Flood Risk and Salience:
New Evidence from the Sunshine State

Laura A. Bakkensen* Xiaozhou Ding† Lala Ma‡

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Abstract

A growing literature finds evidence that flood risk salience varies over time, spiking directly following a flood and then falling off individuals' cognitive radar in the following years. In this paper, we provide new evidence of salience exploiting a hurricane cluster impacting Florida that was preceded and followed by periods of unusual calm. Utilizing residential property sales across the state from 2002 through 2012, our main estimate finds a salience impact of -8%, on average. The salience effect persists when we base estimation only on spatial variation in prices to limit confounding from other simultaneous changes due to shifting hedonic equilibria over time. These effects range from housing prices decreases of 5.4% to 12.3% depending on the year of sale. Understanding flood risk salience has important implications for flood insurance and disaster policy, the benefits transfer literature, and, more broadly, our understanding of natural disaster resilience.

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*Bakkensen: University of Arizona, School of Government & Public Policy, 315 Social Science Building, Tucson, AZ 85721, laurabakkensen@email.arizona.edu.

†Ding: University of Kentucky, Department of Economics, Business & Economics Building, Lexington, KY 40506, xiaozhou.ding@uky.edu.

‡Ma: University of Kentucky, Department of Economics, Business & Economics Building, Lexington, KY 40506, lala.ma@uky.edu.

1 Motivation

A significant and broad literature has assessed the impact of flooding on home prices. In the spirit of Rosen (1974), the aim is to identify willingness to pay to avoid flood risk through the capitalization of underlying flood risk on home prices using property sales data. All else equal, homes at higher risk of flooding should be priced lower to reflect the underlying environmental threat. Existing literature has estimated the impact in a variety of geographic contexts. United States based research often defines flood risk as high if a property is located within the National Flood Insurance Program's Special Flood Hazard Areas (SFHAs), reflecting an average flood risk of at least 1 in 100 per year. Empirical prices for homes within the SFHA are typically lower.[1] A meta-analysis by Daniel, Florax, and Rietveld (2009) finds an overall negative but small price impact.

As long as homebuyers are fully attentive to the underlying property-specific flood risks associated with the homes that they purchase, home price differentials across flood zones return the marginal willingness to pay (MWTP) to avoid flood risk. The estimated MWTP can then be applied in cost-benefit analyses of many policies and projects relating to public flood mitigation. However, a recent stream of literature asserts that flood risk might not be salient. In empirical work, the term *salience* broadly encompasses several channels through which individuals are understanding and updating their beliefs surrounding flood risk probability.

First, it is possible that individuals, due to the cognitive complexities and costs of the home buying process, may be rationally inattentive to flood risk when making their purchasing decision (Sallee, 2014; Matějka and McKay, 2015). Second, individuals may be *ir*rationally

[1] For example, Harrison, T. Smersh, and Schwartz (2001) find a 5% price reduction in flood-prone homes in Alachua County, Florida. Bin et al. (2008) find that coastal flood zone homes are 11% lower in price, relative to lower flood risk homes in coastal New Hanover County, NC. Assessing inland flood risk, Posey and Rogers (2010) find a 8.6% price premium for low flood risk homes in St. Louis County, Missouri . In addition, Zhang (2016) finds flood-prone homes sell for 5.9% less, on average, in the Fargo, ND-Moorhead, MN metro area. However, some literature finds a positive price premium for high flood risk, especially in coastal areas including Bin and Kruse (2006) in Carteret Country, NC and Atreya and Czajkowski (2016) in Galveston, TX, even after controlling for water-related amenities. Some have argued that the price premium may still reflect the difficulty in controlling for the amenity value of proximity to water (Bin et al., 2008).

inattentive (Reis, 2006). Lastly, individuals' perception of flood risk may be different from the true flood risk of their location. This may change over time as events such as natural disasters cause the individual to update their beliefs. For example, in a 2017 door-to-door survey, Bakkensen and Barrage (2017) find that 70% of respondents in a coastal Rhode Island survey underestimate the flood risk of their specific properties. This flood risk probability channel is consistent with existing literature (Bin and Polasky, 2004; Bin and Landry, 2013; Gallagher, 2014). While the specific channel through which salience operates is interesting in its own right, we are unable to separately identify these channels in our data. For example, a change in attention to flood risk (reflected through housing prices) is consistent with an upward revision of an individual's (subjective) belief surrounding flood risk probability, and/or a reduction in the cost to gather information, leading to potential reductions in levels of rational or irrational inattention to flood risk. In this paper we thus employ a broad definition of salience that encompasses housing market responses that arise through any of the above channels.

Regardless of the exact mechanism through which salience operates, the lack of flood risk salience is one plausible explanation for why the literature has, in some cases, found relatively small price differentials in flood-prone versus non-flood-prone homes.[2] Exploring more, researchers have exploited randomly occurring, significant flood events, typically through a difference-in-differences approach, and have found that recent flood events can trigger attention, causing flood risk salience to vary over time.[3] In order to avoid conflating price drops due to flood related damages with flood salience, approaches often analyze prices of *near-miss* homes that were not directly inundated. Bin and Polasky (2004) find a price drop of about 8.3% for homes in Pitt Country, NC following Hurricane Floyd. Hallstrom and Smith (2005) use Hurricane Andrew to estimate the impact in Lee County, FL, a *near-miss*

[2]We define "small" as relative to the difference in expected flood losses that would be rationally capitalized into home price by an attentive buyer. An additional explanation for small housing price differentials across flood risk areas is that flood insurance premiums are below actuarially fair rates and the full value to avoid flood risk is not reflected in housing prices.

[3]Again, this change in flood risk salience is consistent with both changes in subjective flood risk belief and/or factors that impact rational or irrational inattention to flood risk exposure.

location. They find that properties in flood zones experienced a 19% decline in price following the event relative to non-flood zone properties in the same *near-miss* county. Kousky (2010), utilizing a repeated-sales approach, finds values of properties located near rivers fell by 6-10% after a significant flood event in St. Louis County, MO. While the salience effect of recent events is strong, it appears impermanent. Atreya, Ferreira, and Kriesel (2013) find that prices fell significantly but only temporarily following a significant flood event in Dougherty County, GA in 1994: the flood risk discount for 100-year floodplain properties vanishes 4 to 6 years after the flood. Lastly, Bin and Landry (2013) identify cumulative price drops of between 6% and 20.2% following Hurricanes Fran and Floyd in Pitt Country, NC but diminishing to zero after 5 to 6 years.

In this paper, we estimate flood risk salience using a property value hedonic approach. We accomplish this by comparing the change in sales prices of houses in high risk floodplains before and after a period of major hurricanes, and corresponding flood events, in Florida to any price change experienced by houses in low risk floodplains.[4] To isolate the impact of an information change separately from damages incurred as a result of the floods, we follow previous literature in focusing the analysis on a subset of *near-miss* houses, which belong to areas that were *near but not impacted* by these disasters. If homeowners in low risk areas experience smaller information updates relative to those in high risk areas, then our difference-in-differences (DD) estimate recovers (a lower bound for) flood risk salience. To further control for the impact of unobserved house-price characteristics that could send houses in high and low risk flood zones on different price trajectories, we employ a difference-in-difference-in-differences (DDD) design by comparing the DD estimate for our group of *near-miss* sales to one based on sales in areas that are far from the impacted areas, a group that we refer to as *never-hit*. Lastly, we assess the robustness of our salience estimates using a difference-in-differences estimator based only on *spatial* variation in housing prices. This spatial difference-in-differences estimator recovers a salience estimate for each year after the

[4] We use low risk floodplains since no location can technically be at zero risk of flooding. See, for example, FEMA: https://www.fema.gov/national-flood-insurance-program.

hurricane event using post-event data only and compares the differences in prices between the A and X flood zones in *near-miss* counties with the same differences in the *never-hit* group. By relying on spatial variation only, we are able to recover a salience estimate that is robust to time-varying hedonic price functions (Kuminoff and Pope, 2014).[5]

We contribute to the existing literature in three ways. First, our salience estimate is based on the entire sample of residential property sales in Florida from 2002 through 2012. This provides new estimates for a large geographic area that can be compared to estimates from other areas within Florida and throughout the US. Second, our triple-differences design builds upon the previous literature's use of *near-miss* events in a DD framework by exploiting, as an additional control group, houses in *never-hit* counties, defined as those in areas that were adjacent to *near-miss* locations but not impacted by flooding. Third, our spatial DD estimator addresses the concern highlighted by Kuminoff and Pope (2014) surrounding the capitalization of shocks to public goods (or bads) *over time* in a hedonic approach. Namely, the exogenous shocks may alter the underlying hedonic equilibrium and lead to a divergence between price capitalization and underlying MWTP. In the context of flood risk, it is possible that the flood event, itself, may change the makeup of the buyers and sellers in the market just before versus after an event. Capitalization of the event will only represent MWTP if the hedonic equilibrium does not change over time.[6] If heterogeneity across individual MWTP exists in the market, the housing price capitalization of the event for *near-miss* areas may incorporate both changes to flood risk salience as well as changes in the mix of homebuyers (and their preferences). Existing empirical evidence suggests that this assumption may not hold as some have found heterogeneity in mobility and migration across both race and income following intense disasters (Smith et al., 2006, Landry et al., 2007, Groen and Polivka, 2010,

[5]Our identification strategy isolates the impact of the Florida hurricane landfalls in 2004 and 2005. To the extent that information about other prominent hurricanes during this period, such as Hurricane Katrina in 2005, is disseminated by the national media across Florida, this would be differenced out by our empirical design. Thus, we cannot identify the impact of other hurricane events during this period but our results will not be confounded by other events assuming that the information on these out-of-state events is evenly transmitted across Florida.

[6]See Kuminoff and Pope (2014) for the conditions under which the hedonic equilibrium does not change.

Strobl, 2011, Deryugina, Kawano, and Levitt, 2014).

We find salience impacts ranging from -3.2% to -4.4% using a DD strategy that compares housing prices before and after flood events. Our main DDD specification finds larger impacts of up to -8% overall, and up to -14.3% when allowing the estimates to vary by year. In addition, our spatial DD estimates are generally larger than the pre- and post- DD estimates and comparable to our triple-differences estimates, providing evidence that the pre- versus post- DD framework may suffer from the Kuminoff and Pope (2014) critique. We perform placebo checks that randomize treatment exposure in both geographic and temporal dimensions, and confirm that our findings are causal. Taken together, our results using various quasi-experimental approaches, including one that is robust to shifting hedonic equilibria, all support the finding of a *salience* effect with respect to flood risk in Florida.

We highlight some limitations of work. Evident in our previous description of salience, salience impacts may operate through many channels. In this paper, we do not (and cannot) distinguish between these various channels. In addition, it remains an open question as to how the magnitude of post-disaster flood risk salience relates to rational risk perception. For example, salience as a result of a flood event may lead homebuyers to rationally perceive the true underlying flood risk if flood risk was previously underestimated; on the other hand, risk can be overestimated due to over-reaction. While understanding this and the potential contributions of each channel is important, we leave formal analysis of this for future work.

The rest of the paper proceeds as follows. Section 2 lays out our basic theoretical and empirical models. In section 3, we first discuss our data sources. Next, we provide summary statistics and figures that assess the validity of our identifying assumptions. Section 4 presents our results and section 5 concludes.

2 Model

In his seminal paper, Rosen (1974) provides the theoretical link to estimate consumer's Marginal Willingness to Pay (MWTP) for (dis)amenities through the implicit prices recovered from regressing housing prices on house and neighborhood characteristics. Suppose a house is characterized by a bundle of attributes Z, where the price of the house is $P(Z)$. Given prices, a consumer with income Y chooses how to allocate her income between purchasing a house of given characteristics and other consumption x in order to maximize her utility. The consumer's problem is characterized by the following

$$\max_{Z,x} U(Z,x) \quad \text{subject to} \quad Y = x + P(Z) \tag{2.1}$$

where prices and income are normalized to the price of the numeraire good x. Substituting the budget constraint into the utility function and then differentiating, the first order condition with respect to one of the characteristics of interest, e.g. $z_1 \in Z$, is given by

$$\frac{\partial P(Z)}{\partial z_1} = \frac{\partial U/\partial z_1}{\partial U/\partial x} \tag{2.2}$$

The first order condition in equation (2.2) shows that the slope of the hedonic price function with respect to characteristic z_1 is equal to the consumer's willingness to trade off additional units of that characteristic with all other consumption (i.e., her marginal rate of substitution). Embedded in this framework is the assumption that households are perfectly informed of the characteristics of a given house.[7] If information is imperfect, however, the estimated implicit prices of housing characteristics may recover a biased estimate of marginal willingness to pay. That consumers do not have perfect information over the attributes that they care about suggests that the attribute may not be salient. Moreover, the level of attribute salience may vary over time, with attention on dis-amenities, such as pollution or natural disaster risk,

[7]Two other crucial assumptions are that households face no price discrimination or moving costs.

peaking following an information shock such as a toxic spill or hurricane. In this case, if one were to estimate MWTP before an information shock and then compare it to that estimated after the information shock, then the difference in estimated marginal willingnesses to pay would be attributed to a change in salience assuming that all else is held constant.

We apply this framework to our context of flood risk. Let $zoneA$ be an indicator equal to 1 if a house is located in a "high risk" floodplain, with at least a 1 in 100 probability of inundation in a given year (i.e. Zone A), and 0 if it is located in a "low risk" floodplain with an annual risk of flooding less than 1 in 100 but greater than 1 in 500 (i.e. Zone X). We separately distinguish this flood risk variable from all other characteristics, Z, that describe the house. Following the literature, we assume the hedonic price function $P(\cdot)$ is log-linear in its characteristics,

$$\ln P_j = \beta_0 + \beta_1 zoneA_j + Z_j'\gamma + \nu_j \tag{2.3}$$

The term ν_j represents all other characteristics of the house that impact its price but are not observed by the researcher.

The implicit price of locating in the "high risk" floodplain (relative to the low risk floodplain) is measured by β_1. The parameter, assuming perfect information, can be interpreted as the MWTP to avoid flood risk areas according to hedonic theory.[8] Note that since Zone X is an area with low and not zero flood risk, we are more precisely estimating the MWTP to avoid high versus low flood risk areas. From a hydrological perspective, no locations are considered at zero risk of flood as localized intense downpours could potentially occur.[9] If one were instead able to make comparisons to an area with zero flood risk, the estimated MWTP to avoid high risk flood plains would be larger. In this way, the recovered MWTP to avoid flood risk using Zone X houses as a control group is likely to be an underestimate. Moreover, any use of Zone X houses as a comparison group for Zone A houses will also have

[8]Given that flood risk is a disamenity, one would expect $\beta_1 < 0$.

[9]FEMA's official stance is that "no home is completely safe from potential flooding devastation" (https://www.fema.gov/national-flood-insurance-program) and designates zones as low risk but not as no risk. This is also true in other nations (e.g., Duží et al. (2017)).

implications for our salience estimates, which we later discuss.

Now suppose one focused on the zone A houses and measures the discount required to live in this 100-yr floodplain both before and after a large flood event. Within the timeframe examined in this article, changes in the underlying flood risk can be approximated as zero.[10] Assuming that the true flood risk remains unchanged across time, the difference in the estimated discounts could be attributed to a change in the saliency of the flood risk due to the event.[11] The large flood events we exploit occur during the mid-2000's and housing transactions data span the time period from 2002 to 2012.[12] With the housing bubble and Great Recession that followed in 2009, this was a tumultuous time for the housing market. As such, a naive comparison of housing prices before and after our flood events may capture other unobserved changes in the housing market that occurred over the same time period. For example, if the time period after the hurricane event coincided with one of depressed housing prices due to the housing market crash, then one would over-attribute the price drop to salience as it would include the fall in prices that would have occurred in absence of the hurricane event as a result of the recession.

In general, the problem of omitted variables is of first order concern in many property value hedonic analyses. To control for unobserved factors that are both time-invariant and varying, we follow previous work by employing a difference-in-differences (DD) framework that compares the changes in sales price experienced by the original houses of interest (in Zone A) with prices changes of a control group of houses sold over the same period. Specifically, we look to price changes experienced by houses in the low-risk floodplain, Zone X, to proxy for what would have happened to high-risk flood plain houses in absence of the flood event as a way to identify salience impacts while controlling for unobserved, correlated time trends. The

[10]This is unlikely true in the long run in light of climate change.

[11]To estimate the saliency effect in the context of natural disasters, researchers have used exogenous disaster shocks such as hurricanes to induce changes in information that alter risk valuation in order to capture salience. More generally, additional work has measured salience due to policies or programs that impact information.

[12]We describe our data in more detail in section 3.

following gives the regression specification that indexes each house j with the time of sale, t,

$$\ln P_{j,t} = \beta_0 + \beta_1 zoneA_j + \beta_2 Post_t + \beta_3 zoneA_j \times Post_t + Z'_{j,t}\gamma + \theta_t + \theta_j + \nu_{j,t} \tag{2.4}$$

where $Post_t$ is an indicator that is equal to 1 if a house is sold after the event, and 0 otherwise. The DD estimate β_3 returns the change in prices of houses in the high-risk floodplain after the flood event, netting out the price changes experienced by houses in the low-risk floodplain. One can use the potential outcomes framework by Rubin (1974) to show that this relationship is causal as long as the changes in sales prices for those in the low-risk floodplain (i.e. the control group) represent what would have happened to prices of houses in the high-risk floodplain, had the event not occurred. We provide evidence in the subsequent section of the validity of this assumption. In addition, the specification includes year fixed effects θ_t and spatial fixed effects θ_j at various geographic levels from region to census tract to respectively control for time trends and unobserved, time-invariant neighborhood characteristics.

Lastly, given the destructive nature of natural disasters, these types of events can lead to direct damages as well as increased salience, both of which would negatively impact house price. Thus, a common strategy to isolate the impact of saliency is to focus on areas that were near, but not directly impacted by, the natural disaster, i.e. *near-miss* areas. We follow this strategy to identify salience by focusing on a sample of *near-miss* houses, defined as those in counties that are adjacent to counties that were directly damaged by the event. Consistency issues aside, β_3 identifies a salience effect under the assumption that households in the high-risk floodplain internalize additional information from flooding in neighboring counties, while those in the low-risk floodplain do not. This is perhaps a strong assumption as houses in the X zone abutting an A-X boundary may feel similarly threatened at the onset of a flood event nearby. In the extreme case, if all houses in the X zones of near-miss counties experienced a similar change in flood risk salience, our estimate of salience that uses X zone houses as a control group would return a salience estimate that is close to 0. In practice, the spillover effect is likely to be somewhere in between, and our estimate is likely to be an under-estimate of the true salience impact. In robustness checks, we empirically assess the magnitude of this spillover effect.

While the DD estimate from equation (2.4) can control for many time-varying, unobserved factors, concern may still arise if zone-specific impacts cause the price trajectories of houses in zones X and A to diverge in response to the flooding event. An example would be if the flood event propagated local flood mitigation efforts, where efforts were focused on areas that are considered to be high risk (i.e. the A-zone areas). In this case, our DD estimate based on the price difference between sales in A and X zones over time would capture additional differences due to these mitigation efforts.

To deal with this, we include a third source of variation. Specifically, we use houses in non-adjacent counties (that were also not in directly impacted counties) as an additional control group, where we will refer to these houses as being in the *never-hit* group. If an unobserved, zone-specific effect were triggered as a result of the flood event, the relative A-X price difference of this *never-hit* group would capture such changes. We implement this using a difference-in-differences-in-differences (DDD), or a triple-differences approach. The regression specification for the DDD approach is the following,

$$\begin{aligned}\ln P_{j,t} =& \beta_0 + \beta_1 zoneA_j + \beta_2 Post_t + \beta_3 \cdot zoneA_j \times Post_t \\ &+ \beta_4 NearMiss_j + \beta_5 \cdot NearMiss_j \times Post_t + \beta_6 \cdot zoneA_j \times NearMiss_j \\ &+ \pi \cdot zoneA_j \times NearMiss_j \times Post_t + Z'_{j,t}\gamma + \theta_t + \theta_j + \nu_{j,t}\end{aligned} \tag{2.5}$$

where $NearMiss_j$ is an indicator variable that equals to 1 if the house belongs to a county that was adjacent to one that sustained large flood-related damages, and 0 otherwise. The parameter π returns the DDD estimate that compares the DD estimate for houses in the *near-miss* group to that from the *never-hit* group.

The clear limitation of this strategy using a group of home sales that is one county removed from counties that are directly impacted is that this group may also experience a change in flood risk information that affects the salience of flood risk. In other words, as in the case with the X and A zone comparison, there may be spillovers that affect the prices of sales that we considered to be in a control group. If households in *never-hit* counties similarly revise their risk perceptions upward as a result of the flood event (i.e. perceive areas to be riskier), then our DDD estimator would underestimate salience from differencing out the negative impact that salience would have on home prices. On the other hand, the impact could be overestimated if the event causes households in *never-hit* areas to revise risk perceptions downward. This increases housing prices, holding all else constant, and causes our DDD estimate to overstate the salience impact upon removing the positive price impact. While we cannot rule out a downward revision of flood risk, the majority of the literature finds that individuals underestimate flood risk and revise estimates upward after an event. As such, we think that our salience estimates using DDD are more likely to suffer from being underestimated. In section 4, we assess the amount by which the DDD estimate underestimates salience using alternative control groups that are further removed from impacted areas and are less likely to receive an information treatment.

We lastly highlight an additional concern. Similar to strategies that use fixed effects, consistent estimation through difference-in-differences or triple-differences estimation often involves a change in the amenity of interest *over time*. While amenity variation over time within a location can help control for time-invariant unobserved factors, price changes over time potentially mixes information from different hedonic equilibria, which causes a wedge between MWTP and the simple change in price (over time) given a change in an amenity of interest. That is, letting superscripts index time and assuming z_1 is our variable of interest,

$$\frac{\partial U / \partial z_1}{\partial U / \partial x} \neq \frac{P^1(z_1^1, Z) - P^0(z_1^0, Z)}{z_1^1 - z_1^0}$$

The expression to the right in the equation above is formally known as a "capitalization effect." Kuminoff and Pope (2014) demonstrates that MWTP and capitalization are only equal under certain conditions that ensure the equilibrium hedonic price function remains unchanged even as the amenity of interest changes over time. Intuitively, the hedonic equilibrium is formed out of the interactions of buyers and sellers within the housing market. Given a widespread, exogenous change in the amenity (e.g. from a natural disaster or a policy), households are likely to re-optimize over time by moving, thereby potentially altering the underlying hedonic price function. In our flood context, given the growing literature on post-disaster migration discussed in section 1, residents in an area before a natural disaster may be different than those who choose to live in the area afterwards. If the amenity change causes a new post-disaster population (with different preferences for flood risk), the hedonic equilibrium may vary over time, thereby confounding changes in flood salience with changes in underling market participant preferences.[13]

To avoid the assumption of time-invariant hedonic gradients, we additionally estimate salience following recent empirical applications by using only spatial variation in the amenity of interest in a spatial difference-in-differences strategy (Kuminoff and Pope, 2014; Muehlenbachs, Spiller, and Timmins, 2016; Haninger, Ma, and Timmins, 2017). Focusing only on sales that occur after the hurricane event, we alter the traditional DD specification to compare the differences in prices between the A and X flood zones in *near-miss* counties with the same differences in our *never-hit* group, composed of adjacent counties that are even farther away from impacted counties. We then estimate this impact for each year after the event of interest

[13] In general, sorting can also occur across the amount of perceived risk if people with higher MWTP are systematically more likely to have higher risk perceptions. While we cannot control for this type of sorting across risk perception, we note this as further motivation that the assumption of a time-constant hedonic price function may be violated.

using the following specification,

$$\ln P_{j,t} = \beta_{0,t} + \beta_{1,t} \cdot zoneA_j + \beta_{2,t} \cdot NearMiss_j \qquad (2.6)$$
$$+ \beta_{3,t} \cdot zoneA_j \times NearMiss_j + Z'_{j,t}\gamma_t + \theta_t + \theta_j + \nu_{j,t}$$

The spatial DD necessarily means relying on comparisons of different geographic areas for both dimensions of the difference-in-differences framework. The success of this strategy depends on whether hedonic price functions are comparable across space after various spatial controls are included. In the case of this paper, one might ask whether *near-miss* areas are comparable to *never-hit* areas. In the next section, we assess the spatial analog of the parallel trends assumption to check whether price functions in different geographic locations after controls trend in a similar manner. Ultimately, allowing the salience parameters to vary by year avoids assuming that the hedonic price function is constant over time. We thus additionally provide empirical support in the following section that the spatial DD would be better able to deal with concerns related to shifting hedonic gradients than one that relies on temporal variation in prices.

3 Data and Empirical Evidence

In the following section, we first provide an overview of our main data sources. We then present evidence from summary statistics that assess the extent to which omitted variables and post-disaster migration might impact our estimates. We lastly provide graphical evidence to support our identifying assumptions.

Data Sources

Housing Data Housing transactions data come from Dataquick, Inc. and provide the universe of housing sales in Florida between 2002 and 2012. For each property, the data include a property's location and its physical characteristics (e.g. the number of bathrooms and bedrooms) as well as information related to each of its transactions, including the sale date and price. Of the 67 counties in Florida, we lose 13 counties because Dataquick does not collect data for some rural counties, and another 3 counties because no digitized flood insurance maps are available.[14] With the remaining available counties, we clean the data in several steps. First, we limit our analysis to arm's length transactions of single-family residential houses that are owner-occupied and remove those of properties that are missing information on price, number of bathrooms and bedrooms, lot size, or square feet.[15] We calculate house age by subtracting the year a house was built from the year of sale. We drop houses for which age is negative (2.78% of total sales transactions) as these are likely to reflect land sales and the recorded attributes would likely be inaccurate. House prices are deflated to January 2010 dollars using the Bureau of Labor Statistics Price Index for Housing in the Urban South. We drop additional outliers by removing houses with prices below or above the 1st and 99th percentile of the empirical price distribution, respectively.

Neighborhood and Spatial Attributes We augment the housing data by attaching neighborhood (dis)amenities to each house, including crime, industrial activity, and other spatial characteristics, from various other sources. First, we include neighborhood crime statistics through county-by-year arrest rates from the Florida Department of Law Enforcement. Next, we calculate an inverse-distance weighted average of onsite releases from all Toxic Release Inventory (TRI) facilities within 3km of each house in the year of its sale to control for industrial activity in the surrounding area. We additionally map each house to nearby spatial amenities using Geographic Information System (GIS) software and shapefiles obtained from the Yale University Map Department. This allows us to retain the distances

[14] The missing counties are: Baker, Collier, Dixie, Holmes, Lafayette, Leon, Levy, Putnam, Seminole, Sumter, Suwannee, Taylor, and Union, Highlands, Sarasota, and Palm Beach.

[15] We also drop any sale records for which the number of bathrooms exceeds twenty or the number of bedrooms exceeds thirty to omit outliers.

between each house and the nearest airport, railroad, park, and coast.[16]

Flood Risk Data One of the most significant policy responses to flood risk in the United States was the creation of the National Flood Insurance Program (NFIP) in 1968 (Howard et al., 2016). The program aimed at providing affordable flood insurance coverage to the nation's public. One programmatic outcome was the creation of flood risk maps, called Flood Insurance Rate Maps (FIRMs), which spatially differentiated almost all land across the United States by underlying flood risk. Specifically, locations at high risk of inland floods, known as Zone A, exhibit an annual flood risk of at least 1 in 100.[17] Low flood risk zones include Zone X, with an annual risk of inundation less than 1 in 100. Utilizing digitized FIRMs across the state of Florida, we lastly match all properties to their NFIP-designated flood zones using GIS, dropping all properties in the high risk coastal V zones to focus only on inland flood risk across the high risk A zones and low risk X zones.[18]

Hurricane Events Data While enjoying its reputation as the sunshine state, Florida is also at high risk for intense hurricane landfalls given that a majority of the state's landmass is a peninsula between the hurricane-active North Atlantic Ocean and the Gulf of Mexico. Over the past 150 years, 40 percent of hurricanes in these basins have impacted Florida (NOAA, 2016), exposing the state to hurricane losses from intense wind and rain. NFIP data obtained through a Freedom of Information Act request provides detailed information on each flood-related event such as number of claims, month and year of the event, and number of policies at the county level. We recover flood-related hurricane events using these data. Despite the high hurricane frequency, Florida has enjoyed periods of relative calm. From

[16]The main results of the paper rely on a specification that uses census tract fixed effects. Since we are only able to capture a time-invariant measure of proximity to these (dis)amenities, we do not expect that inclusion of these distance measures would greatly alter our main results.

[17]We define Zone A here to include Zone A, Zone AO, Zone AH, Zones A1-A30, Zone AE, Zone A99, Zone AR, Zone AR/AE, Zone AR/AO, Zone AR/A1-A30, Zone AR/A.

[18]Flood insurance purchase is mandatory for properties in the SFHA with a federally backed mortgages. Still, flood risk salience may be low during the home buying process since insurance uptake is generally low and there is much cognitive complexity involved in the home buying process.

2002 to 2012, Florida received hurricane landfalls only during 2004 and 2005. The hurricanes did, however, lead to massive damage in Florida during these two years. We define our event *period* as the Florida hurricane cluster that occurred from Hurricane Charley in August 2004 until Hurricane Wilma in October 2005. During this period, seven hurricanes and tropical storms impacted Florida and led to more than $1.8 billion (real 2005 $USD) in insured flood losses.[19] Figure 8 displays the seven storm tracks. To give a sense of the unusual magnitude of this cluster, the 2005 season was found by Nordhaus (2010) to be a quadruple outlier for hurricane activity in the North Atlantic Ocean. Following 2005, Florida enjoyed an 11-year hurricane "drought" after hurricane Wilma made landfall on October 24, 2005.[20]

Defining Treatment The final sample of housing data matched to various flood risk and neighborhood attributes consists of 778,855 sales records. To geographically assess the impact of exogenous storm shocks, we lastly collect county-event level data from the National Flood Insurance Program on the number of claims, total policies in force, and confirmed payouts for property losses.[21] Relevant for flood salience, NFIP policies only cover water damage and not wind losses. In the spirit of Hallstrom and Smith (2005), and to avoid conflating flood risk salience with direct damages from the event, we drop all counties that received at least 500 insurance claims in total across the seven hurricane period, designating these counties as being directly hit by any of the hurricanes.[22] We therefore only assess counties that were not directly hit by the hurricanes. This includes *near-miss* counties, defined as counties that geographically border a county that was directly hit, and those that were *never-hit*, defined as counties that were neither hit nor nearly missed. Of our final housing

[19]The seven storms were Charlie (August 2004), Frances (September 2004), Ivan (September 2004), Jeanne (September 2004), Dennis (July 2005), Katrina (August 2005), and Wilma (October 2005).

[20]Three tropical cyclone events led to some losses during the "drought". Two hurricanes - Alberto and Ike - did not make direct landfall and led to a less than $3 million in insured losses across the state in total. Tropical Storm Fay impacted Florida in August 2008 but only led to $43.6 million in insured losses across the state and never reached hurricane strength.

[21]The data were provided through a Freedom of Information Act request. The county-level data are matched to each hurricane event in the NFIP data.

[22]In additional sensitivity analysis, we also define a hit as having at least 250 or 750 claims, or having at least $5 million in flood loss payouts.

data, 417,360 transactions are located in *near-miss* or *never-hit* areas. Figure 8 provides a map of *near-miss* and *never-hit* counties using the 500 claims definition. Combining the *near-miss* categorization with house FIRM information, we follow Hallstrom and Smith (2005) and assume that only houses in high risk flood zone A of *near-miss* counties should have a price impact due to flood risk salience. As such, we define properties in zone A (with at least a 1 in 100 annual risk of inland flooding) as our *treatment* group of houses and those in the X zone (with less than a 1 in 100 annual risk of inundation) as our *control* group.

Summary Statistics and Empirical Evidence

Table 1 provides summary statistics for house attributes by flood zone for houses in *near-miss* counties that sold before the hurricane event.[23] Columns (1) - (4) present the means and standard deviations for house characteristics. Column (5) then tests for the equality of means across treatment and control groups. On average, houses in the A zone are more expensive relative to those in the X zone, where the average house prices for A and X zone houses are \$236,000 and \$192,000, respectively. A comparison of the house characteristics from each group makes clear the likely source of the price difference: houses in the high risk floodplain are attached to more desirable characteristics, on average. For example, Zone A houses are closer to the coast and parks, amenities for which households have shown positive willingness to pay (Smith et al., 2006; Conroy and Milosch, 2011; Nyce et al., 2015), and farther from highways and airports, which are often considered disamenities as a result of associated noise and congestion (Smith, Poulos, and Kim, 2002; Pope, 2008; Ahlfeldt and Maennig, 2015). These differences in observable characteristics by floodplain potentially suggest systematic differences in unobserved characteristics as well, motivating the use of a DD framework. The same comparison by flood zone for houses in the *never-hit* group in Table 2 reveals similar differences between A and X zone houses.

Assuming that the changes over time for houses in the control group represent how those

[23]The *near-miss* designation is based on the 500-claim definition. Our results are robust to alternative *near-miss* definitions.

in the treatment group would have behaved had the event not occurred, the DD estimate returns the causal impact of the event on housing prices in the treated group. While Tables 1 and 2 demonstrate that there are clearly observable (and thus, likely unobservable) differences between those in our treatment and control groups, DD will account for these differences as long as they are time-invariant, a requirement that is commonly referred to as the *parallel trends assumption*. We assess the validity of this assumption in Figure 8, which plots the prices for treatment and control houses in the *near-miss* group both before and after the treatment time period. To account for observable differences across houses, we first regress prices on house characteristics and fixed effects for each region and year. We then aggregate the residuals to the floodplain and quarter-of-year level, and plot these residuals over time using local linear regression (Fan and Gijbels, 1996). Figure 8 shows that adjusted prices of the treated group before the event period exhibits a similar trend as those in the control group, even though they are generally higher compared to their control group counterparts.

While lack of evidence of pre-existing trends in Figure 8 is supportive of the common trends assumption, A and X zone houses could begin to trend differently after the event period, threatening the causality of the DD estimates. To check for this possibility, Figure 8 plots the same figure as above but for houses in the *never-hit* group. Notice that immediately following the treatment period, A zone houses are about 10% higher than their X zone counterparts. However, by the middle of the year 2010, the difference widens to be around 20%. This suggests that had A zone houses in the *near-miss* group *not* been exposed to the event, their prices would have been ∼10% higher compared to their X zone counterparts. Our triple-differences specification is set up to account for this type of differential trends between our treatment and control groups. One potential concern is that A zone houses in the *never-hit* group can similarly experience salient impacts from the flood events. While this may be possible, Figure 8 depicts suggestive evidence that this is not the case as A and X zone houses trend in a similar manner immediately after the event period.

Finally, we assess the potential concern in our data that the underlying population (and preferences) in affected areas may change in response to disasters.[24] Table 3 provides evidence of neighborhood turnover as a result of the flood events. Each column represents a separate regression where the dependent variable is an indicator for the race or ethnicity of the homebuyer as self-reported on the mortgage application.[25] Panel A examines changes in the composition of homebuyer race using the DD specification in equation (2.4), whereas Panel B does the same except uses the cross-section comparison as laid out in specification in equation (2.6). The estimates of interest in panels A and B are the interaction terms $ZoneA \times Post$ and $ZoneA \times NearMiss$, respectively. $ZoneA \times Post$ gives the relative change in homebuyer race in the A zone (vs. the X zone) after the event has occurred. While the changes are not large, it is apparent that the share of Hispanics decreases over time in response to the flood event, which is suggestive of post-disaster sorting. In contrast, the magnitude of differences are all smaller in the cross-sectional comparisons in Panel B, where none of the estimates are statistically significant. While the magnitudes of differences are not large, there may be other aspects of the neighborhood turnover that we have not captured as homebuyer race is only one of many facets that defines the character of a neighborhood.

Our spatial DD would be robust to these types of neighborhood changes over time; however, it requires that the hedonic price functions are comparable across space after various house and spatial controls are included. We assess this spatial analog to the common trends assumption in Figure 8. The figure plots 1) the average price difference between A and X zone sales in the *never-hit* counties against the distance between each house to the nearest *near-miss* house, and 2) the same plot for sales in the *near-miss* counties by distance between each house and the nearest *never-hit* house. Specifically, the price differences are recovered using a regression of the log(price) on all interactions between an A zone dummy variable

[24] In other words, that different types of people, with different preferences for flood risk, may enter or leave the housing market following a disaster.

[25] Information on homebuyer race are merged in the housing transactions data using data from the Home Mortgage Disclosure Act (HMDA). This follows the procedure outlined in Bayer et al. (2016), where the merge is based on information that is present in both the transactions and HMDA data, including the lender, loan amount, transaction date and census tract. We were able to match 63% of the housing transactions. Merge diagnostics comparing to Census data are available upon request.

and 5km-distance bins:

$$lprice_{j,t} = \beta_0 + \beta_1 zoneA_j + \sum_k \beta_{2,k} dist_j + \sum_k \beta_{3,k} dist_j \times zoneA_j + e_{j,t}$$

where the coefficients on the interaction terms $\beta_{3,k}$ are plotted in the figure. This regression is done first for the *near-miss* group, and then for the *never-hit* group, resulting in two sets of relative price differences in two spatially disparate areas. All sales used are prior to the hurricane event. In the figure, note that the distance to the nearest *near-miss* house in the top axis is flipped. This is done so that moving from left to right for both axes would imply increasing exposure to the hurricane event: For *never-hit* houses (top axis), those that are farther from *near-miss* houses are also farther from areas that would be directly impacted by the hurricane cluster; this is generally the opposite for *near-miss* houses (bottom axis) as those that are farther from never-hit houses are generally *closer* to areas that would be hit by the hurricane cluster. Comparison of the two price functions finds that while there are clearly level differences in the relative impact of living in zone A, the price functions follow a similar trend. This gives us more confidence to use a spatial difference-in-differences design that makes comparisons across geographic space to deal with the Kuminoff and Pope (2014) critique.

4 Results and Discussion

Main Results

Table 4 presents the DD estimates that compare changes in *near-miss* housing prices for A zone homes over time relative to X zone homes. Each column represents a regression. Standard errors are clustered at the census tract level to allow for spatial correlation between house observations. The baseline specification in column (1) controls for house and neighborhood characteristics only. The importance of limiting comparisons across large geographic areas is clear after the inclusion of spatial fixed effects beginning in column (2). This baseline estimate in column (1) finds a salience impact of -1.2 %, which is not statistically significant at conventional levels. The estimate more than doubles in column (2) and increases in precision once region fixed effects are included.[26] The specifications that follow in columns (3) through (5) increases the geographic specificity of the spatial fixed effects from inclusion of Core-Based Statistical Area fixed effects to county fixed effects, and then lastly to census tract fixed effect. These DD estimates find salience impacts ranging from -3.2% to -4.4%, and are statistically different from 0 at the 5% and 10% levels.

We next turn to our triple-differences design. As one can think of the DDD estimate as the difference between two sets of DD estimates, Table 5 presents the triple-differences estimate as such. Columns (1) through (3) show the DD design as the change in the pre- and post- prices for A zone houses compared to a similar change for the X zone houses, where the DD estimate of -3.5% (with census tract fixed effects) is given in row 3 of column (3). Turning to columns (4) through (5), the same DD estimate is recovered for houses in the *never-hit* group. Consistent with Figure 8, we see that A zone houses in these areas experience a 4.3% increase in house prices relative to their X zone counterparts, suggesting that A zone houses in the *near-miss* group would have experienced this change had it not been for the disaster event. The DDD estimate of -7.8% that accounts for this is given in row 4 of column (6) as the difference between the DD estimates in the *near-miss* and *never-hit* groups.

Table 6 presents the triple-differences estimates using the regression specification in equation (2.5). Again, the baseline specification in column (1) includes house characteristics but omits geographic fixed effects. Columns (2) through (5) adds additional spatial fixed effects as in the DD specifications. The estimates that contain geographic fixed effects range from -5.1 % under region fixed effects (not statistically significant) to -8.0% with census tract fixed effects (statistically significant at the 5% level). As predicted by the graphical analysis

[26]The regions are defined by the Florida Public Archeology Network (FPAN), which is a program of the University of West Florida. FPAN divides the state of Florida into 8 distinct regions: northwest, north central, northeast, east central, central, west central, southwest, and southeast.

and previous DD mean comparisons, the DDD estimate of salience is about two times the DD estimate now that controls for unobserved differential trends over this period between our treatment and control groups are incorporated.

Figure 8 plots the DDD impacts over time beginning from before the treatment period to after. Estimates are recovered from a single regression that allows for leads and lags of the treatment, where the specification includes fixed effects at the census tract level. Specifically, we take the main DDD specification and interact $zoneA_j \times NearMiss_j$ and $zoneA_j \times NearMiss_j \times Post_t$ with a full set of year dummies from 2002 to 2012. The coefficients on the interactions return the relative impact on sales prices of houses in the A zone and *near-miss* group in each year (before and after the event period). In the three years prior to the hurricanes, estimated impacts are small in magnitude and statistically insignificant, ranging between -0.6% to 1.1%. That the leads to treatment finds very little evidence of an effect bolsters the case that the hurricane cluster was exogenous and unexpected. Beginning from 2005, housing prices initially fall by 6.6% and continue to oscillate on a downward trend until a low point of -14.3% in the year 2009, after which the impacts begin to rebound, until reaching about -3.8% in 2012, our final year of data. While the estimated impact may appear to follow the great recession and its recovery, these impacts should be net of any macroeconomic effects as long as the recession's impact on *near-miss* houses in A versus X zones are similar so that any differential impact between A and X houses are appropriately captured by their differences over time in the *never-hit* group.

Unlike other market transactions, those in the housing market are associated with higher search and switching costs in terms of time. This is a first reason why we might observe a lagged impact on housing prices as opposed to an instantaneous increase in insurance policy take-up as found in Gallagher (2014). Second, some of the lag could be attributed to in-migration from hurricane-impacted areas. This is supported by Table 7, which presents the shares of house sales by directly impacted and *near-miss* areas (*never-hit* houses are removed), before and after the hurricane event. Furthermore, the deviation from the pre-hurricane distribution of buyers is much larger when examining the years after the event but before 2010 compared to the period after 2009; this coincides with when the salience impact peaks, which additionally suggests that migration from heavily impacted areas could be driving the trajectory of salience. Though only suggestive (as this could also be driven by new buyers or people who are migrating from outside of the state), it does point to a potential explanation as to why the largest price decreases come several years later as people exposed to impacted areas gradually migrate out of those areas. Flood risks are likely to be most salient for this group of people, and, as previously discussed, there are time costs associated with search and moving. These all contribute to the increasing size of the salience impact over time. Coincidentally, this also highlights an additional reason in support of a spatial-DD approach, the results of which are discussed next, as part of the observed impact is potentially driven by compositional changes in the population of buyers over time.

While the DD and DDD estimates are well-suited for dealing with bias from omitted variables, the use of time variation potentially includes prices from different hedonic equilibria. We check the robustness of our results by estimating salience impacts with the specification in equation (2.6), which utilizes post-hurricane transactions only. This estimates salience in the spirit of Kuminoff and Pope (2014) using a DD framework by comparing A and X zone prices in the *near-miss* group to a similar difference in the *never-hit* group, all *after* the hurricanes have occurred. We estimate this impact by pooling all post-event years, as well as impacts for individual years using only the data from that particular year. Importantly, limiting comparisons to houses in a particular year after the flood event allows us to avoid the assumption of a time-invariant hedonic gradient.[27] Table 8 presents these results. Each *cell* displays the DD estimate and robust standard errors from a separate regression; moving from left to right, each column presents estimates from the inclusion of fixed effects at finer levels of geography. Each row contains all estimates from all post-years ("Overall") followed by individual years from 2006 to 2012. Depending on the level of the fixed effects included,

[27]Implicit to this approach is the assumption that housing across Florida is characterized by a single housing market.

the pooled estimates ranges from -5.4% (tract fixed effects, not statistically significant) to -12.3% (region fixed effects, 5% statistical significance).

The estimates for individual years in Table 8 are all generally larger, although many are not significantly different from 0 at conventional levels. The largest impacts are found in 2009, ranging from -17.4% to -12.3%, which are statistically significant at the 5-10% levels. The smallest impacts are found in 2012, where estimates range between -12.6% to 3.1%, none of which are statistically significant. We attribute the lagged salience effect to potentially indicate substantial search and moving costs in the housing market. However the estimates in the latter years are consistent with the finding of previous literature that flood risk salience eventually wanes in the years following an event as individuals downwardly revise their flood risk probability after periods of calm (e.g., Atreya, Ferreira, and Kriesel (2013) and Gallagher (2014)). Compared to the DD estimates in Table 4, most of the spatial DD estimates find a larger negative salience impact. The direction of the bias from the pre- and post- DD is consistent with the out-migration (in-migration) of those with high (low) willingness to pay to avoid flood risk after the hurricane events. In other words, were one able to prevent this type of sorting, then the DD estimate using pre- and post- event sales would be larger in magnitude than what it actually recovered, since it would not confound differences in willingnesses to pay across different types of people. Taken together with previous literature's observation of differential patterns of post-disaster migration (e.g., Smith et al. (2006); Landry et al. (2007); Groen and Polivka (2010); Strobl (2011), Deryugina, Kawano, and Levitt (2014)), our results imply that it is thus important to acknowledge the potential for hedonic equilibria to shift in response to disasters and to think about the implications this may have for willingness to pay estimation.

Robustness

Before concluding, we assess the robustness of our results. While the paper thus far has treated exposure to flood risk and hurricane events as a binary variable, the information treatment (whether it be along the dimensions of risk zones or distance to directly impacted areas) is likely to be continuous, and could result in our control sales receiving "treatment." To assess how discretization of treatment could impact our results, we re-estimate various specifications that use a more strict definition of a control group. First, we assess the impact of dropping *never-hit* county sales that are close to *near-miss* counties. Specifically, in Table 9, we compare the main DDD estimate in Table 6, column (5) with DDD estimates that drop *never-hit* sales within 5, 10, 15 and 20 kilometers to the nearest *near-miss* house (respectively presented in columns 2 through 5). Even while requiring our control units to be farther from *near-miss* counties, our estimated salience impacts are generally stable, suggesting that information spillovers are limited across *near-miss* and *never-hit* areas are limited.

We also assess the assumption that households in the high-risk floodplain internalize additional information from flooding in neighboring counties, while those in the low-risk floodplain do not. We do this by re-estimating the traditional DD regression specification but drop A zone houses that are within d meters of an X zone house, where d ranges from 200 to 1000 meters. We present these results in Table 10. Compared to the baseline estimate that uses temporal variation in the hurricane treatment event, the salience estimate generally increases from -3.5% to -4.4% as we require X zone sales to be farther from A zone houses. This is likely due to treatment spillovers from A to X zones, which in this case, biases our salience estimate downward.

As some properties are sold multiple times, we are able to estimate our DD and DDD models with house fixed effects, which would allow one to control for time-invariant, house-specific unobserved factors that could impact price. Table 11 presents these estimates along with our baseline estimates with tract fixed effects. Inclusion of house fixed effects leaves us with about 20% of our original sample and significantly reduces the precision of our estimates. Still, we note that the magnitudes of the estimates are similar if not larger than the baseline estimates.

One potential concern for the identification of flood risk in hurricane events is that, in addition to flood risk, hurricanes also transmit information on wind and storm surge risk. We do not include homes in the coastal flood risk zones so our analysis does not address, but is also unlikely to be confounded by, storm surge risk. Regarding wind risk, risk zones are typically highly correlated with distance to the coast and smoothly transition across large distances from higher to lower risk.[28] In contrast, flood risk is geographically heterogeneous, varying sharply across small distances, relative to Florida's wind zones. Given our spatial fixed effects, i.e. conditional on being within some region (e.g. census tract), wind risk should thus be uncorrelated with flood risk zones, allowing our difference-in-difference design that utilizes A and X zone variation to appropriately control for wind risk's impact on our salience estimates.

Lastly, we perform several placebo tests in Table 12 to generate additional evidence in support of a causal interpretation of estimated impacts. To do this, we randomly re-assign one dimension of exposure for each sale transaction in the sample and then re-estimate the DDD specification. We again focus on the specification with census tract fixed effects. Beginning from column (1), we randomly assign sales to either the *near-miss* or *never-hit* groups. Column (2) retains the actual *near-miss* and *never-hit* categorization, but randomly assigns the floodplain zones (i.e. A as opposed to X). In the remaining columns, we randomly assign sales to the pre- or post- treatment period, effectively randomizing the treatment date. Column (3) does this for the entire sample, whereas column (4) limits the treatment date randomization to sales in the post-treatment period. In each case, the DDD estimate is small in magnitude compared to our main estimate of 8.0% and is not statistically significant. These placebo tests reinforce that our estimated price impacts are causal and suggest that there is indeed a flood risk salience effect.

[28]For example, see the following wind risk map of Florida posted by Hernando County: http://www.co.hernando.fl.us/bldg/wind.htm

5 Conclusion

In this paper, we present new evidence on flood risk salience. Utilizing a decade of data from across the state of Florida and exploiting an anomalous hurricane cluster preceded and followed by periods of unusual calm, we compare salience estimates across three approaches including time-varying difference-in-differences, triple-differences, and spatial difference-in-differences designs. We note important limitations that remain in the analysis. First, we are unable to disentangle the exact individual flood risk belief updating structure that we term salience in this paper. Notably, we cannot distinguish the difference between individuals upwardly revising subjective flood risk probabilities after a flood event versus individual (ir)rational inattention to flood risk during the complex home buying process. Second, it remains an open question as to how the magnitude of post-disaster flood risk salience relates to rational risk perception. Lastly, while our main specification assumes that the flood information treatment is uniformly distributed across *near-miss* counties yet does not reach *never-hit* counties, our robustness analysis suggests some level of information spillover that could attenuate our estimates. Thus, our results can be thought of as a lower bound on the true salience effect.

Keeping these limitations in mind, our various quasi-experimental approaches all find robust evidence of a salience effect in Florida in response to the cluster of hurricane events in the mid-2000's. These salience impacts range from -3 to -8%, on average. In addition, we find evidence that disasters may impact more than salience, even in locations just missed by direct damage. Specifically, in the spirit of Kuminoff and Pope (2014), and motivated by a recent but growing literature on differential post-disaster migration, we detect changes in homebuyer demographics following a disaster, which could indicate different buyer populations in pre- versus post- disaster hedonic equilibria. To avoid confounding salience estimates with these concurrent changes, we use a spatial DD approach and still find robust salience effects, which, in our setting, are twice as large as the estimates recovered using a DD approach involving price comparisons across time. Ultimately, these findings highlight the importance of careful interpretation surrounding salience results, as salience identification may be empirically intermingled with other time-variant factors that could impact the hedonic equilibria. In addition, and not explored by this work, it remains an open question how the magnitude of post-disaster flood risk salience correlates with rational risk perception, as individuals may under- or over-perceive the risk directly following the shock. As flooding imposes tremendous risk to life and property across much of the globe, understanding the dynamics of public flood risk perception has important implications for flood insurance and disaster policy, the benefits transfer literature, and our understanding of natural disaster resilience.

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6 Tables

Table 1: Housing Attributes by Flood Zone (Near-Miss Counties Only, Pre-Event)

	Treat (A)		Control (X)			
	Mean	SD	Mean	SD	*t*-Statistics	Reject
House Atrributes	(1)	(2)	(3)	(4)	(5)	(6)
Price	236,185.30	250,680.30	192,295.40	141,351.20	23.92	Y
Age	17.35	16.70	17.13	16.79	1.08	N
Bathrooms	2.17	0.68	2.13	0.59	6.08	Y
Bedrooms	3.09	0.72	3.14	0.69	-5.92	Y
Square footage	1,879.80	742.90	1,795.80	650.50	10.65	Y
Toxic release inventory	9,796.50	55,107.00	5,938.00	51,266.40	6.24	Y
Distance to coast	27.42	31.95	40.51	31.04	-35.13	Y
Distance to river	83.03	70.45	47.79	48.76	57.83	Y
Distance to park	19.63	15.73	26.73	17.60	-33.94	Y
Distance to railway	6.93	5.89	6.14	5.26	12.49	Y
Distance to airport	22.77	16.76	16.66	12.27	40.23	Y
Distance to highway	2.71	4.04	1.64	2.19	37.47	Y
Distance to city	0.24	1.82	0.15	1.34	5.66	Y
Crime rate	1,118.50	510.30	1,059.30	485.50	10.12	Y
Observations	7,585		85,872			

Notes: This table compares mean attributes of house sales of A and X zones in *near-miss* counties only. Attributes are taken from houses selling before the hurricane event period. Crime rate is measured as arrest rate per 100,000 people, distances are measured in kilometers, and Toxic Release Inventory (proxying for industrial activity) is the inverse-distance weighted average of onsite releases. The *t*-statistics to assess the equality of means between the A and X groups are provided in column (5) and an indicator of whether the null of equal means is rejected is given in column (6).

Table 2: Housing Attributes by Flood Zone (Never-Hit Counties Only, Pre-Event)

	Treat (A)		Control (X)			
	Mean	SD	Mean	SD	t-Statistics	Reject
Atrributes	(1)	(2)	(3)	(4)	(5)	(6)
Price	210,126.80	226,162.50	151,052.50	119,848.70	20.75	Y
Age	24.39	16.06	27.01	18.50	-6.32	Y
Bathrooms	2.09	0.72	1.98	0.58	8.59	Y
Bedrooms	2.93	0.80	2.97	0.68	-2.70	Y
Square footage	1,831.60	799.20	1,666.10	618.90	11.70	Y
Toxic release inventory	21,668.60	317,561.40	8,425.10	136,255.20	3.95	Y
Distance to coast	26.25	24.95	30.00	22.63	-7.32	Y
Distance to river	55.76	45.35	40.38	45.54	15.00	Y
Distance to park	14.27	12.08	17.93	11.64	-13.94	Y
Distance to railway	8.28	6.78	6.10	5.45	17.53	Y
Distance to airport	43.33	31.35	31.01	28.18	19.32	Y
Distance to highway	1.56	1.65	1.34	1.39	6.97	Y
Distance to city	1.25	2.87	0.34	2.06	19.20	Y
Crime rate	1,037.00	545.60	1,161.00	543.40	-10.13	Y
Observations	2,063		44,645			

Notes: This table compares the same set of house attributes as in Table 1 except for sales in *never-hit* counties. The sample is limited to all house sales before the event period. The t-statistics to assess the equality of means between the A and X groups are provided in column (5) and an indicator of whether the null of equal means is rejected is given in column (6).

Table 3: Changes in Homebuyer Characteristics

A. Before vs. After Event Comparison

	White	Black	Hispanic
A Zone	-0.00895	-0.00921**	0.0245***
	(0.00673)	(0.00423)	(0.00572)
post	-0.0874	0.0437	0.0711
	(0.0588)	(0.0370)	(0.0500)
A Zone $\times$ *post*	0.0152*	0.00457	-0.0230***
	(0.00861)	(0.00541)	(0.00732)
Observations	152,578	152,578	152,578

B. Near Miss vs. Never Hit Comparison

	White	Black	Hispanic
A Zone	0.0164	-0.00161	-0.0135
	(0.0111)	(0.00744)	(0.00908)
NearMiss	0.0188	0.178	-0.304
	(0.365)	(0.244)	(0.299)
A Zone $\times$ *NearMiss*	-0.00948	-0.00285	0.0154
	(0.0124)	(0.00829)	(0.0101)
Observations	131,367	131,367	131,367

Notes: This table assesses changes in homebuyer race and ethnicity. Panel A regresses an indicator for homebuyer race on a post-event dummy, an A zone dummy, and their interaction, limiting the sample to houses of *near-miss* counties only. Regressions in Panel B use only post-event sales and regresses the buyer characteristic on a *near-miss* dummy, an A zone dummy, and their interaction. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)
A Zone	0.079***	0.088***	0.087***	0.094***	0.054
	(0.029)	(0.030)	(0.031)	(0.031)	(0.035)
post	0.640***	0.017	-0.161*	-0.238***	-0.181***
	(0.026)	(0.071)	(0.086)	(0.056)	(0.056)
A Zone × *Post*	-0.012	-0.032*	-0.038**	-0.044***	-0.035**
	(0.019)	(0.016)	(0.017)	(0.017)	(0.016)
Observations	245,774	245,774	245,039	245,774	245,774
Controls:					
Nbd. & House Attributes	Yes	Yes	Yes	Yes	Yes
Region by Year FE	No	Yes	No	No	No
CBSA by Year FE	No	No	Yes	No	No
County by Year FE	No	No	No	Yes	No
Tract by Year FE	No	No	No	No	Yes

Notes: This table presents results of the DD specification in Equation 2.4 comparing sales before and after the hurricane cluster event. All specifications are based on a 500-hit definition and use *near-miss* counties only. Controls for house and neighborhood characteristics include number of bathrooms, square footage, and age of the house; distances to the nearest coast, river, park, railway, airport,highway, and city, weighted TRI onsite releases and crime rate (arrest rate per 100,000 population). 'CBSA' represents Core-based Statistical Area. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Difference-in-Differences-in-Differences (Mean Comparisons)

Sample	NearMiss (NM)			Never Hit (NH)		
$\Delta \ln P$	Post	Pre	Δ_{NM}	Post	Pre	Δ_{NH}
	(1)	(2)	(3)	(4)	(5)	(6)
A zone	10.489	10.766	-0.216	11.834	11.79	0.044
	(0.387)	(0.394)		(0.216)	(0.213)	
X zone	10.47	10.712	-0.181	11.745	11.745	0.000
	(0.365)	(0.371)		(0.212)	(0.212)	
DD (β_3)			-0.035			0.043
			(0.016)			(0.029)
Difference in DD (π)						-0.078
						(0.033)

Notes: This table provides preliminary triple-differences estimates by using estimates from two separate DD specifications, one for the *near-miss* group and the other for the *never-hit* group. Tract by year level fixed effects are included in addition to the same set of house and neighborhood level controls in column (5) of Table 4. Robust standard errors are clustered at the Census Tract level in parentheses.

Table 6: Difference-in-Differences-in-Differences

	(1)	(2)	(3)	(4)	(5)
A Zone	0.165***	0.165***	0.133***	0.118***	0.044*
	(0.044)	(0.044)	(0.033)	(0.033)	(0.025)
post	-0.048	0.056	0.048	-0.022	-0.043
	(0.046)	(0.049)	(0.052)	(0.037)	(0.035)
A Zone × *post*	0.023	0.019	0.024	0.029	0.050*
	(0.031)	(0.031)	(0.030)	(0.030)	(0.029)
NearMiss	0.098***	-0.037	-0.234	0.120	0.230***
	(0.019)	(0.037)	(0.145)	(0.207)	(0.030)
A Zone × *NearMiss*	-0.086	-0.082	-0.055	-0.035	0.003
	(0.053)	(0.055)	(0.045)	(0.046)	(0.043)
post × *NearMiss*	0.017	0.024*	0.029**	0.026**	0.033***
	(0.012)	(0.013)	(0.012)	(0.012)	(0.011)
A Zone × *post* × *NearMiss*	-0.057	-0.051	-0.057*	-0.068**	-0.080**
	(0.035)	(0.035)	(0.034)	(0.034)	(0.033)
Observations[a]	360,918	360,918	359,856	360,918	360,918
Controls:					
Nbd. & House Attributes	Yes	Yes	Yes	Yes	Yes
Region by Year FE	No	Yes	No	No	No
CBSA by Year FE	No	No	Yes	No	No
County by Year FE	No	No	No	Yes	No
Tract by Year FE	No	No	No	No	Yes

Notes: This table presents the DDD estimates from the specification in Equation 2.5. The same set of controls are used as that in Table 4. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] The difference in observation count in column (3) is attributed to 13 counties in our data that are not considered as being a part of a CBSA (i.e. the CBSA is missing). Our results are robust to retaining those observations without a CBSA designation and including an indicator for a missing.

Table 7: Share of Sales by Impacted and Near-Miss Areas (%)

	Hit	Near-Miss
Pre-Hurricane	65.54	34.46
Post-Hurricane	61.05	38.95
Post, pre-2010	59.5	40.5
Post, post-2009	64.04	35.96

Notes: This table presents shares of house sales by impacted ("hit") and near-miss areas, removing sales in never-hit areas. It then breaks the post-hurricane years into before and after 2009 (when we see our largest salience impact).

Table 8: Difference-in-Differences (Post-Event Only)

$A \times NearMiss$	(1)	(2)	(3)	(4)	(5)
Overall	-0.0671	-0.123**	-0.0966**	-0.0848*	-0.0537
	(0.0512)	(0.0497)	(0.0453)	(0.0450)	(0.0466)
2006	-0.121**	-0.108**	-0.0942*	-0.0917*	-0.0515
	(0.0511)	(0.0515)	(0.0504)	(0.0499)	(0.0538)
2007	-0.109	-0.0753	-0.0673	-0.0549	-0.0443
	(0.0874)	(0.0880)	(0.0884)	(0.0861)	(0.0791)
2008	-0.182	-0.151	-0.123	-0.0757	-0.0211
	(0.113)	(0.111)	(0.111)	(0.109)	(0.124)
2009	-0.174**	-0.159**	-0.136*	-0.139**	-0.123*
	(0.0711)	(0.0736)	(0.0706)	(0.0687)	(0.0722)
2010	-0.107	-0.124*	-0.0648	-0.0765	-0.0968
	(0.0670)	(0.0708)	(0.0679)	(0.0673)	(0.0775)
2011	-0.107	-0.148**	-0.0960	-0.0827	-0.0220
	(0.0718)	(0.0748)	(0.0682)	(0.0665)	(0.0675)
2012	-0.103	-0.126	-0.0794	-0.0530	0.0307
	(0.0914)	(0.0935)	(0.0805)	(0.0801)	(0.0889)
Controls:					
Nbd. & House Attributes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	No	No
CBSA FE	No	No	Yes	No	No
County FE	No	No	No	Yes	No
Tract FE	No	No	No	No	Yes

Notes: This table presents spatial DD estimates using post-hurricane cluster data only. Each cell represents a DD estimate of interaction between $A\ zone \times post$ from a separate regression comparing A and X zone houses in *near-miss* and *never-hit* counties. The same set of house and neighborhood level controls are used as that in column (5) of Table 4. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Remove Never Hit Sales within X km of Near Miss County

	DDD				
	(1)	(2)	(3)	(4)	(5)
	Baseline	5km	10km	15km	20km
$A\ Zone \times post \times NearMiss$	-0.080**	-0.079**	-0.078**	-0.087**	-0.075**
	(0.033)	(0.033)	(0.033)	(0.034)	(0.036)
Observations	360,918	360,323	358,692	353,616	347,598

Notes: This table compares the main DDD estimate in Table 6, column (5) with DDD estimates that drop never-hit sales within 5, 10, 15 and 20 kilometers to the nearest *near-miss* house (respectively presented in columns 2 through 5). The same controls from Table 6, column (5) are used for each regression. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Zone A-X Spillover Effect

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	200m	400m	600m	800m	1km
A zone	0.054	0.052	0.05	0.054	0.058	0.062
	(0.035)	(0.038)	(0.045)	(0.052)	(0.062)	(0.073)
post	-0.181***	-0.175***	-0.157***	-0.150***	-0.133**	-0.083
	(0.056)	(0.055)	(0.057)	(0.058)	(0.059)	(0.064)
A zone$\times$*post*	-0.035**	-0.033**	-0.035**	-0.038**	-0.039**	-0.044**
	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)
Observations	245,774	236,721	205,460	174,252	147,660	114,908

Notes: This table compares the main DD estimate in Table 4, column (5) with DD estimates that drop X zone houses where the nearest A zone house is 200, 400, 600, 800, and 1000 meters away. The controls from Table 4, column (5) are used for each regression. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: House Fixed Effects

	DD	
	(1)	(2)
	Baseline	House FE
A zone$\times$ *post*	-0.035**	-0.047
	(0.016)	(0.034)
Observations	245,774	51,420
	DDD	
	(3)	(4)
	Baseline	House FE
A zone$\times$*post*$\times$*NearMiss*	-0.080**	-0.132
	(0.033)	(0.097)
Observations	360,918	77,828

Notes: This table compares the main DD and DDD estimates respectively in Table 4 column (5) and Table 6 column (5) with house fixed effect estimates. Robust standard errors are clustered at Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Placebo Checks

Random Assignment:	*NearMiss* v. *NeverHit*	Flood Plain (A v. X Zone)	Treatment Dates (Any Year)	Treatment Dates (post-2005)
	(1)	(2)	(3)	(4)
A Zone	0.044	0.004	0.056**	0.081***
	(0.030)	(0.008)	(0.027)	(0.023)
post	-0.036	-0.039	-0.008	0.007
	(0.035)	(0.036)	(0.006)	(0.007)
A Zone × *post*	-0.018	0.002	0.036	-0.022
	(0.018)	(0.011)	(0.031)	(0.029)
NearMiss	-0.004	0.243***	0.254***	0.258***
	(0.004)	(0.028)	(0.030)	(0.030)
A Zone × *NearMiss*	0.001	-0.004	-0.022	-0.051
	(0.013)	(0.009)	(0.044)	(0.041)
post × *NearMiss*	0.009*	0.027**	0.007	-0.005
	(0.005)	(0.013)	(0.006)	(0.008)
A Zone × *post* × *NearMiss*	0.015	0.002	-0.047	0.016
	(0.019)	(0.012)	(0.032)	(0.031)
Observations	360,918	360,918	360,918	360,918

Notes: This table presents placebo checks for the DDD specification. Regressions randomly assigns houses spatially in Columns (1) and (2) and temporally in (3) and (4). Column (1) randomly assign sales to either the *near-miss* or *never-hit* groups. Column (2) randomly assigns the floodplain zones (i.e. A as opposed to X). Randomized treatment dates in the last two columns randomly assign each house to either before or after the disaster event. Column (3) does this for the whole sample, whereas column (4) does this for only post-treatment houses. Controls for all specifications correspond to that in column (5) of Table 6. Robust standard errors are clustered at the Census Tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7 Figure Captions

Figure 1: 2004-2005 Florida Hurricane Cluster

Figure 2: Counties by Hurricane Exposure (500 Claims Definition)

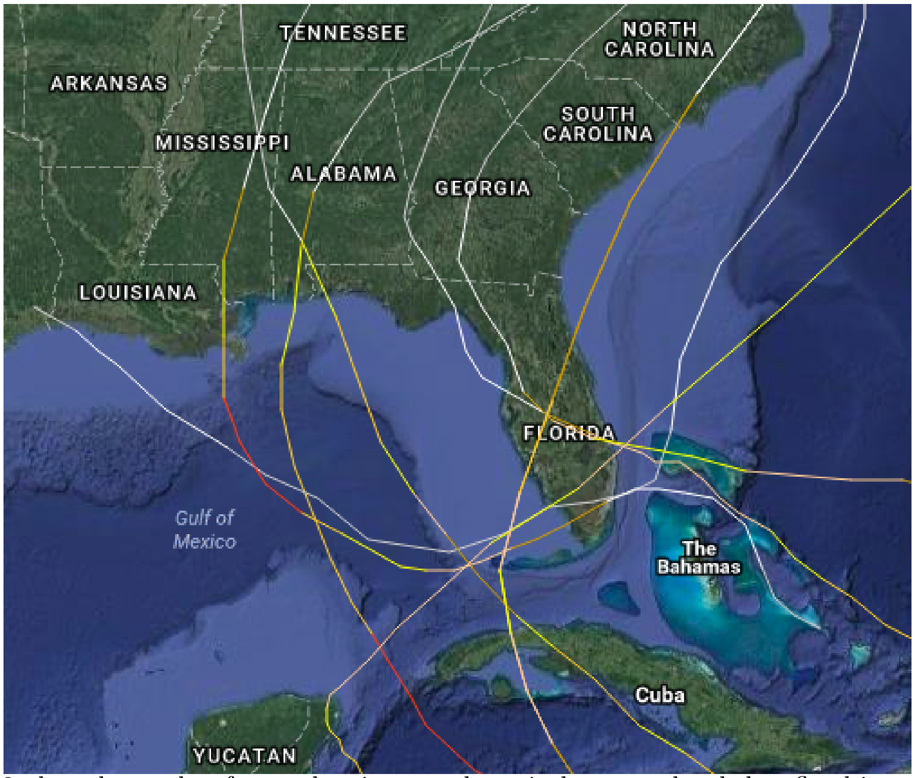
Figure 3: Parallel Trend Test in *Near-Miss* Counties

Figure 4: Parallel Trend Test in *Never-Hit* Counties

Figure 5: Parallel Trend Test in Space

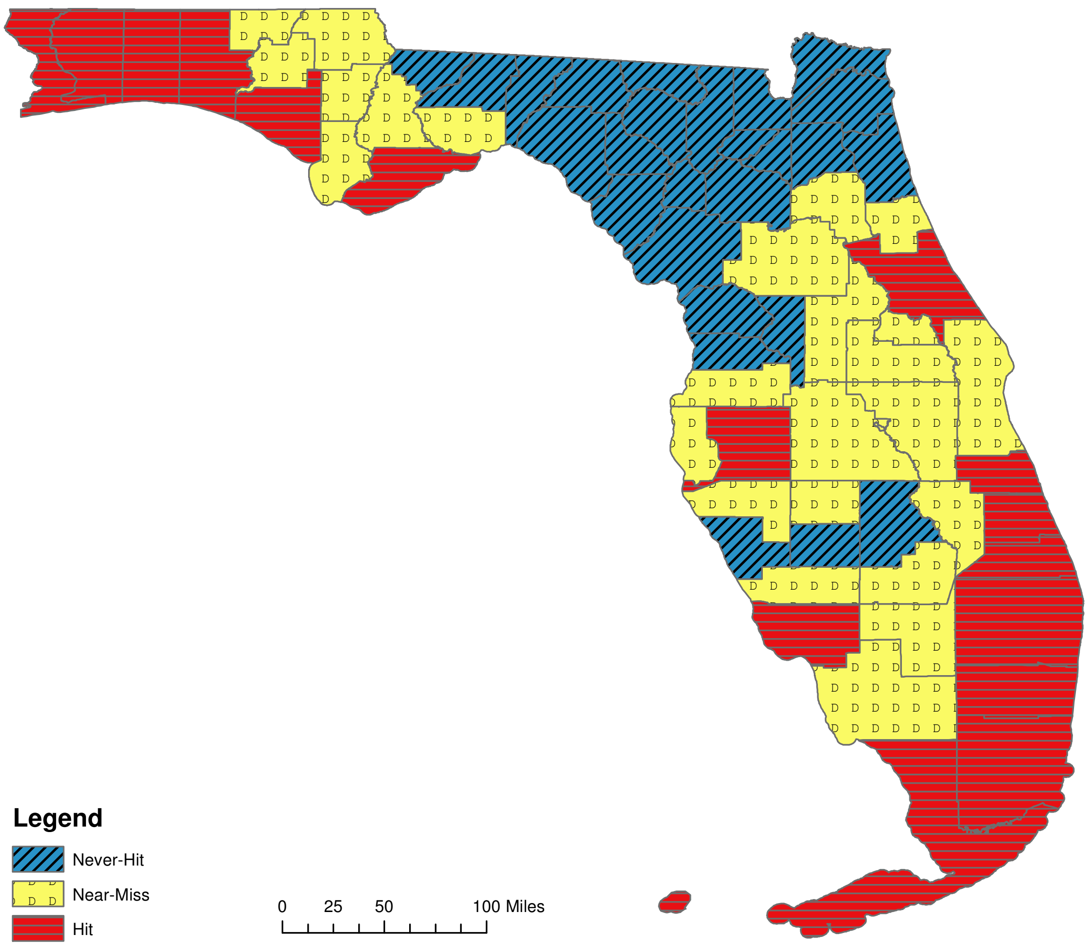
Figure 6: DDD Estimates Over Time

8 Figures



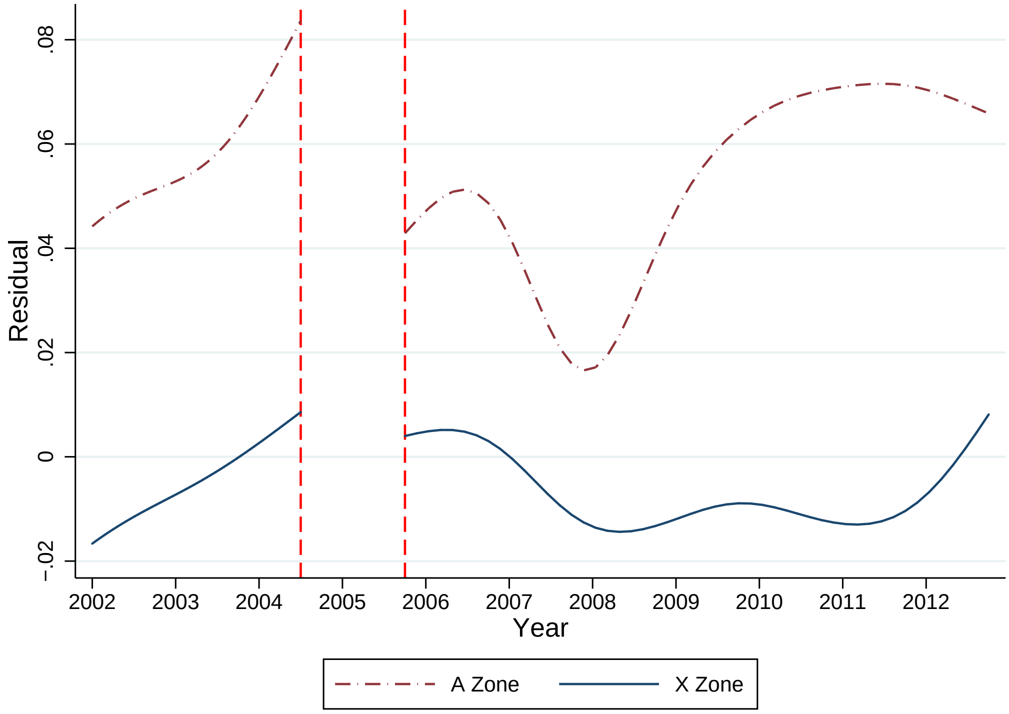
Notes: Figure 8 plots the tracks of seven hurricane and tropical storms that led to flood insurance claims in Florida from 2004 to 2005. The authors generated the figure from the ICAT Damage Estimator tool available online at http://www.icatdamageestimator.com/viewdata.

Figure 1.



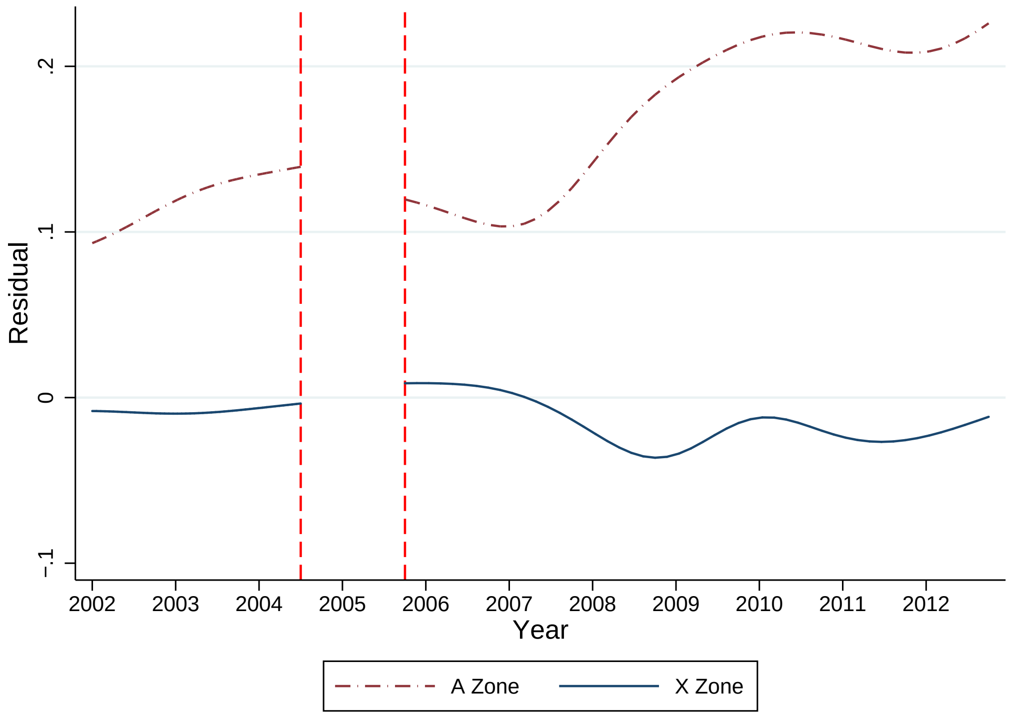
Notes: This figure uses National Flood Insurance Program (NFIP) data to illustrate the status of each county after the cluster of hurricane events hit in Florida between August 2004 and October 2005. *Hit* is defined as there being at least 500 claims at the county level, *near-miss* is defined as counties adjacent to *hit* counties, and *never-hit* are the remaining counties.

Figure 2.



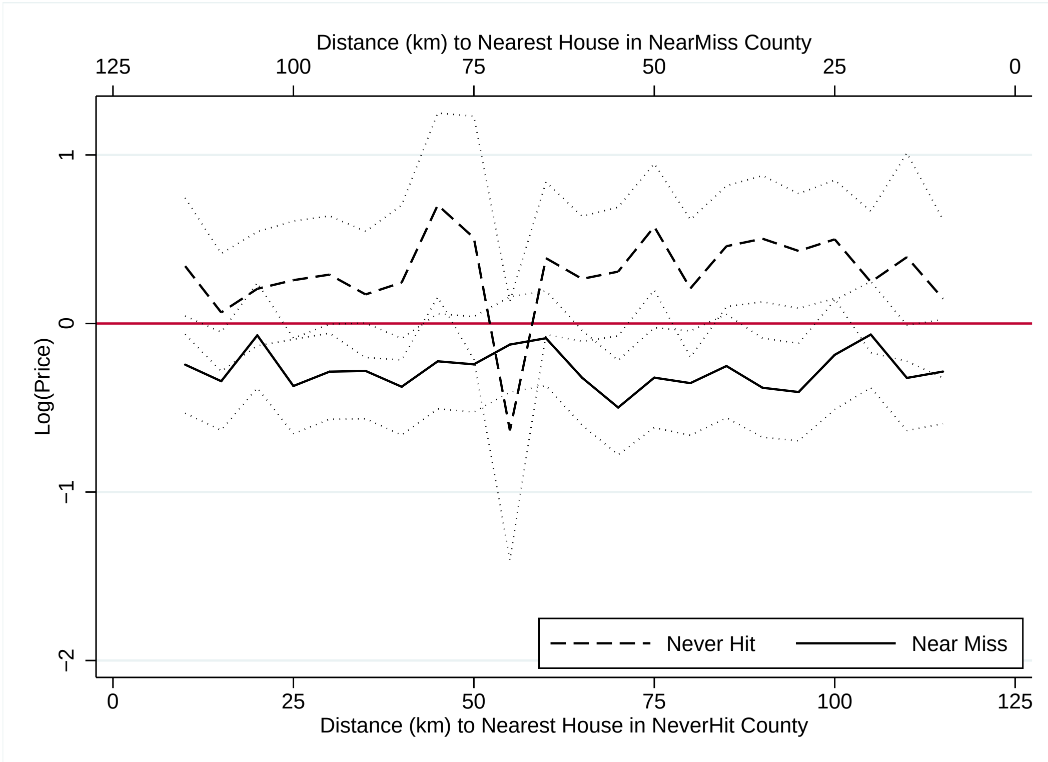
Notes: This figure compares the price trends of zone A home sales to zone X home sales in *near-miss* counties. We first regress log sale price on house attributes, neighborhood controls, and region by year fixed effects for all *near-miss* counties sales. Next, we collapse the residuals obtained from the regression to the quarter level and then plot the residuals over time using local linear regression.

Figure 3.



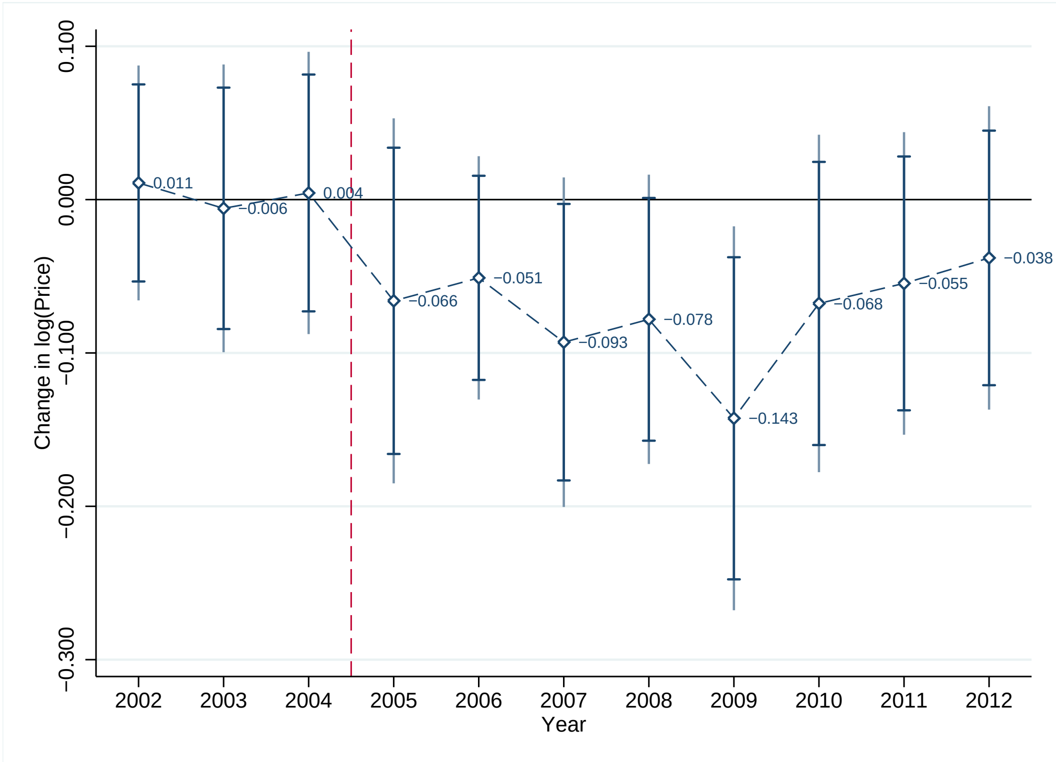
Notes: This figure compares the price trends of zone A home sales to zone X home sales in *never-hit* counties following the same approach in Figure 8.

Figure 4.



Notes: The figure plots 1) the average price difference for A and X zone sales in the *never-hit* counties against the distance between each house to the nearest *near-miss* house (top x-axis), and 2) the average price difference for A and X zone sales in the *near-miss* counties by distance between each house and the nearest *never-hit* house (bottom x-axis). Note that the distance to the nearest *near-miss* house in the top axis is flipped. This is done so that moving from left to right for both axes would imply increasing exposure to the hurricane event: For *never-hit* houses (top axis), those that are farther from *near-miss* houses are also farther from areas that would be directly impacted by the hurricane cluster; this is generally the opposite for *near-miss* houses (bottom axis) as those that are farther from never-hit houses are generally *closer* to areas that would be hit by the hurricane cluster.

Figure 5.



Notes: This figure plots the DDD estimate by each year before and after the disaster period with 90% and 95% confidence bands. The coefficients are recovered from a regression that expands the main DDD specification to allow for a full set of year dummies from 2002 to 2012.

Figure 6.