first 10 days after the reform was introduced were 3.3 percentage-points less likely to renew coverage than homeowners (residing in the same risk zone) in the 10 days prior to the reform (columns 1 and 2). Somewhat differently, the analogous estimated change in average renewal probability for SFHA policyholders with subsidized rates was 1.85 percentage-points.[35] These estimates are remarkably consistent with those reported in Table 7 for the *first post-reform quarter* for residents of the periphery (3.2-3.5 percentage-point reduction) and for SFHA residents with subsidized rates (1.5-1.8 percentage-point reduction). Only for SFHA residents with regular policies do we find a discrepancy between the two estimates. More specifically, the trend-counterfactual estimated ATET for the first quarter after the reform for this group of policyholders ranged between a 1.8 and a 2.2 reduction in the average renewal probability, which amounts to a 1 to 1.5 percentage points discrepancy in the effect of the reform. All in

[35] The estimates in column 3, based on a plus/minus 20 days around the adoption of the reform, depict the same result, with only slightly lower estimated effects of the reform. But one must keep in mind that as we use data further away from the implementation of the reform, there is an increased chance of confounding factors.

all, the estimated effects of the reform under both causal inference approaches are in broad agreement, supporting a causal interpretation of the estimates discussed above.

In conclusion, our estimates clearly show a sudden and sustained drop in average renewal rates, particularly in the periphery and among SFHA residents with subsidized rates, starting precisely in the quarter that RR2 was implemented. In light of previous findings documenting that the demand for flood insurance responds to price changes (Bradt et al., 2021; Wagner, 2022), it seems highly plausible that the reduction in renewal rates (and corresponding increase in exit) was a response to reform-driven increases in average premiums. Moreover, we documented that exit was selective: the homeowners that chose to let their coverage lapse had been paying substantially higher premiums immediately prior to the adoption of the reform. In the context of our model, raising premiums to actuarially fair values only triggers exit due to a binding affordability constraint but, more generally, this response could also reflect underestimation of flood risk by those homeowners.

# 6 Effects of RR2 along the intensive margin

The analysis in the previous section provided strong evidence of a reform-induced increase in exit from the flood insurance market. In theory, there are various factors that could account for this finding. Foremost among them, the increase in exit can be rationalized by increases in average premiums. This section seeks to provide corroborating evidence for this hypothesis. However, doing so requires addressing a fundamental measurement challenge: insurance prices (as well as other policy attributes) at a point in time are only observed for homeowners who actually purchased insurance. Furthermore, imputation of missing values using lagged prices would be at odds with the evidence above of *selective exit*: policyholders already paying high premiums were more likely to exit the market when RR2 was implemented.

Our approach will be to restrict the analysis to the subsample of *always buyers*, defined as policyholders that continuously held flood insurance before and after the reform was implemented. Clearly, this is likely to be a strongly selected sample and the resulting estimates cannot be extrapolated to the overall population of homeowners. Nonetheless, the analysis is still informative in various ways. For instance, under mild assumptions, it is plausible to consider the average insurance price experienced by always buyers as a *lower bound* for the average increase over the set of all homeowners. Additionally, restricting the analysis to always buyers allows us to measure the reform-driven shift in the pricing schedule, albeit only for a subset of all homeowners. Incidentally, identifying always buyers in the NFIP data relies crucially on our dataset of individual insurance histories.

Besides analyzing the effects of the reform on insurance price changes, we will also examine its effects on two other policy attributes: coverage amount and the size of deductibles, both of which offer policyholders a way to mitigate the overall impact of insurance rate increases on the overall cost of insuring against flood risk. We begin this section by estimating the intensive-margin effects of RR2 on insurance prices.

## 6.1 Insurance prices

When FEMA announced the introduction of RR2, it highlighted that while some policyholders would experience premium increases, others would benefit from lower premiums. The pioneer study by Mulder and Kousky (2023) provided early evidence on the heterogeneous effects of RR2 on the premiums paid by policyholders (who purchased insurance *both* immediately before and after RR2 was adopted). In particular, they found that RR2 lowered average premiums in the flood zone but increased them elsewhere.

Our analysis here goes beyond their study in several important ways. First, we focus on coverage-adjusted insurance prices, rather than annual premiums, which provides a cleaner picture of the effects of the reform on the pricing schedule. Secondly, we analyze the effects of the reform on two premium-relevant outcomes (coverage and deductibles). Third, we explicitly define counterfactual (no-reform) scenarios, which arguably allows us to identify the causal effects of the reform (for always-buyers). Last, we disaggregate SFHA policyholders into those paying full-risk rates (regular policyholders) and those enjoying subsidized rates (due to pre-FIRM construction of their homes).

In order to observe how the reform affected the pricing schedule, we restrict the analysis to *always-buyers*, defined as the subsample of individuals who *continuously* purchased insurance in the last two rounds of RR1 (2020Q1-2022Q1) *and* in the first post-reform year (2022Q1-2023Q4). Obviously, for these individuals we observe all policy attributes (including premiums) for the three years of interest. It is worth stressing that the selected nature of the always-buyers sample likely severely underestimates the average price increases for the overall population of homeowners.

We define *insurance price* by $p = 1,000 \times PolCost/InsTot$, which provides the price per $1,000 of coverage. Our analysis of the effects of the reform on the insurance prices paid by always-buyers is based on the same model used to investigate the effects on average renewal probabilities, (Equation 22), which includes a pre-reform linear trend and a flexible post-reform trend. Besides replacing the dependent variable, which now becomes the log of the insurance price, we now add *policyholder fixed-effects*. As a result, our estimates of the (log) price changes around the implementation of the reform net out time-invariant differences in characteristics that produce price differences across the three segments of the market we consider: periphery policyholders, SFHA residents with regular policies and SFHA residents with subsidized policies.[36]

Our analysis is based on the empirical approach described in subsection 5.1, which explicitly accounts for pre-reform trends, where the estimating equation now also includes homeowner fixed-effects and the dependent variable becomes the log of the insurance price. The estimates are reported in Table E.3 (and are based on the subsample of always buyers). Column 1 considers a simpler version of the model that estimates the average change in the dependent variable over the first year of implementation. The estimates indicate that insurance prices in the periphery were about 7 percent higher than immediately before the reform was introduced. The increase was roughly 50 percent smaller for SFHA-policyholders with regular policies. In stark contrast, conditional on renewing coverage, the average price for subsidized SFHA-policyholders fell by roughly 40 percent around the implementation of the reform. Interestingly, the estimates in columns 3-5 show that the same qualitative pattern is found for some of the main regional markets (Florida, Texas and New York).

As discussed in the previous section, pre-existing trends in insurance prices can confound the effect of the reform. Indeed, the estimates in column 1 provide evidence of a statistically and economically significant *upward* trend in insurance prices in the *periphery* in the years leading up to the reform, but a much gentler pre-trend in the SFHA (both for regular and subsidized policies), confirming the pattern found by Mulder and Kousky (2023). To isolate the causal effects of the reform, we next construct counterfactual no-reform scenarios and measure the size of the price changes that correspond to departures from pre-reform trends. Columns 1-2 in Table 8 report estimated ATET of the reform on flood insurance prices. Using our most conservative estimates, based on the extrapolation of pre-treatment trends, we estimate that RR2 increased the average (coverage-adjusted) insurance price in the periphery of the flood zone by 2.1% and by a slightly higher amount (2.3%) for full-risk SFHA policies. In contrast, the reform *lowered* the average price of subsidized SFHA policies by more than 33%.

It is important to keep in mind that these estimates refer to always-buyers and, in all likelihood, provide lower bounds for the average price increases for the overall population of homeowners. From this point of view, the estimated lower bounds strongly suggest that the

[36]The reason we did not include policyholder fixed-effects in the renewal analysis is that the dependent variable is conditional on having purchased insurance in the previous period. In particular, we identify a renewer as someone who purchased insurance in two consecutive years and an 'exiter' as someone who purchased in the previous period but is currently not purchasing again. Thus, in a sense, an exit is identified as a within-individual change in purchase status and, therefore, nets out the effects of time-invariant policy factors.

reform increased prices in the periphery and for regular SFHA policies, which provides an intuitive explanation for the reductions in renewal rates (i.e. increases in exit) discussed in subsection 5.3 for these market segments. However, the estimated lower bound for the price increase experienced by subsidized SFHA policyholders is rather uninformative regarding the large reduction in renewal rates (upward of 6 percentage points) plausibly caused by the reform. As we explain below, the explanation of the puzzle rests on the vastly heterogeneous effects of RR2 on the premiums of pre-FIRM policyholders, reflecting dramatic revisions in the flood-risk estimates for these properties (tied to differences in property elevation).

## 6.2 Within-individual price changes for always-buyers

Having analyzed the *average* effects of the reform, we now turn to examine the degree to which the reform had individually heterogeneous effects *within* each risk zone. We continue our focus on always-buyers and construct the CDFs for the within-individual year-on-year price changes in the years immediately before and immediately the implementation of RR2.

Let us consider first the price changes in the *periphery*. Figure 8 depicts a large increase in the dispersion of year-on-year price changes since the implementation of RR2: many policyholders enjoyed substantial price reductions while others experienced large increases. In fact, the share of periphery policyholders hit with increases above 10% increased from about 10% prior to the reform (blue line) to about 75% when RR2 came into effect (orange line).[37]

Turning now to the SFHA, Figure 9 plots the CDFs for the year-on-year price changes in the last round of RR1 (blue line) and in the year in which RR2 was implemented (orange line). The most striking finding is the large increase in the mass of policyholders receiving large price reductions: almost 20% of policyholders enjoyed *reductions* upward of 10% (0.10 log points). In comparison, fewer than 5% of policyholders saw such price reductions prior to the adoption of RR2. Additionally, the figure also shows that the share of policyholders experiencing large price increases (above 10%) also increased substantially (from about fewer than 10% of policyholders to approximately 60%). Hence, the reform also had highly heterogeneous price effects across individual properties within the SFHA.

As previewed in subsection 4.2, to a large extent, heterogeneity within the SFHA may be linked to policies covering pre-FIRM constructions. In fact, when we exclude subsidized (pre-FIRM) policyholders, we observe a substantial reduction in the mass of policyholders receiving large discounts when RR2 was implemented (Figure 10). Thus, the modernization

[37]Many of the periphery policyholders experiencing large premium increases when RR2 was implemented that were capped at the statutory 18% annual increase. These policyholders will probably experience further price increases in the years to come until their premiums become actuarially fair.

of the risk estimation methods and the switch to individualized pricing seems to have corrected important mispricing for a subset of policies. Specifically, it appears that a number of pre-FIRM constructions were *overpaying*, relative to their actual flood risk, despite enjoying 'subsidized' rates. This pricing anomaly had already been pointed out by Sherman and Kousky (2018). According to these authors, premiums under RR1 for pre-FIRM houses were computed using rates that did not take into account elevation because it was unknown for most of these constructions. An important innovation introduced by the RR2 reform is the availability of individual elevation estimates *for all* properties. As a result, even though all pre-FIRM properties are now subject to the regular (full-risk) pricing schedule under RR2, those with *high elevation* have ended up paying much lower premiums than was the case under RR1, and are likely to make up the bulk of the *always buyers*. It is also worth noting that not all pre-FIRM properties have experienced premium reductions due to RR2. In fact, those with *low elevation* have lost out with the reform due to the elimination of subsidized rates and, possibly, an upward revision of their flood risk. It is very likely that many of these policyholders were not willing or able to afford the higher premiums and chose to exit the insurance market.

Summing up, the empirical estimates presented in this section underscore that RR2 had highly heterogeneous effects on individual insurance prices *within* each risk zone. The degree of heterogeneity is particularly high among subsidized (pre-FIRM) SFHA properties, even conditional on our sample of always buyers. Presumably, those experiencing the largest prices increases exited the market and are not part of the figures just discussed. As we found in Section 5, the implementation of RR2 triggered substantial exit, particularly in the periphery and among SFHA residents with subsidized rates. Relative to the no-reform counterfactual, annual exit rates in these segments of the market increased by 2.3 and 6.2 percentage-points, respectively.[38] In comparison, exit rates for SFHA residents with regular policies increased by less than 1 percentage point.

## 6.3 Coverage and Deductibles

Policyholders confronted with rising insurance prices might have sought to counteract those hikes by adjusting policy attributes, such as coverage and deductibles. Conversely, those enjoying premium reductions may have responded by upgrading their policies. Put otherwise, it is plausible that coverage and deductible choices are price-elastic. As far as we know, the literature has mostly focused on the estimation of the price elasticity of the decision to purchase insurance and virtually no attention has been paid to the estimation of the corre-

[38]These are substantial effects given that pre-reform annual exit rates were around 16% (in the periphery and for pre-FIRM policyholders) in the quarter preceding the implementation of RR2.

sponding elasticities for coverage and deductible choices. This is potentially an important omission given that changes along these dimensions could have important implications regarding the flood risk exposure of *insured* homeowners: reductions in coverage and increases in deductibles effectively increase the financial exposure of policyholders to flood risk.

To examine whether the RR2 reform led policyholders to adjust policy attributes, we estimate the same empirical specification just used to investigate the intensive-margin changes on insurance prices, simply changing the dependent variables to the log of total coverage and the log of the building deductible. Importantly, we are still restricting the sample to always buyers, defined as policyholders who continuously purchased insurance in the last two rounds of RR1 (fy20 and fy21) and in the first round of RR2 (fy22).

Table 8 reports the estimated ATET of the reform on the choices of coverage (columns 3-4) and deductible (columns 5-6). Two results stand out. First, the effect on the choice of coverage amount appears to be very small, well below one percent for (always buyers) in the periphery or in the SFHA with regular policies. The effect of the reform on coverage choices for subsidized policyholders is about 4 times larger than in the other market segments (about 2%) but, given the very large price reduction experienced by these policyholders, still entails a very low price elasticity.

Turning to the choice of deductible, the estimated effects in columns 5-6 reveal a much higher elasticity. On the basis of the more conservative estimate of the ATET of the reform (for always buyers), we find average changes of 12% and 5.6% in the periphery and for regular policies in the SFHA, respectively. As reported earlier, the average effects of the reform on coverage-adjusted insurance prices for these market segments were around 2%, which results in price-elasticities around 6 and 3, respectively. Put differently, while the choice of deductible is appears to be highly responsive to price changes (conditional on maintaining coverage), the corresponding elasticity is much higher among periphery homeowners than for SFHA residents with regular (full-risk) policies. The bottom panel of the table reports the ATET of the reform for subsidized policyholders in the SFHA. As we saw in columns 1-2, conditional on maintaining coverage, the reform lowered average insurance prices by 33%. The estimates in columns 5-6 reveal that policyholders choose to take advantage of this opportunity by lowering deductibles by about 2%. This implies a very low elasticity, perhaps reflecting a binding lower bound on the available deductible choices. It is also worth noting that we find practically flat pre-reform trends in deductibles in the three segments of the market, further reinforcing that the post-reform changes just described were caused by the implementation of RR2.

In sum, our estimates show that some premium-relevant policy parameters are price-elastic. In particular, we found that the adoption of RR2 led to average increases in deductibles for the segments of the market that experienced reform-induced increases in insurance prices, whereas the converse happened in the segment that experienced a price reduction. It is worth noting that this behavior may be a reflection of homeowners' greater willingness to bear flood risk in exchange for lower annual insurance costs.

# 7 RR2 and Entry Decisions

A complete analysis of the effects of RR2 along the extensive margin of insurance demand also requires examining entry decisions by homeowners who had not purchased insurance in the past year, which we refer to as new entry (or re-entry). Unfortunately, our data is not suitable to analyze the entry decision at the individual level, as we did with the renewal-exit decision, because our data does not contain homeowners that never purchased insurance over our period of analysis.[39] Similar to Bradt et al. (2021), our analysis in this section will be based on the estimation of a county-level model for annual entry flows, considering separately the SFHA and its periphery. Importantly, we need to keep in mind that RR2 applied to new entrants from 2021Q4 onward, whereas it only affected renewals from 2022Q2. Thus we relabel quarters so that the first post-reform quarter is now 2021Q4. Since the reform, new entrants into the flood insurance market (or those re-entering after having let their policies lapse in the past) are charged full-risk rates.

Using our panel of individual insurance histories, we decompose annual purchases into renewals ($b_{i,t-1} = b_{i,t} = 1$) and new entry ($b_{i,t-1} = 0, b_{i,t} = 1$). As illustrated in Figure E.1 and Figure E.2, new entry is only a small fraction of all purchases (about 15%) in both risk zones. Two further observations stand out in these figures. First, quarterly entry flows (as well as renewals) exhibit a strong seasonal pattern, which will need to be accounted for in our empirical specification. Secondly, the first period of implementation of RR2 for new entry (labelled interim in the figures) suggests a reduction in entry in the periphery but an increase in the SFHA, although the magnitudes of both changes are relatively small.

To carry out a more formal analysis of the effects of the reform, we estimate a model analogous to those used for the analysis of renewal decisions and policy attribute choices, which includes a linear trend for the pre-reform period, and quarter-specific dummy variables. In addition, the model also includes seasonal dummies, county fixed-effects, and is estimated separately on entry counts in each county's SFHA and entry counts outside of this zone. The estimates are reported in Table E.4 but it is more informative to focus our attention on the

[39] This is most clearly seen in the context of 2-year insurance histories. For any two pair of years $(t-1, t)$, our data contain histories $(b_{i,t-1}, b_{i,t}) \in \{(1,0),(1,1),(0,1)\}$ but $(0,0)$ histories are missing. As a result, conditional on $b_{i,t-1} = 0$, our data implies that $b_{i,t} = 1$, resulting in a degenerate likelihood function.

estimated ATET in Table 9. The top panel reports the estimates for new entry *outside* of the SFHA, relative to the two no-reform counterfactuals discussed in subsection 5.2. In the last quarter prior to the reform, the average county (with positive flood insurance policies over the sample period) had a seasonally adjusted quarterly new entry of about 12 policies for the average county. Our estimates show that, on average, over the 4 post-reform quarters, annual entry fell by between 46% (counterfactual 1) and 26.7% (counterfactual 2).[40]

Let us now turn to the estimated average effect of the reform on new entry in the SFHA.[41] The average quarterly entry in the SFHA immediately prior to the reform was 14.5 new policies (or 58 policies annually) for the average county. The estimates in the bottom panel of the table indicate that the reform led to an *increase* in entry, ranging between 17% and 18% of the baseline (pre-reform) value.[42]

In order to assess the effect of the RR2 reform on the aggregate *net exit*, Table 10 combines our estimates of the ATET for exit (Table 7) and entry Table 9. The top panel summarizes the estimated average effect on exit flows, on the basis of the estimated effects on renewal rates, separately for the SFHA and its periphery. As discussed earlier, the RR2 increased exit in both risk zones by about 2% on the basis of the linear-trend counterfactual. Relative to the pre-reform number of residential policies, this amounts to an approximately 53,000 *increase* in exit (column 2), relative to the pre-reform annual exit flow.

The middle panel reports the estimated increase in entry, which we found to be negative in the periphery (26.6% reduction) and positive in the SFHA (17.4% increase). These figures amount to a 74,000 annual reduction in entry and a 45,000 annual increase in entry in the periphery and SFHA, respectively, on the basis of the linear-trend counterfactual. Combining both risk zones, these estimates entail a 29,000 *reduction* in annual entry, relative to the pre-reform annual entry flow.

Last, the bottom panel combines the annual estimates for the changes in exit and entry. While the periphery experienced a substantial increase in net exit (of 117,000 policies), the SFHA experienced a smaller reduction in net exit (of 34,000 policies) based on the linear-trend counterfactual. The larger outflow experienced by the periphery is not surprising,

[40]To maintain symmetry with our analysis of the effects of the reform on exit-renewal decisions, the ATET in Table 9 average of the 4 quarters following the implementation of the reform. However, Table E.4 reports estimates for the 6 quarters following the reform to allow for an alternative computation over a longer post-reform period.

[41]It is worth pointing out that since the implementation of RR2, the NFIP dataset does not report pre-FIRM (subsidized) status, presumably because insurance rates for these properties are now computed using the (new) regular pricing formula. As a result, we are not able to disaggregate the SFHA into regular and subsidized policyholders.

[42]Table E.4 also reports separate estimates for the states of Florida, Texas and New York, which reveal large reductions in annual entry in Florida, both in and out of the SFHA, and moderate increases in new entry in New York.

given that insurance purchases outside of the SFHA are largely voluntary and premiums increased substantially with the adoption of RR2. Adding the values for both risk zones, and adopting our most conservative estimates of the ATET, we conclude that the reform increased annual net exit by about 83,000 policies, which amounts to an additional net loss of 2.3% of the baseline number of policies. It is also worth noting that this amount would have been twice as high if we had not taken into account the pre-existing trends in entry and exit (column 1).

In conclusion, our analysis suggests that the RR2 reform increased exit by a substantial amount over the first year of implementation. The reform also lowered entry flows, exacerbating the decades-long decline in NFIP enrollment.

# 8 Conclusions

We have developed a simple theory of the demand for flood insurance in a setting where risk-averse homeowners differ in household income and flood risk, and used this framework to analyze the effects of the adoption of *Risk Rating 2.0* (RR2) on enrollment in the NFIP program, and insurance prices. The theoretical model provides a useful guide to the empirical analysis and makes clear that the overall effects of the reform are theoretically ambiguous and crucially depend on how the reform affects entry and exit into the insurance market, in turn determined by the joint distribution of household income and flood risk. The theoretical analysis also highlights that income (and borrowing) constraints play an important role in shaping insurance demand and its effects on overall revenue and social welfare. In particular, the theory predicts that the adoption of RR2 will lead to exit and reduced entry among relatively high-risk homeowners, but induce entry among relatively low-risk homeowners, both in the flood zone and its periphery. The model also predicts heterogeneous effects on the premiums paid by policyholders who were purchasing insurance prior to the reform and continue doing so after its adoption, both within the 100-year flood zone and outside of it.

To conduct our empirical analysis, we extend the approach introduced by Mulder and Kousky (2023) and build a dataset containing individual insurance histories for the whole United States for the period 2019-2023. We then proceed to estimate the effects of RR2 along the extensive and intensive margins of the demand for flood insurance, relative to no-reform counterfactuals that take into account pre-existing trends in outcomes.

Our empirical findings provide support for most of the predictions derived from our theoretical analysis. Most notably, we show that RR2 generated substantial exit from the insurance market, both in the SFHA and its periphery. We also document highly heterogenous changes in insurance costs and *selective exit*: policyholders that did not renew their policies were paying, on average, substantially higher premiums than policyholders located in the same risk zone that chose to continue purchasing flood insurance. Our theoretical model explains the pattern of selective exit (when premiums are actuarially fair) in response to price increases by relying on binding income/credit constrains.

Our analysis of the subset of homeowners that continuously purchased insurance before and after the reform (*always buyers*) shows that policyholders vigorously adjusted the deductibles in their insurance policies in response to the changes in insurance prices to either mitigate the effect of price increases on annual premiums, or to take advantage of price reductions. Thus, many homeowners appear to be willing to bear higher financial exposure to flood risk in exchange for lower premiums. This suggests that the reform may have increased homeowners' incentives to adopt mitigation actions to reduce future flood damages (de Ruig et al., 2022).

Nonetheless, the RR2 reform appears to have exacerbated the decades-long decline in NFIP enrollment, pushing many (high-risk) homeowners out of the insurance market. Further reforms will be needed in order to increase program participation. Future research should explore whether insurance purchase mandates and income-based pricing could lead to more financially resilient floodplain communities (Wagner, 2022; Horn, 2023).

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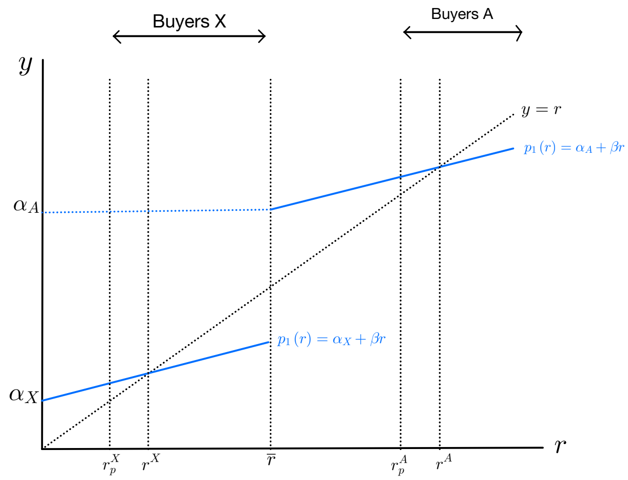
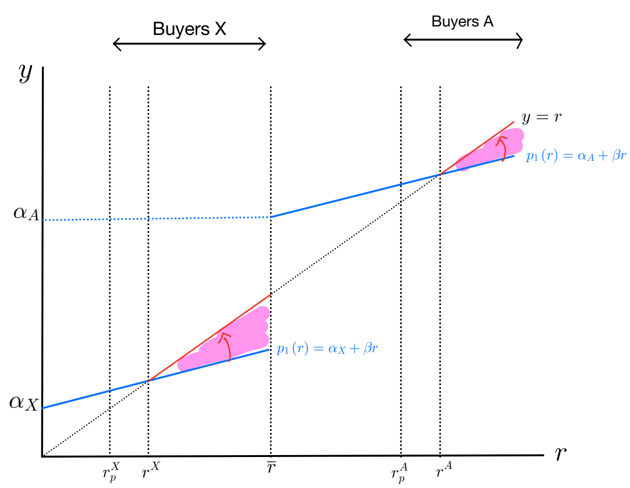
# Tables and Figures

Figure 1: Optimal decisions under RR1



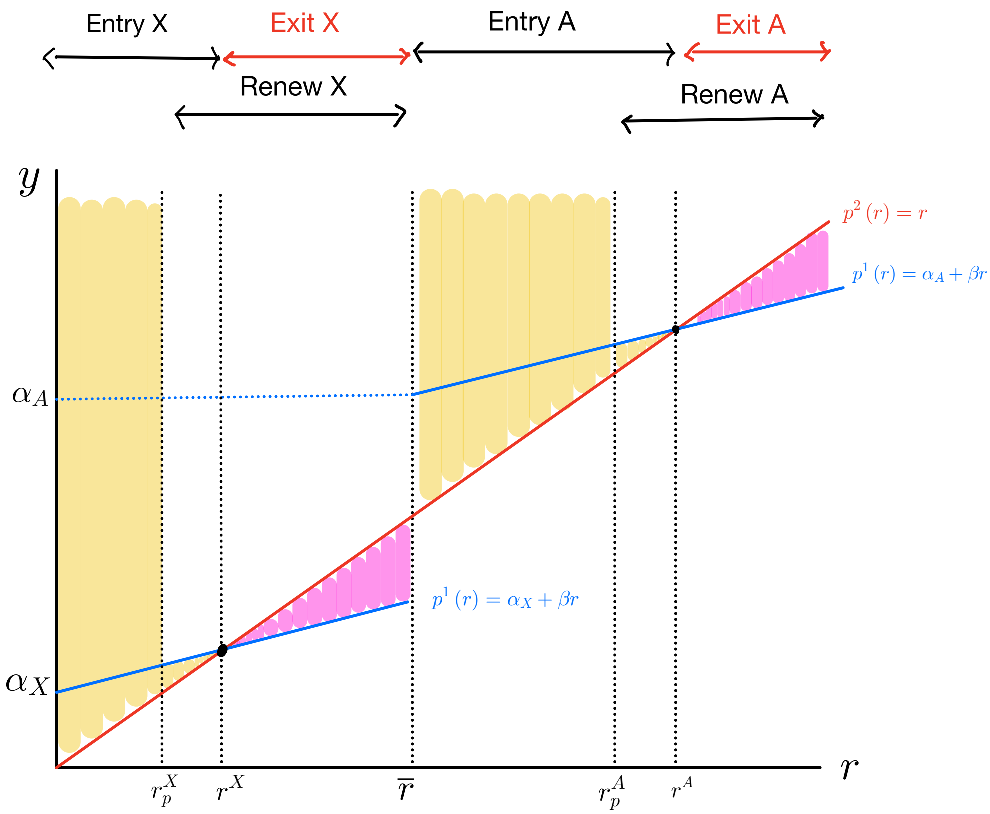

**Notes:** Flood risk ($r$) is measured in the horizontal axis whereas residual household income (after netting out non-insurance expenses) is measured in the vertical axis ($y$). The blue lines refer to the price of insurance (premium) for a property with flood risk $r$. The pricing function depicted in the figure is $p_1(r) = \alpha_z + \beta r$, for $z = A, X$. We assume $0 < \alpha_X < (1 - \beta)\overline{r} < \alpha_A < 1 - \beta$ and $0 < \beta < 1$.

Figure 2: RR1 with reform




**Notes:** The pink area denotes exit triggered by the change in the pricing function. The pricing function depicted is $\widehat{p}_1(r) = p_1(r) = \alpha_z + \beta r$ for $r < r^z$ and $\widehat{p}_1(r) = \widehat{\alpha}_z + \widehat{\beta} r$ for $r^z$, and $z = A, X$. We assume $0 < \alpha_X < (1 - \beta)\overline{r} < \alpha_A < 1 - \beta$ and $0 < \beta < \widehat{\beta} = 1$.

Figure 3: Entry and Exit due to RR2




**Notes:** Flood risk ($r$) is measured in the horizontal axis whereas residual household income (after netting out non-insurance expenses) is measured in the vertical axis ($y$). The blue line depicts the pre-reform pricing function ($p_1(r) = \alpha_z + \beta r$, for $z = A, X$) and the red line depicts the RR2 pricing function $p_2(r) = r$ (actuarially fair rats). The pink (yellow) area denotes exit (entry) triggered by the change in the pricing function. We assume $0 < \alpha_X < (1-\beta)\overline{r} < \alpha_A < 1-\beta$ and $0 < \beta < 1$. Area shaded in yellow is new entry and area shaded in pink is exit. Area not colored indicates homeowners that did not change their purchase decision when RR2 was introduced.

Figure 4: Distribution insurance prices. Flood zone

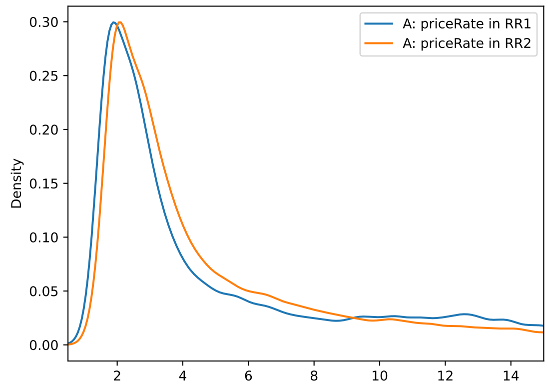



Figure 5: Distribution insurance prices. Flood zone, post-FIRM

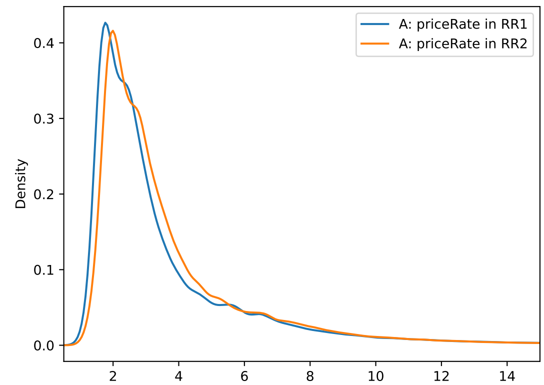
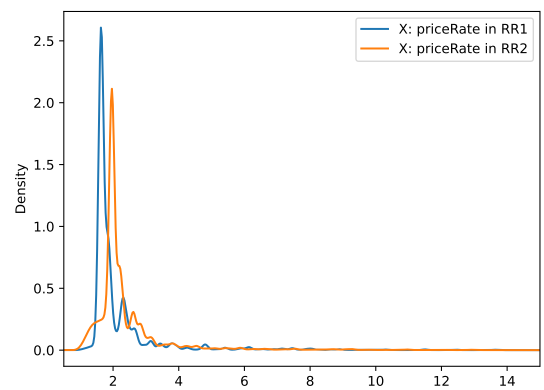
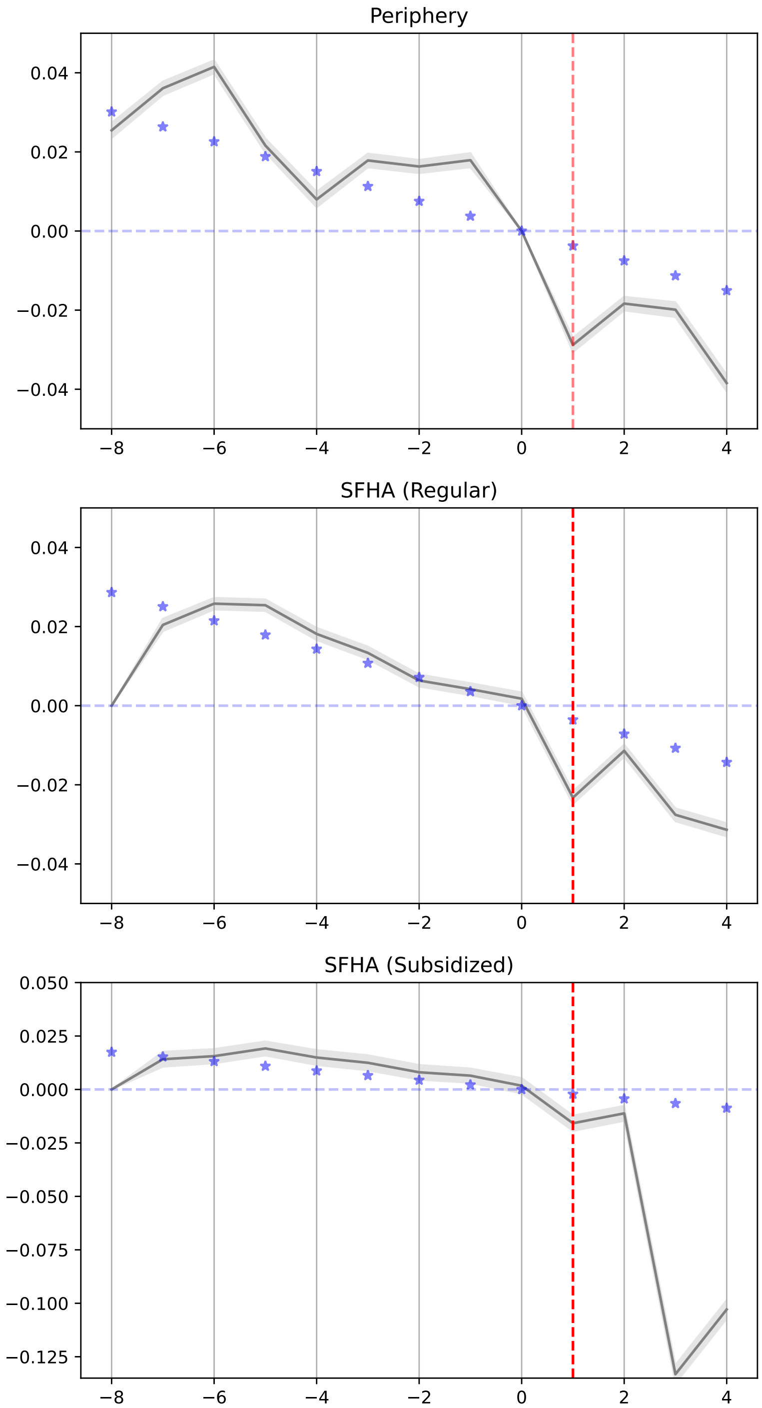



Figure 6: Distribution insurance prices. Periphery




**Notes:** The figures report the insurance price in the last round of purchases under RR1 and the first under RR2. The flood zone (FZ) is defined as flood zone designations A and V. The individual insurance price is calculated as the annual cost of a policy (premium plus fees) divided by the total coverage (of contents and building). The units are $ per $1,000 of coverage. All purchases are included (new or renewals).

Figure 7: Average renewal probability at quarterly frequency. Deviations from baseline value 2022Q1.




**Notes:** The solid lines plot the average renewal probability for the corresponding risk zone estimated at a quarterly frequency (with the corresponding 95-percent confidence interval) for the period 2020Q1 through 2023Q1 in deviations from the baseline value in the last year prior to the implementation of RR2 (2022Q1). The blue stars trace the linear trend estimated on the basis of the pre-reform quarters ($t \leq 0$) in each risk zone, extended through the post-reform period to facilitate the visualization of the counterfactual no-reform scenario. The red vertical line marks the first period after the RR2 reform was implemented ($t = 1$ or 2022Q2).

Figure 8: CDF YoY price changes around RR2 for renewers. Periphery

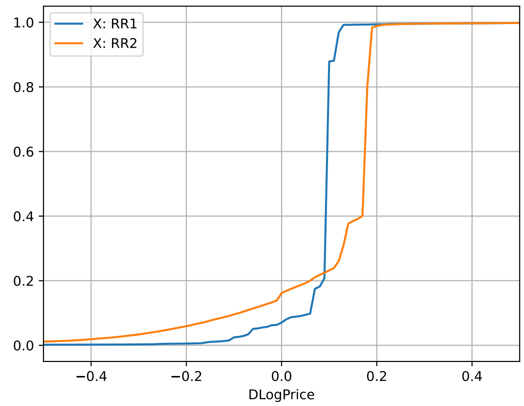



Figure 9: CDF YoY price changes around RR2 for renewers. SFHA

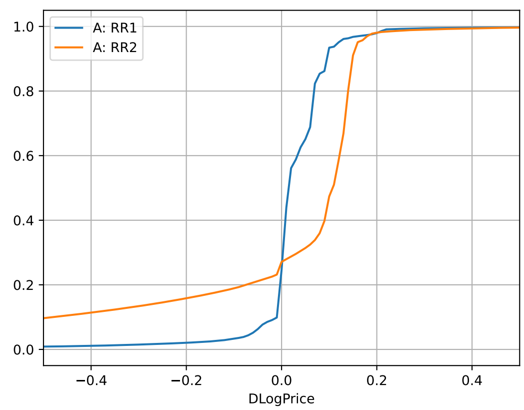
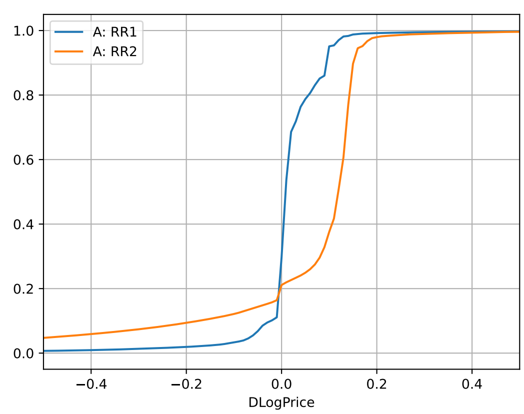



Figure 10: CDF YoY price changes around RR2 for renewers. SFHA, regular policies




**Notes:** The figures report the CDF for annual changes in the log of the insurance price between the first round of purchases under RR2 (fy21-fy22) and the last round of purchases under RR1 (fy20-fy21) for policyholders that continuously purchased insurance in fy20, fy21 and fy22. The SFHA (100-year flood zone) is defined as FEMA flood zone designations A and V. The individual insurance price is calculated as the annual cost of a policy (premium plus fees) divided by the total coverage (of contents and building). The units are $ per $1,000 of coverage. Figure 10 excludes pre-FIRM (subsidized) policyholders.

Table 1: Summary statistics NFIP policies 2019-2023. Up to 4 residential units

| | count | mean | std | min | max |
|---|---|---|---|---|---|
| 100-year Flood Zone (FZ) | 17,053,299 | 0.440 | 0.496 | 0.000 | 1.000 |
| Single Family | 17,053,299 | 0.969 | 0.173 | 0.000 | 1.000 |
| Construction Year | 17,053,083 | 1,983.166 | 24.861 | 1,690.000 | 2,024.000 |
| Primary Residence | 17,053,299 | 0.819 | 0.385 | 0.000 | 1.000 |
| Mandatory Purchase | 17,053,299 | 0.152 | 0.359 | 0.000 | 1.000 |
| Subsidized | 17,053,299 | 0.074 | 0.262 | 0.000 | 1.000 |
| Florida | 17,053,299 | 0.274 | 0.446 | 0.000 | 1.000 |
| Texas | 17,053,299 | 0.191 | 0.393 | 0.000 | 1.000 |
| BRC | 16,150,207 | 308,502.769 | 437,031.195 | 0.000 | 8,000,000.000 |
| Coverage Total | 17,053,299 | 280,013.876 | 91,194.361 | 200.000 | 2,505,000.000 |
| Coverage Building | 17,053,299 | 214,101.636 | 62,065.010 | 0.000 | 2,500,000.000 |
| Coverage Contents | 17,053,299 | 65,912.240 | 40,911.631 | 0.000 | 475,000.000 |
| Total cost | 17,053,299 | 940.901 | 952.872 | 1.000 | 58,912.000 |
| Premium | 17,053,299 | 711.159 | 798.622 | -292.000 | 49,321.000 |
| Price per $1,000 of Coverage | 17,053,299 | 4.488 | 6.742 | 0.004 | 491.000 |
| Deductible | 16,969,390 | 2,048.854 | 1,876.853 | 200.000 | 50,000.000 |

**Notes:** Nationwide NFIP policies 2019-2023 for residential buildings with up to 4 residential units. BRC stands for building replacement cost. The 100-year flood zone (FZ) is the same as the Special Flood Hazard Area defined by the FEMA flood maps and is defined as flood zone designations A and V.

Table 2: Algorithm comparison

| | (1) Groups | (2) Duplicates % policies | (3) Avg spell years | (4) Lapses % groups | (5) Chg. elev. vars. % groups | (6) SalesEndorse % groups | (7) Chg. FZ % groups |
|---|---|---|---|---|---|---|---|
| MK algorithm | | | | | | | |
| CensusTract | 71,156 | 98.02 | 3.75 | 85.15 | 43.58 | 79.77 | 55.45 |
| +OCD | 2,436,741 | 45.24 | 2.84 | 22.04 | 11.21 | 20.43 | 7.98 |
| +ONBD | 4,738,859 | 2.15 | 2.52 | 1.24 | 1.06 | 1.32 | 0.74 |
| +rFZ | 4,781,663 | 1.85 | 2.50 | 1.21 | 0.53 | 1.09 | 0.00 |
| +ElevVars | 4,817,422 | 1.35 | 2.49 | 1.18 | 0.00 | 0.84 | 0.00 |
| +EffectiveM | 4,872,852 | 1.26 | 2.46 | 0.08 | 0.00 | 0.81 | 0.00 |
| OP algorithm | | | | | | | |
| CBg+LatLon+Map+OCD+floors | 3,562,488 | 24.65 | 2.61 | 13.41 | 5.29 | 10.98 | 2.79 |
| +ONBD | 4,817,922 | 1.49 | 2.49 | 1.16 | 0.49 | 0.54 | 0.37 |
| +SaleEndorse | 4,845,509 | 1.27 | 2.48 | 1.14 | 0.38 | 0.00 | 0.29 |

**Notes:** The attributes listed in the table are census tract, original construction date (OCD), original new business date (ONBD), rated flood zone status (rFZ), changes in elevation variables (base flood elevation, lowest floor elevation, lowest adjacent grade), month policy becomes effective, census block group (CBG), latitude and longitude (one decimal), flood map number, number of floors, and house sale with flood insurance policy endorsement. The Mulder & Kousky (MK), Ortega & Petkov (OP) are defined in subsubsection 3.2.2 and subsubsection 3.2.3, respectively.

Table 3: Summary Insurance Purchases in fy2021 (RR1)

| | count | mean | std | min | max |
|---|---|---|---|---|---|
| Renew | 3,571,840 | 0.860 | 0.347 | 0 | 1 |
| FZ | 3,571,840 | 0.481 | 0.500 | 0 | 1 |
| Single Family | 3,072,898 | 0.973 | 0.161 | 0 | 1 |
| Primary Residence | 3,571,840 | 0.827 | 0.378 | 0 | 1 |
| Mandatory Purchase | 3,571,840 | 0.161 | 0.367 | 0 | 1 |
| Construction Year | 3,571,791 | 1,983.387 | 24.647 | 1,690 | 2,023 |
| BRC | 3,571,804 | 306,307.920 | 431,590.473 | 0 | 8,000,000 |
| Total Coverage | 3,072,898 | 282,248.602 | 90,350.358 | 200 | 551,200 |
| Coverage Building | 3,072,898 | 214,924.765 | 61,224.788 | 0 | 501,400 |
| Coverage Contents | 3,072,898 | 67,323.836 | 40,369.205 | 0 | 100,000 |
| Total Cost | 3,072,898 | 955.128 | 1,084.418 | 2 | 58,912 |
| Premium | 3,072,898 | 724.393 | 904.502 | -292 | 49,321 |
| Price per $1,000 of Coverage | 3,072,898 | 4.544 | 7.110 | 0.007 | 472.500 |
| Deductible | 3,060,046 | 1,971.509 | 1,832.696 | 1,000 | 10,000 |
| Subsidized | 3,571,840 | 0.107 | 0.309 | 0 | 1 |

**Notes:** Sample is homeowners who purchased during fy2020 (2020Q2-2021Q1) for 1-to-4 family houses (i.e. excluding condos and coop apartments) with positive values for total coverage and policy cost, and non-missing census tract. The table reports policy attributes for fy2021 (2021Q2-2022Q1) FZ status is a dummy variable taking the value of one for all policies referring to properties in the 100-year flood zone (with designation A or V). BRC stands for building replacement cost. There are 3.57 million homeowners in our sample and 3.07 million renewed their policy in fy2021. For non-renewers in fy21 we report the lagged value of the time-invariant attributes (e.g. FZ or mandatory purchase).

Table 4: Comparison FZ and periphery in fy2021 (RR1)

| Variable | A:mean | X:mean | Dif Mean | p-val |
|---|---|---|---|---|
| Single Family | 0.966 | 0.98 0 | -0.014 | 0.000 |
| Primary Residence | 0.775 | 0.876 | -0.101 | 0.000 |
| Mandatory Purchase | 0.288 | 0.042 | 0.246 | 0.000 |
| Construction Year | 1979.974 | 1986.546 | -6.572 | 0.000 |
| Renew | 0.881 | 0.841 | 0.04 0 | 0.000 |
| BRC | 290157.212 | 321259.052 | -31101.839 | 0.000 |
| Total Coverage | 250960.012 | 312604.291 | -61644.279 | 0.000 |
| Coverage Building | 205611.147 | 223960.69 | -18349.543 | 0.000 |
| Coverage Contents | 45348.866 | 88643.602 | -43294.736 | 0.000 |
| Total Cost | 1308.304 | 612.481 | 695.823 | 0.000 |
| Premium | 1004.631 | 452.509 | 552.122 | 0.000 |
| Price per $1, of Coverage | 6.913 | 2.245 | 4.668 | 0.000 |
| Deductible | 2693.254 | 1268.882 | 1424.372 | 0.000 |
| Subsidized | 0.217 | 0.005 | 0.212 | 0.000 |

**Notes:** The sample is the same as in Table 3, but split by zone: $A$ 100-year flood zone (which also contains flood designation V) and $X$ its periphery (500-year flood zone). The corresponding sample sizes can be computed using the share of policies in FZ times the number of observations (from previous table). For instance, in zone $A$ (plus designation X) the number of policies is $3.57 \times 0.481 = 1.72$ million. The p-values reported in the last column are all below 0.001.

Table 5: Who exits? Characteristics fy2021 (RR1) quitters and renewers

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
|---|---|---|---|---|---|---|
| **Dep. Var. Lags of** | LogPrice | Log BRC | Prim. Res. | LogPrice | Log BRC | Prim. Res. |
| **Observations** | 3574164 | 3499228 | 3574164 | 3474020 | 3251482 | 3474020 |
| **Cov. Est.** | Robust | Robust | Robust | Robust | Robust | Robust |
| **Constant** | 0.6146*** | 12.436*** | 0.8803*** | 0.6821*** | 12.467*** | 0.8843*** |
| | (0.0003) | (0.0006) | (0.0003) | (0.0003) | (0.0006) | (0.0003) |
| **FZ** | 0.8275*** | -0.1353*** | -0.0951*** | 0.7424*** | -0.1127*** | -0.0943*** |
| | (0.0008) | (0.0008) | (0.0004) | (0.0008) | (0.0008) | (0.0004) |
| **Exit** | 0.1094*** | -0.0151*** | -0.0292*** | 0.1391*** | -0.0932*** | -0.0187*** |
| | (0.0010) | (0.0014) | (0.0007) | (0.0009) | (0.0014) | (0.0006) |
| **Exit x FZ** | 0.1963*** | -0.0025 | -0.0626*** | 0.2758*** | -0.1582*** | -0.0308*** |
| | (0.0026) | (0.0023) | (0.0013) | (0.0022) | (0.0022) | (0.0011) |

**Notes:** In columns 1-3 the sample contains homeowners who purchased insurance in the second-to-last round of RR1 (2020m4-2021m3) and estimate linear regression models for various lagged outcomes (such as log insurance price and log of the building replacement cost) where we include an *exit* indicator as a regressor, which takes a value of one for homeowners who did **not** purchase insurance in the last round of RR1, and zero if they did. Instead in columns 4-6 the sample contains policies purchased by homeowners who purchased insurance in the last round of RR1 (2021m4-2022m3) and the *exit* indicator now takes a value of one for homeowners who did **not** purchase insurance in the first round of RR2. Heteroskedasticity-robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Estimates Renewal Probability Models

| | (1) US | (2) US | (3) US | (4) US Regular | (5) FL | (6) TX | (7) NY |
|---|---|---|---|---|---|---|---|
| **Dep. Variable** | renew | renew | renew | renew | renew | renew | renew |
| **No. Observations** | 11,296,273 | 11,296,273 | 11,296,273 | 10,051,322 | 3,073,107 | 2,166,458 | 414,962 |
| **Cov. Est.** | Robust | Robust | Robust | Robust | Robust | Robust | Robust |
| **R-squared** | 0.0070 | 0.0074 | 0.0105 | 0.0218 | 0.0080 | 0.0097 | 0.0214 |
| **Constant** | 0.8488*** | 0.8335*** | 0.8335*** | 0.8687*** | 0.8209*** | 0.8239*** | 0.8844*** |
| | (0.0002) | (0.0004) | (0.0004) | (0.0004) | (0.0008) | (0.0007) | (0.0017) |
| **RR2** | -0.0471*** | -0.0319*** | -0.0319*** | 0.0020*** | -0.0164*** | -0.0377*** | -0.0494*** |
| | (0.0003) | (0.0005) | (0.0005) | (0.0006) | (0.0010) | (0.0009) | (0.0023) |
| $D(t \leq 0)$ **x Trend** | | -0.0038*** | -0.0038*** | -0.0031*** | -0.0056*** | -0.0051*** | -0.0012*** |
| | | (7.403e-05) | (7.403e-05) | (7.398e-05) | (0.0002) | (0.0001) | (0.0004) |
| **FZ** | 0.0410*** | 0.0440*** | 0.0551*** | 0.0812*** | 0.0554*** | 0.0701*** | 0.0704*** |
| | (0.0002) | (0.0005) | (0.0005) | (0.0005) | (0.0010) | (0.0013) | (0.0021) |
| **FZ x RR2** | 0.0076*** | 0.0047*** | 0.0103*** | 0.0316*** | -0.0008 | 0.0001 | 0.0294*** |
| | (0.0005) | (0.0006) | (0.0006) | (0.0007) | (0.0013) | (0.0017) | (0.0029) |
| **FZ x** $D(t \leq 0)$ **x Trend** | | 0.0007*** | 0.0002* | -0.0002** | 0.0016*** | 0.0042*** | 0.0008* |
| | | (9.764e-05) | (0.0001) | (0.0001) | (0.0002) | (0.0003) | (0.0004) |
| **FZ x Subsidized** | | | -0.0514*** | | -0.0370*** | -0.0614*** | -0.0822*** |
| | | | (0.0008) | | (0.0017) | (0.0030) | (0.0027) |
| **FZ x Subsidized x RR2** | | | -0.0356*** | | -0.0212*** | -0.0482*** | -0.0385*** |
| | | | (0.0011) | | (0.0024) | (0.0041) | (0.0038) |
| **FZ x Subdz x** $D(t \leq 0)$ **x Trend** | | | 0.0014*** | | -1.26e-05 | -0.0008 | -0.0004 |
| | | | (0.0002) | | (0.0004) | (0.0006) | (0.0006) |
| **Lag LogPrice** | | | | -0.0540*** | | | |
| | | | | (0.0003) | | | |
| **RR2 x Lag LogPrice** | | | | -0.0438*** | | | |
| | | | | (0.0005) | | | |

**Notes:** The sample contains policies purchased in the 9 quarters preceding the implementation adoption of RR2 ($t = -8, -7, -6, ..., 0$) and 4 quarters after the implementation ($t = 1, 2, ..., 4$). Dummy variable $D(t \leq 0)$ takes a value of one for pre-reform quarters, dummy variable $FZ$ identifies policyholders located in the SFHA, and dummy variable $RR2$ takes a value of one for purchases that took place after RR2 was implemented (2022Q2 onward). $Trend$ is a linear trend whose slope is estimated using only the pre-reform quarters ($t \leq 0$). Dummy variable *Subsidized* takes a value of one for SFHA policyholders with subsidized-rate policies due to pre-FIRM construction. Columns 1-3 use the national sample, column 4 restricts the sample to regular (full-risk rated) policies, and columns 4-6 restrict the analysis to the corresponding state (including also subsidized policies). Heteroskedasticity-robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: ATET on Renewal Probability

| Avg. Renewal Prob. (x100) | Q1 | Q2 | Q3 | Q4 | Average |
|---|---|---|---|---|---|
| Periphery | | | | | |
| Counterfactual 1 $= q_0$ | 83.3 | 83.3 | 83.3 | 83.3 | 83.3 |
| $TE_t^1 = q_t - q_0$ | -3.5 | -2.5 | -2.7 | -4.5 | -3.3 |
| Counterfactual 2 $= q_0 + \beta t$ | 82.9 | 82.5 | 82.1 | 81.7 | 82.3 |
| $TE_t^2 = q_t - (q_0 + \beta t)$ | -3.2 | -1.7 | -1.5 | -3.0 | -2.3 |
| SFHA Regular | | | | | |
| Counterfactual 1 $= q_0$ | 88.9 | 88.9 | 88.9 | 88.9 | 88.9 |
| $TE_t^1 = q_t - q_0$ | -2.2 | -1.0 | -1.4 | -2.1 | -1.7 |
| Counterfactual 2 $= q_0 + \beta t$ | 88.5 | 88.1 | 87.8 | 87.4 | 88.0 |
| $TE_t^2 = q_t - (q_0 + \beta t)$ | -1.8 | -0.3 | -0.3 | -0.7 | -0.8 |
| SFHA Subsidized | | | | | |
| Counterfactual 1 $= q_0$ | 83.7 | 83.7 | 83.7 | 83.7 | 83.7 |
| $TE_t^1 = q_t - q_0$ | -1.8 | -1.3 | -13.5 | -10.5 | -6.7 |
| Counterfactual 2 $= q_0 + \beta t$ | 83.5 | 83.3 | 83.1 | 82.8 | 83.2 |
| $TE_t^2 = q_t - (q_0 + \beta t)$ | -1.5 | -0.9 | -12.8 | -9.6 | -6.2 |

**Notes:** The estimates used in the calculations are reported in Table E.1. The baseline values for the average renewal probabilities in the last quarter before the implementation of RR2 (2022Q1) are: 0.833 (periphery), 0.889 (SFHA regular) and 0.837 (SFHA subsidized). The slopes of the linear pre-reform trends (per quarter) are: -0.0039 (periphery), -0.0036 (SFHA regular) and -0.0022 (SFHA subsidized). Counterfactuals 1 and 2 are described in subsection 5.2.

Table 8: ATET on policy attributes for always-buyers

| Attribute | (1) lnPrice AvgQ1-Q4 | (2) Price AvgQ1-Q4 | (3) lnCoverage AvgQ1-Q4 | (4) Coverage AvgQ1-Q4 | (5) lnDeductible AvgQ1-Q4 | (6) Deductible AvgQ1-Q4 |
|---|---|---|---|---|---|---|
| Periphery | | | | | | |
| Counterfactual 1 $= y_0$ | 0.73 | \$2.1 | 12.62 | \$301,643 | 7.13 | $1,252$ |
| $TE1_t = y_t - y_0$ | 0.08 | 8.3% | 0.00 | 0.0% | 0.12 | 12.4% |
| Counterfactual 2 $= y_0 + \beta t$ | 0.79 | \$2.2 | 12.62 | \$303,534 | 7.13 | \$1,254 |
| $TE2_t = y_t - (y_0 + \beta t)$ | 0.02 | 2.1% | -0.01 | -0.6% | 0.12 | 12.2% |
| SFHA Regular | | | | | | |
| Counterfactual 1 $= y_0$ | 1.11 | \$3.0 | 12.62 | \$302,458 | 7.48 | \$1,770 |
| $TE1_t = y_t - y_0$ | 0.04 | 3.9% | 0.01 | 1.0% | 0.06 | 5.8% |
| Counterfactual 2 $= y_0 + \beta t$ | 1.13 | \$3.1 | 12.62 | \$303,746 | 7.48 | \$1,772 |
| $TE2_t = y_t - (y_0 + \beta t)$ | 0.02 | 2.3% | 0.01 | 0.5% | 0.05 | 5.6% |
| SFHA Subsidized | | | | | | |
| Counterfactual 1 $= y_0$ | 2.72 | \$15.2 | 12.62 | $303,458$ | 8.08 | \$3,227 |
| $TE1_t = y_t - y_0$ | -0.37 | -30.6% | 0.02 | 2.2% | -0.02 | -1.9% |
| Counterfactual 2 $= y_0 + \beta t$ | 2.76 | \$15.8 | 12.63 | \$304,446 | 8.08 | \$3,239 |
| $TE2_t = y_t - (y_0 + \beta t)$ | -0.40 | -33.1% | 0.02 | 1.9% | -0.02 | -2.3% |

**Notes:** All calculations based on always-buyers (who continuously purchased flood insurance in the 2 years before RR2 and in the year immediately after). The estimates used in the calculations are based on models with policyholder fixed-effects and are reported in Table E.3. The baseline values for each attribute in quarter $t = 0$ are the mean values for the corresponding risk zone. Each column reports the average across the four post-reform quarters ($t \geq 1$). Counterfactuals 1 and 2 are described in subsection 5.2.

Table 9: ATET on county-level annual entry

| Entry in risk zone | Q1 | Q2 | Q3 | Q4 | Average | Pct Change |
|---|---|---|---|---|---|---|
| Periphery | | | | | | |
| Counterfactual 1 $= y_0$ | 12.12 | 12.12 | 12.12 | 12.12 | 12.12 | |
| $ATET1_t = y_t - y_0$ | -0.32 | 1.31 | -9.37 | -13.91 | -5.57 | -46.0 |
| Counterfactual 2 $= y_0 + \beta t$ | 10.84 | 9.57 | 8.29 | 7.02 | 8.93 | |
| $ATET2_t = y_t - (y_0 + \beta t)$ | 0.96 | 3.86 | -5.54 | -8.81 | -2.38 | -26.6 |
| SFHA | | | | | | |
| Counterfactual 1 $= y_0$ | 14.56 | 14.56 | 14.56 | 14.56 | 14.56 | |
| $ATET1_t = y_t - y_0$ | 5.42 | 4.22 | 1.81 | -0.75 | 2.68 | 18.4 |
| Counterfactual 2 $= y_0 + \beta t$ | 14.61 | 14.66 | 14.71 | 14.76 | 14.69 | |
| $ATET2_t = y_t - (y_0 + \beta t)$ | 5.37 | 4.12 | 1.66 | -0.95 | 2.55 | 17.4 |

**Notes:** The unit of observation are segments defined by the intersection of the county and the SFHA (or its complement). The estimation sample contains 2 years before RR2 (implemented in October 2021 for new entry) and in the year immediately after. The estimates used in the calculations are based on models with county fixed-effects, reported in Table E.4. The baseline value in quarter $t = 0$ is the annual entry in the county in 2021Q3 in the corresponding risk zone. The average in column 5 is computed over the four post-reform quarters ($t \geq 1$). Counterfactuals 1 and 2 are described in subsection 5.2.

Table 10: Effect of the reform on net exit

| Counterfactual | Baseline $y_0$ | Linear trend $y_0 + \beta t$ |
|---|---|---|
| Chg. Exit | | |
| **ATET (% of baseline)** | | |
| Periphery | 3.3% | 2.3% |
| SFHA | 2.8% | 2.0% |
| **Annual counts (thousands)** | | |
| Periphery | 61 | 43 |
| SFHA | 23 | 11 |
| Sum | 84 | 53 |
| Chg. Entry | | |
| **ATET (% of baseline)** | | |
| Periphery | -46.0% | -26.6% |
| SFHA | 18.4% | 17.4% |
| **Annual counts (thousands)** | | |
| Periphery | -127 | -74 |
| SFHA | 47 | 45 |
| Sum | -80 | -29 |
| Chg. Net Exit | | |
| **Annual counts (thousands)** | | |
| Periphery | 188 | 117 |
| SFHA | -25 | -34 |
| Sum | 164 | 83 |
| Chg. Net Exit (% of baseline) | 4.6% | 2.3% |

**Notes:** ATET for changes in exit and entry from Table 7 and Table 9, respectively. Baseline number of policies (1-4 family houses) in 2023Q1 was 3.57 million. Annual counts in thousands of policies. Column 1 reports estimated changes in exit, entry and net exit (defined as exit minus entry) based on counterfactuals 1 (fixed baseline value) and 2 (projected linear trend) are described in subsection 5.2.

# Appendix

## A Proofs

**Proof of Proposition 1.**

**Claim 1** follows from the assumptions on $u$ (continuous, increasing and strictly concave), the definition of no-insurance certainty equivalent, and implicit differentiation.

**Claim 2** follows from Jensen's inequality and the observation that the risk-neutrality certainty equivalent is simply $(1-r)V + rV$.

Let us now turn to the proof for **claim 3**. Let $r_p$ be the risk level for indifference for purchasing insurance: $u(x(r_p)) = u(V-p)$, given that insurance fully eliminates consumption risk. Thus, $x(r_p) = V - p$. As shown in claim 1, $x(r)$ is continuous and decreasing in $r$, ranging between $V$ and $V-1$. Hence, since $0 < p < 1$, there exists a unique value $r_p$ such that $x(r_p) = V - p$. Households with $r < r_p$ will not purchase insurance, regardless of their income. Households with $r \geq r_p$ will purchase insurance if they can afford it ($y \geq p$).

**Claim 4** follows from specializing Equation 4 and Equation 6 to the case of risk neutrality. ■

**Proof of Proposition 2.**

We shall prove **claim 1** in three steps. First, let us show that homeowners with vanishingly small flood risk will not purchase insurance. It will be helpful to define the surplus from buying insurance (in terms of non-contingent consumption levels):

$$S^1(r;\rho) = V - p_1(r) - x(r;\rho),$$

where $S^1(r;\rho)$ and $x(r;\rho)$ denote the surplus and no-insurance certainty equivalent under risk aversion ($\rho > 0$) or risk neutrality ($\rho = 0$).[43] By **Proposition 1**, the surplus function is continuous in $r$ and $S^1(0;\rho) = -\alpha^X < 0$. Thus, $r_p^X > 0$.

The second step to prove claim 1 is to show that, under *risk neutrality*, there exists a level of risk $r^X < \overline{r}$ above which homeowners are willing to buy insurance. This threshold risk level is pinned down by

$$\begin{aligned} S^1(r^X;\rho=0) &= V - p_1(r^X) - x(r^X;\rho=0) \\ &= V - p_1(r^X) - (1-r^X)V - r^X(V-1) \\ &= -\alpha^X + (1-\beta)r^X = 0. \end{aligned}$$

Solving the equation delivers $r^X = \alpha_X/(1-\beta)$. It is straightforward to check that if $\alpha_X + (\beta-1)\overline{r} < 0$ then $r^X < \overline{r}$.

The third step in proving claim 1 entails showing that, under *risk aversion*, there exist values of $r < r^X$ for which homeowners are willing to purchase insurance. Now consider a

[43] We use $\rho$ simply as an indicator to denote risk averse (versus risk neutral) preferences. When we specialize to CRRA utility, $\rho$ will coincide with the coefficient of relative risk aversion.

**risk averse** homeowner ($\rho > 0$) with flood risk given by the risk-neutral threshold $r = r^X$:

$$S(r^X; \rho) = V - p_1(r^X) - x(r^X; \rho) > S(r^X; 0) = 0, \tag{25}$$

where the inequality follows from the definition of threshold $r^X$ as the indifferent risk level under risk neutrality and claim 2 in **Proposition 1**, which established that the no-insurance certainty equivalent is lower under risk aversion than under risk neutrality (for a given $r$). Hence, Equation 25 implies that a risk averse individual with risk $r^X$ will be willing to buy insurance because of a strictly positive surplus. Since $S(r; \rho)$ is continuous in $r$, there exists $\varepsilon > 0$ such that if $r^X - \varepsilon < r < r^X$ then $S(r^X - \varepsilon; \rho) > 0$. Hence, the threshold for willingness to purchase insurance (under risk aversion) is given by $0 < r_p^X < \overline{r}$. To conclude the proof of claim 1, we simply need to add the affordability constraint.

Let us now turn to **claim 2**, which focuses on zone $z = A$. The thrust of the proof of the previous claim also applies here. The risk-neutral indifference threshold $r^A$ is defined by

$$\begin{aligned} S^1(r^A; \rho = 0) &= V - p_1(r^A) - x(r^A; \rho = 0) \\ &= -\alpha^A + (1 - \beta) r^A = 0. \end{aligned}$$

To ensure $r^A > \overline{r}$, we need to verify that $S^1(\overline{r}; \rho = 0) = -\alpha^A + (1 - \beta)\overline{r} < 0$, which holds by our parametric assumption. Similarly, $r^A < 1$ holds because $S^1(1; \rho = 0) = -\alpha^A + (1 - \beta) > 0$, proving that $\overline{r} < r^A < 1$. By the continuity argument provided in the previous claim, there exists $\varepsilon > 0$ such that if $r \in (r^A - \varepsilon, r^A)$ then a *risk averse* homeowner will be willing to buy insurance. Hence, $\overline{r} \leq r_p^A < 1$.

**Claim 3** follows easily from the observation that the no-insurance certainty equivalent under risk neutrality is simply the expected level of consumption under no-insurance. It is straightforward to verify that if $r > r^z$ (for $z = X, A$), then $r > p_1(r)$. In words, homeowners are only willing to purchase insurance if their expected loss is higher than the premium.

**Claim 4** is trivial. Simply, recall that the expected loss and the premium have been normalized in units of loss ($L). To show **claim 5** simply integrate over the regions of buyers in the $(y, r)$ space under the RR1 pricing system. The affordability constraints become irrelevant when $G(r) = 0$ for all $r < 1$, which simplifies the expressions. ■

**Proof of Proposition 3.** In each zone $z = X, A$, the pricing schedule $\widehat{p_1}(r) = p_1(r)$ for $r < r^z$ and therefore the utility maximizing choices do not change over this range of risk levels. Above the corresponding risk-neutral thresholds, homeowners are willing to purchase insurance but the affordability constraint is more stringent under the reform: $y \geq \widehat{p_1}(r) > p_1(r)$ for $r > r^z$. The second claim follows from evaluating Equation 10 under both pricing systems.■

**Proof of Proposition 4.** The claims in the proof are straightforward given the parametric assumptions and the pricing functions. One simply needs to verify that each region depicted in Figure 3 is non-empty, which is the case under the parameter values for $(\alpha_X, \alpha_A, \beta)$. In particular, this requires verifying that $b_j = 1$ provided $x(r) \leq V - p_j(r)$ and $y \geq p_j(r)$, for $j = 1, 2$, whereas $b_j = 0$ when $x(r) > V - p_j(r)$ or $y < p_j(r)$ (or both). ■

**Proof of Proposition 4.** Under risk neutrality ($\rho = 0$), for any $r$, everyone who can afford the premiums is willing to buy insurance (out of indifference) since

$$\begin{aligned} S^2(r; \rho = 0) &= V - p_2(r) - x(r; 0) \\ &= V - r - [(1 - r)V + r(V - 1)] = 0, \end{aligned}$$

where $x(r; 0) = (1 - r)V + r(V - 1)$ is the no-insurance certainty equivalent under risk neutrality. Since the no-insurance certainty equivalent is *lower* for risk averse individuals ($\rho > 0$) than for risk neutral individuals, the surplus from purchasing insurance is higher under risk aversion:

$$S^2(r; \rho) > S^2(r; \rho = 0) = 0.$$

Hence, at any $r$, all homeowners are (strictly) willing to purchase insurance and everyone who can afford the premium will do so.

To prove the second part of the proposition, simply integrate over the regions of buyers in the $(y, r)$ space under the RR2 pricing system. The affordability constraints become irrelevant when $G(r) = 0$ for all $r < 1$, which simplifies the expressions. ■

**Proof of Proposition 5.** The claims in the proof are straightforward given the parametric assumptions and the pricing functions. One simply needs to verify that each region depicted in Figure 3 is non-empty, which is the case under the parameter values for $(\alpha_X, \alpha_A, \beta)$. In particular, this requires verifying that $b_j = 1$ provided $x(r) \leq V - p_j(r)$ and $y \geq p_j(r)$, for $j = 1, 2$, whereas $b_j = 0$ when $x(r) > V - p_j(r)$ or $y < p_j(r)$ (or both). ■

**Proof of Proposition 6.** When switching from RR1 to RR2, there are four different experiences for homeowners: new entry ($(b_1, b_2) = (0, 1)$), exit ($(b_1, b_2) = (1, 0)$), renewal ($(b_1, b_2) = (1, 1)$) and those that stay out of the market ($(b_1, b_2) = (0, 0)$). The change in total revenue is a weighted sum of the change in the revenue for each of these four groups. Clearly, those that stay out of the market generate zero revenue both under RR1 and RR2. ■

# B Social welfare

Let us now consider the welfare effects of the switch to RR2. To set the stage, consider an allocation that characterizes the state-contingent consumption of all homeowner types $(c_1(r, y), c_2(r, y))$ for homeowners with flood risk and income $(r, y)$, where $c_1(r, y)$ and $c_2(r, y)$ denote the consumption levels in the good state (no flooding) and bad state (flooding), respectively. The corresponding expected utility is given by

$$U(r, y) = (1 - r)u(c_1(r, y)) + ru(c_2(r, y)). \tag{26}$$

As discussed above, homeowners can choose between buying (full) insurance or remaining uninsured. The former enjoy a non-contingent consumption level $c_1 = c_2 = V - p(r)$ whereas the latter's consumption bundle is given by $(c_1, c_2) = (V, V - 1)$. The corresponding expected utility levels are thus given by

$$U(r,y) = \begin{cases} u(V - p(r)) & \text{if insured} \\ (1-r)u(V) + ru(V-1) & \text{if uninsured.} \end{cases} \tag{27}$$

We will evaluate allocations by means of a (utilitarian) social welfare function (where all homeowners are given the same weight):

$$W^u = \int_{r=0}^{r=1} \int_{y=0}^{y=\infty} U(r,y)h(r)g(y)drdy. \tag{28}$$

We can state (and prove) the following results:[44]

**Proposition 7.** *For pricing system $j = 1, 2$, let $B_j = \{(r,y) : b(r,y) = 1\}$ and $\widetilde{B_j} = \{(r,y) : b(r,y) = 0\}$ denote the sets of insured and uninsured homeowners, respectively. Then*

1. *Social welfare under pricing $j = 1, 2$ is given by*

$$\begin{aligned} W^u &= \int_{r \in B_j} u(V - p_j(r))h(r)dr + \int_{r \in \widetilde{B_j}} [(1-r)u(V) + ru(V-1)]drdy & (29) \\ W^c &= \int_{r \in B_j} (V - p_j(r))h(r)dr + \int_{r \in \widetilde{B_j}} x(r)drdy, & (30) \end{aligned}$$

   *where social welfare in Equation 29 and Equation 30 is measured in utils and units of the income numeraire, respectively.*

2. *The switch from RR1 ($j = 1$) to RR2 ($j = 2$) produces winners and losers. Let $U_j$ denote expected utility under pricing system $j = 1, 2$. Then*

$$U_2(r,y) \geq U_1(r,y) \iff r \in (0, r^X) \cup (\overline{r}, r^A), \tag{31}$$

   *with strict gains if $b_2(r,y) = 1$. In words, homeowners for which $p_2(r) < p_1(r)$ will strictly benefit (if they purchase insurance under RR2). Conversely, those for which $p_2(r) > p_1(r)$ will strictly lose out (if they purchased insurance under RR1).*

3. *The change in social welfare as we switch from RR1 ($j = 1$) to RR2 ($j = 2$) in units of the income numeraire can be written as*

$$\begin{aligned} \Delta W^c &= Prob(Entry)E\left(V - p_2(r) - x(r)|Entry\right) & (32) \\ &- Prob(Exit)E\left(V - p_1(r) - x(r)|Exit\right) & (33) \\ &+ Prob(Renew)E\left(p_1(r) - p_2(r)|Renew\right). & (34) \end{aligned}$$

[44] Our analysis of social welfare abstracts from the taxation required to finance the flood insurance program. In a sense, we are considering the floodplain as a "small open economy." We leave for future research the task of incorporating the financing of the insurance program.

**Proof of Proposition 7.** The proof of **claim 1** follows simply from observing that

$$\begin{aligned} b_j(r) = 1 &\iff c_1(y,r) = c_2(y,r) = V - p_j(r) \\ b_j(r) = 0 &\iff (c_1(y,r), c_2(y,r)) = (V, V-1) \end{aligned}$$

and the definition of $x(r)$ as the no-insurance certainty equivalent.

**Claim 2** identifies the individual homeowners gaining / losing from the switch to RR2. Observe that the utility maximization problem has a choice set with only two options: full insurance or no insurance at all. In the former case, expected utility is given by $u(V - p_j(r))$ whereas the no-insurance expected utility can be written as $u(x_r)$, where $x_r$ is the no-insurance certainty equivalent. Clearly, individuals for which $p_2(r) \leq p_1(r)$ will be weakly better off and the improvement will be strictly positive whenever they purchase insurance under RR2 ($b_2 = 1$). By definition of the risk-neutral threshold $r^X$, these individuals are characterized by $r < r^X$ in each zone $z = X, A$. The same argument in reverse identifies the homeowners that lose out from the switch to RR2.

To derive the expression for the change in social welfare (in units of numeraire) in **claim 3**, it helps to proceed region by region, keeping Figure 3 in mind. First, consider the region $(r, y)$ for which there is entry ($b_1 = 0, b_2 = 1$). By virtue of **claim 2** above, $U_2(r, y) > U_1(r, y)$ for all new entrants. Hence, integrating over the entry region, we obtain Equation 32, where the integrand is strictly positive. Next, we turn to the region with exit ($b_1 = 1, b_2 = 0$). Again by **claim 2**, $U_1(r, y) < U_2(r, y)$ for all quitters. Integrating over the exit region, we obtain Equation 33, where the integrand is again strictly positive as it represents a strict welfare loss. Third, consider now the set of renewers. As discussed earlier, there are renewers for which the switch to RR2 will entail a strict welfare loss (since $p_2(r) > p_1(r)$) and renewers for which the opposite will hold. But, either way, the change in consumption entailed by the switch to RR2 is given by $(V - p_2(r)) - (V - p_1(r)) = p_2(r) - p_1(r)$. Integration delivers Equation 34, which may be positive or negative depending on the relative size of each of the two groups. Last, the remaining homeowners do not purchase insurance under either system ($b_1 = b_2 = 0$). Hence, their expected utility does not change when switching to RR2 (and equals $u(x_r)$). ■

It is clear that entry and premium reductions increase social welfare, whereas exit and premium increases lower it. However, the overall change in social welfare depends on parameter values. In other words, the adoption of RR2 could entail a net gain or a net loss and its determination is an empirical question.

Empirical estimation of the social welfare gains (or losses) from the adoption of RR2 requires knowledge of the expected no-insurance certainty equivalent among entrants and quitters (along with the value of $V$). Unfortunately, these terms are hard to identify in the data. The following corollary provides guidance for the estimation of the change in social welfare arising from the adoption of RR2.

**Corollary.** *In regards to the empirical estimation of $\Delta W^c$,*

1. *The intensive margin in Equation 34 can be estimated as the average within-policyholder*

*change in premium.*[45]

2. *Only $Prob(Exit)$ and $Prob(Entry)$ can be estimated in Equation 32 and Equation 33.*
3. *If we assume that the average surplus from purchasing insurance $(V - p_j(r) - x(r))$ is higher for homeowners exiting the market ($r > r^z$) than for entrants ($r < r^z$), the volume of exit matters more for social welfare than the volume of entry. Moreover, a sufficient condition for $\Delta W^c < 0$ is (i) average increase in premiums among renewers and (ii) net exit from the market.*

# C Dynamic model

Let us consider a simple overlapping generations setup where a cohort of homeowners (of size one) is born each period and each one lives for two periods. At birth each individual is drawn from the joint density function $f(r, y) = g(r)h(y)$, as discussed above. Let's denote the measures of young and old individuals in year $t$ as $n_t$ and $m_t$, respectively, and note that $n_t + m_t = 2$ in each period $t$. Each homeowner decides whether to purchase (full) insurance under pricing function $p_t(r)$. The utility-maximizing choices of all homeowners are summarized in indicator function $b_t(r, y)$ (as in Equation 9).

In this setup, there are four possible individual insurance histories:

$$(b_t^n, b_{t+1}^m) \in \{(0,0), (0,1), (1,0), (1,1)\},$$

where the first (second) term indicates if the individual purchased insurance in her youth (old age). Clearly, if the pricing function remains unchanged between two consecutive periods, there will be no switchers and only two possible individual histories $(b_t^n, b_{t+1}^m) \in \{(0,0), (1,1)\}$.

At any point in time, insurance purchases can be partitioned into renewals and first-time purchases (entry). **Renewals** correspond to old homeowners who purchased insurance throughout their lifetimes ($b_{t-1}^n = b_t^m = 1$) and can be aggregated as[46]

$$Renewers_t = m_t \int\int b_{t-1}(r, y) b_t(r, y) h(r) g(y), \tag{35}$$

where the integration limits cover all permissible values for $r$ and $y$. In turn, **entry** can take place among young homeowners or old first-time buyers:

$$Entry_t = n_t \int\int b_t(r, y) h(r) g(y) + m_t \int\int (1 - b_{t-1}(r, y)) b_t(r, y) h(r) g(y). \tag{36}$$

In each period, there is also **exit** from the insurance market, either because some old homeowners who purchased insurance while young now decide not to, or due to the death of homeowners who purchased insurance in the last period. Thus,

$$Exit_t = m_t \int\int b_{t-1}(r, y)(1 - b_t(r, y)) h(r) g(y) + m_{t-1} \int\int b_{t-1}(r, y) h(r) g(y). \tag{37}$$

[45] In settings where insurance take-up is compulsory, this term fully sums up the change in social welfare.
[46] To lighten notation we omit $drdy$ from the integrals.

Consider now two consecutive periods during which the pricing function remains **unchanged**. Obviously, the purchase allocations will not vary either: $b_{t-1}(r,y) = b_t(r,y) = b(r,y)$. Thus, there will be no switching of insurance status over anyone's lifetimes and

$$Renewers_t = Entry_t = Exit_t = \int\int b(r,y)h(r)g(y),$$

where we also used that $n_t = m_t = 1$ at all $t$. Note also that total insurance purchases in each period are simply given by $2\int\int b(r,y)h(r)g(y)$. Moreover, absent population growth and changes in the pricing schedule, the measures of renewers, entry, exit and take-up (purchases) remain constant over time.

# D Subjective Flood Risk Beliefs

Consider zone $z = X, A$ and suppose that the subjective probability of flooding for an individual with objective flood risk $r$ is given by $\pi(r) = \lambda r$ where $0 < \lambda \leq 1$. Naturally, lower values of $\lambda$ entail a larger degree of underestimation of the objective risk.

The essential departure relative to the standard case with objective beliefs is that premiums depend on objective risk ($r$) but homeowners' rank consumption bundles on the basis of subjective risk ($\pi = \lambda r$). This generates a clash between flood risk skepticism (inversely related to $\lambda$) and risk aversion: for a given degree of risk aversion, insurance take-up falls in the discount placed on objective flood risk probabilities. Put otherwise, there is a threshold value of skepticism $\lambda^*(\rho)$, which depends on the degree of risk aversion, below which no homeowner purchases insurance. Our modeling choices can be seen as providing microfoundations for the insurance purchase frictions in Wagner (2022).

It is easy to illustrate this relationship when homeowners are risk neutral. Under RR1 pricing, a homeowner with flood risk $r$ is willing to purchase insurance provided that $p_1(r) \leq \lambda r$, that is,

$$r \geq \frac{\alpha_z}{\lambda - \beta}.$$

It is also straightforward to see that for $0 < \lambda \leq \beta$, no one will buy (because $p_1(r) > \lambda r$ for all $r$). In addition, for $\beta < \lambda < 1$, as $\lambda$ increases, the threshold risk value falls and it goes to infinity as $\lambda$ approaches $\beta$ (from above). Hence, for $\lambda \leq \alpha_A - \beta$, not even homeowners with $r = 1$ will purchase insurance. Combining both observations, $\lambda \leq \lambda^* = min\{\beta, \alpha_A - \beta\}$, no homeowner will buy insurance. With risk averse homeowners, there's a greater preference for insurance. However, at any given level of risk aversion, there exists a threshold value of skepticism below which no homeowner is willing to purchase insurance.[47]

[47] When premiums are actuarially fair, as under RR2, the situation is similar. In fact, under risk neutrality, no homeowner is willing to purchase insurance for any $0 < \lambda < 1$. Under risk aversion, there is also a threshold value of skepticism below which no homeowner purchases insurance.

## E Appendix Tables and Figures

Figure E.1: Purchases, renewals and entry. Periphery only

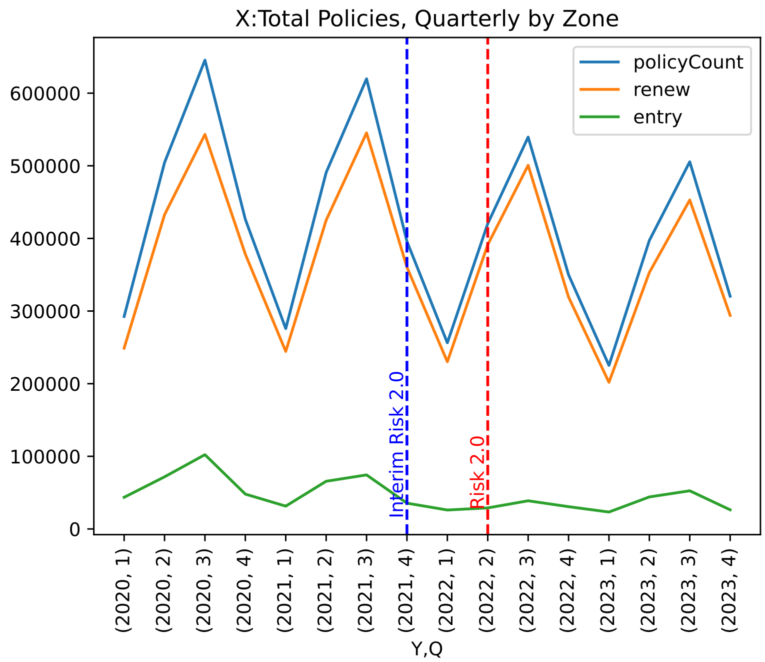
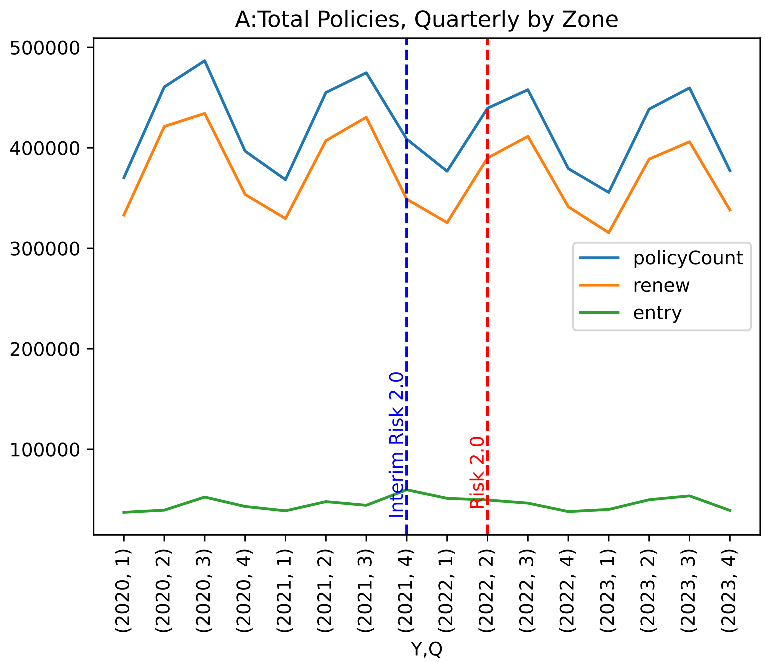



Figure E.2: Purchases, renewals and entry. 100-year FZ only




**Notes:** Quarterly purchases from 2020Q1 through 2023Q4. The flood zone (FZ) is defined as flood zone designations A and V.

Table E.1: Estimates Renewal probabilities with flexible post-reform trend

| | (1) US | (2) US | (3) FL | (4) TX | (5) NY |
|---|---|---|---|---|---|
| **Dep. Variable** | renew | renew | renew | renew | renew |
| **No. Observations** | 11296273 | 11296273 | 3073107 | 2166458 | 414962 |
| **Cov. Est.** | Robust | Robust | Robust | Robust | Robust |
| **R-squared** | 0.0105 | 0.0112 | 0.0082 | 0.0107 | 0.0240 |
| $D(t \leq 0)$ **x LinearTrend** | -0.0038*** | -0.0039*** | -0.0056*** | -0.0052*** | -0.0015*** |
| | (7.403e-05) | (7.392e-05) | (0.0002) | (0.0001) | (0.0004) |
| $D(t \leq 0)$ **x LinearTrend x FZ** | 0.0002* | 0.0003*** | 0.0017*** | 0.0043*** | 0.0011** |
| | (0.0001) | (0.0001) | (0.0002) | (0.0003) | (0.0004) |
| $D(t \leq 0)$ **x LinearTrend x FZ x Subsidized** | 0.0014*** | 0.0014*** | -1.26e-05 | -0.0008 | -0.0004 |
| | (0.0002) | (0.0002) | (0.0004) | (0.0006) | (0.0006) |
| **Constant** | 0.8335*** | 0.8330*** | 0.8206*** | 0.8237*** | 0.8828*** |
| | (0.0004) | (0.0004) | (0.0008) | (0.0007) | (0.0017) |
| **FZ** | 0.0551*** | 0.0556*** | 0.0557*** | 0.0703*** | 0.0720*** |
| | (0.0005) | (0.0005) | (0.0010) | (0.0013) | (0.0021) |
| **FZ x Subsidized** | -0.0514*** | -0.0514*** | -0.0370*** | -0.0614*** | -0.0822*** |
| | (0.0008) | (0.0008) | (0.0017) | (0.0030) | (0.0027) |
| **RR2** | -0.0319*** | | | | |
| | (0.0005) | | | | |
| **RR2 x FZ** | 0.0103*** | | | | |
| | (0.0006) | | | | |
| **RR2 x FZ x Subsidized** | -0.0356*** | | | | |
| | (0.0011) | | | | |
| **Q1** | | -0.0354*** | -0.0199*** | -0.0466*** | -0.0340*** |
| | | (0.0007) | (0.0014) | (0.0012) | (0.0038) |
| **Q2** | | -0.0251*** | -0.0073*** | -0.0341*** | -0.0334*** |
| | | (0.0006) | (0.0013) | (0.0011) | (0.0033) |
| **Q3** | | -0.0265*** | -0.0139*** | -0.0253*** | -0.0543*** |
| | | (0.0007) | (0.0015) | (0.0014) | (0.0034) |
| **Q4** | | -0.0453*** | -0.0346*** | -0.0446*** | -0.0709*** |
| | | (0.0009) | (0.0019) | (0.0019) | (0.0041) |
| **Q1 FZ Regular** | | -0.0219*** | -0.0244*** | -0.0266*** | -0.0152*** |
| | | (0.0007) | (0.0012) | (0.0021) | (0.0027) |
| **Q2 FZ Regular** | | -0.0100*** | -0.0099*** | -0.0191*** | -0.0036 |
| | | (0.0006) | (0.0011) | (0.0020) | (0.0023) |
| **Q3 FZ Regular** | | -0.0262*** | -0.0137*** | -0.0542*** | -0.0297*** |
| | | (0.0007) | (0.0012) | (0.0023) | (0.0025) |
| **Q4 FZ Regular** | | -0.0300*** | -0.0210*** | -0.0568*** | -0.0300*** |
| | | (0.0007) | (0.0012) | (0.0024) | (0.0027) |
| **Q1 FZ Subsidized** | | -0.0175*** | -0.0332*** | -0.0159*** | -0.0102** |
| | | (0.0015) | (0.0031) | (0.0052) | (0.0049) |
| **Q2 FZ Subsidized** | | -0.0129*** | -0.0182*** | -0.0156*** | -0.0027 |
| | | (0.0014) | (0.0031) | (0.0050) | (0.0045) |
| **Q3 FZ Subsidized** | | -0.1349*** | -0.0628*** | -0.2344*** | -0.1353*** |
| | | (0.0019) | (0.0040) | (0.0074) | (0.0061) |
| **Q4 FZ Subsidized** | | -0.1046*** | -0.0565*** | -0.1800*** | -0.1068*** |
| | | (0.0019) | (0.0040) | (0.0078) | (0.0062) |

**Notes:** The sample contains policies purchased in the 9 quarters preceding the implementation adoption of RR2 ($t = -8, -6, ..., 0$) and 4 quarters after the implementation ($t = 1, 2, ..., 4$) for policyholders who purchased insurance in the previous year. Dummy variable $D(t \leq 0)$ takes a value of one for pre-reform quarters, dummy variable $FZ$ identifies policyholders located in the SFHA, and dummy variable $RR2$ takes a value of one for purchases that took place after RR2 was implemented (2022Q2 onward). $LinearTrend$ is a linear trend whose slope is estimated using only the pre-reform quarters ($t \leq 0$). Dummy variable $Subsidized$ takes a value of one for SFHA policyholders with subsidized-rate policies due to pre-FIRM construction. Dummy variables $Q1 - Q4$ capture the post-reform average renewal probability in the periphery (in deviations). Similarly, dummy variables $Q1 - Q4FZRegular$ and $Q1 - Q4FZSubsidized$ capture the post-reform average renewal probability in the SFHA (in deviations) for regular and subsidized policyholders, respectively. Columns 1-2 uses the national sample whereas columns 3-5 restrict the analysis to the corresponding state. Heteroskedasticity-robust SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.2: Regression Discontinuity Model Renewal Probability

| | (1) ± 10 days | (2) ± 10 days | (3) ± 20 days |
|---|---|---|---|
| **Dep. Variable** | renew | renew | renew |
| **No. Observations** | 181,775 | 181,775 | 346,273 |
| **R-squared** | 0.0084 | 0.0101 | 0.0098 |
| **Constant** | 0.8215*** | 0.8215*** | 0.8185*** |
| | (0.0019) | (0.0019) | (0.0014) |
| **FZ** | 0.0567*** | 0.0692*** | 0.0717*** |
| | (0.0023) | (0.0024) | (0.0018) |
| **RR2** | -0.0331*** | -0.0331*** | -0.0271*** |
| | (0.0028) | (0.0028) | (0.0020) |
| **RR2 x FZ** | 0.0060* | 0.0022 | -0.0009 |
| | (0.0035) | (0.0036) | (0.0026) |
| **FZ x Subsidized** | | -0.0574*** | -0.0559*** |
| | | (0.0038) | (0.0028) |
| **FZ x Subsidized x RR2** | | 0.0146** | 0.0139*** |
| | | (0.0058) | (0.0042) |

**Notes:** Columns 1 and 2 restrict the sample to renewal decisions 10 days before to 10 days after the implementation of RR2, in contrast column 3 considers renewals within a plus/minus 20 day window. Dummy variable $RR2$ takes a value of one after RR2 was implemented (i.e. the first 10 or 20 days in April 2022). Conversely, the dummy variable takes a value of zero prior to RR2 (i.e. the last 10 or 20 days in March 2022). Dummy variable $Subsidized$ takes a value of one for SFHA policyholders with subsidized-rate policies due to pre-FIRM construction. Heteroskedasticity-robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.3: Estimates for policy attributes with flexible post-reform effects

| | (1) US | (2) US | (3) FL | (4) TX | (5) NY | (6) US | (7) US |
|---|---|---|---|---|---|---|---|
| **Dep. Variable: Log of** | Price | Price | Price | Price | Price | Covg. | Deduct. |
| **Estimator** | FE | FE | FE | FE | FE | FE | FE |
| **No. Observations** | 7,508,195 | 7,508,195 | 2,019,797 | 1,349,302 | 312,898 | 7,508,195 | 7,486,330 |
| **Cov. Est.** | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered |
| **R-squared** | 0.2615 | 0.2672 | 0.2525 | 0.4822 | 0.2556 | 0.0242 | 0.1554 |
| $D(t \leq 0)$ **x Trend** | 0.0231*** | 0.0235*** | 0.0238*** | 0.0239*** | 0.0245*** | 0.0025*** | 0.0005*** |
| | (1.904e-05) | (2.103e-05) | ( 3.7e-05) | (3.829e-05) | (0.0001) | (1.853e-05) | (1.024e-05) |
| **FZ x** $D(t \leq 0)$ **x Trend** | -0.0175*** | -0.0175*** | -0.0184*** | -0.0174*** | -0.0137*** | -0.0008*** | 0.0001*** |
| | (3.706e-05) | (3.788e-05) | (5.94e-05) | (0.0001) | (0.0002) | (2.551e-05) | (1.978e-05) |
| **FZ x Subz. x** $D(t \leq 0)$ **x Trend** | 0.0084*** | 0.0087*** | 0.0103*** | 0.0069*** | 0.0047*** | -0.0004*** | 0.0009*** |
| | (9.478e-05) | (8.884e-05) | (0.0002) | (0.0004) | (0.0003) | (5.503e-05) | (5.713e-05) |
| **Constant** | 0.8331*** | 0.8337*** | 0.7069*** | 0.7684*** | 1.1056*** | 12.557*** | 7.2261*** |
| | (0.0067) | (0.0067) | (0.0109) | (0.0113) | (0.1208) | (0.0038) | (0.0053) |
| **FZ** | 0.4339*** | 0.4350*** | 0.4375*** | 0.3894*** | 0.4380** | -0.1414*** | 0.2733*** |
| | (0.0130) | (0.0130) | (0.0187) | (0.0437) | (0.2020) | (0.0074) | (0.0101) |
| **FZ x Subsidized** | 0.3632*** | 0.3680*** | 0.2741*** | 0.3760*** | 0.2656*** | -0.0300*** | 0.0506*** |
| | (0.0036) | (0.0036) | (0.0084) | (0.0106) | (0.0101) | (0.0014) | (0.0017) |
| **RR2** | 0.0728*** | | | | | | |
| | (0.0002) | | | | | | |
| **RR2 x FZ** | -0.0342*** | | | | | | |
| | (0.0003) | | | | | | |
| **RR2 x FZ x Subsidized** | -0.4078*** | | | | | | |
| | (0.0013) | | | | | | |
| **Q1** | | 0.0441*** | 0.0303*** | 0.0717*** | 0.0337*** | -0.0021*** | 0.1184*** |
| | | (0.0003) | (0.0006) | (0.0004) | (0.0019) | (0.0001) | (0.0004) |
| **Q2** | | 0.0616*** | 0.0494*** | 0.0947*** | 0.0504*** | 0.0006*** | 0.1222*** |
| | | (0.0003) | (0.0005) | (0.0004) | (0.0016) | (0.0001) | (0.0003) |
| **Q3** | | 0.0940*** | 0.0942*** | 0.1270*** | 0.0838*** | 0.0011*** | 0.1157*** |
| | | (0.0003) | (0.0006) | (0.0004) | (0.0016) | (0.0001) | (0.0004) |
| **Q4** | | 0.1177*** | 0.1086*** | 0.1545*** | 0.1101*** | 0.0010*** | 0.1108*** |
| | | (0.0004) | (0.0009) | (0.0007) | (0.0023) | (0.0002) | (0.0005) |
| **Q1 FZ Regular** | | 0.0201*** | 0.0449*** | -0.0009 | 0.0139*** | 0.0035*** | 0.0562*** |
| | | (0.0006) | (0.0008) | (0.0019) | (0.0032) | (0.0002) | (0.0004) |
| **Q2 FZ Regular** | | 0.0256*** | 0.0519*** | 0.0048*** | 0.0297*** | 0.0092*** | 0.0577*** |
| | | (0.0005) | (0.0008) | (0.0018) | (0.0026) | (0.0002) | (0.0004) |
| **Q3 FZ Regular** | | 0.0526*** | 0.0681*** | 0.0515*** | 0.0506*** | 0.0108*** | 0.0592*** |
| | | (0.0005) | (0.0008) | (0.0018) | (0.0026) | (0.0002) | (0.0004) |
| **Q4FZ Regular** | | 0.0543*** | 0.0688*** | 0.0468*** | 0.0485*** | 0.0154*** | 0.0522*** |
| | | (0.0006) | (0.0009) | (0.0019) | (0.0031) | (0.0002) | (0.0005) |
| **Q1 FZ Subsidized** | | -0.3959*** | -0.1427*** | -0.5224*** | -0.3798*** | 0.0150*** | -0.0187*** |
| | | (0.0022) | (0.0034) | (0.0073) | (0.0079) | (0.0005) | (0.0007) |
| **Q2 FZ Subsidized** | | -0.4001*** | -0.1381*** | -0.5036*** | -0.3629*** | 0.0220*** | -0.0184*** |
| | | (0.0021) | (0.0035) | (0.0069) | (0.0071) | (0.0005) | (0.0006) |
| **Q3 FZ Subsidized** | | -0.3418*** | -0.0853*** | -0.4696*** | -0.3747*** | 0.0249*** | -0.0199*** |
| | | (0.0027) | (0.0041) | (0.0105) | (0.0087) | (0.0007) | (0.0008) |
| **Q4 FZ Subsidized** | | -0.3234*** | -0.0753*** | -0.4298*** | -0.3782*** | 0.0251*** | -0.0208*** |
| | | (0.0027) | (0.0042) | (0.0104) | (0.0090) | (0.0007) | (0.0008) |

**Notes:** The sample contains policies purchased in the 9 quarters preceding the implementation adoption of RR2 ($t = -8, -6, ..., 0$) and 4 quarters after the implementation ($t = 1, 2, ..., 4$) for policyholders who purchased insurance continuously over the whole sample period. Dummy variable $D(t \leq 0)$ takes a value of one for pre-reform quarters, dummy variable $FZ$ identifies policyholders located in the SFHA, and dummy variable $RR2$ takes a value of one for purchases that took place after RR2 was implemented (2022Q2 onward). $Trend$ is a linear trend whose slope is estimated using only the pre-reform quarters ($t \leq 0$). Dummy variable $Subsidized$ takes a value of one for SFHA policyholders with subsidized-rate policies due to pre-FIRM construction. Dummy variables $Q1-Q4$ capture the post-reform average renewal probability in the periphery (in deviations). Similarly, dummy variables $Q1-Q4FZRegular$ and $Q1-Q4FZSubsidized$ capture the post-reform average renewal probability in the SFHA (in deviations) for regular and subsidized policyholders, respectively. Heteroskedasticity-robust SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.4: Estimates for county-level entry with flexible post-reform effects.

| | **(1) US** SFHA | **(2) FL** SFHA | **(3) TX** SFHA | **(4) NY** SFHA | **(5) US** Periph. | **(6) FL** Periph. | **(7) TX** Periph. | **(8) NY** Periph. |
|---|---|---|---|---|---|---|---|---|
| **Dep. Variable** | entry | entry | entry | entry | entry | entry | entry | entry |
| **No. Observations** | 38844 | 869 | 2579 | 806 | 39130 | 865 | 2348 | 808 |
| **Cov. Est.** | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered | Clustered |
| **R-squared** | 0.0076 | 0.0517 | 0.0254 | 0.0902 | 0.0044 | 0.2444 | 0.0213 | 0.0842 |
| $D(t \leq 0)$ **x LinearTrend** | 0.0496 | 7.4037*** | -0.6493 | -0.4731 | -1.2759*** | -9.3170*** | -11.467* | -0.1551 |
| | (0.0655) | (1.9478) | (0.5107) | (0.3141) | (0.4150) | (2.6050) | (6.3445) | (0.1699) |
| **Constant** | 14.564*** | 262.15*** | 22.863*** | 19.113*** | 12.122*** | 128.95*** | 42.675* | 16.246*** |
| | (0.3330) | (11.175) | (2.0276) | (1.1140) | (1.5260) | (10.951) | (24.678) | (2.0961) |
| **Q1** | 5.4153*** | -16.584 | 13.542*** | 23.081*** | -0.3159 | 3.9212 | -6.8666 | 23.981** |
| | (0.8623) | (27.852) | (4.7247) | (8.8574) | (0.8640) | (20.553) | (4.1771) | (10.102) |
| **Q2** | 4.2171*** | -32.361 | 12.467*** | 18.352*** | 1.3112* | 20.750 | 8.0972 | 8.1042** |
| | (0.7404) | (21.666) | (4.7232) | (7.0920) | (0.7700) | (13.823) | (6.0314) | (3.8027) |
| **Q3** | 1.8112*** | -33.980** | 7.6048*** | 8.7903*** | -9.3679*** | -72.655*** | -70.055** | 4.4733 |
| | (0.4764) | (14.940) | (2.8213) | (3.2541) | (2.3724) | (14.463) | (33.939) | (2.8776) |
| **Q4** | -0.7509 | -85.695*** | 3.8371** | 4.6801*** | -13.912*** | -145.49*** | -98.306** | -3.9479 |
| | (0.6741) | (26.042) | (1.9524) | (1.5578) | (3.1624) | (25.722) | (48.436) | (3.5088) |
| **Q5** | -1.8544*** | -79.405*** | 0.7928 | 2.0161* | -1.9195** | 25.264 | -8.8292** | -1.1960 |
| | (0.6540) | (24.638) | (1.3897) | (1.1322) | (0.7979) | (18.123) | (4.0821) | (2.6025) |
| **Q6** | 0.5066 | 7.1462 | 2.5556 | 1.8038* | 0.3866 | 31.841** | 5.3833 | -3.1861** |
| | (0.7626) | (32.003) | (2.8646) | (1.0819) | (0.8447) | (14.203) | (6.6873) | (1.3725) |
| **Season dummies** | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| **FE county** | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Notes:** The dependent variable is the quarterly flow of new entrants into a county for the corresponding risk zone (in or our of the SFHA). The sample contains policies purchased in the 7 quarters preceding the implementation adoption of RR2 ($t = -6, -5, ..., 0$) and 6 quarters after the implementation ($t = 1, 2, ..., 6$). Dummy variable $D(t \leq 0)$ takes a value of one for pre-reform quarters and quarter $t = 1$ correspond to 2022Q4, the first quarter when RR2 applied to policies for new entrants. $Trend$ is a linear trend whose slope is estimated using only the pre-reform quarters ($t \leq 0$). Dummy variables $Q1-Q6$ capture the post-reform average entry. Seasonal dummies and county fixed-effects included in all models. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1