The Effects of a Mandatory Flood Risk Disclosure Law on Rental Prices and Residential Sorting in Texas

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Abstract

Seven U.S. states have adopted mandatory flood-risk disclosure laws requiring landlords to notify prospective tenants when properties lie within designated flood zones. These policies aim to improve market efficiency and protect renters by making environmental risk more transparent. This paper provides the first empirical evaluation of such a law in the rental housing market, focusing on Texas's statewide disclosure mandate, effective January 1, 2022. Using a novel dataset linking over 350,000 geocoded rental listings (2019–2024) to high-resolution flood maps and ZIP-level demographics, I estimate difference-in-differences models comparing rent changes for flood-prone and non-flood-prone units before and after the law. Event-study results show parallel pre-trends and no post-policy rent divergence, while complementary analyses using American Community Survey data reveal no systematic demographic or mobility responses. These findings suggest that disclosure alone, absent salience, timing, or enforcement, was insufficient to reprice or redistribute flood risk in rental markets, underscoring the limits of transparency as an adaptation tool.

Keywords: Flood Risk Disclosure, Rental Housing Market, Climate Risk Salience, Information Asymmetry, Environmental Justice, Household Sorting

JEL Codes: Q54, Q58, R31, R38

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I Introduction

Flooding is the most frequent and financially destructive natural disaster in the United States. As the frequency and severity of extreme weather intensify, flood risk is becoming a central determinant of housing affordability and neighborhood stability. Renters, who generally have fewer financial resources and less access to insurance than homeowners, often remain unaware of a property's flood history or hazard status at the time of lease signing. This lack of information prevents markets from fully pricing climate risk and leaves tenants disproportionately exposed to damage and displacement. To address this problem, seven U.S. states, including Texas, have adopted mandatory flood risk disclosure laws that require landlords to inform prospective tenants when dwellings lie within designated flood zones. These policies aim to make environmental risk salient and promote more efficient and equitable housing outcomes. Whether they achieve these goals, however, remains largely unknown.

In this paper I provide the first empirical evaluation of a mandatory flood risk disclosure policy in the rental housing market. I study Texas's statewide disclosure law, which took effect on January 1, 2022 and requires landlords to disclose, prior to the signing of a lease agreement, whether the property is located in a Federal Emergency Management Agency (FEMA) 100-year floodplain and whether it has flooded in the past five years. The disclosure must be provided as a separate form, distinct from the lease agreement, to ensure that tenants receive and review the information before committing to the property.[1] Leveraging a new dataset that links geocoded rental listings to detailed flood hazard maps and neighborhood demographics, I examine how rents and local sociodemographic composition changed after the law took effect. The analysis shows no evidence that the disclosure law repriced flood risk or altered neighborhood composition, indicating limited behavioral responses to the policy's implementation.

Rental markets warrant distinct attention because they reflect a different margin of adjustment than owner-occupied housing. Homeowners internalize both the financial and amenity components of flood risk, since their property is both an investment and a place of residence. Renters, by contrast, bear only the amenity risk associated with potential flooding, such as disruption or loss of personal

[1] The full statutory text of the law and the standardized disclosure form are provided in Appendix C. Texas is one of seven states (California, Georgia, Indiana, New Jersey, Oklahoma, Oregon, and Texas) that have enacted similar flood-risk disclosure laws for rental properties, while twenty-eight states have comparable statutes governing home sales.

belongings, but not the long-term asset exposure. Because they typically sign shorter leases, face lower search costs, and often lack flood insurance, renters can in principle respond more quickly to new information about risk. If disclosure meaningfully alters perceived flood risk, it should be visible in rent prices or in the sorting of tenants across neighborhoods. Conversely, the absence of such effects would suggest that the amenity value of flood risk is small or that information arrives too late in the housing search to influence decisions.

These dynamics are particularly salient in Texas, where 37% of households were renters in 2024 (Texas Housers, 2024), above the national average of 34% (U.S. Census Bureau, 2024; Arbor Realty Trust, 2024). Moreover, about one in six Texans lives or works in a known flood hazard area, and roughly one-fifth of the state's land area lies within the Federal Emergency Management Agency's 100-year floodplain (Texas Water Development Board, 2024b).[2] With its high rental share, rapid turnover, demographic diversity, and extensive flood exposure, Texas provides an ideal setting to study how mandatory risk disclosure affects renter behavior and the distribution of environmental risk.

The analysis draws on a novel dataset linking more than 350,000 geocoded rental listings (2019–2024) from CoreLogic's proprietary Multiple Listing Service (MLS) data to high-resolution flood hazard maps from the Texas Water Development Board and ZIP-level demographic data from the U.S. Census Bureau (Texas Water Development Board, 2024a; U.S. Census Bureau, 2021, 2023). Each listing is spatially matched to the Federal Emergency Management Agency's (FEMA) 100-year and 500-year floodplains, which are areas with a 1 percent and 0.2 percent annual chance of flooding, respectively, and to locally identified flood-risk zones derived from municipal and regional flood studies (Texas Water Development Board, 2024b). To estimate the causal effect of disclosure salience on rents, I implement a difference-in-differences design comparing pre- and post-policy rental prices for flood-prone and non-flood-prone units, controlling for property characteristics, ZIP-by-year fixed effects, and ZIP-by-100-year-floodplain fixed effects. To capture persistent and transient rental responses, in addition to the pooled sample, I estimate a repeat-listing specification with property-level fixed effects that isolates within-unit rent changes over time. Event-study specifications confirm parallel pre-trends, supporting the validity of the identification strategy. In further analyses, I incorporate county-level unemployment and COVID-19 incidence rates, ZIP-level

[2]See Figure 1 for a full map of Texas's floodplains.

baseline socioeconomic characteristics, and contemporaneous flood activity (NFIP claims) to test robustness. To evaluate potential sorting responses, I complement the rent analysis with ZIP-level regressions of demographic and mobility changes using American Community Survey (ACS) 5-year estimates and pre-2022 flood-risk exposure intensity.

Across all specifications, the analysis finds no evidence that Texas's 2022 disclosure law repriced flood risk into rents. Estimated difference-in-differences coefficients for properties inside the 100-year FEMA floodplain are small, precisely estimated, and statistically indistinguishable from zero. Confidence intervals rule out even modest rent increases or decreases, suggesting that rental markets did not incorporate flood-risk information into equilibrium pricing following the law's implementation. The null effects persist across pooled and repeat-listing samples, year-specific models, and specifications that vary the treatment definition, the level of spatial aggregation, and the inclusion of property, socioeconomic, and contextual controls. The precision of these estimates indicates that the absence of a price response is not an artifact of limited power but a robust empirical finding: disclosure alone did not alter how rental markets priced flood risk.

Complementary analyses of neighborhood composition reach the same conclusion. ZIP-level regressions linking changes in demographic and mobility characteristics between the 2017–2021 and 2019–2023 ACS 5-year estimates to pre-2022 flood-risk exposure reveal no systematic sorting along flood-risk lines. Across all model specifications, from simple bivariate regressions to fully adjusted models with baseline socioeconomic controls and county fixed effects, estimated coefficients on poverty, racial and ethnic composition, educational attainment, renter share, median age, and mobility outcomes are uniformly small and statistically indistinguishable from zero. The consistency of these null effects across specifications and weighting schemes indicates that the law neither repriced flood risk nor reshaped the spatial distribution of who bears it. Flood-risk disclosure, as implemented, left both rental prices and neighborhood composition effectively unchanged.

This paper makes two primary contributions. First, it extends the literature on the capitalization of flood risk in housing markets. A large hedonic literature examines how flood risk becomes salient and priced into property values, typically by comparing homes inside and outside mapped flood zones and exploiting variation over time following major flood events (Hallstrom & Smith, 2005; Gibson et al., 2017; Ortega & Taspınar, 2018), revisions to official FEMA flood maps (Bin & Landry, 2013; Daniel et al., 2009; Kousky, 2010; Johnston & Moeltner, 2019; Pollack et al., 2023; Pope, 2008b), or the introduction of flood-risk disclosure mandates (Hsieh, 2021; Troy & Romm, 2003). Most studies find single-digit price discounts for homes located within floodplains, implying that buyers partially internalize flood risk once it becomes salient. Other work, however, reports effects that are small, short-lived, or statistically insignificant once neighborhood amenities and geographic heterogeneity are carefully controlled (Beltrán et al., 2018; Bin & Kruse, 2006; Rajapaksa et al., 2017; Sado-Inamura & Fukushi, 2019). Recent comprehensive analyses conclude that U.S. housing markets systematically undervalue flood risk, with aggregate mispricings in the tens to hundreds of billions of dollars (Bakkensen & Barrage, 2022; Gourevitch et al., 2023; Hino & Burke, 2021; Ouazad & Kahn, 2022). Virtually all of this work centers on owner-occupied housing, and no prior research has empirically evaluated how flood-risk disclosure affects rental markets, which account for more than one-third of U.S. households. This paper fills that gap by providing the first causal evidence on the capitalization, or lack thereof, of flood-risk disclosure in the rental market; a rapidly growing but understudied segment that increasingly determines which households are exposed to climate-related risks.

Second, the paper contributes to the literature on mandatory environmental disclosure, sorting, and environmental justice by examining how information about risk affects housing outcomes and the spatial distribution of vulnerability. Prior studies show that disclosure policies can shape housing and location decisions across a wide range of environmental contexts, including airport noise (Pope 2008a; Sanders 2014), toxic releases (Currie et al. 2015), groundwater and drinking-water contamination (Bui & Mayer 2003; Meeks et al. 2016), septic and soil hazards (Athnos 2019), and energy efficiency labeling (Myers 2020; Breshears 2022; Cassidy 2022). Much of this evidence concerns owner-occupied housing and uses hedonic or matching frameworks. Building on foundational research documenting that environmental risks and amenities are unevenly distributed and that more affluent households are better able to adapt when risks become salient (Been 1994; Pastor et al. 2001; Currie, Voorheis & Walker 2020), this paper provides new evidence from rental markets, where shorter lease terms, weaker financial attachment, and limited insurance coverage may heighten vulnerability despite greater physical mobility. It also connects the disclosure literature to work on migration and sorting in response to economic and environmental shocks (Boustan et al. 2020; Diamond 2016; Hornbeck 2014; Deryugina et al. 2018), highlighting how information, mobility, and inequality interact to shape who ultimately bears climate-related risk. By linking flood-risk disclosure to both capitalization and sorting dynamics, the paper bridges the literatures on environmental information, housing behavior, and environmental justice within the context of a large and economically significant rental market.

The remainder of the paper is organized as follows. Section II describes the data sources, sample construction, and empirical framework. Section III presents the main results on rental prices and demographic sorting, along with a series of robustness checks assessing the sensitivity of the findings. Section IV offers a broader discussion of the results, highlighting their policy implications and situating them within the disclosure and sorting literatures. Section V concludes. Tables and figures appear in Sections VI and VII, respectively. References appear in Section VIII, and supplementary materials are provided in Appendix A, Appendix B, and Appendix C.

II Data and Methodology

II.I Data Sources

This paper draws on a uniquely detailed dataset combining proprietary transaction-level rental listings from CoreLogic's Multiple Listing Service (MLS) with flood hazard geospatial data from the Texas Water Development Board (2024a), alongside a rich set of controls. This combination enables an empirical analysis of how flood risk salience, brought about by a mandatory disclosure policy implemented on January 1, 2022, affects rental prices across the state of Texas. The primary dataset is provided by CoreLogic, a leading real estate data provider that compiles information from a range of public and private MLS boards. The data include rental property listings submitted by licensed real estate agents affiliated with participating MLS boards. Listings by individual homeowners or informal rentals are excluded, as only agents who are members of MLS boards can submit listings. While CoreLogic's MLS data are widely used in studies of the home sales market, the rental component remains underutilized despite its depth and granularity. The data span the years 1970 to 2024 and include rich property-level information such as original and final rental prices, listing dates, geographic coordinates, and structural characteristics (e.g., square footage, number of bedrooms and bathrooms, and year built).

For the main analysis, I use the final rental price rather than the original (asking) price, for two main reasons. First, any gap between the asking and final price could capture the treatment effect itself, as prospective tenants may negotiate lower rents when flood risk becomes salient. Second, the final price indicates that a lease agreement was successfully executed, providing a cleaner measure of market-clearing rents even though this field is available for fewer listings. Focusing on final prices therefore allows for a more precise assessment of how disclosure shaped realized rental outcomes rather than advertised ones. Results are similar when using original asking prices (see Table A7). For the purposes of this paper, I restrict the sample to listings from 2019 to 2024. This window provides several years of observations before and after the January 1, 2022 policy implementation, enabling a credible difference-in-differences design while minimizing noise from longer-term structural shifts in the housing market.

With the outcome measure and study window defined, the next step is to link each property to spatial flood hazard data to identify treatment status. To measure flood risk exposure, I match each property to the Texas Water Development Board's "Existing Condition Flood Hazard" geodatabase (Texas Water Development Board, 2024a). This geospatial dataset includes both FEMA-designated 100-year (1.0% annual chance) and 500-year (0.2% annual chance) floodplains, as well as additional flood-prone areas identified through local agency input and community meetings. This matching allows for the construction of several binary indicators: (i) *FEMA100*, indicating whether a property lies within a 100-year floodplain; (ii) *FEMA500*, for 500-year floodplains; (iii) a combined *FEMA(100+500)* indicator; and (iv) *Non-FEMA Map*, identifying properties outside FEMA zones but still designated as at risk through alternative sources. Because the disclosure form explicitly requires landlords to disclose whether a property is located within the 100-year FEMA floodplain, *FEMA100* serves as the primary treatment variable in this study. The remaining indicators are used to explore heterogeneous effects of flood risk exposure on rental prices and to conduct robustness checks using broader definitions of flood risk, including a composite *FloodRisk* variable equal to one if any of these conditions hold. Finally, I define a binary post-treatment indicator equal to one for listings after January 1, 2022, the law's effective date.

In addition to the primary datasets, I integrate several auxiliary data sources to account for local shocks and socioeconomic conditions that may influence rental market dynamics. Flood activity during the study period is measured using ZIP-level aggregates of National Flood Insurance Program (NFIP) claims, obtained from FEMA (U.S. Federal Emergency Management Agency, 2025). To capture broader economic and public health shocks, I use county-level unemployment data from the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) and county-level COVID-19 incidence rates from the Johns Hopkins University Center for Systems Science and Engineering (CSSE). Finally, ZIP-level sociodemographic characteristics, such as median household income, renter share, educational attainment, Hispanic population share, and median age, are drawn from the U.S. Census Bureau's American Community Survey (ACS) 5-year Data Profiles (2017–2021 and 2019–2023). These data serve as contextual controls in robustness exercises and form the basis of the sorting analysis discussed in Section II.III.

The dataset includes all residential rental listings and excludes home sales transactions. Listings with missing rent values are removed, and price outliers are trimmed at the 0.5th and 99.5th percentiles, following CoreLogic's data quality guidance. Only properties listed as residential are retained, which account for 99.05% of all rental observations. A small fraction of the dataset consists of commercial (0.95%) and land (0.005%) listings, which are excluded from the analysis. The final sample contains 356,117 residential rental listings across 165 counties in Texas. Among these counties, Harris County alone accounts for 34.6% of all listings, followed by Dallas (10.2%), Tarrant (9.1%), Collin (7.9%), and Fort Bend (6.4%), reflecting the population density and rental market concentration in urban regions. Consistent with this, 97.15% of the sample is classified as urban. The majority of listings consist of single-family homes and condominiums, which together represent roughly 80% of the sample. For more information, please refer to Table 2.

Each row in the dataset represents a property at a specific point in time. The variable *FullAddress* serves as a unit-level identifier and uniquely identifies 275,757 rentals. Repeated listings under the same address value occur because a unit was relisted at different times. Of the total 275,757 uniquely identified rental units, 213,260 addresses (77.34%) are non-repeating, while 62,497 addresses (22.66%) appear more than once. Among these repeated addresses, 48,865 (78.2%) appear exactly twice, 10,614 (17.0%) appear three times, and 2,235 (3.6%) appear four times. A much smaller share of addresses (the remaining 1.2%) appear five or more times, with frequency dropping off sharply as the count increases.

II.I Descriptive Statistics

Descriptive statistics for the main analysis variables are presented in Table 1. The average original rent price is \$2,247 (in 2022 dollars), with a standard deviation of \$1,018. The final price at which the property was listed is slightly lower on average, at $2,131. Fifteen percent of properties are exposed to flood risk under at least one of the four definitions. Among these, 6 percent are located within the FEMA-designated 100-year floodplain and 7 percent within the 500-year floodplain. These categories are defined to be mutually exclusive, such that no property is double-counted across floodplain types. Two percent of listings are flagged as flood-prone via non-FEMA local assessments. The average listing year is 2021. On average, rental units have 2.3 bathrooms, 3.0 bedrooms, and a living area of 1,792 square feet. The average structure was built in 1992, and listings remain on the market for an average of 36 days. To complement these summary statistics, I also examine how average rents and listing characteristics evolved over time across treatment groups.

Figure 2 shows mean rent trends for listings located inside and outside the FEMA 100-year floodplain between 2019 and 2024. To facilitate comparison, average log final rents are residualized with respect to property characteristics (number of bedrooms, number of bathrooms, and log living area) and ZIP-by-year fixed effects, then normalized to equal zero in 2021, the year preceding the disclosure law. This adjustment removes variation attributable to differences in rental unit composition or local market shocks, allowing the figure to isolate average rent dynamics by flood-risk status. The trends reveal broadly parallel trajectories before 2022 and a modest, short-lived divergence afterward, suggesting a temporary post-policy uptick in flood-zone rents that later dissipates.

To assess whether the brief post-2022 fluctuation in average rents reflects compositional shifts rather than price changes, Figure 3 plots average square footage and number of bedrooms for flood-zone listings over time, indexed to 2021 = 100. Both characteristics exhibit a modest temporary increase in 2022, suggesting that the observed uptick in mean rents coincides with a shift toward larger or higher-amenity listings. Together, these descriptive figures provide an intuitive depiction of rent and listing composition dynamics around the disclosure law's implementation and motivate the difference-in-differences framework developed in Section II.II.

Supplementary descriptive statistics for auxiliary data sources are reported in Appendix Table A2. The log of NFIP claims per 100 listings (plus one) has a mean of 0.63. Quarterly changes in county unemployment rates average 0.05 percentage points, and COVID-19 incidence averages approximately 1,535 new cases per 100,000 residents quarterly. Together, these time-varying county- and ZIP-level measures capture short-term shocks that may influence rental markets. Complementing these are baseline demographic and socioeconomic indicators that characterize the longer-run attributes of the areas represented in the sample. ZIP-level socioeconomic covariates from the U.S. Census Bureau's American Community Survey (ACS) 2017–2021 5-year Data Profiles provide additional context for the areas represented in the rental dataset. Summary statistics are calculated from the subset of ZIP codes that appear in the CoreLogic sample, ensuring geographic comparability. The average ZIP code has a median household income of approximately $75,200 (in 2022 dollars) and a population of about 25,000 residents. On average, 33.8% of households are renter-occupied, 28.1% of adults hold a bachelor's degree or higher, and 33.2% of residents identify as Hispanic, while 12.1% identify as Black. The median age is 38 years, and roughly 11% of the population lives below the poverty line. Residential stability is high, with 85.9% of residents living in the same house one year prior, 7.6% having moved within the same county, and 1.6% having moved from a different state. These ZIP-level covariates, along with their counterparts from the 2019–2023 ACS 5-year estimates, form the basis of the sorting analysis described in Section II.III.

While the data are well suited for evaluating pricing dynamics, three limitations should be acknowledged. First, the binary flood exposure variables may understate true risk in areas where flood mapping is outdated or incomplete, potentially introducing measurement error. Second, the dataset lacks information on the unit's floor level, which could affect vulnerability to flooding, particularly in multi-unit buildings. However, this limitation is less concerning given that most observations are single-family homes and condominiums. Lastly, the fact that 97% of the sample is urban may limit the generalizability of the findings to rural areas, where housing markets and flood risk dynamics could differ.

II.II Methodology

II.I Difference-in-Differences Framework

To estimate the effect of Texas's mandatory flood-risk disclosure policy on rental prices, I implement a difference-in-differences model comparing listings inside and outside the 100-year FEMA floodplain before and after January 1, 2022. The estimating equation is:

$$\ln(P_{izt}) = \beta_0 + \beta_1(1[\text{Post}]_t \cdot 1[\text{FEMA100}]_i) + \text{X'}_{izt}\beta_2 + \alpha_{zt} + \psi_{zf} + \varepsilon_{izt}$$

where $\ln(P_{izt})$ is the log of the final listed rent for property i in ZIP code z and year t. The indicator $1[\text{Post}]_t$ equals one for listings on or after January 1, 2022, the effective date of the disclosure law, and zero otherwise. The indicator $1[\text{FEMA100}]_i$ equals one if the listing is located within the 100-year FEMA floodplain and zero otherwise. Their interaction, $1[\text{Post}]_t \times 1[\text{FEMA100}]_i$, captures the post-policy rent differential between flood-zone and non-flood-zone listings. $X'_{izt}\beta_2$ is a vector of property-level characteristics, including the log of living area, number of bedrooms, and number of bathrooms. α_{zt} are ZIP-by-year fixed effects that absorb local time-varying shocks common to all listings within the same ZIP code and year. ψ_{zf} are ZIP-by-100-year-floodplain fixed effects that control for time-invariant differences between flood-zone and non-flood-zone listings within a ZIP code, where $f = 1$ if the listing is located inside the 100-year FEMA floodplain and $f = 0$ otherwise. Standard errors, ε_{izt}, are clustered at the ZIP-code level to account for within-ZIP correlation.

The coefficient of interest, β_1, captures the average causal effect of the flood-risk disclosure policy on listings located inside the 100-year FEMA floodplain relative to comparable listings outside the floodplain within the same ZIP code. Identification relies on the standard parallel-trends assumption: absent the disclosure law, average rent trajectories for flood-zone and non-flood-zone listings within a ZIP code would have evolved similarly. Because this assumption underpins the validity of the difference-in-differences framework, it is important to assess its plausibility directly.

II.II Parallel Trends Assumption

To evaluate the validity of the parallel trends assumption and to trace the evolution of the policy's effects over time, I estimate an event-study model at a yearly frequency, spanning 2019 to 2024 and using 2021 as the omitted reference year. The specification mirrors the baseline difference-in-differences framework presented in Equation (1) and Table 3, including ZIP-by-year and ZIP-by-100-year-floodplain fixed effects as well as controls for property characteristics such as the log of living area, number of bedrooms, and number of bathrooms. The resulting estimates, displayed in Figure 4, show no discernible pre-treatment differences between flood-zone and non-flood-zone listings: a joint F-test of the 2019 and 2020 interaction terms fails to reject the null hypothesis that pre-policy coefficients are equal to zero ($F(2, 732) = 0.65$, $p = 0.521$), providing empirical support for the parallel-trends assumption. Post-2022 coefficients remain close to zero and statistically insignificant, indicating no detectable change in rental prices following the disclosure law's implementation.

As a robustness check, I re-estimate the event-study specification on the repeat-listings subsample, which includes properties observed more than once, to account for potential unit-level unobserved heterogeneity. Figures B1 and B2 report these results, corresponding respectively to specifications (3) and (4) in Table 3. Both models, with and without property-level controls, yield patterns consistent with the baseline estimates: pre-treatment coefficients are jointly insignificant ($F(2, 579) = 0.76$, $p = 0.466$ and $F(2, 581) = 0.74$, $p = 0.477$), and post-treatment effects remain near zero. Together, these findings reinforce the validity of the identifying assumption and indicate that the observed null effects are not driven by sample composition or omitted property-specific factors.

II.III Demographic Sorting Analysis

To complement the property-level difference-in-differences analysis, I examine whether demographic composition evolved differentially across ZIP codes with higher versus lower exposure to flood risk following the disclosure law. The analysis draws on ZIP Code Tabulation Area (ZCTA)–level sociodemographic characteristics from the U.S. Census Bureau's American Community Survey (ACS) 5-year Data Profiles, specifically the 2017–2021 and 2019–2023 releases. These consecutive 5-year estimates provide statewide coverage at the ZCTA level and are the most reliable source for small-area analysis. While ACS 5-year data are available at even finer geographies such as block groups, I use ZCTAs to align with ZIP-code identifiers in the rental dataset and to limit sampling noise in very small areas.

Following standard practice in the applied urban and environmental economics literature (Bakkensen & Ma, 2020; Guerrieri, Hartley, & Hurst, 2021; Hino & Burke, 2021), I interpret differences between overlapping ACS 5-year estimates as capturing medium-run demographic adjustments rather than short-term shifts. Although the two ACS periods share three years of data (2019–2021), the estimates are independently reweighted using new survey responses and updated population controls, meaning that overlapping years do not mechanically cancel out. Instead, these rolling samples smooth local sampling error and yield robust measures of gradual neighborhood change, which is appropriate for evaluating the type of slow-moving sorting processes that follow policy interventions such as mandatory risk disclosure (Bakkensen & Barrage, 2021).

For each ZIP code and sociodemographic variable, I compute the change between periods as the difference between the 2019–2023 and 2017–2021 ACS 5-year estimates. The dependent variables encompass income, poverty, racial and ethnic composition, educational attainment, renter share, median age, and residential mobility indicators. All variables are drawn from ACS Data Profile tables and income variables are deflated to 2022 U.S. dollars using the Consumer Price Index.

The key independent variable is the pre-2022 flood-risk share, defined as the proportion of rental listings within each ZIP code that were located inside any flood-risk zone (FEMA 100-year, FEMA 500-year, or non-FEMA local hazard areas) prior to the implementation of the disclosure law. This variable captures baseline exposure to flood risk before information became salient to tenants. I estimate ordinary least squares (OLS) regressions of demographic change on this measure, controlling for baseline ZIP-level socioeconomic characteristics such as median household income, poverty rate, renter share, and educational attainment. All models include county fixed effects to absorb unobserved county-level heterogeneity, and standard errors are clustered at the county level. The regressions are weighted by 2017–2021 population so that larger ZIP codes receive proportionally greater influence in the estimation. This population-weighted, county–fixed-effects specification with baseline controls serves as the preferred specification throughout the analysis.

This approach isolates within-county differences in demographic change associated with pre-existing flood-risk exposure, while controlling for baseline composition. A positive and significant coefficient on the flood-risk share would indicate that higher-risk ZIP codes experienced larger increases in the given demographic outcome (for example, poverty or renter share) relative to low-risk areas. Conversely, statistically insignificant or near-zero coefficients would imply that population sorting by flood risk was limited in the years immediately following policy implementation.

III Results

III.I Rent Price Effects

Table 3 presents regression estimates from a series of difference-in-differences models that progressively vary the inclusion of fixed effects and controls to assess the robustness of the estimated impact of Texas's 2022 flood-risk disclosure law on final rental prices. Across all specifications, the coefficient on the post-policy interaction term, *FEMA100* $\times$ *Post*, remains small and statistically indistinguishable from zero, with confidence intervals that rule out economically meaningful effects.

All regressions are estimated using high-dimensional fixed effects with standard errors clustered at the ZIP-code level.

Column (1) reports the preferred specification. This model includes property characteristics, namely the number of bedrooms, number of bathrooms, and the log of living area, together with ZIP-by-year and ZIP-by-100-year-floodplain fixed effects, thereby absorbing both local demand shocks and time-invariant flood-zone differences within ZIP codes. The estimated treatment effect is small (0.0018, s.e. = 0.005) and indistinguishable from zero (p = 0.703), with a 95% confidence interval of [–0.008, 0.011]. This range rules out average rent changes larger than roughly ±1 percent, indicating that rental prices in flood-zone listings moved essentially in parallel with those outside the floodplain following the law's implementation.

Column (2) extends the preferred model by interacting the treatment term with each ZIP code's share of listings located within the 100-year floodplain. This interaction, *FEMA100 × Post × ZIP Floodplain Share*, tests whether the policy's effect varies with local flood-exposure intensity. The coefficient on the interaction is near zero (–0.0028, s.e. = 0.023; p = 0.904), and the main effect remains negligible, suggesting that even in ZIP codes with higher concentrations of flood-zone listings, rents did not respond detectably to the disclosure requirement.

Columns (3) and (4) restrict the sample to repeat listings, defined as rental units observed more than once, thereby identifying within-unit rent changes over time. This repeat-listing framework provides an even more granular test of the policy's effects by holding rental-unit–specific, time-invariant attributes constant. In these models, ZIP-by-100-year-floodplain fixed effects are replaced with rental-unit fixed effects to capture unobserved heterogeneity at the individual listing level. Column (3) maintains the structural controls, while Column (4) presents an unadjusted version without them. The estimated treatment effects remain small (–0.0011, s.e. = 0.0038) and (–0.0038, s.e. = 0.0040), respectively, both showing no statistically discernible effect, reinforcing the robustness of the null finding across different fixed-effects structures and estimation samples.

Together, these results indicate that the 2022 flood-risk disclosure law did not produce measurable changes in rental prices for properties located within the 100-year FEMA floodplain. The stability of the coefficients across progressively restrictive models, and especially the consistency of the preferred specification, provide reassurance that the null effect is not an artifact of omitted variable bias, while the narrow confidence intervals show that economically meaningful effects of even modest magnitude can be credibly ruled out. The absence of a detectable price response is itself economically and policy relevant: a precisely estimated null result suggests that the 2022 disclosure law did not generate large rent adjustments in either direction, pointing instead to alternative margins of adjustment such as gradual re-sorting of renters across flood-risk zones, explored in subsequent sections.

Table 4 reports estimates allowing the treatment effect to vary by year. Across all models, the point estimates for *FEMA100* $\times$ *Year* remain small, statistically insignificant, and tightly bounded, indicating no meaningful post-policy rent response. In the preferred specification (Column 1), effects for 2022–2024 are 0.0077 ($p = 0.167$), –0.0027 ($p = 0.654$), and –0.0016 ($p = 0.838$), ruling out rent changes exceeding roughly ± 2 percent in any year. Mirroring the structure of Table 3, Column 2 interacts the treatment with the ZIP-level floodplain share, while Columns 3 and 4 restrict the sample to repeat listings; throughout, the estimates remain near zero and statistically indistinguishable from zero. The year-specific results therefore corroborate the pooled findings, confirming the absence of a persistent or economically meaningful disclosure effect on rental prices.

Finally, Table 5 examines heterogeneous treatment effects across three mutually exclusive flood-risk categories: FEMA 100-year floodplains, FEMA 500-year floodplains, and non-FEMA locally mapped flood zones. Across all specifications, the estimated coefficients remain small and tightly bounded, providing no evidence of statistically discernible rent differentials emerging in any risk tier following the 2022 disclosure law. In the preferred specification (Column 1), the estimated effects are 0.006 (s.e. = 0.005; $p = 0.268$) for FEMA 100-year zones, –0.0068 (s.e. = 0.005; $p = 0.189$) for FEMA 500-year zones, and 0.019 (s.e. = 0.013; $p = 0.148$) for non-FEMA areas, none of which are statistically or economically meaningful. The repeat-sample results in Columns 2 and 3, are likewise centered near zero across all categories. Overall, the findings indicate that the disclosure law did not generate heterogeneous pricing responses by flood-risk intensity, reinforcing the precision and robustness of the null effects reported in the baseline models.

Taken together, the evidence indicates that Texas's 2022 flood-risk disclosure law did not systematically affect rental prices, whether in pooled estimates, year-specific effects, or across flood-risk categories. The lack of detectable price responses suggests that the policy's influence, if any, likely operated through non-price channels such as tenant awareness or demographic sorting, examined next.

III.II Demographic Sorting

Table 6 reports population-weighted ZIP-level regressions linking changes in sociodemographic characteristics between the 2017–2021 and 2019–2023 ACS 5-year estimates to the pre-2022 share of rental listings located in flood-risk areas. Each column corresponds to a different outcome—poverty rate, Hispanic population share, Black population share, share of adults with a bachelor's degree or higher, and median age—with all models controlling for baseline (2017–2021) ZIP-level covariates, county fixed effects, and county-clustered standard errors. Estimated coefficients are uniformly small and statistically indistinguishable from zero, indicating no systematic demographic re-sorting along flood-risk lines in the two years following the law's enactment. Point estimates are positive for poverty, Black share, and college attainment, and negative for Hispanic share and median age, but all confidence intervals span zero, ruling out economically meaningful shifts. Overall, ZIP codes with greater pre-existing flood exposure did not undergo disproportionate demographic change relative to less-exposed areas, suggesting that the muted response observed in rental prices extended to neighborhood composition as well.

Table 7 extends this analysis to housing composition and residential mobility. Regressions relate changes in renter share, real median household income, the share of residents who moved from another state, and the share remaining in the same house to pre-2022 flood-risk exposure, once again controlling for baseline characteristics and county fixed effects. Coefficients are modest and statistically insignificant, indicating no meaningful re-sorting or migration response following the law's implementation. ZIP codes with higher flood exposure did not experience unusual changes in renter concentration, income growth, or residential stability. Estimated effects for renter share and same-house residence are slightly positive, while those for income and out-of-state movers are mixed, but all confidence intervals encompass zero, ruling out economically important shifts.

Viewed together with the demographic results in Table 6, these patterns suggest that Texas's 2022 flood-risk disclosure law did not trigger short-run compositional or mobility responses at the ZIP-code level, reinforcing that the policy's lack of price effects was paralleled by muted neighborhood-level adjustment. To ensure these conclusions are not sensitive to modeling choices or measurement assumptions, the following section conducts a series of robustness checks that test the consistency of the estimated null effects.

III.III Robustness Checks

Rent Price Effects

To evaluate the sensitivity of the main estimates, I conduct a series of robustness checks on the preferred specification. Full results are reported in Appendix Tables A5 - A10.

First, I re-estimate the baseline difference-in-differences model using a broader treatment definition that classifies listings as treated if they fall within any flood-risk zone—combining FEMA 100-year, FEMA 500-year, and non-FEMA locally mapped areas. This specification tests whether the null result in Table 3 depends on the narrower FEMA100 definition used in the main analysis. As shown in Table A5, the estimated treatment effect remains small (0.0007, s.e. = 0.004) and statistically indistinguishable from zero. The coefficient's stability under this broader definition of flood exposure indicates that the absence of a price response is not sensitive to how flood exposure is defined, reinforcing the robustness of the null effect across conceptualizations of flood risk.

Second, I consider alternative outcome variables to verify that the null results are not specific to the use of final rental prices. In Table A6, I replace the dependent variable with *Days on Market* (DOM), which proxies for local demand and liquidity. The estimated treatment effect is small (0.81 days, s.e. = 1.34) and statistically indistinguishable from zero, indicating no discernible change in listing duration following the disclosure law. In Table A7, I instead use the log of the original asking rent as the dependent variable. The estimated coefficient (0.0053, s.e. = 0.006) is likewise close to zero and statistically insignificant. Together, these results suggest that the absence of price effects extends to related housing market margins, including initial pricing behavior and time to lease.

Third, I examine whether listing frequency influences the results by restricting the sample to non-repeated rental units, which are those that appear only once in the dataset. This test ensures that the main estimates are not driven by units observed multiple times, which could mechanically smooth rent variation over time. As shown in Table A8, the estimated treatment effect remains small (0.0072, s.e. = 0.006) with no statistically discernible effect. The similarity of the results across both repeated and unique listings indicates that the null finding is not an artifact of sample composition or listing frequency, reinforcing the stability of the estimated price effects under alternative sample definitions.

Fourth, I restrict the sample to single-family homes, which constitute the majority of listings in the dataset. This restriction tests whether the baseline results are driven by multi-family or apartment units, which may have distinct pricing dynamics. As shown in Table A9, the estimated treatment effect remains small (0.0023, s.e. = 0.005) with no statistically meaningful difference in post-policy rents. The persistence of null results within this dominant housing segment indicates that the absence of price effects is not specific to property type and holds among the most common rental structure in the market.

Finally, I re-estimate the main model at a broader level of spatial aggregation to assess whether the results depend on ZIP-specific variation. The dependent variable remains the log of final rent for individual listings, but the specification now includes county-by-year and county-by-100-year-floodplain fixed effects, identifying within-county-year variation in floodplain exposure over time.

To further isolate the policy's effect, the model incorporates property characteristics (number of bedrooms, number of bathrooms, and log living area) along with several contextual controls. Time-varying county-level shocks include quarterly changes in unemployment rates, constructed from the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (BLS, 2025), and quarterly COVID-19 incidence rates per 100,000 residents, derived from the Johns Hopkins University Center for Systems Science and Engineering's COVID-19 Data Repository (Johns Hopkins University CSSE, 2020). Both capture localized economic and public-health disruptions during 2019–2024. A ZIP-year measure of flood activity, calculated as the log of one plus National Flood Insurance Program (NFIP) claims per 100 rental listings, is also included to control for contemporaneous flood events. Population weights based on 2017–2021 ACS 5-year total population estimates are applied to prevent over representation of small ZIP codes.

Baseline ZIP-level socioeconomic characteristics from the 2017–2021 ACS 5-year estimates, such as median household income (inflation-adjusted to 2022 USD), renter share, the share of adults with a bachelor's degree or higher, and Hispanic population share, are added to account for pre-existing neighborhood differences that might co-vary with both flood risk and rental prices.

As shown in Table A10, the estimated coefficient on *FEMA100* $\times$ *Post-2022* remains small (0.0046, s.e. = 0.007) with no statistically discernible price response. Control coefficients behave as expected. Larger and higher-quality units, as well as ZIP codes with higher educational attainment or renter shares, are associated with higher rents, while increases in unemployment correspond to lower rents. These patterns confirm the model's internal consistency. The stability of results under this alternative identification strategy, which combines county-level fixed effects, population weighting, and extensive contextual controls, provides strong evidence that the null finding is not driven by omitted local shocks or baseline socioeconomic heterogeneity. This robustness check reinforces the credibility of the main estimates and confirms that the absence of measurable price effects persists even under broader spatial and econometric specifications.

Overall, these robustness checks demonstrate that the null effects documented in the main analysis are not sensitive to alternative exposure definitions, listing frequency, outcome measures, or property types. Across every variant, the estimated coefficients remain small in magnitude and lack statistical significance, reinforcing the conclusion that Texas's 2022 disclosure law did not produce detectable price effects in the rental market.

Demographic Sorting

To evaluate the robustness of the ZIP-level sorting results, I re-estimate the models under a series of alternative specifications. Full results are reported in Appendix Tables A11 - A13.

First, Table A11 presents bivariate regressions of ZIP-level changes in demographic and housing characteristics between the 2017–2021 and 2019–2023 ACS 5-year estimates on the pre-2022 flood-risk share. No controls or fixed effects are included, providing a simple descriptive benchmark that shows no meaningful associations between flood-risk exposure and subsequent demographic or mobility changes.

Second, Table A12 adds baseline (2017–2021) ZIP-level ACS covariates to control for pre-existing socioeconomic differences. For each outcome, the corresponding baseline level (e.g., pre-period renter share, education, income, or mobility composition) is included as a predictor, along with other demographic controls. Coefficients on pre-2022 flood-risk share remain modest in size and lack statistical significance, suggesting that controlling for baseline ZIP characteristics does not materially alter the results. The absence of significant relationships continues to indicate limited demographic or mobility sorting along flood-risk lines.

Finally, Table A13 introduces county fixed effects and clusters standard errors at the county level to address potential spatial dependence and unobserved county-level shocks. This specification identifies within-county contrasts in demographic and mobility change across ZIP codes with varying degrees of flood exposure. The estimated coefficients remain small, statistically indistinguishable from zero, and directionally consistent with the baseline results. The inclusion of county fixed effects therefore provides further reassurance that the null findings are not driven by omitted county-level heterogeneity or cross-sectional compositional bias.

Considered jointly, these robustness checks confirm that the null results documented in Tables 6 and 7 are stable across a range of empirical choices. Whether estimated bivariately, adjusted for baseline characteristics, or restricted to within-county variation, the data provide no evidence of systematic post-policy sorting or mobility responses by flood-risk exposure. The muted effects observed for rental prices are mirrored by a comparably muted response in neighborhood composition, reinforcing the conclusion that Texas's 2022 flood-risk disclosure law did not generate detectable short-run redistributive effects at the ZIP-code level.

IV Discussion and Policy Implications

The findings from this study indicate that Texas's 2022 flood-risk disclosure law did not generate measurable adjustments in rental prices or evidence of demographic sorting across flood-risk zones. Rent levels in flood-prone areas moved in parallel with those outside the floodplain, as shown by event-time estimates in Figure 4 that are flat before and near zero after the policy. ZIP-level demographic and mobility patterns also remained stable in the two years following the law's enactment. These results suggest that the policy's introduction neither repriced flood risk nor triggered noticeable re-sorting of residents by risk exposure, implying that limited salience and weak enforcement as likely explanations for the muted market response. These findings remain stable when county-level unemployment and COVID-19 incidence controls are introduced in robustness checks, indicating that macro-level shocks do not drive the null estimates.

The Texas disclosure form, reproduced in Appendix C, requires landlords to indicate whether a property is located within a 100-year floodplain and whether it has flooded within the past five years. It also advises tenants that they can determine a property's flood risk by visiting FEMA's website and searching by address. Importantly, the policy itself compels landlords to disclose if a unit is located within a flood zone, meaning that tenants should, in principle, already receive this information directly from their landlord rather than having to consult flood maps.

The form is typically presented at the end of the leasing process, often immediately before a lease is signed, so tenants may learn of a property's flood risk only after they have effectively made their housing decision. At that point, the information maybe arrives too late to meaningfully alter choices. Moreover, the form directs tenants to FEMA's difficult-to-navigate flood maps, as a means to cross-validate the landlord's disclosure, rather than linking to the state's more intuitive platform, *map.texasflood.org*. As a result, these design choices limit the law's ability to make flood risk salient at the point when it could most influence behavior, turning disclosure into a procedural requirement rather than a preventative tool.

Correspondence with the state and regional entities responsible for administering the statute revealed that no official compliance or enforcement mechanism currently exists. None of the agencies contacted, including the Texas Real Estate Commission and the Texas Floodplain Management Association, were aware of any systematic monitoring, reporting, or penalty procedures for non-compliance. Without clear oversight, the law functions less as a mandatory disclosure and more as a voluntary guideline, relying on landlords' initiative rather than state enforcement. This institutional gap undermines the credibility of the mandate and helps explain the null effects observed in rental prices. Tenants who incur flood-related losses due to nondisclosure are legally permitted to terminate their lease under Texas Property Code § 92.0135 (see full text in Appendix C), yet in the absence of compliance data, the practical reach of this remedy remains unknown.

These findings also need to be understood within the broader context of the post-pandemic rental market. The analysis period (2019–2024) coincides with one of the sharpest rent inflations in recent U.S. history, driven by pandemic-related supply constraints, remote work shifts, and construction delays. In this environment of rapid statewide rent growth, small risk-related price differences may have been masked by broader inflationary pressures. Even if disclosure influenced tenant perceptions or search behavior at the margin, those effects were likely overshadowed by general market tightening and post-COVID volatility. The law's limited influence therefore reflects not only weak implementation, but also a market context in which flood risk salience was unlikely to dominate more immediate economic constraints.

The absence of sorting responses further underscores the gap between policy intent and behavioral adjustment. While these results indicate limited sorting, the overlapping ACS 5-year windows may partially obscure shorter-term compositional adjustments that occur within the post-policy period. Rather than reshaping the spatial distribution of risk exposure, the disclosure law left existing residential patterns largely intact. This contrasts with prior evidence from other environmental information interventions, where higher-income or more mobile populations responded more strongly once risk information became salient (e.g., Currie, Voorheis, & Walker, 2020). In Texas, however, both rents and neighborhood composition remained stable, suggesting that limited awareness, enforcement, and usability blunted the potential for disclosure to affect housing choices.

From a policy perspective, these findings highlight that information mandates alone are insufficient without credible enforcement and accessible design. As Ben-Shahar and Schneider (2011) emphasize, disclosure laws often fail because recipients cannot interpret or act upon the information provided. In the Texas case, weak administrative follow-through effectively rendered the law self-enforcing, diminishing its intended deterrent and signaling effects. Technically, tenants who incur flood-related losses due to a landlord's nondisclosure are permitted to terminate their lease, but without data on compliance or enforcement, the extent to which this provision constrains landlord behavior remains unknown. Documenting compliance and non-compliance rates would help clarify adherence levels and the practical reach of the tenant-termination remedy. Policymakers seeking to improve transparency in housing markets should pair disclosure mandates with accountability mechanisms, such as random audits, certification requirements, or penalties for verified non-compliance, and ensure that tenants receive risk information through clear, interactive formats that link directly to authoritative, address-specific maps.

Finally, several limitations warrant caution in interpreting these results. The CoreLogic data primarily capture formal listings and may underrepresent informal rental arrangements, smaller landlords, or transient housing segments where compliance could differ. The dataset also skews toward urban areas and single-family homes, limiting generalizability to rural or multi-unit contexts. Additionally, while the analysis spans a multi-year window around the policy's implementation, longer-term responses, such as gradual re-sorting, property investment shifts, or cumulative adaptation, may emerge only with more time. Future research combining micro-level lease data, survey evidence on awareness, and enforcement tracking could help disentangle these dynamics.

In sum, the evidence suggests that Texas's 2022 flood-risk disclosure law did not meaningfully alter market outcomes, not because flood risk is irrelevant, but because the policy's informational channel lacked enforcement, clarity, and salience amid an extraordinary period of rental market volatility. The broader policy lesson extends beyond Texas: transparency mandates must be designed not only to inform but also to compel compliance and enable comprehension if they are to translate knowledge into meaningful behavioral or market change.

V Conclusion

Using a novel dataset linking transaction-level rental listings from CoreLogic's MLS platform to high-resolution flood hazard maps from the Texas Water Development Board, this paper examines how Texas's 2022 mandatory flood risk disclosure law affected the rental market. Leveraging a difference-in-differences design with rich property, socioeconomic, and local shock controls, the analysis isolates the causal impact of disclosure salience on rent prices and neighborhood composition.

Across all specifications, the results are consistent and precise: the law did not reprice flood risk into rents, nor did it alter neighborhood composition. Event-study estimates show parallel trends before the policy and no discernible post-policy divergence, while ZIP-level demographic and mobility patterns remained stable. The absence of market or compositional responses indicates that disclosure alone, without salience, timing, or enforcement, was insufficient to change behavior.

This paper makes two contributions. First, it extends the flood-risk capitalization literature (e.g., Bin & Polasky 2004; Hino & Burke 2021; Gourevitch et al. 2023) to the rental sector, a domain of growing relevance for climate adaptation yet often excluded from property-based analyses. Second, it contributes to the literature on information disclosure, sorting, and environmental justice (e.g., Been 1994; Pastor et al. 2001; Currie, Voorheis, & Walker 2020) by providing causal evidence that mandated flood-risk information did not alter either pricing or population composition. In doing so, it highlights the distinction between risk awareness and behavioral response.

Overall, these findings underscore that transparency alone does not guarantee behavioral change. The law's informational channel, limited in salience, timing, and enforcement, failed to shift either rents or residential patterns. Information here did not translate into "truth" in the market.

This study provides rare causal evidence from the rental sector, a segment increasingly central to climate adaptation yet often overlooked in property-based analyses. Linking micro-level rental listings to granular flood risk data reveals both the potential and the limits of disclosure as a climate adaptation tool. Future work should extend this analysis to examine longer-run effects, directly measure compliance, and explore informal rental markets where disclosure is least likely to occur, as well as extend the framework to other states. Although comparable rental data remain difficult to obtain, such efforts are essential for understanding not only whether disclosure shifts prices, but who ultimately bears the risk when it does not.

VI Tables

Table 1: Summary Statistics for Residential Rental Properties

Variable	Mean	Std Dev	Min	Max	Observations
Flood Risk	0.15	0.35	0	1	356,117
FEMA100	0.06	0.24	0	1	356,117
FEMA500	0.07	0.25	0	1	356,117
FEMA(100+500)	0.13	0.33	0	1	356,117
Non-FEMA Map	0.02	0.13	0	1	356,117
Listing Year	2021	1.63	2019	2024	355,940
Original Price ($)	2,247	1,018.39	488	10,850	352,427
Final Price ($)	2,131	817.06	651	7,053	259,221
Days on Market	36	42.72	0	385	355,031
Living Area (sq. ft.)	1,792	781.64	430	7,769	349,876
Bathrooms	2.3	0.90	0	7	355,797
Bedrooms	3.0	0.98	0	6	354,115
Year Built	1992	24.00	1900	2024	345,842

[a] Sample includes 356,117 rental listings across Texas between 2019 and 2024.
[b] All prices have been adjusted to 2022 U.S. dollars using the Consumer Price Index (CPI) for inflation.

Table 2: Summary Statistics for Categorical Variables

Variable	Unique Values	Observations
Property Type	10	355,918
	Single-Family	66.85 (%)
	Condominium	13.93 (%)
	Residential Income	8.95 (%)
	Multi-Family	5.43 (%)
	Townhouse	2.68 (%)
Urbanicity	3	356,098
	Urban	97.15 (%)
	Suburban	2.36 (%)
	Rural	0.49 (%)
County	165	356,117
	Harris	34.64 (%)
	Dallas	10.18 (%)
	Tarrant	9.08 (%)
	Collin	7.86 (%)
	Fort Bend	6.35 (%)

Table 3: Regression Estimates on Final Rental Price

	Log(FinalRent)			
	(1)	(2)	(3)	(4)
*FEMA100*post*	0.0018	0.0025	-0.0011	-0.0037
	(0.005)	(0.007)	(0.004)	(0.004)
*FEMA100*post*ZIP Floodplain Share*		-0.0028		
		(0.023)		
Log of Living Area Control (sq. ft.)	Yes	Yes	Yes	No
No. of Bedrooms Control	Yes	Yes	Yes	No
No. of Bathrooms Control	Yes	Yes	Yes	No
Fixed Effect: ZIP × Year	Yes	Yes	Yes	Yes
Fixed Effect: ZIP × FEMA100	Yes	Yes	No	No
Fixed Effect: Rental Unit	No	No	Yes	Yes
No. of Observations	254,376	254,376	82,402	83,837

Note: * p<0.05, ** p<0.01, *** p<0.001.

Table Notes: Dependent variable is the log of final listed rent. All models are estimated using high-dimensional fixed effects (HDFE) regressions with standard errors clustered at the ZIP-code level. Column (1) reports the baseline difference-in-differences specification including ZIP-by-year and ZIP-by-100-year-floodplain fixed effects, identifying within-ZIP-year variation in floodplain status over time. Column (2) extends the baseline by interacting the post-policy treatment effect with the ZIP-level share of listings located within the 100-year FEMA floodplain, allowing the post-disclosure effect to vary with local flood exposure intensity. Columns (3) and (4) restrict the estimation sample to repeat listings (rental units observed more than once) and replace ZIP-by-100-year-floodplain fixed effects with unit-level fixed effects, thereby identifying changes in rent within the same property over time. All specifications except Column (4) control for the number of bedrooms, number of bathrooms, and the log of living area. The key coefficient, *FEMA100* × *Post*, captures the post-2022 rent differential between listings located inside versus outside the 100-year FEMA floodplain. The interaction term, *FEMA100* × *Post* × *ZIP Floodplain Share*, measures how this effect varies with the local prevalence of 100-year floodplain listings. See text for full details on variable construction.

Table 4: Regression Estimates on Final Rental Price with Varying Treatment Year Effects

	Log(FinalRent)			
	(1)	(2)	(3)	(4)
*FEMA100*2022*	0.0077	0.0084	-0.0001	-0.0034
	(0.006)	(0.007)	(0.005)	(0.005)
*FEMA100*2023*	-0.0027	-0.0020	-0.0023	-0.0036
	(0.006)	(0.007)	(0.005)	(0.005)
*FEMA100*2024*	-0.0016	-0.0010	-0.0001	-0.0059
	(0.008)	(0.009)	(0.009)	(0.011)
*FEMA100*post*ZIP Floodplain Share*		-0.0028		
		(0.023)		
Log of Living Area Control (sq. ft.)	Yes	Yes	Yes	No
No. of Bedrooms Control	Yes	Yes	Yes	No
No. of Bathrooms Control	Yes	Yes	Yes	No
Fixed Effect: ZIP × Year	Yes	Yes	Yes	Yes
Fixed Effect: ZIP × FEMA100	Yes	Yes	No	No
Fixed Effect: Rental Unit	No	No	Yes	Yes
No. of Observations	254,376	254,376	82,402	83,837

Note: * p<0.05, ** p<0.01, *** p<0.001.

Table Notes: Dependent variable is the log of final listed rent. All models are estimated using high-dimensional fixed effects (HDFE) regressions with standard errors clustered at the ZIP-code level. This table extends the baseline specification in Table 1 by allowing the treatment effect to vary by year following the policy's implementation. The interaction terms *FEMA100 × 2022*, *FEMA100 × 2023*, and *FEMA100 × 2024* capture, respectively, the rent differentials for listings located within the 100-year FEMA floodplain in each post-disclosure year relative to 2021. Column (1) reports the baseline model including ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Column (2) augments this specification by interacting the post-policy treatment effects with the ZIP-level share of listings located within the 100-year FEMA floodplain, allowing year-specific effects to vary with local flood exposure intensity. Columns (3) and (4) restrict the estimation sample to repeat listings (rental units observed more than once) and replace ZIP-by-100-year-floodplain fixed effects with unit-level fixed effects, thereby identifying within-property rent changes over time. All specifications except Column (4) control for the number of bedrooms, number of bathrooms, and the log of living area. The coefficient on *FEMA100 × post × ZIP Floodplain Share* in Column (2) represents the interaction between the ZIP-level prevalence of 100-year floodplain listings and the average post-policy treatment effect across years. See text for further details on variable definitions and estimation.

Table 5: Heterogeneous Effects of Flood Risk Exposure on Final Rental Prices

	Log(FinalRent)		
	(1)	(2)	(3)
*FEMA100*post*	0.0060	-0.0004	-0.0033
	(0.005)	(0.004)	(0.004)
*FEMA500*post*	-0.0068	0.0038	0.0021
	(0.005)	(0.003)	(0.003)
*Non-FEMA Map*post*	0.0192	-0.0063	-0.0043
	(0.013)	(0.010)	(0.012)
Log of Living Area Control (sq. ft.)	Yes	Yes	No
No. of Bedrooms Control	Yes	Yes	No
No. of Bathrooms Control	Yes	Yes	No
Fixed Effect: ZIP × Year	Yes	Yes	Yes
Fixed Effect: ZIP × FloodRisk	Yes	No	No
Fixed Effect: Rental Unit	No	Yes	Yes
No. of Observations	254,388	82,402	83,837

Note: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.
Table Notes: Dependent variable is the log of final rent. All models are estimated using high-dimensional fixed effects (HDFE) regressions with standard errors clustered at the ZIP-code level. This table extends the baseline specification by allowing for heterogeneous treatment effects across multiple flood-risk categories. The coefficients on *FEMA100* × *Post*, *FEMA500* × *Post*, and *Non-FEMA Map* × *Post* capture, respectively, the post-2022 rent differentials for listings located inside the 100-year FEMA floodplain, the 500-year FEMA floodplain, and areas mapped as flood-prone outside both FEMA zones. Column (1) reports the baseline model including ZIP-by-year and ZIP-by-flood-risk fixed effects, identifying within-ZIP-year variation in flood-risk categories over time. Columns (2) and (3) restrict the estimation sample to repeat listings (rental units observed more than once) and replace ZIP-by-flood-risk fixed effects with unit-level fixed effects, thereby identifying rent changes within the same property over time. All specifications except Column (3) control for the number of bedrooms, number of bathrooms, and the log of living area. See text for details on variable definitions and estimation.

Table 6: ZIP-Level Regressions of Demographic Changes on Pre-2022 Flood-Risk Exposure

	Δ Between 2017–2021 and 2019–2023 ACS 5-Year Estimates				
	Δ Poverty Rate	Δ Hispanic Share	Δ Black Share	Δ BA+ Share	Δ Median Age
Pre-2022 Flood-Risk Share	0.6548	-0.0798	0.2276	0.3211	-0.0233
	(0.470)	(0.355)	(0.454)	(0.443)	(0.210)
Population-Weighted (2017–2021)	Yes	Yes	Yes	Yes	Yes
Baseline ZIP-Level ACS Controls (2017–2021)	Yes	Yes	Yes	Yes	Yes
Fixed Effects: County	Yes	Yes	Yes	Yes	Yes
No. of Observations	901	901	901	901	901

Note: * p<0.05, ** p<0.01, *** p<0.001.
Table Notes: Each column reports results from a separate population-weighted ZIP-level regression of demographic change between the 2017–2021 and 2019–2023 ACS 5-year estimates on the pre-2022 share of rental listings located within any flood-risk zone. Regressions include baseline (2017–2021) ZIP-level covariates, county fixed effects, and standard errors clustered at the county level. The independent variable, *Pre-2022 Flood-Risk Share*, measures the proportion of rental listings within each ZIP code that were located inside FEMA 100-year, FEMA 500-year, or non-FEMA-mapped flood-risk areas prior to the implementation of Texas's 2022 disclosure law. Dependent variables represent percentage-point changes in ZIP-level population characteristics over time.

Table 7: ZIP-Level Regressions of Mobility and Housing Changes on Pre-2022 Flood-Risk Exposure

	Δ Between 2017–2021 and 2019–2023 ACS 5-Year Estimates			
	Δ Renter Share	Δ Median HH Income	Δ Out-of-State Movers	Δ Same House
Pre-2022 Flood-Risk Share	0.0895	1515	-0.0190	0.0368
	(0.376)	(886.5)	(0.189)	(0.457)
Population-Weighted (2017–2021)	Yes	Yes	Yes	Yes
Baseline ZIP-Level ACS Controls (2017–2021)	Yes	Yes	Yes	Yes
Fixed Effects: County	Yes	Yes	Yes	Yes
No. of Observations	901	898	909	909

Note: * p<0.05, ** p<0.01, *** p<0.001.
Table Notes: Each column reports results from a separate population-weighted ZIP-level regression of mobility or housing composition changes between the 2017–2021 and 2019–2023 ACS 5-year estimates on the pre-2022 share of rental listings located within any flood-risk zone. Regressions follow the preferred specification, which includes baseline (2017–2021) ZIP-level covariates, county fixed effects, and standard errors clustered at the county level. Observations are weighted by total population from the 2017–2021 ACS 5-year estimates. The independent variable, *Pre-2022 Flood-Risk Share*, measures the proportion of rental listings within each ZIP code that were located inside FEMA 100-year, FEMA 500-year, or non-FEMA-mapped flood-risk areas prior to the implementation of Texas's 2022 disclosure law. Dependent variables represent percentage-point changes in ZIP-level outcomes related to mobility and housing between the two ACS periods: the renter share, real median household income, the share of residents who moved from a different state, and the share of residents living in the same house as one year ago.

VII Figures

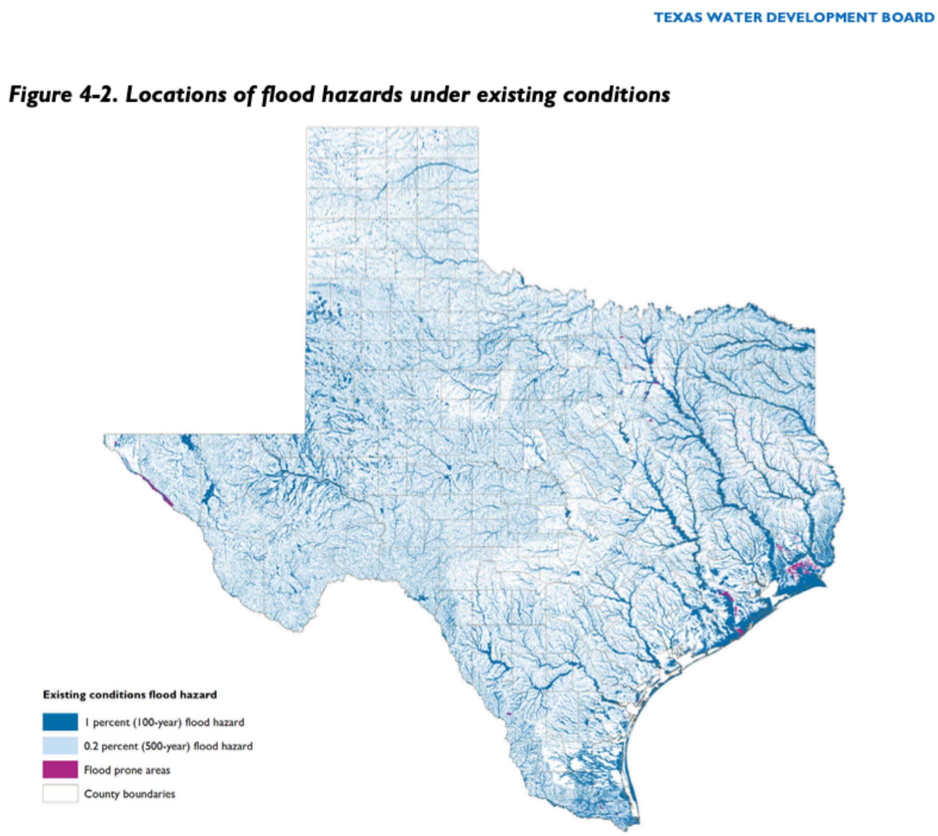


Figure 1: Texas Floodplains
Note: This figure is obtained from the Texas Water Development Board's *2024 State Flood Plan, Volume I*. Source: Texas Water Development Board (2024), Figure 4-2.

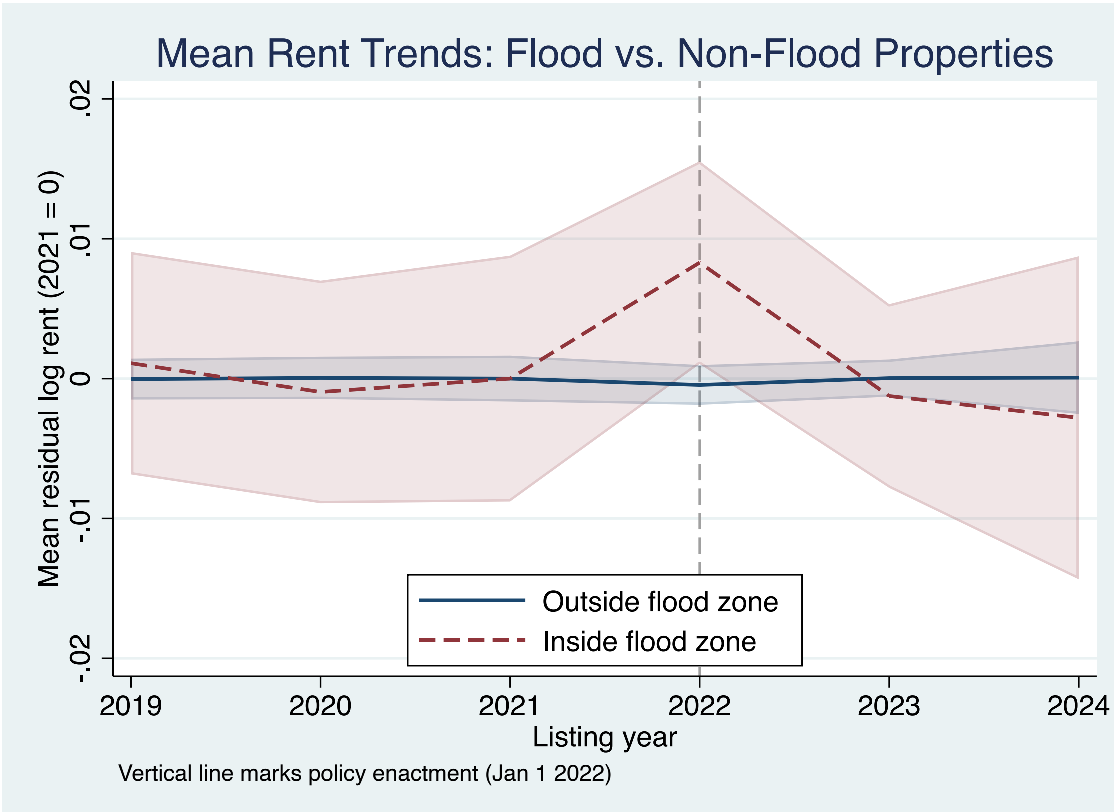


Figure 2: Mean Rent Trends for Flood- and Non-Flood-Zone Rental Properties, 2019–2024

Notes: This figure plots mean residual log rent prices for properties located inside and outside mapped flood zones using CoreLogic MLS rental listings from 2019–2024. Values are normalized to 2021 = 0, so each series shows deviations from the 2021 mean. The vertical line marks the implementation of Texas's mandatory flood-risk disclosure law on January 1, 2022. Mean rents in flood-zone listings show a brief uptick in 2022 that quickly dissipates. This pattern appears to coincide with a temporary shift in the composition of listings toward larger and higher-bedroom units, shown in Figure 3, rather than reflecting a genuine change in underlying rent levels. Figure 4 further confirms that the disclosure law did not generate a statistically significant or sustained change in rents.

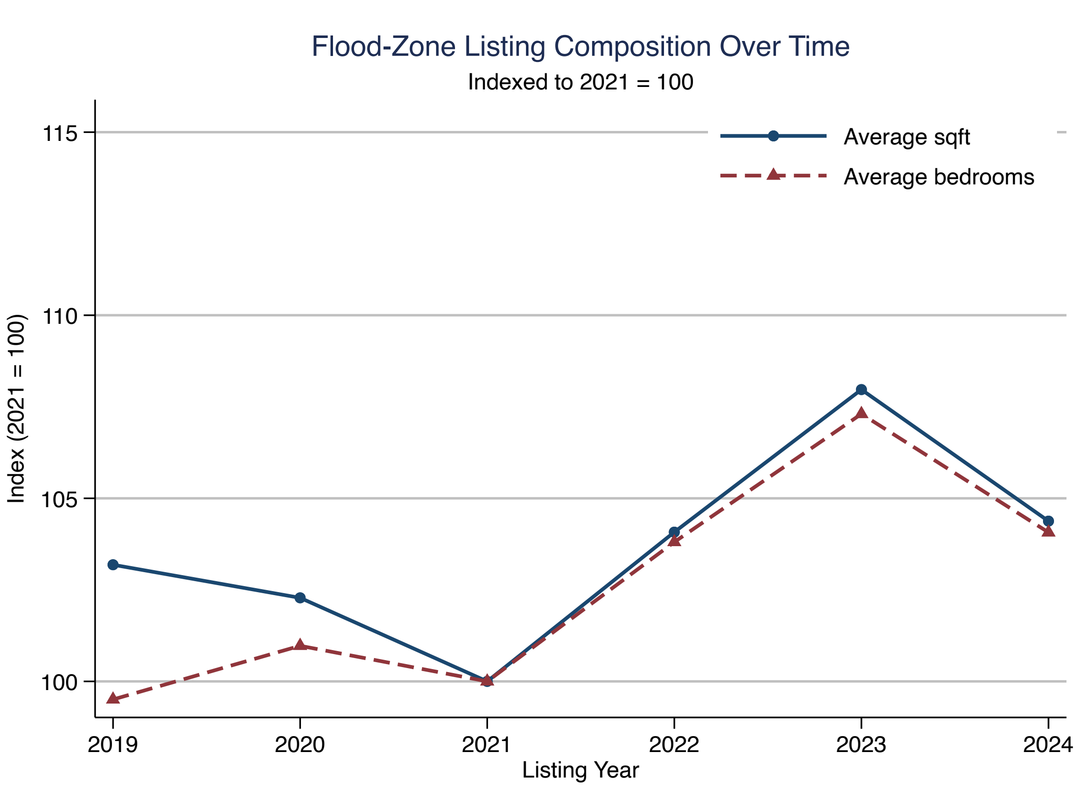
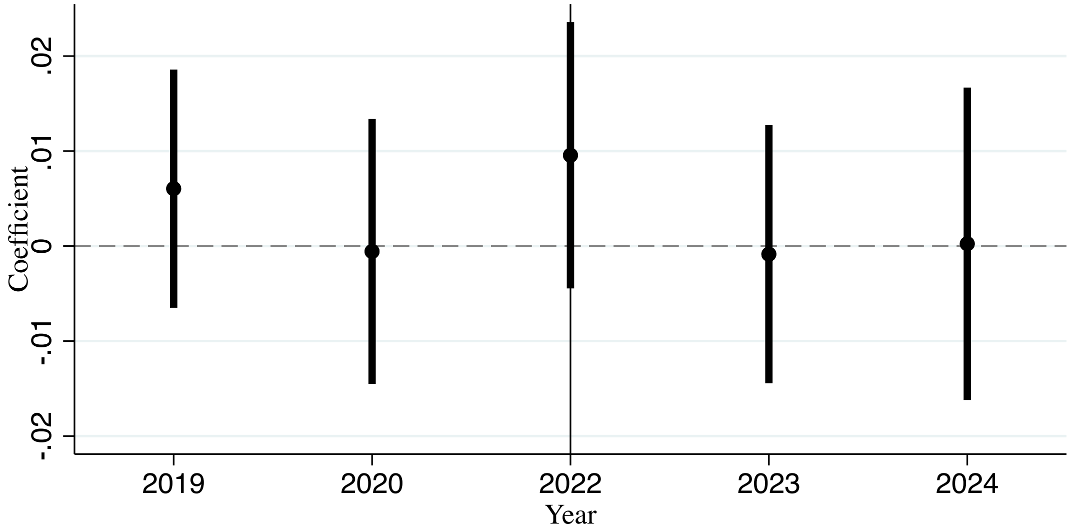


Figure 3: Flood-Zone Listing Composition Over Time, 2019–2024

Notes: This figure plots the average square footage and average number of bedrooms for properties located within mapped flood zones using CoreLogic MLS rental listings from 2019–2024. Each series is indexed to 2021 = 100, so a value of 110 corresponds to a 10 percent increase relative to the 2021 mean. Both measures rise briefly in 2022, indicating a temporary compositional shift toward larger and higher-bedroom listings. This pattern aligns with the transient rent uptick shown in Figure 2, suggesting that short-run changes in mean rents reflect listing composition rather than genuine price appreciation.

Figure 4. *Event-Study Results of Texas Rental Prices, 2019–2024*



Notes. Coefficient estimates and 95% confidence intervals from the event-study specification are shown. The dependent variable is log(final rent). Each coefficient represents the treatment effect for units located in the 100-year floodplain in the indicated year, relative to 2021, which is the baseline year. The specification includes ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Property-level characteristics include number of bedrooms, number of bathrooms, and log(living area). Standard errors are clustered at the ZIP-code level. The sample covers the years 2019–2024. Joint F-test of 2019–2020 coefficients fails to reject parallel pre-trends ($F(2,732) = 0.65$, $p = 0.521$).

Figure 4: Event-Study Estimates of Texas Rental Prices, 2019–2024

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Appendix A: Supplementary Tables

Supplementary Summary Statistics

Table A1: Summary Statistics Pre- and Post-2022

Variable	Pre-2022 (n = 176,900)		Post-2022 (n = 179,217)	
	Mean	Std Dev	Mean	Std Dev
Flood Risk	0.15	0.35	0.15	0.35
FEMA100	0.06	0.24	0.06	0.24
FEMA500	0.07	0.25	0.07	0.25
FEMA(100+500)	0.13	0.33	0.13	0.33
Non-FEMA Map	0.02	0.13	0.02	0.13
Listing Year	2020	0.81	2023	0.72
Original Price ($)	2,116	962.66	2,220	959.53
Final Price ($)	2,040	783.78	2,105	760.83
Days on Market	37	44.88	36	40.46
Living Area (sq. ft.)	1,782	786.18	1,801	777.00
Bathrooms	2.3	0.90	2.3	0.91
Bedrooms	3.0	0.97	3.0	0.98
Year Built	1990	23.36	1994	24.44

[a] All prices have been adjusted to 2022 U.S. dollars using the Consumer Price Index (CPI) for inflation.
[b] Not all variables have complete information.

Table A2: Supplementary Summary Statistics: ZIP-, County-, and ACS-Level Controls

Variable	Mean	Std. Dev.	Min	Max	Observations
Panel A: Time-varying ZIP- and County-Level Controls					
ln(NFIP Claims + 1) per 100 Listings (ZIP-Year)	0.63	0.94	0	10.86	356,117
Quarterly Δ in Unemployment Rate (pp)	0.05	1.84	-3.97	10.77	355,939
Quarterly COVID-19 Cases per 100k (winsorized)	1,534.95	1,749.90	0	10,189.11	261,649
Panel B: Baseline ZIP-Level ACS Controls (2017–2021)					
Median Household Income ($)	75,207	30,774.3	14,591	260,977	901
Poverty Rate (%)	10.81	7.81	0	62.4	908
Renter Share (%)	33.75	18.50	0	100	909
Bachelor's Degree or Higher (%)	28.14	17.13	0	87.3	909
Median Age	38.00	6.51	20.0	65.0	908
Total Population	25,221	21,323	23	129,165	909
Hispanic Population Share (%)	33.21	24.17	0	99.6	909
African American Population Share (%)	12.07	13.84	0	94.9	909
Same House 1-Year Ago (%)	85.86	7.15	31.4	100	909
Moved Within Same County 1-Year Ago (%)	7.63	4.71	0	42.4	909
Moved Out of State 1-Year Ago (%)	1.64	1.54	0	11.3	909

[a] All monetary values have been adjusted to 2022 U.S. dollars using the Consumer Price Index (CPI) for inflation.
[b] ACS variables come from the 2017–2021 American Community Survey 5-year estimates at the ZIP Code Tabulation Area (ZCTA) level.

Table A3: Rent Prices Pre- and Post-2022 for Residential Properties With and Without Flood Risk

	Pre-2022 (n = 176,900)			Post-2022 (n = 179,217)		
	No Flood Risk	**Flood Risk**	**Difference**	**No Flood Risk**	**Flood Risk**	**Difference**
Variable	Mean	Mean	(FR - No FR)	Mean	Mean	(FR - No FR)
Original Price ($)	2,124	2,075	-49	2,233	2,124	-109
Final Price ($)	2,046	1,993	-53	2,122	2,017	-105

All prices have been adjusted to 2022 U.S. dollars using the Consumer Price Index (CPI) for inflation.
Pre-2022: Listings before 2022; Post-2022: Listings from 2022 onward.
Difference column = (Flood Risk Mean - No Flood Risk Mean).

Table A4: Days on Market Pre- and Post-2022 for Residential Properties With and Without Flood Risk

	Pre-2022 (n = 176,900)			Post-2022 (n = 179,117)		
	No Flood Risk	**Flood Risk**	**Difference**	**No Flood Risk**	**Flood Risk**	**Difference**
Variable	Mean	Mean	(FR - No FR)	Mean	Mean	(FR - No FR)
Days on Market Derived	37	40	3	35	38	3

Pre-2022: Listings before 2022; Post-2022: Listings from 2022 onward.
Difference column = (Flood Risk Mean - No Flood Risk Mean).

Robustness Checks

Table A5: Regression Estimates Using Any Flood Risk Exposure as Treatment

	Log(FinalRent)
	(1)
*FloodRisk*post*	0.0007
	(0.004)
Log of Living Area Control (sq. ft.)	Yes
No. of Bedrooms Control	Yes
No. of Bathrooms Control	Yes
Fixed Effect: ZIP × Year	Yes
Fixed Effect: ZIP × FloodRisk	Yes
No. of Observations	254,388

Note: * p<0.5, ** p<0.01, *** p<0.001.
Table Notes: This table presents a robustness check using any type of flood risk exposure (FEMA100, FEMA500, Non-Fema Map) as the treatment variable. Dependent variable is the log of final rent. Regression is a high-dimensional fixed effects (HDFE) linear model estimated by `reghdfe`, with ZIP-by-year and ZIP-by-flood-risk fixed effects. Standard errors clustered at the ZIP level (735 clusters). Property characteristics (bedrooms, bathrooms, log living area) are included as controls. The key coefficient is the treatment indicator (FLoodRisk × post-2022), which is the only coefficient displayed for brevity. See text for full details on variable construction.

Table A6: Regression Estimates Using Days on Market as Outcome

	Days on Market
	(1)
*FEMA100*post*	0.8088
	(1.336)
Log of Living Area Control (sq. ft.)	Yes
No. of Bedrooms Control	Yes
No. of Bathrooms Control	Yes
Fixed Effect: ZIP × Year	Yes
Fixed Effect: ZIP × FEMA100	Yes
No. of Observations	346,920

Note: * p<0.5, ** p<0.01, *** p<0.001.
Table Notes: This table presents a robustness check using days on market as the outcome variable. Regression is a high-dimensional fixed effects (HDFE) linear model estimated by `reghdfe`, with ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Standard errors clustered at the ZIP level (830 clusters). Property characteristics (bedrooms, bathrooms, log living area) are included as controls. The key coefficient is the treatment indicator (FEMA100 × post-2022), which is the only coefficient displayed for brevity. See text for full details on variable construction.

Table A7: Regression Estimates Using Original Rental Price

	Log(OriginalRent)
	(1)
*FEMA100*post*	0.0053
	(0.006)
Log of Living Area Control (sq. ft.)	Yes
No. of Bedrooms Control	Yes
No. of Bathrooms Control	Yes
Fixed Effect: ZIP × Year	Yes
Fixed Effect: ZIP × FEMA100	Yes
No. of Observations	344,509

Note: * p<0.5, ** p<0.01, *** p<0.001.
Table Notes: This table presents a robustness check using the log of the original asking rental price as the dependent variable. Regression is a high-dimensional fixed effects (HDFE) linear model estimated by `reghdfe`, with ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Standard errors clustered at the ZIP level (808 clusters). Property characteristics (bedrooms, bathrooms, log living area) are included as controls. The key coefficient is the treatment indicator (FEMA100 × post-2022), which is the only coefficient displayed for brevity. See text for full details on variable construction.

Table A8: Regression Estimates Using Only Non-Repeated Listings

	Log(FinalRent)
	(1)
*FEMA100*post*	0.0072
	(0.006)
Log of Living Area Control (sq. ft.)	Yes
No. of Bedrooms Control	Yes
No. of Bathrooms Control	Yes
Fixed Effect: ZIP × Year	Yes
Fixed Effect: ZIP × FEMA100	Yes
No. of Observations	156,022

Note: * p<0.5, ** p<0.01, *** p<0.001.
Table Notes: This table presents a robustness check using the singletons subsample, which includes only rental listings that appear once in the dataset. Dependent variable is the log of final rent. Regression is a high-dimensional fixed effects (HDFE) linear model estimated by `reghdfe`, with ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Standard errors clustered at the ZIP level (700 clusters). Property characteristics (bedrooms, bathrooms, log living area) are included as controls. The key coefficient is the treatment indicator (FEMA100 × post-2022), which is the only coefficient displayed for brevity. See text for full details on variable construction.

Table A9: Regression Estimates Using Only Single-Family Homes

	Log(FinalRent)
	(1)
*FEMA100*post*	0.0023
	(0.005)
Log of Living Area Control (sq. ft.)	Yes
No. of Bedrooms Control	Yes
No. of Bathrooms Control	Yes
Fixed Effect: ZIP × Year	Yes
Fixed Effect: ZIP × FEMA100	Yes
No. of Observations	185,302

Note: * p<0.5, ** p<0.01, *** p<0.001.

Table Notes: This table presents a robustness check using only single-family homes. Dependent variable is the log of final rent. Regression is a high-dimensional fixed effects (HDFE) linear model estimated by `reghdfe`, with ZIP-by-year and ZIP-by-100-year-floodplain fixed effects. Standard errors clustered at the ZIP level (643 clusters). Property characteristics (bedrooms, bathrooms, log living area) are included as controls. The key coefficient is the treatment indicator (FEMA100 × post-2022), which is the only coefficient displayed for brevity. See text for full details on variable construction.

Table A10: Regression Estimates with County × Year and County × Flood-Risk Fixed Effects and Additional Zip-Level Controls

	Log(Final Rent)
	(1)
*FEMA100*post*	0.0046
	(0.007)
No. of Bedrooms	0.0605***
	(0.013)
No. of Bathrooms	0.0748***
	(0.006)
Log Living Area (sq. ft.)	0.3724***
	(0.039)
Log(NFIP Claims + 1) per 100 Listings	0.0079***
	(0.002)
Quarterly Δ Unemployment Rate	-0.0027***
	(0.000)
Quarterly COVID-19 Cases per 100k	0.0000***
	(0.000)
Median HH Income (ZIP-level, 2017–2021)	0.0000
	(0.000)
% with BA or Higher (ZIP-level, 2017–2021)	0.0062***
	(0.001)
% Renter (ZIP-level, 2017–2021)	0.0009*
	(0.000)
% Hispanic (ZIP-level, 2017–2021)	0.0024***
	(0.000)
R-squared	0.764
Fixed Effect: County × Year	Yes
Fixed Effect: County × FEMA100	Yes
Population Weights	Yes
No. of Observations	254,927

Note: * p<0.5, ** p<0.01, *** p<0.001.
Table Notes: This table presents a robustness check against the main ZIP × Year FE results. The dependent variable is the log of final rent for individual rental listings. The model is estimated via high-dimensional fixed effects (`reghdfe`) and includes county-by-year and county-by-100-year-floodplain fixed effects, identifying within-county-year variation in floodplain status over time. Weights correspond to 2017–2021 ACS 5-year total population estimates to prevent over representation of small ZIP codes. Controls include property characteristics, quarterly county-level shocks (unemployment and COVID-19 cases), and pre-2022 ZIP-level socioeconomic covariates (median household income, educational attainment, renter share, and Hispanic share). The coefficient on *FEMA100 × Post-2022* captures the average treatment effect of the Texas flood-risk disclosure law for listings located within the FEMA 100-year floodplain.

Table A11: Bivariate ZIP-Level Regressions of Demographic and Housing Changes on Pre-2022 Flood-Risk Exposure

	Pre-2022 Flood-Risk Share
Δ Poverty Rate	0.55
	(1.25)
Δ Hispanic Share	-0.34
	(0.49)
Δ Black Share	-0.10
	(0.37)
Δ Renter Share	0.90
	(0.61)
Δ BA+ Share	-0.73
	(0.47)
Δ Median Age	-0.20
	(0.57)
Δ Same House	-0.56
	(0.38)
Δ Same County	0.49
	(0.34)
Δ Median HH Income	-386
	(1010)
Δ Out-of-State Movers	0.03
	(0.11)
No. of Observations	898–909

Note: Each coefficient is from a separate bivariate regression of ZIP-level change in the listed outcome (2017–2021 → 2019–2023 ACS 5-year estimates) on the pre-2022 share of rental listings located within any flood-risk zone. Robust standard errors in parentheses. No controls or fixed effects are included.

Table A12: ZIP-Level Regressions with Baseline Controls: Changes on Pre-2022 Flood-Risk Exposure

	Pre-2022 Flood-Risk Share
Δ Poverty Rate	0.68
	(0.47)
Δ Hispanic Share	0.51
	(0.54)
Δ Black Share	-0.11
	(0.40)
Δ Renter Share	0.36
	(0.53)
Δ BA+ Share	-0.27
	(0.42)
Δ Median Age	0.41
	(0.36)
Δ Same House	-0.50
	(0.39)
Δ Same County	0.57
	(0.30)
Δ Median HH Income	-1,600
	(925)
Δ Out-of-State Movers	-0.09
	(0.12)
No. of Observations	898–909

Notes: Each row reports the coefficient on the pre-2022 flood-risk share from a separate ZIP-level OLS regression of changes between the 2017–2021 and 2019–2023 ACS 5-year estimates. Robust standard errors in parentheses. All specifications include baseline (2017–2021) ZIP-level ACS controls; the demographic outcomes include the corresponding pre-period shares (e.g., poverty, renter, bachelor's-or-higher, income, race/ethnicity as appropriate), and the mobility outcomes include the relevant pre-period mobility composition (e.g., same house, same county, out-of-state). No county fixed effects and no weights are used in this table. Dependent variables are percentage-point changes, except Median Household Income (real 2022 dollars).

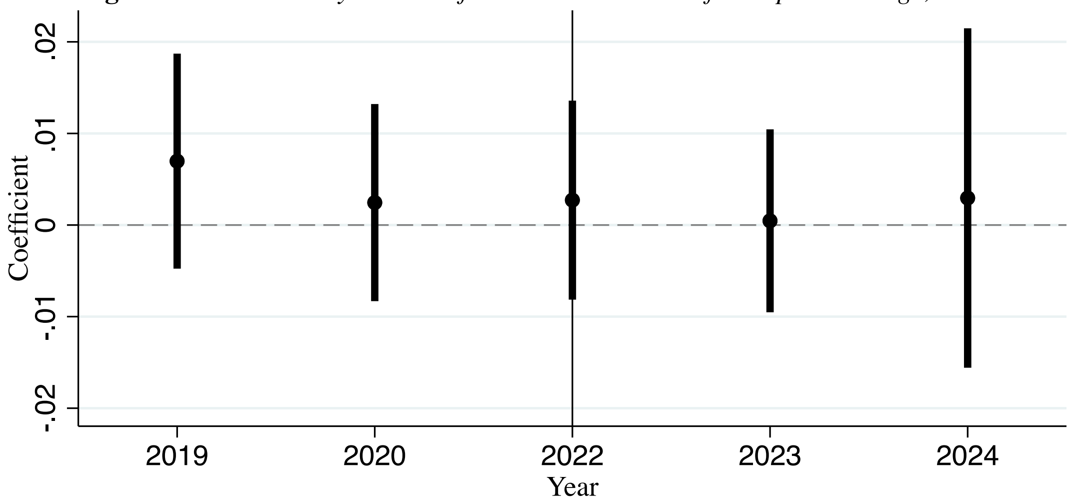
Table A13: ZIP-Level Regressions with Baseline Controls and County Fixed Effects: Changes on Pre-2022 Flood-Risk Exposure

	Pre-2022 Flood-Risk Share
Δ Poverty Rate	0.32
	(0.69)
Δ Hispanic Share	0.40
	(0.76)
Δ Black Share	-0.19
	(0.58)
Δ Renter Share	0.36
	(0.61)
Δ BA+ Share	-0.20
	(0.56)
Δ Median Age	0.57
	(0.52)
Δ Same House	-0.88
	(0.59)
Δ Same County	0.53
	(0.41)
Δ Median HH Income	81
	(1,531)
Δ Out-of-State Movers	-0.12
	(0.29)
No. of Observations	898–909

Notes: Each row reports the coefficient on the pre-2022 flood-risk share from a separate ZIP-level OLS regression of changes between the 2017–2021 and 2019–2023 ACS 5-year estimates. Cluster-robust standard errors (in parentheses) are adjusted for 131 county clusters. All specifications include baseline (2017–2021) ZIP-level ACS controls and county fixed effects. Demographic outcomes include the relevant pre-period shares (e.g., poverty, renter, education, race/ethnicity, income), while mobility outcomes include pre-period mobility composition (e.g., same-house, same-county, out-of-state). Dependent variables are percentage-point changes, except Median Household Income (real 2022 dollars).

Appendix B: Supplementary Figures

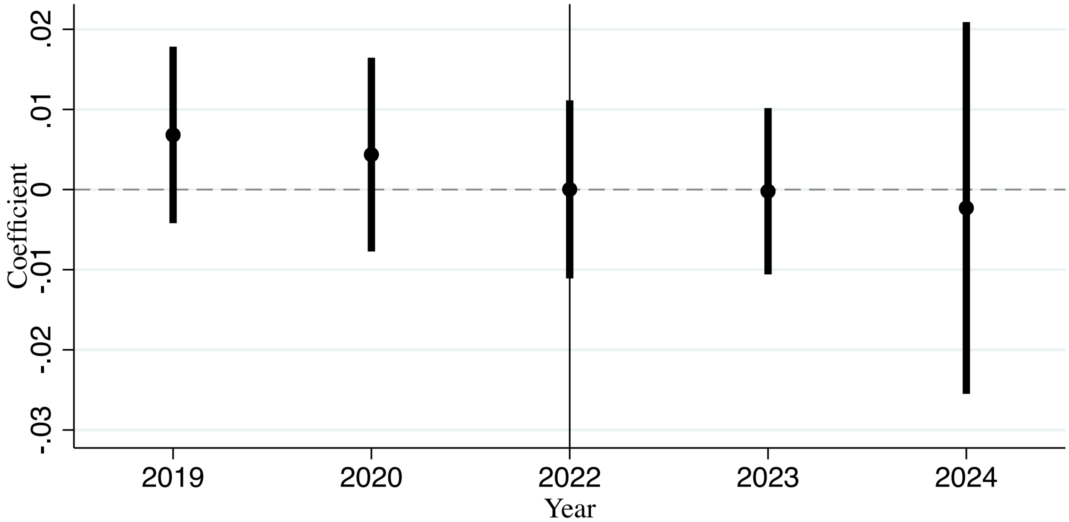
Figure B1. *Event-Study Results of Texas Rental Prices for Repeat Listings, 2019–2024*



Notes. Coefficient estimates and 95% confidence intervals from the event-study specification are shown. The dependent variable is log(final rent). Each coefficient represents the treatment effect for units located in the 100-year floodplain in the indicated year, relative to 2021, which is the baseline year.
The sample is restricted to repeat listings-properties observed in multiple periods-to control for unobserved unit-level heterogeneity, and it covers the years 2019-2024.
The specification includes ZIP-by-year and ZIP-by-100-year-floodplain fixed effects.
Property-level characteristics include number of bedrooms, number of bathrooms, and log(living area).
Joint F-test of 2019–2020 coefficients fails to reject parallel pre-trends ($F(2,579) = 0.76$, $p = 0.466$).
Standard errors are clustered at the ZIP-code level.

Figure B1: Event-Study Estimates of Texas Rental Prices for Repeat Listings, 2019–2024

Figure B2. *Event-Study Results of Texas Rental Prices for Repeat Listings Without Property Controls, 2019–2024*



Notes. Coefficient estimates and 95% confidence intervals from the event-study specification are shown. The dependent variable is log(final rent). Each coefficient represents the treatment effect for units located in the 100-year floodplain in the indicated year, relative to 2021, which is the baseline year.
The sample is restricted to repeat listings-properties observed in multiple periods-to control for unobserved unit-level heterogeneity, and it covers the years 2019-2024.
The specification includes ZIP-by-year and ZIP-by-100-year-floodplain fixed effects.
Joint F-test of 2019–2020 coefficients fails to reject parallel pre-trends ($F(2,581) = 0.74$, $p = 0.477$).
Standard errors are clustered at the ZIP-code level.

Figure B2: Event-Study Estimates of Texas Rental Prices for Repeat Listings Without Property Controls, 2019–2024

Appendix C: Texas Flood Risk Disclosure Law and Form

Texas Mandatory Flood Risk Disclosure Law

10/10/24, 12:11 PM 87(R) HB 531 - Enrolled version - Bill Text

H.B. No. 531

AN ACT

relating to notice requirements for a leased dwelling located in a floodplain.

BE IT ENACTED BY THE LEGISLATURE OF THE STATE OF TEXAS:

SECTION 1. Subchapter A, Chapter 92, Property Code, is amended by adding Section 92.0135 to read as follows:

Sec. 92.0135. NOTICE FOR DWELLING LOCATED IN FLOODPLAIN. (a) In this section:

(1) "100-year floodplain" means any area of land designated as a flood hazard area with a one percent or greater chance of flooding each year by the Federal Emergency Management Agency under the National Flood Insurance Act of 1968 (42 U.S.C. Section 4001 et seq.).

(2) "Flooding" means a general or temporary condition of partial or complete inundation of a dwelling caused by:

(A) the overflow of inland or tidal waters;

(B) the unusual and rapid accumulation of runoff or surface waters from any established water source such as a river, stream, or drainage ditch; or

(C) excessive rainfall.

(b) A landlord shall provide to a tenant a written notice substantially equivalent to the following:

"(Landlord) ( ) is or ( ) is not aware that the dwelling you are renting is located in a 100-year floodplain. If neither box is checked, you should assume the dwelling is in a 100-year floodplain. Even if the dwelling is not in a 100-year floodplain, the dwelling may still be susceptible to flooding. The Federal Emergency Management Agency (FEMA) maintains a flood map on its Internet website that is searchable by address, at no cost, to determine if a dwelling is located in a flood hazard area. Most tenant insurance policies do not cover damages or loss incurred in a flood. You should seek insurance coverage that would cover losses caused by a flood."

(c) Notwithstanding Subsection (b), a landlord is not required to disclose on the notice that the landlord is aware that a dwelling is located in a 100-year floodplain if the elevation of the dwelling is raised above the 100-year floodplain flood levels in accordance with federal regulations.

(d) If a landlord knows that flooding has damaged any portion of a dwelling at least once during the five-year period immediately preceding the effective date of the lease, the landlord shall provide a written notice to a tenant that is substantially

https://capitol.texas.gov/tlodocs/87R/billtext/html/HB00531F.htm 1/3

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equivalent to the following:

"(Landlord) ( ) is or ( ) is not aware that the dwelling you are renting has flooded at least once within the last five years."

(e) The notices required by Subsections (b) and (d) must be included in a separate written document given to the tenant at or before execution of the lease.

(f) If a landlord violates this section and a tenant suffers a substantial loss or damage to the tenant's personal property as a result of flooding, the tenant may terminate the lease by giving a written notice of termination to the landlord not later than the 30th day after the date the loss or damage occurred. For purposes of this subsection, a tenant suffers a substantial loss or damage to personal property if the total cost of repairs to or replacement of the personal property is 50 percent or more of the personal property's market value on the date the flooding occurred. Termination of a lease under this subsection is effective when the tenant surrenders possession of the dwelling.

(g) Not later than the 30th day after the effective date of the termination of a lease under Subsection (f), the landlord shall refund to the tenant all rent or other amounts paid in advance under the lease for any period after the effective date of the termination of the lease.

(h) This section does not affect a tenant's liability for delinquent, unpaid rent or other sums owed to the landlord before the date the lease was terminated by the tenant under this section.

SECTION 2. Section 92.0135, Property Code, as added by this Act, applies only to a lease agreement entered into or renewed on or after the effective date of this Act.

SECTION 3. This Act takes effect January 1, 2022.

______________________________ ______________________________

President of the Senate Speaker of the House

I certify that H.B. No. 531 was passed by the House on April 1, 2021, by the following vote: Yeas 119, Nays 26, 2 present, not voting; and that the House concurred in Senate amendments to H.B. No. 531 on May 24, 2021, by the following vote: Yeas 94, Nays 52, 1 present, not voting.

____

Chief Clerk of the House

I certify that H.B. No. 531 was passed by the Senate, with amendments, on May 19, 2021, by the following vote: Yeas 31, Nays 0.

____

Page 2 of Texas Flood Disclosure Bill

Secretary of the Senate

APPROVED: ____________________
Date

____
Governor

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Texas Flood Risk Disclosure Notice Form

FLOOD DISCLOSURE NOTICE

In accordance with Texas law, we are providing the following flood disclosure:

- We ❒ are or ❒ are not aware that the unit you are renting is located in a 100-year floodplain. If neither box is checked, you should assume the unit is in a 100-year floodplain. Even if the unit is not in a 100-year floodplain, the unit may still be susceptible to flooding. The Federal Emergency Management Agency (FEMA) maintains a flood map on its Internet website that is searchable by address, at no cost, to determine if a unit is located in a flood hazard area. Most renter's insurance policies do not cover damages or loss incurred in a flood. You should seek insurance coverage that would cover losses caused by a flood.

- We ❒ are or ❒ are not aware that the unit you are renting has flooded at least once within the last five years.

Signatures of All Residents

____

____

____

____

____

____

Signature of Owner or Owner's Representative

____

Date

____

Commentary. *The new requirement for flood disclosure is outlined in Sec. 92.0135 of the Texas Property Code, and a notice of this type is required with all new and renewed leases beginning January 1, 2022. This form will automatically batch print with all leases executed in TAA Click & Lease as of the effective date.*

Note that if the unit is in the 100-year floodplain but has been raised above it in accordance with federal regulations, you are not required to disclose it above.

Texas Apartment Association