Adapting to Natural Disasters through Better Information: Evidence from the Home Seller Disclosure Requirement

Seunghoon Lee*

2025-02-14

Abstract

Despite intensifying climate change, population exposure to flood risk in the US remains high, amplifying the economic toll of floods. This paper leverages two quasi-experimental variations of a Home Seller Disclosure Requirement to study whether providing flood risk information to homebuyers can reduce flood damage by lowering flood exposure. Using a hydrological measure of flood size, I first demonstrate that mandating flood risk disclosure decreases the probability of experiencing flood damage by 38 percent. Additionally, the policy reduces the population living in high-risk areas, highlighting that easing information frictions can promote voluntary adaptation to natural disasters.

Click Here for the Latest Version.

*Department of Economics, University of Missouri (615 Locust Street Columbia MO 65211, seunghoon.lee@missouri.edu).
I am deeply grateful to Ryan Kellogg, Koichiro Ito, and Dan Black for their guidance and mentorship. I also benefited tremendeously from numerous discussions with Cory Koedel and Peter Mueser. I thank H. Spencer Banzhaf, Lint Barrage, Zarek Brot-Goldberg, Fiona Burlig, Tatyana Deryugina, Eyal Frank, Justin Gallagher, Jonathan Gourley, Michael Greenstone, Solomon Hsiang, Amir Jina, Seojeong Lee, George Lefcoe, Christian Leuz, Philip Mulder, Brittany Street, Anant Sudarshan, Richard L. Sweeney, Katherine Wagner, Margaret Walls, Austin Wright, Derek Wu, Siqi Zheng, and seminar participants at the AERE Summer Conference, AREUEA National Conference, FHFA Fall Economic Summit, KDI School of Public Policy and Management, Midwest Energyfest, MIT, NBER Summer Institute, OSWEET (Online Summer Workshop in Environment, Energy, and Transportation), University of Chicago, and University of Missouri for their helpful comments. All errors are my own.

1 Introduction

Since 1980, floods in the United States have caused over $1 trillion in damage, and climate scientists predict they will become more frequent and severe in the future (Milly et al. 2002, NOAA 2020). Despite these warnings, many Americans continue to settle in flood-prone areas, amplifying the economic toll of floods (Weinkle et al. 2018, Redfin 2023, Titus 2023). Previous studies suggest that this seemingly counterintuitive behavior may stem from homebuyers' limited awareness of flood risks, which prevents them from fully internalizing the potential costs of their choices (Bakkensen and Barrage 2021, Wagner 2022).[1] Recently, flood risk information policies have gained attention as a strategy to reduce flood damage, but their effectiveness remains largely unknown.[2]

This paper leverages quasi-experimental variations from a Home Seller Disclosure Requirement (hereafter, "the disclosure requirement") to examine (1) whether improving access to flood risk information can reduce flood damage and (2) if so, whether this effect arises from a decline in population in high-risk areas. The policy mandates that home sellers disclose known property defects to buyers through a standardized form (Lefcoe 2004). Regarding flood risk, a typical question is if a property is located in a Special Flood Hazard Area (SFHA)—an area with elevated risk defined by the official flood map. Given that limited awareness may result from the costs of acquiring and processing information (Kunreuther and Pauly 2004), the disclosure requirement could alleviate the problem by efficiently informing homebuyers about flood risks.

The disclosure requirement was rolled out across 26 states in the contiguous US from 1992–2003. The variation in implementation timing is from plausibly exogenous state court rulings on the extent of realtor liability for incomplete disclosure, which facilitates a difference-in-differences research design (Roberts 2006). I also leverage additional variation stemming from the spatial discontinuity in flood risk information at SFHA borders, which allows me to identify the effect of information while holding true flood risk constant (Noonan et al. 2022). To further control for potentially confounding time-invariant differences at the SFHA border, I use the difference-in-discontinuity approach (Grembi et al. 2016).

[1] Although official flood maps have long been publicly available, a large body of evidence suggests a lack of flood risk awareness among homebuyers. For instance, Chivers and Flores (2002) find only 14 percent of homebuyers in high-risk areas learned about flood risk before closing.

[2] For instance, FEMA has proposed a reform to the National Flood Insurance Program (NFIP) that would make a community's eligibility contingent on mandatory flood risk disclosure (U.S. Department of Homeland Security 2022).

I collect multiple datasets to leverage these variations. For flood damage, I use damage records from flood insurance adjuster reports. To measure the physical size of flooding, I construct a novel dataset based on a hydrological measure of flood intensity, which objectively documents flood events from various meteorological causes (Saharia et al. 2017, England Jr et al. 2019). For population, I use block-level Decennial Census data and complement it with community-level annual Census data. Because the main outcome variables have a mass point at zero with a long right tail, I estimate the extensive and intensive margin effects separately in my regression models (Chen and Roth 2022).

The empirical exercise produces two key results. First, I show that the disclosure policy reduces flood damage. To do this, I first estimate a non-parametric flood damage function—a mapping between community-level flood size and the corresponding damage. Then, I estimate the causal effect of the disclosure requirement on the damage function using a stacked difference-in-differences approach (Cengiz et al. 2019, Brot-Goldberg et al. 2020). The results show that the policy significantly flattens the damage function, with the annualized probability of any flood damage at the community level decreasing by 2.7 percentage points, or 38 percent of the baseline. Moreover, the reduction in damage is three times larger in high-risk (i.e., high treatment intensity) communities.

In the subsequent section, I explore the mechanism by testing whether the disclosure policy reduces population in high-risk areas. By leveraging spatial discontinuities, I find that census blocks (with non-zero populations) in SFHA areas experience a 7 percent decline in population after the disclosure policy. At the extensive margin, the disclosure lowers the probability of a block in an SFHA having any population by 0.01, or 1.5 percent from the baseline. I further show that these effects are driven by diverted in-migration (and resulting suppressed development) rather than active out-migration from SFHA areas. These results survive a battery of robustness checks that account for potential treatment spillovers (e.g., doughnut regressions) or concurrent policy changes (e.g., models allowing for time-varying discontinuities at the border). Additionally, no population or damage reduction effects are observed in the "placebo" states, which had implemented a home seller disclosure requirement without a question on flood risk.

This paper contributes to four different bodies of literature. First, it is related to prior studies on factors that reduce damage from climate change. While earlier studies primarily focus on technology as a driver of adaptation (Miao and Popp 2014, Barreca et al. 2016, Burke and Emerick 2016), I focus on the role information can play in aligning private incentives with socially desirable outcomes. A recent paper by Fairweather et al. (2024), which experimentally demonstrates that Redfin users are more likely to make offers on safer properties when provided with flood risk information, is an important exception. I complement Fairweather et al. (2024) by studying responses to flood risk information across a broader population, beyond users of a single service provider. More importantly, I go further than investigating homebuyer responses alone to directly test whether, and to what extent, information provision can flatten the damage function (i.e., reduce flood damage)—an ultimate goal of climate adaptation policies (IPCC 2022).

Second, I add to the literature on the role of government in shaping household adaptation behaviors (Kousky et al. 2006, 2018, Gregory 2017, Peralta and Scott 2020, Baylis and Boomhower 2022). Perhaps the closest papers conceptually are Baylis and Boomhower (2021) and Ostriker and Russo (2023), which show how building code policies can reduce wildfire damage or flood risk exposure, respectively. A key difference is that the policies studied by these papers directly mandate adaptation, whereas disclosure policies encourage voluntary adaptation such as choosing safer places to live.

Third, and more broadly, I build on earlier work studying the impacts of flood risk information on housing market. These studies typically explore the impact of information shocks on housing prices to test whether housing market is efficient (Bin and Landry 2013, Hino and Burke 2021, Bakkensen and Barrage 2021) or to estimate households' willingness to pay to avoid flood risk (Pope 2008, Bosker et al. 2019).[3] This paper departs from these studies by examining how information provision affects flood damage, thereby offering a more direct assessment of its contribution to social welfare.

Finally, I contribute methodologically by constructing a novel measure of flood exposure (i.e., size), which is a critical step in identifying climate effects (Hsiang 2016). My approach leverages hydrological measures, which allow me to objectively document flood events for various causes including rainfall, snow melt, or storm surge. This extends the existing measures that are either endogenous or more focused in scope (Strobl 2011, Felbermayr and Gröschl 2014, Davenport et al. 2021).

The paper proceeds as follows. Section 2 provides background on the Home Seller Disclosure Requirement and the Special Flood Hazard Area. Section 3 details the data sources. Sections 4 and 5 show the disclosure policy effect on flood damage and population, respectively. Section 6 concludes.

[3] For example, Hino and Burke (2021) uses flood map updates as the main source of information shock and tests if the housing market efficiently prices flood risk. In contrast, I focus on net population flows following an information shock and the associated changes in flood damage.

2 Background

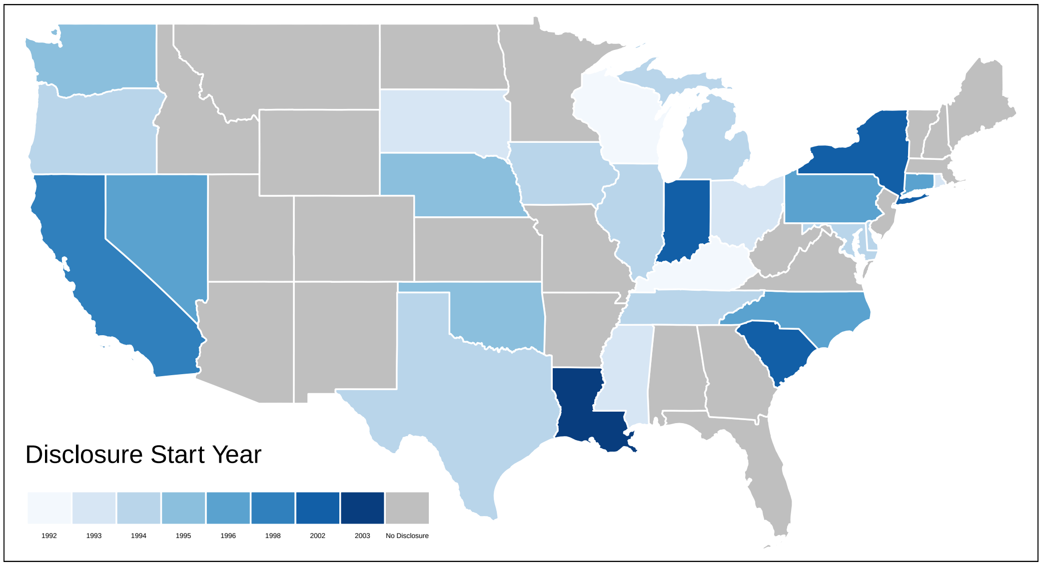


Figure 2.1: The Disclosure Requirement Implementation over Time

Background of policy adoption. Traditionally, homebuyers were expected to practice caution regarding property defects ("*caveat emptor*" or "let the buyer beware" doctrine). However, due to increasing consumer protectionism and public awareness of environmental and health concerns, state courts began holding listing agents accountable for incomplete disclosures (Weinberger 1996, Lefcoe 2004). In response, the National Association of Realtors issued a resolution in 1991 urging state associations to develop and support legislation regarding the statutory disclosure requirement in an effort to deflect potential liability to sellers (Tyszka 1995, Washburn 1995).

Consequently, as Figure 2.1 shows, between 1992 and 2003, 26 states (excluding DC) in the contiguous US adopted a disclosure requirement with an explicit question on flood risk while the remaining 22 states did not implement such a requirement until at least the late 2010s. Importantly, five of the 22 non-disclosure states adopted a home seller disclosure mandate without a question on flood risk. These "placebo" states are useful for checking the robustness of the main results.[4]

Disclosure content. A statutory disclosure requirement mandates that home sellers fill out a standardized form regarding known property conditions and typically deliver it before closing (Stern 2005). Importantly, the disclosure requirement is not exclusively about flood risk. As Appendix Fig-

[4]Appendix Table D.1 reports disclosure timing for disclosed and placebo states.

ure D.1 illustrates, a typical form covers a wide range of property conditions including structural issues (e.g., problems with walls) and surroundings (e.g., flood risks).[5] This implies that the policy adoption decision is likely to be uncorrelated with underlying flood characteristics or policies. Indeed, Appendix Figures D.2 show little correlation between the timing of disclosure requirements and (a) the size of flood damage, (b) the probability of having any flood damage, (c) ex-ante flood risk levels, (d) other local flood policies (Community Rating System participation), or (e) recent flood history.

The exact language of disclosure on flood risk varies slightly from state to state, but some combination of the following three questions usually appears: whether a property is in the SFHA; whether a property has flood damage history; and whether a property has flood insurance.[6] Because properties on the SFHA are more susceptible to flooding, answers to these questions are highly correlated: flood insurance policy and claims data show that 71% (75%) of the claims (flood insurance policies) are from properties in the SFHA. Thus, irrespective of the language, the disclosure requirement is likely to raise homebuyers' flood risk awareness for properties in SFHAs relative to those outside.

It is worth highlighting that while the disclosure policy helps determine whether a given property is at high flood risk, it provides no information on (1) flood zone boundaries or (2) the flood risk status of other properties on the market. As a result, it is likely difficult for homebuyers to engage in strategic avoidance behavior, such as selectively purchasing properties just outside flood zones, based solely on the information provided by the disclosure.

Flood Map and Special Flood Hazard Area (SFHA). Three facts about SFHA, an area designated by an official flood map for potential inundation by a 100-year flood, are worth highlighting. First, the SFHA boundary is determined by comparing water surface and ground elevations under a 100-year flood scenario (FEMA 2005). This gives rise to the spatial discontinuity design because the disclosure treats flood risk as binary, while actual flood risk varies more gradually across boundaries (Noonan et al. 2022). Second, as shown in Appendix Figure D.4, most communities have only a small fraction of area in SFHA, which suggests that potential treatment spillovers into non-SFHA

[5] Since the disclosure delivers a bundle of information, discerning treatment mechanism can be challenging if there is a positive correlation between flood risk and other property defects. In Appendix Table D.2, I demonstrate that properties in tracts with SFHAs are notably newer compared to those in tracts without SFHAs. As property defects typically emerge over time, this table suggests that the disclosure policy's impact stems from flood risk information rather than other property defects.

[6] As of 2021, 5 states ask just the first question about the SFHA status, 15 states ask about SFHA status and past flood experience, and 4 states ask all three questions. MI and TN ask about the latter two only.

areas are likely minimal (Busso et al. 2013, Alves et al. 2024). Third, SFHA areas change occasionally due to flood maps updates, albeit much less frequently than legally mandated (DHS Office of Inspector General 2017). Such a map update is a source of information shock (Gibson and Mullins 2020, Bakkensen and Ma 2020, Hino and Burke 2021, Weill 2023), which may confound the disclosure effect. In my empirical analysis, I test the robustness of my results against potential spillover effects and changes in flood maps.

3 Data

Flood damage. I use damage records from the flood insurance adjuster's report, which I acquired through Freedom of Information Act requests. The damage amount is defined by the actual cash value—a replacement value net of depreciation (FEMA 2014). I observe individual property level damage with loss date, community ID, and building type. I restrict the sample to damage records from single-family houses that has sustained the largest flood event for a given community-year. Then I collapse the data to the community-year level to merge it with the flood size data.

Flood size. I construct hydrology-based community-year-level flood size data using daily water volume records from over 3,000 USGS and NOAA stations (Gourley et al. 2013, Mallakpour and Villarini 2015, Slater and Villarini 2016). Under this approach, flood size is characterized by the recurrence interval, or the expected number of years for a flood of similar magnitude to reoccur. This measure can be heuristically understood as deviations from long-term, gauge-specific averages. Importantly, this objective and comprehensive gauge-based flood data is a major step forward in measuring flood exposure, which is a crucial for estimating credible climate damage functions (Hsiang 2016). Further details on background, procedure, summary statistics, and validation on the flood data are in Appendix A.

Population and vacancy rates. I use census-block-level population and vacancy rate data from Decennial Censuses.[7] To account for changing block boundaries and resulting one-to-many matches across different Decennial Census years, I calculate the weighted sum of count variables using inter-

[7] A property is considered vacant if no one is residing in the unit at the time of enumeration unless its occupants are only temporarily absent (US Census Bureau 2000).

Table 3.1: Summary Statistics for Key Variables

Variables	Min.	Q25	Median	Mean	Q75	Max.	N
Prob. of Having Any Damage	0	0	0	0.042	0	1	529,394
Flood Damage Per Community ($)	0	0	0	33,655	0	579,990,057	529,394
N of 2-Year Floods (For 20 Years)	0	9	11	11.2	13	20	7,821
Census Block Population	0	0	10	34.5	40	7,597	1,483,356

polation weights from the NHGIS block-to-block crosswalk (Manson et al. 2022).[8] I also complement the Decennial Census data with Census Place-level population data, the smallest unit with annual population information. In practice, I leverage data from Gallagher (2014), which links communities to Places for the purpose of obtaining annual community population estimates, and extend it to cover additional years as necessary.[9]

Other data sources. To determine flood risk profiles of census blocks or communities, I use the Q3 map—the first generation of a digitized flood map—that reflects flood risk as of the mid-1990s (FEMA 1996). Also, the primary data source to track the disclosure requirement legislative history is the *Nexisuni* database. I cross-validate this database with prior studies on the disclosure requirement (Washburn 1995, Pancak et al. 1996, Lefcoe 2004) and reports from the National Association of Realtors (National Association of Realtors 2019). For flood map update records, I leverage the Community Map History table from FEMA's Flood Insurance Study (FIS) reports.[10]

Descriptive statistics. Table 3.1 shows summary statistics for the main variables used in the empirical analysis. Flood damage and occurrences are for the NFIP communities in my sample and population figures are for the census blocks within the optimal bandwidth from Table 5.1 column (2). A notable aspect of the data is the high prevalence of zeros among the dependent variables. For instance, 95 percent of the observations of community-level flood damage per housing unit and 27

[8]For instance, block G06000104003003006 in 2000 is matched to five different blocks in 2010 ending in 3010, 3011, 3017, 3020, and 3028. More generally, 26% of blocks from 2000 and 41% from 1990 are matched to multiple blocks in 2010. Interpolation weights represent the expected proportion of the source block's counts (e.g., population or housing units) located in each target block (Manson et al. 2022).

[9]A community, as defined by the NFIP, is a local political entity (e.g., village, town, city) that is similar to, but not always aligned with, a US Census Place (Gallagher 2014).

[10]Practically, I extract the information from the National Flood Hazard Layer (NFHL). The Community Map History table serves as a credible source for tracking map revisions; for example, flood map panel notes, "For community map revision history, refer to the Community Map History table located in the FIS report for this jurisdiction." An alternative source is FEMA's Compendium of Map Updates (Hino and Burke 2021), which documents all changes, including both revisions and initial mappings, to the flood maps. However, I prefer the NFHL as my focus is on identifying revisions to the Q3 map. Further details and validation of the NFHL map revision data are in Appendix B.

percent of observations for the block population are zero. In addition, these variables also exhibit substantial skewness (long and thin right tails), as the difference between median and mean values suggests. To account for this, I follow Chen and Roth (2022) and estimate extensive and intensive margin effects separately in my regression models.[11] Also, in Appendix B, I show that the frequency of zeros in my data is consistent with external sources.

4 The Effect of the Disclosure Requirement on Flood Damage

4.1 Estimation Framework

Conditional on flood size, how does flood damage change after the disclosure requirement? To answer this question, I estimate a damage function, which is a mapping between flood size and damage, and show how the functional relationship changes due to the policy. As Hsiang (2016) notes, a crucial step in estimating any damage function is constructing an objective and continuous measure of exposure, and this paper leverages the hydrology-based flood history dataset described in Section 3.

$$\text{Damage} = \sum_k [\alpha_1^k F^k + \alpha_2^k F^k D] \tag{1}$$

Consider equation (1), where the dependent variable is flood damage for a community, D is an indicator variable for the disclosed group and F^k is an indicator variable equal to 1 when the annual maximum flood size is in bin k where $k \in \{$2-5, 5-10, 10-20, 20-30, 30-50$\}$.

Here, floods of size 1–2 serve as the omitted category, and thus α_1^k represents the additional damage when a community in a state without a disclosure policy in force experiences a flood of size k, as opposed to a flood of size 1–2. α_2^k allows a different damage function slope for the disclosed group for flood size k. Equation (1) follows a non-parametric approach of Barreca et al. (2016), which lets the data rather than the functional form assumption, determine the shape of the damage function.

I focus on flood sizes 1–50 for the main analysis because (1) larger floods are accompanied by interrelated perils, which cause substantial measurement errors (Kron et al. 2012), (2) chosen flood sizes cover a wide enough band to capture floods of different severity, from minor to major (Appendix Table A.2), and (3) the frequency of flood events reduces exponentially as flood size increases, which

[11] This approach resonates with a hurdle or two-part model, which is used extensively in modeling health expenditures that are characterized by a similar distribution (Mullahy and Norton 2022).

impose challenges on statistical power (Appendix Figure A.3 (b)). However, I also show that the results are robust to the expansion of flood sizes to 100.

The assumption behind binning is that the outcome is identical within each k. While flood sizes of 31 and 49, for example, are likely to have a different effect in reality, I choose bin sizes to strike a balance between flexibility and precision. Also, measuring flood exposure using F^k implies that smaller floods in the same community-year are ignored. However, this is unlikely to be a practical concern because as Appendix Figure A.3 (c)-(d) show, 65% (95%) of community-year had just one flood over size 2 (10).

$$\text{Damage} = \sum_k [\beta_1^k F^k + \beta_2^k F^k I + \beta_3^k F^k D + \beta_4^k F^k ID] \tag{2}$$

Equation (2), which mirrors a canonical difference-in-differences model, shows how equation (1) changes when the post disclosure indicator I is introduced. The coefficient for the interaction term (β_4^k) captures the disclosure effect.

$$Y_{mtd} = \sum_k [\beta_1^k F_{mtd}^k + \beta_2^k F_{mtd}^k I_{mtd} + \beta_3^k F_{mtd}^k D_{mtd} + \beta_4^k F_{mtd}^k I_{mtd} D_{mtd}] + \theta_{md} + \omega_{td} + \epsilon_{mtd} \tag{3}$$

For estimation, I take a stacked difference-in-differences approach as equation (3) to overcome potential bias from conventional two-way fixed effect models (Cengiz et al. 2019, Brot-Goldberg et al. 2020, Goodman-Bacon 2021). In equation (3), Y_{mtd} is either an indicator variable for positive flood damage (extensive margin), or log(Damage) conditional on positive damage (intensive margin), for community m, in year t for data stack d.[12] I include year × stack (ω_{td}) and community × stack (θ_{md}) fixed effects, to account for overall time trend and unobserved community characteristics.

Importantly, I use late adopting states as a control group in equation (3) because as Appendix Table D.3 shows, (1) the 22 never-adopted states are different in demographic, economic, and political characteristics from the 26 ever-adopted states but (2) such a difference does not appear in the early—14 states that have implemented the policy by 1994—vs. late—12 states implemented after 1994—adopting states comparison.[13] Thus, each stack d consists of communities in the treated

[12]While I report both the extensive and intensive margin effects, an emphasis is on the former due to greater generalizability—only a small fraction of communities experience repeated damage—and higher statistical power.

[13]Deshpande and Li (2019) also exploit the timing of treatment because eventual treatment status was predictable based on covariates. Roth and Sant'Anna (2023) also states that in non-experimental contexts, the quasi-randomness in identifying variation may be more plausible when never-treated units are excluded.

states, which adopted the disclosure policy in year t^*, and communities in the control states, which adopted the policy in $\tilde{t} > t^*$.[14] Each d covers a window of 10 years before and after t^*.[15] I drop observations from the control states for $t >= \tilde{t}$ because they are no longer "not-yet-treated".

Because the impact of natural disasters is not confined by administrative units, previous studies on cyclone damage function have used spatial-HAC standard errors (Hsiang 2010). Following this, I allow spatial correlation of up to 500 miles for inference (Newey and West 1987, Conley 1999), but I also show that state-level clustering produces similar results.[16]

4.2 Findings

In Figure 4.1, I plot the damage functions for the (a) control—not-yet-disclosed—and (b) treatment—disclosed—groups using the estimated coefficients from equation (3). For instance, $\hat{\beta}_1^k$ and $\hat{\beta}_1^k + \hat{\beta}_3^k$ for each k are used to plot the pre-treatment period damage functions for Panels (a) and (b), respectively. Because the dependent variable in Figure 4.1 is the probability of any damage, the estimated coefficients indicate the additional probability of damage when the baseline flood ($k = 1 - 2$) is replaced by a flood of size k. In Appendix Figure D.5, I reproduce Figure 4.1 with a 95% confidence interval.

Figure 4.1 allows visual inspection of the estimated damage function. To begin, I first focus on the slope, which reveals a monotonically increasing relationship between flood size and the probability of any flood damage. Further, high risk communities in Panels (c)–(d) (an above-median fraction of the area covered by an SFHA) have much higher vertical levels and steeper slopes in comparison to the low risk communities in Panels (e)–(f), which further validates the estimated function.[17]

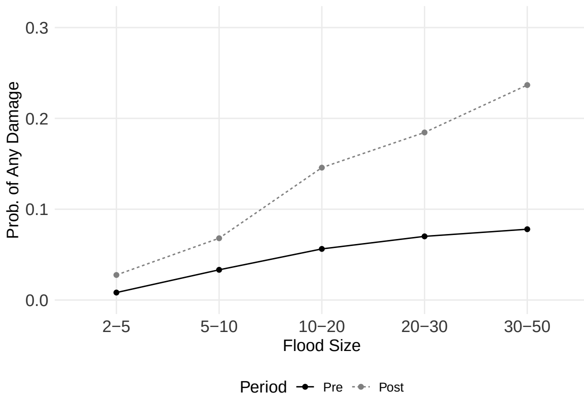
Table 4.1 highlights the impact of the disclosure requirement on flood damage. For brevity, I only report $\hat{\beta}_4^k$ from equation (3), but the full sets of coefficients are in Appendix Table D.4. In column

[14] Stack refers to data that is created for a specific treatment year (or a cohort year). A state can belong to both treatment or control groups depending on the stack. For instance, PA and CT, which changed their policy in 1996 are in the "treatment group" in a stack for $t^* = 1996$. The two states belong to the "control group" when $t^* < 1996$.

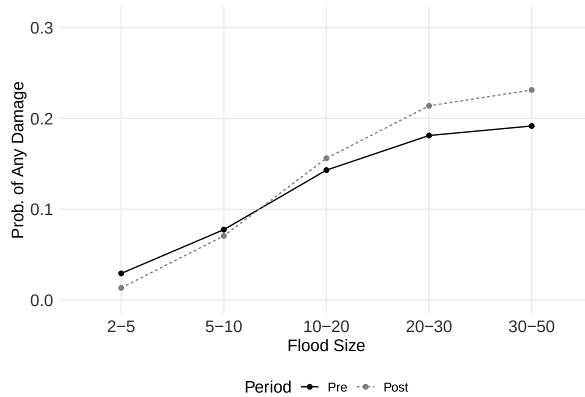
[15] IN, NY, and SC implemented the policy in 2002, leaving only one not-yet-treated state, LA, which adopted the policy in 2003. I show that my results are robust even when excluding the 2002 adoption cohort from the treated units—namely, utilizing IN, NY, and SC solely as control (i.e., not-yet-treated) units.

[16] Weights in this matrix are uniform up to that cutoff distance. When the variance-covariance matrix is not positive-semidefinite, I use eigendecomposition of the estimated variance matrix and convert any negative eigenvalue(s) to zero following Cameron et al. (2011).

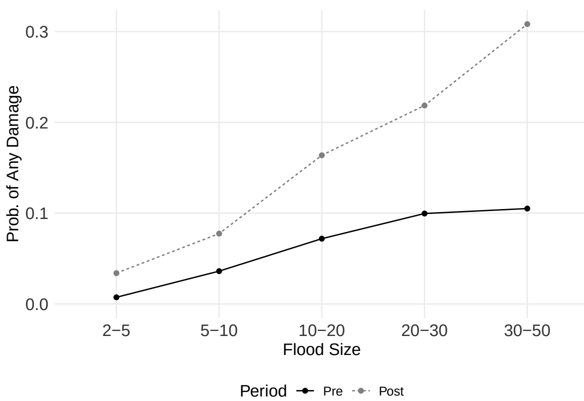
[17] To illustrate this point, consider two communities, A and B, with distinct risk profiles: A is entirely within the SFHA, while B lies outside it. During a 100-year flood, all properties in A (B) are expected to be underwater (unaffected) by the definition of SFHA, and thus damage should be significantly larger for community A.



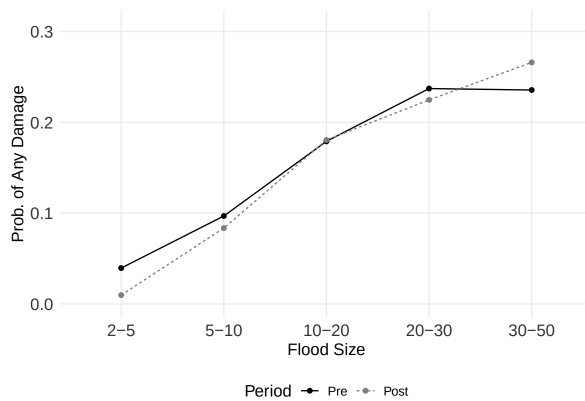
(a) Control Group (All Communities)



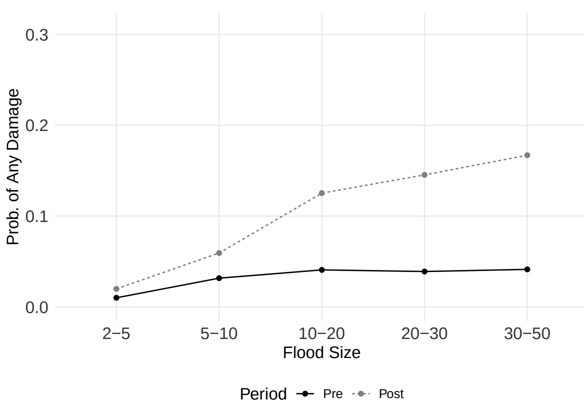
(b) Treatment Group (All Communities)



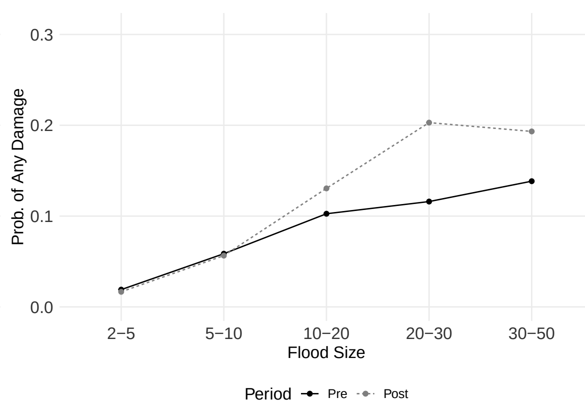
(c) Control Group (High Risk Communities)



(d) Treatment Group (High Risk Communities)



(e) Control Group (Low Risk Communities)



(f) Treatment Group (Low Risk Communities)

Figure 4.1: The Effect of Disclosure on the Damage Function. These plots illustrate estimated damage functions (dep.var: probability of any damage) from equation (3). Panels (a)–(b) are damage functions for all communities. Panels (c)–(d) and (e)–(f) illustrate the damage functions for high (above–median SFHA ratio) and low (below–median SFHA ratio) flood risk communities, respectively. Appendix Figure D.5 reproduces Figure 4.1 with 95% confidence intervals.

Table 4.1: The Effect of Disclosure on Flood Damage

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Post × Disclosure (Size 2-5)	$-.035^{*}$	$-.056^{**}$	$-.012$	$-.134$
	(.021)	(.028)	(.011)	(.300)
Post × Disclosure (Size 5-10)	$-.042^{*}$	$-.055^{*}$	$-.030$	.164
	(.022)	(.030)	(.018)	(.243)
Post × Disclosure (Size 10-20)	$-.076^{**}$	$-.090^{*}$	$-.057^{**}$	.051
	(.037)	(.052)	(.023)	(.210)
Post × Disclosure (Size 20-30)	$-.082^{*}$	$-.131^{***}$	$-.019$	.467
	(.049)	(.043)	(.070)	(.554)
Post × Disclosure (Size 30-50)	$-.119^{*}$	$-.173^{**}$	$-.071$	$-.334^{**}$
	(.063)	(.078)	(.055)	(.133)
Annual Effect	-0.027**	-0.039**	-0.014	-0.003
	(0.013)	(0.017)	(0.01)	(0.109)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	529394	254989	274405	22319

Note: The dependent variable in columns (1)–(3) is the probability of any flood damage. Column (1) uses the full sample of communities, while columns (2) and (3) repeat the analysis for subsamples of communities with different levels of flood risk exposure. The dependent variable in column (4) is log-transformed flood damage. Spatial-HAC standard errors, allowing for spatial correlation up to 500 miles, are used for inference. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

(1), I report that the disclosure requirement reduces the probability of having any flood damage by 4–12 percentage points for different values of k for the communities in the disclosed states relative to the ones in the not-yet-disclosed states. The damage reduction effect can be verified visually as well: Figure 4.1 shows that in Panel (a) (not-yet-disclosed), flood probability has substantially increased over time, whereas in Panel (b) (disclosed), it remains nearly identical.

Using equation (4), I summarize the coefficients in Table 4.1 into probability-weighted average treatment effects. Note, because $Pr(K = k)$ is the annual likelihood of occurrence for flood size k and β_4^k is the change in probability of having damage from flood size k, equation (4) can be interpreted as the reduction in annualized damage probability due to the disclosure policy.[18] For inference, I use the delta method.

$$\sum_k Pr(K = k) \times \beta_4^k \tag{4}$$

In Table 4.1 column (1), I report that the reduction in the annualized damage probability is 2.7 percentage points. When I compare this with the baseline of 7.1—average probability of having

[18] Since the flood size is defined by recurrence interval, the inverse of the size corresponds to $Pr(K = k)$. For instance, the probability of having a flood of size 30–50 in a given year is $\frac{1}{40}$.

any damage conditional on exposure to a flood of size 2 or larger—the effect size is a 38 percent reduction.[19] In columns (2) and (3), I show that the reduction in annualized damage probability is nearly three times larger for high-SFHA communities than for low-SFHA communities, which is consistent with the fact that the treatment intensity is higher for high-SFHA communities.

Column (4) reports the intensive margin effect, where the dependent variable is the log of damage. Because the sample for this exercise is restricted to community-years with positive damage, the model is underpowered. Still, I find evidence that the disclosure policy reduces damage for communities with repetitive flood events for the costliest flood category ($k = 30 - 50$).

Figures 4.1 provide initial insights into the mechanism behind the damage reduction effect. Panel (c) shows that, in the absence of a disclosure policy, flood damage has significantly increased over time in high-risk communities. In contrast, Panel (d) reveals that high-risk communities in disclosed states did not experience such an increase, presumably because the policy helped limit the rise in flood risk exposure. In the next section, I directly test this conjecture by estimating the impact of the disclosure policy on population distribution. Notably, the damage reduction effect of the policy is much smaller for low-risk communities in Panels (e) and (f), where disclosure intensity is lower.

Robustness checks. In Appendix Table D.5, I estimate equation (3) using placebo states and find that damage from larger ($k = 30 - 50$) floods increases after the disclosure policy in these states.[20] This aligns with Figures 4.1 (a), which show rising flood damage over time in the absence of flood risk information from the disclosure policy. In Appendix Figure D.6, I present an event study plot corresponding to column (1) of Table 4.1. It shows no pre-trend and persistent reduction in the probability of damage for larger floods after the disclosure policy. Finally, Appendix Tables D.6, D.7, D.8, D.9 demonstrate that the results in Table 4.1 remain robust to excluding communities with map revisions, clustering standard errors at the state level, expanding the flood size threshold from 50 to 100, and excluding the 2002 adoption cohort from the treated group, particularly for the annualized effect estimates.

[19] The baseline probability might seem low, but for larger floods, the probability of damage is substantially higher. For instance, for a flood of size between 20 and 50, the baseline probability is 21 percent. Additionally, it should be noted that the damage data comes from the flood insurance adjuster's report, so for areas with little to no flood insurance policies, damage probabilities are low by construction. For reference, as of 1995, about 1/3 of communities had no flood insurance policies.

[20] For statistical power, in Appendix Figure D.6 and Appendix Table D.5, I estimate a more parsimonious version of equation (3) by grouping flood events into baseline ($k = 1 - 2$), small ($k = 2 - 30$) and large ($k = 30 - 50$).

5 The Effect of the Disclosure Requirement on Population

5.1 Estimation Framework

To assess whether the damage reduction effect in Section 4 arises from lower flood risk exposure due to the disclosure—specifically, relative population declines in risk-prone areas—I begin by analyzing annual community-level population data using the triple difference design in equation (5).

$$log(Y_{mstd}) = \alpha_0 H_m I_{std} + \alpha_1 H_m D_{std} I_{std} + \omega_{std} + \psi_{md} + \epsilon_{mstd} \tag{5}$$

Y_{mstd} denotes population for community m in state s in year t in stack d. H_m is an indicator variable for high-SFHA communities, equal to one if m has an above-median fraction of the area in an SFHA. I_{std} is a post disclosure indicator and D_{std} is a disclosure group indicator. Other terms in a standard triple difference model are subsumed by fixed effects. α_1 captures the disclosure policy's impact on population in high-risk communities relative to low-risk communities in the post-period.[21]

Since equation (5) exploits the staggered, year-by-year adoption timing across states, it can adequately control for the impact of potentially concurrent flood policy changes. However, because communities do not perfectly align with Census Places (Gallagher 2014), and not all Census Places appear on the Q3 flood map, half of the communities in the sample are excluded from the estimation. Moreover, since location responses to flood risk are typically local (Elliott and Wang 2023), α_1—which cannot capture intra-community relocation—may significantly underestimate the disclosure policy's effect on reducing risk exposure.

Thus, I complement equation (5) by analyzing census block-level population data, leveraging a spatial discontinuity in flood risk information from the disclosure at the SFHA border.[22] Importantly, because other policies such as flood insurance requirements also change at the border, I employ a difference-in-discontinuity approach following Grembi et al. (2016) and Gottlieb et al. (2022), which

[21] For details on creating data stack, see Section 4.1.

[22] While Noonan et al. (2022) shows that (true) flood risk changes continuously at the border in Texas, true risk may change more abruptly in other states within my sample. However, because flood risk, which is largely a function of land contour, is likely to remain stable at least in a relatively short term, such differences will also be controlled by the difference-in-discontinuity approach.

controls for time-invariant cross-sectional differences between non-SFHA and SFHA areas.[23]

$$\begin{aligned} Y_{bst} =& \delta_0 + \delta_1 X_{bs} + \delta_2 D_{bs} + \delta_3 X_{bs} * D_{bs} + \\ & T_{st}[\delta_4 + \delta_5 X_{bs} + \delta_6 D_{bs} + \delta_7 X_{bs} * D_{bs}] + \epsilon_{bst} \end{aligned} \tag{6}$$

In equation (6), Y_{bst} represents population outcomes in block b in state s in time t. X_{bs} is the distance between a block border and the closest SFHA border in meters (negative if in a non-SFHA area), which is approximated by taking the difference of (1) the distance between block centroids and the closest SFHA border and (2) a block diameter. $D_{bs} = 1$ if $X_{bs} > 0$ is a high risk group (i.e., SFHA) indicator variable, and $T_{st} = 1$ if $t > T_s^*$ is a post disclosure-period indicator variable, where T_s^* is the policy change date for s. δ_6 captures the impact of the disclosure policy for blocks located in close proximity to the border.

To estimate δ_6, I first estimate the optimal bandwidth for each outcome variable. Then, I estimate equation (6) using blocks within the optimal bandwidth (Calonico et al. 2014, Cattaneo et al. 2019).[24] For states that implemented disclosure policies between 1990-1999 (2000-2009), the 1990 (2000) decennial census is used to estimate δ_0 to δ_3 whereas 2000 and 2010 (2010 and 2020) decennial censuses are used to estimate δ_4 to δ_7. Throughout Section 5, standard errors are clustered at state—the level of disclosure treatment. Also, I remove 17% of blocks that contain SFHA borders from the analysis because X_{bs} may not be well defined for them.

Notably, as Grembi et al. (2016) points out, equation (6) differs from studies that apply a difference-in-differences approach within a regression discontinuity (RD) style sample to improve comparability between treated and control units (Greenstone and Gallagher 2008, Lemieux and Milligan 2008, Pettersson-Lidbom 2012), as their identification still relies on the parallel trends assumption.[25] In contrast, in equation (6), identification rests on the assumption that there are no other important time-varying differences at the discontinuity.[26]

[23]There might be a concern for time-varying policy changes at the border as well. I test robustness in Section 5.2.

[24]I estimate the mean squared error optimal bandwidth for 2000 and 2010 and take the average of them following Grembi et al. (2016). I ignore 1990 and 2020 because these years have only a subset of the states in the sample.

[25]Analyzing the decennial data using a version of equation (5) has at least two critical challenges. First, there is only a single post-treatment period because, by 2010, there are no "not-yet-treated" states remaining. Second, there is only one pre-treatment period unless I forgo about 80% of observations due to the 1980 Census covering only urban or metropolitan areas. Note that these issues do not arise in my analyses using annual data.

[26]Thus, the discussion in Section 4.1 on control group selection—between never-treated and not-yet-treated units—is irrelevant for equation (6) as identification relies on the cross sectional differences between SFHA and non-SFHA areas, before and after the disclosure policy, *within* ever-disclosed states. This contrasts with equation (3), which compares

5.2 Findings

Table 5.1: The Effect of Discosure on Net Population Flow

	Log Population		Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)	(4)
High SFHA × Disclosure × Post	$-.017^{**}$			
	(.007)			
SFHA × Post		$-.074^{**}$	$-.011^{***}$	$.014^{***}$
		(.030)	(.003)	(.004)
State × Year × Stack FE	X			
Community × Stack FE	X			
Avg D.V.			0.675	0.095
Bandwidth		301	138	262
Num. obs.	239471	1915717	1483356	1700002

Note: Column (1) is estimated based on equation (5) using annual Census Place population estimates. Columns (2)–(4) are estimated based on equation (6) using the decennial census block-level data. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5.1 column (1) shows that the disclosure policy reduces population in high-risk communities—where disclosure treatment intensity is higher—relative to low-risk communities by 1.7%. This suggests that some homebuyers respond to disclosure by choosing homes in lower-risk communities rather than high-risk ones, thereby reducing flood risk exposure. However, the magnitude of this effect may be attenuated since homebuyers can substantially lower their flood risk without necessarily making a large location adjustment. For instance, 58% (74%) of recipients in FEMA's buyout program for flood-prone properties relocate within a 10-mile (20-mile) drive of their original homes while still reducing their flood risk score by over 60% (Elliott and Wang 2023).

Motivated by this, in columns (2)–(4), I exploit census block-level data to explore more granular location responses, with a caveat that δ_6 in equation (6) represents the local average treatment effect near the SFHA boundary. While the magnitude may differ in other areas unless homogeneity in treatment effect is assumed, it should be noted that the SFHA boundary still captures a substantial portion of flood-prone locations, given that only 8% of a typical community land falls within SFHA (Appendix Figure D.4). In column (2), I examine the intensive margin effect, focusing on blocks with a non-zero population, and finds a 7 percent reduction in SFHA population relative to non-SFHA blocks after the disclosure requirement. Column (3) reports the extensive margin effect—the disclosure reduces the probability of having any population in an SFHA block by 0.01 relative to a

disclosed vs. non-disclosed states—rather than SFHA vs. non-SFHA.

non-SFHA block (or 1.5 percent of the baseline value of 0.68).[27] Consistent with these population effects, in column (4), I report that the disclosure increases the vacancy rate for an SFHA block from 0.095 to 0.109. These findings resonate with prior research on population declines in areas affected by negative environmental shocks (Banzhaf and Walsh 2008, Boustan et al. 2012, Hornbeck 2012, Hornbeck and Naidu 2014).

Figure 5.1 (a), corresponding to column (2) of Table 5.1, illustrates the impact of disclosure policy on the population. The horizontal axis is X in equation (6) and blocks with $D = 1$ are presented on the right-hand side of the border. The vertical axis is the difference in log population between pre and post treatment periods. The solid lines is the regression fit and points reflect the difference in log population for each distance bin. δ_2 from equation (6) is normalized to 0 to enhance readability.

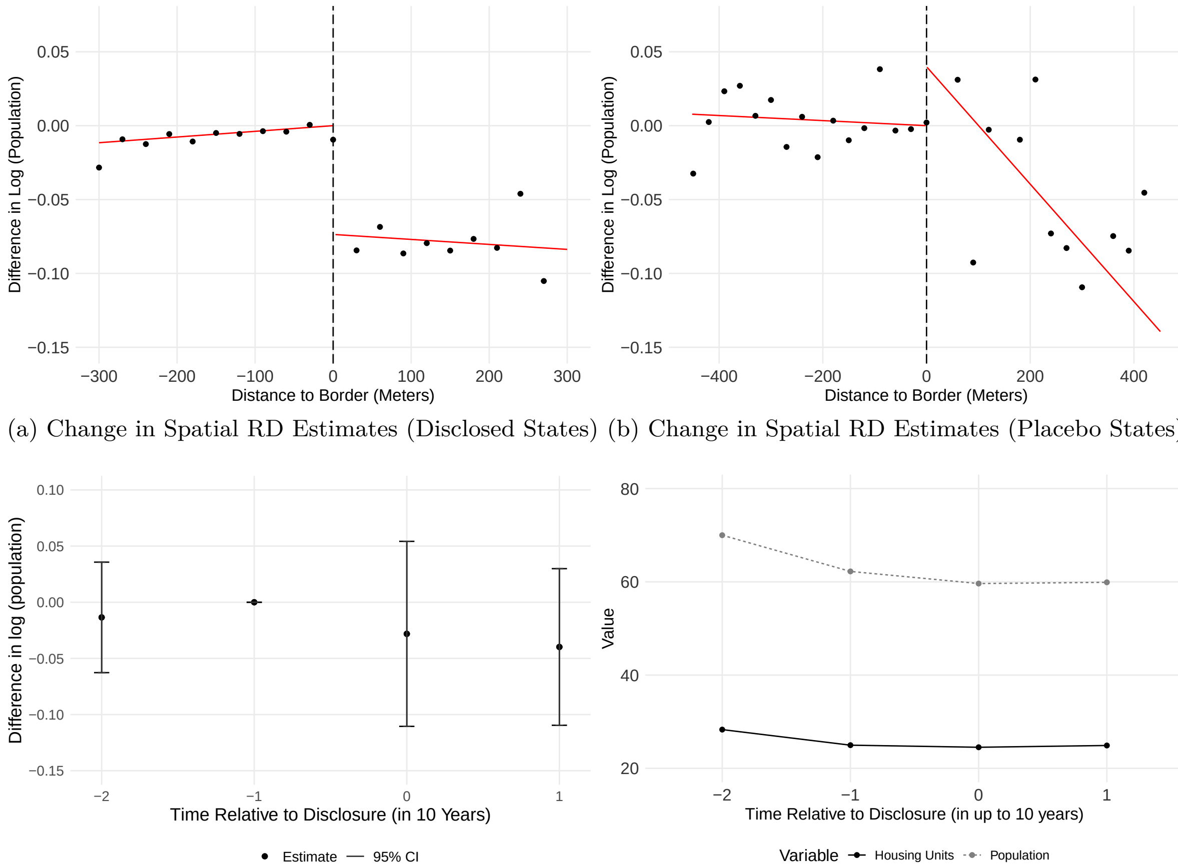
The figure indicates there is a 7 percent drop in the population for SFHA blocks relative to non-SFHA blocks at the SFHA boundary after the disclosure requirement. The tight fit of the regression line to the scatter plot suggests that the choice of functional form for the running variable likely has minimal impact on the estimates. Importantly, as Figure 5.1 (b) shows, no such population change is observed when a (placebo) disclosure requirement does not provide information on flood risk.

In Figure 5.1 (c), I plot coefficients correspond to δ_2 from the equation $Y_{bst} = \delta_0 + \delta_1 X_{bs} + \delta_2 D_{bs} + \delta_3 X_{bs} D_{bs}$, estimated separately for each time period relative to the disclosure policy change, and normalized to δ_2 at relative time -1.[28] The analysis focuses on blocks that appear in at least four decennial censuses, which implies that 80% of observations excluded due to the 1980 census covering only urban or metropolitan areas. Although the results are underpowered due to this sample restriction, Panel (c) indicates that there was no population difference between SFHA and non-SFHA areas before the disclosure, but a 3-4% relative decline in SFHA area population after the disclosure. Interestingly, the raw population and housing units for an average SFHA block in Panel (d) remains constant even after the disclosure, suggesting that the population adjustments stem from diverted in-migration and suppressed development, rather than active out-migration. This is consistent with the policy's role in informing prospective buyers rather than existing homeowners.

Two potential concerns regarding the difference-in-discontinuity results merit attention. First,

[27] Note that the number of observations in column (2) is *greater* than in column (3), despite its sample restrictions (i.e., focusing on non-zero population blocks), because the estimated optimal bandwidth is larger for column (2).

[28] Relative time indicators are defined as -2 (-1) for 19 to 10 (9 to 1) years before the policy change, and 0 (1) for 0 to 9 (10 to 19) years after the policy change.



(a) Change in Spatial RD Estimates (Disclosed States) (b) Change in Spatial RD Estimates (Placebo States)

(c) Spatial RD Estimates Over Time (d) Raw Trends for SFHA Blocks

Figure 5.1: The Effect of Disclosure on Population. Panels (a) and (b) illustrate difference-in-discontinuity estimates for the disclosed and placebo states, respectively. The dependent variable is the change in log of population after the disclosure. The running variable is the distance between a census block and the nearest SFHA border. The discontinuity at the threshold (dashed vertical line) represents δ_6 in equation (6). Panel (c) plots the disclosure effect over time relative to the disclosure policy change timing. Panel (d) plots population and housing unit trends (in absolute terms) over time for SFHA blocks. For Panels (c) and (d), I limit the sample to blocks that are observed in at least four decennial censuses.

time-varying confounders may affect the results. Over a 10-year period, many factors could change differentially between SFHA and non-SFHA areas, such as the enforcement of flood insurance purchase requirements. Second, the disclosure requirement may have a spillover effect (i.e., violate the stable unit treatment value assumption (SUTVA)) (Donaldson 2015). For instance, homebuyers who would have chosen properties on SFHAs may instead choose nearby properties in non-SFHAs after the disclosure policy.

Per the first issue, it is useful to highlight that Table 5.1 column (1) reports that the disclosure policy reduces population in high-risk areas even when I use the *annual* data, mitigating concerns about other time-varying policy changes. As for the second issue, strategic avoidance based on disclosure information is unlikely, as discussed in Section 2, since the policy only provides risk information for a given property, not broader flood risk patterns. Additionally, in Appendix C.2, I conduct formal robustness tests to further assess these concerns.

Taken together, the disclosure policy appears to reduce flood damage, presumably by preventing an increase in flood risk exposure through its effect of diverting in-migration. However, given that purchasing insurance is a potential alternative to self-protection, namely choosing a safer location (Ehrlich and Becker 1972), one might wonder about the disclosure's impact on flood insurance take-up decisions.[29] Findings in Appendix C.1 show that take-up rates in high-SFHA areas do not increase relative to low-SFHA areas after a disclosure policy change, suggesting that homebuyers primarily respond through self-protection. Moreover, despite relative declines, the absolute number of flood insurance policies in high-risk areas does not appear to decline (columns (3) and (6) of Appendix Table C.1). This implies that the damage reduction effect in Section 4.2 is not merely an artifact of a decline in absolute flood insurance counts in high-SFHA areas.

Such a choice may reflect relative benefits and costs of the two protection measures: choosing a safer location is often less costly for homebuyers, as they plan to move regardless and typically consider more than 10 properties before closing (Zumpano et al. 2003). Moreover, the NFIP coverage may provide limited benefits: it does not (1) cover asset losses over $250,000 and (2) compensate for numerous economic costs (e.g., loss of income or use value) beyond asset losses (Lee et al. 2024).

6 Conclusion

Floods are the costliest natural disaster in the US and are expected to become more frequent and severe in the future. Thus, reducing economic loss from these events is of first-order importance. By exploiting plausibly exogenous variations created by a Home Seller Disclosure Requirement, I find that the policy reduces the annualized probability of flood damage by 2.7 percentage points (or 38 percent from the baseline) by decreasing the population in high-risk areas. These findings indicate that alleviating information frictions regarding flood risk in the housing market can facilitate volun-

[29] Investigating both margins is important because they have starkly different implications for flood damage. Self protection can reduce flood risk exposure whereas buying insurance simply redistribute income from "dry" to "flood" state, without reducing exposure unless self-protection is rewarded with lower insurance premiums (Ehrlich and Becker 1972). However, the NFIP premium is heavily subsidized and too coarse to account for all self-protection measures (Kousky 2019). Wagner (2022) also finds that substitution between two margins is prevalent in the NFIP context.

tary adaptation by helping homebuyers make more informed choices. Further, given that compliance costs for this policy are likely moderate at most,[30] the disclosure requirement should produce a large social welfare gain.

[30] Moore and Smolen (2000) documents that home sellers spend less than 40 minutes to fill out the disclosure form.

References

Alves, G., W. H. Burton, and S. Fleitas. 2024. Difference in Differences in Equilibrium: Evidence from Placed-Based Policies. Working Paper.

Bakkensen, L. A., and L. Barrage. 2021. Going Underwater? Flood Risk Belief Heterogeneity and Coastal Home Price Dynamics. The Review of Financial Studies:hhab122.

Bakkensen, L. A., and L. Ma. 2020. Sorting over flood risk and implications for policy reform. Journal of Environmental Economics and Management 104:102362.

Banzhaf, H. S., and R. P. Walsh. 2008. Do People Vote with Their Feet? An Empirical Test of Tiebout's Mechanism. American Economic Review 98:843–863.

Barreca, A., K. Clay, O. Deschenes, M. Greenstone, and J. S. Shapiro. 2016. Adapting to Climate Change: The Remarkable Decline in the US Temperature-Mortality Relationship over the Twentieth Century. Journal of Political Economy 124:105–159.

Baylis, P. W., and J. Boomhower. 2021. Mandated Vs. Voluntary Adaptation to Natural Disasters: The Case of U.S. Wildfires. NBER Working Paper.

Baylis, P., and J. Boomhower. 2022. The Economic Incidence of Wildfire Suppression in the United States. American Economic Journal: Applied Economics:51.

Bin, O., and C. E. Landry. 2013. Changes in implicit flood risk premiums: Empirical evidence from the housing market. Journal of Environmental Economics and Management 65:361–376.

Bosker, M., H. Garretsen, G. Marlet, and C. Van Woerkens. 2019. Nether Lands: Evidence on the Price and Perception of Rare Natural Disasters. Journal of the European Economic Association 17:413–453.

Boustan, L. P., M. E. Kahn, and P. W. Rhode. 2012. Moving to Higher Ground: Migration Response to Natural Disasters in the Early Twentieth Century. American Economic Review 102:238–244.

Brot-Goldberg, Z., T. Layton, B. Vabson, and A. Y. Wang. 2020. The Behavioral Foundations of Default Effects: Theory and Evidence from Medicare Part D. Working Paper:87.

Bureau of the Census. 1994. Geographic Areas Reference Manual.

Burke, M., and K. Emerick. 2016. Adaptation to Climate Change: Evidence from US Agriculture. American Economic Journal: Economic Policy 8:106–140.

Burke, M., S. M. Hsiang, and E. Miguel. 2015. Global non-linear effect of temperature on economic production. Nature 527:235–239.

Busso, M., J. Gregory, and P. Kline. 2013. Assessing the Incidence and Efficiency of a Prominent Place Based Policy. American Economic Review 103:897–947.

Calonico, S., M. D. Cattaneo, and R. Titiunik. 2014. Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs: Robust Nonparametric Confidence Intervals. Econometrica 82:2295–2326.

Cameron, A. C., J. B. Gelbach, and D. L. Miller. 2011. Robust Inference With Multiway Clustering. Journal of Business & Economic Statistics 29:238–249.

Carleton, T. A., and S. M. Hsiang. 2016. Social and economic impacts of climate. Science 353:aad9837–aad9837.

Cattaneo, M. D., N. Idrobo, and R. Titiunik. 2019. A Practical Introduction to Regression Discontinuity Designs: Foundations. arXiv:1911.09511 [econ, stat].

Cattaneo, M. D., and R. Titiunik. 2022. Regression Discontinuity Designs. Annual Review of Economics 14:821–851.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer. 2019. The Effect of Minimum Wages on Low-Wage Jobs. The Quarterly Journal of Economics 134:1405–1454.

Chen, J., and J. Roth. 2022. Log-like? Identified ATEs Defined with Zero-valued Outcomes are

(Arbitrarily) Scale-dependent. Working Paper.
Chivers, J., and N. E. Flores. 2002. Market Failure in Information: The National Flood Insurance Program. Land Economics 78:515–521.
Cicco, L. A. D., D. Lorenz, R. M. Hirsch, and W. Watkins. 2018. dataRetrieval: R packages for discovering and retrieving water data available from U.S. Federal hydrologic web services. U.S. Geological Survey, Reston, VA.
Conley, T. G. 1999. GMM estimation with cross sectional dependence. Journal of Econometrics 92:1–45.
Davenport, F. V., M. Burke, and N. S. Diffenbaugh. 2021. Contribution of historical precipitation change to US flood damages. Proceedings of the National Academy of Sciences 118:e2017524118.
Deryugina, T. 2017. The Fiscal Cost of Hurricanes: Disaster Aid versus Social Insurance. American Economic Journal: Economic Policy 9:168–198.
Deshpande, M., and Y. Li. 2019. Who Is Screened Out? Application Costs and the Targeting of Disability Programs. American Economic Journal: Economic Policy 11:213–248.
DHS Office of Inspector General. 2017. FEMA Needs to Improve Management of Its Flood Mapping Programs. Department of Homeland Security.
Donaldson, D. 2015. The Gains from Market Integration. Annual Review of Economics 7:619–647.
Edmund, H., S. Chamberlain, and K. Ram. 2014. Rnoaa: NOAA climate data from R.
Ehrlich, I., and G. S. Becker. 1972. Market Insurance, Self-Insurance, and Self-Protection. Journal of Political Economy 80:623–648.
Elliott, J. R., and Z. Wang. 2023. Managed retreat: A nationwide study of the local, racially segmented resettlement of homeowners from rising flood risks. Environmental Research Letters 18:064050.
England Jr, J. F., T. A. Cohn, B. A. Faber, J. R. Stedinger, W. O. Thomas Jr, A. G. Veilleux, J. E. Kiang, and R. R. Mason Jr. 2019. Guidelines for determining flood flow frequency—Bulletin 17C. US Geological Survey.
Fairweather, D., M. E. Kahn, R. D. Metcalfe, and S. S. Olascoaga. 2024. Expecting climate change: A nationwide field experiment in the housing market. National Bureau of Economic Research.
Felbermayr, G., and J. Gröschl. 2014. Naturally negative: The growth effects of natural disasters. Journal of Development Economics 111:92–106.
FEMA. 1996. Q3 Flood Data Users Guide, Draft. FEMA.
FEMA. 2005. National Flood Insurance Program (NFIP) Floodplain Management Requirements: A Study Guide and Desk Reference for Local Officials. FEMA.
FEMA. 2014. Transaction Record Reporting and Processing (TRRP) Plan. FEMA.
Fuller, W. E. 1913. Flood Flows. American Society of Civil Engineers.
Gallagher, J. 2014. Learning about an Infrequent Event: Evidence from Flood Insurance Take-Up in the United States. American Economic Journal: Applied Economics 6:206–233.
Gibson, M., and J. T. Mullins. 2020. Climate Risk and Beliefs in New York Floodplains. Journal of the Association of Environmental and Resource Economists 7:1069–1111.
Goodman-Bacon, A. 2021. Difference in Differences with Variation in Treatment Timing. Journal of Econometrics 225:254–277.
Gottlieb, J. D., R. R. Townsend, and T. Xu. 2022. Does Career Risk Deter Potential Entrepreneurs? The Review of Financial Studies 35:3973–4015.
Gourley, J. J., Y. Hong, Zach ary L. Flamig, Ami Arthur, Robert Clark, Martin Calianno, Isabelle Ruin, Terry Ortel, Mich ael E. Wieczorek, and Kirstetter. 2013. A Unified Flash Flood Database over the US. American Meteorological Society:799–805.
Greenstone, M., and J. Gallagher. 2008. Does Hazardous Waste Matter? Evidence From the Housing Market and the Superfund Program. Quarterly Journal of Economics:951–1003.

Gregory, J. 2017. The Impact of Post-Katrina Rebuilding Grants on the Resettlement Choices of New Orleans Homeowners:53.

Grembi, V., T. Nannicini, and U. Troiano. 2016. Do Fiscal Rules Matter? American Economic Journal: Applied Economics 8:1–30.

Hino, M., and M. Burke. 2021. The effect of information about climate risk on property values. Proceedings of the National Academy of Sciences 118:e2003374118.

Hornbeck, R. 2012. The Enduring Impact of the American Dust Bowl: Short- and Long-Run Adjustments to Environmental Catastrophe. American Economic Review 102:1477–1507.

Hornbeck, R., and S. Naidu. 2014. When the Levee Breaks: Black Migration and Economic Development in the American South. American Economic Review 104:963–990.

Hsiang, S. 2016. Climate Econometrics. Annual Review of Resource Economics 8:43–75.

Hsiang, S. M. 2010. Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America. Proceedings of the National Academy of Sciences 107:15367–15372.

Hsiang, S., and A. Jina. 2014. The Causal Effect of Environmental Catastrophe on Long-Run Economic Growth: Evidence From 6,700 Cyclones. National Bureau of Economic Research, Cambridge, MA.

IPCC. 2022. Climate Change 2022 Impacts, Adaptation and Vulnerability.

Jackson, L. E. 2013. Frequency and Magnitude of Events. Pages 359–363 *in* P. T. Bobrowsky, editor. Encyclopedia of Natural Hazards. Springer, Dordrecht.

Kousky, C. 2019. The Role of Natural Disaster Insurance in Recovery and Risk Reduction. Annual Review of Resource Economics 11:399–418.

Kousky, C., E. F. P. Luttmer, and R. J. Zeckhauser. 2006. Private investment and government protection. Journal of Risk and Uncertainty 33:73–100.

Kousky, C., and E. Michel-Kerjan. 2015. Examining Flood Insurance Claims in the United States: Six Key Findings: Examining Flood Insurance Claims in the United States. Journal of Risk and Insurance 84:819–850.

Kousky, C., E. O. Michel-Kerjan, and P. A. Raschky. 2018. Does federal disaster assistance crowd out flood insurance? Journal of Environmental Economics and Management 87:150–164.

Kron, W., M. Steuer, P. Löw, and A. Wirtz. 2012. How to deal properly with a natural catastrophe database – analysis of flood losses. Natural Hazards and Earth System Sciences 12:535–550.

Kunreuther, H., and M. Pauly. 2004. Neglecting Disaster: Why Don't People Insure Against Large Losses? Journal of Risk and Uncertainty 28:5–21.

Lee, S., X. Wan, and S. Zheng. 2024. Beyond Asset Losses: Estimating the Economic Cost of Floods.

Lefcoe, G. 2004. Property Condition Disclosure Forms: How the Real Estate Industry Eased the Transition from Caveat Emptor to "Seller Tell All". Real Property, Probate and Trust Journal 39:193–250.

Lemieux, T., and K. Milligan. 2008. Incentive effects of social assistance: A regression discontinuity approach. Journal of Econometrics 142:807–828.

Maimone, M., and T. Adams. 2023. A Practical Method for Estimating Climate-Related Changes to Riverine Flood Elevation and Frequency. Journal of Water and Climate Change 14:748–763.

Mallakpour, I., and G. Villarini. 2015. The changing nature of flooding across the central United States. Nature Climate Change 5:250–254.

Manson, S. M., J. Schroeder, D. V. Riper, T. Kugler, and S. Ruggles. 2022. IPUMS national historical geographic information system: Version 17.0. Minneapolis, MN.

Miao, Q., and D. Popp. 2014. Necessity as the Mother of Invention: Innovative Responses to Natural Disasters. Journal of Environmental Economics and Management 68:280–295.

Milly, P. C. D., R. T. Wetherald, K. A. Dunne, and T. L. Delworth. 2002. Increasing risk of great floods in a changing climate. Nature 415:514–517.

Moore, G. S., and G. Smolen. 2000. Real Estate Disclosure Forms and Information Transfer. Real Estate Law Journal 28:319–337.

Mullahy, J., and E. C. Norton. 2022. Why Transform Y? A Critical Assessment of Dependent-Variable Transformations in Regression Models for Skewed and Sometimes-Zero Outcomes. NBER Working Paper.

National Association of Realtors. 2019. State Flood Hazard Disclosures Survey.

National Weather Service. 2019. Natinal Weather Service Manual 10-950: Definitions and General Terminology. National Weather Service.

Newey, W. K., and K. D. West. 1987. A simple, positive semi-definite, heteroskedasticity and autocorrelationconsistent covariance matrix. Econometrica 55:703–708.

NOAA. 2020. US Billion-Dollar Weather & Climate Disasters 1980-2020.

Noonan, D., L. Richardson, and P. Sun. 2022. Flood Zoning Policies and Residential Housing Characteristics in Texas. SSRN Electronic Journal.

Ostriker, A., and A. Russo. 2023. The Effects of Floodplain Regulation on Housing Markets. Working Paper.

Pancak, K. A., T. J. Miceli, and C. F. Sirmans. 1996. Residential Disclosure Laws: The Further Demise of Caveat Emptor. Real Estate Law Journal 24:291–332.

Peralta, A., and J. B. Scott. 2020. Does Subsidized Flood Insurance Alter Location Incentives? Evidence from the National Flood Insurance Program. Working Paper.

Pettersson-Lidbom, P. 2012. Does the size of the legislature affect the size of government? Evidence from two natural experiments. Journal of Public Economics 96:269–278.

Pope, J. C. 2008. Do Seller Disclosures Affect Property Values? Buyer Information and the Hedonic Model. Land Economics 84:551–572.

Redfin. 2023. Migration to Flood-Prone Areas Has More Than Doubled Since 2020.

Reeves, A. 2011. Political Disaster: Unilateral Powers, Electoral Incentives, and Presidential Disaster Declarations. The Journal of Politics 73:1142–1151.

Roberts, F. Y. 2006. Off-Site Conditions and Disclosure Duties: Drawing the Line at the Property Line. Brigham Young University Law Review 957:39.

Roth, J., and P. H. C. Sant'Anna. 2023, May. Efficient Estimation for Staggered Rollout Designs. arXiv.

Saharia, M., P.-E. Kirstetter, H. Vergara, J. J. Gourley, Y. Hong, and M. Giroud. 2017. Mapping Flash Flood Severity in the United States. Journal of Hydrometeorology 18:397–411.

Sangal, B. P. 1983. Practical Method of Estimating Peak Flow. Journal of Hydraulic Engineering 109:549–563.

Slater, L. J., and G. Villarini. 2016. Recent trends in U.S. Flood risk: Recent Trends in U.S. Flood Risk. Geophysical Research Letters 43:12, 428–12, 436.

Stern, S. 2005. Temporal Dynamics of Disclosure: The Example of Residential Real Estate Conveyancing. Utah L. Rev.:57–95.

Strobl, E. 2011. The Economic Growth Impact of Hurricanes: Evidence from U.S. Coastal Counties. Review of Economics and Statistics 93:575–589.

Titus, J. G. 2023. Population in floodplains or close to sea level increased in US but declined in some counties—especially among Black residents. Environmental Research Letters 18:034001.

Tyszka, S. C. 1995. Remnants of the Doctrine of Caveat Emptor May Remain Despite Enactment of Michigan's Seller Disclosure Act. Wayne Law Review 41:1497–1530.

U.S. Department of Homeland Security. 2022. Summary of Proposed Reforms.

US Census Bureau. 2000. 2000 Census of Population and Housing Technical Documentation.

Wagner, K. R. H. 2022. Adaptation and Adverse Selection in Markets for Natural Disaster Insurance. American Economic Journal: Economic Policy 14:380–421.

Washburn, R. M. 1995. Residential Real Estate Condition Disclosure Legislation. DePaul L. Rev. 44:381–459.

Weill, J. A. 2023. Flood Risk Mapping and the Distributional Impacts of Climate Information. Finance and Economics Discussion Series:1–95.

Weinberger, A. M. 1996. Let the Buyer Be Well Informed? - Doubting the Demise of Caveat Emptor. Maryland Law Review 55:387–424.

Weinkle, J., C. Landsea, D. Collins, R. Musulin, R. P. Crompton, P. J. Klotzbach, and R. Pielke. 2018. Normalized hurricane damage in the continental United States 1900–2017. Nature Sustainability 1:808–813.

Zervas, C. 2013. Extreme Water Levels of the United States 1893-2010. National Oceanic; Atmospheric Administration, Silver Spring, Maryland.

Zumpano, L. V., K. H. Johnson, and R. I. Anderson. 2003. Internet use and real estate brokerage market intermediation. Journal of Housing Economics 12:134–150.

A Appendix A: Flood History Data

A.1 Background and Procedure

Background. A key input to the flood damage function is flood size data. I create a measure that satiesfies the following four conditions. First, it should be a continuous measure that allows a non-linear relationship between flood size and damage (Burke et al. 2015, Hsiang 2016).

Second, it should be objective. For instance, the widely used EM-DAT measures flood size using economic cost or death tolls, which are directly correlated with outcome variables of interest (Felbermayr and Gröschl 2014). Another example of a potentially endogenous measure is the occurrence of the Presidential Disaster Declaration (PDD) floods, which depends on the discretion of the president and thus could reflect political interests (Reeves 2011).

Third, it should be comprehensive. A few existing studies have leveraged meteorological measures to objectively measure disasters, but most of them focus on a subset of events. For instance, Deryugina (2017), Hsiang and Jina (2014), and Strobl (2011) have used physical measures of hurricane intensity while Davenport et al. (2021) leveraged precipitation data. Despite objectivity, such an approach has limits in coverage—for instance, precipitation changes alone can explain only one-third of cumulative flood damages (Davenport et al. 2021).

Lastly, since I measure flood damage at the community by year level, flood size should be measured at the same level. This is not trivial because most climate data are collected to answer physical science questions, and thus are not readily mapped into an administrative unit such as a community (Carleton and Hsiang 2016).

To the best of my knowledge, no existing dataset satisfies all of these properties. In this paper, I construct an objective measure of past flood events by applying a hydrologic method to the USGS/NOAA water gauge records. This approach does not distinguish the cause of floods—hurricanes, rainfall, snow melt, etc, as long as it is reflected in the water gauge level. Flood size is defined and recorded by a recurrence interval, which represents the expected number of years for a flood of a given size to come back, and thus is continuous by construction. Also, by matching gauge stations to a community, I can measure flood size at the community level.

Procedure. Following the USGS guideline (England Jr et al. 2019), I implemented the following steps using USGS/NOAA water levels data from 3,505 gauge stations distributed in the 26 ever-disclosed states in the contiguous US (Appendix Figure A.1).[31]

First, I construct a site-specific flood frequency distribution. For this, I retrieved annual peak flow records using the R package "dataRetrieval" and fit the Log-Pearson III distribution to estimate gauge-specific parameters (Cicco et al. 2018). Importantly, as I use annual peak discharge data to fit the distribution, the quantile of the distribution has an intuitive interpretation. For instance, if a certain water level is the 95th percentile of the distribution, it means that such an event would happen with a 5 percent probability in a given year. Equivalently, such an event is called a 20-year ($\frac{1}{0.05} = 20$) flood. I keep stations with at least 10 or more annual peak observations following the USGS guideline. Also, I use annual peak data until 1990, from gauge stations with at least 10 annual peak observations following the USGS guideline, to fix flood thresholds and make flood size comparable across different years.

Second, I convert the daily water level into the recurrence interval using the fitted flood size distribution from the previous step. For this, I need an instantaneous flow, because flood exposure is determined by the maximum, rather than mean, water level. The problem is that for most of the stations, the maximum daily flow (or more precisely the instantaneous peak flow which enables cal-

[31] I randomly sampled 1000 sites in Appendix Figure A.1 for visibility.

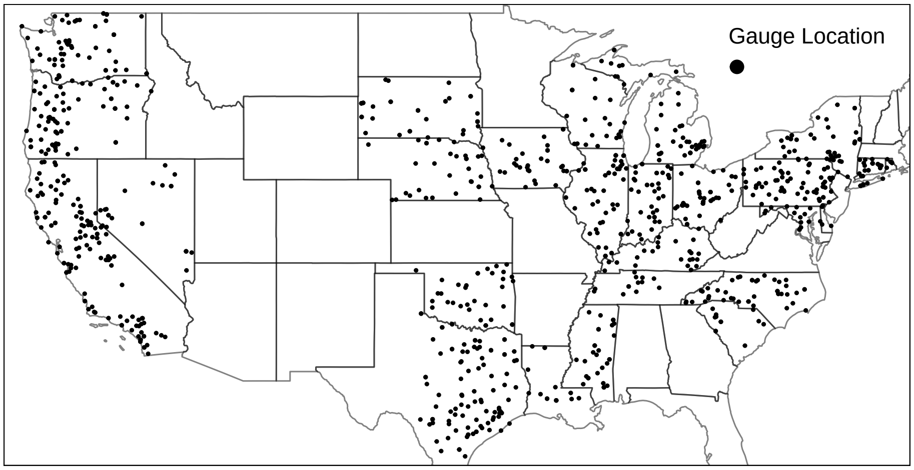


Figure A.1: The Distribution of a Sample of USGS/NOAA Gauges in Disclosure States

Table A.1: Number of Stations with Non-Missing Water Levels Records in Iowa

Name	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
N Gauges (Mean Daily Flow)	112	112	105	107	109	109	105	109	112	111
N Gauges (Maximum Daily Flow)	3	8	40	72	34	31	29	34	59	95

culating maximum daily flow) data have too many missing values. This is problematic because, with many missing observations, flood events will be significantly under-recorded. To overcome this problem, I estimate a projected instantaneous peak flow from the *mean* daily flow. Appendix Table A.1 illustrates the benefit of using mean daily flow in alleviating the missing data problem. For this, I report the number of water gauge stations in Iowa that have daily water level records for at least 80 percent of the days (i.e., 292 days or more) for a given year. It can be easily seen that there can be an order of magnitude difference in the number of stations that have mean versus maximum daily water records.

To estimate the daily maximum water level from the daily mean water level, I use the Fuller method in the following steps (Fuller 1913).[32] Step 1, I list up gauge stations that (1) are located in a given geographic units (state, HUC4, and HUC2) and (2) have both instantaneous peak flow (Q_{it}^{IPF}) and mean daily flow (Q_{it}^{MDF}) records.[33] Step 2, using these gauge stations, I estimate Fuller coefficients using equation (7) (Fuller 1913).[34] Step 3, using the estimated coefficients, I calculate the projected instantaneous peak flow and compare that with the actual instantaneous peak flow to pick the geographic unit (state, HUC2, HUC4) that minimizes the prediction error for each gauge. Step 4, using the chosen Fuller coefficients, I estimate instantaneous peak flow for gauges that only have daily mean flow records.

$$Q_{it}^{IPF} = Q_{it}^{MDF}(1 + \alpha A^{\beta}) \tag{7}$$

[32] I also did conversion following Sangal (1983), but the error between actual and the estimated IPF was much smaller with Fuller (1913).

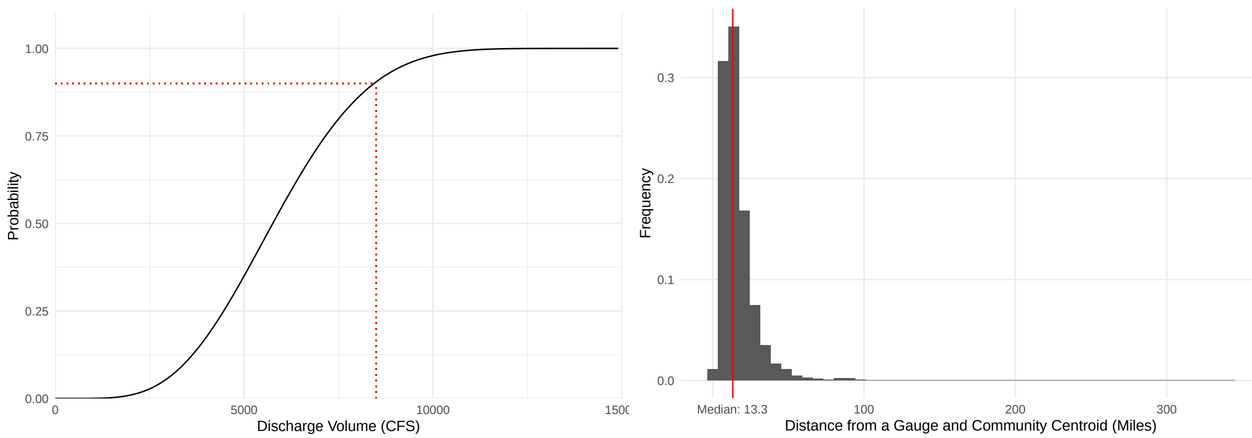
[33] A watershed is uniquely identified by a hydrologic unit code (HUC). There are six levels in the hierarchy, and HUC2 (regions) and HUC4 (sub-regions) are the two highest levels. There are a total of 18 and 202 HUC2s and HUC4s in the contiguous US (Maimone and Adams 2023).

[34] I manipulate equation (7) as $\frac{Q_{it}^{IPF}}{Q_{it}^{MDF}} - 1 = \alpha A^{\beta}$ and take log on both sides to estimate α and β.

Now, by converting the estimated instantaneous peak flow to the quantile of the estimated Log-Pearson III CDF from step 1, I identify each day's flood size.

Third, I translate the quantiles into recurrence intervals and take the maximum recurrence interval for each year.[35]

Finally, to translate gauge-level flood events to community-level data, I match each community to its three nearest gauges, determined by the distance between the community's centroid and the gauge stations. I then calculate a community-level flood size using an inverse distance-weighted average of the flood sizes recorded at these gauges. Appendix Figure A.2 (b) presents the distribution of the average distance between gauges and community centroid. Over 90 percent of them are within 20 miles with a median distance of 13 miles.



(a) Illustration: Flood Frequency Analysis

(b) Distance between Gauge and Community Centroids

Figure A.2: Flood Frequency Analysis and Gauge Matching. Panel (a) is an example of flood frequency analysis. The black solid line represents the CDF of the fitted Log-Pearson III distribution using annual peak flows from the USGS site 03251000. If a daily discharge volume is 8,500 CFS, it corresponds to the 90th quantile or a 10-year flood. Panel (b) presents the distribution of the distance between a gauge and community centroid. Over 90% of them are within 20 miles with the median distance 13 miles.

Appendix Figure A.2 (a) illustrates how to construct flood size from the daily water level using a site-specific flood frequency distribution. The black solid line is the fitted Log-Pearson III CDF from the USGS site 03251000. To fit the distribution, I use the annual peak flow data from 1947 to 1990 to calculate the mean, standard deviation, and skewness parameters. Now suppose that on a given date, the daily discharge volume is 8,500 CFS. As it corresponds to the 90th percentile of the CDF, it can be concluded that there was a 10-year flood on that day.

Note, because the USGS gauge stations rarely cover coastal areas, I add 45 additional NOAA sites to the gauge station data. Zervas (2013) documents GEV distribution parameters for all NOAA sites, so I adopt them and calculate gauge specific recurrence intervals. NOAA water level data are retrieved using the R package "Rnoaa" (Edmund et al. 2014).

Unified flash flood database. The Unified Flash Flood Database (Gourley et al. 2013) is a USGS-gauge record-based dataset constructed following a similar procedure described above. It is a comprehensive and objective measure of flood events that can present the overall trend of flood events for the contiguous US, which overcomes many limitations of the existing data. However, I have opted

[35]The recurrence interval for quantile q is $\frac{1}{1-q}$. For instance, a discharge volume of the 90th percentile, which means it is the 90th highest among 100 yearly maximum observations, corresponds to a 10-year flood.

not to utilize this database due to its likely substantial underreporting of flood events. This underreporting arises because the database is constructed using instantaneous peak flows, which, as shown in Appendix Table A.1, have a significant number of missing observations (and missing data is regarded as "no flood").

A.2 Validation and Summary Statistics

To validate the flood history data, I check the number of average 2-year flood events over a 20-year period for the 7,821 communities from the 26 ever-disclosed states that are on the Q3 map. By definition, a 2-year flood happens 10 times in a 20-year period on average. Figure A.3 (a) shows that most communities had ten 2-year floods over the 20 years whereas the average number of 2-year floods is 11.1. While this is slightly higher than 10, it is plausible given that I stop updating annual peak flow beyond 1990 for consistency over time. Although this approach can be problematic as the period in consideration gets longer, it should not be a major problem for this paper as the sample period is 20 years.

Figure A.3 (b) shows the distribution of flood size (i.e., recurrence interval), where flood size is truncated at 100 for readability. As well documented in the literature, the histogram follows a log-normal distribution, and the frequency decreases as an inverse power function of the flood size (Jackson 2013).

In Panel (c), I plot the number of unique flood events for each community-year, conditional on having an event with a flood size between 2 and 50. The histogram shows that 65 percent of the community-years have exactly one event. This alleviates a concern over measuring flood exposure as the maximum flood size for a given year. More importantly, when I limit attention to floods with size over 10 in Panel (d), which incurs disproportionately large damage, 95 percent of the community-year pairs have only one such event.

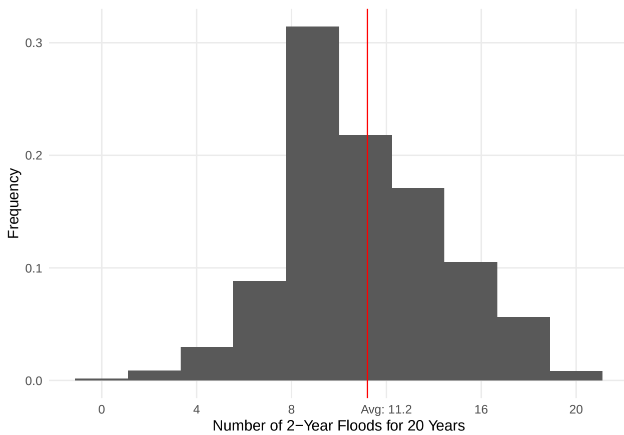
Table A.2: Comparing the Estimated Flood Size Thresholds with the NWS Threshold

	2 Year Flood	10 Year Flood	50 Year Flood	100 Year Flood
Minor	0.778***	1.285***	1.74***	1.944***
	(0.052)	(0.071)	(0.102)	(0.124)
Moderate	0.594***	0.994***	1.36***	1.526***
	(0.042)	(0.06)	(0.085)	(0.103)
Major	0.45***	0.771***	1.081***	1.226***
	(0.034)	(0.043)	(0.051)	(0.06)

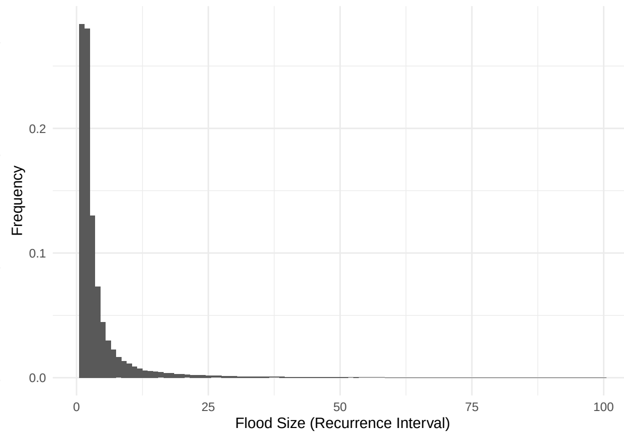
Note:
Note: The entries report the results from 12 separate regressions where each column represents four different dependent variables and each row represents three different regressors. Standard errors are clustered at the gauge level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

For further validation, in Appendix Table A.2, I compare flood size with the gauge-specific NWS thresholds for minor, moderate, and major floods.[36] Specifically, I estimate equation (8) where Q_{ik} is the estimated flood threshold for site i for flood size k where $k \in \{2, 10, 50, 100\}$. NWS_{ij} is flood thresholds from the NWS for site i for flood severity j where $j \in \{\text{minor, moderate, major}\}$.

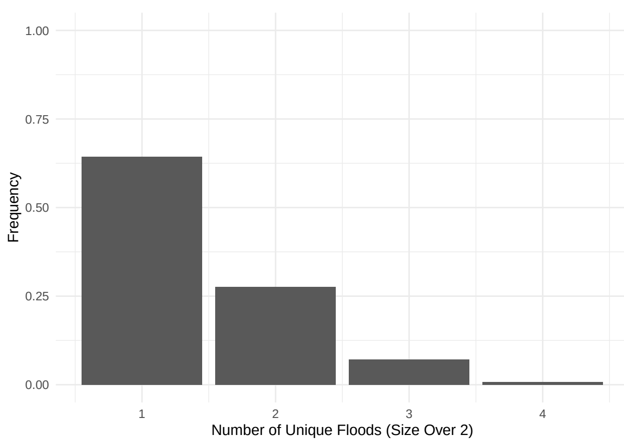
[36] NWS defines each flood category as the following (National Weather Service 2019). Minor: minimal or no property damage, but possibly some public threat (e.g., inundation of roads). Moderate: some inundation of structures and roads near a stream, evacuations of people, and/or transfer of property to higher elevations. Major:extensive inundation of structures and roads, significant evacuations of people, and/or transfer of property to higher elevations.



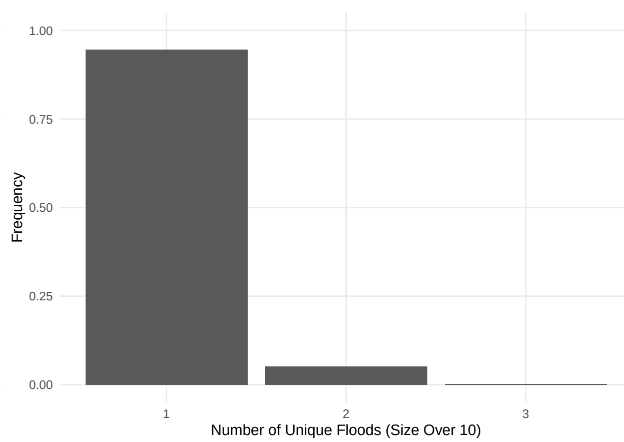
(a) Number of 2-Year Floods over 20 Years



(b) Flood Size



(c) N of Unique Floods (size between 2 and 50) by Community-year



(d) N of Unique Floods (size between 10 and 50) by Community-year

Figure A.3: Flood Data Summary Plots. Panel (a) shows the distribution of the number of 2-year flood exposures at the community level over 20 years. Panel (b) shows the distribution of flood event size (i.e., recurrence interval), where flood size is truncated at 100 for readability. Panel (c) illustrates the number of unique floods (size over 2) for community-year. Panel (d) repeats Panel (c) for floods with size over 10.

$$Q_{ik} = \beta NWS_{ij} + \epsilon_{ijk} \tag{8}$$

β is the coefficient of interest which illustrates how comparable the two thresholds are. For this analysis, I use 2,093 sites that have both recurrence interval-based flood size and the NWS flood thresholds. Appendix Table A.2 reports the estimated β for 12 separate regressions and provides useful insights. First, a minor flood from the NWS is comparable to a flood of size between 2 and 10. To see this, observe that when a minor threshold increases by 1 unit, a 2-year flood threshold increases by only 0.78 units. Conversely, when a minor threshold increases by 1 unit, a 10-year flood threshold increases by 1.29 units. Second, a 10-year flood threshold is tightly comparable to a moderate flood threshold ($\beta = 0.99$). Similarly, a 50-year flood closely matches a flood with a major impact ($\beta = 1.08$). Note, a 100-year flood threshold is 23 percent higher than a major flood threshold, which is plausible given that a 50-year flood threshold is comparable to the major category. The monotonic relationship between the recurrence interval based flood size and NWS impact metric adds further credibility to the adequacy of the flood history data.

B Appendix B: Additional Data Validation

Key dependent variables. A prevalence of zeros are consistent with findings from external sources. For flood damage, no prior studies have cataloged the fraction of community-years with zero flood damage. However, a back-of-the-envelop calculation suggests that this statistic is in line with existing studies. For that, I take the average probability (1.45 percent) of filing a claim per policy over 1980–2012 from Kousky and Michel-Kerjan (2015) and multiply it by the number of flood insurance policies by the community in my sample. The result reveals that 17 percent of communities are predicted to have more than one claim in a given year (i.e., 83 percent of community-year observations are predicted to have zero claims). Note, while 83 percent is substantially lower than 95 percent as discussed in Section 3, this is a direct consequence of sample restriction: as I discuss in detail in Section 4.1, I remove floods with size 50 or above from my analysis for various economic and statistical reasons. When I undertake the same calculation without imposing these sample restrictions, I find that 86 percent of community-year observations have zero claims, a figure consistent with the 83 percent calculated based on Kousky and Michel-Kerjan (2015).

For block population, Bureau of the Census (1994) reports that a substantial number of blocks have zero population, with state-level proportions ranging from 14 percent (RI) to 65 percent (WY), and a median value of 31 percent (WA). In my sample, the numbers are slightly different at 17 percent for RI and 26 percent for WA (WY is a non-disclosure state). A minor discrepancy is not surprising given that blocks not included in the digitized flood map are excluded from the analysis.

Flood map revision data. According to the Community Map History from the Flood Insurance Study (FIS) reports, 10.5% of communities in my sample revised their flood maps during the sample period—spanning 10 years before and after each state's disclosure policy change. While this figure may seem too low in comparison to other studies, such as Weill (2023), which, in Figure 2.A, suggests that *at least* twice as many geographic units experienced a map revision by 2020, this discrepancy can largely be attributed to differences in sample periods. Weill (2023) primarily focuses on map revisions during the 2010s, while my primary sample period includes the 1990s and 2000s, which had significantly fewer revisions. As shown in Appendix Figure B.1, which plots the distribution of flood map revision years from the FIS reports, the number of revisions notably increased in the 2010s. In fact, 84% of communities in my sample revised their flood maps at least once over 1980—2020.

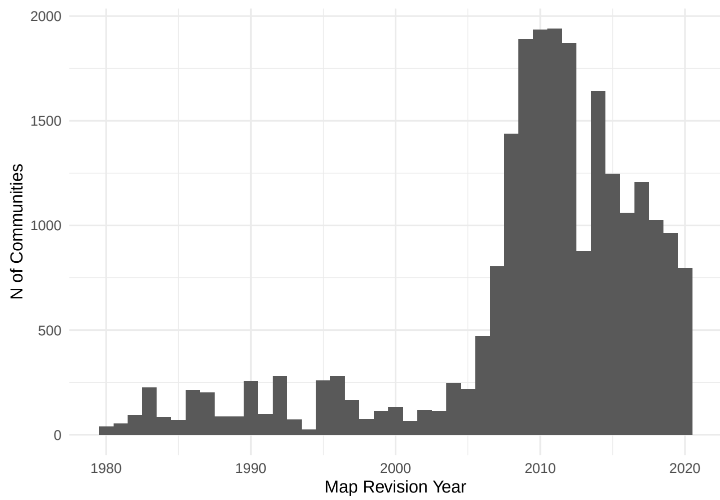


Figure B.1: The Number of Communities with Flood Map Revisions by Year. This figure illustrates the number of communities with flood map revision from 1980 to 2020 using Community Map History from the Flood Insurance Study (FIS) reports.

Further, FEMA's Compendium of Map Updates (Hino and Burke 2021) seems to document any changes made in a given year. For example, in 2009, communities 371038, 42027C, 21189C, and 17083C are all listed in the compendium as having map updates, but the corresponding FIS reports indicate that these maps were created for the first time. Since my goal is to account for potential changes in flood zones delineated in the Q3 flood map, I focus exclusively on map revisions. The Community Map History data from the FIS reports is therefore more appropriate for this purpose.

C Appendix C: Further Results on the Disclosure and Homebuyer Responses

C.1 Disclosure and Flood Insurance Take Up

To evaluate the impact of the disclosure requirement on flood insurance take up, I collect the number of flood insurance policies at the National Flood Insurance Program (NFIP) community level for 1982–2011.[37] The empirical approach is identical to equation (5).

In Appendix Table C.1 column (1), I show that the disclosure policy lowers the probability of having at least one flood insurance policy in high-risk communities relative to low-risk communities by 0.01 (or 1.3 percent from the baseline of 0.84). Column (4) indicates the intensive margin effect of the disclosure policy on the number of insurance policies is also small at –5 percent. Further, columns (2) and (5), I show that removing communities that have experienced map updates during the sample period produces somewhat attenuated but similar results as columns (1) and (4). Given the estimated coefficients, flood insurance does not seem to be the primary margin homebuyers respond to the disclosure policy.

In columns (3) and (6), I estimate a difference-in-differences, as opposed to a triple difference, version of equation 5 for high-risk communities to evaluate the impact of the disclosure policy on the absolute count of flood insurance policies. The estimates in columns (3) and (6) suggest that

[37]For the 1982–2007 period, I leverage data from Gallagher (2014). For the 2009–11 period, I use publicly available NFIP policy data from FEMA. For 2008, I leverage policy counts data that I acquired through Freedom of Information Act requests.

Table C.1: The Effect of Discosure on Flood Insurance Take-Up

	Prob. of Any Insurance			Log Insurance		
	(1)	(2)	(3)	(4)	(5)	(6)
High SFHA × Disclosure × Post	−.012	−.010		−.048	−.029	
	(.012)	(.013)		(.062)	(.062)	
Disclosure × Post			.021			.081
			(.013)			(.083)
Avg D.V.	0.897	0.89	0.937			
State × Year × Stack FE	X	X		X	X	
Year × Stack FE			X			X
Community × Stack FE	X	X	X	X	X	X
Sample	All	No Map Update	High Risk	All	No Map Update	High Risk
Num. obs.	239471	214928	239471	214742	191251	214742

Note: Columns (1), (2), (4), and (5) are produced from equation (5) using community-level NFIP data. For columns (3) and (6), a parsimonious version of equation (5) that ignores high vs. low flood risk is used. In columns (2) and (5), communities with flood map updates are excluded. Columns (3) and (6) are estimated using high flood risk communities. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

individuals residing in high risk areas do not seem to reduce flood insurance purchases following the disclosure requirement, even though the relative decline compared to low-risk areas is documented in columns (1) and (4). These findings imply that the damage reduction effect in Section 4.2 is not merely an artifact of smaller flood insurance counts.

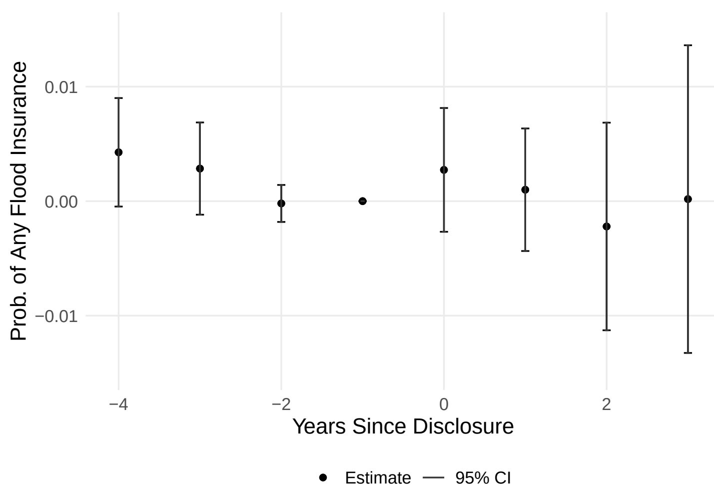


Figure C.1: The Effect of Disclosure on the Probability of Having Any Flood Insurance. This figure depicts the impact of disclosure on the probability of having any flood insurance policy at the community level using an event study version of equation (5). The error bar represents the 95% confidence interval.

In Appendix Figure C.1, I plot the differential impact of disclosure policy on the probability of having flood insurance for high-risk communities in event time using an event study version of equation (5). The estimated coefficients do not show economically meaningful changes in the probability of flood insurance take up after the policy change.

C.2 Robustness Checks with Difference-in-Discontinuity Results

In this section, I conduct more formal robustness tests. For the first issue—time-varying confounders—I conduct three additional tests. First, I replicate Table 5.1 for five placebo states. If my findings are driven by concurrent policy changes rather than the disclosure, the placebo states should show similar effects. However, Appendix Table C.2 reveals no evidence of reduced population or increased vacancy rates in high-risk areas for the placebo states. Also, as there are only five placebo states, I use wild bootstrap for inference and report p.values in parentheses.

Table C.2: The Effect of Discosure on Net Population Flow (Placebo States)

	Log Population	Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)
SFHA × Post	.040	−.001	.000
	(.601)	(.899)	(.987)
Avg D.V. (Within BW)		0.659	0.089
Bandwidth	459	452	324
Num. obs.	169094	253533	130398

Note: This table is produced from equation (6). Columns (1)–(3) are estimated using the decennial census block-level data in 1990, 2000, 2010, and 2020. Bootstrapped p.values are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Instead of running a separate regression for the placebo states as in Appendix Table C.2, in Appendix Table C.3, I modify equation (6) to incorporate placebo states as additional control units. Specifically, I fully interact the right-hand side of equation (6) with $R_s = 1$, an indicator for ever-disclosed status. The coefficient of $R_s * T_{st} * D_{bs}$ captures the disclosure impact, while controlling for potential time-varying changes at the border (i.e., differential trends between SFHA and non-SFHA areas). Appendix Table C.3 shows that the point estimates for log of population and vacancy rates are nearly identical to those in Table 5.1, although the results are underpowered, perhaps due to the increased number of parameters.

Table C.3: The Effect of Discosure on Net Population Flow (Fully Interact)

	Log Population	Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)
SFHA × Disclosure × Post	−.107	−.001	.010
	(.087)	(.005)	(.010)
Avg D.V. (Within BW)		0.67	0.095
Bandwidth	278	138	254
Num. obs.	1913275	1588398	1765316

Note: This table is produced with an estimating equation that fully interacts the right-hand side of equation (efeq:diffdisc) with $R_s = 1$, an indicator for ever-disclosed status. Columns (1)–(3) are estimated using the decennial census block-level data in 1990, 2000, 2010, and 2020. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Second, I allow time-varying discontinuity at the SFHA border to more directly control for confounding policy changes. For this, I estimate $Y_{bst} = \delta_0 + \delta_1 X_{bs} + \delta_2 D_{bs} + \delta_3 X_{bs} D_{bs} + \sum_{t=\{1990,2000,2010,2020\}} G_t[\delta_0^t + \delta_1^t X_{bs} + \delta_2^t D_{bs} + \delta_3^t X_{bs} D_{bs}] + T_{st}[\delta_4 + \delta_5 X_{bs} + \delta_6 D_{bs} + \delta_7 X_{bs} D_{bs}] + \epsilon_{bst}$

where G_t is an indicator that takes 1 if the time period is in $t = \{1990, 2000, 2010, 2020\}$ and the rest of the notations follow equation (6). The coefficient of interest is δ_6 as before. Appendix Table C.4 columns (1) and (3) shows that if anything, the effect size is much larger (in magnitude) with time-varying discontinuities. One exception is column (2), which shows that the policy has a positive impact on the probability of having any population. However, when compared with the impact on placebo states in column (5), the net effect still seems to be negative.

Table C.4: The Effect of Discosure on Net Population Flow (Time Varying Discontinuity)

	Log Population	Prob. of Any Population	Vacancy Rate	Log Population	Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)	(4)	(5)	(6)
SFHA × Post	$-.289$	.260	.018	.811	.492	$-.061$
	(.043)	(.000)	(.476)	(.309)	(.010)	(.251)
Group	Treated	Treated	Treated	Placebo	Placebo	Placebo
Avg D.V.		0.675	0.095		0.659	0.089
Bandwidth	301	138	262	459	452	324
Num. obs.	1900591	1331286	1685653	167160	225007	128772

Note: This table is produced from equation in footnote 33. Columns (1)-(3) show results from the ever-disclosed states whereas columns (4)-(6) report results from placebo states. P.values, which are calculated using clustered standard error for columns (1)–(3) and bootstrapping for columns (2)–(6), are reported in parentheses.

Third, in Appendix Table C.5, I reproduce Table 5.1 after removing blocks that had flood map update(s) during the sample period and find that the conclusion remains unchanged.

Table C.5: The Effect of Discosure on Net Population Flow (Exc. Blocks with Map Revision)

	Log Population	Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)
SFHA × Post	$-.070^{**}$	$-.009^{***}$	$.011^{***}$
	(.028)	(.003)	(.004)
Avg D.V.		0.67	0.098
Bandwidth	301	138	262
Num. obs.	1466190	1146705	1304308

Note: Estimates are based on equation (6) after removing geographic units that have experienced flood map update. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

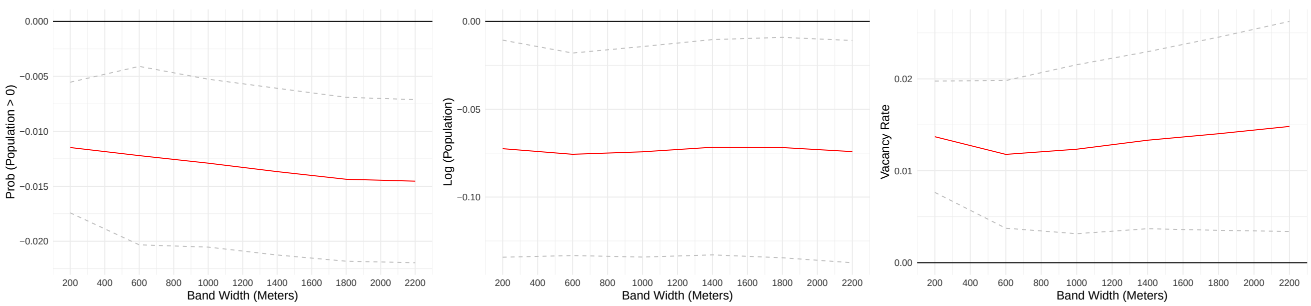
Regarding the treatment spillovers, it is worth highlighting that the SFHA covers a small area—median community has only 7.8% of its land in SFHA (Appendix Figure D.4)—making it unlikely that non-SFHA areas will be significantly "contaminated" by the disclosure policy (Busso et al. 2013, Alves et al. 2024). Moreover, 7.8% is likely an upper bound given that flood maps do not necessarily cover low risk areas within a community.

To more formally test potential spillover effects, I reproduce Table 5.1 using a doughnut difference-in-discontinuity approach with the idea that if there is endogenous sorting near the border, the treatment effect may change without those observations (Cattaneo and Titiunik 2022). Appendix Table C.6 shows that the estimates remain nearly identical even if I remove blocks within 20 to 40 meters from the border.

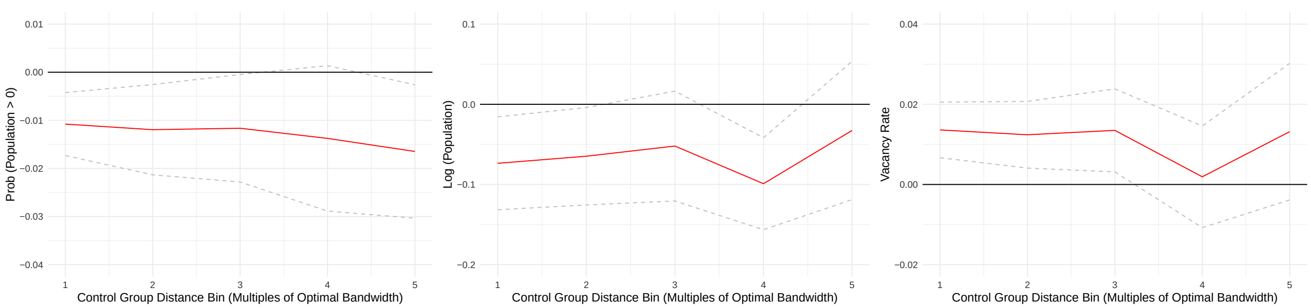
Table C.6: The Effect of Discosure on Population and Vacancy Rate (Doughnut Specification)

	Log Population	Prob. of Any Population	Vacancy Rate	Log Population	Prob. of Any Population	Vacancy Rate
	(1)	(2)	(3)	(4)	(5)	(6)
SFHA × Post	−.079**	−.012**	.013***	−.080**	−.007	.014**
	(.031)	(.004)	(.004)	(.033)	(.005)	(.006)
Avg D.V. (Within BW)		0.692	0.093		0.704	0.092
Doughnut Size	20	20	20	40	40	40
Num. obs.	1763552	1227096	1549019	1607388	984066	1394047

Note: This table is produced from equation (6) after excluding observations closest to the SFHA border. In columns (1)–(3), doughnut sizes are 20 meters and in columns (4)–(6) doughnut sizes are 40 meters. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.



(a) Prob. of Any Population (b) Log Population (c) Vacancy Rate



(d) Prob. of Any Population (e) Log Population (f) Vacancy Rate

Figure C.2: The Effect of the Disclosure Requirement on Population and Vacancy Rate by Bandwidths and Control Group Distance Bin. Panels (a)-(c) plot $\hat{\delta_6}$ from equation (6) for a range of bandwidths. Panels (d)-(f) plot $\hat{\delta_6}$ from equation (6) for control groups of varying distance. In these panels, the horizontal axis indicates the distance bin of control group in multiples of variable specific optimal bandwidth (i.e., distance bin r on x-axis indicates that control group blocks are within $(r - 1)$ and r times optimal bandwidth). In all panels, the level of observation is census block, which is the smallest census geographical unit. Standard errors are clustered at the state level.

Similarly, the policy effect does not diminish even if I expand the bandwidth or use progressively farther away blocks as control units (Appendix Figure C.2 Panels (a)-(c) and Panels (d)-(f)). Note, for Panels (d)-(f), I estimate equation (6) using control blocks that are within the distance of $(r-1) \times$ *optimal bandwidth* and $r \times$ *optimal bandwidth* for $r \in \{1, 2, 3, 4, 5\}$.

D Appendix D: Additional Tables and Figures

Table D.1: Disclosure Policy Adoption by Year

Policy Change Year	Disclosed	Disclosed (w/o Flood)
1992	KY, WI	
1993	MS, OH, RI, SD	
1994	DE, IA, IL, MD, MI, OR, TN, TX	ID, NH
1995	NE, OK, WA	KS
1996	CT, NC, NV, PA	
1998	CA	
1999		ME
2002	IN, NY, SC	
2003	LA	MN

Note:
This table presents the list of states that adopted a disclosure policy by year. The last column includes states that implemented a disclosure policy without a question on flood risk.

Back to 2.

Table D.2: Building Age by the SFHA Status

	N of Houses (< 5Yrs)	N of Houses (> 40Yrs)	(%) Houses (< 5Yrs)	(%) Houses (> 40Yrs)
	(1)	(2)	(3)	(4)
SFHA > 0	119.507***	−254.004***	.057***	−.192***
	(13.437)	(29.864)	(.007)	(.034)
Constant	73.182***	661.449***	.041***	.471***
	(24.349)	(54.139)	(.010)	(.053)
Num. obs.	32391	32391	31490	31490

Note: This table compares the proportion of older and newer housing stocks in census tracts with and without SFHA areas within a tract using the 1990 decennial census data. Standard errors are clustered at the state level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 2.

Table D.3: State Characteristics in 1990 by Disclosure Status

	Ever/Early		Never/Late		Difference	
Variables	Mean	SE	Mean	SE	Mean	P.Value
Panel A: Ever vs. Never States						
Population (millions)	6.57	1.31	3.43	0.651	3.143	0.048
Median Age	33.04	0.204	32.82	0.409	0.22	0.616
(%) White	0.827	0.019	0.879	0.018	-0.053	0.051
(%) BA	0.121	0.005	0.129	0.006	-0.007	0.324
Unemployment Rate	0.06	0.003	0.061	0.002	-0.001	0.773
GDP (billions)	152	34.38	74	14.95	78	0.057
N Housing Units (millions)	2.66	0.506	1.47	0.291	1.187	0.059
(%) Vacancy	0.095	0.005	0.132	0.008	-0.037	0
Democratic Party Vote Share	0.455	0.01	0.425	0.012	0.03	0.06
Average Flood Damage per Housing Unit	3.86	1.99	0.964	0.497	2.891	0.199
Flood Size	6.34	0.805	3.58	0.713	2.76	0.015
(%) in SFHA	0.16	0.012	0.132	0.013	0.028	0.117
Panel B: Early (Before 1994) vs. Late (After 1994) States						
Population (millions)	5.53	1.29	7.8	2.42	-2.274	0.397
Median Age	33.07	0.286	33	0.302	0.071	0.865
(%) White	0.842	0.026	0.808	0.027	0.034	0.374
(%) BA	0.119	0.006	0.124	0.008	-0.005	0.592
Unemployment Rate	0.061	0.004	0.06	0.004	0.001	0.89
GDP (billions)	119	29.72	191	66	-72	0.306
N Housing Units (millions)	2.25	0.527	3.12	0.917	-0.87	0.402
(%) Vacancy	0.095	0.007	0.096	0.007	-0.001	0.908
Democratic Party Vote Share	0.47	0.013	0.438	0.014	0.031	0.118
Average Flood Damage per Housing Unit	3.81	2	3.9	3.75	-0.09	0.983
Flood Size	6.17	1.01	6.55	1.34	-0.388	0.816
(%) in SFHA	0.157	0.01	0.163	0.023	-0.006	0.788

Note:
This table compares key characteristics of ever-disclosed vs. never-disclosed (Panel A) and early-disclosed vs. late-disclosed (Panel B) states. All variables are as of 1990 except for the Democratic party vote share variable, which comes from 1988 presidential election. The last two columns show mean differences and p-values.

Back to 4.1.

Table D.4: The Effect of Disclosure on Flood Damage

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Flood Size 2-5	.008	.007	.010*	.144
	(.006)	(.007)	(.006)	(.175)
Flood Size 5-10	.033***	.036***	.032***	.416***
	(.008)	(.007)	(.010)	(.069)
Flood Size 10-20	.056***	.072***	.041***	1.124***
	(.011)	(.014)	(.009)	(.100)
Flood Size 20-30	.070***	.100***	.039***	2.025***
	(.018)	(.026)	(.009)	(.286)
Flood Size 30-50	.078***	.105***	.041*	1.574***
	(.029)	(.033)	(.025)	(.358)
Disclosure × Size 2-5	.021**	.032**	.009**	−.030
	(.009)	(.016)	(.004)	(.177)
Disclosure × Size 5-10	.044***	.061***	.027***	−.087
	(.008)	(.015)	(.007)	(.149)
Disclosure × Size 10-20	.087***	.107***	.062***	−.038
	(.009)	(.017)	(.011)	(.080)
Disclosure × Size 20-30	.111***	.138***	.077***	−.295**
	(.014)	(.026)	(.012)	(.137)
Disclosure × Size 30-50	.114***	.131***	.097***	.031
	(.026)	(.030)	(.029)	(.176)
Post × Size 2-5	.019*	.027**	.010	.577***
	(.011)	(.012)	(.009)	(.210)
Post × Size 5-10	.035**	.041**	.028*	.607***
	(.014)	(.016)	(.016)	(.157)
Post × Size 10-20	.089***	.092***	.085***	.306***
	(.019)	(.017)	(.023)	(.088)
Post × Size 20-30	.114***	.119***	.106**	−.174
	(.041)	(.044)	(.054)	(.294)
Post × Size 30-50	.159***	.203***	.126**	.850***
	(.051)	(.055)	(.054)	(.194)
Post × Disclosure × Size 2-5	−.035*	−.056**	−.012	−.134
	(.021)	(.028)	(.011)	(.300)
Post × Disclosure × Size 5-10	−.042*	−.055*	−.030	.164
	(.022)	(.030)	(.018)	(.243)
Post × Disclosure × Size 10-20	−.076**	−.090*	−.057**	.051
	(.037)	(.052)	(.023)	(.210)
Post × Disclosure × Size 20-30	−.082*	−.131***	−.019	.467
	(.049)	(.043)	(.070)	(.554)
Post × Disclosure × Size 30-50	−.119*	−.173**	−.071	−.334**
	(.063)	(.078)	(.055)	(.133)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	529394	254989	274405	22319

Note: This table shows the full sets of coefficients for Table 4.1. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Table D.5: The Effect of Disclosure on Flood Damage (Placebo States)

	Prob. of Any Damage		
	(1)	(2)	(3)
Post × Disclosure (Size 2-30)	$-.015^{***}$	$-.035^{**}$	.001
	(.005)	(.015)	(.008)
Post × Disclosure (Size 30-50)	$.223^{***}$	$.246^{***}$	$.185^{***}$
	(.053)	(.083)	(.049)
Sample	All	High SFHA	Low SFHA
Year × Stack FE	X	X	X
Community × Stack FE	X	X	X
Num. obs.	31246	14984	16262

Note: This table repeats Table 4.1 using the placebo states. The dependent variables in columns (1) to (3) are the probability of having any flood damage. Column (1) is based on the entire set of communities while in columns (2) and (3), I repeat (1) using the subsample of communities with an above median SFHA ratio and below median SFHA ratio. Spatial-HAC standard errors that allow spatial correlation of up to 500 miles are estimated for inference for columns (1)–(3). $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Table D.6: The Effect of Disclosure on Flood Damage (Exc. Communities with Map Revision)

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Post × Disclosure (Size 2-5)	$-.032$	$-.052$	$-.010$	.030
	(.022)	(.032)	(.009)	(.148)
Post × Disclosure (Size 5-10)	$-.036$	$-.050$	$-.023$	.188
	(.024)	(.036)	(.018)	(.287)
Post × Disclosure (Size 10-20)	$-.062^{*}$	$-.075$	$-.045^{***}$	.163
	(.037)	(.059)	(.017)	(.259)
Post × Disclosure (Size 20-30)	$-.061$	$-.120^{***}$	.007	.396
	(.044)	(.043)	(.055)	(.658)
Post × Disclosure (Size 30-50)	$-.121^{*}$	$-.170^{**}$	$-.075$	$-.617^{***}$
	(.067)	(.073)	(.069)	(.137)
Annual Effect	-0.023*	-0.036*	-0.011	0.045
	(0.014)	(0.02)	(0.008)	(0.075)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	483852	227168	256684	18974

Note: This table repeats Table 4.1 after removing communities that have experienced map updates during the sample period. Spatial-HAC standard errors that allow spatial correlation of up to 500 miles are estimated for inference. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Table D.7: The Effect of Disclosure on Flood Damage (State Level Clustering)

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Post × Disclosure (Size 2-5)	−.035	−.056*	−.012	−.134
	(.021)	(.032)	(.010)	(.399)
Post × Disclosure (Size 5-10)	−.042*	−.055*	−.030*	.164
	(.021)	(.030)	(.016)	(.284)
Post × Disclosure (Size 10-20)	−.076*	−.090	−.057*	.051
	(.042)	(.064)	(.029)	(.268)
Post × Disclosure (Size 20-30)	−.082	−.131*	−.019	.467
	(.059)	(.074)	(.067)	(.585)
Post × Disclosure (Size 30-50)	−.119	−.173*	−.071	−.334
	(.071)	(.095)	(.057)	(.335)
Annual Effect	-0.027*	-0.039*	-0.014	-0.003
	(0.013)	(0.019)	(0.009)	(0.147)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	529394	254989	274405	22319

Note: This table repeats Table 4.1 with state level clustering. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Table D.8: The Effect of Disclosure on Flood Damage (Flood Size 2-100)

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Post × Disclosure (Size 2-5)	−.036*	−.058*	−.013	−.124
	(.021)	(.032)	(.010)	(.406)
Post × Disclosure (Size 5-10)	−.043**	−.056*	−.032**	.175
	(.020)	(.029)	(.015)	(.280)
Post × Disclosure (Size 10-20)	−.081*	−.096	−.061*	.104
	(.041)	(.064)	(.030)	(.276)
Post × Disclosure (Size 20-40)	−.093	−.138*	−.039	.220
	(.064)	(.073)	(.071)	(.499)
Post × Disclosure (Size 40-100)	−.146***	−.123	−.174***	−.516
	(.050)	(.081)	(.053)	(.337)
Annual Effect	-0.027**	-0.037**	-0.016**	-0.005
	(0.012)	(0.016)	(0.008)	(0.104)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	529198	254833	274365	23238

Note: This table repeats Table 4.1 with alternative flood bins. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Table D.9: The Effect of Disclosure on Flood Damage (Treatment Until 1998)

	Prob. of Any Damage			Log Damage
	(1)	(2)	(3)	(4)
Post × Disclosure (Size 2-5)	$-.037^{*}$	$-.058^{*}$	$-.015^{*}$	$-.103$
	(.019)	(.030)	(.008)	(.437)
Post × Disclosure (Size 5-10)	$-.043^{**}$	$-.053^{*}$	$-.034^{**}$	.203
	(.020)	(.030)	(.014)	(.316)
Post × Disclosure (Size 10-20)	$-.073^{*}$	$-.089$	$-.048^{*}$	.105
	(.041)	(.064)	(.026)	(.274)
Post × Disclosure (Size 20-40)	$-.110^{*}$	$-.164^{**}$	$-.040$	.667
	(.055)	(.071)	(.066)	(.617)
Post × Disclosure (Size 40-100)	$-.149^{**}$	$-.201^{**}$	$-.103^{*}$	$-.242$
	(.071)	(.095)	(.059)	(.396)
Annual Effect	-0.029**	-0.041**	-0.016*	0.025
	(0.013)	(0.017)	(0.009)	(0.129)
Sample	All	High SFHA	Low SFHA	Damage > 0
Year × Stack FE	X	X	X	X
Community × Stack FE	X	X	X	X
Num. obs.	501078	240953	260125	20369

Note: This table repeats Table 4.1 by using states treated after 1998 as control (i.e., not-yet-treated) states only. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Back to 4.2.

Illinois REALTORS®
RESIDENTIAL REAL PROPERTY DISCLOSURE REPORT
(765 ILCS 77/35)

NOTICE: THE PURPOSE OF THIS REPORT IS TO PROVIDE PROSPECTIVE BUYERS WITH INFORMATION ABOUT MATERIAL DEFECTS IN THE RESIDENTIAL REAL PROPERTY. THIS REPORT DOES NOT LIMIT THE PARTIES' RIGHT TO CONTRACT FOR THE SALE OF RESIDENTIAL REAL PROPERTY IN "AS IS" CONDITION. UNDER COMMON LAW, SELLERS WHO DISCLOSE MATERIAL DEFECTS MAY BE UNDER A CONTINUING OBLIGATION TO ADVISE THE PROSPECTIVE BUYERS ABOUT THE CONDITION OF THE RESIDENTIAL REAL PROPERTY EVEN AFTER THE REPORT IS DELIVERED TO THE PROSPECTIVE BUYER. COMPLETION OF THIS REPORT BY THE SELLER CREATES LEGAL OBLIGATIONS ON THE SELLER; THEREFORE SELLER MAY WISH TO CONSULT AN ATTORNEY PRIOR TO COMPLETION OF THIS REPORT.

Property Address: ______________________________

City, State & Zip Code: ______________________________

Seller's Name: ______________________________

This Report is a disclosure of certain conditions of the residential real property listed above in compliance with the Residential Real Property Disclosure Act. This information is provided as of ______________________________, 20___, and does not reflect any changes made or occurring after that date or information that becomes known to the seller after that date. The disclosures herein shall not be deemed warranties of any kind by the seller or any person representing any party in this transaction.

In this form, "am aware" means to have actual notice or actual knowledge without any specific investigation or inquiry. In this form, a "material defect" means a condition that would have a substantial adverse effect on the value of the residential real property or that would significantly impair the health or safety of future occupants of the residential real property unless the seller reasonably believes that the condition has been corrected.

The seller discloses the following information with the knowledge that even though the statements herein are not deemed to be warranties, prospective buyers may choose to rely on this information in deciding whether or not and on what terms to purchase the residential real property.

The seller represents that to the best of his or her actual knowledge, the following statements have been accurately noted as "yes" (correct), "no" (incorrect), or "not applicable" to the property being sold. If the seller indicates that the response to any statement, except number 1, is yes or not applicable, the seller shall provide an explanation, in the additional information area of this form.

	YES	NO	N/A	
1.	___	___	___	Seller has occupied the property within the last 12 months. (No explanation is needed.)
2.	___	___	___	I am aware of flooding or recurring leakage problems in the crawl space or basement.
3.	___	___	___	I am aware that the property is located in a flood plain or that I currently have flood hazard insurance on the property.
4.	___	___	___	I am aware of material defects in the basement or foundation (including cracks and bulges).
5.	___	___	___	I am aware of leaks or material defects in the roof, ceilings, or chimney.
6.	___	___	___	I am aware of material defects in the walls, windows, doors, or floors.
7.	___	___	___	I am aware of material defects in the electrical system.
8.	___	___	___	I am aware of material defects in the plumbing system (includes such things as water heater, sump pump, water treatment system, sprinkler system, and swimming pool).
9.	___	___	___	I am aware of material defects in the well or well equipment.
10.	___	___	___	I am aware of unsafe conditions in the drinking water.
11.	___	___	___	I am aware of material defects in the heating, air conditioning, or ventilating systems.
12.	___	___	___	I am aware of material defects in the fireplace or wood burning stove.
13.	___	___	___	I am aware of material defects in the septic, sanitary sewer, or other disposal system.
14.	___	___	___	I am aware of unsafe concentrations of radon on the premises.
15.	___	___	___	I am aware of unsafe concentrations of or unsafe conditions relating to asbestos on the premises.
16.	___	___	___	I am aware of unsafe concentrations of or unsafe conditions relating to lead paint, lead water pipes, lead plumbing pipes or lead in the soil on the premises.
17.	___	___	___	I am aware of mine subsidence, underground pits, settlement, sliding, upheaval, or other earth stability defects on the premises.
18.	___	___	___	I am aware of current infestations of termites or other wood boring insects.
19.	___	___	___	I am aware of a structural defect caused by previous infestations of termites or other wood boring insects.
20.	___	___	___	I am aware of underground fuel storage tanks on the property.
21.	___	___	___	I am aware of boundary or lot line disputes.
22.	___	___	___	I have received notice of violation of local, state or federal laws or regulations relating to this property, which violation has not been corrected.
23.	___	___	___	I am aware that this property has been used for the manufacture of methamphetamine as defined in Section 10 of the Methamphetamine Control and Community Protection Act.

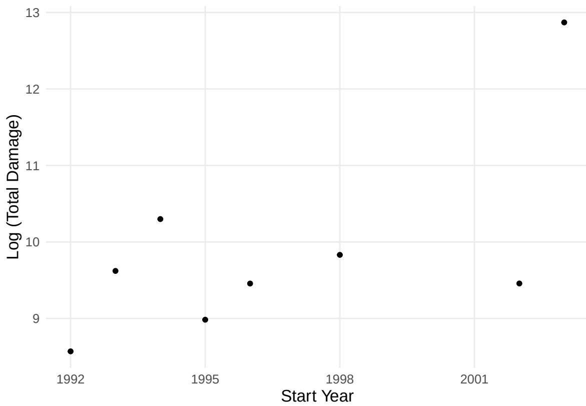
Note: These disclosures are not intended to cover the common elements of a condominium, but only the actual residential real property including limited common elements allocated to the exclusive use thereof that form an integral part of the condominium unit.

Note: These disclosures are intended to reflect the current condition of the premises and do not include previous problems, if any, that the seller reasonably believes have been corrected.

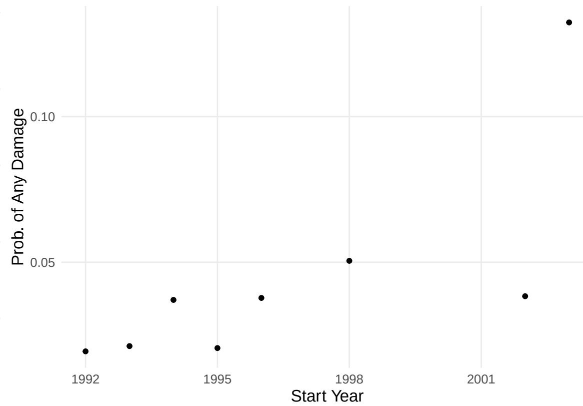
FORM 108 (05/2019) COPYRIGHT ILLINOIS REALTORS® Page 1 of 4

Figure D.1: Example of the Home Seller Disclosure Form (IL)

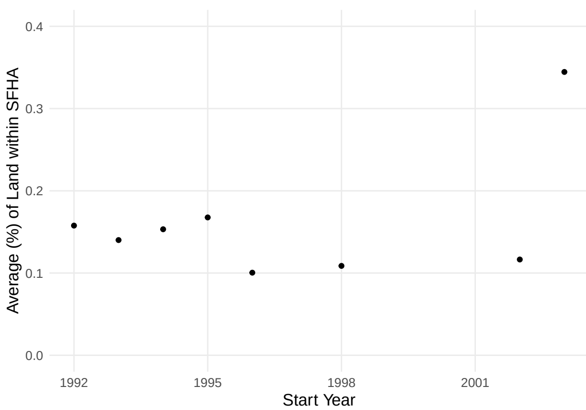
Back to 2.



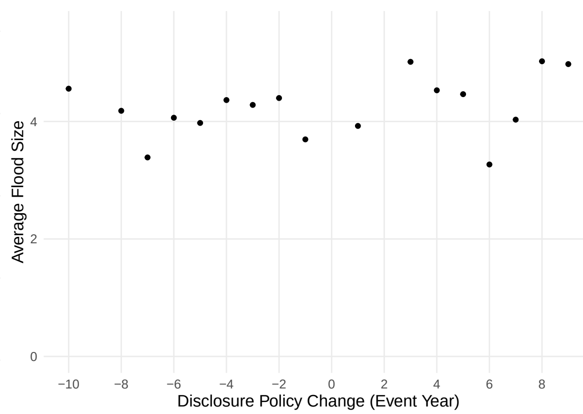
(a) Timing and Flood Damage



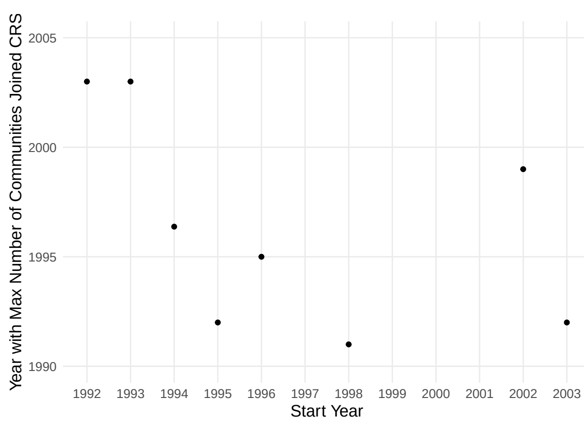
(b) Timing and the Probability of Flood Damage



(c) Timing and (%) of Land in SFHA



(d) Timing and Participation to the CRS Program



(e) Timing (in Event) and Flood Size

Figure D.2: Correlation Between Disclosure Timing and Flood Profiles. These figures plot the disclosure policy timing against (a) past flood damage, (b) past flood damage probability, (c) ex-ante flood risk profile, and (d) flood policy (timing of participation to the Community Ratings System). Panel (e) plots the average flood size in event time. Values on the y-axis is pooled across all states with the same treatment or event year.

Back to 2.

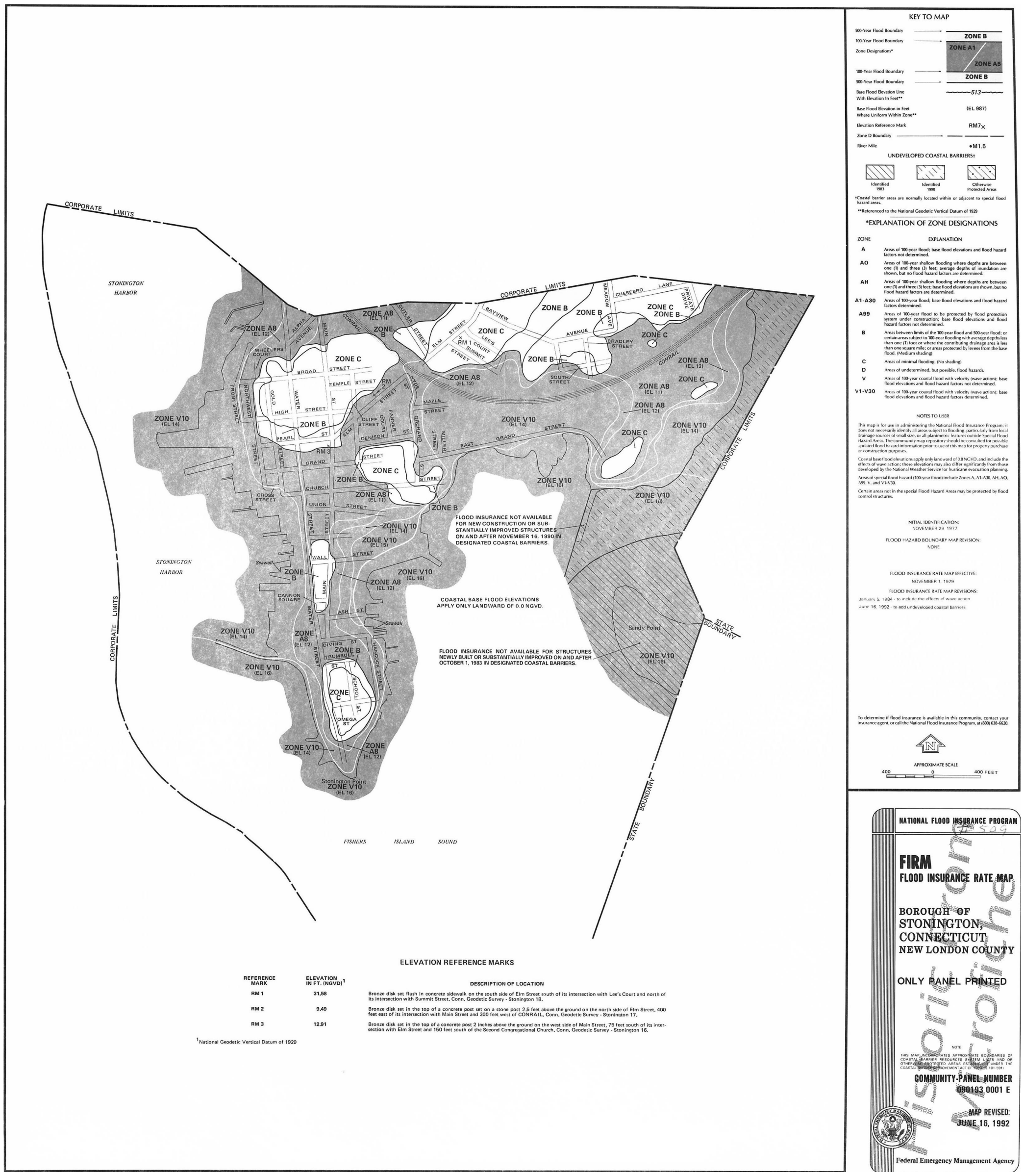


Figure D.3: Sample Flood Insurance Rate Map (Borough of Stonington, CT)

Back to 2.

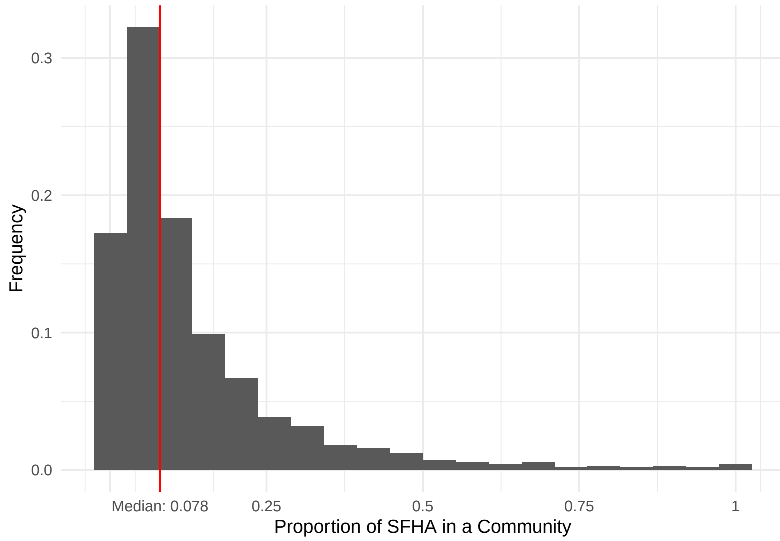
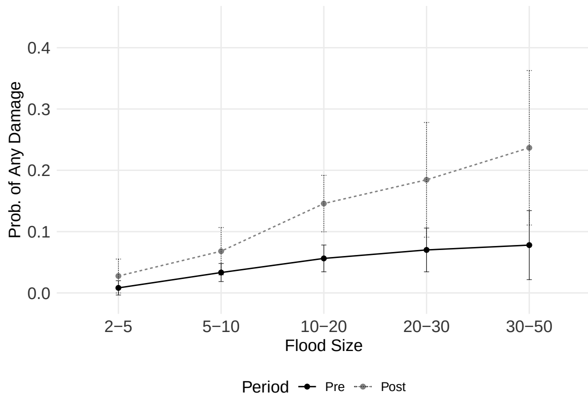
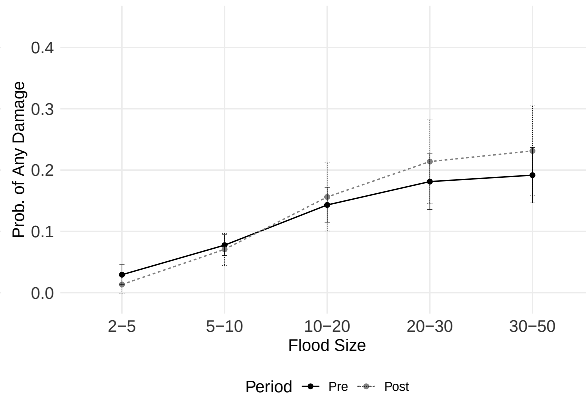


Figure D.4: Histogram of the Proportion of the SFHA at the Community Level. The plot shows the distribution of the SFHA ratio for the 7,821 communities that are on the Q3 map (first generation of digitized flood map) and in the 26 ever-disclosed states.

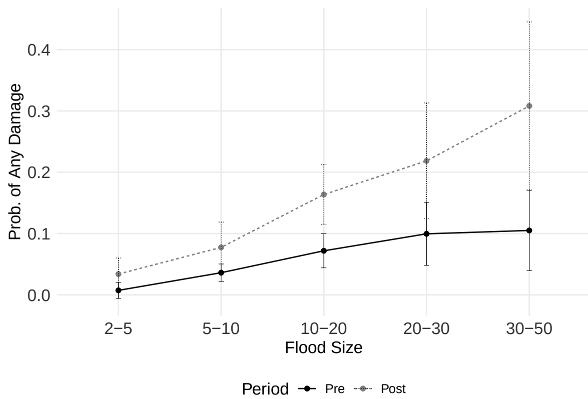
Back to 3.



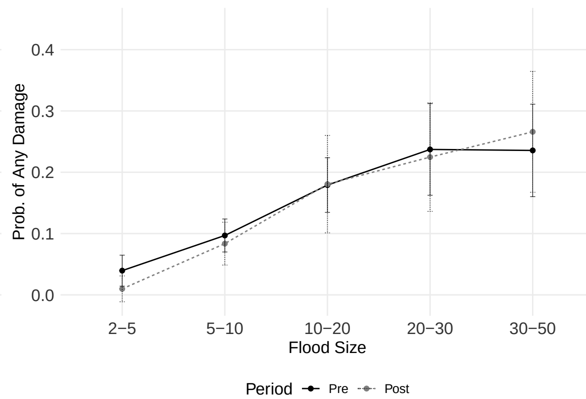
(a) Control Group (All Communities)



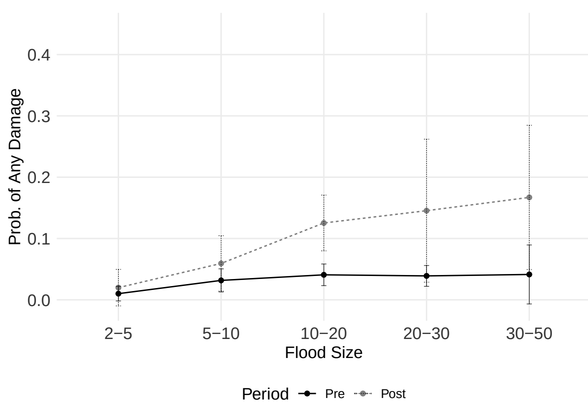
(b) Treatment Group (All Communities)



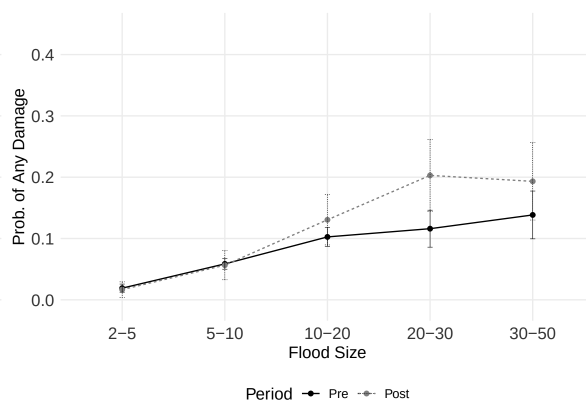
(c) Control Group (High Risk Communities)



(d) Treatment Group (High Risk Communities)



(e) Control Group (Low Risk Communities)



(f) Treatment Group (Low Risk Communities)

Figure D.5: The Effect of Disclosure on the Damage Function with 95% Confidence Intervals. These plots reproduce Figure 4.1 with corresponding confidence intervals.

Back to 4.2.

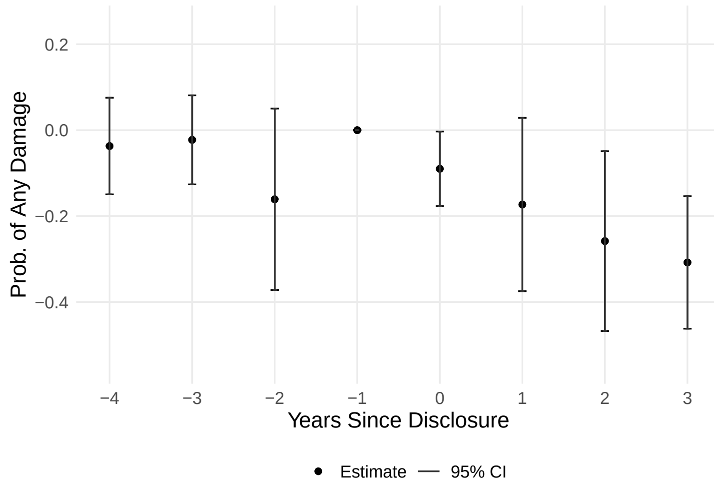


Figure D.6: The Effect of Disclosure on the Damage in Event Time. This figure depicts $\hat{\beta_{4,t}^{30-50}}$ for flood size of 30-50 in event time t where the dependent variable is probability of having any damage. Endpoint restrictions are imposed at event time -5 and 4. The error bar represents the 95% confidence interval.

Back to 4.2.