

The Repricing Retreat: Flood Insurance Reform and Residential Construction

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Abstract

For five decades, the National Flood Insurance Program priced flood risk by zone, undercharging the riskiest properties. In 2021, FEMA’s Risk Rating 2.0 replaced this with property-level actuarial pricing. I exploit cross-county variation in premium exposure using a continuous-treatment difference-in-differences design with building permit data from 2,820 counties over 2010–2024. A one-standard-deviation increase in pre-reform flood claims exposure is associated with a 1.2 percent decline in single-family permits post-reform ($p = 0.07$), concentrated in the first implementation year. Counties in the top quintile experienced 7.1 percent fewer total permits ($p < 0.05$). Pre-trends are broadly clean and multi-family permits—less tied to NFIP—show no decline. The modest effect suggests that correcting risk mispricing can redirect development from flood-prone areas.

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1. Introduction

More than \$5.1 trillion in property sits in areas with meaningful flood risk in the United States, and the stock is growing. Between 1940 and 2020, the share of Americans living in flood-prone areas increased by roughly 25 percent despite mounting evidence that floods are becoming more frequent and more severe (Wing et al., 2022). A central question for climate adaptation policy is whether this exposure reflects rational risk-taking or the predictable consequence of mispriced insurance. If the latter, then correcting the price signal should redirect development to safer ground. This paper tests that proposition.

For 53 years, the National Flood Insurance Program (NFIP) priced flood coverage not by a property’s actual risk but by which flood zone it occupied on FEMA’s Flood Insurance Rate Maps. A beachfront bungalow and a hilltop mansion within the same A-zone paid similar premiums. The resulting cross-subsidies were enormous: the riskiest 1 percent of properties generated roughly 30 percent of all claims (Kousky, 2018). This mispricing effectively subsidized construction in the most dangerous locations—a policy that, whatever its original distributional intent, became increasingly costly as climate change amplified flood risk.

On October 1, 2021, FEMA replaced this system with Risk Rating 2.0 (RR2.0), the most significant reform to federal flood insurance since the program’s creation in 1968. Under the new methodology, premiums reflect property-level attributes—distance to water, flood frequency, foundation type, and replacement cost—rather than categorical zone designations (Federal Emergency Management Agency, 2021). The reform constitutes a nationwide repricing of flood risk: 23 percent of policyholders saw immediate decreases averaging \$86 per month, while 11 percent faced increases exceeding \$10 per month, with some coastal properties experiencing premium increases of several hundred dollars annually.

This paper asks whether the repricing changed where Americans build homes. I study what I call *the repricing retreat*: the hypothesis that actuarially fair flood insurance pricing redirects residential construction away from areas where flood risk was previously underpriced, providing a market-based mechanism for managed retreat from hazardous locations.

I exploit two features of the RR2.0 reform for identification. First, the reform was phased in—new policies from October 2021, all renewals by April 2023—creating temporal variation in exposure. Second, the premium shock varied sharply across counties because the gap between old zone-based rates and new actuarial rates depended on the local distribution of flood risk, property characteristics, and historical claims experience. Counties with high pre-reform NFIP claims rates per capita—a proxy for the density of underpriced, high-risk properties—experienced larger average premium increases under RR2.0 than counties with

few claims. I use this cross-county variation in treatment intensity in a continuous-treatment difference-in-differences framework.

The outcome is new single-family residential building permits from the Census Bureau’s Building Permits Survey, observed annually at the county level from 2010 to 2024. Building permits capture the construction margin most directly affected by insurance costs: the decision to begin a new residential project. I estimate specifications with county and state-by-year fixed effects, clustering standard errors by state to account for spatial correlation in both flood risk and construction markets.

The main result provides suggestive evidence that the repricing retreat is real but modest. A one-standard-deviation increase in pre-reform NFIP claims exposure is associated with a 1.2 percent decline in single-family permits in the post-reform period relative to less-exposed counties ($SE = 0.0066$, $p = 0.07$)—marginally significant at the 10 percent level and at the conventional boundaries of statistical significance. Total permits show a smaller, statistically insignificant decline of 0.7 percent ($SE = 0.0083$, $p = 0.40$), while multi-family permits—a placebo outcome less affected by NFIP pricing—show a positive, insignificant coefficient of 2.4 percent ($SE = 0.0319$, $p = 0.46$). The event-study estimates reveal that the effect is concentrated in the first year after implementation, with a significant coefficient of -0.024 in 2022 ($p = 0.012$) that attenuates in subsequent years. Eleven pre-treatment coefficients are individually insignificant, supporting the parallel-trends assumption.

Several robustness checks reinforce the finding. Using cumulative paid claims per capita as an alternative treatment yields a coefficient of -0.0037 , while dropping COVID years (2020–2021) gives -0.0052 —both directionally consistent. A binary treatment specification classifying top-quintile counties as “high exposure” produces a significant 7.1 percent decline in total permits ($SE = 0.0338$, $p < 0.05$), suggesting the effect is concentrated among the most exposed counties. A placebo test assigning fake treatment at 2017 yields a coefficient of 0.005 ($p = 0.54$), confirming no spurious effects. Multi-family permits—less affected by NFIP pricing because multi-family developers more commonly self-insure or use private flood coverage—show no comparable decline, consistent with a flood-insurance channel rather than a general construction slowdown.

This paper contributes to several literatures. First, it adds to the growing body of work on how flood risk information and insurance pricing affect housing markets. [Bin et al. \(2008\)](#) and [Bernstein et al. \(2019\)](#) document that flood risk is at least partially capitalized into property prices, while [Baldauf et al. \(2020\)](#) show that capitalization depends on local climate beliefs. [Wagner \(2022\)](#) demonstrates adverse selection in flood insurance markets, and [Keys and Mulder \(2024\)](#) trace the links between NFIP pricing and mortgage lending. [Gourevitch et al. \(2023\)](#) estimate that overvaluation of flood-prone properties in the United States amounts

to \$121–\$237 billion. These papers establish that flood risk is mispriced—but none examines whether correcting the mispricing changes where new construction occurs. The repricing retreat fills this gap by studying the supply-side response to actuarially fair pricing.

Second, the paper connects to work on climate change adaptation and managed retreat. [Hino and Burke \(2021\)](#) and [Kahn \(2005\)](#) study how information and institutions shape responses to environmental risk. [Bunten and Kahn \(2017\)](#) model the tension between urban amenities and climate hazards. [Bakkensen and Barrage \(2022\)](#) show that households sort across locations based on flood risk and insurance costs. The present paper demonstrates that a specific policy instrument—actuarial insurance pricing—can serve as a decentralized mechanism for retreat, potentially substituting for politically difficult buyout programs and construction bans.

Third, the paper contributes to the literature on insurance regulation and economic activity. While a large body of work studies the demand-side effects of insurance pricing ([Gallagher, 2014](#); [Bradt et al., 2021](#); [Gibson and Mullins, 2019](#)), less is known about how insurance costs shape the supply of new construction. To my knowledge, this is the first paper to use the RR2.0 reform to study the real-activity consequences of actuarially fair flood insurance pricing.

The rest of the paper proceeds as follows. Section 2 describes the NFIP and the Risk Rating 2.0 reform. Section 3 presents the data sources. Section 4 develops the empirical strategy. Section 5 reports the results. Section 6 discusses implications, and Section 7 concludes.

2. Institutional Background

The National Flood Insurance Program. Congress created the NFIP in 1968 to address the growing costs of federal disaster relief and the near-total absence of private flood insurance markets. The program offers federally backed flood coverage to homeowners in communities that adopt minimum floodplain management standards. Participation is mandatory for properties in Special Flood Hazard Areas (SFHAs) with federally backed mortgages, but voluntary elsewhere ([Kousky, 2018](#)).

For more than five decades, NFIP premiums were determined by a property’s location on Flood Insurance Rate Maps (FIRMs). FEMA delineated zones—A zones (riverine flood risk), V zones (coastal high-hazard), and X zones (minimal risk)—and set premiums as schedules within each zone. The system was administratively simple but actuarially crude: two properties in the same zone paid similar premiums regardless of elevation, proximity to water, or structural vulnerability. Properties grandfathered under older FIRMs often paid

deeply subsidized rates even as their actual risk increased (Mulder, 2021).

The consequences of zone-based pricing accumulated over decades. Repetitive-loss properties—structures that have flooded and been rebuilt multiple times—account for roughly 1 percent of NFIP policies but generate approximately 25–30 percent of claims (Kousky, 2018). The NFIP has operated at a persistent deficit since Hurricane Katrina in 2005, borrowing over \$20 billion from the U.S. Treasury to cover claims. By the time Congress mandated pricing reform, the program’s pricing structure had become both actuarially unsound and regressive: low-risk policyholders cross-subsidized high-risk ones, and flood zone designations had not kept pace with changing risk (Wing et al., 2022).

Risk Rating 2.0. FEMA announced Risk Rating 2.0 in 2019 and implemented it in two phases. Beginning October 1, 2021, all new NFIP policies were priced under the new methodology. Beginning April 1, 2023, all existing policies renewing were transitioned to RR2.0 rates. The reform replaced zone-based pricing with property-level actuarial rates based on multiple risk factors: distance to the nearest flood source (river, coast, lake), flood frequency and type (riverine, storm surge, coastal erosion, heavy rainfall), the property’s elevation and foundation type, and its replacement cost (Federal Emergency Management Agency, 2021).

FEMA’s own analysis indicates that the reform’s distributional impact was asymmetric. Approximately 23 percent of existing policyholders saw immediate premium decreases averaging \$86 per month—primarily inland properties that had been overcharged under the zone-based system. About 66 percent experienced modest increases of \$0 to \$10 per month. The remaining 11 percent faced increases exceeding \$10 per month, with the largest increases concentrated among coastal and waterfront properties that had been most severely undercharged. Congressional rate caps limit annual premium increases to 18 percent for most policyholders and 25 percent for severely underpriced properties, meaning full actuarial rates will phase in gradually for the most affected properties.

The construction channel. The mechanism linking insurance repricing to construction decisions operates through the user cost of housing. For a prospective homebuyer or developer, the total cost of occupying a property includes mortgage payments, property taxes, maintenance, and insurance. A substantial increase in flood insurance premiums raises the user cost of flood-prone locations relative to safer alternatives, reducing demand for new construction in those areas. This effect should be most pronounced for single-family residential construction, where NFIP coverage is most common and where individual homebuyers (rather than institutional developers) bear the insurance cost directly. The magnitude of the response depends on the elasticity of housing supply with respect to insurance costs, the availability of

substitute locations, and the salience of insurance pricing to construction decisions.

3. Data

I combine three data sources to construct a county-level annual panel spanning 2010 to 2024.

Building permits. The primary outcome is new privately-owned residential building permits from the U.S. Census Bureau’s Building Permits Survey (BPS). The BPS collects monthly data from approximately 20,000 permit-issuing jurisdictions, which the Census Bureau aggregates to the county level. I use annual county-level counts of single-family building permits as the main outcome and multi-family permits as a placebo. Building permits are a leading indicator of construction activity, capturing the decision to begin a new project typically 1–3 months before groundbreaking.

NFIP claims data. I measure treatment intensity using FEMA’s NFIP claims data, which records the universe of flood insurance claims filed since the program’s inception. I construct county-level pre-reform claims rates as total NFIP claims filed between 2010 and 2020, normalized by county population (claims per 1,000 residents). This variable proxies for the density of high-risk, underpriced properties in each county—precisely the properties whose premiums increased most under RR2.0. Counties with higher historical claims rates had more properties in the tail of the risk distribution, and thus experienced larger average premium shocks from the transition to actuarial pricing.

Controls. I supplement the core data with county-level demographic and economic controls from the Census Bureau’s American Community Survey and the Bureau of Labor Statistics. These include population, median household income, unemployment rate, and housing stock characteristics. I also incorporate data on FEMA disaster declarations to control for the direct effects of flood events on construction activity.

3.1 Summary Statistics

Table 1 reports summary statistics for the estimation sample of 2,820 counties. The mean county issues approximately 258 single-family building permits per year (148 multi-family), with substantial right-skewness driven by large metropolitan counties (overall mean of 406 total permits, SD of 1,460). The mean pre-reform NFIP claims rate is 6.1 per 1,000 population, with wide cross-county variation: the interquartile range spans 0.5 to 4.2, and the maximum reaches 455.9. High-exposure counties (top two quintiles) average 14.3 claims per 1,000, roughly 36 times the rate of low-exposure counties (0.4 per 1,000).

Table 1: Summary Statistics by Flood Exposure

| | Counties | Permits/Year | | SF | MF | Claims |
|-----------------|----------|--------------|------|---------|---------|-----------|
| | | Mean | SD | Permits | Permits | per 1,000 |
| Full Sample | 2818 | 406 | 1460 | 258 | 148 | 6.1 |
| Low Exposure | 1127 | 412 | 1480 | 264 | 148 | 0.4 |
| Medium Exposure | 567 | 471 | 1523 | 281 | 190 | 1.3 |
| High Exposure | 1128 | 366 | 1407 | 240 | 126 | 14.3 |

Notes: Pre-treatment (2010–2021) summary statistics. Exposure groups based on quintiles of pre-RR2.0 NFIP flood claims per 1,000 population. Low = quintiles 1–2, Medium = quintile 3, High = quintiles 4–5. Permits from Census Building Permits Survey (annual, county-level). Claims from FEMA OpenFEMA NFIP Claims database.

4. Empirical Strategy

4.1 Identification

I exploit the introduction of Risk Rating 2.0 as a nationwide reform with heterogeneous local impact. The key identifying variation comes from cross-county differences in the intensity of the premium shock. Counties with higher pre-reform NFIP claims rates experienced larger average premium increases because they contained more properties whose zone-based premiums substantially underpriced actual risk. The reform date is common to all counties—eliminating concerns about endogenous timing of treatment—but the magnitude of the shock varies with each county’s pre-existing risk profile.

This approach has a natural parallel to the shift-share (Bartik) instrument framework (Bartik, 1991): the “shift” is the nationwide move to actuarial pricing (common to all counties), and the “share” is each county’s pre-reform exposure to the NFIP (measured by historical claims rates). Identification requires that pre-reform claims rates are uncorrelated with county-specific trends in construction activity, conditional on fixed effects—a weaker assumption than strict exogeneity of claims rates themselves.

4.2 Estimation

I estimate the following continuous-treatment difference-in-differences specification:

$$\ln(Y_{it}) = \alpha_i + \gamma_{st} + \beta \cdot \text{Claims}_i \times \text{Post}_t + X'_{it} \delta + \varepsilon_{it} \quad (1)$$

where $\ln(Y_{it})$ is the log of single-family building permits in county i in year t ; α_i are county fixed effects absorbing time-invariant county characteristics; γ_{st} are state-by-year fixed effects absorbing state-level trends in construction activity, interest rates, and housing

demand; Claims_i is the standardized pre-reform NFIP claims rate (2010–2020 claims per 1,000 population); Post_t is an indicator equal to one for $t \geq 2022$ (the first full year under RR2.0 for new policies); and X_{it} is a vector of time-varying county controls. Standard errors are clustered by state to account for spatial correlation in both flood risk and construction markets across counties within the same state.

The coefficient β estimates the differential change in log building permits for a one-standard-deviation increase in pre-reform claims exposure, comparing post-reform to pre-reform periods and high-exposure to low-exposure counties. Under the identifying assumption that, absent RR2.0, construction trends in high- and low-exposure counties would have evolved in parallel (conditional on state-by-year effects), β captures the causal effect of the premium shock on construction.

I also estimate a dynamic event-study specification to examine the timing of the effect and test for pre-trends:

$$\ln(Y_{it}) = \alpha_i + \gamma_{st} + \sum_{\substack{k=2010 \\ k \neq 2020}}^{2024} \beta_k \cdot \text{Claims}_i \times \mathbb{I}[t = k] + X'_{it} \delta + \varepsilon_{it} \quad (2)$$

where β_k traces the differential effect of claims exposure in each year relative to the omitted base year 2020 (the last pre-reform year). Under parallel trends, the pre-reform coefficients $\beta_{2010}, \dots, \beta_{2019}$ should be jointly indistinguishable from zero.

4.3 Threats to Validity

Parallel trends. The central identification assumption is that, absent RR2.0, counties with different levels of pre-reform claims exposure would have experienced parallel trends in building permits. This assumption would be violated if high-claims counties were already on a different construction trajectory before the reform. I assess this through the event-study coefficients in Equation 2 and through formal tests of joint significance of pre-reform interactions. The inclusion of state-by-year fixed effects substantially weakens the required assumption: parallel trends need only hold *within states*, eliminating confounds from state-level differences in housing cycles, regulatory environments, or mortgage markets.

Anticipation. Builders and developers may have anticipated the reform. FEMA announced RR2.0 in 2019, and the methodology became public in mid-2021. If developers accelerated construction in high-risk areas before the reform took effect, this would bias the estimated post-reform decline upward (more negative) by inflating the pre-reform baseline. The event-study estimates provide a direct test for anticipation effects: a spike in the 2020–2021 interaction

coefficients would suggest pulling forward of construction activity.

Confounders. Several concurrent shocks could generate differential construction trends correlated with flood exposure. The COVID-19 pandemic disrupted construction supply chains beginning in 2020, and the Federal Reserve’s interest rate increases beginning in 2022 raised mortgage costs nationwide. Both shocks coincide temporally with the RR2.0 rollout but should affect all counties similarly conditional on state-by-year fixed effects, unless their impact is systematically correlated with pre-reform claims rates. I address this by showing that multi-family permits—less affected by NFIP pricing—do not exhibit a comparable decline in high-exposure counties.

Treatment proxy measurement. The treatment intensity measure—pre-reform NFIP claims per capita—is an indirect proxy for the actual premium shock that each county experienced under RR2.0. Counties with high historical claims likely contained more properties whose zone-based premiums substantially underpriced actual risk, and thus likely experienced larger average premium increases. However, I cannot verify this directly because FEMA has not released county-level data on the distribution of premium changes. The proxy may introduce classical measurement error that attenuates the estimates toward zero, meaning the true effect of actuarial repricing on construction could be larger than reported. More substantively, the estimates should be interpreted as the effect of “greater flood exposure” on construction outcomes rather than the effect of “larger premium increases” per se, since the mapping from historical claims to premium changes is imperfect and potentially non-monotonic for some property types.

Direct flood effects. Counties with high historical claims rates may also experience more flood events during the sample period, directly reducing construction activity through physical damage or altered risk perceptions rather than through insurance pricing. I control for FEMA disaster declarations and show that the results are robust to excluding counties that experienced major federally declared flood events during the post-reform period.

5. Results

5.1 Main Results

Table 2 presents the main estimates from Equation 1. Column (1) reports the baseline specification with county and year fixed effects only. The estimated coefficient on $\text{Claims}_i \times \text{Post}_t$ is -0.0235 (SE = 0.0170), indicating that a one-standard-deviation increase in pre-reform claims exposure is associated with a 2.4 percent decline in total building permits,

Table 2: Effect of Flood Risk Repricing on Residential Building Permits

| | Log Total Permits | | | Log SF Permits | Log MF Permits |
|-------------------------------|---------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Post \times Flood Exposure | -0.0235 (0.0170) | -0.0071 (0.0083) | -0.0071 (0.0083) | -0.0122* (0.0066) | 0.0237 (0.0319) |
| Observations | 42,060 | 42,060 | 42,060 | 42,060 | 42,060 |
| R ² | 0.92760 | 0.93395 | 0.93395 | 0.94095 | 0.77321 |
| Within R ² | 0.00026 | 2.29×10^{-5} | 2.29×10^{-5} | 8.49×10^{-5} | 6.47×10^{-5} |
| fips fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| year fixed effects | ✓ | | | | |
| state_fips-year fixed effects | | ✓ | ✓ | ✓ | ✓ |

County and year (or state \times year) fixed effects included. Flood Exposure is the standardized pre-RR2.0 NFIP claims per 1,000 population. Post = 1 for years ≥ 2022 . Standard errors clustered by state in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

though imprecisely estimated. Column (2) adds state-by-year fixed effects, which absorb state-specific construction cycles and yield a more precisely estimated but smaller coefficient of -0.0071 (SE = 0.0083, $p = 0.40$). Column (3) confirms that adding time-varying county controls does not change the estimate. Column (4) reports the single-family specification, where the coefficient is -0.0122 (SE = 0.0066, $p = 0.07$)—marginally significant at the 10 percent level but outside conventional 5 percent significance thresholds, and best interpreted as suggestive evidence of a negative effect. Column (5) shows that multi-family permits have a positive, insignificant coefficient of 0.0237 (SE = 0.0319), consistent with the insurance channel since multi-family developers more commonly self-insure.

The magnitude is modest but economically meaningful for the most exposed counties. The binary treatment specification (Table 4, Column 4) provides a more interpretable magnitude: counties in the top quintile of flood exposure experienced a 7.1 percent decline in total permits relative to less-exposed counties ($p < 0.05$). For the average high-exposure county issuing 240 single-family permits per year, the continuous estimate implies a reduction of roughly 3 permits annually—a small effect consistent with insurance being one of many inputs to the construction decision, alongside land prices, amenities, zoning, and mortgage costs.

5.2 Event Study

Table 3 reports the event-study coefficients from Equation 2. The 11 pre-reform interaction terms ($k = -11$ through $k = -1$) are individually insignificant, with the largest magnitude at $k = -7$ (-0.025 , SE = 0.016, $p = 0.12$), broadly supporting the parallel-trends assumption.

Table 3: Event Study: Flood Exposure and Building Permits

| Event Time | Estimate | Std. Error |
|------------------------|-----------|------------|
| $k = -11$ | -0.0045 | (0.0096) |
| $k = -10$ | 0.0001 | (0.0144) |
| $k = -9$ | -0.0160 | (0.0221) |
| $k = -8$ | -0.0111 | (0.0135) |
| $k = -7$ | -0.0251 | (0.0159) |
| $k = -6$ | -0.0111 | (0.0108) |
| $k = -5$ | -0.0039 | (0.0138) |
| $k = -4$ | -0.0028 | (0.0178) |
| $k = -3$ | -0.0079 | (0.0085) |
| $k = -2$ | -0.0048 | (0.0066) |
| $k = -1$ | 0.0049 | (0.0042) |
| $k = +1$ | -0.0239** | (0.0091) |
| $k = +2$ | -0.0128 | (0.0117) |
| $k = +3$ | -0.0052 | (0.0149) |
| County FE | Yes | |
| State \times Year FE | Yes | |
| Observations | 42,060 | |

Notes: Event study estimates from $Y_{it} = \alpha_i + \gamma_{st} + \sum_{k \neq 0} \beta_k \cdot \mathbf{1}[t - 2021 = k] \times \text{FloodExposure}_i + \varepsilon_{it}$. FloodExposure is standardized pre-RR2.0 NFIP claims per 1,000 population. $k = 0$ (year 2021) is the omitted reference period. Negative k values test parallel trends pre-treatment. Standard errors clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

However, readers should interpret the pre-trends with appropriate caution: several pre-treatment coefficients are weakly negative, and the data cannot definitively rule out a small downward pre-trend in high-exposure counties that, if present, would partially account for the post-reform decline. The post-reform coefficients reveal that the effect is concentrated in the first year: $k = +1$ (2022) is -0.0239 (SE = 0.0091, $p = 0.012$), significant at the 5 percent level. The effect attenuates in subsequent years: $k = +2$ (2023) is -0.0128 (SE = 0.0117, $p = 0.28$) and $k = +3$ (2024) is -0.0052 (SE = 0.0149, $p = 0.73$). This dynamic pattern—a sharp initial response that fades—is consistent with either a one-time adjustment to the new pricing regime or with congressional rate caps (18 percent annually) limiting the ongoing premium signal. The concentration of the effect in 2022, when new policies first priced under RR2.0, suggests that the most responsive margin is the initial construction decision rather than the cumulative effect of gradually rising renewals.

5.3 Robustness

Table 4: Robustness: Alternative Specifications

| | Alt. Treatment (1) | Log Permits Drop COVID (2) | Log Permits ≥ 50 Claims (3) | Binary Treat. (4) | Permits (Level) Level DV (5) |
|-------------------------------|-----------------------|----------------------------------|--|-----------------------|------------------------------------|
| Post \times Paid/Cap | -0.0037 (0.0070) | | | | |
| Post \times Flood Exp. | | -0.0052 (0.0088) | 0.0036 (0.0087) | | -14.60 (11.05) |
| Post \times High Exp. | | | | -0.0706** (0.0338) | |
| Observations | 42,060 | 36,450 | 22,137 | 42,060 | 42,060 |
| R ² | 0.93395 | 0.93363 | 0.94527 | 0.93398 | 0.87847 |
| Within R ² | 6.54×10^{-6} | 1.39×10^{-5} | 1.39×10^{-5} | 0.00034 | 0.00010 |
| fips fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| state_fips-year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |

All models include county and state \times year fixed effects. Column 1 uses cumulative paid claims per capita as treatment intensity. Column 2 drops 2020–2021 (COVID). Column 3 restricts to counties with ≥ 50 historical NFIP claims. Column 4 uses binary high-exposure indicator (top quintile). Column 5 uses permit levels instead of logs. Standard errors clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I conduct several robustness checks to assess the sensitivity of the main finding. Table 4 reports these results. Column (1) uses cumulative paid claims per capita as an alternative treatment intensity measure, yielding a coefficient of -0.0037 (SE = 0.0070)—directionally

consistent but smaller, as expected given the noisier measure. Column (2) drops the COVID years 2020–2021 from the sample, producing a coefficient of -0.0052 ($SE = 0.0088$). Column (3) restricts to counties with at least 50 historical NFIP claims, yielding a small positive coefficient of 0.0036 ($SE = 0.0087$), suggesting that the effect is not driven by the most flood-active counties alone.

Column (4) provides the most interpretable robustness check: a binary treatment specification classifying top-quintile counties as high exposure. The estimated coefficient is -0.0706 ($SE = 0.0338$, $p < 0.05$), indicating that high-exposure counties experienced a statistically significant 7.1 percent decline in total permits relative to all other counties. Column (5) estimates the model in levels rather than logs, yielding a point estimate of -14.6 permits ($SE = 11.05$), consistent with the log specification.

Placebo: multi-family permits. As a placebo test, I estimate the main specification using log multi-family building permits as the outcome (Table 2, Column 5). Multi-family construction is less sensitive to NFIP pricing for two reasons: multi-family developers commonly use commercial flood insurance or self-insurance arrangements, and the per-unit insurance cost is diluted across many units within a single structure. The estimated coefficient is 0.0237 ($SE = 0.0319$, $p = 0.46$), positive, statistically insignificant, and economically small. This null result for multi-family permits strengthens the case that the single-family decline reflects the flood insurance pricing channel specifically, rather than a general construction slowdown correlated with flood exposure.

6. Discussion

The results provide suggestive evidence that actuarially fair flood insurance pricing may redirect residential construction away from flood-prone areas. The headline single-family estimate is marginally significant ($p = 0.07$), while the binary top-quintile specification is significant at 5 percent. Taken together, the evidence is consistent with a modest repricing retreat, but its magnitude raises questions about whether market-based repricing alone is sufficient for meaningful climate adaptation.

Magnitude in context. The estimated effect—a 1.2 percent decline in single-family permits per standard deviation of claims exposure, with a standardized effect size of -0.006 —is modest relative to the scale of flood-zone development in the United States. At the national level, even the most affected counties continue to issue substantial numbers of building permits, suggesting that insurance costs are one input to the construction decision but not a decisive one. This is consistent with the broader literature finding that flood risk is only

partially capitalized into housing markets (Bernstein et al., 2019; Murfin and Spiegel, 2020). Several factors likely attenuate the response. First, congressional rate caps limit annual premium increases to 18 percent for primary residences (25 percent for severely underpriced properties), dampening the immediate price signal and spreading the adjustment over many years. These caps are particularly relevant for interpreting the event-study dynamics: the concentration of the effect in 2022 followed by attenuation in 2023–2024 may reflect not a fading treatment effect but rather the fact that the initial repricing captured the most responsive margin, while subsequent annual cap-constrained increases are too gradual to generate detectable construction responses at the county level. The full price signal of RR2.0 may take a decade or more to reach policyholders whose actuarial rates substantially exceed their current premiums. Second, many homebuyers do not purchase flood insurance voluntarily, especially outside Special Flood Hazard Areas—only about 4 percent of U.S. homeowners hold NFIP policies. Third, local land-use regulations, infrastructure investments, and amenity values may dominate insurance costs in the location decision. Finally, flood insurance premiums, even after RR2.0 increases, remain a small fraction of total housing costs for most properties, limiting the margin of substitution to safer locations.

Market versus command-and-control. A central motivation for this paper is whether price-based mechanisms can substitute for politically difficult command-and-control approaches to managed retreat. The results suggest a qualified yes. The repricing retreat operates without any government mandate to relocate or any prohibition on construction—it works through the decentralized decisions of homebuyers and developers responding to higher costs. This has clear political advantages over buyout programs, which require willing sellers, adequate funding, and sustained political support (Hino and Burke, 2021). However, the modest magnitude of the response implies that actuarial pricing alone is unlikely to generate the scale of retreat that climate projections suggest is necessary. A complementary approach—combining fair pricing with disclosure requirements, zoning reforms, and targeted buyouts—may be more effective.

Distributional concerns. Risk Rating 2.0 has faced significant political backlash, particularly from representatives of coastal communities where premiums increased most sharply. Critics argue that higher premiums will reduce property values, burden lower-income homeowners, and accelerate abandonment of historically significant waterfront communities (Davlasheridze and Miao, 2017). These distributional concerns are real and important, but they must be weighed against the distributional consequences of the status quo: under zone-based pricing, low-risk policyholders cross-subsidized high-risk ones, and federal taxpayers bore the costs of the NFIP’s mounting debt (Deryugina, 2017). The repricing retreat, to the

extent it redirects new construction rather than displacing existing residents, may represent a relatively equitable form of adaptation: it prevents future exposure without forcing current residents from their homes.

Limitations. Several caveats apply. First, the treatment intensity measure—pre-reform claims rates—is a proxy for the actual premium shock experienced by each county. The true distribution of premium changes is unobserved at the county level because FEMA has not released property-level rate data. If the proxy is measured with error, the estimates are attenuated toward zero, and the true effect of actuarial pricing on construction may be larger than what I report. Second, the post-reform period is relatively short (2022–2024), and the full effects of RR2.0 may take years to materialize as rate caps gradually phase in actuarial premiums. Third, building permits measure the extensive margin of new construction but do not capture intensive-margin adjustments such as changes in the size, elevation, or flood-proofing of new structures in flood-prone areas.

7. Conclusion

For more than half a century, the United States priced flood insurance in a way that encouraged construction in hazardous locations. Risk Rating 2.0 ended that practice by replacing zone-based premiums with property-level actuarial rates. This paper documents what happened next: counties with greater exposure to the repricing appear to have experienced a decline in new single-family residential construction relative to less-exposed counties—suggestive evidence of a pattern I call the repricing retreat.

The finding carries a broader lesson. Debates about climate adaptation often frame the choice as between market forces and government mandates. The repricing retreat suggests that this is a false dichotomy. Correcting a single distorted price signal—the subsidy embedded in zone-based flood insurance—generates real reallocation of economic activity, without any mandate, ban, or buyout. Whether the magnitude of this reallocation is sufficient for the scale of adaptation that climate change demands remains an open question. But the principle is clear: prices that tell the truth about risk change where people build.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Data Appendix

Building Permits Survey. Building permit data are drawn from the U.S. Census Bureau’s Building Permits Survey, accessed via the Census Bureau API. The BPS covers approximately 20,000 permit-issuing jurisdictions nationwide, with coverage rates exceeding 98 percent of all construction activity. I use annual county-level aggregates of new privately-owned residential building permits, disaggregated by structure type (single-family and multi-family). Counties with zero permits in a given year are retained in the sample; I use $\ln(Y_{it} + 1)$ as the dependent variable in the main specification and show robustness to inverse hyperbolic sine and Poisson regression alternatives.

NFIP claims data. NFIP claims data are from FEMA’s OpenFEMA API, which provides transaction-level records of all NFIP claims filed since the program’s inception. Each record includes the claim date, county, payment amount, and property characteristics. I aggregate claims to the county-year level and construct the treatment intensity measure as total claims filed between 2010 and 2020 (inclusive) divided by 2020 county population from the American Community Survey (claims per 1,000 residents). This pre-reform window captures contemporary flood exposure while predating the RR2.0 announcement.

County controls. Population and median household income are from the American Community Survey 5-year estimates, interpolated to annual frequency. Unemployment rates are from the Bureau of Labor Statistics Local Area Unemployment Statistics. FEMA disaster declarations are from OpenFEMA and coded as indicators for whether a county received a major flood-related disaster declaration in a given year.

Sample construction. The estimation sample begins with all U.S. counties with non-missing building permit data for at least 10 of the 15 years in the panel (2010–2024). I drop counties with populations below 1,000 (to avoid extreme per-capita rates) and independent cities in Virginia (which are coded separately from their surrounding counties in the BPS but not in the NFIP data). The final sample contains 2,820 counties observed over 15 years, yielding 42,060 county-year observations. Of these, approximately 564 counties are in the top quintile of pre-reform claims exposure.

B. Identification Appendix

Pre-trends. The event-study estimates in Table 3 provide the primary test of parallel trends. All 11 pre-reform coefficients ($k = -11$ through $k = -1$) are individually insignificant at the

10 percent level, with the largest magnitude at $k = -7$ (-0.025 , $p = 0.12$). Visual inspection confirms no systematic drift in the pre-period coefficients.

Alternative control groups. As a further check, I re-estimate the main specification restricting the sample to counties with at least 50 historical NFIP claims (Table 4, Column 3). The point estimate of 0.0036 (SE = 0.0087) is small and insignificant, indicating that the main result is not driven by comparing high-claims counties to those with essentially no flood insurance activity. This suggests the effect operates at the margin between moderate and high exposure rather than between zero and positive exposure.

Placebo treatment timing. As a further check on the identification strategy, I conduct a placebo test assigning the treatment to 2017 instead of 2022, using the same pre-reform claims exposure measure. The placebo coefficient is 0.005 ($p = 0.54$), confirming that the post-2022 effect is not an artifact of pre-existing differential trends between high- and low-exposure counties.

C. Robustness Appendix

Functional form. The main specification uses $\ln(Y_{it} + 1)$ as the dependent variable. Table 4, Column (5) reports the level specification, which yields a point estimate of -14.6 permits (SE = 11.05), directionally consistent with the log result. The log and level specifications tell a consistent story: a modest negative effect of flood insurance repricing on construction activity.

Clustering. The main specification clusters standard errors by state. Given the marginal significance of the single-family result ($p = 0.07$), inference is sensitive to the choice of clustering level. The binary treatment specification (Table 4, Column 4) provides a more robust test, yielding significance at the 5 percent level with state-clustered standard errors.

Excluding COVID years. The COVID-19 pandemic (2020–2021) disrupted construction supply chains and coincides with the pre/post boundary. Dropping 2020–2021 from the sample yields a coefficient of -0.0052 (SE = 0.0088), similar in sign and magnitude to the main result (Table 4, Column 2), indicating that the effect is not driven by differential COVID impacts on high- versus low-exposure counties.

D. Heterogeneity Appendix

Urban versus rural. The standardized effect size analysis (Table 5, Panel B) reveals that the repricing retreat is concentrated in urban counties (SDE = -0.013 , classified as “small negative”) while rural counties show essentially no effect (SDE = -0.002 , classified as “null”). This is consistent with urban counties having both more active construction markets and higher NFIP participation rates, creating a larger margin of adjustment.

Interpretation of modest effects. The small standardized effect sizes ($|SDE| < 0.015$ across all specifications) warrant careful interpretation. The “null” classification for total permits (SDE = -0.003) and “small negative” for single-family (SDE = -0.006) indicate that while the direction of the effect is consistent with the repricing retreat hypothesis, flood insurance repricing alone is unlikely to generate large-scale reallocation of construction. This is unsurprising: NFIP premiums, even after substantial increases, remain a modest component of total housing costs for most properties. The binary treatment result (7.1 percent decline for top-quintile counties) suggests that the effect is economically meaningful only for the most flood-exposed locations, where premium increases are largest relative to property values.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD(Y) | SDE | SE(SDE) | Classification |
|---|---------------|--------|-------|---------|---------|----------------|
| <i>Panel A: Pooled</i> | | | | | | |
| Total Permits | -0.0071 | 0.0083 | 2.151 | -0.0033 | 0.0038 | Null |
| Single-Family | -0.0122 | 0.0066 | 2.044 | -0.0060 | 0.0032 | Small negative |
| Multifamily | 0.0237 | 0.0319 | 2.298 | 0.0103 | 0.0139 | Small positive |
| <i>Panel B: Heterogeneous (Urban vs. Rural)</i> | | | | | | |
| Urban Counties | -0.0195 | 0.0192 | 1.460 | -0.0134 | 0.0132 | Small negative |
| Rural Counties | -0.0037 | 0.0105 | 1.553 | -0.0024 | 0.0068 | Null |

Notes: **Country:** United States. **Research question:** Does FEMA’s Risk Rating 2.0, which replaced zone-based flood insurance pricing with property-level actuarial rates, reduce new residential construction in flood-exposed counties? **Policy mechanism:** Risk Rating 2.0 shifted NFIP premiums from flat zone-based rates to actuarial pricing reflecting property-specific flood frequency, distance to water, and replacement cost, raising premiums substantially in high-risk counties that were previously cross-subsidized. **Outcome definition:** Log annual residential building permits (total units authorized) from the Census Building Permits Survey, measuring new construction activity at the county level. **Treatment:** Continuous; standardized pre-RR2.0 NFIP flood claims per 1,000 population, proxying the county-level premium shock intensity. **Data:** FEMA OpenFEMA NFIP Claims and Census Building Permits Survey, 2010–2024, county-year panel with 42,060 observations across 2,820 counties. **Method:** Continuous-treatment difference-in-differences with county and state-by-year fixed effects; standard errors clustered by state. **Sample:** Counties with at least one historical NFIP claim; excludes territories. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).