THE UNIVERSITY OF CHICAGO

ESSAYS IN THE ECONOMICS OF NATURAL DISASTERS

A DISSERTATION SUBMITTED TO

THE FACULTY OF THE IRVING B. HARRIS

GRADUATE SCHOOL OF PUBLIC POLICY STUDIES

IN CANDIDACY FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

BY

MYTHILI VINNAKOTA

CHICAGO, ILLINOIS

JUNE 2024

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGMENTS

I would like to sincerely thank Professor Koichiro Ito, Professor Ryan Kellogg, and Professor Manasi Deshpande for their valuable advice and support throughout the past six years and for their instruction during several pivotal PhD courses. I am grateful to Professor Victor Lima for his support of my career for the last decade. I also thank my co-authors Anna Ziff and Nicole Karwowski, for being incredibly supportive teammates. I thank Eyal Frank and Seunghoon Lee for their support and data acquisition help, as well as the EPIC faculty and the Harris School staff and faculty for many helpful comments and advice over the years.

On a personal note, I would like to thank my parents. They instilled in me a love for learning and knowledge, and showed me that girls could be good at math and make an impact in the world. I am deeply grateful for the strong foundation they built for me. I would like to also thank my brother Sireesh, for his support and commiseration during the long PhD journey, and for his lifelong friendship. I would be nowhere without my husband Harsha, who has walked beside me on the road to the PhD for the last twelve years. He has been both a sounding board for every decision, and an enormous source of comfort on the rainy days. I would like to thank all of my extended family - the Gurla, Vinnakota, and Honasoge families who encouraged and cheered me on. Thank you also to all my dear friends who reminded me of the joys of life beyond research and lent a sympathetic ear on the tough days. Thank you to Ruchi Mahadeshwar, Bharat Chandar, and Advik Shreekumar for the regular doses of research advice and friendship. Finally, an enormous thank you to all of my Harris classmates: Devika, Terence, Emileigh, Goya, Scott, Lucas, Yuqi, Maya, Val, Mayaya, Angela, Ari, Claire, Daisy, Pepi, Myera, and countless other wonderful people, you are the best friends and colleagues one could hope for. I may not know the counterfactual, but I am quite certain I couldn't have done it without you all, and absolutely certain that it would have been less fun.

ABSTRACT

Natural disasters are an enormous source of destruction in the United States, and the target of many policy interventions. This dissertation research focuses on hurricane and flooding disaster events, evaluating the implementation of complementary flood policies and addressing the various economic impacts of hurricane exposure.

The first chapter considers the interaction of two key flood policy instruments commonly used in the US, levee infrastructure and flood insurance, and measures how much flood insurance take-up changes in response to levee provision. Levees are critical infrastructure that reduce expected flood damage in a protected area. When a levee is constructed, and later accredited by the Federal Emergency Management Agency (FEMA), it alters inherent flood risk, flood insurance prices, and mandatory insurance purchase requirements. Using a novel panel dataset drawing from the National Levee Database, manually collected levee accreditation documentation, and FEMA flood insurance data, we leverage variation in levee construction and accreditation timing within a difference-in-differences design. Construction timing allows us to examine insurance take-up as a result of decreased flood risk, while take-up responses to accreditation reflect changes in insurance prices and mandatory purchase requirements. This chapter has three main findings: first, we find that on net, households substitute flood insurance for levee provision, decreasing insurance take-up by 16 percent. When we further decompose this effect, we find that households initially respond to levee construction with a large decrease in demand, which is then mitigated by price reductions following levee accreditation. Third, we estimate that decreases in flood insurance take-up due to levee provision crowds out aggregate household insurance spending by $183,325 per levee-mile.

The second and third chapters consider the impacts of hurricane exposure on the business and housing sectors. In the United States, hurricanes can cause billions of dollars of dam-

ages through property and infrastructure damage, and the interruption of routine economic activity. Previous research has studied macroeconomic indicators of economic progress following these devastating disasters, but there is limited evidence on how hurricanes impact business survival and outcomes, and how hurricanes impact heterogeneous populations such as renters within the housing market. Chapter 2 combines detailed spatial data on hurricane trajectories with county-level characteristics on establishment volume to measure business volume changes following a natural disaster. Using an event study design, I find that establishment volume increases by up to 4.7 percent following a hurricane event, and this increase persists for the decade following the disaster. Through a size- and industry-based heterogeneity analysis, I find that very small establishments displace bigger establishments over time, while the share of large establishments remains constant over time. Regarding industry, I find that the construction, service, and retail trade industries largely follow the positive aggregate trend, though the positive impacts on the construction industry peak soon after the hurricane exposure. Chapter 3 combines spatial data on hurricane trajectories with median contract rent data from HUD within an event study design. We find that following a hurricane, median rent persistently decreases, by up to $75 for three-bedroom properties, or 6.2 percent of baseline rental rates.

CHAPTER 1

LEVEES: INFRASTRUCTURE AND INSURANCE AS ADAPTATION TO FLOOD RISK

1.1 Introduction

Floods are an extremely common natural disaster in the US. Ninety-nine percent of US counties have experienced at least one flood event in the last 25 years, and damages from flooding have accounted for over $300 billion in losses since 1960 [Center for Emergency Management and Homeland Security, 2022]. Given that the severity of flooding is expected to dramatically increase in the coming years due to climate change [Intergovernmental Panel on Climate Change, 2022], optimal policy provisions for flood mitigation and adaptation is a particularly salient issue. To that end, the US government has spent billions of dollars on flood mitigation and damages, through a variety of policy instruments such as insurance, disaster aid, infrastructure, and adaptation grants [Sarmiento and Miller, 2006] For instance, the Congressional Budget Office estimated that the national flood insurance program cost the government $1.4 billion, and that FEMA annually spends $12.5 billion on disaster relief programs (CBO, 2017 and CBO, 2022). Despite these enormous outlays, very little is known of the interactions between the various policy instruments at our disposal, and the extent to which households' exposure to multiple public flood mitigation policies crowds out private investment in flood protection.

Flood policy instruments can be broadly categorized into three main groups: structural, non-structural, and risk transferring policies [Kahn and Lall, 2021, National Research Council, 2013]. Structural policies, such as levees, reduce the expected damages of flooding by protecting against flood damage, up to a certain degree of flood severity. Non-structural policies, such as land-use planning, may also impact the the inherent probability that flooding occurs. Lastly, risk transfer policies such as flood insurance allow households to smooth consumption across different states of the world. This paper explores the relationship between two prominent structural and risk transfer policies: levees and flood insurance. Levees are a common flood infrastructure project, often sponsored and maintained by local government agencies or large private entities. They largely consist of earthen embankments along rivers, lakes, and other flooding sources. Levees alter the fundamental flood risk of a region by reducing the damages from low-intensity, high-probability flood events, but they do not completely eliminate the flood risk of a region. Flood insurance is predominantly provided through the National Flood Insurance Program (NFIP), which is run by the Federal Emergency Management Association (FEMA). Since levees reduce expected damages, they may serve as a substitute for flood insurance [Ehrlich and Becker, 1972]. However, levees also may reduce prices and relax mandatory insurance purchase requirements. These mechanisms may imply that levees could complement flood insurance, to some extent. Therefore, our paper empirically estimates how levees interact with the insurance system, and which of these opposing mechanisms prevails.

To understand the degree of complementarity between levees and flood insurance, we measure the impact of levee provision on households' flood insurance take-up. We combine several sources of data on levees and flood insurance policies, constructing a unique panel dataset of levee provision, including construction completion dates. Additionally, through manual compilation of FEMA flood map revision documents, we construct an original dataset of levee accreditation timing. These documents provide the exact timing when the levee is accredited by FEMA, officially changing flood insurance zones and altering insurance price and mandatory purchase requirements. To estimate the *causal* effect of levee provision on insurance take-up, we leverage variation in timing of levee construction and accreditation within a difference-in-differences design.

We find that on net, levees reduce flood insurance take-up by 16 percent, indicating that households consider levees as substitutes for flood insurance. However, when we decompose the effect of levees, we find households initially substitute up to 60 percent of take-up for levee provision upon the construction of the levee, and that amount is largely offset by an *increase* in insurance demand following levee accreditation. This decomposition indicates that accredited levees, through insurance price decreases and changes to mandatory purchase requirements, do exhibit some complementarity with the flood insurance program. We also perform a back-of-the-envelope calculation to estimate the aggregate change of insurance spending following levee provision. We find that across both the extensive and intensive margins, levee provision crowds out $183,325 of insurance spending per levee-mile.

Our paper provides three main contributions to the literature on the economics of flood insurance. First, we provide the first empirical evidence of the causal impact of levee provision on flood insurance take-up, finding that household insurance take-up decreases by 20 percent following levee construction. Second, we compile the first known dataset of historic and effective levee accreditation dates, since accreditation timing information is not directly incorporated into the Army Corps' of Engineers' National Levee Database (NLD), which is the predominant source of administrative and spatial data regarding levees. Using National Flood Insurance Program rate maps and archived map revision documentation, we use a combination of manual and spatial methods to match map revision documentation to levees within the NLD database. Third, we provide the first empirical estimates of forgone household insurance spending after levee provision, adding to recent literature examining households' demand for flood protection (Bradt and Aldy [2023]; Wagner [2022]). Our paper quantifies the degree to which households substitute between demand for various flood policy instruments, and we estimate that a mile of levee provision crowds out $0.18 million of household insurance spending.

The closest work on the complementarity of various flood policy instruments studies the correlation between flood insurance take-up and other mitigation policies (e.g., land-use planning). Zahran et al. [2009] find a significant, positive correlation, while Atreya and Kunreuther [2016] find a non-significant, negative correlation. Kousky [2011], a study focusing on the general determinants of flood insurance demand using data from St. Louis, finds a negative correlation between flood insurance take-up and proximity to a levee, providing initial evidence that the two policies are substitutes. We expand upon this finding through both our data and empirical strategy, constructing a nationally representative dataset and leveraging variation in the precise timing of levee provision to analyze levees' impact on insurance take-up. Our empirical results align with those in Kousky [2011], as we find that flood insurance take-up decreases after levee provision.

Our paper also contributes to a broader literature of the mechanisms behind flood insurance demand by estimating demand responses to levee provision and distinguishing between demand changes due to levee construction and accreditation. Past work on this topic has considered the behavioral elements that help explain low take-up rate of flood insurance, relative to other types of insurance [e.g., Kunreuther, 1996]. Empirical work has shown that exposure to flooding, either directly [Atreya and Kunreuther, 2016, Kousky, 2017] or through media coverage [Gallagher, 2014], increases take-up, but only in the short-term. Qualitative evidence suggests that homeowners systematically misunderstand the residual risk of levees [Ludy and Kondolf, 2012]. Possibly due to these information frictions, the flood insurance program is chronically under-subscribed, despite mandatory purchasing requirements and increased awareness about local flood risk. For example, Bradt et al. [2021] estimates that in 2019, only 48 percent of households in 100-year floodplains held a flood insurance policy. If policymakers wanting to maximize community flood protection do not account for changes in insurance take-up following the provision of additional flood policies, they may mis-estimate the net contribution towards flood protection of the additional policy implementation. Hence, a deeper understanding of how insurance demand is affected by different flood policy instruments is crucial.

Within the broader public finance literature, our study provides empirical estimates of crowding out of household-level risk transfer take-up by public infrastructure investments. Influential work in the crowd-out literature has focused on policy contexts in which public-sector health insurance expands and crowds out private-sector health insurance [Cutler and Gruber, 1996a]. In the realm of natural disaster insurance, there has been some research studying how other risk transfer policies, such as *ex-post* aid provision, may reasonably crowd out household-level flood insurance take-up. Kousky et al. [2018c] finds that *ex-post* disaster aid reduces flood insurance coverage in subsequent years. It is also possible that other risk transfer mechanisms crowd out households' take-up of flood insurance. For example, Liao and Mulder [2021] study how home equity affects the demand for flood insurance, by leveraging cycles in the housing market as identifying variation, they find that the option of defaulting on a mortgage is a risk transfer mechanism that crowds out insurance take-up. Our findings will have implications for other policy contexts with uncertainty, including policies to mitigate damages from wildfires and other types of natural disasters.

The rest of the paper proceeds as follows: Section 1.2 describes the context and background of each of the two policies. Section 1.3 describes the data. Section 1.4 introduces the empirical strategy and Section 1.5 presents the estimates of the effect of levee provision on flood insurance take-up. Section 1.6 outlines a back-the-envelope exercise to calculate the aggregate crowd-out of insurance spending from levee provision. Section 1.7 concludes.

1.2 Background on Flood Risk and Mitigation

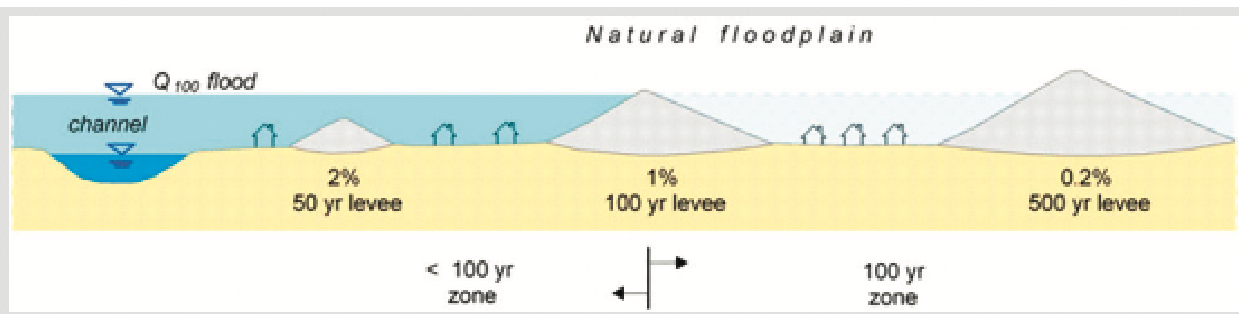
A levee is a "man-made structure, usually an earthen embankment, designed and constructed in accordance with sound engineering practices to contain, control, or divert the flow of water so as to provide protection from temporary flooding" [CFR, 44, §59.1]. Levees are an ancient technology that protect subsets of floodplains, known as "leveed areas," from a certain degree of flood damage by providing physical barriers to floodwaters. Construction of a levee is a complex process, involving political capital and coordination, upfront monetary investment, and a plan to maintain the levee once construction is complete. Public or private actors fund and oversee the construction of a levee, and the ownership of the levee may be transferred during or after construction. Local funding of levees may be accomplished using debt mechanisms or tax revenue, including taxes collected through levee districts. Larger levee projects often require cost-sharing with state and federal funding sources, with costs ranging from millions to billions of dollars depending on the size and complexity of the levee [Keegan et al., 2011]. The timeline of funding, designing, and constructing a levee can be protracted, taking a few years or even decades.

Although levees do not completely eliminate flood risk, engineers design levees to protect against certain degrees of flood risk. Figure 1.1 illustrates the varying protective capabilities of levees against a 100-year flood.[1] Levees may protect against losses for up to the degree of flood risk for which they were built, but there is still residual flood risk possible in severe circumstances. In other words, a levee does not completely erase local flood risk: leveed areas are still susceptible to a certain amount of flood damage, if there is a flood that exceeds the protective capabilities of the levee. For example, a 50-year levee is not designed to protect against a 100-year flood, as depicted in Figure 1.1. In the event of "overtopping," when floodwaters breach the levee, it is possible for losses to match or even exceed the counterfactual losses in the state of the world where the levee did not exist. Flood damages in leveed areas are also possible if there is some failure of the levee either due to an error in its construction, a lack of maintenance, or an unanticipated failure of one component of the levee

1. This terminology refers to a flood that has a 1 percent, or one-in-hundred chance of occurring each year. This flood intensity is used as a benchmark for several flood insurance conditions. For example, accredited levees are certified to protect at least against 100-year floods.

system. Thus, levees *lower, but do not eliminate* expected flood losses for properties in leveed areas. This residual risk necessitates consideration of additional flood mitigation policies such as flood insurance [National Research Council, 2013]. Within the National Levee Database, 33 percent of levees have been screened to calculate the "annual exceedance probability" (AEP), which denotes the probability-level event which will maximize the protective capacity of the levee. For example, a levee with an AEP of 0.01 has a maximum level of protection against a 100-year flood event. Lower AEP values denote a higher degree of safety: if a levee is more protective, it will have the capacity to withstand a lower-probability, higher-intensity flood event [U.S. Army Corps of Engineers, 2022b, Wobus et al., 2019].

Figure 1.1: Illustration of Levees



Note: This illustration is reproduced from National Research Council [2013]. It shows that a 50-year levee would not protect against 100-year flood, while 100-year and 500-year levees would protect against higher-intensity, lower-probability events. The percentage listed denotes the "annual exceedance probability" (AEP) of the levee.

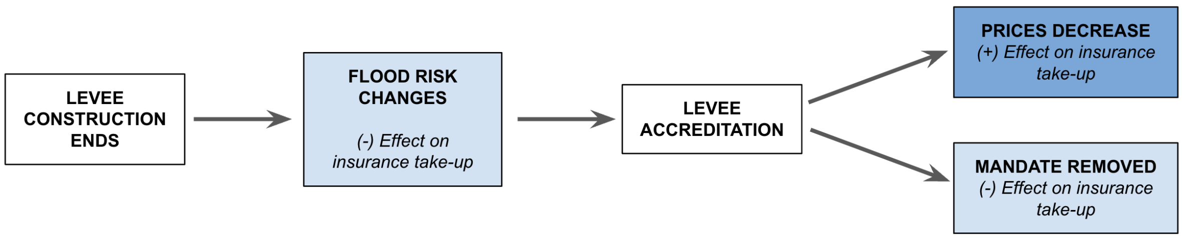
Flood insurance allows individuals to smooth consumption across states of the world, given that there is uncertainty of flood timing, frequency, severity, and damage. Unlike other types of insurance, the government, rather than a private market, is the primary provider of flood insurance [Kousky et al., 2018a]. Public flood insurance is available through the National Flood Insurance Program (NFIP), which the National Flood Insurance Act of 1968 established to protect homeowners, reduce expenditure on federal disaster aid, and share the risk more than was possible under a predominantly private insurance market [U.S.C., 42, §4001]. Due to the nature of flood events, there may be a large number of claimants concurrently filing for extensive damages, known as "correlated risk". Private insurance providers would not be able to maintain profitability in this context, whereas the NFIP does not aim for profitability and theoretically faces the very large borrowing limits of the federal government. In order to ensure the viability of the program, legislation requires that NFIP imposes mandatory purchase requirements for most properties holding a mortgage, and offers extensive subsidies. All of these features make the NFIP operate differently than private insurance providers [National Research Council, 2013], but provides a valuable context to study attributes of public insurance programs, such as insurance subsidies and mandatory purchase requirements.

FEMA operates the NFIP and sets insurance prices using Flood Insurance Rate Maps (FIRMs), which designate zones according to flood risk and property characteristics (e.g., structure age).[2] Floodplains with at least a 100-year flood risk are considered Special Flood Hazard Areas (SFHAs). The NFIP's mandatory purchase requirement decrees that all properties with federally backed mortgages in SFHAs purchase flood insurance, although enforcement varies across lenders and there is significant non-compliance. Bradt et al. [2021] estimates that less than half of properties in these high-risk SFHAs purchased flood insurance.

Levee provision serves as a valuable context to study homeowners' insurance take-up responses to public infrastructure investment. Notably, *construction of a levee alone does not necessarily or immediately change NFIP prices.* In order for a levee to reduce insurance prices or relax the mandatory purchasing requirement, the levee must undergo a process known as accreditation [U.S.C., 42, §4012a]. However, many levees never undergo accreditation: within the NLD, only 17 percent of levees are currently actively accredited (see Table 1.2).

2. Between 1968 and 1979, the Department of Housing and Urban Development (HUD) oversaw the NFIP until FEMA was established and took over operations [National Research Council, 2013]. Starting in 2021, FEMA implemented Risk Rating 2.0, changing the price structure to better account for residual risk and property-specific risk factors [Horn, 2019], including proximity to levees. For the purposes of this analysis, we will not consider these substantial changes to the pricing mechanisms of NFIP.

Figure 1.2: Timeline of Levee Construction and Accreditation



This setting provides us with a unique opportunity to observe homeowners' responses to the infrastructure construction *before* their insurance prices or requirements change, providing suggestive evidence of the initial household response to flood risk changes, and how their behavior evolves following insurance market changes. Studying household insurance take-up following levee accreditation provides suggestive evidence of additional household responses to the price and mandatory purchasing requirements, which we will refer to as "insurance market changes."

1.2.1 The Process of Levee Provision

We consider levee provision as a bundled treatment, changing various factors in tandem. Figure 1.2 depicts the timeline of levee provision, which includes levee construction and the accreditation process. The physical construction of the levee results in a change in inherent flood risk. Shortly thereafter, certain municipalities will undergo the process of accreditation, during which rigorous engineering analyses of the levee are conducted and reviewed by FEMA. The process of accreditation is initiated by municipalities and levee owners, and requires the input of engineers to sign off that the levee meets a strict set of criteria [Federal Emergency Management Agency, 2021]. After the levee is deemed to be accredited by FEMA, they issue an updated map, or a "Letter of Map Revision" (LOMR) that changes the flood zone in the region affected by the levee. Once this change takes effect, flood insurance prices are lowered and mandatory purchase requirements are relaxed. Figure 1.3 illustrates an example of these changes. On the left is the original flood map for a portion of Fargo, North Dakota. The highlighted area of the map depicts the geographic region that is considered a high-risk flood zone. On the right, the updated map in the LOMR illustrates the region that is now considered protected by the levee (the striped area). We consider levee provision to be a bundled treatment of these three separate effects, split across the construction and accreditation processes:

1. **Flood risk**: First and foremost, the levee will protect a leveed area against a certain degree of flooding, reducing flood risk for certain types of floods. Figure 1.1 illustrates this point, depicting how a levee could protect against floods of a particular magnitude, but not others. *A levee will reduce overall flood risks, reducing insurance take-up.*

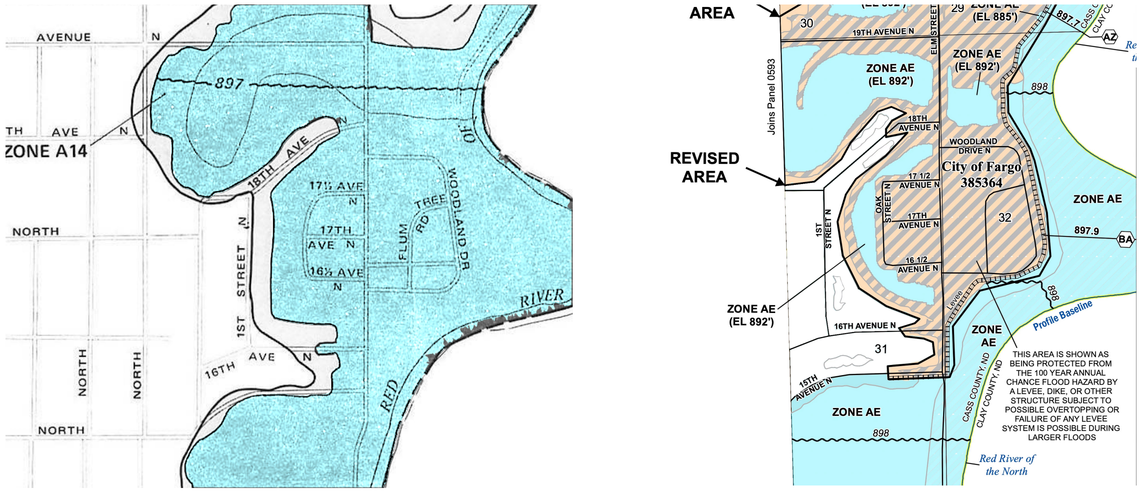
2. **Prices**: After the levee is accredited, insurance prices will decrease, reflecting the changes in expected damages from flooding. *A levee will reduce insurance prices, increasing insurance take-up.*

3. **Mandate**: After the levee is accredited, insurance requirements within a leveed area will be relaxed. This will lower the costs of not being insured for homeowners, since they no longer have the threat of flouting the mandate if they decide not to purchase insurance. *A levee will reduce insurance mandate requirements, reducing insurance take-up.*

Due to these three competing effects, the direction in which levee provision will affect flood insurance take-up is ambiguous. Through our analyses, we will first study the dynamics of insurance take-up across the construction date using an event study regression, which will estimate the effects of risk changes on insurance take-up rates. Next, a event study regression using the accreditation date as an alternate definition of treatment will pick up

Figure 1.3: Example Flood Map in Fargo, ND, Pre- and Post-Accreditation

(a) Pre Accreditation (b) Post Accreditation



Note: These two images depict a portion of Fargo, North Dakota before and after the levee accreditation process. On the left is the original flood map from 1978. The blue area denotes the Special Flood Hazard Area (SFHA) of the region. On the right is the updated flood map from 2015, following the accreditation of the levee (denoted by the black striped line towards the right of the frame). The gray-and-tan striped area denotes the geographic region that is now considered reduced risk (Zone X) due to the protective capabilities of the levee. This updated map was published as part of a "Letter of Map Revision" (LOMR), following the accreditation process.

the net effects of price and mandate changes. We further elucidate the accreditation effect with an additional specification that controls for the construction effect *while* estimating the accreditation effect, which will help us estimate the specific effect of the price and mandate changes, after controlling for trends caused by the preceding construction event.

1.3 Data

We collect and combine several data sources listed in Table 1.1 into a panel dataset that includes information about levee location, construction and accreditation dates, insurance policy counts, and housing and population characteristics. All levee information is drawn from The National Levee Database ("NLD"). The US Army Corps of Engineers (USACE) actively maintains and updates the NLD. [NLD; U.S. Army Corps of Engineers, 2022b];[3] The

3. Our data extract was most recently updated in May 2023.

NLD contains the end date of construction, current accreditation status, and information on USACE oversight. Table 1.2 includes data availability and covariate averages for several characteristics included in the NLD. Additionally, the NLD contains detailed spatial data which characterize (1) the location of the levee system and (2) the boundaries of the leveed area, as determined by a number of engineering analyses, and reviewed regularly by local districts. Only 24 percent of levees in the NLD have a construction date listed. Since we require information about the precise timing of construction for our empirical strategy, the levee sub-sample used in our construction date analysis consists of 165 levees. Additionally, 17 percent of all levees are listed as currently being accredited, indicating that the process of accreditation is relatively infrequent and not guaranteed for all levees following construction. Additionally, the levees in our analyses tend to have a lower annual exceedance probability (Table 1.2), meaning that they have higher protective abilities than the typical levee in the general NLD. Thus, we expect that our estimated treatment effect would serve as an upper bound of the effect of levees on insurance take-up, when compared to the larger levee population. This direction of our bound rests on the assumption that households would react more strongly to levees with higher protective capabilities.

The dates of levee accreditation are not included in the National Levee Database. We use detailed documentation from the FEMA Map Service Center to identify and match areas with levees that received a "Letter of Map Revision" (LOMR) from FEMA. These documents include information about the date the revision went into effect, which we use to identify the year of accreditation. In order to ensure that the map change is specifically due to the levee, we use OCR software to identify every LOMR that includes verbiage about levees and/or levee certification, and then map each LOMR to its corresponding levee in the NLD. For LOMR documents that are still considered effective, we are able to use digital methods to match the levee to the document, and thus the accreditation date. This is because the National Levee Database includes identifying information linking levee systems to effective

Table 1.1: Summary of Data Sources

Data	*Source*	*Relevant Variables*
Levee characteristics	National Levee Database	Levee geography, protected leveed area geography, year constructed
Accreditation information	FEMA Map Service Center - Letters of Map Revision	Accreditation dates and geographic information about map revisions
Insurance policies ('09-'22)	OpenFEMA Data Portal	Policy counts and average insurance price data
Insurance policies ('80-'07)	Gallagher [2014]	Policy counts
Housing and population metrics	IPUMS; FRED; SEER	Housing units, personal income, Case-Shiller housing price indices, population
Property-level spatial data	CoreLogic Tax Data	Property longitude and latitude information
NFIP Claims	OpenFEMA Data Portal	Adjusters' estimates of building and contents damages

flood map documentation, such as LOMR documents. However, for LOMR documents that are considered “historical", and are no longer effective, we do not have systematic identifying information linking them to particular levee systems. Therefore, we use manual methods to link levee systems to the LOMR documents, using maps available at the end of every LOMR document that show the precise location of the levee system, and matching them up with the maps available in the National Levee Database. In order to cross-validate this manual matching method, we manually matched some of the effective LOMR documents, and compared the matches to our digital match results.

Figure 1.4 displays the location of the population of levees in the continental US, demonstrating the broad geographic range of levees in the US. Our sub-sample of analysis includes 165 levees within the construction event study analysis, and 99 levees within the accreditation event study analysis. Table 1.2 summarizes some of the available information on levees and describes our sample of analysis. Relative to the full population of levees, our study sub-samples contain more currently accredited levees, but similar sized levee systems. The leveed areas of the sub-samples are on average smaller than those of the full population. Regarding accreditation, our data collection of accreditation dates helps contribute to better understanding of the ever- versus currently-accredited population. For example, in the sub-sample of levees used in the accreditation date analyses, the vast majority (87.9 to 94.9 percent) of those levees are still classified as accredited on the National Levee Database. Although levees can lose their accreditation status if they are not properly maintained, we see that the majority of the levees that we document as ever-accredited, are still currently-accredited.

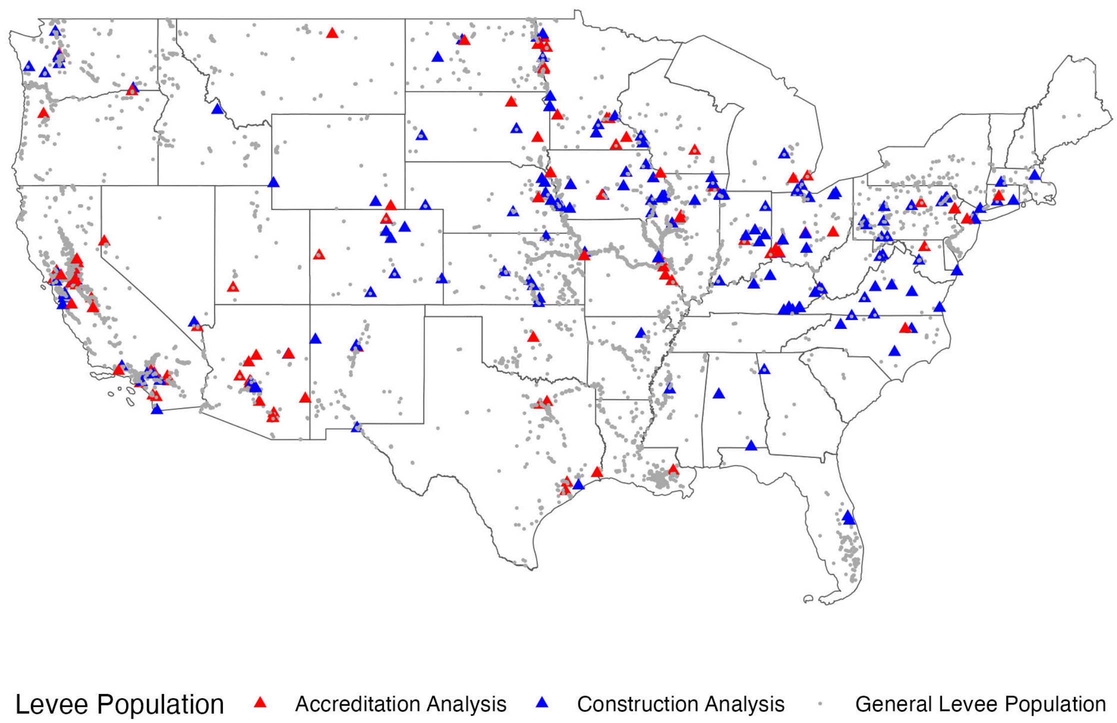
FEMA publishes the NFIP Policies dataset through OpenFEMA [OpenFEMA, 2022]. The data contain information on the universe of flood insurance policies since 2009, with a selection of earlier observations going back to 1978. From these data, we can observe details of policy prices. The data are at the policy level, although the only available geographic identifiers are at the Census tract or ZIP code level. Before 2009, the policy data only include policies that were still in effect as of 2009 or later. Due to this constraint, we do not know the full volume of insurance policies prior to 2009, and thus we limit our Census tract-level analysis to 2009 to 2022. Since the average construction date of levees in the NLD is 1965, it is helpful to be able to study levee construction by considering insurance policy take-up earlier than 2009. Therefore, we supplement the OpenFEMA data with data from Gallagher [2014] that include policy counts data for NFIP at a Census place/NFIP community level, from 1980 to 2007. Since this dataset provides the longest possible time horizon of flood insurance policy counts, we also aggregate the Census tract data to a Census place level and append it to this dataset, forming a panel ranging from 1980 to 2022. Census tracts are the smallest possible geographic unit, so a higher percentage of the unit is within the leveed area. However, having a longer time horizon, as we do in the Census place-level data sample, allows us to study levees built in the 1980s and 1990s, significantly expanding the breadth of levees in the study sample. See Table 1.3 for a summary of these data sources.

Table 1.2: Summary Statistics of Levees within Full Population and Study Sub-samples

		Census Place			*Census Tract*
	All Levees	Construction	Accreditation	Both	Accreditation
Levee Count	6878	165	99	40	59
System Length (miles)	3.38	3.12	2.22	3.17	2.07
% Availability	98.65	96.97	92.93	97.5	93.22
Leveed Area (sq.miles)	9.21	4.7	1.93	3.42	2.01
% Availability	100	100	100	100	100
AEP	0.0258	0.0094	0.0024	0.0026	0.0018
% Availability	32.55	87.88	54.55	92.5	52.54
Construction Year	1965	1995	1997	1998	1995
% Availability	24.25	100	38.38	100	35.59
Accreditation Year	2009	2008	2007	2006	2015
% Availability	3.01	18.79	100	100	100
Current Accred. Status					
% Accredited	17.24	48.48	87.88	90	94.92
% Not Accredited	75.62	36.36	8.08	0	1.69
% Other	4.57	12.73	4.04	10	3.39
% Unknown	2.57	2.42	0	0	0
Managed by USACE					
% Yes	23.07	72.73	40.4	72.5	40.68
% No	76.93	27.27	59.6	27.5	59.32

Note: This table provides total counts and average characteristics for the levees in the full population of levees ("All Levees") documented in the National Levee Database (NLD), as well as several study sub-samples. The sub-samples are divided into two categories: sub-samples used in the main Census place-level specifications, and the sub-sample used in the Census tract-level specification. The column header "Construction" refers to the event study and difference-in-differences regression analysis using only construction end date as the definition of treatment (see Table 1.4), and the column header "Accreditation" refers to the event study using accreditation date as the definition of treatment. The column header "Both" refers to the additional specification in Table 1.5, where event time for both construction and accreditation is considered. The final column refers to the levees used in the Census Tract-level accreditation analysis. Since the Census tract insurance policy data only starts at 2009, this sub-sample includes more recently accredited levees. *Levee Count* provides the number of levee systems included in the sample or sub-sample. *System Length* is the average length in miles of the levee system, which may comprise of one of many segments. *Leveed Area* is the average square mile area that is protected by the levee. *AEP* refers to the "annual exceedance probability", or the annual chance that the levee will be breached by floodwaters. *Current Accred. Status* refers to the current accreditation status of levees in the NLD. *Managed by USACE* refers to the levees that are regularly maintained and overseen by the Army Corps of Engineers. Some of the variables have limited availability in the National Levee Database. For those variables, we also present the percent of availability for that particular variable underneath the summary statistics.

Figure 1.4: National Distribution of Levee Locations



Note: The gray points indicate the locations of all 6,878 levees documented in the National Levee Database (NLD). The red triangular points represent the locations of the 165 levees utilized in the construction date analysis, and the blue triangular points represent the location of the 128 levees used in the accreditation date analysis. Throughout each of the analyses, we only consider levees within the contiguous US. All 48 contiguous US states and the District of Columbia are represented in the NLD, and the construction and accreditation analysis sub-sample includes levees from 42 states.

Additionally, we obtain demographic information from the sources listed in Table 1.1. We use county-level information on housing prices [Federal Reserve Economic Data, 2022], income [Federal Reserve Economic Data, 2022], and population [Surveillance, Epidemiology, and

Table 1.3: Matrix of Flood Insurance Policy Data Sources

Source	*Geographic Unit*	*Years*	*Available Variables*
Gallagher [2014]	Census Place	1980-2007	Policy counts
OpenFEMA	Census Tract	2009-2022	Policy counts, purchased insurance prices

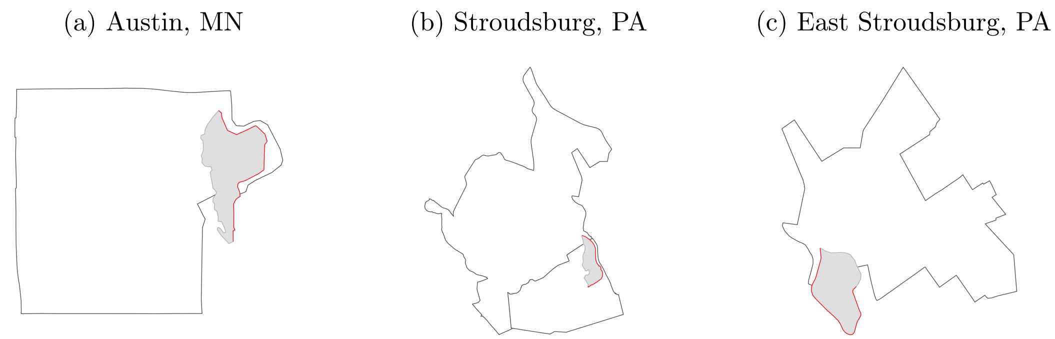
Note: This table lists two separate sources of NFIP policy data used in the empirical analyses of this paper. Although the OpenFEMA public-access dataset includes a rich number of characteristics including purchased insurance prices, coverage amounts, and more, the data is only fully available from 2009 onwards. Due to the limited number of levees with known construction dates past 2009, it is not possible to precisely estimate the effect of levee construction on insurance take-up using just the OpenFEMA data. Therefore, we supplement our analysis with the replication data from Gallagher [2014], which provides policy counts at a Census place (community) level. We aggregate the post-2009 OpenFEMA data to a Census place level using geographic information on Census tracts that is available in the OpenFEMA database.

End Results Program, 2022] for summary statistics and falsification tests. We standardize our metrics for policy volume by relying on estimates of the number of housing units from IPUMS [Ruggles et al., 2022] and the Historical Housing Unit and Urbanization Database [HHUUD; Markley et al., 2022]. This information is available once per decade and we linearly interpolate between decennial Census years to obtain estimates for the interim years. This allows us to construct a “policy take-up rate", by calculating the number of policies per housing unit for a given geographic unit. We use this rate as our primary outcome of interest.

We define treated geographic units as those that spatially overlap with leveed areas. Figure 1.5 demonstrates the procedure of defining treatment from three different levee systems on geographic units. Since the treatment definition is broader than that of the geographic units, our estimated treatment effect will include insurance take-up behavior of both directly protected households immediately within the protected “leveed area", and households outside of the leveed area, for whom the protection benefits are uncertain.

Our main specification includes results from the Census Place population, since that constitutes the longest possible panel data, spanning 42 years. Within that time frame, we find that 195 Census places were treated by 165 constructed levees, and 114 Census places

Figure 1.5: Examples of Geographic Unit Treatment Assignment



Note: This figure shows the treatment definition used for geographic units in our sample. We demonstrate the method using three sample Census tracts. In some specifications, we use Census places as well. Each spatial component is denoted by a black outline and represents a Census tract. The levee system is demarcated by the red line, and the gray area indicates the leveed area, as defined within the National Levee database. We define a geographic unit as treated if it overlaps at all with the protected leveed area of a levee system.

were treated by 99 accredited levees. However, very little additional information about the attributes of these insurance purchases is known at the Census place level. Therefore, we supplement the analysis with a study of changes of purchased insurance prices before and after levee accreditation at a Census tract level, which includes 148 tracts treated by 59 levee systems. Table 1.6 shows the results of this analysis.

1.4 Empirical Design

1.4.1 Identification Strategy

The purpose of our empirical analysis is to identify the causal effect of levee provision on household insurance take-up. A primary threat to the naive comparison of policy take-up before and after levee provision is that levee provision may be correlated with a number of characteristics that also affect insurance take-up. For example, if we simply measured the correlation between levee provision and insurance take-up, we would likely find a positive relationship, since both levee provision and insurance take-up are positively correlated with the inherent flood risk of an area and local household income.

To avoid this threat to identification, we leverage variation in the exact timing of levee construction and levee accreditation within a difference-in-differences regression design. The identifying variation of this empirical strategy is that the *precise timing* of levee provision is uncorrelated with characteristics that could affect insurance take-up decisions. We limit the current analysis to only ever-treated geographies, since locations with and without levees may differ substantially on a number of observable and unobservable characteristics. For example, geographic units that have levees may have greater availability of resources to fund expensive infrastructure projects, indicating different trends in population or household income than geographic units without levees.

The specific methodology we use closely follows the "stacked difference-in-differences" method used in Deshpande and Li [2019]. This approach avoids the issue of comparing geographic units experiencing levee provision to those that have *already* experienced levee provision, which is known as the "negative weights" issue (De Chaisemartin and d'Haultfoeuille [2020]). This is done by comparing treated units to a sample of "not yet treated" geographic units, which are places that eventually receive a levee but have not yet done so in the defined period when the dynamic treatment effects are estimated. It is important to note that this method makes the assumption that geographic units treated in different years by different levees have homogeneous treatment effects.

First, we define a "levee cohort" of geographic units treated by a particular levee l. For each cohort l, we define a set of "not-yet-treated" control units, which consists of other geographic units that were treated four or more years after the cohort. This allows us to compare treated units to a control group that was not treated until after the observed post-trend. The treatment effect is estimated by calculating the difference in outcomes between treated and control units, before and after the treated cohort was treated, and averaging the estimated effect across all levee cohorts. We cluster our standard errors at a levee cohort level l. We perform this analysis by estimating the following event study specification, for geographic unit i, levee l and year t:

$$Y_{ilt} = \phi_i + \gamma_t + \delta_0 Treated_{il} + \sum_{\tau} \alpha D_{ilt}^{\tau} + \sum_{\tau} \left(\delta_\tau Treated_{il} \times D_{ilt}^{\tau}\right) \tag{1.1}$$

Our primary outcome of interest is the flood insurance take-up rate, Y_{ilt}, which we calculate by dividing the number of policies in i and year t by the estimated number of housing units in the geographic unit ilt. $Treated_{il}$ takes a value of 1 for the treated units for the particular levee cohort l, and 0 for the not-yet-treated geographic units, which includes any unit that did not receive a levee within 3 years of levee l's construction or accreditation date. The term D_{ilt}^{τ} is a binary indicator for whether a particular unit-year observation it in levee cohort l is in event time τ. Event time is defined by the number of years between the calendar year t and the year of levee provision for levee cohort l. For example, if levee l was constructed in year 2004, then an observation in calendar time $t = 2005$ would take the value $D_{il,t=2005}^{\tau=1} = 1$. Positive event time (i.e., positive values of τ) includes observations in years *after* the levee has been constructed or accredited, and negative event time includes observations in years *before* the levee has been constructed or accredited. Therefore, the parameter of interest δ_τ estimates the difference in average insurance take-up rates between treated and not-yet-treated geographic units at different event times τ. In the event study regression centering around the construction timing, δ_τ measures the take-up response to the flood risk reduction resulting from levee construction. In the event study regression centering around the accreditation timing, δ_τ measures the take-up response to the price and mandate changes.

For both specifications, we consider $\tau \in \{-3, 3\}$, estimating changes to insurance take-up three years before and after the levee construction or accreditation event. We also include a top-coded and a bottom-coded element of τ to control for any event time years beyond the range of $[-3, 3]$. Importantly, we limit our data sample to levee cohorts that are balanced on event time, only including cohorts for which we can observe three years before and after the levee construction or accreditation. [4] Finally, our specification also includes geographic unit- and year- fixed effects.

To capture the static treatment effect, aggregated over event time for three post-periods used in the event study specification, we additionally estimate the following static difference-in-differences regression, where the coefficient of interest, δ, denotes the average change in household insurance take-up across the three years following levee provision. The point estimate from the TWFE estimation is represented by the blue horizontal line on the event study graph in Figure 1.6, and presented in Table 1.4.

$$Y_{ilt} = \phi_i + \gamma_t + \delta_0 Treated_{il} + \sum_{\tau} \alpha D^{\tau}_{ilt} + \delta Treated_{il} \times Post_{lt} \tag{1.2}$$

Our primary analyses focus on Census place as the level of geographic unit i, due to the broad range of years with available insurance policy take-up information. We supplement our accreditation date analysis with Census tract-level results, since the geographic granularity of the Census tract allows for a larger range of levee accreditation events to be included. It also allows us to study a greater variety of insurance-related characteristics, such as purchased insurance prices. Table 1.3 provides a summary of the data availability for each sample, and Table 1.2 documents the differences between the levees included in the Census place construction, accreditation, and joint analyses, as well as between the Census place- and Census tract-level accreditation analyses.

4. Appendix Figure A.1 presents the results from the event study specification when we estimate event time coefficients for $\tau \in \{-5, 5\}$.

1.5 Results

Our primary set of results consists of the empirical estimates of the stacked difference-in-differences regression methodology. We first present results from the dynamic event study (Equation 1.1), first centering the timing of levee provision around the time of construction and then around the timing of accreditation. Next, we estimate the static difference-in-differences specification (Equation 1.2) for construction timing. We then present estimates from a joint specification (Equation 1.3), that allows us to comment on the isolated impact of accreditation timing on insurance take-up, after controlling for construction timing. Finally, we comment further on the impact of accreditation timing on auxiliary outcomes such as purchased insurance prices.

1.5.1 Construction Date Analysis

The first set of results estimates the specification in Equation 1.1, separately estimating the impact of levee construction and of accreditation. In Figure 1.6, we present the estimates of the δ_τ coefficients from Equation 1.1 for levee construction. These coefficients are estimated on a subsample of Census places for which we viewed construction date, as well as event time for both three years prior and following the construction event. This data stipulation of balancing our sample on event time allows us to compare coefficients across event time, since each coefficient was estimated on the same Census place sample.

We find that prior to construction, there is no significant difference in policy take-up trends between treated- and not-yet-treated Census places. However, following construction, treated Census places decrease their policy take-up compared to the not-yet-treated control group. We interpret this estimate as evidence that levee construction causes policy take-up to decrease by 0.5 to 1.8 percentage points. This estimate is net of accreditation effects, since we are not disaggregating or controlling for eventual accreditation. Therefore, this decrease represents households' net response to changes in risk, prices, and mandates.

Since the construction event study results presented in Figure 1.6 provide sufficient evidence of the parallel trends assumption being satisfied, due to the lack of substantial pre-trends in insurance take-up in the years prior to levee construction, we further analyze levee construction using a static difference-in-differences specification (Equation 1.2).[5] In Table 1.4, we present the aggregated treatment effect β from this specification. We find that in aggregate, levee construction causes a decrease in insurance take-up of 1.6 percentage points. On a baseline *ex-ante* take-up rate of 8 percentage points, this corresponds to a 20 percent decrease in insurance take-up. The result from the static specification is similar to the range of treatment effect we estimated from the event study specification. We demonstrate the consistency of these estimates by demarcating the static estimate with a blue dashed line on the event study coefficient plot in Figure 1.6.

Table 1.4: Static Estimates of the Effect of Construction on Flood Insurance Take-up

	Dependent variable:
	Policy Take-up Rate
Post Construction	-0.016^{**}
	(0.008)
Observations	8,190
Baseline Mean	0.08

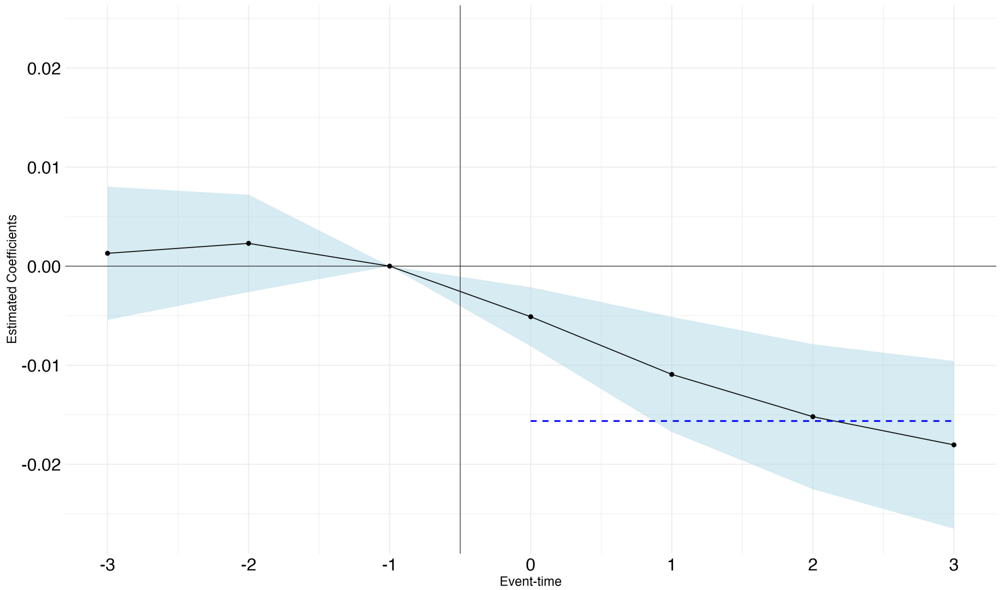
Note: This table presents the coefficient estimates of δ from Equation 1.2. This specification is an static version of the dynamic event study difference-in-differences specification from Equation 1.1. Instead of separately estimating the treatment effect for each positive event time value, this specification pools the positive (post-event) event time into one δ coefficient. We find results that are similar in magnitude to the results of the event study presented in Figure 1.6. Statistical significance is represented by the following notation: *p<0.1; **p<0.05; ***p<0.01.

1.5.2 Accreditation Date Analysis

We next separately estimate the effect of levee accreditation on our Census place population. The results from the event study specification of accreditation dates are presented in

5. Further evidence of the parallel trends assumption being satisfied is presented in a series of falsification tests in Section 1.5.5.

Figure 1.6: Event Study Estimates of the Effect of Construction on Flood Insurance Take-up

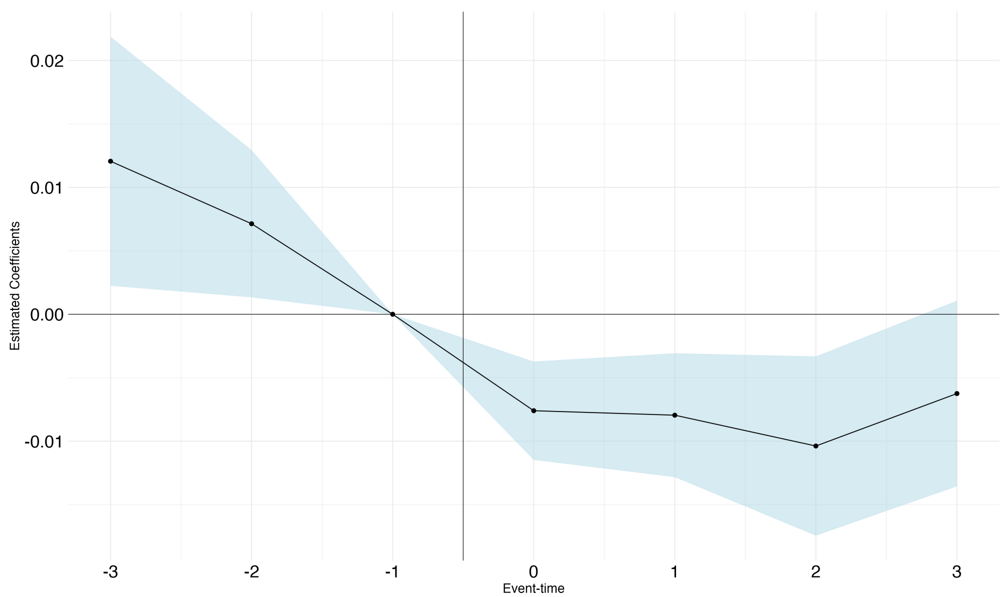


Note: This figure plots the coefficient estimates on each $Treated \times D^{\tau}$ variable from Equation 1.1, using construction year as the source of variation. The x-axis plots event time, where '0' denotes the year of construction ending, negative event-time denotes the years prior to construction, and positive event-time denotes the years following construction. The single point denotes the point estimate of the coefficient, and the vertical bar denotes the 95 percent confidence interval. All event study specifications are estimated using a data sample that is balanced in event-time, so all geographic units have policy take-up information for the full range of event time: [-3,3]. The red line depicts the coefficient estimate on the $Treated \times Post$ variable from static difference-in-differences regression in Equation 1.2. The baseline insurance take-up rate for this specification is 8 percent, so our static estimate of 1.6 percentage points corresponds to a 20 percent decrease in baseline flood insurance take-up.

Figure 1.7. Following levee accreditation, treated- and not-yet-treated Census places have a difference in policy take-up ranging from 0.7 to 1.1 percentage points, which corresponds to a 3.6 to 5.4 percent decrease in flood insurance take-up. Here, we see a statistically significant pre-trend: even prior to accreditation, treated- and not-yet-treated Census places have differing trends in policy take-up. This pre-trend is in line with our understanding of the construction-accreditation timeline, as documented in Figure 1.8, which shows that more than 65 percent of levees with known construction and accreditation dates are accredited within 5 years of construction. Therefore, it is likely that the pre-trend observed in the accreditation date event study is affected by the residual decrease in insurance take-up following levee construction, and in this specification, we do not control for the construction effect. However, we can qualitatively assess the potential impact of accreditation by observing the change in the trend of insurance take-up within the event study specification. What

we see is that immediately following the levee accreditation, we observe a change in the slope of the trend, indicating that the price and mandate changes precipitated by accreditation alter the existing negative take-up trend caused by levee construction. In order to quantitatively assess this accreditation effect, we conduct an additional analysis in Section 1.5.3 where we estimate the accreditation effect, *after controlling for construction.*

Figure 1.7: Event Study Estimates of the Effect of Accreditation on Flood Insurance Take-up

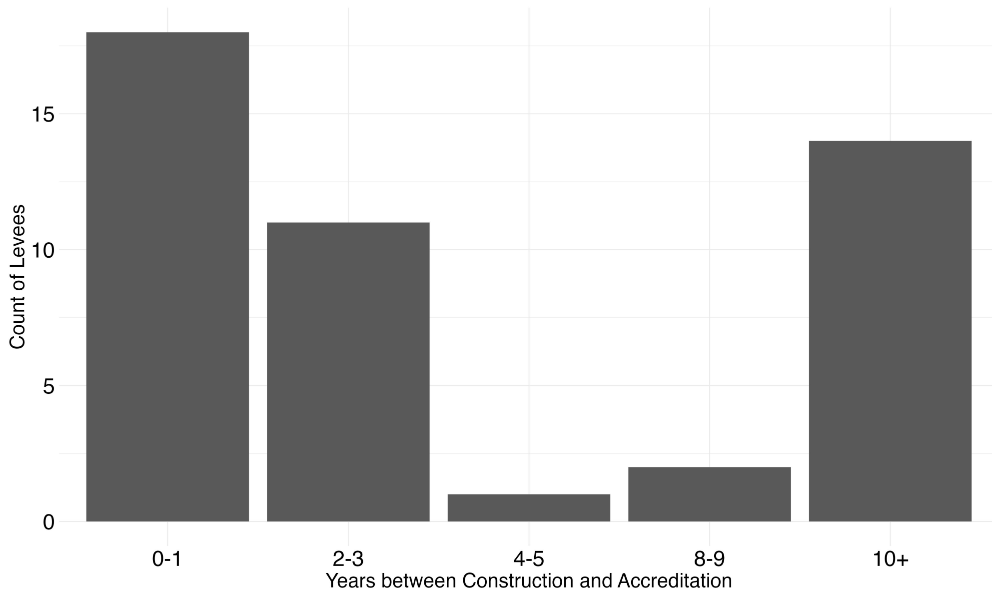


Note: Similar to Figure 1.6, this figure plots the coefficient estimates of Equation 1.1 on $Treated \times D^{\tau}$, now using the accreditation date as the source of timing variation. We do not include a static estimation, since the pre-trend in the accreditation event study indicates that it is likely that the parallel trends assumption of the difference-in-differences identification strategy is not valid. We attribute the pre-trend to the impact of construction in the years preceding accreditation, and we observe that the timing of accreditation coincides with a change in that negative trend of insurance take-up. In order to test this assumption, we estimate a joint specification, presented in Table 1.5.

1.5.3 Joint Levee Provision Analysis

In this section, we provide quantitative evidence of the individual accreditation effect, through an analysis of the staggered timing of construction and accreditation. Since we have limited data on both construction and accreditation dates, the sample of levees used this auxiliary accreditation analysis is different from the sample of levees in the prior analyses. Here, we use a sub-sample of levees for which we have both a construction and accreditation date.

Figure 1.8: Histogram of Number of Years between Construction and Accreditation Events



Note: For the levee sub-sample for which we have both construction and accreditation dates, we plot the distribution of time in years between the two dates. From this histogram, we see that more than 50 percent of the levees in the sub-sample were accredited within 5 years of construction.

We use a modified difference-in-differences specification, similar to an analysis used in Deshpande et al. [2021]. This specification is similar to that of Equation 1.1, but includes controls for both construction event time ($\sum_{\tau_C} \alpha_c D_{ilt}^{\tau_C}$) *and* accreditation event time ($\sum_{\tau_A} \alpha_A D_{ilt}^{\tau_A}$), allowing us to estimate the isolated impact of each construction and accreditation, holding constant the event time of the other event. Since there is variation in the amount of time between construction and accreditation, we use this variation to estimate the effect of accreditation timing on insurance take-up, after controlling for construction having occurred. Figure 1.8 plots the distribution of the duration between construction and accreditation, for the full sub-sample of levees for which we observe the construction and accreditation dates. From this distribution, we see that more than 50 percent of the levees with known dates have a gap of 5 years or less. Therefore, we expand our balanced data sample to include all possible geographic units that (1) had both construction and accreditation dates available and (2) had information on policy take-up rate for the event time range of [-5,5]. This expansion from the time range of [-3,3] in our previous difference-in-differences analyses allows us to observe longer post-trends after the construction and accreditation events, allowing for a more precise estimate of each distinct effect.

$$
\begin{aligned}
Y_{ilt} = \phi_i + \gamma_t + \delta_0 Treated_{il} + \sum_{\tau_C} \alpha_{\tau_C} D_{ilt}^{\tau_C} + \sum_{\tau_A} \alpha_{\tau_A} D_{ilt}^{\tau_A} \\
+ \beta_C Treated_{il} \times PostCons_{lt} + \beta_A Treated_{il} \times PostAcc_{lt}
\end{aligned}
\tag{1.3}
$$

Equation 1.3 shows the amended specification, modeling both construction and accreditation. The treatment effects of interest are represented by the coefficients β_C and β_A. The coefficient β_C estimates the difference in outcomes between treated and not-yet-treated units, before and after construction, controlling for accreditation. The coefficient β_A estimates the additional difference in outcomes between constructed/accredited and constructed/not-yet-accredited units. Our specification combines all non-negative construction and accreditation event time units into *PostCons* and *PostAcc* binary variables, respectively. We perform this aggregation to increase the power of our construction and accreditation effects in this specification, since the pool of levees for which we have *both* construction and accreditation timing information is far smaller than the levee populations used in the separate construction and accreditation specifications (Equation 3.1). In Appendix Table A.1, we present estimates from an alternate specification of Equation 1.3 where we estimate each distinct event time coefficient. As we expected, the results are not statistically significant, but the sign and magnitude closely align with the results of Equation 1.3.

The empirical estimates for β_C and β_A are presented in Table 1.5. The results are in line with our findings from the estimation of Equation 1.1. We find that even after accounting for accreditation timing, construction causes a decrease in insurance take-up, although this decrease is larger in magnitude, amounting to a 60 percent decrease in take-up compared to baseline. Consistent with the qualitative trend depicted in Figure 1.7, we find that accreditation partially offsets the initial decrease in insurance take-up following from construction, and there is a positive take-up in insurance, of 40 percent of baseline. This is consistent with the hypothesis that of the opposing price and mandate effects, the positive price effect on insurance take-up prevails. If the removal of mandatory purchase requirement has zero effect on insurance demand, then our estimate will accurately represent the effect from the isolated price channel. If the removal of the mandatory purchase requirement actually decreases demand, then our estimate serves as an *lower bound* for the effect on demand through purely the price channel. Since baseline take-up of flood insurance remains low throughout the US, with below 50 percent estimated take-up rates among high-risk flood zones, then it is possible that the mandatory purchase requirement is not strongly enforced, and subsequently, a removal of the requirement would not have a strong impact on insurance demand.

Our net effect of construction and accreditation amounts to a 16 percent decrease in insurance take-up from baseline, which is similar in magnitude to our estimate from the construction-only static difference-in-differences specification in Section 1.5.1, which found a 20 percent change in take-up following construction.

Table 1.5: Census Place-level Estimated Treatment Effect for Combined Specification

	Dependent variable:
	Policy Take-up Rate
Post Construction	-0.032^{*}
	(0.017)
Post Accreditation	0.024^{*}
	(0.014)
Observations	1,680
Baseline Mean	0.051

Note: This table presents the coefficient estimates of δ from Equation 1.3. In this specification, we jointly model construction and accreditation within the same specification, in order to estimate the effect of levee accreditation while holding constant the effect of construction. This specification is a modification from the static version of the dynamic event study difference-in-differences specification from Equation 1.2. Statistical significance is represented by the following notation: *p<0.1; **p<0.05; ***p<0.01.

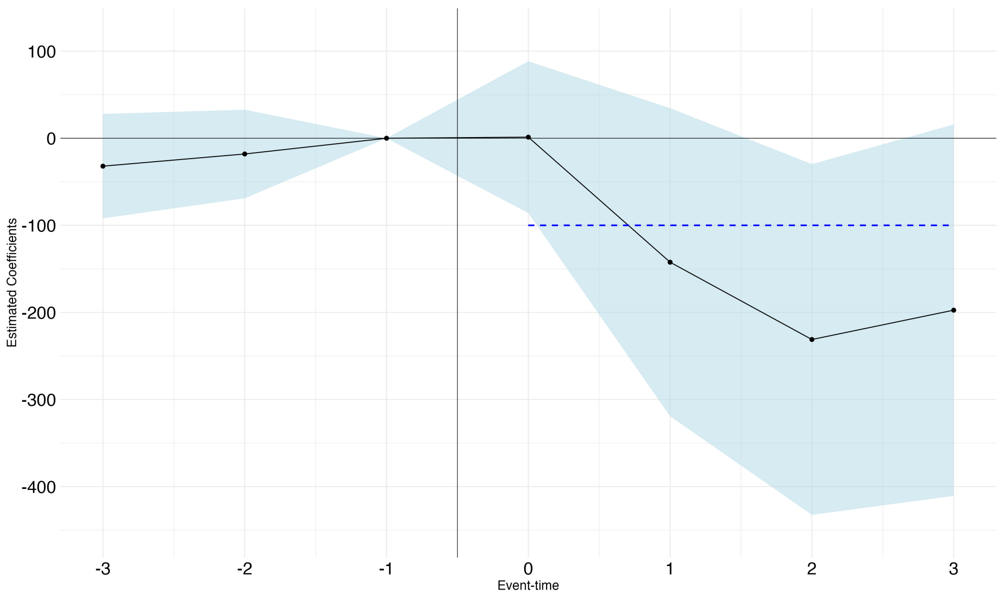
1.5.4 Auxiliary Analysis

In order to further understand the price channel of the accreditation date effect, we conduct an auxiliary analysis of levee accreditation using our Census tract sample. The broader availability of insurance premium information within the Census tract sample, which is sourced solely from the OpenFEMA database, allows us to measure whether prices are indeed responding to accreditation. Estimating Equations 1.1 and 1.2 using purchased insurance prices as the outcome of interest, and the accreditation date as the treatment of interest, clarifies how accreditation affects the insurance prices. We find that in the years immediately preceding levee accreditation, there is very little change in the averaged purchased prices. This is in stark contrast with the event study estimates presented in Figure 1.7, where we saw that policy take-up significantly varied in the years preceding levee accreditation. In the one to three years following levee accreditation, we see that purchased insurance prices decrease by $100.03, or 10 percent of the baseline insurance rates. We take this result to indicate that accreditation is indeed the sole channel by which insurance prices change due to a levee, consistent with the dynamics outlined in the conceptual framework from Figure 1.2. We account for this reduced spending within our aggregate spending calculation in Section 1.6. Not only do households avert spending on the extensive margin by dropping their flood insurance altogether, they also reduce spending on the intensive margin through these lowered prices.

1.5.5 Falsification Tests

A concern to our causal identification strategy is that the precise timing of the levee provision may be correlated with other contemporaneously changing characteristics, such that it is unclear whether the levee or the other changing characteristics are causing the perceived change in insurance take-up. For Equation 1.2, in order for β to causally identify the average treatment effect on the treated, we must assume that the parallel trends assumption holds.

Figure 1.9: Census Tract-level Event Study Estimates of the Effect of Accreditation on Purchased Insurance Prices



Note: We plot the $Treated \times D^{\tau}$ coefficient estimates of Equation 1.1, with purchased insurance prices as our outcome of interest. This specification is estimated using the Census tract-level panel data, which ranges from 2009 to 2022. All insurance price data are inflation-adjusted to 2021 \$. We plot the static difference-in-differences estimate with the blue dashed line, showing that following accreditation, the average purchased insurance price decreases by \$100. We do not consider the actual year of accreditation as part of the post- treatment time period when measuring our static treatment effect. The levee accreditation can occur at any point during the year, and the new, reduced prices would likely only be available in the year following accreditation.

Table 1.6: Census Tract-level Estimates of the Effect of Accreditation on Insurance Prices and Purchase Decisions: Difference-in-differences

	Dependent variable:
	Average Purchased Premium (2021 \$)
Post Accreditation	-100.035^{*}
	(57.74)
Baseline Mean	\$1,096
Tract FE	Yes
Year FE	Yes

Note: This table presents the coefficient estimates of δ from Equation 1.2, where the outcome variable is average purchased insurance premium in 2021 USD, and the treatment variable is whether the geographic unit has a levee that is accredited. This analysis is conducted at a Census tract geographic level. Statistical significance is represented by the following notation: *p<0.1; **p<0.05; ***p<0.01.

Specifically for our context, this means that in the absence of the levee's provision, affected geographic units would have had similar trends in flood insurance take-up or average insurance price to those units that did not yet have a levee. In other words, the exact timing of the levee provision is uncorrelated with unobserved, time-varying factors that could affect flood insurance take-up. We provide two pieces of evidence to support this claim.

First, the estimates of $\delta_\mathcal{T}$ from Equation 1.1 are presented in a coefficient plot in Figure 1.6. In these event study specifications, we include three time periods of "negative event time," denoting the time periods prior to the levee construction. We see in Figure 1.6 that these pre-periods do not exhibit any statistically significant differences in policy take-up rates, which serves as evidence that there were not pre-existing differences in already- and not-yet treated geographic units prior to levee construction. Appendix Figure A.1 shows the results from a similar event study plot for a specification with five, not three pre-periods. There, we see a similar statistically insignificant pre-trend between already- and not-yet-treated units, before construction.

Second, we would like to consider levee construction timing to be "as if random." In order to demonstrate this, we estimate the regression in Equation 1.4, where $LeveeTiming_i$ denotes the year that Census place i had a levee constructed or accredited.

$$LeveeTiming_i = \alpha Population_i + \beta PersonalIncome_i + \gamma HPI_i + \varepsilon_i \tag{1.4}$$

In this specification, we regress the specific timing of levee provision, both the construction and the accreditation date, as a function of observable local economic factors: population, aggregate personal income, and the Case-Shiller housing price index. The results are presented in columns (1) and (2) of Table 1.7. We find that these economic factors are not predictive of the specific timing of levee provision. Therefore, we are confident that the precise timing of levee provision is exogenous to trends in these observable characteristics, and we can interpret our estimated treatment effects as causal from levee provision.

Table 1.7: Coefficient Estimates of the Falsification Test from Equation 1.4

	Dependent variable:	
	Construction Date	Accreditation Date
	(1)	(2)
Total Population (1980)	0.00003	0.0001*
	(0.00003)	(0.00004)
Total Personal Income (1980)	−0.00002	−0.00001*
	(0.000003)	(0.000003)
Case-Shiller HPI (1980)	−0.07	0.01
	(0.07)	(0.07)
Constant	1,998.07***	2,006.22***
	(3.68)	(3.63)
Observations	122	80

Note: This table presents the coefficient estimates α, β, and γ from Equation 1.4, where the outcome variable is year of construction or the year of accreditation, and the independent variables are demographic characteristics as of 1980, such as population, total personal income, and a housing price index. This analysis is conducted at a Census place geographic level. Statistical significance is represented by the following notation: *p<0.1; **p<0.05; ***p<0.01.

1.6 Quantifying the Impacts of Levee Provision

Our empirical analysis supports the hypothesis that, after accounting for responses to both levee construction and accreditation, households view levee infrastructure as a substitute for flood insurance. In this section, we use two distinct approaches to provide estimates of households' value for levee provision.

1.6.1 *Aggregate Changes in Insurance Spending*

The first approach to quantifying household's valuation of levees is to aggregate the amount of household insurance spending that is displaced by levee provision, accounting for both extensive- and intensive-margin changes in spending. We utilize the following equations to estimate changes in insurance spending:

$$\textit{Extensive-Margin}\Delta = \textit{Household Count} \times \Delta \textit{Policy Take-up Rate} \times \textit{Ex-Post Premium} \tag{1.5}$$

$$\textit{Intensive-Margin}\Delta = \textit{Household Count} \times \textit{Ex-Post Policy Take-up Rate} \times \Delta \textit{Premium} \tag{1.6}$$

We only include housing units that were directly within the leveed area, using precise household longitude and latitude coordinates from CoreLogic tax assessment data to calculate the exact number of housing units within the leveed area. This leaves us with 206,112 households within the leveed area regions considered in this analysis, or 895 properties per levee-mile.

We estimate the number of directly affected housing units by calculating the proportion of a Census place geography that overlaps with a "leveed area," and multiplying it by the total number of housing units in the Census place. The per-household extensive-margin change in spending is estimated by multiplying the change in the policy take-up rate by the average *ex-ante* change in insurance premium. We can include the intensive-margin take-up savings by multiplying the *ex-post* number of insurance-purchasing households times the change in insurance premium, since continuing insurance customers will save money on lower insurance rates.

$$\Delta \textit{Total Spending} = \Delta \textit{Extensive-Margin Spending} + \Delta \textit{Intensive-Margin Spending} \tag{1.7}$$

From this calculation, we estimate that levee provision causes aggregate household insurance spending to reduce by \$183,325 per levee-mile constructed.

Table 1.8: Statistics for Calculation of Changes in Household-level Spending due to Levee

Statistic	Estimate
Household Count	206,112
Δ Policy Take-up	-0.20
Ex-Post Premium	$998.50
Ex-Post Policy Take-up Rate	0.0496
Δ Premium	-$100.04
Total Levee-Miles	230.10
Total Decrease in Insurance Spending, per Levee-Mile	$183,324.70

1.6.2 Aggregate Changes in Flood Damages

In order to compare the amount of crowd-out in household insurance spending to other approaches to estimating levee benefits, we also calculate the average amount of flood damages averted by the levee. For this alternate estimate, we use building and contents damage estimates from the National Flood Insurance Program Claims data. This dataset consists of filed insurance claims from 1980 to 2022, and includes both the adjusters' damage estimate and the amount paid out to the policy holder. Since the paid amount is correlated with the amount of insurance coverage purchased, we focus on the adjuster's damage estimate as our source of damage information. The claims data has additional information about the risk zone where the claim was filed, which we sort into high-risk (Zone A) and low-risk (Zone X) areas. This is analogous to the zone change occurring during the accreditation process, where leveed regions are re-assigned from Zone A to Zone X. Due to data limitations, we do not currently know the precise breakdown of damages as a function of flood intensity. This is key to estimating the averted damages due to a levee, since a low-level, high-frequency flood event would be plausibly averted by a levee, while a high-level, low-frequency flood event would likely still continue to incur damages even in a “safer" leveed area. However, by observing damage amounts over a long time horizon, which allows for the occurrence of a broad range of flood intensities, we can back out the expected damages each of the two flood zones, without knowing the exact distribution of flood damages as a function of intensity.

Taking the difference of the expected damages from the two flood zones provides an estimate of the decrease in expected damages due to the levee. To account for geographic variation in the distribution of flood intensity and damages, we calculate average damages at a state level, impute these estimated averted damages to levees within that state. The results for in-sample levees is presented in Table 1.9. From this calculation, we find that the total decrease in annual expected flood damages totals $4.5 million per levee-mile. It is important to note that our calculation is likely under-estimating the protective capabilities of many levees in our sample. The low-risk flood zone we use as a proxy for post-levee regions are generally 200-year floodplains, or regions with a 0.5% chance of flooding each year. However, from Table 1.2, we see that levees within our analyses have an annual exceedance probability of 0.0026, which is a much lower risk profile than 0.01.

Despite this under-estimation, this approach results in a much, much larger estimate than the amount of averted insurance spending. The difference between the averted insurance spending estimates and the averted damages estimates are likely due to households' demand preferences for flood insurance, information constraints regarding flood risk and levee benefits, and the gap between actuarially fair insurance prices and the real, subsidized prices.

Table 1.9: Average Estimated Damages to Building and Contents

Statistic	Estimate
Total Household Count	206,112
Δ Average Per-Household Decrease in Annual Expected Flood Damages	$5,045.46
Total Levee-Miles	230.10
Total Decrease in Annual Expected Flood Damages, per Levee-Mile	$4,519,469

1.6.3 Comparing Levee Benefits to Costs

In the previous section, we provided two different estimates of "levee benefits": households' averted insurance spending, and their averted damages. In order to contextualize these figures, it is helpful to have a estimate of the construction costs of a levee. Bradt and Aldy [2023] compile all possible known sources of levee construction costs and find that on average, levees cost \$60.8 million to construct per levee-mile. If we use our estimate of expected averted damages as the sole benefit of the levee, amounting to \$4.5 million per levee-mile annually, we estimate that it will take 13.5 years for the levee to break even between our estimated benefits and costs. Since the average levee in our sample is 28 years old, this is a reasonable frame of reference for a break-even point. However, this is not accounting for a discount rate, or the depreciation or maintenance costs of the levee.

However, if we instead consider the averted insurance spending as the main metric of levee benefits to households, we then arrive at a much different answer, since forgone spending on insurance due to the levee is markedly lower than the averted damages. This would lead to a whopping 337 years for the levee to break even with the construction costs. Since insurance take-up is so low, if we only use insurance spending to infer households' willingness to pay for lower flood damages from the levee, it will take many years for the levee investment to break even with the measured benefits. Thus, much more research is required on the determinants of flood insurance demand and households' willingness to pay for safety from floods.

1.7 Conclusion

In this paper, we present novel empirical findings concerning the relationship between levee construction and flood insurance take-up. We find a statistically and economically significant decrease in insurance take-up immediately following levee construction. Following levee accreditation, where prices and mandate requirements are decreased, the relationship between the policy instruments changes, and the negative effect of the levee construction is partially mitigated. From a policy-maker's perspective, these results underscore the importance of considering the interaction of multiple policy instruments in mitigating households' exposure to flood risk. Households living near levees are still susceptible to flood risks, and our findings suggest that levee provision has a net negative impact on insurance take-up, despite accreditation provisions that presumably reduce insurance prices to more closely reflect the decreased flood risk. Concurrent provision of flood adaptation policies will likely have to be coupled with large-scale information programs to educate the public about the lingering risks, despite the prevalence of protection programs in the area.

The final section of our paper conducts an auxiliary back-of-the-envelope calculation to calculate the aggregate insurance spending that is crowded out by levee provision. We find that each levee-mile constructed crowds out \$0.18 million of household insurance spending. To provide context to that magnitude, we calculate averted damages due to levee provision, and estimate that each levee-mile prevents \$4.5 million in building and contents damages, which corresponds to a break-even point of 13 years. The large gap between these figures likely reflects households' low baseline demand for flood insurance, despite incentives like price subsidies and mandatory purchase requirements. In other words, if policymakers measure the benefit of the levee solely through the amount of spending crowded out by the levee, they would likely largely under-estimate the societal benefit of levees.

CHAPTER 2

NATURAL DISASTERS AND LOCAL BUSINESS TRENDS

2.1 Introduction

Heavy rainfall and strong winds from hurricanes, also known as cyclones and typhoons, wreak havoc on coastal regions, causing widespread flooding and posing significant threats to human lives, ecosystems, and global economies (NOAA). Over the past 45 years, nineteen states in the contiguous United States have experienced expensive and consequential damages caused by hurricanes: for instance, the National Oceanic and Atmospheric Administration ("NOAA") estimates that Hurricane Katrina cost the US almost $170 billion, and more recently, Hurricane Harvey cost almost $131 billion. Moreover, scientists have documented that due to climate change, these storms have had increased in severity, emphasizing the growing importance of understanding the human and economic costs associated with these prolific disasters [GAO, 2020].

Despite extensive damages to capital, public infrastructure, private property, and business operations inflicted by hurricanes, there is mixed evidence on how local economic conditions are impacted by hurricanes. For example, the cross-country analysis in Hsiang and Jina [2014] reveals that hurricanes cause a decrease to countries' gross domestic product (GDP) for up two decades after the storm. In the context of the United States, Strobl [2011] examines the impact of hurricane exposure on local economic growth rates, discovering a decrease of 0.45 percentage points, primarily attributed to the out-migration of higher-income households. However, Tran et al. [2020] present contradictory findings that natural disasters, including hurricanes, contribute to an increase in aggregate personal income, attributing this phenomenon to a short-term employment boost and a long-term rise in wages.

Given the varied evidence regarding the impacts of hurricanes on macroeconomic indicators such as GDP and employment, studying micro-level outcomes of households and firms could shed light on the complicated dynamics behind these varied results. While existing knowledge about micro-level outcomes predominantly focuses on household characteristics and insurance mechanisms (Gallagher and Hartley [2017]; Deryugina [2017]), very little is known about how hurricane exposure specifically influences business survival and outcomes. Businesses are subject to a different set of explicit and implicit insurance mechanisms than households to protect them against large negative shocks. The existing literature within this strand has been limited to case studies of specific storms, or examined the impact of disaster loan programs on small business survival ([Davlasheridze and Geylani, 2017]). Due to this granular focus in the literature, further research is imperative to (1) measure the extent to which businesses are affected by hurricanes, and (2) discern the contributing factors to these trends.

This paper will contribute to the first objective by providing empirical estimates of the dynamics of business survival, by utilizing county-level Census data to gauge the impacts of hurricane exposure on establishment volume. I leverage variation in hurricane exposure, using detailed spatial data documenting the exact path of hurricane trajectories. I find that establishment volume steadily increases in the decade following hurricane exposure, leading to a five percent growth in establishment volume ten years after exposure. The persistence of this positive trend aligns with particular US-based, macro-focused studies documenting an increase in personal income and employment following hurricane exposure (Tran et al. [2020]), and contradicts other studies documenting growth stagnation and losses following hurricanes (Strobl [2011]). I also conduct a sub-analysis using size- and industry-based heterogeneity, finding that the share of very small establishments increases over time, while the share of mid-size establishments decreases over time, and that the construction, retail trade, services, and financial industries exhibit similar establishment volume trends over time.

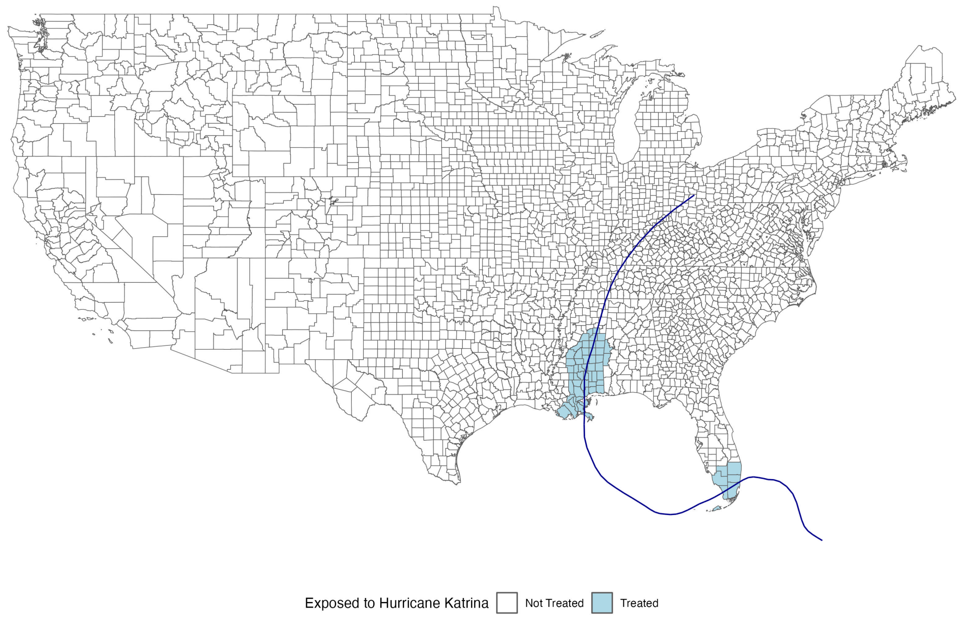
The rest of the paper proceeds as follows: Section 2.2 describes the hurricane and Census business data used in the analysis. Section 2.3 introduces the empirical strategy and Section 2.4 presents the estimates of the effect of hurricane exposure on establishment volume. Section 2.5 discusses the results of the size- and industry-based heterogeneity analyses. Section 2.6 concludes.

2.2 Data

I combine two key sources to build a unique county-year panel dataset: spatial hurricane data from NOAA, and the establishment volume data from the Census Bureau. NOAA maintains the International Best Track Archive for Climate Stewardship (IBTrACS), which contains detailed longitude and latitude information about hurricane trajectories, using data from satellite observations. The IBTrACS data also includes information on maximum wind speeds and the designation of the storm on the Saffir-Simpson hurricane index at regular geographic intervals. To estimate my outcomes of interest, I utilize the County Business Patterns data, available from 1986 to 2016, which contains county-year information about establishment volume, and includes sub-totals for various establishment size and industry groups. According to the Bureau of Labor Statistics, "an establishment is considered to be a single physical location where one predominant activity occurs. A firm is an establishment of a combination of establishments". Census data includes aggregated information about establishments, but I cannot link establishments to their broader network of firms. Therefore, this analysis is conducted at an county level, and studies establishment volume, rather than firm volume.

In order to define treatment, or a county's exposure to a hurricane event, I combine spatial county boundary data with the hurricane trajectory spatial data. Since the average hurricane

Figure 2.1: Example of Treatment Definition: Counties Exposed to Hurricane Katrina



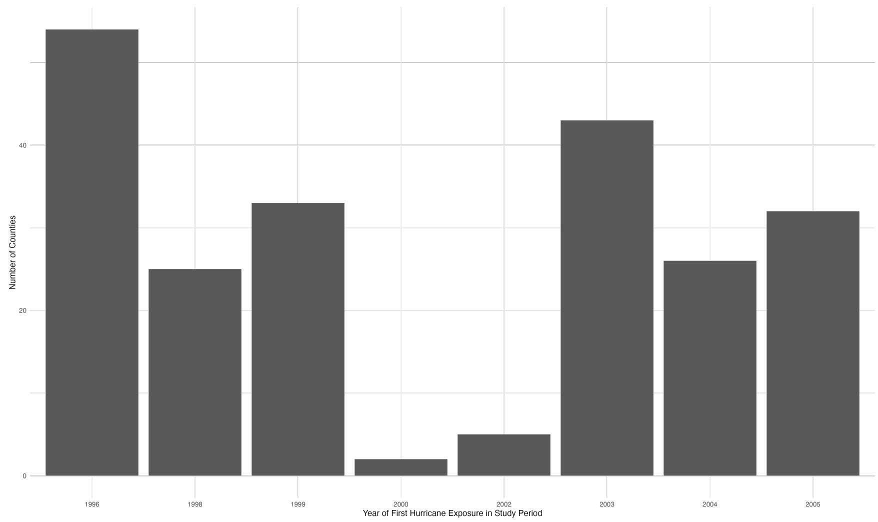
Note: This map depicts the contiguous United States, and demonstrates how hurricane spatial trajectory data was used to determine which counties were exposed, and therefore "treated" by a hurricane event. The bold blue line demarcates the path of Hurricane Katrina as it made landfall with the contiguous United States. Since the trajectory only denotes the path of the eye of the hurricane, I define a radius of 50 miles around the path to consider counties that were not directly hit by the eye of the hurricane, but likely sustained intense rainfall and high wind speeds due to the storm. Thus, counties that were within 50 miles of the hurricane trajectory, and thus considered to be treated within my empirical strategy, are denoted in red.

has a 150-mile radius [1], I employ a conservative approach and use 50 miles as the radius around the eye of the hurricane to define treatment. Thus, I identify a county as "treated" if any portion of the county is located within a 50 mile radius of a storm, while it was classified as a Category 1 hurricane or worse. Figure 2.1 shows an example of this treatment definition with Hurricane Katrina, whose path is demarcated with the bold blue line. The counties highlighted in red represent those "treated" by Hurricane Katrina: they were within 50 miles of the eye of the storm, while it was considered a Category 1 or worse hurricane.

Overall, I limit my analysis to 30 hurricane events that occurred between 1996 and 2006, to allow for a full ten-year pre- and post- period of study. There are 184 counties that have been

1. Statistic from Weather.gov

Figure 2.2: Counts of Treated Counties, by Year of First Exposure in Study Period



Note: This bar chart depicts the distribution of first hurricane exposure for the 220 counties exposed to hurricanes in my analysis. Counties are considered to be exposed to a hurricane if the 50 mile radius of the hurricane intersects with the county boundaries, and the storm is categorized as a hurricane at some point during the intersection. I only consider counties once, even if the county was exposed to several different hurricanes during the study period. A county is included in an annual total if that is the *first* year that that county was affected by a hurricane during the study period. I only consider hurricane events that happened in the study period of 1996-2006, because my event study analysis requires that I observe the establishment volume of a county for ten years prior and following the hurricane event.

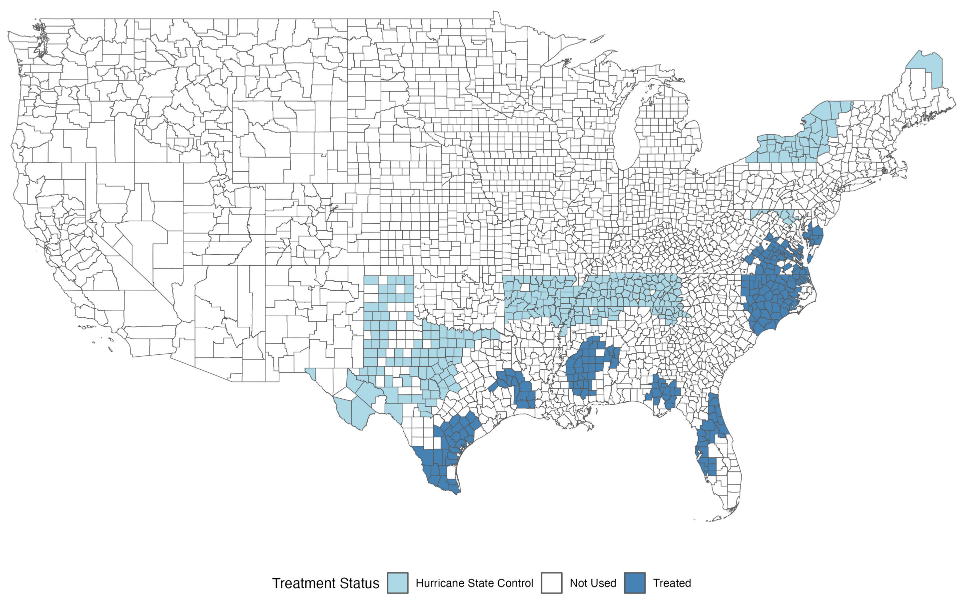
affected by these 30 hurricanes between 1996 and 2006. For the sake of cleanly measuring changes in establishment volume in the decade prior to a hurricane event, I omit counties that experienced another hurricane event in the decade prior to the studied hurricane. Additionally, since it is possible that places were affected multiple times by hurricanes in the study period, I use the first instance of a hurricane. Figure 2.2 depicts the distribution of the 184 US counties considered treated within my analysis, by the year of the studied hurricane (i.e., the first hurricane exposure within the study period of 1996 to 2006). The geographic distribution of these counties is shown in Figure 2.3, spanning 10 US states.

2.3 Empirical Strategy

2.3.1 Choice of Control Group

The purpose of my empirical design is to measure the causal effect of hurricane exposure on local business volume. A naive comparison of local business volume before and after a

Figure 2.3: Geographic Distribution of Treated and Hurricane State Control Counties



Note: This map depicts the treatment and control county groups within my event study design. The dark blue counties are the treatment group: the 220 counties that were within 50 miles of at least one Category 1 or worse hurricane between 1996 and 2006. The light blue counties are control counties as per the "hurricane state" definition: the 312 counties that are within the same state as a county that ever experienced a hurricane between 1980-2021, but were not themselves within 100 miles of a hurricane. The counties in white are not utilized in the event study design, because they are either not credible counterfactual for the treated counties, or they may have been partially treated by one of the studied hurricane storms (i.e. within 100 miles of one of those hurricanes).

hurricane may be subject to selection bias, since aggregate economic trends of a region may play a role in changing levels of business volume over time. In that case, we may mistakenly ascribe business growth as related to the hurricane, when it was actually the effect of an underlying economic trend. To account for this selection bias issue, I consider a natural experiment utilizing the *precise* trajectories of hurricane paths. The identifying assumption is that the precise path of the hurricane is uncorrelated with characteristics such as economic growth, and comparing the business volume of counties that were directly in the path of the hurricane to that of counties that were not directly exposed would allow me to estimate the *causal* impact of the hurricane itself on business volume.

This analysis requires the careful consideration of the "control" counties, or the counties that were not directly exposed to the hurricane. Hurricanes have affected only nineteen out of the 48 contiguous states, and the study sample includes 220 treated counties, while there are 1,882 counties in the contiguous US that are considered "never-treated", i.e. did not experience a hurricane event between 1986 and 2016. However, it is unlikely that every single never-treated county is a viable counterfactual for the treated counties considered in my analysis. For instance, counties on the West coast of the US, where hurricane exposure is non-existent, may have different composition of industry or differential trends in economic growth than Eastern hurricane-prone counties. Therefore, I employ a method similar to a strategy used in Jerch, Kahn [2021] and Deryugina [2017], where the sample of never-treated counties is reduced to counties that are located within *hurricane states*, or states that were exposed to *any* hurricane event anytime between 1980 and present.

Within this control group of hurricane state counties, I may worry that some of these counties were geographically close to one of the studied hurricanes, and therefore may be partially affected by the storm. In order to circumvent that issue, I perform a further refinement of the control group, where I remove any never-treated county that came within 100 miles of a storm trajectory while it was a Category 1 or worse hurricane. This helps to alleviate the concern that there are counties acting as controls that may have been partially exposed to a hurricane during the study period. I also eliminate any control county that experienced a hurricane between 1980 and the beginning of the study period, to alleviate concerns that those counties may have been exposed to a hurricane prior to the storm.

The final set of studied counties includes 220 “treated" counties, or counties that were within 50 miles of the eye of a Category 1 or worse storm, and 312 never-treated control counties that are in hurricane states but did not experience any hurricane events after 1980. Table 2.1 presents summary statistics of these county groups for a variety of characteristics. I find that the treated counties have more similar populations, employment levels, and aggregate personal income to hurricane states than to the nationwide sample of never-treated counties, underscoring the importance of limiting the control county group to more comparable counties. I quantitatively test this observation with a difference in means t-test between the treated county group and each of the control county groups. Overall, I cannot reject the null hypothesis that the average population, average employment, or average personal income varies between the treated group and each of the control groups, but I do find smaller p-values for the larger nationwide control county group.

2.3.2 Event Study Regression Analysis

I aggregate the treated and hurricane state control counties' data into a county-year panel dataset to run an event study regression, that leverages the plausible exogeneity of exact hurricane trajectories. To estimate the treatment effect of the hurricane exposure, I primarily utilize the specification in Equation 2.1. Here, Y_{cy} is the log number of establishments in a given county-year cy, t is event-time (ET), which is the calendar year subtracted by the year of the first hurricane experienced by county-year cy within the 1996 - 2006 study period. $EverHurr_{cy}$ is a binary variable indicating whether an county ever experienced a hurricane

Table 2.1: Summary Statistics for Treated and Control County Groups

	Treated	Hurricane State Never-Treated	All Never-Treated
Number of Counties	220	312	1,882
Population	66,399.84	60,702.61	73,760.68
P-value	-	0.58	0.45
Employment	19,927.61	19,917.61	25,052.57
P-value	-	1.00	0.19
Personal Income.	894,679.9	873,130.0	1,128,797.2
P-value	-	0.90	0.14

Note: This table presents averages of county characteristics for three different county groups, and the p-values from a difference in means t-test between the treated group and the two different control groups. The first group is the treated counties, which come within 50 miles of a hurricane between 1996 and 2006. The second group is the never treated counties that will be used as controls in the event study regression analysis, selected because they are in states that had a hurricane at least once after 1980, but did not come within 100 miles of a hurricane themselves. The final group is the full population of never-treated counties, including both hurricane state and non-hurricane state counties. The first row presents the number of counties in each sub-group. The second row presents the average pre-treatment population of the county sub-group, as of 1986. The third row presents the average employment volume of the county sub-group, as of 1986. The fourth row presents the aggregate personal income, measured in billions of USD$, as of 1986. The population dataset is from NBER; the employment dataset is from the Census County Business Patterns data, and the personal income dataset is from FRED.

within the study period. Finally, δ_y and ϕ_c are year- and county-fixed effects, respectively. I limit the county-year panel dataset to be balanced on covariates and on event time, so that I can observe the full time series of establishment counts for ten years prior, and ten years following, the first hurricane event. In other words, I limit my pool of "treated" counties to include those that experienced a hurricane event between 1996 and 2006.

$$\begin{aligned} Y_{cy} = \alpha + \sum_{t=-10\backslash-1}^{10} \left[\beta_t \mathbb{1}[ET = t]_{cy} * EverHurr_{cy} \right] + \\ \beta_l \mathbb{1}[ET \leq -11]_{cy} * EverHurr_{cy} + \\ \beta_u \mathbb{1}[ET \geq 11]_{cy} * EverHurr_{cy} + \delta_y + \phi_c + \varepsilon_{cy} \end{aligned} \tag{2.1}$$

The identifying assumption of the event study design is that "treated" county-years, would have similar trends in establishment volume as untreated county-years, had they not been affected by a hurricane. Since hurricane trajectories are not easily predictable until very close to the actual storm itself, it is possible that places that were closely hit by a hurricane are on similar trajectories of observable and unobservable characteristics to places that were missed. I test this assumption by observing the pre-hurricane trends in establishment volume, and do not find any statistically significant variation in the decade prior to the hurricane. This is encouraging evidence that treated- and never-treated counties had similar trends in establishment volume for the decade prior to the hurricane exposure. Future iterations of this paper will establish further tests of this assumption, including an analysis that determines whether county-level characteristics can predict the timing of hurricane exposure.

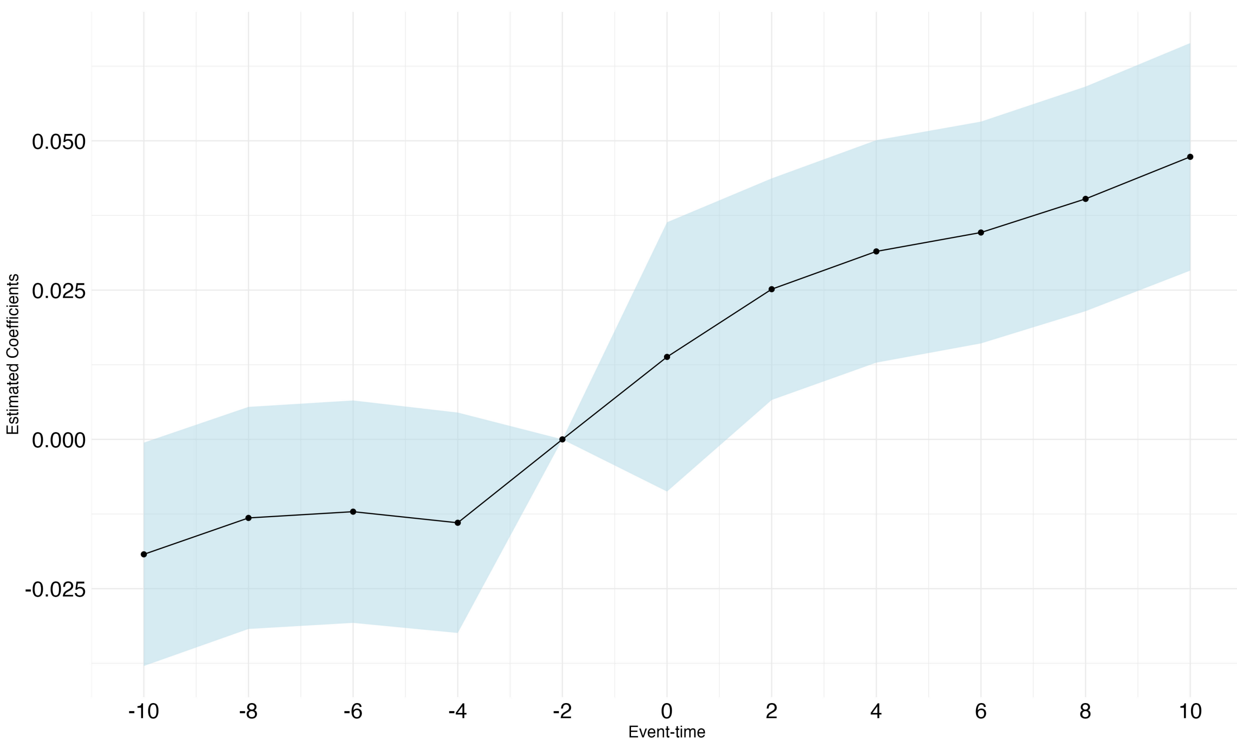
2.4 Results

In Figure 2.4, I present the estimates of the β coefficients from the event study specification in Equation 2.1. Since I estimate specific treatment effects at each individual event-time, I am able to observe trends in log establishment volume over time. In the decade prior to hurricane exposure, I do not find statistically significant differences in establishment volume. However, immediately after hurricane exposure, I observe an increase in log establishment volume, and this increases persists for the decade following hurricane exposure. Since I model a log transformation of establishment volume, I can interpret this coefficient estimate as an increase in establishment volume of up to 4.7 percent.

2.5 Heterogeneous Responses to Hurricane Exposure

In the previous section, I establish that establishment volume increases following a hurricane. This result is consistent with a “build-back-better" narrative, where older capital and businesses affected by the hurricane are newly replaced by superior quality alternatives (Hsiang and Jina [2014]). To further comment on the mechanisms behind these increases, I study

Figure 2.4: Event Study Estimates: Establishment Volume



Note: This figure plots the β_t coefficient estimates from Equation 2.1, using first year of hurricane exposure as the source of variation. The regression is estimated on log establishment volume. The x-axis plots event time, where '0' denotes the year of exposure, negative event-time denotes the ten years prior to exposure, and positive event-time denotes the years following exposure. The single point denotes the point estimate of the coefficient, and the vertical bar denotes the 95 percent confidence interval. All event study specifications are estimated using a data sample that is balanced in event-time, so all county units have establishment volume information for the full range of event time: [-10,10].

heterogeneity in outcomes by business size and industry type.

2.5.1 Size-based Heterogeneity

When considering the "build back better" hypothesis in the context of establishment size heterogeneity, I consider two competing mechanisms by which differential outcomes could arise:

1. *Selection mechanism*: Larger, more well-resourced businesses enter or remain in the market and smaller, less-resourced businesses exit or fail to enter the market following a disaster

2. *Sorting mechanism*: Existing or new large businesses have a greater ability and preference to move into, or form within an *unaffected* area, and existing or new small businesses are more likely to remain within, or form within an *affected* area

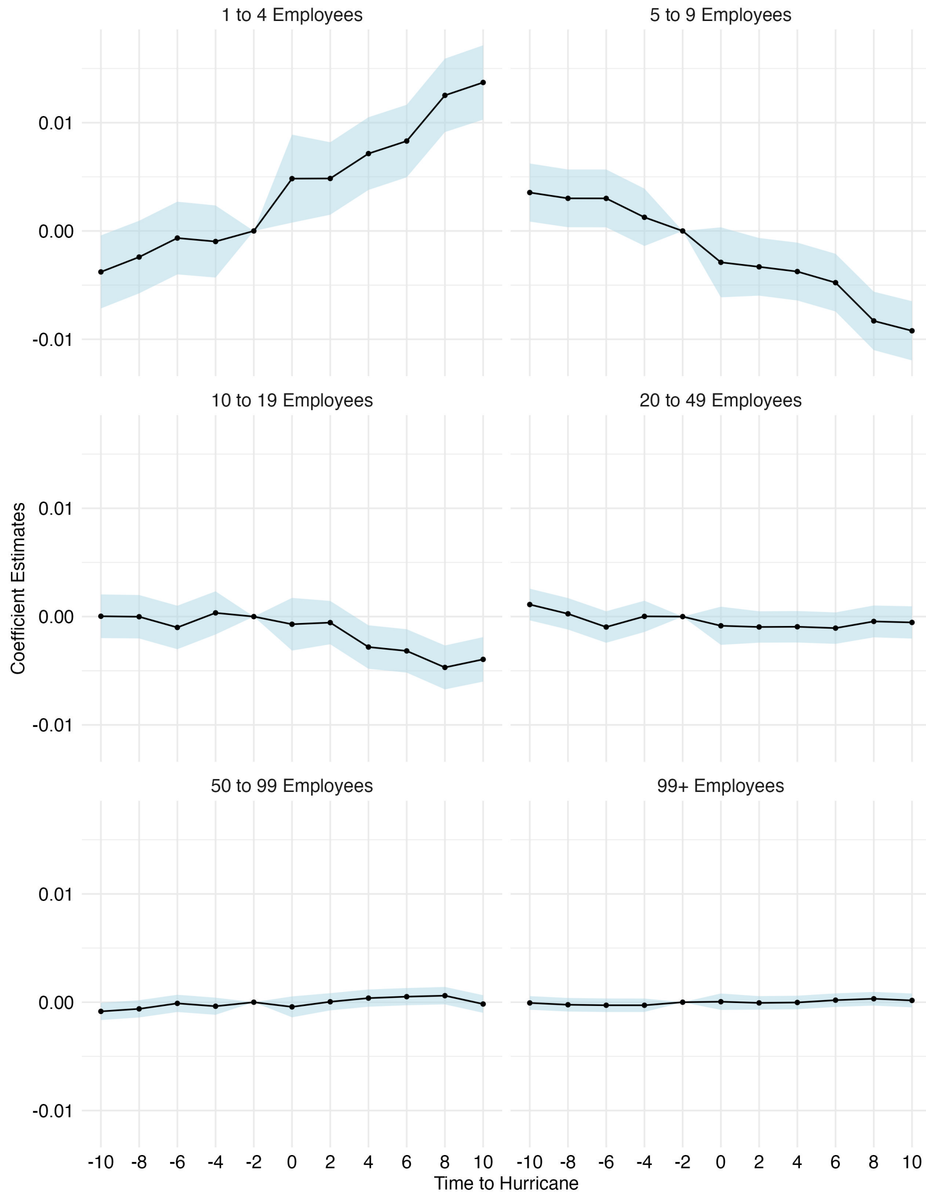
By disaggregating business outcomes by business size, I can disentangle which of these mechanisms is more salient. Figure 2.5 depicts the results of the estimation of Equation 2.1 on subgroups of the County Business Patterns establishment volume data that have different number of employees. I find that the share of very small establishments, with 1 to 4 employees, increases over time, while the share of slightly establishments, particularly those with 5 to 9 and 10 to 14 employees, actually decreases over time. I do not find statistically significant evidence of any change in the share of large establishments, such as those with more than 50 employees. However, those establishments generally comprise a very small fraction of the overall establishment distribution. These findings are suggestive that the sorting mechanism may be more prevalent, as the very small establishments comprise of a larger share of establishments within hurricane-affected areas, and slightly larger establishments form at lower rates, or leave at higher rates, within hurricane-affected areas. Further research on establishment movement, using more granular data on establishment survival and formation, is required to determine whether these trends are caused by the movement of existing establishments, or the inception of new establishments.

Additionally, to be able to tell a more complete story about the role of business size on post-hurricane outcomes, further research is required on the nature of firm-establishment networks. Since this analysis is at an establishment level, it is difficult to tell which of the establishments are part of a larger firm network, and may potentially benefit from the safety-net of subscription to a larger system. For instance, it may be misleading to study the outcomes of an establishment with a small number of employees, because it may be a part of a larger firm and benefit from that subscription during a large shock such as a hurricane. If establishments with 1 to 4 employees are more likely to be a part of a larger firm than establishments with 5 to 9 employees, then that may partially explain the divergent results between the two groups. In addition to the size of the firm-establishment network, it is likely that the geographic *distribution* of the firm-establishment network will also play a role in the long-term survival of the business. This is a rich area for further research.

2.5.2 Industry-based Heterogeneity

I also explore heterogeneity in the dimension of industry, to understand whether the positive trends in establishment volume are consistent across various industries. If I consider a hurricane exposure to be a bundled shock of capital destruction and an interruption to operations, different industries would be affected differently, depending on which aspect of this sudden exposure is more salient for that industry, i.e. more likely to lead to long-term impacts. For example, if interrupted operations are easier to bounce back from following disasters, businesses within labor-intensive industries are more likely to survive in the long run. On the other hand, if capital destruction is easier to mitigate, then capital-intensive industries would be more likely to bounce back in the long-run.

Figure 2.5: Event Study Estimates for Share of Establishments, by Employee Count



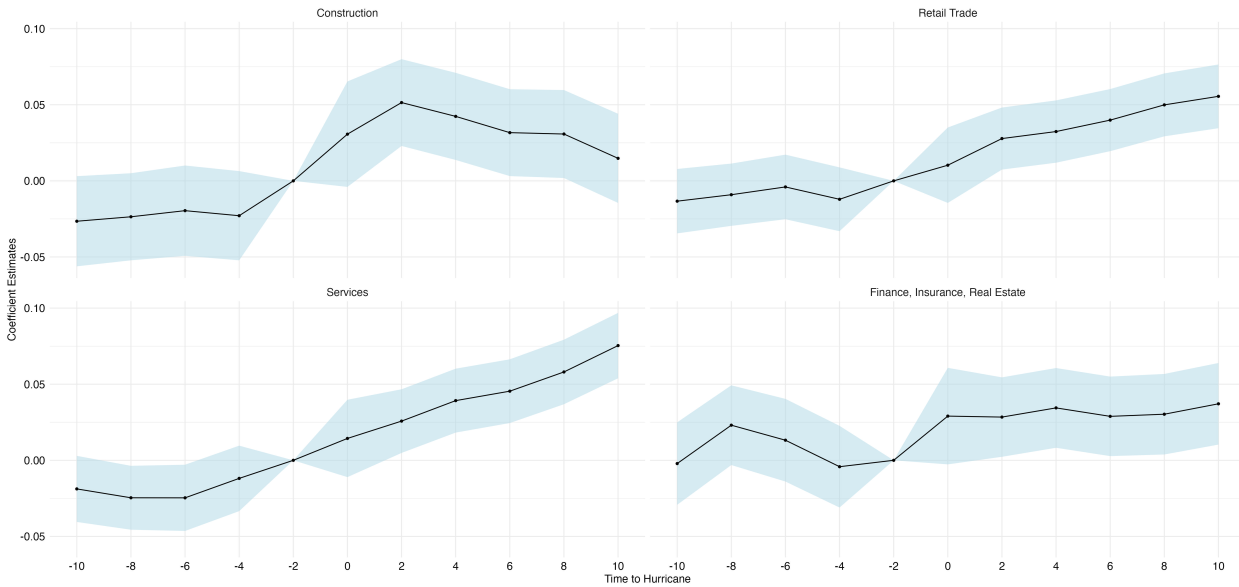
Note: This figure plots the β_t coefficient estimates from Equation 2.1, using first year of hurricane exposure as the source of variation. The regression is estimated on the percentage of establishments with a certain range of employee counts. For instance, the variable "1 to 4 Employees" indicates the share of total establishments in a county that have 1 to 4 employees. The x-axis plots event time, where '0' denotes the year of exposure, negative event-time denotes the ten years preceding exposure, and positive event-time denotes the ten years following exposure. The single point denotes the point estimate of the coefficient, and the blue shaded area denotes the 95 percent confidence interval of the point estimate. All event study specifications are estimated using a data sample that is balanced in event-time, so all county units in have establishment volume information for the full range of event time: [-10,10].

I study heterogeneity in responses to hurricane exposure within industry sub-groups, as defined by the broad NAICS/SIC (the North American Industry Classification System, and the Standard Industry Classification) industry classification codes. These codes are used within the Census County Business Patterns data. Figure 2.6 below presents the estimation of Equation 2.1 on four industry groups: construction, retail trade, services, and finance/insurance/real estate. Of these, I find that the sub-industry trends, with the exception of construction, largely follow the aggregate trend, but the magnitude of growth varies by industry. For instance, the finance/insurance/real estate industry class exhibits a 3 percent growth in the decade following the hurricane, and the retail trade industry class experiences up to a six percent growth in the following decade, while the services industry class experiences up to seven percent growth. The construction industry deviates from the aggregate establishment volume trend, exhibiting an immediate spike in growth of up to 5 percent in the two years following the hurricane, and then stabilizing and eventually decreasing in the following decade.

Taken together, these results further underscore the prevalence of the "build-back-better" story, where the hurricane initiates an boost in business volume over time. The initial jump in the construction industry class may be suggestive evidence of the increased demand for construction immediately following the hurricane, and this demand perhaps helps to sustain the steady long-term growth of some of the other industries. Overall, these results provide encouraging suggestive evidence that the United States' disaster response provisions are successfully maintaining, and even helping to grow and expand business operations over time.

Appendix Figure B.1 depicts the results of the event study specification for additional industry classes not depicted in Figure 2.6, although the standard difference-in-differences assumptions are largely unsatisfied for those industries, since many of them exhibit substan-

Figure 2.6: Event Study Estimates for Share of Establishments, by Industry



Note: This figure plots the β_t coefficient estimates from Equation 2.1, using first year of hurricane exposure as the source of variation. The regression is estimated on log establishment volume for four industry sub-classifications. The x-axis plots event time, where '0' denotes the year of exposure, negative event-time denotes the ten years preceding exposure, and positive event-time denotes the ten years following exposure. The single point denotes the point estimate of the coefficient, and the blue shaded area denotes the 95 percent confidence interval of the point estimate. All event study specifications are estimated using a data sample that is balanced in event-time, so all county units in have establishment volume information for the full range of event time: [-10,10].

tial difference in trends between the treated and control counties in the years prior to the hurricane event. The lack of internal validity also means that we cannot speak to the plight of capital-intensive industries such as transportation and manufacturing over time. I would expect that those industries may behave differently than the labor-intensive industries documented in Figure 2.6, since hurricane-induced capital destruction may require a different time frame for recovery.

2.6 Conclusion

I use publicly available Census data on county-level establishment volume, entry, and exit, as well as detailed spatial hurricane data, within an event study regression framework, to measure the causal effect of hurricane exposure on business dynamics. I find that establishment volume increases by 5 percent in the years following a hurricane, which is similar to the results of increasing personal income from Roth Tran and Wilson (2022), but contrasts with many of the macro-level results in the literature that find a decrease in growth and employment in hurricane-affected areas. In a size-based heterogeneity analysis, I find that the share of very small establishments increases over time, while the share of mid-size establishments decreases over time. I find no change in the share of very large establishments over time. This is suggestive evidence that post-hurricane economic conditions or mitigation measures incentivize the entry of small establishments in local economics. Finally, I also explore heterogeneity among broad industry sub-classes and find results largely consistent with the aggregate establishment volume trend, but with some differences - for instance, the increase in construction establishment volume peaks a few years after the hurricane event, while other industries such as services and retail trade increase for up to a decade following the hurricane event. Overall, these trends are consistent with a “build back better" hypothesis, where local economies are more amenable to growth after a large shock such as a hurricane.

CHAPTER 3

THE EFFECTS OF HURRICANE EXPOSURE ON RENTAL MARKETS

3.1 Introduction

Hurricane winds and storm-related flooding are responsible for the majority of economic losses from disasters in the United States (NOAA, 2021). A 2019 Congressional Budget Office report states that expected annual losses from these disaster amounts to $54 billion. The majority of these costs arise from losses for the residential sector: the annual costs of home repair and temporary housing are estimated to total $34 billion (CBO, 2019). These damages can have massive consequences on housing markets, leading to many short and long-term impacts for households.

There have been numerous studies documenting the impact of natural disaster exposure on housing sales markets. Hurricanes, flooding, and other large natural disasters have been found to cause billions of dollars in property damage (Boustan et al. [2020a]). Beltrán et al. [2019] studies property values in England, finding that home values decline significantly after coastal flooding. Ortega and Taşpınar [2018] find a price penalty for properties affected by Hurricane Sandy, whether or not they were actually damaged. Comparatively, there is little to no evidence about trends in *rental* markets following disasters. Renters make up a significant portion of households in the United States: more households are renting than ever in the last fifty years. As of the first quarter of 2020, *a third of households were renters*.

There are a number of mechanisms potentially at play as to how homeowners and renters each weather natural disaster shocks, leading to heterogeneous impacts across housing tenure status. To begin with, there are significant racial and socioeconomic disparities in who rents and who owns their housing in the United States. The median income for homeowners is \$52,000, versus only \$34,000 for renters, and more than half of Black households rent, versus only 27 percent of White, non-Hispanic households (Schuetz [2017]).

Moreover, the very nature of the relationship of each group with their housing arrangement could significantly affect how each group is able to recover from natural disaster exposure. For instance, homeownership is an important wealth-building strategy for Americans. Home equity constitutes a large portion of total family assets for all but highest earners. There are many incentives to homeownership, including tax subsidies, access to credit, and a "built-in" savings structure. Additionally, there are informational constraints that make homeowners more likely to be aware of natural disaster risk with making homeownership and migratory decisions. Laws disclosing flood risk are oriented specifically towards informing homeowners, as too are aid channels such as FEMA insurance and grants (Kellman et al. [2020]). Therefore, homeowners may have more access than renters to *ex-ante* and *ex-post* mitigation mechanisms that allow them to adequately insurance themselves from natural disaster shocks.

The dearth of research on renters and rental markets is presumably due to the lack of a centralized database of rental transactions, unlike for home sales. This makes it very difficult to conduct a nationwide analysis of renters and their outcomes.

This paper will focus specifically on rental price dynamics following hurricane exposure. Rental markets and housing markets do not necessarily track each other closely–in fact, the direction and magnitude of the "rent-to-price ratio" within property markets is uncertain and has been debated in finance literature (Bracke [2015]; Zheng et al. [2018]), and deviations in the housing price-to-rent ratio are even considered signs of housing bubbles (Gallin [2008]). Additionally, renters and owners may face different price fluctuations after disasters: rent is a monthly expense, while home value changes may only mater in certain context, such as property tax appraisal, a property sale, or while refinancing. The extent to which a natural disaster provides a housing wealth shock to a household will vary significantly by housing tenure.

Tamara Sheldon and Zhan [2018] finds a decrease in homeownership following hurricane exposure, which is indicative of a decrease in demand for homeownership following a natural disaster. Additionally, Boustan et al. [2020a] finds that natural disasters cause a supply shock through extensive and expensive damage to the housing shock. Together, these two results allow us to hypothesize that the effect of hurricane exposure on rental prices will be positive, since rental properties will increase in demand and decrease in supply following a disaster. We will test this analysis by observing changes in median rent following hurricane exposure: we expect to see the median rent estimate to increase following a disaster. However, to fully support this hypothesis, we will need to also understand how housing supply changes, to better understand why rental prices may vary following a disaster. To that end, we plan to study housing units, and vacancy rates.

Our project combines public-access data on median rent from the Department of Housing and Urban Development with spatial hurricane data within an event study design, leveraging variation in the exact location of hurricane trajectories. Using this empirical strategy, we estimate the causal effect of hurricane exposure on rental prices. We find that in the years prior to hurricane exposure, median rent does not substantially vary, and following hurricane exposure rental prices decrease significantly and persistently over time.

Section 3.2 will outline the data sources. Section 3.3 will discuss the empirical strategy. Section 3.4 will provide results and discussion. Section 3.5 will discuss the intended next steps for this project. Section 3.6 will conclude.

3.2 Data

To our knowledge, there is no publicly available, nationally representative rental price data that is available at a housing unit-year level. Therefore, we use county-year level median rental price data from the Department of Housing and Urban Development (HUD). The rental price data from HUD covers 3,240 US counties in the contiguous United States from years 2003 to 2021. We also adjust the mean rent for inflation and report all rental values in 2021 dollars.

NOAA maintains the International Best Track Archive for Climate Stewardship (IBTrACS), which contains detailed longitude and latitude information about hurricane trajectories, using data from satellite observations. In order to define treatment, we combine spatial county boundary data with the hurricane trajectory spatial data. We define a county as "treated" if any part of the county is within 50 miles of the trajectory of a storm while it is classified as a Category 1 or worse hurricane. Since the average hurricane has a 150-mile radius, we employ a conservative approach and use 50 miles as the radius around the eye of the hurricane to define treatment (hur).

3.3 Empirical Strategy

In order to design a credible counterfactual for hurricane-prone counties, we employ a method used in Strobl [2011], where we limit the control group to never-treated *coastal* counties, where coastal proximity and inclusion in a coastal watershed region is defined by NOAA (National Oceanic and Atmospheric Administration (NOAA) [2012]). Specifically, coastal boundaries are defined as "those that have at least 15 percent of their land in the coastal watershed or that comprise at least 15 percent of a coastal cataloguing unit" (Strobl [2011]). We select never-treated counties across the US that are considered to be coastal as a more credible control group for the treated counties, with the assumption that coastal regions across the US would have similar trends in housing markets and local economic conditions as the treated counties. Table 3.1 presents some summary statistics to test this assumption, by comparing average population, personal income, and age distributions across the three county sub-groups: treated counties, coastal control counties, and the full population of never-treated counties. We hypothesize that this analysis will underscore that coastal control counties have similar economic conditions to the treated counties, meaning that they are a more suitable counterfactual than the control group consisting of the entire contiguous US. However, we find that the coastal counties are actually quite different from the treated counties, along these dimensions –future iterations of this project will explore this result further, potentially utilizing a suite of controls in the regression specification to account for the disparities. We also intend to compare the treated, coastal never-treated, and nationwide never-treated populations over housing market characteristics such as housing price indices or housing stock, to gain more understanding of whether coastal areas nationwide are similar to hurricane-treated areas along the dimension of housing market dynamics.

In addition, we remove any potential control counties that came within 100 miles of a storm while it was a Category 1 or worse hurricane. This helps to alleviate the concern that the control counties may have been partially exposed to a hurricane during the study period, and are not truly “never treated". We also eliminate any control county that experienced a hurricane between 1980 and the beginning of the study period, to alleviate concerns that those counties may have been treated prior to the study period.

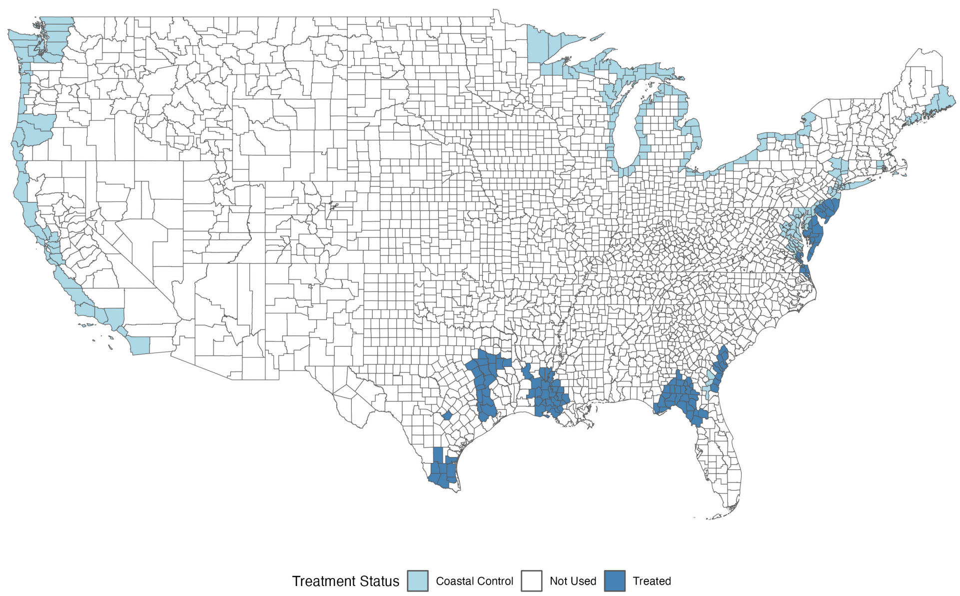
We analyze the county-year panel dataset within an event study regression, leveraging the plausible exogeneity of exact hurricane trajectories. The counties included in the analysis includes a nationwide sample of never-treated counties, or counties that did not experience a hurricane event during the study period, which serve as a counterfactual for the treated

Table 3.1: Summary Statistics for Treated and Control County Groups

	Treated	Coastal Never-Treated	All Never-Treated
Number of Counties	114	190	2,573
Population	112,079.41	395,413.98	86,509.18
P-value	-	0.00	0.42
Personal Income	3,212,367	14,071,834	2,655,058
P-value	-	0.00	0.61
Percent Age < 20	0.29	0.27	0.28
P-value	-	0.00	0.00
Percent Age > 65	0.13	0.14	0.15
P-value	-	0.00	0.00

Note: This table presents averages of county characteristics for three different county groups, and the p-values from a difference in means t-test between the treated group and the two different control groups. The first group is the treated counties, which come within 50 miles of a hurricane between 1996 and 2006. The second group is the never treated counties that will be used as controls in the event study regression analysis, selected because they considered coastal counties, but did not come within 100 miles of a hurricane themselves. The final group is the full population of never-treated counties, including both coastal and non-coastal counties. The first row presents the number of counties in each group. The second row presents the average pre-treatment population of the county group, as of 2003. The third row presents the aggregate personal income, measured in billions of USD$, as of 2003. The fourth and fifth rows presents the average percentage of a county population that is under the age of 20, and over the age of 65.

Figure 3.1: Geographic Distribution of Treated and Coastal Control Counties



Note: This map depicts the treatment and control county groups within the event study design. The dark blue counties are the treatment group: the 118 counties that were within 50 miles of at least one Category 1 or worse hurricane between 2007 and 2017. The light blue counties are control counties as per the "coastal county" definition: the 190 counties that are considered to be coastal, but were not themselves within 100 miles of a hurricane. The counties in white are not utilized in the event study design, because they are either not credible counterfactual for the treated counties, or they may have been partially treated by one of the studied hurricane storms (i.e. within 100 miles of one of those hurricanes).

counties. The primary specification is Equation 3.1.

$$Y_{cy} = \alpha + \sum_{t=-5\backslash-1}^{15} \left[\beta_t \mathbb{1}[ET = t]_{cy} * EverHurr_{cy} \right] + \delta_y + \phi_c + \varepsilon_{cy} \tag{3.1}$$

In this specification, Y_{cy} is the median rent in a given county-year cy, t is event-time (ET), which is the calendar year subtracted by the year of the first hurricane experienced by county-year cy. $EverHurr_{cy}$ is a binary variable indicating whether an county ever experienced a hurricane within the study period. Finally, δ_y and ϕ_c are year- and county-fixed effects, respectively.

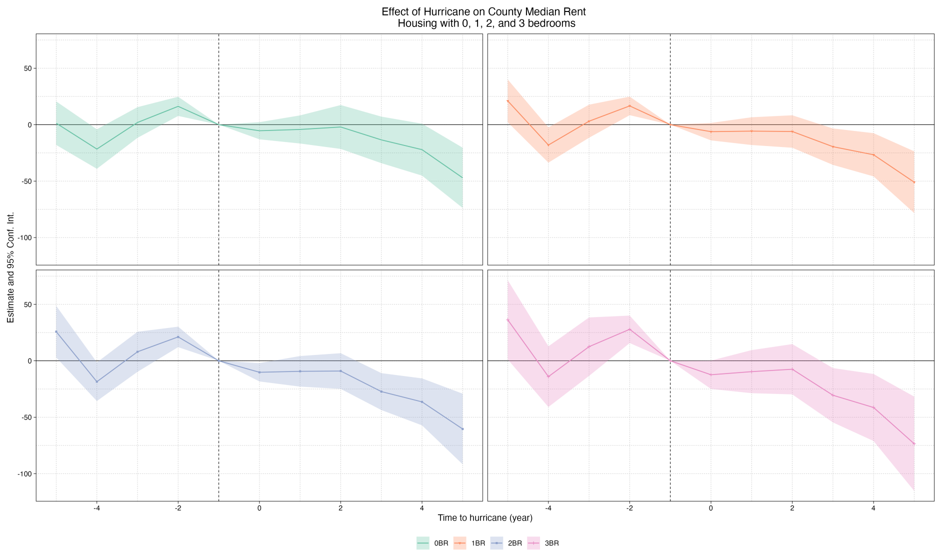
3.4 Results

The β_t coefficient estimates of our event study specification from Equation 3.1 are presented in Figure 3.2. We find that in the years prior to hurricane exposure, there is little statistically significant variation in median contract rent: in other words, the difference in median contract rent between eventually-treated and never-treated counties remains constant in the years prior to the hurricane exposure. For the five years following hurricane exposure, we find that median rent persistently decreases, by up to \$75 for three-bedroom properties. This corresponds to a 6.2 percent decrease on a baseline of \$1,204 for three-bedroom properties.

3.5 Discussion

Our initial findings of an decrease in rental prices following hurricane exposure is consistent with housing sales dynamics documented in earlier literature. We document an equilibrium effect of hurricanes on rental rates: elucidating the extent to which supply-side and demand-side factors lead to this overall decline in rental prices is beyond the scope of this study. The extent to which demand or supply shocks contribute to our result requires further analysis

Figure 3.2: Event Study Estimates of Equation 3.1, for Median Contract Rent



Note: This figure plots the β_t coefficient estimates from Equation 3.1, using first year of hurricane exposure as the source of variation. The regression is estimated on median contract rent for 0-, 1-, 2-, and 3-bedroom units. The x-axis plots event time, where '0' denotes the year of exposure, negative event-time denotes the years prior to exposure, and positive event-time denotes the years following exposure. The single point denotes the point estimate of the coefficient, and the shaded region denotes the 95 percent confidence interval. All event study specifications are estimated using a data sample that is balanced in event-time, so all county units have median contract rent data for the full range of event time: [-5,6].

of alternate attributes of the rental market. We can also study other metrics of housing expenses, other than median rent. We plan to do this with restricted-access Census data, which we have gained permission to access. We can study changes in the following attributes, which would help us understand how rental supply varies with hurricane exposure, as well as housing quality.

- *Rental quality*: Percent renters and owners in Census block-year (from the American Community Survey). Home quality indicators such as prevalence of home improvements, prevalence of mold on the property, number of buildings abandoned nearby, etc. (from the American Housing Survey)

- *Rental expenses*: Rental price per month, property tax amounts, mortgage payment amounts, rent subsidy amounts, maintenance expenses, monthly housing costs, monthly utility amounts (from the American Community Survey and the American Housing

Survey)

We also hope to further develop understanding of the heterogeneity in post-disaster outcomes between homeowners and renters by examining differences in adaptive responses such as migration, and comparing *ex-ante* and *ex-post* disaster protection and aid policy provision between groups. In other words: are renters and homeowners equally protected against disaster risks, and how does it impact their adaptive responses?

3.6 Conclusion

This project seeks to study a pre-dominantly understudied population - renters - and how natural disasters impact rental markets. We find that hurricane exposure substantially and persistently decreases median rent for properties of various sizes. In future iterations of this project, we hope to better understand the supply-side and demand-side factors behind this rent increase, and document heterogeneity between homeowner and renter disaster protection and adaptive responses. Understanding the impacts of natural disasters on renters and rental markets is essential to fully understanding the distributional impacts of natural disasters.

APPENDIX A

APPENDIX TO LEVEES: INFRASTRUCTURE AND INSURANCE AS ADAPTATION TO FLOOD RISK

A.1 Alternate Specification of Joint Construction-Accreditation Specification

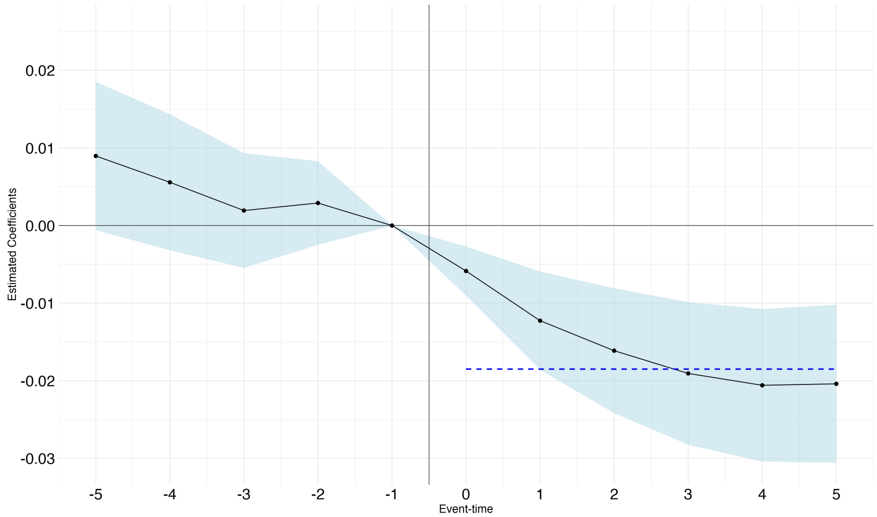
In this section, we present an alternate specification of Equation 1.3, which models the construction and accreditation effects jointly in order to measure the effect of accreditation after controlling for the pre-trend caused by construction. In this specification, instead of estimating static effects for post-construction and post-accreditation we dynamically estimate the effects of construction and accreditation for up to five years following the events. The results of the alternate specification (Equation A.1) is in Table A.1. The coefficients of interest are β_{τ_C} and β_{τ_A}, which measure the causal effect of construction and accreditation on insurance take-up, at various points in event time. τ_C denotes event time around the year of construction, and τ_A denotes event time around the year of accreditation. Overall, we find results where the sign and magnitude is consistent with what we observe in the estimation of the static specification, but not statistically significant. Thus, the static specification where we pool all post-construction coefficients, and post-accreditation coefficients is our preferred specification.

$$
\begin{aligned}
Y_{ilt} = \, & \phi_i + \gamma_t \\
& + \delta_0 Treated_{il} + \sum_{\tau_C} \alpha_{\tau_C} D_{ilt}^{\tau_C} + \sum_{\tau_A} \alpha_{\tau_A} D_{ilt}^{\tau_A} \\
& + \sum_{\tau_C} \beta_{\tau_C} Treated_{il} \times D_{ilt}^{\tau_C} \\
& + \sum_{\tau_A} \beta_{\tau_A} Treated_{il} \times D_{ilt}^{\tau_A}
\end{aligned}
\tag{A.1}
$$

Table A.1: Event Study Estimates of Equation A.1

	Dependent variable:
	Policy Take-up Rate
Treated	0.011** (0.005)
$D^C(\tau < -5)$	−0.003 (0.002)
$D^C(\tau = -5)$	0.001 (0.001)
$D^C(\tau = -4)$	0.001* (0.001)
$D^C(\tau = -3)$	0.001** (0.001)
$D^C(\tau = -2)$	0.001** (0.0004)
$D^C(\tau = 0)$	−0.001** (0.0005)
$D^C(\tau = 1)$	−0.002** (0.001)
$D^C(\tau = 2)$	−0.002** (0.001)
$D^C(\tau = 3)$	−0.003** (0.001)
$D^C(\tau = 4)$	−0.002* (0.001)
$D^C(\tau = 5)$	−0.003* (0.002)
$D^C(\tau > 5)$	−0.003 (0.003)
$D^A(\tau < -5)$	0.033*** (0.003)
$D^A(\tau =)$-5	0.013*** (0.001)
$D^A(\tau =)$-4	0.008*** (0.001)
$D^A(\tau =)$-3	0.006*** (0.001)
$D^A(\tau =)$-2	0.0002 (0.0001)
$D^A(\tau =)$0	−0.007*** (0.0005)
$D^A(\tau =)$1	−0.011*** (0.001)
$D^A(\tau =)$2	−0.016*** (0.002)
$D^A(\tau =)$3	−0.011*** (0.001)
$D^A(\tau =)$4	−0.013*** (0.002)
$D^A(\tau =)$5	−0.013*** (0.002)
$D^A(\tau =)$6	−0.005*** (0.002)
Treated $\times D^C(\tau < -5)$	0.032** (0.015)
Treated $\times D^C(\tau = -5)$	0.017* (0.010)
Treated $\times D^C(\tau = -4)$	0.013 (0.008)
Treated $\times D^C(\tau = -3)$	0.008 (0.006)
Treated $\times D^C(\tau = -2)$	0.006** (0.003)
Treated $\times D^C(\tau = 0)$	−0.007 (0.004)
Treated $\times D^C(\tau = 1)$	−0.008 (0.011)
Treated $\times D^C(\tau = 2)$	−0.012 (0.014)
Treated $\times D^C(\tau = 3)$	−0.017 (0.014)
Treated $\times D^C(\tau = 4)$	−0.018 (0.015)
Treated $\times D^C(\tau = 5)$	−0.020 (0.016)
Treated $\times D^C(\tau > 5)$	−0.024 (0.017)
Treated $\times D^A(\tau < -5)$	−0.036** (0.014)
Treated $\times D^A(\tau = -5$	−0.015* (0.009)
Treated $\times D^A(\tau = -4)$	−0.008 (0.007)
Treated $\times D^A(\tau = -3)$	−0.008 (0.007)
Treated $\times D^A(\tau = -2)$	0.0002 (0.003)
Treated $\times D^A(\tau = 0)$	0.002 (0.005)
Treated $\times D^A(\tau = 1)$	0.005 (0.007)
Treated $\times D^A(\tau = 2)$	0.011 (0.010)
Treated $\times D^A(\tau = 3)$	0.011 (0.010)
Treated $\times D^A(\tau = 4)$	0.013 (0.011)
Treated $\times D^A(\tau = 5)$	0.018 (0.013)
Treated $\times D^A(\tau > 5)$	0.010 (0.015)
Observations	20,080
R^2	0.787
Adjusted R^2	0.786
Residual Std. Error	0.039 (df = 19952)
Note:	*p<0.1; **p<0.05; ***p<0.01

Figure A.1: Event Study Estimates of the Effect of Construction on Flood Insurance Take-up, with +/- 5 Year Event Time



A.2 Event Study Specification with 5-year Pre- and Post-Periods

In this section, we present a robustness check of our primary event study specification of the effect of levee construction on flood insurance take-up, using a wider range of event time than our preferred specification. This robustness check estimates Equation 1.1, but with an event time range of $\tau \in [-5, 5]$. Overall, we find results that are largely consistent in sign, magnitude, and statistical significance with our preferred specification, which uses $\tau \in [-3, 3]$.

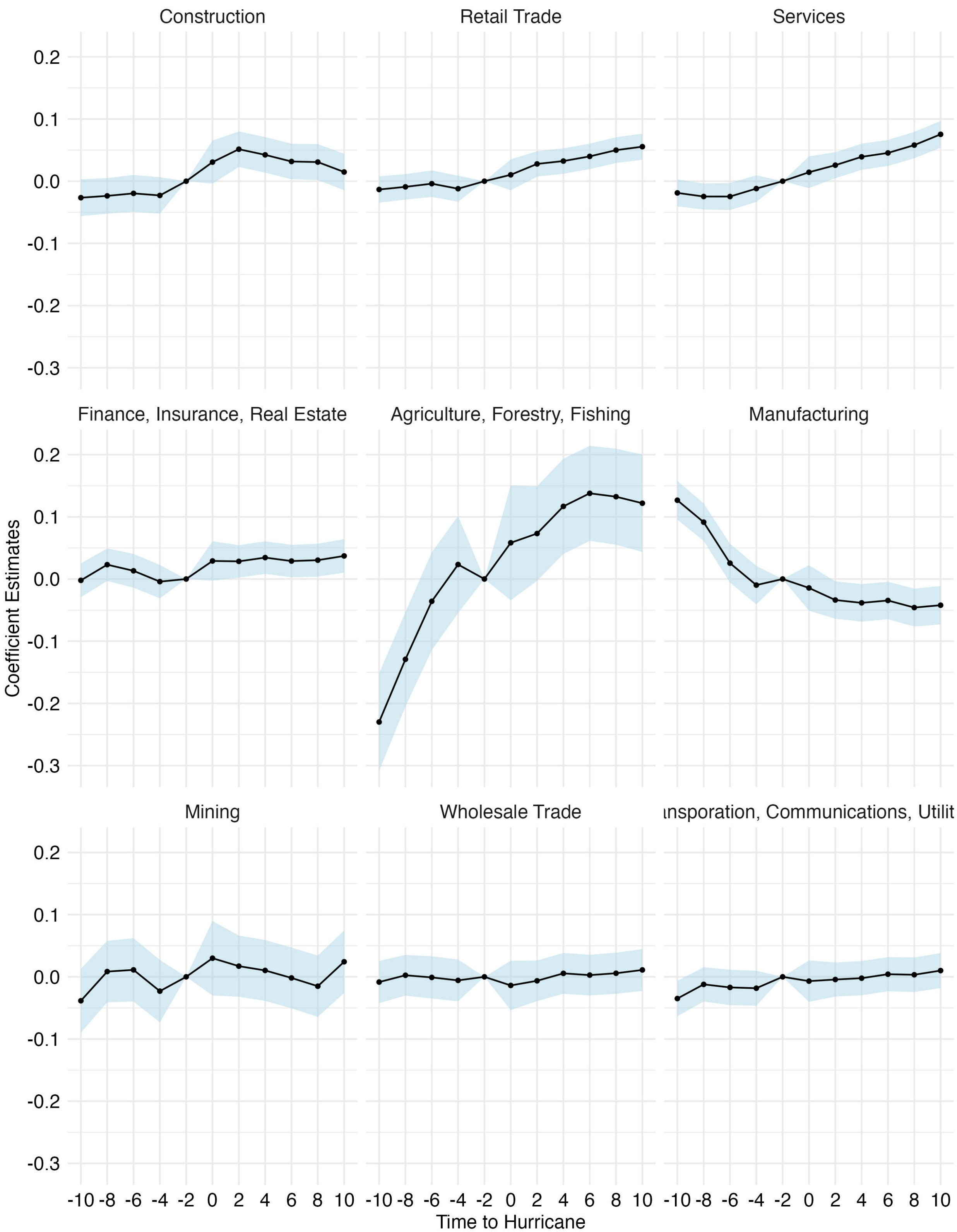
APPENDIX B

APPENDIX TO NATURAL DISASTERS AND LOCAL BUSINESS TRENDS

B.1 Event Study Results for Full Set of Industry Sub-Classes

Appendix Figure B.1 depicts the results of the event study specification for additional industry classes not depicted in Figure 2.6. However, the standard difference-in-differences assumptions are largely unsatisfied for those industries, since many of them exhibit substantial “pre-trends", or in other words, a statistically significant difference in trends between the treated and control counties in the years prior to the hurricane event. Because of this lack of internal validity, I cannot causally identify the effect of hurricane exposure on several industries, particularly capital-intensive industries such as transportation and manufacturing. This is a rich area for future research, to create more individualized control groups by industry, or explore more granular industry-level data to gain a better sense of how these industries are affected by hurricane exposure.

Figure B.1: Event Study Estimates for Share of Establishments, by Industry



REFERENCES

Measuring Community Resilience: The Role of the Community Rating System (CRS) (June 13, 2016). Available at SSRN: https://ssrn.com/abstract=2788230 or http://dx.doi.org/10.2139/ssrn.2788230.

Hurricane anatomy. URL `https://www.weather.gov/source/zhu/ZHU_Training_Page/tropical_stuff/hurricane_anatomy/hurricane_anatomy.html`.

Sep 2017. URL `https://www.cbo.gov/system/files/115th-congress-2017-2018/reports/53028-nfipreport2.pdf`.

Nov 2022. URL `https://www.cbo.gov/publication/58840`.

Kristin Aarland and Carolina K. Reid. Homeownership and residential stability: does tenure really make a difference? International Journal of Housing Policy, 19(2):165–191, 2019. ISSN 19491255. doi:10.1080/19491247.2017.1397927.

J.C.J.H. Aerts, W.J. Botzen, K.C. Clarke, S.L. Cutter, J.W. Hall, B. Merz, E. Michel-Kerjan, J. Mysiak, S. Surminski, and H. Kunreuther. Integrating human behaviour dynamics into disaster risk assessment. Nature Climate Change, 8:193–199, March 2018.

Susan Athey and Guido W. Imbens. Design-based analysis in Difference-In-Differences settings with staggered adoption. Journal of Econometrics, (xxxx), 2021. ISSN 03044076. doi:10.1016/j.jeconom.2020.10.012. URL `https://doi.org/10.1016/j.jeconom.2020.10.012`.

Ajita Atreya and Howard C. Kunreuther. Measuring Community Resilience: The Role of the Community Rating System (CRS). SSRN Electronic Journal, 2016. ISSN 1556-5068. doi:10.2139/ssrn.2788230.

Ajita Atreya, Susana Ferreira, and Erwann Michel-Kerjan. What drives households to buy flood insurance? New evidence from Georgia. Ecological Economics, 117:153–161, September 2015. ISSN 09218009. doi:10.1016/j.ecolecon.2015.06.024.

Maximilian Auffhammer and Ryan Kellogg. Clearing the Air? The Effects of Gasoline Content Regulation on Air Quality. American Economic Review, 101(6):2687–2722, October 2011. ISSN 0002-8282. doi:10.1257/aer.101.6.2687.

Laura A Bakkensen and Lint Barrage. Going Underwater? Flood Risk Belief Heterogeneity and Coastal Home Price Dynamics. The Review of Financial Studies, 00:1–44, 2021.

Laura A. Bakkensen and Lala Ma. Sorting over flood risk and implications for policy reform. Journal of Environmental Economics and Management, 104:1–23, 2020. ISSN 10960449. doi:10.1016/j.jeem.2020.102362.

Laura Bakkensen Lint Barrage and Working. Climate Shocks, Cyclones, and Economic Growth: Bridging the Micro-Macro Gap. Working Paper 24893, (2):1–36, 2018.

Lint Barrage and Jacob Furst. Housing investment, sea level rise, and climate change beliefs. Economics Letters, 177:105–108, April 2019.

Allan Beltrán, David Maddison, and Robert Elliott. The impact of flooding on property prices: A repeat-sales approach. Journal of Environmental Economics and Management, 95:62–86, 2019. ISSN 10960449. doi:10.1016/j.jeem.2019.02.006.

Asaf Bernstein, Matthew T. Gustafson, and Ryan Lewis. Disaster on the horizon: The price effect of sea level rise. Journal of Financial Economics, 134(2):253–272, 2019. ISSN 0304405X. doi:10.1016/j.jfineco.2019.03.013. URL https://doi.org/10.1016/j.jfineco.2019.03.013.

Stephen B. Billings, Emily Gallagher, and Lowell Ricketts. Let the Rich Be Flooded: The Unequal Impact of Hurricane Harvey on Household Debt. SSRN Electronic Journal, jun 2019. ISSN 1556-5068. doi:10.2139/ssrn.3396611. URL https://papers.ssrn.com/abstract=3396611.

Okmyung Bin and Craig E. Landry. Changes in implicit flood risk premiums: Empirical evidence from the housing market. Journal of Environmental Economics and Management, 65:361–376, may 2013. ISSN 00950696. doi:10.1016/j.jeem.2012.12.002.

Okmyung Bin, Jamie Brown Kruse, and Craig Landry. Flood Hazards, Insurance Rates, and Amenities: Evidence from the Coastal Housing Market. The Journal of Risk and Insurance, 75(1):63–82, 2008.

Leah Platt Boustan, Matthew E Kahn, and Paul W Rhode. COPING WITH ECONOMIC AND ENVIRONMENTAL SHOCKS: INSTITUTIONS AND OUTCOMES: Moving to Higher Ground: Migration Response to Natural Disasters in the Early Twentieth Century. The American economic review, 102(3):238–244, 2012. ISSN 0002-8282. URL https://www.aeaweb.org/articles.php?doi=10.1257/aer.102.3.238.

Leah Platt Boustan, Matthew E. Kahn, Paul W. Rhode, and Maria Lucia Yanguas. The effect of natural disasters on economic activity in US counties: A century of data. Journal of Urban Economics, 118, 2020a. ISSN 00941190. doi:10.1016/j.jue.2020.103257.

Leah Platt Boustan, Matthew E. Kahn, Paul W. Rhode, and Maria Lucia Yanguas. The effect of natural disasters on economic activity in US counties: A century of data. Journal of Urban Economics, 118:103257, July 2020b. ISSN 00941190.

Philippe Bracke. House prices and rents: Microevidence from a matched data set in central london. Real Estate Economics, 43(2):403–431, 2015.

Jacob T. Bradt and Joseph E. Aldy. Private Benefits from Public Investment in Climate Adaptation and Resilience. Unpublished Manuscript, July 2023.

Jacob T. Bradt, Carolyn Kousky, and Oliver E.J. Wing. Voluntary Purchases and Adverse Selection in the Market for Flood Insurance. Journal of Environmental Economics and Management, 110:102515, October 2021. ISSN 00950696. doi:10.1016/j.jeem.2021.102515.

Jesse Bricker, Brian Bucks, Arthur Kennickell, Traci Mach, and Kevin Moore. Finance and Economics Discussion Series Divisions of Research Statistics and Monetary Affairs Federal Reserve Board , Washington , D . C . Surveying the Aftermath of the Storm : Changes in Family Finances from 2007 to 2009 Surveying the Aftermath of the. Technical report, Federal Reserve Board, Washington D.C., 2011.

Samuel D. Brody, Sammy Zahran, Praveen Maghelal, Himanshu Grover, and Wesley E. Highfield. The Rising Costs of Floods: Examining the Impact of Planning and Development Decisions on Property Damage in Florida. Journal of the American Planning Association, 73(3):330–345, September 2007. ISSN 0194-4363, 1939-0130. doi:10.1080/01944360708977981.

Burges & Niple, Inc. West Columbus Local Flood Protection Project, 2022.

Brantly Callaway and Pedro H.C. Sant'Anna. Difference-in-Differences with multiple time periods. Journal of Econometrics, (xxxx):1–31, 2020. ISSN 18726895. doi:10.1016/j.jeconom.2020.12.001. URL https://doi.org/10.1016/j.jeconom.2020.12.001.

Brantly Callaway and Pedro HC Sant'Anna. Difference-in-differences with multiple time periods. Journal of econometrics, 225(2):200–230, 2021.

Colin F. Camerer and Howard Kunreuther. Decisions Processes for Low Probability Events: Policy Implications. Journal of Policy Analysis and Management, 8(4):565–592, 1989.

Center for Emergency Management and Homeland Security. 1960-2019 U.S. Hazard Losses. Technical report, Arizona State University, 2022.

CFR. Code of Federal Regulations: Title 44. Emergency Management and Assisstance, 44.

Joyce J. Chen, Valerie Mueller, Yuanyuan Jia, and Steven Kuo Hsin Tseng. Validating migration responses to flooding using satellite and vital registration data. American Economic Review, 107(5):441–445, 2017. ISSN 00028282. doi:10.1257/aer.p20171052.

David M. Cutler and Jonathan Gruber. Does Public Insurance Crowd out Private Insurance? The Quarterly Journal of Economics, 111(2):391–430, 1996a.

David M. Cutler and Jonathan Gruber. Does Public Insurance Crowd Out Private Insurance? The Quarterly Journal of Economics, 111(2):391–430, May 1996b.

Meri Davlasheridze and Pinar C Geylani. Small business vulnerability to floods and the effects of disaster loans. Small Business Economics, 49:865–888, 2017.

Clément De Chaisemartin and Xavier d'Haultfoeuille. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. American Economic Review, 110(9):2964–2996, 2020.

Brooks Depro, Christopher Timmins, and Maggie O'neil. White flight and coming to the nuisance: can residential mobility explain environmental injustice? Journal of the Association of Environmental and Resource Economists, 2(3):439–468, 2015. ISSN 23335963. doi:10.1086/682716.

Tatyana Deryugina. The fiscal cost of hurricanes: Disaster aid versus social insurance. American Economic Journal: Economic Policy, 9(3):168–198, 2017. ISSN 1945774X. doi:10.1257/pol.20140296.

Tatyana Deryugina and David Molitor. Does when you die depend on where you live? Evidence from hurricane katrina. American Economic Review, 110(11):3602–3633, 2020. ISSN 19447981. doi:10.1257/AER.20181026.

Manasi Deshpande and Yue Li. Who Is Screened Out? Application Costs and the Targeting of Disability Programs. American Economic Journal: Economic Policy, 11(4):213–248, November 2019.

Manasi Deshpande, Tal Gross, and Yalun Su. Disability and Distress: The Effect of Disability Programs on Financial Outcomes. American Economic Journal: Applied Economics, 13 (2):151–178, April 2021.

Klaus Desmet, Robert Kopp, Scott Kulp, Dávid Nagy, Michael Oppenheimer, Esteban Rossi-Hansberg, and Benjamin Strauss. Evaluating the Economic Cost of Coastal Flooding. National Bureau of Economic Research, pages 1–34, 2019. ISSN 0898-2937.

Robin L Dillon-merrill. Natural Disasters and Housing Markets . The Tenure Choice Channel . Working Paper, 2018.

Issac Ehrlich and Gary S. Becker. Market Insurance, Self-Insurance, and Self-Protection. Journal of Political Economy, 80(4):623–648, August 1972.

Federal Emergency Management Agency. Meeting the Criteria for Accrediting Levee Systems on Flood Insurance Rate Maps. Technical report, US Department of Homeland Security, May 2021. https://www.fema.gov/sites/default/files/documents/fema_meeting-criteria-accrediting.pdf.

Federal Housing Finance Agency. All-Transactions House Price Indexes, 2022.

Federal Reserve Economic Data. Personal Income. St. Louis Fed, October 2022.

Harrison Fell and Carolyn Kousky. The value of levee protection to commercial properties. Ecological Economics, 119:181–188, November 2015.

Melanie Gall, Kevin A. Borden, Christopher T. Emrich, and Susan L. Cutter. The Unsustainable Trend of Natural Hazard Losses in the United States. Sustainability, 3(11): 2157–2181, November 2011. ISSN 2071-1050. doi:10.3390/su3112157.

Justin Gallagher. Learning about an Infrequent Event: Evidence from Flood Insurance Take-Up in the United States. American Economic Journal: Applied Economics, 6(3): 206–233, July 2014. ISSN 1945-7782, 1945-7790. doi:10.1257/app.6.3.206.

Justin Gallagher and Daniel Hartley. Household Finance after a Natural Disaster: The Case of Hurricane Katrina. American Economic Journal: Economic Policy, 9(3):199–228, 2017. doi:10.1257/pol.20140273. URL https://doi.org/10.1257/pol.20140273.

Joshua Gallin. The Long-Run Relationship Between House Prices and Rents. Technical report, 2008.

Gerald E. Galloway, Gregory B. Baecher, Douglas Plasencia, Kevin G. Coulton, Jerry Louthain, Mohamed Bagha, and Antonio R. Levy. Assessing the Adequacy of the National Flood Insurance Program's 1 Percent Flood Standard, October 2006.

US GAO. Natural disasters: Economic effects of hurricanes katrina, sandy, harvey, and irma. Accessed on October, 9:2020, 2020.

Will Georgic and H. Allen Klaiber. A Flood of Construction: The Role of Levees in Urban Floodplain Development. Land Economics, 98(1):78–97, February 2022.

Andrew Goodman-Bacon. Difference-in-Differences With Variation in Treatment Timing. National Bureau of Economic Research, 17(5):684–694, 2018. ISSN 00027642. URL https://doi.org/10.1016/j.smr.2019.08.005%0Ahttp://dx.doi.org/10.1016/j.smr.2009.02.001%0Ahttps://scholarship.shu.edu/student_scholarship.

Joshua S. Graff Zivin, Yanjun Liao, and Yann Panassie. How Hurricanes Sweep Up Housing Markets: Evidence from Florida. Working Paper, 2020. ISSN 0898-2937.

Hannah Hennighausen and Jordan F. Suter. Flood Risk Perception in the Housing Market and the Impact of a Major Flood Event. Land Economics, 96(3):366–383, jan 2020. ISSN 15438325. doi:10.3368/le.96.3.366. URL http://creativecommons.org/licenses/by-nc-nd/4.0.

Herdiana. Does Information About Climate Risk Affect Property Values? Journal of Chemical Information and Modeling, 53(9):1689–1699, 2013. ISSN 1098-6596.

Rebecca Hersher. Levees Make Mississippi River Floods Worse, But We Keep Building Them, May 2018.

Rebecca Hersher and Robert Benincasa. How Federal Disaster Money Favors the Rich, 2019.

Miyuki Hino and Marshall Burke. Do property markets price environmental risk ? 2019.

Caroline A Hopkins. Flood Hazard Mitigation and the Role of the Government : A Dynamic Model of Local Government Investment in a Public Good. 2020.

Diane P. Horn. National Flood Insurance Program: The Current Structure and Risk Rating 2.0. CRS Report, Congressional Research Service, November 2019.

Diane P. Horn and Baird Webel. Introduction to the National Flood Insurance Program (NFIP). CRS Report R44593, Congressional Research Service, 2022.

Solomon M Hsiang and Amir S Jina. The causal effect of environmental catastrophe on long-run economic growth: Evidence from 6,700 cyclones. Technical report, National Bureau of Economic Research, 2014.

Youqin Huang, Daichun Yi, and William A V Clark. A homeownership paradox: why do Chinese homeowners rent the housing they live in? Housing Studies, 2020. doi:10.1080/02673037.2020.1793916. URL `https://www.tandfonline.com/action/journalInformation?journalCode=chos20`.

P. Hudson, L. T. De Ruig, M. C. de Ruiter, O. J. Kuik, W. J.W. Botzen, X. Le Den, M. Persson, A. Benoist, and C. N. Nielsen. An assessment of best practices of extreme weather insurance and directions for a more resilient society. Environmental Hazards, 19 (3):301–321, 2020. ISSN 18780059. doi:10.1080/17477891.2019.1608148.

Intergovernmental Panel on Climate Change. Climate Change 2022 – Impacts, Adaptation and Vulnerability: Working Group II Contribution to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, 1 edition, 2022. ISBN 978-1-00-932584-4. doi:10.1017/9781009325844. `https://www.cambridge.org/core/product/identifier/9781009325844/type/book`.

Lin Jerch, Kahn. Local Public Finance Dynamics and Hurricane Shocks. NBER, 2021. ISSN 1098-6596.

Ruixue Jia, Xiao Ma, and Victoria Wenxin Xie. Expecting floods: Firm entry, employment, and aggregate implications. 2022.

Di Jin, Porter Hoagland, Donna K. Au, and Jun Qiu. Shoreline change, seawalls, and coastal property values. Ocean & Coastal Management, 114:185–193, September 2015.

Matthew E. Kahn and Somik V. Lall. Enhancing Climate Change Resilience through Self-Protection, Public Investment, and Market Insurance. Brookings, October 2021.

Natalie Keegan, Rawle O. King, Nicole T. Carter, and Megan Stubbs. Locally Operated Levees: Issues and Federal Programs, April 2011.

Ryan Kellman, Rebecca Hersher, and Lauren Sommer. Undisclosed : Most Homebuyers And Renters Aren ' t Warned About Flood Or Wildfire Risk, 2020.

David L. Kelly and Renato Molina. Adaptation Infrastructure and its Effects on Property Values in the Face of Climate Risk, 2022.

C. Kousky. Learning from Extreme Events: Risk Perceptions after the Flood. Land Economics, 86(3):395–422, August 2010.

Carolyn Kousky. Understanding the Demand for Flood Insurance. Natural Hazards Review, 12(2):96–110, May 2011.

Carolyn Kousky. Disasters as Learning Experiences or Disasters as Policy Opportunities? Examining Flood Insurance Purchases after Hurricanes: Examining Flood Insurance Purchases after Hurricanes. Risk Analysis, 37(3):517–530, March 2017.

Carolyn Kousky. Financing Flood Losses: A Discussion of the National Flood Insurance Program: Financing Flood Losses. Risk Management and Insurance Review, 21(1):11–32, March 2018.

Carolyn Kousky and Howard Kunreuther. Improving Flood Insurance and Flood-Risk Management: Insights from St. Louis, Missouri. Natural Hazards REview, 11(4):162–172, November 2010.

Carolyn Kousky and Howard Kunreuther. Addressing Affordability in the National Flood Insurance Program. Journal of Extreme Events, 01(01):1450001, August 2014. doi:10.1142/S2345737614500018.

Carolyn Kousky and Erwann Michel-Kerjan. 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(3):819–850, September 2017.

Carolyn Kousky, Erzo F. P. Luttmer, and Richard J. Zeckhauser. Private investment and government protection. Journal of Risk and Uncertainty, 33(1-2):73–100, September 2006.

Carolyn Kousky, Howard Kunreuther, Brett Lingle, and Leonard Shabman. The Emerging Private Residential Flood Insurance Market in the United States. Technical report, Risk Management and Decision Processes Center, Wharton, University of Pennsylvannia, July 2018a.

Carolyn Kousky, Erwann O. Michel-Kerjan, and Paul A. Raschky. Does federal disaster assistance crowd out flood insurance? Journal of Environmental Economics and Management, 87:150–164, 2018b. ISSN 10960449. doi:10.1016/j.jeem.2017.05.010. URL `http://dx.doi.org/10.1016/j.jeem.2017.05.010`.

Carolyn Kousky, Erwann O. Michel-Kerjan, and Paul A. Raschky. Does Federal Disaster Assistance Crowd Out Flood Insurance? Journal of Environmental Economics and Management, 87:150–164, January 2018c.

Howard Kunreuther. Mitigating Disaster Losses through Insurance. Journal of Risk and Uncertainty, 12:171–197, 1996.

Howard Kunreuther. Disaster Mitigation and Insurance: Learning from Katrina. The Annals of the American Academy, 604, March 2006.

Howard Kunreuther and Mark Pauly. Rules rather than discretion: Lessons from Hurricane Katrina. Journal of Risk and Uncertainty, 33(1-2):101–116, September 2006.

Finn E. Kydland and Edward C. Prescott. Rules Rather than Discretion: The Inconsistency of Optimal Plans. Journal of Political Economy, 85(3):473–491, June 1977.

Jee Young Lee and Shannon Van Zandt. Housing Tenure and Social Vulnerability to Disasters: A Review of the Evidence. Journal of Planning Literature, 34(2):156–170, 2019. ISSN 15526593. doi:10.1177/0885412218812080.

Jee Young Lee and Shannon Van Zandt. Housing Tenure and Social Vulnearbility to Disasters: A Review of Evidence. Journal of Planning Literature, 34(2):156–170, 2019.

Seunghoon Lee. Adaptation to Natural Disasters by Better Information : Evidence from the Home Seller Disclosure Requirement. pages 1–46, 2020.

Seunghoon Lee. Adapting to Natural Disasters through Better Information: Evidence from the Home Seller Disclosure Requirement. SSRN Electronic Journal, November 2021.

Yanjun Liao and Philip Mulder. What's at Stake? Understanding the Role of Home Equity in Flood Insurance Demand, August 2021.

Jessica Ludy and G. Matt Kondolf. Flood Risk Perception in Lands "Protected" by 100-Year levees. Natural Hazards, 61(2):829–842, March 2012.

Parag Mahajan and Dean Yang. Taken by storm: Hurricanes, Migrant networks, and US immigration. American Economic Journal: Applied Economics, 12(2):250–277, 2020. ISSN 19457790. doi:10.1257/app.20180438.

Scott Markley, Steven R. Holloway, Taylor Hafley, and Mathew Hauer. Historical Housing Unit and Urbanization Database, 2022.

Erwann Michel-Kerjan and Howard Kunreuther. Redesigning Flood Insurance. page 3.

Erwann Michel-Kerjan and Howard Kunreuther. Redesigning Flood Insurance. Science (New York, N.Y.), 333, July 2011.

Erwann O. Michel-Kerjan and Carolyn Kousky. Come Rain or Shine: Evidence on Flood Insurance Purchases in Florida. Journal of Risk and Insurance, 77(2):369–397, January 2010.

Douglas L Miller. An introductory guide to event study models. Journal of Economic Perspectives, 37(2):203–230, 2023.

P. C. D. Milly, Julio Betancourt, Malin Falkenmark, Robert M. Hirsch, Zbigniew W. Kundzewicz, Dennis P. Lettenmaier, and Ronald J. Stouffer. Stationarity Is Dead: Whither Water Management? Science (New York, N.Y.), 319(5863):573–574, February 2008. ISSN 0036-8075, 1095-9203. doi:10.1126/science.1151915.

Missouri Census Data Center. Geocorr 2010: Geographic Correspondence Engine. Office of Social and Economic Data Analysis, 2014.

Nicholas Muller and Caroline Hopkins. Hurricane Katrina Floods New Jersey: The Role of Information in the Market Response to Flood Risk. Technical report, National Bureau of Economic Research, Cambridge, MA, jun 2019. URL http://www.nber.org/papers/w25984.pdf.

Jamie T Mullins, Prashant Bharadwaj, and Umassedu Bharadwaj. Weather, Climate, and Migration in the United States. Technical report, 2021.

National Centers for Environmental Information. Climate at a Glance: Divisional Mapping, Average Temperature, October 2022.

National Oceanic and Atmospheric Administration (NOAA). Defining Coastal Counties. Technical report, NOAA, 2012. URL https://coast.noaa.gov/data/digitalcoast/pdf/defining-coastal-counties.pdf.

National Research Council. Levees and the National Flood Insurance Program: Improving Policies and Practices. National Academies Press, Washington, D.C., July 2013. ISBN 978-0-309-28290-1. doi:10.17226/18309.

(NOAA). Hurricanes. URL https://www.noaa.gov/education/resource-collections/weather-atmosphere/hurricanes.

OpenFEMA. FIMA NFIP Redacted Claims and Policies. FEMA, August 2022.

Francesc Ortega and Süleyman Taşpınar. Rising sea levels and sinking property values: Hurricane Sandy and New York's housing market. Journal of Urban Economics, 106: 81–100, 2018. ISSN 00941190. doi:10.1016/j.jue.2018.06.005.

B. Ouattara and E. Strobl. Hurricane strikes and local migration in US coastal counties. Economics Letters, 124(1):17–20, 2014. ISSN 01651765. doi:10.1016/j.econlet.2014.03.025. URL http://dx.doi.org/10.1016/j.econlet.2014.03.025.

Eloisa Rodriguez-Dod and Olympia Duhart. Evaluating Katrina: A Snapshot of Renters' Rights Following Disasters. Nova Law Review, 31:467, 2007.

Steven Ruggles, Sarah Flood, Ronald Goeken, Megan Schouweiler, and Matthew Sobek. IPUMS USA. IPUMS, 2022.

Abdul-Akeem Sadiq, Jenna Tyler, Douglas S. Noonan, Richard K. Norton, Shannon E. Cunniff, and Jeffrey Czajkowski. Review of the Federal Emergency Management Agency's Community Rating System Program. Natural Hazards Review, 21(1):03119001, February 2020. ISSN 1527-6988, 1527-6996. doi:10.1061/(ASCE)NH.1527-6996.0000320.

Camilo Sarmiento and Ted R. Miller. Costs and Consequences of Flooding and the Impact of the National Flood Insurance Program. Technical report, Pacific Institute for Research and Evaluation, American Institute for Research, October 2006.

Jenny Schuetz. Who is the new face of American homeownership? Technical report, Brookings Institute, 2017. URL `https://www.brookings.edu/blog/the-avenue/2017/10/09/who-is-the-new-face-of-american-homeownership/`.

Claude E. Shannon. A mathematical theory of communication. The Bell System Technical Journal, 27(3):379–423, 1948. doi:10.1002/j.1538-7305.1948.tb01338.x.

Anne B. Shlay. Low-income homeownership: American dream or delusion? Urban Studies, 43(3):511–531, 2006. doi:10.4324/9780203722671.

Todd Sinai and Nicholas S. Souleles. Owner-occupied housing as a hedge against rent risk. Quarterly Journal of Economics, 120(2):763–789, 2005. ISSN 00335533. doi:10.1162/0033553053970197.

Paramita Sinha, Martha Caulkins, and Maureen Cropper. The value of climate amenities: A comparison of hedonic and discrete choice approaches. Journal of Urban Economics, 126:103371, November 2021.

Adam B. Smith. U.S. Billion-dollar Weather and Climate Disasters, 1980 - present (NCEI Accession 0209268), 2020.

Paolo Sodini, Steffen Andersen, Edward Glaeser, Ravi Jagannathan, Holger Mueller, Julien Pennasse, Laszlo Sandor, Phillip Schnabel, and Johannes Stroebel. Identifying the Benefits From Home Ownership :. NBER Working Paper, 2016.

Eric Strobl. The economic growth impact of hurricanes: Evidence from us coastal counties. Review of Economics and Statistics, 93(2):575–589, 2011.

Surveillance, Epidemiology, and End Results Program. U.S. County Population 1969-2022. National Cancer Institute, 2022.

Mojgan Taheri Tafti and Richard Tomlinson. Decisions of Landlords about Their Renters and Rental Units after Disasters. Natural Hazards Review, 22(2):05021004, 2021. ISSN 1527-6988. doi:10.1061/(asce)nh.1527-6996.0000450.

Tamara Sheldon and Crystal Zhan. Impact of Natural Disasters of US Home Ownership. Journal of Association of Environmental and Resource Economics, 6(6):1169–1202, 2018. ISSN 13556037. doi:10.1136/hrt.2009.178616.

Adam Theising and Daniel J. Phaneuf. Using urban migration flows for non - market amenity valuation. Working Paper, 2020.

Graham A. Tobin. The Levee Love Affair: A Stormy Relationship? JAWRA Journal of the American Water Resources, 31(3):359–367, June 1995.

Brigitte Roth Tran, Daniel J Wilson, et al. The local economic impact of natural disasters. Federal Reserve Bank of San Francisco, 2020.

U.S. Army Corps of Engineers. Evaluation, Design, and Construction of Levees: Enginner Manual. Draft 1110-2-1913, Department of the Army, Washington, DC, April 2022a.

U.S. Army Corps of Engineers. National Levee Database. `https://levees.sec.usace.army.mil/`, 2022b.

U.S.C. National Flood Insurance Act of 1968, 42.

Katherine Wagner. Adaptation and Adverse Selection in Markets for Natural Disaster Insurance. Working Paper, 2020. ISSN 1556-5068. doi:10.2139/ssrn.3467329.

Katherine R.H. Wagner. Adaptation and Adverse Selection in Markets for Natural Disaster Insurance. American Economic Journal: Economic Policy, 14(3):380–421, 2022.

Patrick Walsh, Charles Griffiths, Dennis Guignet, and Heather Klemick. Adaptation, Sea Level Rise, and Property Prices in the Chesapeake Bay Watershed. Land Economics, 95 (1):19–34, February 2019.

G.F. White. Human Adjustment to Floods. Unpublished Manuscript, 1945.

Oliver E J Wing, Paul D Bates, Andrew M Smith, Christopher C Sampson, Kris A Johnson, Joseph Fargione, and Philip Morefield. Estimates of present and future flood risk in the conterminous United States. Environmental Research Letters, 13(3):034023, March 2018.

Cameron Wobus, Pearl Zheng, Justin Stein, Claire Lay, Hardee Mahoney, Mark Lorie, David Mills, R Spies, B Szafranski, and J Martinich. Projecting Changes in Expected Annual Damages from Riverine Flooding in the United States. Earth's future, 7(5):516–527, 2019.

Sammy Zahran, Stephan Weiler, Samuel D. Brody, Michael K Lindell, and Wesley E. Highfield. Modeling National Flood Insurance Policy Holding at the County Scale in Florida, 1999–2005. Econological Economics, 68(10):2627–2636, 2009.

Xian Zheng, Yu Xia, Eddie CM Hui, and Linzi Zheng. Urban housing demand, permanent income and uncertainty: Microdata analysis of hong kong's rental market. Habitat International, 74:9–17, 2018.