THE UNIVERSITY OF CHICAGO

ESSAYS ON HOUSEHOLD RESPONSES TO ENVIRONMENTAL POLICIES

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

SEUNGHOON LEE

CHICAGO, ILLINOIS

JUNE 2021

ACKNOWLEDGEMENTS

This dissertation would not be here without the attention, encouragement, and guidance of my doctoral advisors Ryan Kellogg, Koichiro Ito, and Dan Black. Ryan and Koichiro introduced me to the world of environmental economics and mesmerized me into it. They took full responsibility for what they have done. Ryan has devoted countless hours listening to my half-baked ideas and lending his A-class research acumen on what would work and what would not. He was the one who has always kept me grounded, always pushing me to be practical, and focused on my research. Koichiro is a role model and an inspiration. His accolades on my works were one of the strongest ego-booster that helped me sail through the dark times of graduate school. Dan always threw hard questions and made me explore the other side of the coin. Also, his advice oftentimes made me relax, which is badly needed for a typical PhD student. From all three of them, I learned so much about how to do research.

I am also grateful to Eyal Frank and Amir Jina for their comments and emotional support at every stage of graduate school. It was such a great fortune to work as a TA for their class and get to know them. Time spent with them made me realize that chatting with professors can be not only illuminating but fun as well.

I also thank H. Spencer Banzhaf, Lint Barrage, Zarek Brot-Goldberg, Fiona Burlig, Justin Gallagher, Jonathan Gourley, Michael Greenstone, Seojeong Lee, George Lefcoe, Christian Leuz, Anant Sudarshan, and Austin Wright for many fruitful discussions. More broadly, I have benefited immensely from the array of visitors and fellow students with whom I have interacted at the Energy Policy Institute at Chicago and Harris School of Public Policy.

Finally, I thank my family.

DEDICATION

To Jin and Logan

Contents

List of Figures

List of Tables

ABSTRACT

An increasing number of governments implement market-based environmental policies because, in theory, such policies can incentivize economic agents to undertake socially desirable activities for their own good ("harnessing market power"). Whether this claim is true or not, however, critically hinges on the design of the policy: specifically, whether it aligns private benefit to socially desirable outcomes. To that respect, understanding economic agents' responses to market-based environmental policies is important to evaluate a welfare impact. This dissertation studies how the household responds to various incentive-based climate change mitigation and adaptation policies and investigate such responses implication for social welfare.

The first chapter studies if easing information friction in the housing market regarding flood risk could reduce the population in high-risk areas and thus flood damage. By exploiting the staggered adoption of the Home Seller Disclosure Requirement across 27 states, I first show this policy reduces the average price of properties and population by 6% and 9% while increases the number of flood insurance policy counts by 40% in high-risk areas. Further, I construct a novel and objective measure of flood size using a method from hydrology and estimate the effect of the disclosure policy on flood damage conditional on flood size. I find the policy reduces damage from small-to-moderate-sized floods by an average of 9.8% in the treated communities.

In the second chapter, I study household response to a unit based food waste tax scheme (UPS) in South Korea and its welfare implications. By exploiting plausibly exogenous variation in the UPS status of each municipality due to a central government's initiative, I first show that the UPS reduces food waste by 27%

on average where 1/3 of the observed reduction in food waste is attributable to higher illegal dumping. Then, using consumer panel data on grocery shopping, I find that households reduce their grocery purchase by 3.5% after the UPS, which explains another 1/3 of the food waste reduction. Further, I calculate the lower bound of the welfare effect focusing on reduction in GHGs emissions and show that the policy creates a substantial welfare gain because benefits from the environmentally advantageous changes in consumption patterns dominate losses from illegal dumping.

In the last chapter, I investigate the importance of pricing on the conservation effort. Many public service usages are measured and thus are charged at a group level. Such a pricing scheme sets a private marginal cost lower than a group marginal cost which leads to free-riding and overuse of the services. Using apartment-level panel data, I find that switching from the group pricing to individual pricing leads to a 20% reduction in per unit food waste quantity. Further, the policy effect exhibits substantial heterogeneity along with group size, apartment price, and pre-treatment food waste quantity. Finally, I estimate the social cost of free-riding with an emphasis on government finance and greenhouse gas emission implications.

Chapter 1

Adaptation to Natural Disasters by Better Information: Evidence from the Home Seller Disclosure Requirement

1.1 Introduction

Since 1980, floods in the United States have damaged over $1 trillion, making it the costliest type of natural disaster over the last 40 years (NOAA 2020). Climate scientists predict flooding is likely to happen with higher frequency and intensity in the future (Milly et al. 2002, Ghanbari et al. 2019). Thus, effective adaptation, which is an activity to moderate or avoid harm, is becoming ever more important (IPCC 2014, Aldy and Zeckhauser 2020). While flood damage is determined by both flood intensity (i.e., physical characteristics) and the size of the population exposed to risk, the US flood policy has focused primarily on managing the former by adding engineering structures, such as levees (Changnon et al. 2000, Field et al. 2012, Tarlock 2012, Liao 2014). This approach, however, attracts more people and development to floodplains (so-called "levee-effect") by creating a false sense of security, which paradoxically increases a society's exposure to flood risk (Pinter 2005, Collenteur et al. 2015).[1] Consequently, when those engineering structures fail, either due to extreme weather conditions or improper maintenance, flood damage could become even larger than before (Pinter et al. 2016).[2] Governments end up spending billions of dollars for disaster relief and recovery even after investing a tremendous amount of resources for flood prevention (CBO 2016).

[1] Other shortcomings include (1) spillover of flood in other areas, (2) environmental degradation of the affected areas, (3) the risk of even larger damage when a flood of size exceeding the protection level comes (FEMA 2005).

[2] Flood protection structures frequently fail. For instance, over 1,000 levees failed during the Midwest Flood of 1993 (LARSON 1996). An important reason is the lack of maintenance. For instance, Pinter et al. (2016) find that only 1.9% of the levees in the US are rated "Acceptable".

This paper studies if easing information friction on flood risk in the housing market could reduce the population in high-risk areas and thus flood damage. Although an official flood map has long been publicly available, numerous earlier studies and anecdotal evidence show a lack of flood risk awareness among home buyers. For instance, Chivers and Flores (2002) find only 14% of home buyers whose property is located in high-risk areas learned about flood risk before closing.[3] Such low awareness hinders home buyers from fully internalizing the flood risk during the real estate transaction, and thus making them consume more than the privately optimal level of flood risk. As a result, more people end up living in high-risk areas. Given that a potential reason for the friction is information acquisition and processing costs (Kunreuther and Pauly 2004), the Home Seller Disclosure Requirement (hereafter "the disclosure requirement") could alleviate the problem by efficiently delivering to home buyers risk information embedded in the official flood map.[4] The policy mandates during the real estate transaction, home sellers must disclose any known property defects using a standardized form (Lefcoe 2004). Between 1992 and 2003, 27 states in the contiguous United States have implemented this requirement that included an explicit question on flood risk. A typical form asks whether a property is located in the Special Flood Hazard Area (SFHA)–an area with higher flood risk–defined by the official flood map. These forms comprise simple, easy-to-understand checkbox questions that home sellers can answer with a yes-or-no answer. Home sellers are generally required to deliver the disclosure to the home buyers before closing on the property (Stern 2005).

The disclosure requirement provides a compelling setting for evaluating the effect of providing more information on flood risk to home buyers for two reasons. First, the policy rolled out across different states with substantial variation in

[3]A group of studies has shown the price of properties located in the high flood risk area drops substantially when the perceived level of flood risk increases (Hallstrom and Smith 2005, Bin and Landry 2013, Muller and Hopkins 2019).

[4]Learning how to read the flood map is a non-trivial task that requires both physical and cognitive effort. Also, people had to make a time-consuming trip to local map repositories, especially in the 1990s.

timing over a decade, where the variation came from plausibly exogenous state court rulings on the extent of realtor liability for incomplete disclosure (Roberts 2006). In addition, the policy treats properties located in and out of the SFHA differentially, allowing me to implement a triple difference design by interacting it with the staggered adoption timing. Second, because the disclosure form asks about flood risk status in a binary manner, home buyers could receive starkly different information for two different properties on opposite sides of an SFHA border even if the actual flood risk levels are similar. This spatial discontinuity of information yields an opportunity to disentangle the information effect from the actual flood risk effect. One potential concern is that being located in the SFHA could invite other policy effects as well. To account for that possibility, I use the difference-in-discontinuity approach following Grembi et al. (2016).

To leverage these variations, I collect and construct a wide range of data on flood damage and its determinants. To explore household responses and housing price change as a result of the disclosure policy implementation, I collect data on individual property transactions and community-level National Flood Insurance Program (hereafter "flood insurance") policy counts, as well as census tract-level demographic data. To measure flood damage, I collect damage records, which is based on the replacement cost net of any depreciation, from the flood insurance adjuster's report. In addition, I construct an objective measure of past flood events for each community by conducting flood frequency analysis using the USGS/NOAA gauge records (Saharia et al. 2017b, England Jr et al. 2019). The data overcome a potential endogeneity problem with self-reported flood events data, as is the case with the National Weather Service Storm Events data (Gall et al. 2009).

Empirical exercises produce three key results. First, I find that the disclosure policy reduces the price of the properties in the SFHA by 6% in comparison to the non-SFHA properties. At the pre-disclosure average price of the properties located in the SFHA, the reduction in housing price amounts to $20,013 in 2020 dollars. I also show that the effect is entirely driven by the properties located in communities without actual flood incidents over the sample period. Because what the disclosure policy can affect is flood risk awareness, the policy will have a smaller effect when risk awareness is already high due to the actual flood incidents. Further, the difference-in-discontinuity analysis that credibly singles out the effect of flood risk information corroborates these findings. The first set of results provide a first pass at the effectiveness of the disclosure policy on flood risk information delivery and its impact on buyers' flood perception.

Second, the disclosure policy reduces the population by 9% and increases the number of flood insurance policies by over 40% in the high-risk area relative to the low-risk areas, respectively. Coinciding with population reduction, I also find that the vacancy rate goes up by 17%. Investigating household response is important because while both of these responses reduce the price of the affected properties, they have different implications for flood damage. Choosing a safer house unambiguously reduces the exposure to flood risk and thus flood damage, but subsidized flood insurance could distort such location adjustment by incentivizing households to take more risk (Peralta and Scott 2020). A substantial reduction in population in the high risk area suggests that the disclosure policy can potentially curb flood damage. It is also worth pointing out that the proportion of more educated households shrinks in the risky area after the disclosure policy while the median income of households does not change. This suggests that the effect of the disclosure policy is constrained by the households' information processing capacity rather than financial resources.

Third, the disclosure policy reduces damage from small-to-moderate-sized floods by 9.8% on average for the treated communities. The effect is driven by a combination of a reduction in the number of damaged properties and also a smaller average damage amount per damaged property, which can be explained by a higher vacancy rate. When multiplied with the pre-disclosure average community-level annual flood damage of $27,596, and the total number of communities (24,474), this translates into a $66 million reduction each year. The result is produced by exploiting a plausibly exogenous variation in flood size across communities hit by the same flood event, after accounting for a common time trend and time-invariant community characteristics, and the disclosure policy change timing. This approach allows me to estimate how the flood damage conditional on flood size changes as a result of the disclosure policy. The findings are robust to an alternative measure of flood damage and various robustness tests.

This paper contributes to three bodies of literature. First, this paper is the first comprehensive evaluation of a major flood risk information provision policy. To the best of my knowledge, two prior works studied the same policy instrument, but they either focused on a small geographic area (Pope 2008) or exploited cross-sectional variations of the disclosure requirement to explore a heterogeneous treatment effect of flood map updates on housing prices (Hino and Burke 2020). My paper studies the disclosure policy effect at the national level and uses both triple-difference and difference-in-discontinuity design to provide a causal estimate that controls for potential confounding factors. In addition, I explore the mechanism behind the housing price adjustment by studying household responses to the disclosure policy. Most importantly, my work is the first attempt to trace the impacts of flood risk information from housing prices and household responses up to a key welfare outcome of interest–flood damage–by constructing and collecting novel data on flood history and flood damage. This is a departure from a broader set of works that focused on estimating and rationalizing the effect of flood risk on housing prices from various information sources (Hallstrom and Smith 2005, Bin and Landry 2013, Muller and Hopkins 2019, Bernstein et al. 2019, Hino and Burke 2020).

Second, my work is related to the nascent literature on the economics of climate change adaptation. Whereas earlier works primarily focused on technology as a determinant of adaptation, I focus on the role of information that facilitates the alignment of private incentives and socially desirable outcomes (Miao and Popp 2014, Barreca et al. 2016, Burke and Emerick 2016, Ortiz-Bobea and Tack 2018). This finding indicates that a policy that facilitates information flow can be a powerful adaptation tool. Besides, it also has practical importance as information provision policies are getting more attention as a potential flood risk management policy tool.[5]

Third, this paper contributes to the literature on disclosure mandate policies. Earlier works have found that information affects school quality, restaurant hygiene, calorie intake, and energy efficiency (Jin and Leslie 2003, Figlio and Lucas 2004, Bollinger et al. 2011, Myers et al. 2019). However, these results are not directly transferable to the natural disaster setting, because key parameters of the disclosure policy effectiveness, such as the accuracy or comprehensibility of information are different. The findings of this paper also have policy implications for managing other environmental risks such as wildfire.

The paper proceeds as follows. Section 3.2 describes households' location choice problem with an option to purchase flood insurance after the disclosure requirement and its implications for housing price and flood damage. Section 3.3 provides background on the Home Seller Disclosure Requirement policy and the Special Flood Hazard Area. Section 3.4 details the data sources and provides some summary statistics. Section 3.5 lays out the empirical strategy. Section 1.6 presents estimation results on household responses and housing price changes while Section 1.7 shows the disclosure policy effect on flood damage. Section 1.8 discusses the results, while Section 3.7 concludes.

1.2 Conceptual Framework

In this section, I describe a spatial equilibrium in the presence of unaware flood risk following Banzhaf and Walsh (2008) to help guide the empirical analysis.[6] Suppose households' indirect utility function V is defined as $V = V(y, g, p)$, where y is income, g is an index of the amenity level in a community including flood

[5] After a series of devastating floods in recent years, both federal and state governments work toward strengthening the disclosure of flood risk. For instance, the House of Representatives passed a bill ("21st Century Flood Reform Act") that made the disclosure on flood risk a prerequisite for joining the National Flood Insurance Program (Committee on Financial Services 2017), although it did not pass the Senate. Texas drastically strengthened its existing disclosure requirement on flood risk after Hurricane Harvey ("TEXAS PROPERTY CODE" 2019).

[6] For more formal treatment including proofs, see Banzhaf and Walsh (2008).

risk, and p is the housing price.[7] Following the literature, I assume household preferences satisfy the "single-crossing" property, which means the indifference curves in the (g, p) plane are strictly increasing in income. Households choose to live in one of two communities, indexed by $j \in \{1, 2\}$. Conditional on location choice, they choose their optimal level of housing.[8]

The single-crossing property implies the equilibrium will exhibit three characteristics (Epple and Platt 1998). First, a household exists that will be indifferent between two communities ("boundary indifference"). Second, households are going to be perfectly stratified in terms of their income ("stratification"). In other words, households with income below the boundary income will prefer the lower-ordered community and vice versa. Third, the rank of communities by amenity must match the rank of the housing price ("ordered bundles").

Now, suppose community 1 is prone to flood risk and 2 is not, but without a disclosure policy, households cannot tell the difference. Also, without loss of generality, suppose the initial overall amenity level of community 2 is higher ($g_1 < g_2$), and thus $p_1 < p_2$. Household income is distributed following a distribution $f(y)$ with continuous support in $[y_L, y_H]$.The boundary indifference condition implies equation (1.1), where $\tilde{y}$ is the boundary income. For households with income level $\tilde{y}$, both communities yield the same level of utility $\bar{V}$. The boundary condition also determines the initial population share. Specifically, $N_1 = \int_{y_L}^{\tilde{y}} f(y)dy$ and $N_2 = \int_{\tilde{y}}^{y_H} f(y)dy$.

$$V(\tilde{y}, g_1, p_1) = V(\tilde{y}, g_2, p_2) = \bar{V} \tag{1.1}$$

Suppose a disclosure policy is implemented and the flood risk is disclosed. In the model's context, the risk information can be interpreted as a negative shock in the

[7] I abstracted away from the taste parameter for simplicity, but I could either introduce taste parameter α in addition to income y or replace y with α and derive the same result in terms of taste. Sieg et al. (2004), for instance, consider both income and taste, and Epple and Platt (1998) present a two dimensional stratification result.

[8] The disclosure policy can induce location choice at either individual property level or community level. The conceptual framework I present here is applicable to both scenarios. Specifically, g will be property characteristics and p is going to be housing price at the individual property level.

level of amenity such that $g_1' < g_1$.[9] As a benchmark case, first suppose the flood insurance does not exist.

As $V' = V(\tilde{y}, g_1', p_1) < V(\tilde{y}, g_2, p_2) = \bar{V}$, households at the boundary are strictly better off by moving to community 2. In a new equilibrium, N_2 and p_2 will be higher, and $\tilde{y}$ will be lower than the initial equilibrium.

Now, let's introduce an option to purchase flood insurance and explore how it affects the equilibrium. First, it is convenient to introduce the monetized reduction in indirect utility due to the flood risk revelation. Using the equivalent variation, we can define the monetized reduction in utility due to the flood risk as F in the following expression: $V(\tilde{y} - F, g_1, p_1) = V(\tilde{y}, g_1', p_1)$. Further assume $F = f + h$, where f reflects the financial cost and h is the non-financial (e.g., hassle cost) cost. Alternatively, h can be seen as a combination of non-financial and financial costs that go beyond the maximum available coverage.[10]

Reflecting the subsidized premium structure of the flood insurance, households can purchase flood insurance at premium $q < f$ and get fully insured for f. Note the insurance is still incomplete in the sense that households are still exposed to h. Importantly, with the insurance, equation (1.2) holds, and thus the magnitude of migration and housing price adjustment will be smaller than the benchmark (without insurance) case. The intuition is that the insurance is, in effect, a subsidy for living in community 1, reducing the utility gap between V' and V (Peralta and Scott 2020).

$$V' = V(\tilde{y}, g_1', p_1) = V(\tilde{y} - F, g_1, p_1) < V(\tilde{y} - (q + h), g_1, p_1) < V(\tilde{y}, g_2, p_2) = \bar{V} \quad (1.2)$$

A few additional observations are worth noting. First, p_1 is lower than the ini-

[9] Gayer et al. (2000) show that when individuals overestimate health risk in the absence of information, information revelation could reduce the perceived level of risk. This suggests the disclosure requirement could be seen as a positive shock depending on the prior perception. However, earlier works (for review, see Beltrán et al. (2018)) on flood risk information consistently found a negative impact on housing price, which indicates the disclosure of flood risk could be treated as a negative shock.

[10] For instance, the National Flood Insurance Program covers up to $250,000 for a residential property.

tial equilibrium after the disclosure in both (with and without insurance) scenarios as long as the disclosure policy successfully increases flood risk awareness ($g_1' < g_1$). N_2, the share of households living in a community with no flood risk, is also higher than the initial level.[11] Conversely, if the disclosure policy is ineffective either because people do not comply or because the information is common knowledge, the housing price will not change. Second, although both migration and insurance increase households' welfare, they could have starkly different implications for flood damage. To see this, observe that flood damage is the foregone stream of utility or rent as a result of flood and such opportunity cost will be smaller when the magnitude of the housing price adjustment for community 1 is larger.[12] Consequently, if households respond to the disclosure policy primarily by purchasing more insurance and choosing to stay in community 1, the flood damage reduction effect will be limited. Third, households are more likely to choose to migrate when the premium subsidy $f - q$ is smaller and when the non-financial cost h is relatively larger than f.

1.3 Background

1.3.1 Home Seller Disclosure Requirement

A publicly available Flood Insurance Rate Map should allow home buyers to learn if a specific property belongs to the SFHA. Also, the Flood Insurance Reform Act of 1994 requires flood insurance as a condition for federally-backed mortgage approval, which should let affected home buyers learn about the flood risk. However, prior works show home buyers, in general, are not well aware of the flood risk (Chivers and Flores 2002, Pope 2008, Bin and Landry 2013) either because infor-

[11] The model abstracts away from a moving or search cost. If we introduce it, the migration effect will be smaller.

[12] In theory, a flood insurance premium could encourage self-protection behaviors when individuals get enough reward in terms of a lower insurance premium. Unfortunately, the flood insurance program's premium structure is coarse in the sense that its premium does not reflect self-protection measures comprehensively. Moreover, the financial incentive might not be "enough" given the premium is highly subsidized (Wagner 2019, Kousky 2019) to encourage participation.

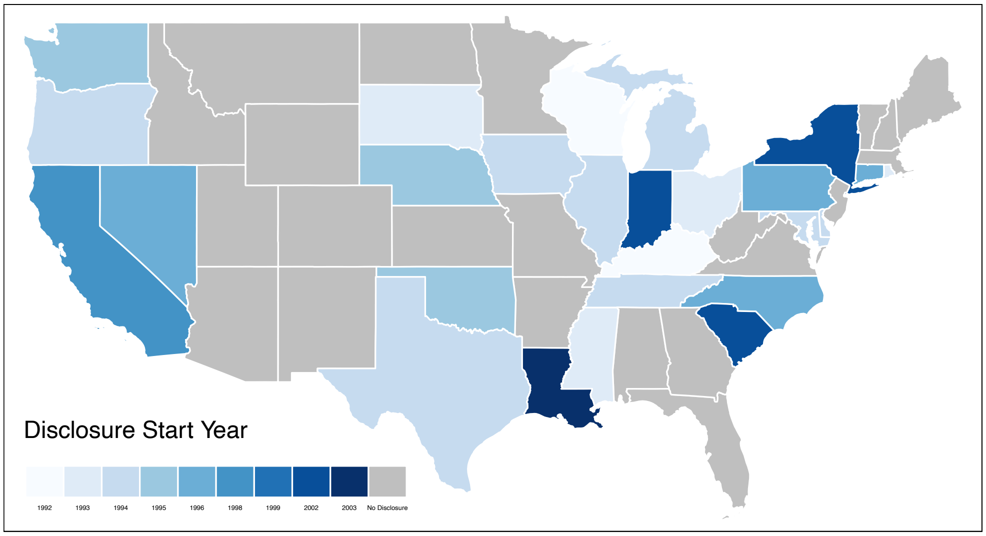


Figure 1.1: The Disclosure Requirement Implementation over Time

mation acquisition is costly (Kunreuther and Pauly 2004) or compliance with the flood insurance purchase requirement is far from perfect (Michel-Kerjan 2010).[13]

A statutory disclosure requirement could be a useful policy tool to fill this information gap. It mandates that home sellers provide buyers a detailed account of known material defects about the listed property by filling out a standardized form. A typical form asks questions about both structural components (e.g., problems with walls, roofs, or plumbing) and surroundings (e.g., natural hazards). In particular, 27 states in the contiguous US implemented the disclosure requirement between 1992 and 2003 (see Figure 1.1) with an explicit question on flood risk. While the specific language of the requirement varies from state to state, the most prevalent question asks whether a property is located in the SFHA.[14]

[13]In a similar context, Bernstein et al. (2019) show consumer sophistication matters a lot in the capitalization of climate change risk. In particular, they found that sophisticated investors drive the sea level rise exposure discount. Rich anecdotal evidence exists of ill-informed home buyers as well. For instance, see Flavelle (2017) and Satija et al. (2017). Finally, Michel-Kerjan (2010) find only 20%-30% of home owners in the SFHA purchased flood insurance in 2000.

[14]Other questions include past flood history and the flood insurance premium amount of a

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

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

Property Address: ______________________________

City, State & Zip Code: ______________________________

Seller's Name: ______________________________

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

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

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

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

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

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

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

 Page 1 of 4

Figure 1.2: Example of the Home Seller Disclosure Form (IL)

The rollout of the disclosure requirement is closely related to the demise of the *caveat emptor* or "let the buyer beware" doctrine in real estate transactions in state courts, which increasingly held listing agents responsible for the "failure" to disclose the material facts.[15] In response, the National Association of Realtors issued a resolution in 1991, encouraging state associations to develop and support legislation of the disclosure requirement (Tyszka 1995). It was primarily an effort to deflect potential liability from realtors to sellers (Washburn 1995). Importantly, the disclosure requirement is not exclusively on flood risk, but on a long list of items related to the housing condition. In addition, the timing of the policy implementation was slower in states with *Caveat Emptor* courts (Roberts 2006). Taken together, the difference in policy timing across different states is likely to be uncorrelated with the flood risk level of each state.

Earlier works on disclosure mandates suggest that multiple factors could determine the effectiveness of the disclosure policy on buyer perception. In particular, even if the policy is well-enforced, information has to be easy to understand, salient, accurate, and new to have an effect (Marshall et al. 2000, Figlio and Lucas 2004, Pope 2009, Dellavigna and Pollet 2009). As Figure 1.2 shows, the disclosure forms consist of simple checkbox questions, which deliver straightforward and easy-to-understand information regarding the property.[16] To ensure compliance, many states levy fine or even allow buyers to rescind the agreement without penalty for failure to disclose.[17] While these facts suggest that the disclosure requirement should effectively deliver the information, many states also allow waiver

property.

[15] Under *caveat emptor*, buyers are expected to exercise proper caution on potential defects of a property. Seller's duty, on the other hand, is limited to not making any false representations or actively concealing any material facts (Washburn 1995). One of the primary factors that initiated this change is higher public attention to environmental and health issues during the 1980s (Weinberger 1996).

[16] This feature is in stark contrast to many other disclosures, such as mortgages or automobile leases (White and Mansfield 2002), which could undermine the policy effect due to the information barrier.

[17] As such, the statutory requirements induce higher usage of the disclosure forms. For instance, Lahey and Redle (1997) and Lefcoe (2004) report a dramatic increase in the use of the form after the statutory requirement in Ohio and California, respectively.

upon mutual agreement. Also, the disclosure policy might fail to raise flood risk awareness if home sellers do not provide the form, home sellers provide false information, or home buyers fail to fully grasp the implication of the SFHA. Thus, the effectiveness of the disclosure requirement is an empirical question. While I cannot directly observe home buyer perceptions on flood risk, in section 1.6.1 I conduct indirect tests on the effectiveness of the disclosure policy on buyers' flood awareness. Specifically, I present results that cannot be rationalized if the disclosure was not effective.

1.3.2 Flood Risk and Special Flood Hazard Area (SFHA)

Because the disclosure form delivers flood risk information by informing whether a property is in the SFHA or not, this section briefly discusses it. The SFHA is an area that is going to be inundated with a 100-year flood, which is determined by Flood Insurance Rate Map, an official community map.[18]

The flood mapping process involves three key steps (FEMA 2005): (1) hydrologic analysis that determines the water amount in a stream channel for a given weather event; (2) hydraulic analysis that determines the water surface elevation for given water amount; and (3) floodplain mapping, which compares water elevation with the ground elevation to determine the boundary of inundation. The procedure implies that as long as the ground elevation changes continuously, flood risk is continuous as well. The continuity of flood risk gives rise to the spatial discontinuity design near the SFHA border because the disclosure form delivers flood risk information in a dichotomous manner for two areas with almost identical actual flood risk. This gives rise to a spatial discontinuity analysis but a potential concern is that the flood risk zone status invites other regulations as well.[19] Thus,

[18]Flood is defined as "a general and temporary condition of partial or complete inundation of two or more acres of normally dry land area or two or more properties from an overflow of inland or tidal waters, from unusual and rapid accumulation or runoff of surface waters from any source, or from mudflow" (FEMA 2005).

[19]Two regulations are worth noting. First, a new development in the SFHA needs to be elevated high enough to withstand the 100-year flood (Horn and Brown 2018). Second, owners of properties in the SFHA are required to purchase flood insurance as a condition of receiving

in section 1.6.1, I provide additional evidence of housing price adjustment with difference-in-discontinuity analysis, which relies on the difference of two spatial discontinuity coefficients before and after the disclosure policy.

It is also worth noting that these maps are updated occasionally. While the National Flood Insurance Reform Act of 1994 requires that FEMA assess the need to revise and update all flood maps every 5 years, the vast majority of the maps fail to meet the required update cycle (DHS Office of Inspector General 2017). This is favorable for this paper's research design because it ensures that the flood zone status of properties remains constant over the study period. Put it differently, when a flood map is updated, a fraction of properties' SFHA status would change, which makes it challenging to determine if an observed policy effect is attributable to the disclosure policy or the map update. Thus, in section 1.6.1, I present results after removing properties located in the communities that had a map update over the sample period.

The jurisdiction of each flood map is "community," a local political entity (e.g., village, town, city) defined by the National Flood Insurance Program. These entities are comparable to a US Census place. Figure B.1 in the appendix shows a sample Flood Insurance Rate Map from a part of the Town of West Hartford, Connecticut. Similar to this community, a typical entity has multiple panels that delineate flood zones. The dark area in the map represents the SFHA, and the light area is the non-SFHA. Most communities have both SFHA and non-SFHA areas within the jurisdiction. Figure B.2 in the appendix is a histogram of the fraction of the SFHA area for 7,306 communities used in the main sample. It shows that substantial variation exists in the SFHA ratio across communities.

a federally backed mortgage. However, the enforcement of these regulations is imperfect. As briefly mentioned in section 1.3.1, Michel-Kerjan (2010) find only 20%-30% of home owners in the SFHA purchased flood insurance in 2000. Also, a non-trivial number of official flood maps have been created using the "approximate method". These maps do not have the Base Flood Elevation, which is used to enforce the elevation requirement (FEMA 2005).

1.4 Data

1.4.1 Data Description

I compile a data set from six data sources: individual property transactions, community-level flood insurance takeup, tract-level demographics, property-level flood damage, and county-level federal disaster relief for five years before and after the disclosure policy change, except for the demographic characteristics, which comes from the decennial census. I also construct a community-level flood history dataset. In this section, I describe each data source and provide descriptive statistics.

Housing price, flood insurance, and population. For housing price, I use the Zillow Transaction and Assessment Database (ZTRAX).[20] It documents transaction dates, sales prices, and housing characteristics such as type (e.g., single house, condominium, etc.), exact longitude and latitude, year built, and the number of bedrooms. For flood insurance, I use community-level policy counts data.[21] Demographic characteristics come from the tract level decennial census from Geolytics. The data document the overall population level as well as demographic characteristics such as income, age, race, and education. A benefit of Geolytics data is that they keep tract boundaries consistent across different years.

Flood risk. As the disclosure policy differentially treats the properties in and out of the SFHA, I spatially merge individual property, community, and tract with the digitized flood map to determine the SFHA status. Specifically, I use the Q3 flood map, which reflects the flood risk of each community near the disclosure policy change timing.[22] The map covers about half of the entire FEMA communities

[20] I thank Eyal Frank for his generous help with data access.

[21] I thank Justin Gallagher for sharing the data. In Gallagher (2014), flood insurance participating communities with non-missing population data are used for the analysis, but I use the entire universe of communities as long as a community is part of the Q3 map.

[22] Q3 files are produced by FEMA as an earlier step of its 10-year automation program, by converting the existing hard-copy flood map to machine-readable formats. Approximately 1,300 counties (out of approximately 3,000 counties with the flood map) were chosen for the Q3 Flood Data production (FEMA 1996).

based on population density and the intensity of past flood incidents, and my main sample consists of these communities.

Flood history.[23] As flood size is a key determinant of flood damage, I need data that documents flood size at each community by year level. However, no flood data with an objective measure of size are readily available.[24] Thus, I construct community-level flood history data by conducting flood frequency analysis using records from over 3,000 USGS and NOAA stations located within the 27 disclosure requirement states (Milly et al. 2002, Mallakpour and Villarini 2015, Slater and Villarini 2016). Under this approach, flood size is measured by the recurrence interval (Task Committee on Hydrology Handbook of Management Group D of ASCE 1996): the expected number of years before a flood of the same magnitude comes back.[25]

Translating the daily discharge volume at each gauge into the maximum flood size at each community-year involves (1) constructing the gauge-specific frequency distribution by fitting Log-Pearson III distribution using the annual peak flow of each gauge, (2) converting the yearly maximum discharge volume to quantiles of the fitted distribution from step (1) and translating the quantiles into recurrence intervals,[26] and (3) matching each community to the three nearest gauges and calculating community-year-level flood size by taking the average of three gauges' recurrence interval using inverse distance as the weight. More details on flood data construction can be found in Appendix **??**.

Flood damage. I use the damage records from the National Flood Insurance

[23]I especially thank Jonathan J. Gourley at NOAA, Rod Lammers at the University of Georgia, and Tony Ladson at Moroka Pty Ltd for helpful comments and advice.

[24]An exception is the Unified Flash Flood Database (Gourley et al. 2013), but I choose to construct my own data for reasons described in the data appendix. For an overview of prior approaches, see appendix **??**.

[25]Flood size is increasing in the recurrence interval. For instance, a 10-year flood is a size of a flood that would happen on average once every 10 years, which would be less severe than a 100-year flood that is large enough to happen only once in 100 years on average.

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

Program adjuster's report, which I acquired through the Freedom of Information Act (FOIA) requests. Damage amount is defined as the actual cash value of flood damage, which is replacement value net of depreciation, to structures and contents (FEMA 2014). I observe an individual property level damage with loss date, community ID, building type, and flood event ID.[27] I restrict the sample to single-family houses and collapse it to the community by year by the largest flood event level to match it with the yearly maximum flood events data. I supplement the main damage data with the Individual Assistance (IA) program payout records. Because the adjuster's report covers properties subscribing to the flood insurance only, I combine the IA payout with the flood insurance claims to measure damage for both insured and uninsured households. I acquired county-level IA data from 1989 to 1998 through an FOIA request and split it up into the community-level data based on flood size following Deryugina (2017). I connect the 1989-1998 period to the post-1998 zip code level IA data available at FEMA's open library.

Other data sources. The primary data source to track the disclosure requirement legislative history is *Nexisuni* database. I cross-check it with prior works on the disclosure requirement (Washburn 1995, Pancak et al. 1996, Lefcoe 2004) and the National Realtor Association reports (National Association of Realtors 2019).

1.4.2 Summary Statistics

Table 1.1 presents summary statistics for key variables used in the analysis. Except for the number of 10-year flood events, which is plausibly exogenous to the disclosure policy, I present numbers from the pre-disclosure period observations. Also, housing prices and flood damage are inflation-adjusted using 2020 as the base year. For the 10-year flood events variable, I use 10 years of observations around the disclosure policy change.

A few points are worth noting. First, properties located within the SFHA are slightly more expensive than those outside of the SFHA. This price gap could

[27] About 25% of the claims are without flood ID. For these observations, I impute ID by grouping claims within the same state in the same week as the same flood.

Table 1.1: Summary Statistics for Key Variables

Variables	Mean	Std.Dev.	N
Housing Price	273,056	288,249	843,732
In SFHA	328,914	419,632	38,445
Flood Insurance Policy Counts	166	1,711	40,970
(%) SFHA > 90%	656	1,944	290
Population	3,559	1,622	43,550
(%) SFHA > 90%	3,418	1,837	540
Number 10-Year Floods (For 10 Years)	0.998	1.07	7,306
(%) SFHA > 90%	1.38	1.29	56
Annual Flood Damage	27,671	547,918	36,530
(%) SFHA > 90%	134,334	1,190,893	280

reflect better amenities but also a lack of flood risk awareness before the disclosure requirement For other variables, I present summary statistics for the entire pre-period sample and a subsample of high flood risk communities, which are defined as 90% or higher SFHA fraction. Not surprisingly, the average flood insurance policy counts, 10-year flood incidents, and flood damage are substantially higher for the high flood risk communities. Also, a community had on average 0.99 10-year floods for a 10-year period, which partly validates the flood history data I constructed.[28] In appendix Table **??**, I also present a balance table for properties located in and out of the SFHA. Results show the property characteristics are well balanced before the disclosure policy.

1.5 Empirical Strategy

In this section, I describe a strategy for the first set of empirical exercises that studies the disclosure requirement's impact on housing prices and household behaviors. I use the staggered adoption of the disclosure policy as a main strategy but also exploit the spatial discontinuity of the disclosure policy to provide addi-

[28] A 10-year flood is defined as a flood that is large enough to come back every 10 years on average. Thus, for a 10-year period, a community is expected to have one such event on average. For more details, see appendix **??**.

tional evidence.

1.5.1 Staggered Adoption of the Disclosure Requirement

A combination of the different policy implementation timing and the differential treatment of properties located in and out of the SFHA allow me to employ a triple difference design. Because the level of observation is different for the housing price (individual property level) and other outcome variables, I explain them separately. Equation (1.3) estimates the impact of the flood risk information on housing price.

$$log(Price_{ijmst}) = \beta T_{ijmst} + \theta_{mjhl} + \mu_{jt} + \lambda_{mt} + \epsilon_{ijmst} \tag{1.3}$$

$Price_{ijmst}$ is the log of housing price and T_{ijmst} is the treatment status dummy, which takes a value of 1 when a property i with SFHA status j in community m in state s at time t is required to disclose that the property is located in the SFHA. I also include a complete set of two-way fixed effects μ_{jt}: SFHA $\times$ Time, λ_{mt}: Community $\times$ Time, and θ_{mjhl}: Community $\times$ SFHA $\times$ Building Age $\times$ Number of Beds to estimate β. These fixed effects allow me to estimate the policy effect using the sales price variation before and after the disclosure policy, inside and outside of the SFHA while controlling for the community by SFHA specific property characteristics. For building age h, I grouped construction year into 10-years bins (e.g., 2000-2009, 1990-1999, etc.) and for the number of bedrooms l, I grouped it into 1-3, 4-6, 7-10, and 10-or-more bedrooms bins.

I also estimate the event study version of equation (1.3) to check the parallel trends assumption. k in equation (1.4) indicates event time, namely, the time relative to the disclosure policy change date. In constructing the event study sample, I impose the endpoint restrictions following Ito and Zhang (2020). Specifically, $\beta_k = \underline{\beta}$ for $k < -5$ and $\beta_k = \bar{\beta}$ for $k > 4$, where the unit of k is a half-year and all states in the analysis have a balanced panel in the event time within $k = [-5, 4]$. Importantly, creating a balanced panel for $k = [-5, 4]$ reduces the number of states to 13.[29] I use these 13 states for my main analysis.[30]

$$log(Price_{ijmst}) = \sum_{k=-5}^{4} \beta_k T^k_{ijmst} + \theta_{mjhl} + \mu_{jt} + \lambda_{mt} + \epsilon_{ijmst} \quad (1.4)$$

Outcome variables other than the housing price are observed at either the community or tract level. Thus, I estimate a continuous treatment version of equation (1.3), which is in equation (1.5) below. (%)SFHA$_m$ is the proportion of land in the SFHA for community m (or tract m), which essentially means that α_1 is the marginal effect of (%)SFHA$_m$ and α_3 estimates how the marginal effect changes after the implementation of the disclosure requirement

$$Y_{mst} = \alpha_1(\%)SFHA_m + \alpha_2 D_{mst} + \alpha_3[(\%)SFHA_m \times D_{mst}] + \omega_t + \psi_m + \epsilon_{mst} \quad (1.5)$$

In equation (1.5), Y_{mst} indicates various outcome variables, such as the number of flood insurance policies in community m at time t, and population, vacancy rate, median income, proportion of senior citizens, college graduates, and black population in tract m at time t. For population and demographic variables, I use the tract level 1990, 2000, and 2010 decennial census because the community is often too large, especially for the unincorporated county areas, so that it might mask demographic changes that happen within each community. D_{mst} is a dummy variable that takes a value of 1 if a community or tract m in state s has implemented the disclosure requirement at time t. I also include ω_t, the time fixed effect to account for year-specific common shocks and a community or tract fixed effect ψ_m, which captures an unobserved community or tract characteristics.

Importantly, I keep the tracts that contain the SFHA boundary within it so

[29] I lose DC, DE, IA, IN, KY, MD, MI, MS, NC, NE, NV, OH, SD, and TN out of the 27 ever-disclosed states in the contiguous US. These are states with a smaller population and/or relatively early policy change date.

[30] I apply the following additional sample restrictions. First, I drop observations without longitude and latitude information. Second, I keep only single-family houses in the sample, reflecting the fact that the disclosure requirement in many states is applied only to one to four dwelling units. Third, I restricted the transaction price to be between $10,000 and $100,000,000.

that I can compare the change in the marginal effect after the disclosure requirement for similar tracts. As appendix Table **??** shows, tracts with and without the SFHA border could be fundamentally different. This observation makes sense because having the border within a tract means it is near water, which could have a much different amenity level. Narrowing focus to the tracts containing the border greatly mitigates the differences in the potential determinants of population flow.

Throughout the analysis, standard errors are clustered at the state by SFHA (for housing price) and state level, which corresponds to the level of treatment.

1.5.2 Difference-in-Discontinuity Analysis at the SFHA Border

I exploit the spatial discontinuity created by the disclosure policy and provide additional evidence on the disclosure policy effect on housing prices and demographic variables. Importantly, to account for other confounding treatments including flood insurance requirements, I conduct a difference-in-discontinuity analysis. The design allows me to disentangle the information effect from the actual flood risk effect while controlling time-invariant confounding factors.

Following Grembi et al. (2016), I estimate equation (1.6) for housing prices in three steps. First, I restrict the sample to those near the cutoff. In practice, I remove properties beyond 400 meters (0.25 miles) from the border. Next, I estimate the optimal bandwidth and subsequently equation (1.6) using observations within the optimal bandwidth. δ_6 is the coefficient of the interest.[31] In equation (1.6), $Price_{imt}$ is the housing price of property i in community m in year t, X_{im} is the distance from a border in meters (negative if in non-SFHA area) for property i in community m, treatment group dummy $D_{im} = 1$ (i.e., in the SFHA) if $X_{im} > 0$, and post period dummy $T_t = 1$ if $t > T^*$, where T^* is the policy change date. Importantly, I include community fixed effect ψ_m in both bandwidth and diff-in-disc estimations to account for spatial dispersion of the SFHA boundaries. Standard

[31] I estimate the optimal bandwidth for the pre and post period separately and use the average of the two following Grembi et al. (2016).

Table 1.2: Effect of Discosure Requirement on Housing Price

	(1)	(2)	(3)	(4)
SFHA × Disclosure	−.061	−.033	−.103	−.131
	(.022)	(.013)	(.044)	(.028)
Disclosure				.217
				(.031)
Community × Time FE	X	X	X	
SFHA × Time FE	X	X	X	
Comm × SFHA × Age × N Bed FE	X	X	X	
Bandwidth				80.6
Sample	All	Pre-Flooded Comm	No Flood Comm	Louisiana
Num. obs.	2515874	1067375	713661	15013
N Clusters				4

Note: This table is produced from equation (1.3) and (1.6). The dependent variable is log(sales price). Columns (1) to (3), which show $\hat{\beta}$ from equation (1.3), include Community × SFHA × Building Age × Number of Beds fixed effect along with Community × Time and SFHA × Time fixed effects. In columns (2) and (3), I repeat (1) using the subsample of properties located in flooded and not flooded communities. In column (4), I estimate equation (1.6) and present $\hat{\delta_6}$. Standard errors are clustered at the state × SFHA (for columns (1) to (3)) and community (column (4)) level.

errors are clustered at the community level.

$$\begin{aligned} log(Price_{imt}) =& \delta_0 + \delta_1 X_{im} + \delta_2 D_{im} + \delta_3 X_{im} * D_{im} + \\ & T_t[\delta_4 + \delta_5 X_{im} + \delta_6 D_{im} + \delta_7 X_{im} * D_{im}] + \psi_m + \epsilon_{imt} \end{aligned} \tag{1.6}$$

For this exercise, I choose Louisiana for two reasons. First, it is one of the flattest states. A practical implication is that two different properties on opposite sides of an SFHA border are almost indistinguishable with the bare eyes as Figure **??** illustrates.[32] Second, flood maps for the state remained unchanged during the study period, which prevents picking up superiors relationship.

I also estimate a version of equation (1.6) with demographic variables as the dependent variable. I use the block group level decennial census data, which is the smallest geographical unit for which the bureau publishes sample data. The distance to the border is defined by the distance between a block group centroid and the SFHA border.

1.6 Household Response to the Disclosure Requirement

1.6.1 Housing Price

Housing price response to the disclosure policy is of interest in its own right, but it also is a first pass at the efficacy of the disclosure policy. Admittedly, it is ideal to directly measure how home buyers' flood risk awareness has changed from the disclosure policy. However, no such survey was conducted around the disclosure policy change timing, and thus I (indirectly) establish the disclosure policy's effect on awareness by presenting various housing price effects that cannot be explained by factors other than the policy.

Table 1.2 presents results from equation (1.3). Column (1) shows that the disclosure requirement reduces the price of the properties in the SFHA by 6.1% in comparison to those outside of the SFHA. To put these numbers in context, I multiply the estimate from column (1) to the average price of properties located in the SFHA in the pre-disclosure period from Table 1.1 ($38,445), and the reduction in the housing price amounts to $2,339.

In columns (2) and (3), I split the sample into houses located in communities that experienced a 10-year flood before the disclosure policy and in communities without flood experience over the sample period. The point estimates suggest the effect size is about three times larger for properties located in no-flood experience communities. The difference between the two estimates reassures that the effect in column (1) is indeed driven by the disclosure policy. Because what the disclosure requirement can affect is flood risk awareness, the policy will have little effect when the awareness level is high due to actual floods that happened before the disclosure policy implementation.

Column (4) provides additional evidence from a difference-in-discontinuity analysis in equation (1.6) with an optimal bandwidth estimated by the mean squared error optimal algorithm (Calonico et al. 2014, Cattaneo et al. 2019). The estimate

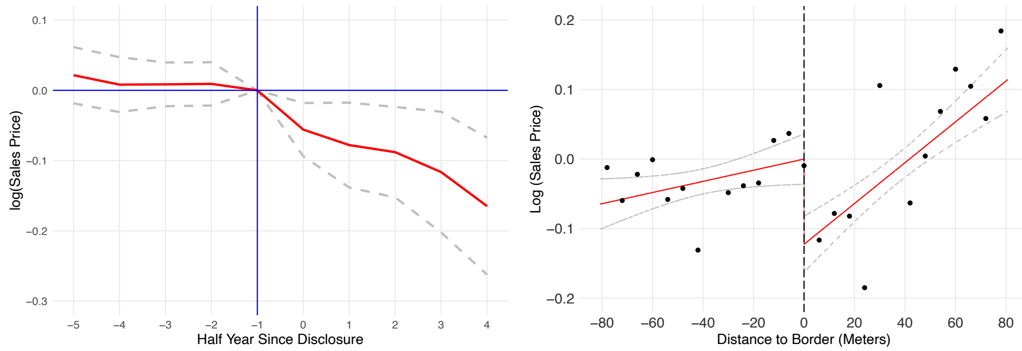
[32] The picture is from Pitt Street in New Orleans. The property on the very right is inside of the SFHA while the rest (on the left) are not, but it is almost indistinguishable from visual inspection.

shows the effect of flood risk awareness because the actual flood risk lever near the SFHA border is almost identical. The magnitude of the reduction is larger in column (4) at 13.1% in comparison to 6.1% from column (1), presumably because Louisiana has one of the most stringent and comprehensive disclosure requirements on flood risk.[33] Figure B.6 in appendix **??** shows the policy effect is stable across a wide range of bandwidth choices. Results from columns (2) through (4) show that the disclosure policy was effective in raising home buyers' flood risk awareness.

The staggered adoption estimation results are robust to a different set of specifications. In appendix Table **??**, I show that conditioning on a more standard two-way fixed effects does not change the result. Specifically, I include Community $\times$ SFHA fixed effect, which is less granular than Community $\times$ SFHA $\times$ Building Age $\times$ Number of Beds fixed effect from equation (1.3). The result is almost identical although the magnitude is slightly smaller at 5.1% reduction in the housing price. Further, I also show that the main results are robust to occasional flood map updates. In appendix Table **??**, I repeat Table 1.2 after removing 6% of communities that have experienced a flood map update over the sample period. The results are essentially the same as Table 1.2, suggesting that map updates are orthogonal to the disclosure policy implementation.

Figure 1.3 (a) presents an event study style graph from equation (1.4), measuring the policy effect over event time. The pre-disclosure period exhibits no pre-trend, and in the first six months of the policy change, the price of affected properties fell by 5%. In Figure 1.3 (a), I use half-year as a time unit, but as appendix Figure B.3 shows, the result is similar even if I use a year as an interval. The downside of using a year is that I lose about 8% of the sample. Figure 1.3 (b) portraits the difference-in-discontinuity term in column (4) of Table 1.2. The negative distance is outside of the SFHA properties and it shows a sharp drop in the treated housing price at the SFHA border. Note, the logged sales price is

[33] In addition to the SFHA classification, Louisiana's disclosure form asks about past flood history, if the property has ever received federal disaster relief, and if the property has flood insurance.



(a) Staggered Adoption (b) Difference-in-Discontinuity

Figure 1.3: The Effect of the Disclosure Requirement on Housing Price

Table 1.3: Effect of Discosure Requirement on Household Responses

	(1)	(2)	(3)	(4)	(5)
(%)SFHA × Disclosure	324.7	−321.3	2.1	−1.3	14937.3
	(107.2)	(164.5)	(1.2)	(.7)	(4777.4)
Disclosure	−41.4	53.6	−.2	.5	856.5
	(16.2)	(44.9)	(.5)	(.2)	(3560.4)
D.V	Num Policy	Population	(%) Vacant	(%) College	Median Income
Avg D.V. (90% > SFHA)	656	3,373	10.7	19.8	81,901
Year FE	X	X	X	X	X
Community FE	X				
Tract FE		X	X	X	X
Num. obs.	75230	76968	76968	76968	76968

Note: This table is produced from equation (1.5). Column (1) is estimated using community-level flood insurance policy counts. Columns (2) to (5) are estimated using the decennial census data in 1990, 2000, and 2010. Outcome variables and their pre-disclosure period average values can be found in the table text. All standard errors are clustered at the state level.

normalized so that $\Delta Y^- = 0$. Figure B.4 shows no evidence exists of bunching at the SFHA border.

1.6.2 Flood Insurance Policy Counts and Population Flow

In Table 1.3, I present estimated coefficients of equation (1.5) on various outcome variables that illustrate how households respond to the flood risk information. Importantly, because the level of observation is at community (tract), the coefficient of the "(%) SFHA" term captures the policy effect from the intensity of treatment, which is defined by the fraction of land in the SFHA for each community (tract). In column (1), I find the disclosure requirement leads to more flood insurance policies for the high risk communities relative to the low risk communities. In particular, the marginal effect of the proportion of the SFHA on policy count is 292 for a community with a 90% SFHA area. When compared to the pre-disclosure average policy counts for a community with higher than 90% of SFHA, it is over a 40% increase.

In column (2), I look at how households respond by location choices. The estimate shows the population of the high risk tract is decreasing after the disclosure policy relative to the low risk tract. For a tract with a 90% or higher SFHA area, the population is reduced by 289, or 9% when compared to the pre-disclosure average population. In addition, the estimate in column (3) indicates that the proportion of vacant properties goes up by 1.9%p or about 18% in comparison to the baseline vacancy rate. These findings suggest that the disclosure policy makes properties in high risk tracts less attractive and thus can reduce society's exposure to flood risk. Although statistically insignificant at the conventional level, in Table **??**, I also find that the number of housing units decrease in high risk tracts, consistent with findings in columns (2) and (3). The findings in columns (1) through (3) indicate that the housing price reduction from section 1.6.1 is driven by a combination of higher insurance take-up and population outflow from the high risk areas.

In columns (4) to (5), I explore changes in demographic characteristics. Notably, high risk tracts become less educated in comparison to the low risk tracts. The proportion of college graduates of tracts with a 90% SFHA decrease by 1.17%p or 6.6% in comparison to a 0% SFHA tract. One caveat is that the proportion of the college graduate is statistically significant at the 90% level, with a p-value of 0.07. Interestingly, I do not observe demographic change along the income or race margin: column (5) in Table 1.3 and column (1) in Table **??** show that the disclosure policy's impact on income and race are neither economically nor statistically significant. The findings from columns (5) and (6)–the disclosure policy has a larger effect for educated households but not so much for affluent households–suggests that cognitive rather than financial capability is a more crucial determinant of households' location choices after the disclosure requirement.

To credibly estimate the causal effect of the disclosure policy on demographic variables, I restrict the sample to the census tracts with a positive SFHA area only.[34] To provide additional credibility, I exploit the spatial discontinuity created by the disclosure policy as equation (1.6). For this exercise, I use the block group level decennial census data, which is the smallest geographic unit with income and education attainment information.[35] Block groups in my sample has an average population size of 1,170 and an average size of 0.25 square mile. I calculate the distance to the SFHA border from each block group using the distance between a block group centroid and the SFHA border. I remove block groups that have both SFHA and non-SFHA areas because distance is not defined for those block groups.[36] In appendix Table B.3, I present difference-in-discontinuity estimates for population size, (%) college graduates, and median income. Note that the policy effects closely matches estimates from Table 1.3, providing additional credibility to the results. In appendix Figure **??**, I estimate the coefficient from Table B.3 for a wide range of bandwidths, which present stable effect sizes.

1.7 The Effect of the Disclosure Requirement on Flood Damage

1.7.1 Damage Function Estimation

A prediction from the discussion in section 3.2 is that if more households choose to live in places with a lower level of flood risk, flood damage will decrease. Given the evidence from section 1.6.2 that the disclosure requirement leads to a smaller population in the high flood risk area, this section empirically investigates if and by how much the disclosure policy reduces flood damage conditional on flood size. Practically, I estimate the change in a damage function, which is a mapping between flood size and damage due to the disclosure policy using equation (1.7) (Auffhammer 2018).

[34] For more discussion, see section 1.5.1

[35] While census block is the smallest geographic unit, it does not have information on median income or education attainment, which are available only in the sample data (e.g., SF3 for the 1990 decennial census).

[36] In practice, I remove observations that have 5-95% of the SFHA area within a block group.

$$sinh^{-1}(dmg_{mfst}) = \gamma_1 F_{mt} + \gamma_2 D_{mst} + \gamma_3 [F_{mt} \times D_{mst}] + \boldsymbol{X_{mt}} + \omega_t + \psi_m + \xi_f + \epsilon_{mfst} \quad (1.7)$$

The dependent variable here is the inverse hyperbolic sine-transformationed damage for community m hit by flood f in state s at time t. I choose inverse hyperbolic sine transformation over log because damage is zero when a community is not hit by a flood. F_{mt} is an annual maximum flood size measured by the recurrence interval. For the main analysis, I focus on flood size between 0 and 50 because larger flood events invite larger measurement error.[37] D_{mst} is a dummy that takes a value of 1 if the disclosure requirement is turned on and $\boldsymbol{X_{mt}}$ are lagged annual maximum flood size for the past 5 years. I include three sets of fixed effects: ω_t, which controls for the overall time trend; ψ_m which accounts for community specific unobserved characteristics; and ξ_f, a flood fixed effect. ξ_f, in particular, controls two things: flood characteristics not captured by F_{mt}, such as the flood type (hurricane, severe storm, inland heavy rainfall, snow melt, etc.), and duration; and flood damage inspector idiosyncrasy between different flood events. These inspectors are short-term contract workers who are recruited by FEMA from the vicinity of the flooded area, and thus could be different across region and time. The identifying variation thus comes from a plausibly exogenous variation in flood size across different communities affected by the same flood event after accounting for overall trend and community specific characteristics.

I also allow a more flexible relationship between flood size and damage. In particular, I divide the flood size measure into 10 bins of equal size and estimate a semi-parametric version of equation (1.7). In other words, $k \in \{1, 2, \ldots, 10\}$ are flood size bins in equation (1.8) and $k = c$ when flood size $f \in [(c-1) \times 5, c \times 5]$.

[37] Larger floods are driven by multiple, interrelated perils, such as wind and mudslide (Kron et al. 2012). This fact creates a measurement error between flood size and damage.

Table 1.4: Effect of Disclosure Requirement on Flood Damage

	(1)	(2)	(3)	(4)	(5)
Flood Size	1.15	1.58	0.32	0.52	1.04
	(0.18)	(0.27)	(0.24)	(0.09)	(0.21)
Disclosure × Flood Size	−0.41	−0.81	0.28	−0.22	−0.59
	(0.21)	(0.30)	(0.30)	(0.11)	(0.24)
Year FE	X	X	X	X	X
Community FE	X	X	X	X	X
Flood ID FE	X	X	X	X	X
Flood Lags	X	X	X	X	X
Sample	All	High SFHA	Low SFHA	High SFHA	High SFHA
Num. obs.	80366	43010	37356	43010	43010

Note: The dependent variable in columns (1) to (3) is the inflation-adjusted (base = 2020) inverse hyperbolic sine-transformed total damage amount at community-year. Column (1) corresponds to equation (1.7). In columns (2) and (3), I repeat (1) using the subsample of communities with an above median SFHA ratio and below median SFHA ratio. Comumns (4) and (5) are estimated using communities with an above median SFHA ratio. The dependent variables in column (4) is the total number of claims and (5) is the inflation-adjusted (base = 2020) inverse hyperbolic sine-transformed average damage amount at the community-year level. All coefficients are multiplied by 100 for readability. All standard errors are clustered at the community level.

For instance, $k = 1$ for flood size between 0 (included) and 5 (excluded).

$$sinh^{-1}(dmg_{mfst}) = \sum_{k=1}^{10} \gamma_1^k F_{mt}^k + \gamma_2 D_{mst} + \sum_{k=1}^{10} \gamma_3^k [F_{mt}^k \times D_{mst}] + \boldsymbol{X_{mt}} + \omega_t + \psi_m + \xi_f + \epsilon_{mfst} \tag{1.8}$$

By comparing γ_1 and γ_1^k, and γ_3 and γ_3^k, I can test if the linear parameterization in equation (1.7) is a good approximation of a more flexible estimation model.

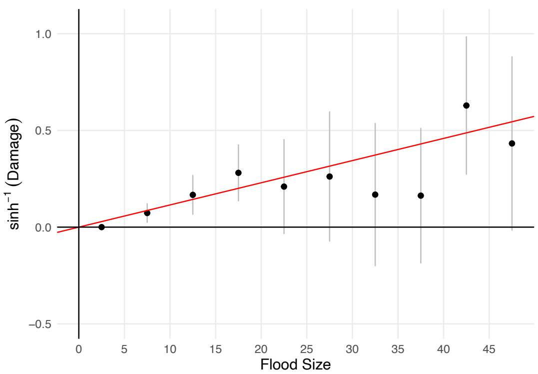
1.7.2 Change in Damage Function from the Disclosure Requirement

Table 1.4 presents the effect of the disclosure requirement on the damage function. As all the dependent variables are inverse hyperbolic sine transformed, the estimates can be interpreted in percentage. Flood damage, which is the dependent variable for columns (1) to (3), is measured by the damage incurred on the properties subscribing to flood insurance from the adjuster's report. More specifically, the damage amount reflects "actual cash value", which is replacement cost at the time of loss, less depreciation for depreciation. The age and condition of the item determine the value of physical depreciation (FEMA 2014). In column (1), I estimate equation (1.7). Flood Size and Disclosure × Flood Size terms correspond to $\hat{\gamma_1}$ and $\hat{\gamma_3}$, respectively. The results show the disclosure requirement flattens the damage function substantially: the initial relationship between flood size and damage–a 1.15% increase per unit increase in size–changes to 0.74%. Note the Disclosure × Flood Size term measures the disclosure policy effect for an *average* community in a treated state, without distinguishing the treatment intensity by the proportion of the SFHA within the community. In columns (2) and (3), I split the sample into communities above and below the median level of (%) SFHA to explore the heterogeneous effect. If the damage reduction is driven by the disclosure policy, we should see a larger effect from the higher SFHA communities, which are disproportionately affected by the policy. The coefficient of Disclosure × Flood in columns (2) and (3) show the damage reduction effect is entirely driven by the above median SFHA communities.

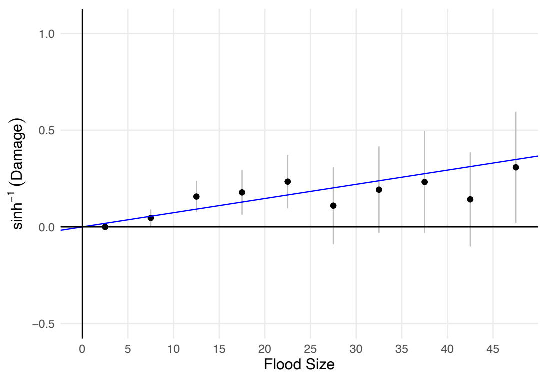
In columns (4) and (5) I explore potential explanations for the damage reduction. Given that the total damage is determined by the number of damaged properties and the average damage per property, I estimate how those two variables change as a result of the disclosure policy implementation for communities above the median SFHA ratio. Disclosure × Flood coefficient in column (4) suggests that the number of damaged properties conditional on flood size is reduced substantially as a result of the disclosure policy. This coincides with the reduction in population in the high flood risk area.[38] Column (5) shows that the disclosure policy reduces the average damage as well–a 1.04% increase in average damage per unit increase in flood size changes to 0.45%. This can be explained by the increased vacancy, which again reflects a smaller population, in the high flood risk area. Specifically, vacant properties lead to smaller average damage measured by actual cash value via two channels. First, vacant properties do not have personal belongings to be damaged. Second, the actual cash value of the vacant properties will be lower because hose houses are not as well-maintained.

The straight line in Figure 1.4 (a) plots the coefficient of Flood Size term from

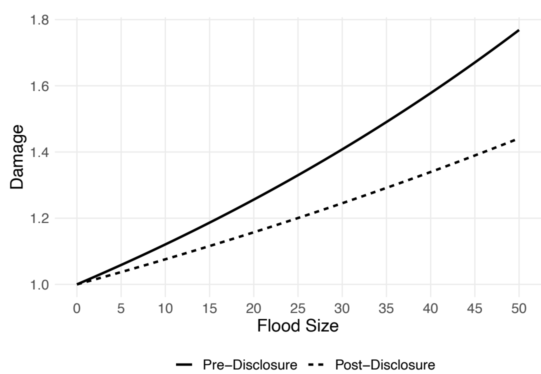
[38]The estimate in column (4) might look unexpected given that the number of flood insurance policies in the high flood risk area has increased as a result of the disclosure policy as in section 1.6.2. It is important to note, however, that the estimate in Table 1.3 reflects a *relative* increase in flood insurance policies. As Table B.4 in appendix **??** shows, the *absolute* number of insurance policies have decreased even in the above median SFHA ratio communities.



(a) Pre-disclosure (Dep.Var: arcsinh(Damage))



(b) Post-disclosure (Dep.Var: arcsinh(Damage))



(c) Damage Function (Dep.Var: Damage)

Figure 1.4: The Effect of Disclosure on the Damage Function.

column (1) of Table 1.4 on the normalized inverse hyperbolic sine transformed damage and flood size plane, which corresponds to the initial relationship between the two variables. Each dot in the same plot is $\hat{\gamma_1^k}$ from equation (1.8), where $k = 1$ is omitted. In panel (b), I plot the changed damage function after the disclosure requirement implementation. Namely, the straight line is $\hat{\gamma_1} + \hat{\gamma_3}$ from equation (1.7) and dots are $\hat{\gamma_1^k} + \hat{\gamma_3^k}$ from equation (1.8).[39] Importantly, the straight line in (b) is flatter, reflecting the negative sign of $\hat{\gamma_3}$. The figures also suggest the parameterization in equation (1.7) is a good approximation of a more flexible specification.

For interpretation, it is useful to calculate the percent change in flood damage for a given flood size x using equation (1.9). δ is damage for a 1-year flood in dollar terms, $\hat{\gamma_1}$ and $\hat{\gamma_3}$ are the estimated coefficients from equation (1.7), and x is flood size (e.g., $x = 10$ for a 10-year flood).[40]

$$\%\Delta Damage_x = \frac{\delta(1+\hat{\gamma_1}+\hat{\gamma_3})^x - \delta(1+\hat{\gamma_1})^x}{\delta(1+\hat{\gamma_1})^x} \times 100 \tag{1.9}$$

Panel (c) of Figure 1.4 portraits the damage function before (solid line) and after (dotted line) the disclosure requirement in the damage-flood size plane. Note, the y-axis informs flood damage relative to Damage_1. For instance, before the disclosure policy, a 50-year flood was about 80% more costly than a 1-year flood. The gap between the two lines corresponds to the numerator from equation (1.9). At 50 years, where the difference is the largest, $\%\Delta Damage_{50}$ = -19. Average damage reduction for $x \in \{1, 2, \ldots, 50\}$ is -9.8%. Alternatively, I also conduct a direct back-transforming of the dependent variable following MacKinnon and Magee (1990); Wooldridge (2006) and the result is very similar at a 10.6% reduction.[41]

[39]The standard error of the dots in panel (b) is calculated analytically using the clustered variance-covariance matrix.

[40]A simplification here is that δ, which is the damage amount for a 1-year flood is the same before and after the disclosure policy change. This is verifiable in data: the median flood damage size (out of the non-zero damage observations) for an above-median SFHA community hit by a flood size between 1 and 2 is $22,995 and $20,962, respectively.

[41]To back-transform, I first calculate inverse hyperbolic transformed predicted damage using the estimated explanatory variables. Then, I apply the sine function on both sides to transform

Table 1.5: Effect of Disclosure Requirement on Flood Damage (Placebo States)

	(1)	(2)	(3)	(4)	(5)
Flood Size	0.84	1.10	0.46	0.18	0.89
	(0.31)	(0.39)	(0.47)	(0.11)	(0.35)
Disclosure × Flood Size	0.09	−0.19	0.13	0.12	−0.32
	(0.40)	(0.54)	(0.57)	(0.17)	(0.46)
Year FE	X	X	X	X	X
Community FE	X	X	X	X	X
Flood ID FE	X	X	X	X	X
Flood Lags	X	X	X	X	X
Sample	All	High SFHA	Low SFHA	High SFHA	High SFHA
Num. obs.	18095	10065	8030	10065	10065

Note: This table repeats Table 1.4 using placebo states. The dependent variable in columns (1) to (3) is the inflation-adjusted (base = 2020) inverse hyperbolic sine-transformed total damage amount at community-year. Column (1) corresponds to equation (1.7). In columns (2) and (3), I repeat (1) using the subsample of communities with an above median SFHA ratio and below median SFHA ratio. Comumns (4) and (5) are estimated using communities with an above median SFHA ratio. The dependent variables in column (4) is the total number of claims and (5) is the inflation-adjusted (base = 2020) inverse hyperbolic sine-transformed average damage amount at the community-year level. All coefficients are multiplied by 100 for readability. All standard errors are clustered at the community level.

The result in Table 1.4 is robust to three different robustness checks. First, I conduct a placebo test by exploiting seven states (CA, IN, ME, MN, NH, and VA) that had implemented the disclosure policy but without a question on the flood risk.[42] The idea is that if flood risk information delivered by the disclosure requirement had reduced flood damage, the disclosure policy implemented in these placebo states–which do not cover flood–should not have an effect. Table 1.5 shows the estimation results from the placebo states. Note that Disclosure × Flood coefficient in column (1) is 0.09, which suggests that the estimated change in flood damage is near zero after the disclosure policy. This is a start contrast from Table 1.4, which reduced the rate of increase by about 40%. The null effect in Table 1.5 makes sense given that the disclosure policies in these states are unrelated to flood.

Second, I use a different dependent variable to repeat the analysis in Table 1.4, which reflects damage incurred on properties subscribing to the flood insurance. One might worry that the overall flood damage might not have decreased (or even increased) while the damage for insured properties had reduced. I use the sum of the flood insurance claims and FEMA Individual Assistance (IA) as an outcome

the dependent variable in dollar terms. Finally, I multiply an "adjustment" factor ($e^{\sigma^2/2}$) to correct for attenuation due to Jensen's inequality.

[42]Among these states, CA and IN later changed the law to include flood risk.

variable. IA is targeted for under or uninsured households with serious needs, and thus by combining IA payment and flood insurance claims amount, I can measure damage for both insured and uninsured households.[43] Appendix Table **??** shows the estimation results, and using coefficients from column (1), the estimated damage reduction is -10.6%, which is very similar to the -9.8% from Table 1.4.

Third, I produce an event study style graph in appendix Figure B.7. Here, $\hat{\gamma}_3$ for each event time is plotted, which is the marginal effect of flood size on arcsinh(damage) for five years before and after the policy change. It shows no pretrend, and more importantly, a clear reduction in the marginal effect, after the policy change. This effect corresponds to a flatter damage function after the disclosure policy.

1.8 Discussion

1.8.1 Interpreting the Damage Reduction Effect

Section 1.7 shows that providing more information on flood risk changes the initial mapping between flood size and its damage, in a way that reduces flood damage. This finding is important because a reduction in flood damage is a primary source of welfare gain from the disclosure policy. In this section, I conduct a back-of-the-envelop calculation to quantify the policy effect in dollars.

Before the disclosure policy, average damage from a flood between size 1 and 50 is \$27,671 for an average community. Using the fact that the disclosure policy reduces flood damage by -9.8% for an average flood size, an average reduction in damage for a community amounts to \$2,699. Multiplying this with the total number of FEMA communities (as of Jul 2019) 24,474, the total reduction in damage is \$66 million dollars annually. This number is likely to underestimate the true effect because the analysis excludes floods larger than the 50-years recurrence interval, which incur disproportionately large damage. Besides, I also abstracted

[43] IA is authorized for a fraction of Presidential Disaster Declaration (PDD) floods and also covers only basic needs; therefore it still underestimates flood damage for uninsured.

away from a potential gain due to a better matching (in terms of flood risk preferences) between properties and home buyers (Bakkensen and Ma 2019).

For a complete welfare analysis, we need an estimate for the social cost of the disclosure policy as well. However, to the best of my knowledge, there is no such estimate. Nonetheless, given the nature of the policy, the cost is likely to be fairly low. For instance, in terms of the administrative cost, creating the form incurs a small one-time cost. The compliance cost imposed on home sellers–the time and effort required to furnish the form–is likely to be small as well. One survey result shows that home sellers on average spend less than 40 minutes to fill out the form (Moore and Smolen 2000). Combining this with the reduced flood damage, the policy produces a substantial welfare gain.

The findings are driven by the population reduction in high-risk areas. But why do home buyers engage in self-protection, namely choosing a safer place to live, although they have an option to buy flood insurance? One of the predictions of section 3.2 is that when the non-insurable cost is large, households will change the location. The flood insurance covers up to $250,000 for a residential property, and the premium on average is lower than actuarially fair premium, but it is still incomplete insurance. A flood could negatively affect an individual's health (Kahn 2005, Bloom et al. 2009), employment status (Deryugina 2017), or income, which is not covered by the flood insurance.[44] Also, cleaning up and finding a new living arrangement incurs huge time and cognitive costs. Natural disasters even reduce subjective well-being (Rehdanz et al. 2015, Berlemann 2016). Given these non-trivial uninsurable costs, people might choose to migrate instead of purchasing insurance and living in risky places.

[44] Anecdotal evidence shows floods could impose serious health threats. For instance, Hurricane Harvey disrupted operations at 40 wastewater treatment plants, which caused at least 25,000 gallons of sewage-tainted water to flood streets and waterways (Constible 2018).

1.8.2 Why Do Home Sellers Not Disclose Voluntarily?

Given the disclosure requirement's significant impact on housing prices, home buyers clearly care about flood risk. Earlier works on "unraveling" have pointed out that when a seller has better information about the product quality than consumers, and the cost of verifiable disclosure is zero, voluntary disclosure is going to happen (Milgrom 1981, Grossman 1981). Under this circumstance, a mandatory disclosure policy would have no or small effect because the information is already provided to home buyers. Why it was not the case for flood risk?

There are a couple of potential explanations. First, making a credible disclosure on flood risk could be costly for home sellers. What the disclosure requirement effectively does is similar to a product guarantee. It delivers the best available and truthful information a home seller has to a home buyer, and if the information is significantly misleading or false, home sellers can be held responsible later (Lefcoe 2004). Without an institution like the disclosure requirement, delivering credible information could induce a non-trivial cost (e.g., third-party certification). Conversely, self-generated information from a home seller might have little effect on home buyers if the information is not deemed credible or easily verifiable (Stern 2005).

Second, one of the key assumptions for unraveling is that a product is vertically differentiated along a single, well-defined dimension of quality because it allows consumers to interpret the lack of disclosure as inferior quality, which in turn induces voluntary disclosure from the producers (Dranove and Jin 2010). However, a house is a bundle of attributes with physical characteristics (e.g., number of bedrooms) and amenities such as crime rate, school quality, and pollution. Thus, vertically differentiating a house along a single dimension is not easy.

Third, voluntary disclosure might not happen when the standard is unclear (Harbaugh et al. 2011), which can be true with flood risk. In what language should home sellers and buyers communicate concerning flood risk? Using past flood experience? If so, for how many past years? Or should they use the flood insurance subscription status or premium? Or the SFHA status? The disclosure policy standardizes risk communication, thus facilitating information flow.

1.9 Conclusion

Floods are the costliest natural disaster in the US and are expected to become more frequent and severe in the future. Thus, curbing economic loss from these events is of first-order importance. A prevalent policy prescription in the US has been structural flood water control, namely, adding more physical structures. However, this approach is criticized for disincentivizing people to adapt–it rather attracts more people to areas with flood risk, by distorting the location choice.

In this paper, I study whether alleviating information friction regarding flood risk in the housing market can be a more effective way to foster adaptation by exploiting the staggered adoption of the disclosure requirement across different states and spatial discontinuity in flood information. The policy mandates that sellers disclose property defects, and 27 states in the contiguous US have an explicit question on flood risk. I explore if and how households respond to the disclosure policy and investigate its implications for flood damage. The results show that when property-specific flood risk information is provided, housing prices of the affected properties drop by 6%, indicating the policy is binding and effective in delivering flood risk information. The price adjustment is driven by a combination of additional flood insurance purchase and population reduction in a flood risky area. Fewer people in flood risky areas reduce overall exposure to flood risk, which in turn reduces damage from a small to moderate flood by 9.8%.

The findings of this paper suggest the disclosure policy is an effective flood risk management tool. By removing market frictions, it makes home buyers bear flood costs, which facilitates voluntary adaptation, namely living in an area with lower flood risk. This is a double dividend for a government because it can not only save money on flood management infrastructure but also reduce post-disaster recovery spending. Also, it is fair from the taxpayer's perspective given that a disproportionately large amount of resources are devoted to protect and relieve people living near water–who are also more affluent–under the current system (GAO 2013). Further, the disclosure policy could contribute to the stability of the housing market and the financial system by preventing home buyers to be overly optimistic about the future housing market and climate exposure (Bakkensen and Barrage 2017). Finally, it is worth pointing out that the disclosure policy effect could be larger when the flood insurance premium subsidy is removed and the premium reflects the true flood risk level.

Although I exploit one of the best available data sets to direct damage on properties, the measurement of flood damage is still incomplete. A more comprehensive measure should also include indirect losses from floods, such as loss of income, business disruption, and use time loss (Gall et al. 2011). Developing a measure for these costs and examining whether a disclosure policy could reduce the indirect cost of a flood would be an important future research topic.

Chapter 2

Household Responses to Pigouvian Tax and Climate Change Mitigation: Evidence from Food Waste Tax

2.1 Introduction

Annual waste production is over 2 billion tons worldwide, creating substantial environmental externality. For instance, 5% of the global greenhouse gases (GHGs) emissions are from waste (Kaza et al. 2018). In addition to the environmental burden, waste treatment is the largest budget item for many municipalities, preventing public investments in more productive areas (OECD 2000, Kaza et al. 2018). As such, since the 1990s, a unit-based waste pricing scheme (UPS), which is designed to make households internalize the social cost of waste and thus reduce the waste quantity, has been implemented in many countries.[1] However, we have a limited understanding of the welfare implication of the policy partly because little is known about the behavioral responses to such taxes. For instance, to avoid the waste fee, some households might adjust their grocery shopping patterns to produce less waste in the first place while others might engage in illegal dumping. If the latter effect dominates, the policy could be hardly welfare-improving.

In this paper, I provide the first estimates on households' behavioral responses to the UPS while explicitly accounting for "upstream" consumption changes. Specifically, I study the effect of the unit based food waste tax in South Korea on food waste quantity, illegal dumping quantity, and grocery purchase and conduct a welfare analysis of the tax. Focusing on food waste has at least two benefits. First, in contrast to landfill waste that invites waste from a myriad of household activities, food waste comes from a single source: uneaten food. This allows me to

[1] See Kinnaman (2006) and OECD (2007) for the UPS policies in the US and the OECD countries.

observe households' adjustment from the upstream using grocery purchase data. Second, I can measure illegal dumping behavior, using the quantity of the landfill waste, which is charged at a lower per unit fee in South Korea, as a proxy. Using the estimates on the change of illegal dumping and grocery purchases, I can show where the observed reduction in food waste quantity comes from, which in turn allows me to determine if the policy is worth implementing despite the "leakage".

The empirical application is based on a quasi-experiment that exploits the food waste tax expansion in South Korea during 2013-15. Since 2005, households have been required to segregate food and landfill waste. A small fee for food waste collection has been imposed since then, but the majority of the households were charged a flat monthly fee.[2] The initiative, which was pushed by a presidential committee in an effort to reduce food waste, required all urban municipalities to implement the UPS on food waste for households and small restaurants and to increase the per unit fee. As a result, 77% of households faced a positive marginal waste fee by 2015.[3] Even after the fee increase, per unit fee was small: an average municipality charged less than \$0.1 for KG of waste, or an average household paid less than \$1 each month. Importantly, as the policy was driven by the central government, these variations are plausibly exogenous to an unobserved municipality or household characteristics that could potentially be correlated with waste generation.

For the empirical analysis, I compile multiple data sets. Annual food and landfill waste quantity and unit fee information for each municipality comes from the UPS Yearbook from the Ministry of Environment. To estimate the effect of the UPS on grocery purchases, I use Consumer Grocery Panel Data from the Rural Development Administration. This data, which is comparable to the Nielsen

[2] In contrast, a per unit fee has been imposed on landfill waste since 1995.

[3] In this paper, I define UPS as a waste fee with a positive marginal price. This rules out a group-based UPS, where each household in an apartment complex pays 1/N of the apartment-wise waste fee. Given a large size of N (in general over 100) and small waste fee, a group based UPS effectively has zero marginal price. Note, the government considered the group-based UPS a valid, though less desirable, UPS so under that definition, nearly 100% of households were under UPS by 2015.

Homescan data, closely documents approximately 700 households' grocery shopping history with detailed information on a shopping date, product, expenditure, and unit price along with rich demographic information. I link these datasets to the UPS status and fee information of each municipality, which I construct based on municipality ordinances, newspaper articles, and Official Information Disclosure Act requests.

The empirical analysis produces three key results. First, I find that the policy reduces food waste by 27% (2KG per month per capita) while illegal dumping, which is measured by the change in the landfill waste quantity, rises by 9% (0.7KG per month per capita). I also show that the effect sizes are larger for a municipality with a higher % change in the proportion of the households subject to the UPS. This confirms that the effect is driven by the UPS policy. A comparison of the change in food and landfill waste quantity indicates that about 1/3 of the reduction in the measured food waste quantity is attributable to illegal dumping.

Second, the UPS policy reduces grocery purchases per capita by 3.5% or 0.7KG per month. Assuming that people do not change the amount of food they actually consume due to the policy, the finding indicates that the policy encourages people to reduce uneaten food purchases. I also estimate the policy effect separately for perishable and storable items.[4] The idea is that if the observed effect reflects an effort to save the fee, we should observe a stronger effect from perishable items which becomes food waste in a relatively short period of time. The results show that the policy effect is twice as large for perishable items in comparison to storable items. Compared against the reduction in food waste amount, another 1/3 of the observed reduction in food waste is attributable to the grocery purchase adjustment.

Third, the lower bound of net policy benefit is substantial. As I can explain only 2/3 of the observed reduction in food waste from the proxy of illegal dumping and reduction in grocery purchases, I assume that the unexplained 1/3 is

[4]Perishable items are vegetable and fruits whereas storable items are protein, grains, and processed foods. More details can be found in the data appendix.

also illegally dumped and calculate the lower bound of the policy benefit. I calculate reduced GHGs emissions from reduced food waste and reduced uneaten food production and multiply it by the social cost of carbon. I also take into account savings on government spending from smaller waste quantities. I find that the social marginal cost of producing uneaten food is more than 10 times higher than the extra social cost of illegal dumping, which indicates that the policy creates a substantial welfare gain.

This paper contributes to four different bodies of literature. First, this is the first paper to empirically show that a Pigouvian tax on waste can reduce negative environmental externality throughout the product life cycle by encouraging households to change their consumption pattern in the upstream (OECD 2000). While a few papers have shown that disposable fees, for instance, can send signals upstream to *producers* (Fullerton and Kinnaman 1995, Calcott and Walls 2000, Walls and Palmer 2001, Acuff and Kaffine 2013), downstream instruments' effect on *consumer*'s upstream behavior have rarely been studied. One important exception is Katare et al. (2017), who derived a theoretical model for socially optimal food waste tax while explicitly factoring in consumer's food choices. The key parameter in their model for the socially optimal food waste tax is the elasticity of food waste quantity to the tax level and my paper presents the first empirical estimate on it.

Second, I show that a Pigouvian tax on waste can generate substantial welfare gain. Earlier studies find the UPS on municipal solid waste fail to pass the cost-benefit analysis because the saved social cost cannot be justified given the risk of illegal dumping and administrative costs (Fullerton and Kinnaman 1996, Kinnaman 2006, Allers and Hoeben 2010).[5] However, these papers abstracted away from two important policy benefits. First, they do not take GHGs emissions into account when calculating the social cost of waste. For instance, a widely cited

[5]These papers study the UPS on municipal solid waste which *includes* food waste. Food is the largest (44%) waste category globally and is responsible for the majority of the GHGs emissions from the overall waste (Kaza et al. 2018).

Repetto et al. (1992) estimate that the social cost of waste is \$5/ton.[6] When GHGs emissions from the landfill waste are considered, the social cost is at least over \$40/ton.[7] Second, earlier works also ignore environmentally advantageous changes in consumption patterns, implicitly assuming that the total waste quantity remains constant even after the UPS.[8] By explicitly taking these two factors into account, my paper presents a more comprehensive welfare estimate.

Third, I contribute to the literature on climate change mitigation. While earlier studies have found the effect of a carbon tax on the transportation sector or overall economy, to the best of my knowledge, no prior work has studied the effect of tax on GHGs emissions from waste (Davis and Kilian 2011, Andersson 2019, Pretis 2019). Importantly, tax on food waste produces a "double dividend" with a potential to reduce GHGs from food production as well. It is of practical importance given that food production accounts for a quarter of anthropogenic GHGs emissions (IPCC 2014). Findings of the paper also complement scientific literature that emphasizes the importance of demand side measures to curb food-driven GHGs by evaluating a policy in action and quantifying the benefit (Foley et al. 2011, Bajželj et al. 2014, Hiç et al. 2016, IPCC 2018).

Finally, my paper presents a lower bound of policy benefit which could help evaluate a policy in the presence of "leakage". Earlier studies have pointed out that environmental regulations oftentimes are incomplete and there is leakage (Davis 2008, Fowlie 2009, Taylor 2019). My paper not only provides a way to measure such leakage but also shows how we can measure the actual reduction in externality generating behaviors, which forms the basis of the lower bound of the

[6] They consider "air and water pollution, noise, and other disamenities" as potential non-market cost.

[7] According to EPA (2016), GHGs emissions from landfill sites in 2014 were 148 million metric tons (MMT). By dividing this by the total amount of landfill waste in 2014 (136 MMT), and multiplying it with the social cost of carbon (\$36), the social cost of waste due to the GHGs emissions is \$39/ton. Importantly, \$39/ton is after taking into account the fact that about 50% of the total waste generated in the US is recycled, composted, or used for energy production.

[8] In addition, many earlier works exploit cross-sectional variation or a panel of small geographical areas to identify the elasticity. This empirical approach further limits the credibility of the conclusions.

policy benefit.

2.2 A Conceptual Framework for Welfare Analysis

As I show later, since levying Pigouvian tax on food waste induces multiple types of household responses, the overall welfare effect of it depends on the welfare consequence of each element. To pin down the idea, consider a simple equation for static food waste generation in households as equation (2.1).

$$W \equiv F - C^* - I - X^* \tag{2.1}$$

Households purchase food of quantity F and consume C^*. For the majority of the households, $F - C^* > 0$, namely, there exists "excessive food" purchase because a careful meal planning could incur non-trivial cognitive cost or there might be a penchant for precautionary stockpiling. Let $Q^* = F - C^*$ be true food waste quantity, namely, an excess food purchase.

Now, assuming that they do not grow their own food, measured food waste quantity W is identical to the true food waste quantity Q^* in the absence of outside options. If outside options $I + X^*$ are exercised, where I is measured illegal dumping X^* is the ubonserved (to analyst) outside options such as home composting or dehydration then there is a gap between true and measured food waste quantity by $I + X^*$. The asterisk indicate what I cannot observe.

Now suppose, a Pigouvian tax of T is imposed on legally disposed food waste. Unless $Q^* = 0$, the tax makes food more expensive in effect and thus purchased food quantity F would decrease ($\frac{\partial F}{\partial T} < 0$). Also, a higher cost of legal food waste disposal would lead to higher utilization of the substitutes to legal disposal ($\frac{\partial C^*}{\partial T} > 0$, $\frac{\partial I}{\partial T} > 0$, $\frac{\partial X^*}{\partial T} > 0$). Note that while $\Delta F < 0$ or $\Delta C^* > 0$ amount to an actual reduction in food waste quantity, $\Delta I > 0$ and $\Delta X^* > 0$ mean people dispose food waste in different ways without changing the quantity generated. The distinction is important because a policy benefit, if any, will come from the reduction, not from the displacement.[9]

$$\Delta W = \underbrace{\Delta F - \Delta C^*}_{\text{reduction}} - \underbrace{(\Delta I + \Delta X^*)}_{\text{displacement}} \tag{2.2}$$

Since I do not observe ΔC^* and ΔX^*, I estimate the lower bound of the food waste reduction quantity from the tax, which becomes the basis for the lower bound of the welfare effect. Under a plausible assumption that people do not reduce the consumption of food in a response to the tiny tax on food waste (namely, $\Delta C^* \geq 0$), the lower bound of waste reduction quantity is ΔF, which is attained when $\Delta C^* = 0$.[10] Importantly, $\Delta C^* = 0$ also allows me to observe ΔQ^* using ΔF. Also, I treat ΔX^*, which is unobserved food waste reduction options, as additional illegal dumping. Practically, these two assumptions, namely $\Delta C^* = 0$ and treating ΔX^* as ΔI, mean that I consider any reduction in the observed food waste that is beyond the reduction in food purchase as illegal dumping. Thus the welfare effect of the policy reduces to understanding the welfare effect of ΔQ^* (equivalently ΔF) and $\Delta W - \Delta F$.

To translate the lower bound of the reduction into the lower bound of the welfare effect, I estimate the change in the dead weight loss (DWL). Figure 2.1 illustrates the welfare effect. The left panel shows the demand for food waste generation Q. Note, before the tax, food waste quantity was at Q_0, which is much larger than the socially optimum level of Q_{opt}, assuming that Q is disposed legally. The DWL is the difference between two triangles ABC and CDE Now, suppose that a Pigovian tax on food waste imposed at the level $T < SMC_L$. This reduces food waste generation to Q_T and DWL reduces by a trapezoid DEFG.

[9]The marginal displacement of food waste is welfare-harming when $SMC_L < SMC_I$, where SMC_L is social marginal cost of legally disposed food waste. In a policy settings where illegal dumping ends up in landfill sites while legally collected food waste is carefully processed, this condition is easily satisfied.

[10]I present suggestive evidence supporting this assumption. Appendix table **??** show that the effect of the UPS on the each municipality's percentage of obese population. Although statistically insignificant, the 95% confidence interval still suggest that the effect is quite limited, if any.

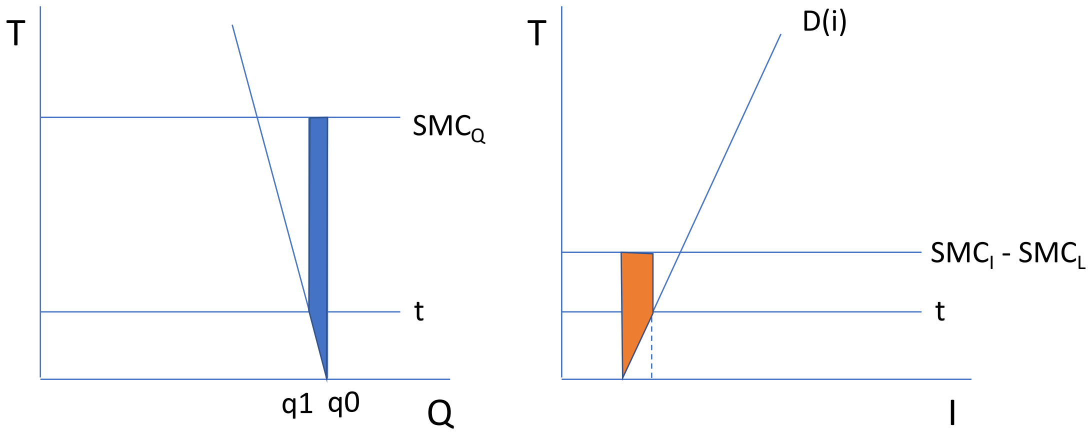


Figure 2.1: Welfare Effect of the UPS Policy

When the reduction of the true food waste is equal to the reduction of the measured food waste quantity, the left pane completely illustrates the welfare effect. However, when households exercise illegal dumping, and thus when the measured food waste quantity reduces more than the reduction of the true food waste quantity, we should consider welfare loss due to the "leakage". For that, see the right pane in Figure 2.1. As mentioned earlier, I consider any reduction in the measured food waste beyond the reduction in food purchase as illegal dumping. Thus, the demand for illegal dumping $D(\bar{I}) = D(W) - D(Q)$. Without the tax, illegal dumping quantity was $\bar{I}_0$, creating welfare loss of a rectangle ABCD. Illegal dumping is likely to be non-zero even without any unit fee on food waste because waste segregation requires households' effort. Note, I use $SMC_I - SMC_L$ as a relevant social marginal cost because the welfare loss from illegal dumping is the additional social marginal cost that is beyond the social marginal cost of the legally disposed food waste. Now, suppose that a Pigouvian tax T is introduced. The level of illegal dumping increases to $\bar{I}_T$, which incurs additional DWL of BCEF. The overall welfare effect of the tax will be the sum of the two.

2.3 Background

2.3.1 Food Waste Generation in South Korea

Food waste in South Korea has increased rapidly as the nation became richer. In 2000, the nation produced 11.4 thousand ton/day of food waste, which became 14.5 thousand ton/day in 2007. The majority of the food waste (70%) is from households or small restaurants (smaller than 200 m^2), whereas the rest is from bulk generators such as hotels, large restaurants, and office/school cafeterias. Since the Waste Control Act imposes responsibility on bulk generators for managing their own waste, municipalities provide waste pickup services to households and small restaurants only. Earlier studies suggest that a small restaurant on average produce as much food waste as 10 households (Kim et al. 2010).[11]

Until 2004, any non-recyclable item including food waste from households and small restaurants was sent to landfill sites. However, due to the mounting environmental concerns and escalating complaints from the nearby residents, the Ministry of Environment banned landfilling of food waste from 2005.[12] Instead, the central government urged municipalities, which have been responsible for and have had jurisdiction over waste management, to recycle the food waste.[13] In response, municipalities started collecting food waste separately so that they can send it to composting or animal feed producing sites.

Food waste policy in this period focused on "recycling" rather than "reduction". A practical implication is that the waste collection was designed to attain the maximum efficiency. For a typical municipality, therefore, apartment residents were asked to dispose their food waste using a standardized 120 liter (32 gallon)

[11] I cross check this using the waste bag sales data from the UPS yearbook.

[12] Food waste creates a massive amount of leachate and bad odor, which is not only environmental disamenity but a public health threat. Also, as the income level goes up, it becomes exponentially harder to find landfill sites. For instance, greater Seoul metropolitan area, which is a home to 25 million people has been using a single landfill site since 1992. The site is constructed on a reclaimed land because creating a landfill on an existing land is extremely contentious.

[13] Korea has 226 municipalities with average population of 229,108 and area of 444km^2 (as of Dec 2017). It is comparable to counties in the US. For instance, Illinois (1.5 times larger than South Korea) has 102 counties with an average area of 1470 km^2.

(a) Landed house

(b) LH - Designated bags

(c) Apartment

(d) APT - RFID

Figure 2.2: Two different types of residential area and food waste collection

communal dumpsters. Municipalities levied a flat monthly fee ($1-2 month per household) directly on management offices. Subsequently, management offices collected the fee from each unit through management fee. Many landed houses and small restaurants, in contrast, were asked to use a designated plastic food waste bag that had to be purchased in advance because finding a space for common dumpster was challenging and fee collection much easier this way given that landed houses do not have a management office. This created a substantial variation across municipalities in the level of UPS implementation even before the expansion.

2.3.2 The national Expansion of the UPS Policy

While converting food waste into fertilizer or animal feed is clearly better than sending everything to landfill, there has been a continuing criticism about the approach from both environmental and government finance perspectives. For instance, food waste reduction through abstaining from excessive food purchase is more environmental-friendly than recycling already generated food waste because greenhouse gas emission from the production stage can be saved. Also, the government spends more than $6 billion annually for treating food waste (GGC 2010). To fix these problems, the Green Growth Committee, a presidential committee established following the 2008 presidential election, launched an initiative in 2010 to expand the UPS policy nation wide. Subsequently, the ministry issued a series of executive orders from 2010 to 2012 urging municipalities to implement the UPS by 2013.

Figure 2.3 show the overall trend in UPS and per unit fee over time. In 2010, for instance, the average fraction of household equivalents subject to UPS is about 40 percent, but it goes up to near 80 percent in 2015, where the most dramatic change happens in 2013 and 2014.[14] In terms of unit fee level, we can observe a similar pattern: the average per unit price in 2010 is about 0.05 dollar per gallon[15], which change to 0.18 dollar in 2015.[16] This suggests that the initiative substantially increased the number of households subject to the UPS as well as unit fee level.

First, for landed house and small restaurants, the mode of waste pickup was the same as before for most cases simply because many of them were already under UPS (Figure 3 top panel). The initiative for this group effectively meant per unit fee increases (Figure 3 bottom panel). This is a contrast to apartments, which switched from either flat fee or group pricing[17] to the UPS mostly through the radio frequency identification (RFID) system.[18] The equipment is a simple electric weighing machine combined with an RFID sensor that incubates a communal con-

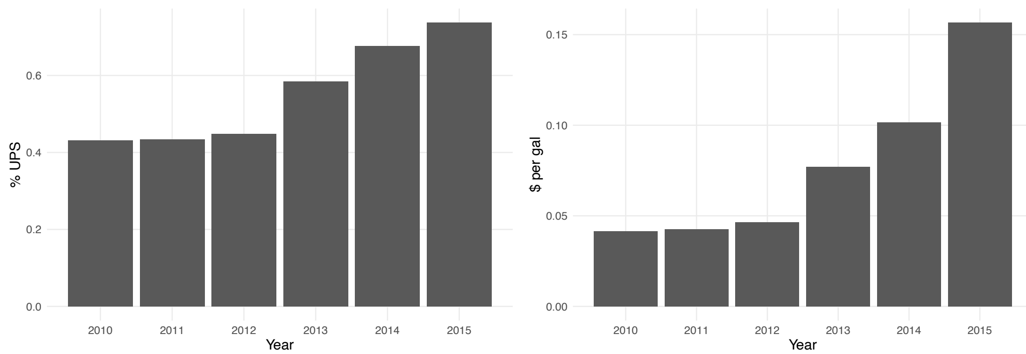
[14]More details about “household equivalents" are in section 3.5.

[15]I converted Korean Won to USD at 1000KRW/1USD for readability. As of Jan 2019, the exchange rate is approximately 1110/1.

[16]Unit fee in this figure ignores households and small restaurants that are not subject to the UPS. In other words, instead of set price equals zero, I drop them for mean calculation.

[17]Each household pays 1/N of the apartment-wise waste tax.

[18]There are a handful of municipalities that implemented UPS through the plastic waste bag. This is relatively rare, since the central government is against the idea of expanding the usage of plastic bags.



(a) Fraction of Household Equivalents under UPS

(b) Average Unit Fee

Figure 2.3: Change in UPS Policy Over Time

tainer inside. The equipment has a lid that opens if a resident tags his uniquely identified RFID card on the sensor. At the time of disposal, it weighs and informs residents the weight and corresponding fee amount. Similar to the plastic bag based system, the RFID system also levies fee proportional to the individual household's waste quantity.

Second, there is a gap between the concept of UPS–set marginal price for food waste positive–and what the central government recognize as UPS. Since the cost of RFID equipment was non-trivial,$800-$1000 per equipment where one equipment can serve 60-100 households, and the central government tried to minimize the plastic bag usage, municipalities were allowed to implement UPS on apartments through group pricing. A typical apartment household is charged 1/N of the apartment-wise waste bill. Considering per unit fee level and the number of households per apartment complex, the group pricing in practice is not much different from the flat fee pricing from an individual household's perspective[19]. Thus in this paper, I do not treat group pricing as UPS. Note, this is going to underestimate the effect of UPS policy, if the group pricing has any positive marginal

[19]There are 7,153 apartment complex with more than 150 units only in Seoul metropolitan area, which covers more than half of the population of the area. These apartments on average has 613 households. Now, the highest per unit fee level is $0.4/gallon. The marginal cost of disposing one additional unit of food waste for an average household at this fee level is less than $0.0006/gallon

pricing effect.

Although illegal dumping is a critical issue in any waste reduction policies, measure the extent of it has been challenging (Fullerton et al. 2008). Earlier studies relied on subjective measures like interview or survey (Reschovsky and Stone 1994, Fullerton and Kinnaman 1996), but the reliability of such data is limited. In this paper, I use landfill waste quantity, which arguably is the easiest and cost-effective way to engage in illegal dumping, as a measure of it.[20]

Illegal dumping of food waste in principle can happen at multiple places including toilet, kitchen sink, backyards, and landfill waste bag. In the urban South Korea setting, however, the first three are not the easiest option. Toilet or kitchen sink can cause extremely costly plumming problems, especially considering the fact that food grinder is illegal and thus not common in South Korea.[21] Third, Also, as (a) and (c) of figure 1 show, it is not easy to find empty space or backyard in living arrangements in large South Korean cities and thus illegal dumping on empty land or composting in backyard are not easy options. Instead, a prevalent mode of illegal dumping is mixing food waste into landfill waste bag, which in general is cheaper than a food waste bag.[22] Since the landfill waste is charged at much lower price than the food waste, reflecting lower marginal treatment cost, and many people reside in dense, urban setting where other forms of waste disposal (e.g., roadside dumping, illegal incinerating, or backyard composting), arguably landfill waste bag is the most convenient way of engaging in food waste illegal dumping.

[20]In addition, it has the highest social marginal cost among a few different options, making lower bound welfare calculation easier. More details in section 2.7.

[21]This especially true for apartment residents. There are anecdotal stories where people end up paying over $10,000 for clogging the main plumming pipe of an apartment.

[22]South Korea implemented UPS for landfill waste in 1995, and everyone has to purchase a designated landfill waste bag. In most cases, landfill waste bag is much cheaper–on average xx% of the food waste unit price–reflecting lower cost of waste treatment.

2.4 Data

2.4.1 Data Description

This analysis has compiled an unprecedented set of waste related data to assess a more comprehensive impact of the food waste tax. That is, I collect data not only on food waste but also on landfill waste quantity, food purchase quantity, and energy consumption that can shed lights on how and why people respond to the policy.

Food waste and landfill waste quantity. The food waste and landfill waste quantity comes from the UPS yearbook by the Ministry of Environment. It documents each municipality's annual waste quantity from households and small restaurants. I use 2010 to 2015 as main sample because 2010 is the first year in the consumer grocery panel and also because the data is discontinued in 2016. Also, I focus on the greater Seoul area for the same reason: the grocery data covers only the greater Seoul area. Note, although geographically small portion of the country, it covers 50% of the country's population.

UPS policy. To document each municipality's policy on UPS, I review 61 municipality's current and past ordinances on food waste management. Specifically, I track the month of the year the UPS was implemented for landed houses, apartments and small restaurants and the corresponding per unit fee. When ordinances are unclear, I use the UPS yearbook for fee level. For unclear UPS status, I file requests in accordance with the Official Information Disclosure Act and cross-check with newspaper articles. Finally, I validate the UPS status with the ministry's internal document. I combine these data with various municipality characteristics from Census on establishments, regional statistics (Statistics Office) and Statistics Yearbook (Metropolitan city governments). These variables, which are closely related with waste generating behavior, include number of restaurants, population, number of companies, education level, number of households, number of apartment units and landed houses, age, income proxy for each year and municipality.

Grocery purchase. To study the impact of UPS on food purchase behavior, I use the consumer grocery panel data from the Rural Development Administration in South Korea, which is comparable to the Nielsen Homescan data in the US. The survey starts in 2010 and has approximately 1000 panelists (households) each year. For the main analysis, I use the balance panel of 676 households from 2010 to 2017. As mentioned earlier, survey region is limited to the metropolitan Seoul area. For data collection, a journal is mailed to the panelists each month, and they are required to keep the record of their grocery and dining for each month. The data set documents each purchase in great detail with variables such as type of store, shopping date and time, item category (wide, middle, detail, product name), expenditure, and unit price.[23]

2.4.2 Summary Statistics

Table 2.1: Summary Statistics for Key Variables

Variables	Mean	Std.Dev.	N
Per Capita Monthly Food Waste (KG)	7.38	2.37	180
Per Capita Monthly landfill waste (KG)	8.22	3.88	180
Pre-2013 % UPS	0.438	0.257	180
Per Capita Monthly Grocery Purchase (KG)	18.81	13.23	35,524

Table 2.1 presents summary statistics for key variables used in the analysis. The first three rows are from 2010 to 2012 and the last row, which is from the consumer panel data, is based on pre-UPS observations. A few points are worth noting. First, food waste and landfill waste quantity, on average, are similar to each other. This corroborates earlier studies that find food waste as the largest single component in municipal solid waste (Kaza et al. 2018). Second, before

[23] About 56 percent of the observations has missing unit price information for balance panel. In these cases, I impute the missing values using price information from the same municipality, month, store type, and food category. This recovers 64 percent of the missing price information. For values still missing, I expand the geographic region to the cluster of (5-6) nearby municipalities. This recovers additional 17 percent of missing price information. I drop 19 percent of the observations without price information after two rounds of imputation. For more details, see the data appendix.

the expansion in 2013, about 44% of household equivalents were subject to the UPS for an average municipality. There is substantial variation, however, in this ratio as the relatively large standard deviation suggests. Third, per capita grocery purchase is 18.8KG. Given that an average person consumes about 2 to 2.5 KG of food per day, this suggests that the survey captures most of the food people consume. However, there are non-trivial number of households with inconsistent reporting as suggested by the minimum value. Later, I provide estimates after removing these households, and the results remain the same.

2.5 Empirical Strategy

I exploit a staggered expansion of the UPS policy to estimate the causal effects of the food waste tax on various outcome variables. The UPS expansion is driven by the central government's initiative, and thus this creates a plausibly exogenous variation in the UPS status at the municipality (for food waste and landfill waste quantity) and individual household (for grocery purchase quantity) level. More specifically, I estimate the following model:

$$log(Y_{mt}) = UPS_{mt}\beta + \boldsymbol{X_{mt}}\delta + \theta_m + \tau_t + \epsilon_{mt} \tag{2.3}$$

Here, Y_{mt} is per capita food waste quantity for municipality m in year t, UPS_{mt} is the fraction of the "household equivalents" subject to the UPS, $\boldsymbol{X_{mt}}$ is municipality specific characteristics such as unit price of landfill waste, the fraction of the single-person household, population, education (fraction of the BA holders), number of preschool children, fraction of the apartments out of total households. θ_m, τ_t are municipality and year fixed effects, controlling for unobserved time-invariant municipality characteristics that could affect waste generation and overall time trend. β is the coefficient of interest, which estimates the marginal effect of changes in the fraction of the household equivalents subject to the UPS. To explore UPS impact on illegal dumping of the food waste, I repeat the estimation in equation (2.3) using per capita landfill waste quantity as Y_{mt}. Also, I replace UPS_{mt} to $log(Fee_{mt})$, which is log of per unit waste fee, to estimate the price elasticity of food and landfill waste generation. Standard errors are clustered at the municipality level, allowing for arbitrary serial correlation within each municipality.

One practical issue in construction of the UPS variable is that the policy covers not only households but also small restaurants, and a restaurant and a household do not produce the same amount of food waste on average. For instance, Kim et al. (2010) found that a typical restaurant produce as much food waste as 7 to 11 households. I also find the ratio 10 from the UPS yearbook data using municipalities that allow me to observe food waste waste quantity separately from households and restaurants.[24] Using this conversion ratio, I construct "household equivalent" by calculating the weighted sum of households and restaurants where the weight are 1 and 10, respectively. Using this I construct the UPS_{mt} term as the following: $UPS_{mt} = \sum_k ups_{mkt} z_{mkt}$ where ups_{mkt} is a dummy variable that takes 1 if a municipality m implements the ups in year t for k where $k = \{landed\ house,\ apartment,\ restaurant\}$ and z_{mkt} is share of k (out of household equivalents) in each municipality.[25] Fee_{mt} is constructed in the same way, where Fee_{mt} is set to zero when $UPS_{mt} = 0$.

I consider ϵ_{mt} as a municipality-year shock to the outcome of interest that is unrelated to the expansion of the UPS policy. Since the policy is expanded due to the central government's initiative for every municipality, it is unlikely that municipalities select into expanding (implementing) the UPS or increasing the fees.

However, there can still be a concern over, for instance, differential rate of expansion over different municipalities. This is mainly driven by the differential

[24] I use Jongno-gu and Dongdaemun-gu in Seoul to verity this.

[25] For instance, suppose that a municipality has 350 landed houses, 200 apartment units, and 30 restaurants. Also, suppose that only restaurants are under the UPS. Then, household equivalent is $350 + 200 + 30 \times 10 = 850$ (using 10 as the conversion ratio), and $UPS = 0 \times \frac{350}{850} + 0 \times \frac{200}{850} + 1 \times \frac{300}{850} = 0.353$.

composition of the housing types across different municipalities. Figure shows a clear negative relationship between the fraction of apartments and the change in the UPS implementation rate. As long as having more apartments are systemically related to higher or lower environmental awareness or any other factors that affect food waste quantity, the error term should be orthogonal to the treatment variable conditional on control variables and fixed effects.

To study the UPS policy effect on the upstream behavior, namely change in grocery purchases, I slightly modify equation (2.3) to equation (2.4). While the source of variation is identical, modification is needed because the level of observation is individual household i rather than municipality m. Q_{iw} is the per capita grocery purchase quantity (and per capita GHG emissions from purchased grocery) for household i in week w, UPS_{iw} is a dummy variable that takes 1 if a household i is subject to the UPS. $\boldsymbol{X_{iw}}$ is household characteristics such as a dummy variable for apartment, income level. I also include individual household fixed effect λ_i, month of the year fixed effect ω_m, and year fixed effect τ_t.

$$log(Q_{iw}) = UPS_{iw}\beta + \boldsymbol{X_{iw}}\delta + \lambda_i + \omega_m + \tau_t + \epsilon_{iw} \tag{2.4}$$

I also estimate an event study version of equation (2.4) as equation (2.5). I aggregate the weekly level observation to yearly level because weekly observations are sensitive to potential stockpiling in response to promotion, for instance.

$$log(Q_{it}) = \sum_{k=-5}^{4} \beta^k UPS_{it}^k + \boldsymbol{X_{it}}\delta + \lambda_i + \omega_m + \tau_t + \epsilon_{it} \tag{2.5}$$

2.6 Results

2.6.1 UPS on Waste Quantity

I first study the effect of the UPS on food and landfill waste quantity. Table 2.2 shows the result from estimating the equation (2.3). In column (1), I regress

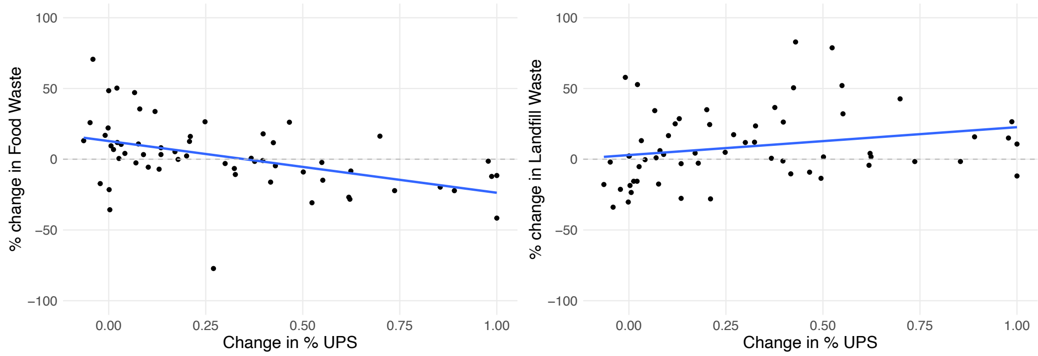
Table 2.2: Effect of Unit Pricing Scheme on Per Capita Waste Quantity

	(1)	(2)	(3)
% UPS	−.231	.122	
	(.052)	(.056)	
Fee (USD)			−2.821
			(.591)
Dep.Var	Food Waste	Landfill Waste	Food Waste
Municipality FE	X	X	X
Year FE	X	X	X
Num. obs.	360	360	360

Note: This table is produced from equation (2.3). The dependent variable is log(food waste) for columns (1) and (3) and log(landfill waste) for column (3). % UPS is the fraction of the household equivalents that are subject to the UPS. Standard errors are clustered at the municipality level.

UPS_{mt} on log quantity of per capita food waste with municipality and year fixed effects. The point estimate shows that the policy effect is economically large and statistically significant: it indicates that when the fraction of household equivalent subject to the UPS increases from 0 to 100 percent, it leads to a 27 percent reduction in food waste quantity.[26] Given that the average % UPS in 2010 was 44% and it has increased to 74% in 2015 (see figure 2.3 (a)), the actual reduction in the food waste quantity for an average municipality is about 8%. In column (2), I estimate the price elasticity of the food waste tax, which is -0.033. This reconciles earlier studies, and suggests that the different conclusion of my paper is not driven by more elastic demand response.[27] In column (3) and (4), I repeat the same analysis using landfill waste quantity per capita as an outcome variable. As stated earlier, it is the easiest and least costliest way of illegal dumping in urban South Korea setting, and is a proxy for illegal dumping. Column (3) shows that when the fraction of household equivalent subject to the UPS increases from 0 to 100 percent, it leads to a 9.4 percent increase in landfill waste quantity. Again, an average municipality experienced about 30%p increase in % UPS, which indicates that the increase in the landfill waste is about 3% over 2010 to 2015. In column (4), I estimate the price elasticity and it is neither economically nor statistically

[26]Since the magnitude of the change is large, I exponentiate the coefficient: $e^{-0.31}-1 = -0.266$.
[27]For instance, an estimate from Fullerton and Kinnaman (1996) is -0.076.



(a) Food Waste

(b) landfill waste

Figure 2.4: The Effect of the UPS on Waste Quantity

significant.

To put this results in perspective, and to identify the source of reduction in the measured food waste quantity, I calculate the effect of UPS on food and landfill waste quantity per person per month using the estimates in column (1) and (3) of table 2.2. From the summary table, we know that the average waste quantity per person per month is about 7.3 KG from 2010 to 2012. If UPS is going from 0 to 100 percent, it translates into $7.3 \times 0.268 = 1.96KG$ reduction in food waste per person per month.[28]

Repeating the same exercise using the estimates from column (3), we know that if UPS is going from 0 to 100 percent, it translates into $8.2 \times 0.09 = 0.74KG$ increase in the landfill waste per person per month, where 8.2KG is the average landfill waste quantity per person per month from 2010 to 2012. Given that the UPS reduces food waste by 1.96KG, 0.74KG increase in landfill waste suggests that about 38 percent of the reduction is due to the illegal dumping.

Figure 2.4 presents a graphical illustration of the results in Table 2.2. Recall that the identification exploits the plausibly exogenous change in the % UPS within each municipality and investigates its impact on the waste quantity. In

[28]If we use the actual change in the fraction of the UPS policy over time, which went up from 0.43 in 2012 to 0.74 in 2015, the expansion policy reduced approximately 0.66 KG of food waste. This number can be calculated using $log(x) - log(7.3) = -0.312 \times 0.31$. Solving for x, we get 6.62, thus the reduction is 7.3-6.62 = 0.68.

Table 2.3: Effect of UPS on Grocery Purchases

	(1)	(2)	(3)	(4)
UPS	−.048	−.076	−.043	
	(.017)	(.027)	(.019)	
Fee (USD)				−.631
				(.294)
Year FE	X	X	X	X
Month FE	X	X	X	X
Household FE	X	X	X	X
Sample	All	Perishable	Storable	All
Num. obs.	266831	266831	266831	266831

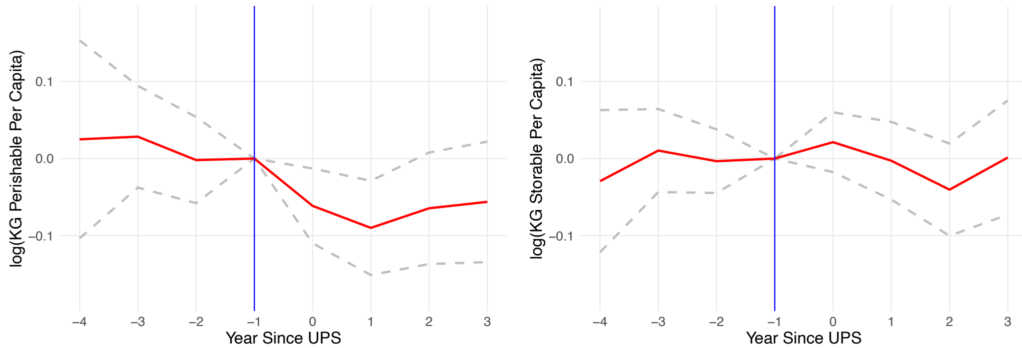
Note: This table is produced from equation (2.4). Dependent variables in column (1) is the total weekly grocery purchase amount per household. In columns (2) and (3), weekly perishable and storable grocery purchase amount per household are used, respectively. UPS is a dummy variable that takes 1 if a household is subject to the UPS. Fee (USD) is per KG waste fee converted to USD. All standard errors are clustered at the municipality level.

each panel, the horizontal axis represent the change in % UPS and the vertical axis shows resulting change in the per capita waste quantity from 2010 to 2015 where each dot represents a municipality. Panel (a) shows that the reduction in the food waste quantity is larger when the change in the UPS ratio is larger whereas panel (b) shows the opposite. Also observe that the slope of the fitted line is much steeper in panel (a), reflecting the larger coefficient (in absolute terms) of column (1) over column (3) in Table 2.2.

2.6.2 UPS on Food Purchase and Food-Induced GHGs Emissions

Table 2.3 shows the estimation result of the equation (2.4). I use the balance panel of 676 households for main analysis since new panlists from the wave 2 are different from the initial panelists in important ways. Specifically, wave 2 panel have single-person households and tend to be younger. But I also present the results from the entire panel in appendix, which are qualitatively similar.

The outcome variable is log of the total weekly per capita food purchase measured in KG. Similar to table 2.2, I present results based on both the UPS dummy and log of per unit food waste fee. Note, the UPS variable is a dummy variable since the level of observation is individual households. In every column, I include individual panelist, year, and month fixed effects to account for unobserved



(a) Perishable Items (b) Storable Items

Figure 2.5: The Effect of the UBP on Grocery Purchases

household characteristics, overall trend and seasonality in food purchase quantity. Standard errors are clustered at the municipality, which is the level of the policy change.

In column (1), I show that the UPS policy reduces the purchased food quantity by 3.5%. The magnitude is small but non-trivial, especially given the fact that the per unit fee is very small. Assuming that people do not reduce the amount of food they consume (namely, they eat the same amount of food), the finding suggests that households become more careful about their grocery shopping and waste less uneaten food. Column (2) shows the semi-elasticity estimate where the explanatory variable is per KG food waste fee in USD.[29] At the average fee level in 2017 (0.058), the policy effect is 2.1% reduction in food purchase. Note, this is smaller than the estimate from column (1), suggesting that the policy could have a non-price effect such as increasing salience.

In columns (3) and (4), I estimate equation (2.4) using per capita grocery purchase amount for perishable and storable items. If the reduction in column (1) and (2) is driven by the UPS policy, we should observe a larger policy effect on perishable items than the storable items. Estimates in columns (3) and (4) show that the policy effect is driven by the perishable items. Specifically, perishable

[29] I approximate Korean Won to USD at 1000KRW/1USD. As of Jan 2019, the exchange rate is 1110/1.

purchase has decreased by 5.1% while storable purchase has decreased by 2.8%. In addition to providing additional evidence on interpreting the results in columns (1) and (2) as the effect of the UPS policy, estimates in column (4) is important in its own right. The fact that the households buy less storable items means that the UPS is not simply inducing substitution between perishable and storable by effectively increasing the relative price of the perishable items. Rather, it leads to an overall reduction in excessive food purchase.

Using the estimates, I calculate the amount of food purchase decreased after the UPS policy. Using 2012 food purchase per person per month (18KG) as baseline, 3.5 percent reduction is translated to 0.64KG less food purchase. Comparing this to the reduction in food waste from the UPS (1.96KG), I find that at least 33 percent of the reduction in food waste comes from the reduction in food purchase.

In figure **??**, I plot β^k from equation (2.5), separately for perishable (panel (a)) and storable (panel (b)) items. To produce these figures, I follow Kline (2012) to impose end point restriction such that $\beta_k = \underline{\beta}$ for $k < -5$ and $\beta_k = \bar{\beta}$ for $k > 4$, where the unit of k is a year. Because the sample is unbalanced in event time, these endpoint coefficients give unequal weight to households experienced the UPS early or late in the sample. For this reason, I focus the analysis on the event-time coefficients falling within $k = [-4, 3]$ that are identified off of a nearly balanced panel. Consistent with Table 2.3, panel (a) shows a consistent reduction in the quantity purchased for the perishable items after the UPS whereas no clear pattern is observed for storable items.

2.7 Welfare Analysis

In this section, I present a back-of-an-envelope welfare calculation result explicitly accounting for the substitution patterns. Figure 2.1 illustrates necessary elements to calculate lower bound of the policy effect. Since I have estimates for $D(q)$ and $D(\tilde{i})$ from section 2.6, I start by calculating the four SMCs. Each of these four SMCs is sum of private marginal cost (PMC), which is consist of waste pickup,

	L	Q	I	X^*
PMC	\$160	\$160	\$170	\$80
EC	\$3.6	\$3.6 + \$142.2	\$24.3	\$5.6
SMC	\$163.6	\$305.8	\$194.3	\$85.6

Table 2.4: Summary of SMC Estimates

delivery, and treatment cost and external cost (EC). For EC, I primarily consider greenhouse gas (GHGs) emissions because it is the largest external cost from waste (Davies and Doble 2004).[30] Table 2.4 presents the summary of the SMC estimates.

First, consider SMC_L, which is the benchmark case. Since waste management is widely outsourced, and the market has almost zero entry-cost, market price can be deemed the competitive price. Thus, I use 160 dollar per ton of food waste as PMC_L, based on Seoul Metropolitan Government's spending figures.[31]. For EC_L, I use the per unit methane emissions information from the national carbon inventory report (GGIRC 2015), which relies on IPCC 2006. Since more than 95 percent of the food waste is processed in composting or animal feed processing sites in Korea, GHGs emissions is 0.1 ton CO2eq per ton of food waste, which is 1/7 of that of food waste in landfill.[32] Using the 2015 EPA social cost of carbon estimate \$36 per ton of CO2eq, EC_L is \$3.6/ton of food waste. Thus $SMC_L = \$160 + \$3.6 = \$163.6$ per ton of food waste.

Assuming that Q is legally disposed, SMC_Q is identical to SMC_L with one addition: producing the leftover food, which could have been saved if producers respond to lower demand, induce extra external cost. To account for this, I need

[30]Two main externalities from food waste is disamenity (bad odor, leachate, etc) and methane emissions from degradation. The first part is relatively well-managed in South Korea, for instance, by putting a ban on landfilling food waste.

[31]I acquire this information through OIDA request. Similar information can be found on the Seoul Metropolitan City website at https://seoulsolution.kr/ko/content/3438 (accessed on Jan 23, 2020)

[32]However, it is questionable how well accepted the product is in the market. A series of investigative news articles point out that many of those "fertilizers" or "animal feeds" are actually illegally dumped in empty lot because no one wants it. In this case, the social external cost would be much higher, justifying a more aggressive waste reduction measures.

to know the GHGs from the production of the leftover food. Recall that from section 2.6, I find that half of the reduction in leftover food comes from perishable although it accounts for only 35 percent of the food basket. Assuming that the composition of leftover food resembles that of reduced leftover food rather than overall food basket, I take average of the mean per ton GHGs emissions from perishables and storables, respectively. To operationalize this, I convert food purchase quantity in KG to its GHGs emissions using the 40 food-item specific GHGs emissions estimates from Poore and Nemecek (2018). Then I calculate per ton GHG emissions for the entire perishable food and entire storable food, respectively. It is very stable during 2010-2012 at 0.9 ton CO2eq per ton of perishable food and 7 ton for storable food. Since the Q basket consists of half perishable and half storable, GHGs emissions from this basket is 3.95 ton CO2eq per ton. Finally, by multiplying the same SCC, we know that the additional EC in $SMC_Q = SMC_L + 3.95 \times \$36 = \$305.8$ per ton of food waste. Surprisingly, the additional EC is as large as the PMC.

Since illegal dumping is measured through landfill waste quantity, I use PMC of landfill waste treatment for SMC_L calculation. Again, based on government budget data, the amount comes at $170 per ton of illegal dumping. It is worth noting that this price reflects landfill space limitation: starting from 2020, any municipality failing to reduce their waste by 10 percent in comparison to 2018 amount, face twice as high landfill usage fee ($\$60 \rightarrow \$120$). Additional illegal dumping will certainly trigger the space constraint, so I apply $120 per ton as treatment cost. For EC, I use the per unit GHGs emissions coefficient from IPCC GPG2000 (Hiraishi et al. 2000) (0.675ton CO2eq/ton of food waste).[33]. Using the same SCC estimate, $SMC_L = \$170 + 0.675 \times \$36 = \$194.3$ per ton of food waste. Note that this is about 30 dollars higher than SMC_L, thus substitution towards illegal dumping is welfare-harming.

For $D(q)$, I use an estimate in column (4) of table 2.3. By multiplying -0.63

[33]In particular, I rely on default method which could be less accurate but allows comparison across different waste disposal methods.

by the average fee level in 2015 \$0.04 and the average grocery purchase quantity of 18.8KG, the estimated reduction in grocery purchases is 0.47KG. With this information, I calculate the area of the trapezoid DEFG, which becomes \$ 144 per person per month $(305.8 \times 0.47 - 0.47 \times 0.04 \times 0.5)$. Note, because the size of the tax is negligible in comparison to the social cost, the welfare improvement is almost identical to the size of the rectangule.

Next, I need to calculate the trapezoid BCEF. As I calculate the lower bound of welfare effect, I treat the reduction in the observed food waste quantity unexplained by the reduction in grocery purchases as illegal dumping. To proceed, I first estimate the observed reduction in food waste using column (3) of table 2.2. By multiplying -2.82 by the average fee level in 2015 \$0.04 and the average food waste quantity per capita before ups 7.4, the estimated reduction in the observed food waste quantity is 0.83KG. This suggests that 0.36KG is the maximum amount of illegal dumping. Next, multiplying 0.36 with $SMC_I - SMC_L = 30.7$ and subtracting $0.04 \times 0.36 \times 0.5$, the estimated welfare loss from illegal dumping is \$11.1 per person per month. This suggests that the net benefit of UPS per person per month is \$133, which scaled up by the population size of South Korea (50 million) translates into a \$6.6 billion welfare gain per year.

2.8 Conclusion

In this paper, I study the welfare impact of unit based food waste tax in South Korea. I show that the welfare effect critically hinges on households' behavioral responses to the policy, and estimate the policy effect on various outcome variable. Specifically, I estimate how overall food waste quantity, illegal dumping, and grocery purchase changes after the tax. Importantly, while illegal dumping makes the policy welfare-harming, reduction in grocery purchases, which reflect reduction in wasted food quantity, is welfare improving because it reduces social marginal cost of both food waste and food production. A simple welfare analysis shows that abstracting away from the environmentally advantageous consumption change substantially underestimates the welfare benefit of the policy.

The findings of this paper have important policy implications. First, this is politically less contentious policy in comparison to other types of carbon taxes. In comparison to energy or gasoline, waste is less likely to be regressive given that higher income leads to higher waste generation. Also, people in general agree that wasting food is a bad thing such that it is easier to justify. Second, waste policies that focus on recycling might not be the best approach. It not only gives moral justifications for producing waste, but also disincentivizes peole to reduce waste in the first place. It is true that recycling saves resources in comparison to landfilling, but it is still imperior to not producing waste in the first place.

Chapter 3

Group Pricing and Free-Riding: Evidence from Food Waste Tax

3.1 Introduction

In many countries, usages for public services such as electricity, water, and waste management, are measured and charged at a group level.[1] Under a group pricing scheme, each household faces a substantially lower private marginal cost–especially when group size is large–than a collective marginal cost. Such an attenuated price signal leads to overuse of the services and resources, which creates two layers of externality First, is an economy-wide externality: over usage imposes a substantial burden on both public finance and the environment.[2] The second is an interhousehold externality, which makes each household end up with a higher public service bill.

In this paper, I study how solving a free-riding problem between households by changing the pricing scheme from group-based to individual-based can reduce service overuse. Even if the per-unit service price remains constant, the conversion from group to individual substantially increased the private marginal cost of waste generation. This induces conservation efforts from households. Further, I explore heterogeneous treatment effects along group size, wealth, and pre-treatment service usage dimensions to investigate the determinants of the magnitude of free-riding. This paper also estimates welfare gains from eliminating interhousehold

[1]For instance, in the US, cities like New York and Chicago introduced meters, which are collective rather than individual metering, only recently. Boston's smart metering is done at the building level only. In several European countries, water companies serve the whole building with only one meter: they send a single bill to the building manager (OECD 2013). The same is true in Bengaluru, a large metropolitan city in India, where many waste pickup service companies measure waste quantity at the apartment level.

[2]Municipal solid waste management consists of as large as 20% of municipality budget (Kaza et al. 2018). Electricity and waste generation are major sources of greenhouse gas emissions and drought is imposing serious threats to health and other economic activities.

externality focusing on government finances and greenhouse gas emissions.

I focus on apartment complexes in South Korea to exploit the conversion from a group-level food waste unit-based pricing (hereafter "group pricing") to an individual-level food waste unit-based pricing (hereafter "individual pricing). In a response to increasing food waste, the South Korean government carried out a policy initiative to expand the unit-based pricing nationwide in 2013. At first, many apartments were under the group pricing because it was costly to measure waste quantity at the individual household level. Under group pricing, an entire apartment receives a single bill and its residents split it up by the number of households. Since 2014, local municipalities have expanded the Radio-Frequency Identification (RFID) food waste collection system to apartment complexes on an application basis, which allows implementing the individual pricing. Due to the budget constraint, the RFID system expansion rolled out over multiple years creating the main identifying variation.

From the perspective of an individual household within a large apartment complex, the conversion from group to individual pricing is plausibly exogenous because decisions on apartment management are almost exclusively made by the management office and the representatives in most South Korean apartments (Lee and Choi 2015). However, to account for a potential selection in treatment, I present results based on two different variations, namely whether treated or not and whether treated early versus late. For estimation, I employ Callaway and Sant'Anna (2020) estimator as opposed to a conventional two-way fixed effect model, reflecting recent advances in the difference-in-difference literature (Goodman-Bacon 2018, Baker et al. 2021).

To leverage the variation, I construct an apartment complex by month-level panel data on food waste quantity and bill, number of households, apartment size, housing prices, and individual pricing start date. Data related to food waste and individual pricing comes from three separate Official Information Disclosure Act (South Korean equivalent of FOIA) requests. The dataset covers four districts in Gwangju Metropolitan City, one of the largest urban areas in South Korea.

Importantly, the waste data was collected for billing purposes, which ensures high credibility.

The empirical analysis produces two key results. First, I find that switching from the group pricing to individual pricing while holding per KG price constant leads to a 20% reduction in per unit food waste quantity. The finding indicates that overproduction of food waste resulting from free-riding was rampant under the group-pricing. The effect remains persistent irrespective of which control group–between never-treated and not-yet-treated apartments–is used. Also, the effect is persistent even after 18 months from the implementation date.

Second, substantial heterogeneity in treatment effect is observed. Specifically, I show that the policy effect is larger as the number of households within an apartment complex ("group size") increases. However, I also find that the group size effect plateaus after a certain point. In particular, the difference in the magnitude of the effect size is negligible when the group size becomes larger than 300 to 500 households. In addition, policy effect is driven by groups with larger pre-treatment food waste quantity, which is consistent with the free-riding hypothesis. I also investigate how the treatment effect varies as the wealth level, which is proxied by per square foot housing price, changes but it does not present a clear pattern.

This paper relates to two different bodies of literature. First, it relates to the literature examining the effect of changing the pricing scheme (Levinson and Niemann 2004, Elinder et al. 2017, Jessoe et al. 2020). While earlier works mostly focused on electricity, this paper points out the importance of pricing schemes in waste management services. Also, the status quo in these studies is fixed pricing while the status quo in this paper is variable pricing at the group level, which is widely used given the cost of individual measurement but has never been thoroughly evaluated. Another closely related paper is Jack et al. (2020), which studied intrahousehold externality in water consumption using a financial reward field experiment. While their paper also shows that financial incentives can mitigate free-riding, this paper exploits an actual increase in private marginal cost, which could have a different effect from positive financial compensations. Further, the incentive to free-ride would be somewhat limited within a household because economic interest is shared to large extent.

Second, the heterogeneous treatment effect presented in this paper provides empirical evidence on the determinant of free-riding behaviors in a real-world setting. Earlier works focused on identifying determinants of free-riding behavior using mostly lab experiments and showed that factors like group size, monetary stake, monitoring, punishment, and repetition could critically determine the extent of the free-riding behavior (Isaac and Walker 1988, Ledyard 1995, Goeree et al. 2002, Baland and Platteau 2003, Carpenter 2007, Diederich et al. 2016). While these works substantially enhance our understanding of free-riding behavior, the results from lab experiments need to be carefully evaluated before applied to a real-world problem. It is because many factors such as the group size, stake, types of population, and so forth can be substantially different between lab and real-world (Isaac et al. 1994, Levitt and List 2007). Zhang and Zhu (2011) is, to the best of my knowledge, the only existing paper studying the relationship using a real world setting. They exploit the Chinese government's policy that restricted mainland China residents' access to Chinese Wikipedia and find that smaller group size leads to a fewer number of Wikipedia contributions. This paper differs from Zhang and Zhu (2011) in the sense that I can directly observe the group size using the number of units per apartment complex, which allows me to pin down the group size effect more precisely.

The paper proceeds as follows. Section 3.2 presents a simple conceptual framework that connects waste quantity to free-riding. Section 3.3 provides background on the unit-based food waste pricing policy in South Korea. Section 3.4 details the data sources and provides some summary statistics. Section 3.5 lays out the empirical strategy. Section 3.6 presents results while Section 2.7 discusses welfare implications due to free riding. Section 3.7 concludes.

3.2 Conceptual Framework

In this section I present how per unit food waste quantity can be a proxy measure for households' contribution to public good. As the discussion in chapter 2 shows, households can reduce food waste quantity by putting various types of efforts such as increasing grocery shopping frequency or paying more attention to grocery planning. This helps households to save food waste fee, but effort is costly as well. Thus, households seek to minimize the combined cost by choosing an optimal level of effort. More formally, households under individual pricing scheme, which is the benchmark case, seek to put effort level e^* that minimizes equation (3.2). p is per unit fee, q is food waste quantity, θ is monetized cost of effort, and e is the effort level. Assuming $q' < 0$, $q'' > 0$, $\theta' > 0$, and $\theta'' < 0$, there is unique e^* that solves (3.2). Specifically, e^* equates the marginal benefit of effort, which is saved waste fee, to the marginal cost of effort in absolute terms ($pq'(e^*) + \theta'(e^*) = 0$).

$$C_{ind} = pq(e) + \theta(e) \tag{3.1}$$

Now, let's examine how the households' problem changes under the group pricing scheme where each unit is responsible for 1/N of the apartment-wise waste fee. In other words, apartment-wise waste fee is unit price times apartment-wise food waste quantity $Q = \sum_i^N q_i$ where q_i is each unit's waste quantity.

$$C_{grp} = \frac{p \times \sum_i^N q_i(e_i)}{N} + \theta(e) \tag{3.2}$$

Note, under the group pricing scheme, marginal benefit is smaller (in absolute terms) than marginal cost at e^* because $q'(e^*) > \frac{q'(e^*)}{N}$. Importantly, an optimal effort level under the group pricing ($\tilde{e}$) is smaller than e^* and decreasing in group size N, suggesting that the contribution to the common good will decrease in group size.

3.3 Background

Gwangju is a metropolitan city with 1.5 million people. The area is well-known for its unique cuisine–not only for its top-notch savor but also for its serving portion. As such, the food waste quantity per capita is highest among the seven metropolitan areas in South Korea (Gwangju Metropolitan City 2016). Although the city collects food waste fee from households, it covers 42% of the operating cost, and thus the city spends more than $20 million for food waste management alone. Moreover, the city's processing sites' combined capacity is below the quantity generated, making the city to ship the food waste to neighboring municipalities.[3] This is not only about twice as expensive, but also invites political tension between Gwangju and hosting municipalities due to environmental disamenity from food waste (Gwangju Metropolitan City 2016).

Given the problems, the city government implemented an initiative in 2016 aiming at reducing food waste by 30% by 2020. The initiative has two major pillars: educating on food waste reduction and expanding the RFID (Radio-Frequency IDentification) food waste collection system for apartment residents. As apartment residents produced 60% of the entire food waste, the RFID system expansion on apartments was one of the key strategies for food waste reduction.

The RFID system consists of an ID card for each household, an electric weighing machine combined with an RFID receptor, and a central server that communicates with each electric weighing. The weighing equipment incubates a large waste bin inside and has a lid that opens if a resident tags his uniquely identified RFID card on the sensor (see figure 3.1). At the time of disposal, it weighs and informs residents of the weight and corresponding fee amount. During the sample period (2016-2018), the per KG fee was KRW 63 or $0.06. The fee is either subtracted from the pre-loaded value or added later to the monthly apartment management fee. The key benefit of the RFID system is that it allows an individual household level unit-based pricing.

[3]Constructing a new food waste processing site could be a solution, but given its political risk, the city does not consider it as a viable, short-term solution.

(a) Individual Pricing - RFID

(b) Group Pricing

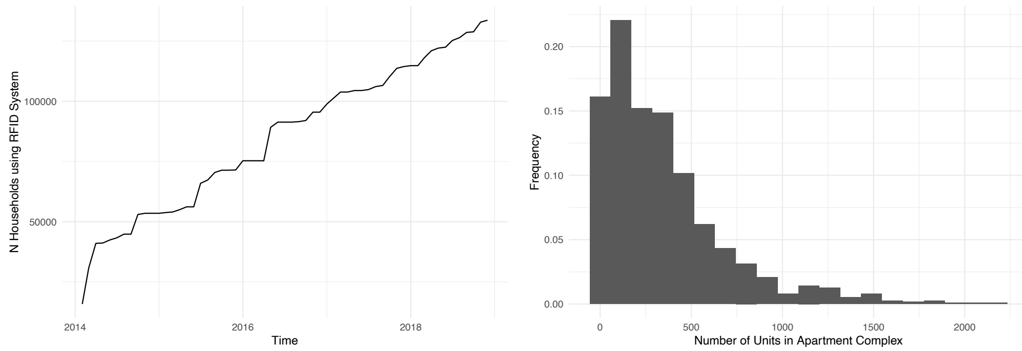
Figure 3.1: Two Different Types of Waste and Waste Tax Collection

The RFID system rolled out over multiple years. First, the government installed the RFID system for an apartment complex upon application for apartment complexes over a certain size (usually 60 units). Second, the metropolitan government amended its ordinance to make it mandatory for newly built apartments to install the RFID system and a small subset of apartments in the dataset starts the RFID system from the onset. Figure 3.2 panel (a) shows the number of households using the RFID system has increased over three folds between 2016 and 2018.[4]

The status quo for apartments without the RFID system is group pricing. In 2013, the central government has pushed for a nation-wide expansion of unit based food waste pricing and as a result, the majority of the apartments in the nation hava adopted group pricing. For sure, group pricing was not ideal in incentivizing households for waste reduction. However, it was a much cheaper way to implement the "unit-based pricing": RFID equipment which serves 50 households costs approximately $2,000 while no additional investment was needed to implement the group pricing scheme other than small administrative costs.[5] As a result, many municipalities–including Gwangju–started the unit-based pricing

[4] Based on the total number of apartment units in Gwangju as of December 2018 (403,235), 43.5% of the residents (175,259) were using the RFID system.

[5] Even before the unit-based pricing, food waste was collected separately in South Korea. For apartments, a prevalent method was to collect it using a large communal waste bin. In Gwangju, group pricing was implemented by adding up the weight of the communal bins at each apartment by month level and divide it equally across each household.



(a) Number of Households Using the RFID System

(b) Distribution of Apartment Complex Size

Figure 3.2: Characteristics of Apartments in Gwangju

with the group pricing scheme where each apartment complex is charged based on the quantity the complex generated and residents pay 1/N of the fee. As Figure 3.2 panel (b) shows, the size of N varies substantially from apartment to apartment. This allows me to explore the relationship between group size and free riding behavior in a real world setting.

As the RFID system has never been used for food waste collection before, a typical apartment has 2-4 weeks of pilot period.[6] During that time, residents are still charged as a group, but are required to use the RFID system for their waste disposal. Importantly, the equipment starts informing users how much waste they have disposed of. A practical implication is that there is going to be an information effect even during the pilot period while the price effect kicks in only after the actual treatment date.

3.4 Data

I collect data on food waste quantity from January 2016 to December 2018 at the apartment level from Gwangju, which is one of the major metropolitan cities in

[6]This is verifiable using the billing information. A subset of district offices separately keeps track of pilot and actual start date and the average pilot period lasts for 18 days. Conversation with other district offices also confirmed that 2-4 weeks is a typical pilot period for a typical apartment.

South Korea. The city has a 1.5 million population and has one of the highest ratio of apartment residents in the nation at 78%. The sample has 1,188 unique apartment complexes and 22% of them have switched from the group pricing to individual pricing over the sample period. Waste data and the individual pricing implementation timing comes from two separate Official Information Disclosure Act (South Korean equivalent of FOIA) requests. It has the entire universe of apartments in the city and documents each apartment's number of units, monthly waste quantity, per unit fee, number of households exempted from the fee, monthly waste fee, the start date of the individual pricing at the apartment level.[7] Because the data was populated for billing purposes and actually used to calculate the fee, the weight information in the data is highly credible.

I combine the food waste data with publicly available apartment transaction data from the Ministry of Land, Infrastructure, and Transportation. The dataset documents every apartment transaction. The collected information includes apartment name, transaction date, price, unit size (square meter), and construction year with the exact address. I connect this with food waste data using apartment name and address.

Table 3.1 presents summary statistics for key variables used in the analysis for the full sample (panel A) and a subsample of apartments that have non-missing values over the entire sample period (panel B). Per-unit food waste quantity is calculated by dividing apartment complex level monthly food waste quantity by the number of units. Ideally, the number of occupied units should be used but to the best of my knowledge, no such data exists at the apartment level so I use the physical number of units instead. I take log to account for extreme values. Housing price per size is documented in terms of the Korean currency and square meter, respectively but I translate them into the dollar and square feet for ease of interpretation.[8] Overall, the two panels exhibit similar characteristics, but the full sample has higher standard errors. I present results from both of the samples.

[7]A small fraction of low-income households are exempt from the waste fee.

[8]I used an exchange rate of 1 USD = 1,100KRW. Also 1 m^2 = 10.7639 ft^2.

Table 3.1: Summary Statistics for Key Variables

Variables	Min.	Max.	Mean	Std.Dev.	N
Panel A: Full Sample					
log(per unit KG)	-5.84	5.02	2.95	0.537	63,325
Num Units	9	2,185	350	340	63,325
Price ($) per sqft	55	792	178	73	49,762
Sqft	161	2,960	816	288	51,903
Year Built	1,970	2,016	1,997	9.7	51,903
Panel B: Balanced Sample					
log(per unit KG)	-1.39	5.02	3	0.453	53,867
Num Units	9	2,020	360	341	53,867
Price ($) per sqft	55	792	173	71	43,660
Sqft	172	2,271	820	273	45,371
Year Built	1,970	2,014	1,996	9.02	45,371

In appendix **??**, I compare characteristics for ever-treated vs. never-treated and early-treated vs. later-treated apartments using pre-treatment characteristics. Specifically, I regressed treatment status (either ever-treated or early-treated, where early is defined as before the median individual pricing implementation date) on various covariates and pre-treatment dependent variables using the balanced panel. The regression includes year and month fixed effects to account for seasonality and overall time trend.

Table **??** and Table **??** show the results. Observe that the apartments that have selected into the individual pricing (ever-treated) have higher per unit food waste quantity, higher number of units per apartment complex, are more expensive, bigger, and built in later years all at the statistically significant level. This is not surprising to some extent because an apartment has to be at least 60 units to be eligible for the RFID installation subsidy and also newly constructed large apartment complexes, which tends to have higher housing prices, are required to install the RFID system from the onset. As ever-treated apartments show different observed characteristics from the never-treated apartments, I compare early vs. late treated apartments in Table **??**. Note, the difference between the two groups have shrinked substantially and none of the variables are statistically different between two groups anymore. This suggests that the difference between early vs. late groups are not as stark as ever vs. never groups. Thus, to migitate concern over selection into the treatment, in section 3.6, I present estimation results from different control groups, namely (1) never-treated only, (2) not-yet-treated only, and (3) both never-treated and not-yet-treated observations.

3.5 Empirical Strategy

I exploit a staggered adoption of individual pricing scheme to estimate the causal effect of free-riding behavior on per household waste quantity. I first estimate the treatment effect using a conventional two-way fixed effect (TWFE) approach as in equation (3.3).

$$log(y_{it}) = \beta D_{it} + \alpha_i + \theta_t + \epsilon_{it} \tag{3.3}$$

Here, y_{it} is per unit food waste quantity for apartment complex i in month t, D_{it} is a dummy variable takes 1 if unit i in month t is under individual pricing and 0 if is under group pricing. I include apartment complex fixed effect i and time fixed effect t to control for unobserved heterogeneity across different apartments and also general time trends in food waste generation. β estimates the effect of individual pricing on per unit food waste generation and measures the extent of extra food waste generated because of free-riding behavior.

While equation (3.3) represents a widely-used empirical method, recent advances in the econometrics literature have pointed out that β in equation (3.3) in general is biased and cannot be interpreted as estimand of interest such as ATE or ATT (Goodman-Bacon 2018, Callaway and Sant'Anna 2020).[9] The problem arises because (1) β in equation (3.3) is a weighted average of all possible two-group by two-period difference-in-difference estimators and (2) some of those estimators could be biased. In particular, estimates from later-treated group vs. early-treated

[9] Baker et al. (2021) shows that the equation (3.3) produces unbiased true ATT when (1) there is single period of treatment or (2) when treatment effect is homogeneous over units or over time.

group comparison where the early-treated group serves as a control group are especially problematic when there is a dynamic treatment effect.[10]

Thus, I follow Callaway and Sant'Anna (2020) for the main specification. The estimation works in two separate steps: (i) identify policy-relevant disaggregated causal parameters and (ii) flexibly aggregate these parameters. In practice, the disaggregated causal parameters are "group-time average treatment effects" $ATT(g,t) = E[Y_t(g) - Y_t(0)|Gg = 1]$. $ATT(g,t)$ is ATT for units in group g at time period t where g is the time period of initial treatment. It is identified under the parallel trend assumption that naturally extends the version of a canonical difference-in-difference estimation (Callaway and Sant'Anna 2020). Namely, in the absence of treatment, average untreated potential outcomes of the units in group g and the "never treated" group should have followed parallel trend in all post-treatment periods $t \geq g$.[11]

I estimate $ATT(g,t)$ for each g for $t \geq g$ periods using the regression method as in equation (3.4).[12] Observe, equation (3.4) is simply a weighted average of within-unit long differences in the outcome variable.[13]

$$ATT(g,t) = E[\frac{G_{ig}}{E[G_{ig}]}(Y_{it} - Y_{i,g-1} - E[Y_{it} - Y_{i,g-1}|C = 1])] \tag{3.4}$$

I aggregate the estimated $ATT(g,t)$ in three different ways. First, I aggregate $ATT(g,t)$ for each g over t. It allows me to aggregate the treatment effect over

[10]To see why, it is helpful to revisit the parallel trend assumption for a conventional difference-in-difference estimation: control and treatment groups' potential outcomes follow the same trend in the absence of treatment. For early-treatment group with dynamic treatment effect, this cannot hold true because what we observe is a combination of potential outcome in the absence of treatment and dynamic treatment effect and thus we cannot separately observe the potential outcome.

[11]An analogous assumption is needed when we use "not-yet-treated" units as well as a control group. I present results from two different approaches to construct a control group.

[12]Callaway and Sant'Anna (2020) recommends using a doubly-robust method, but I produce main results with the regression method because I do not have covariates. However, I confirm that the results produced based on the DR method are almost identical.

[13]Alternatively, $ATT(g,t)$ can be implemented by partitioning the dataset to include units $G_g = 1$ and $C = 1$ at time t and $g - 1$ and estimating the canonical difference-in-difference (Callaway and Sant'Anna 2020).

Table 3.2: Effect of Individual Pricing on Per Unit Waste Quantity

	(1)	(2)	(3)	(4)	(5)
Treated	−.205	−.238	−.238	−.220	−.367
	(.087)	(.092)	(.100)	(.122)	(.015)
Control: Never-Treated	X	X	X		X
Control: Not-Yet-Treated			X	X	X
Control: Already-Treated					X
Model	CS	CS	CS	CS	TWFE
Sample	All	Balanced	Balanced	Balanced	Balanced
Num. obs.	63089	53631	53631	15812	53867

Note: This table is produced from equation (3.4) and . The dependent variable is log(per unit food waste KG). Treated is a dummy variable that takes 1 when an observation (apartment complex) switches to the individual pricing. Columns (1) to (4) are estimated using a regression estimator in [?] (implemented using `did` package). Column (5) is estimated using a conventional TWFE model in equation (3.3).Standard errors are clustered at the apartment complex level.

time period for groups treated at different time periods. These parameters are useful to test if units selected into treatment timing. Second, I further aggregate ATT for each g into a single number. This corresponds to β from equation (3.3) in a sense that this parameter summarizes the overall effect of treatment across all groups that are ever-treated. Finally, I aggregate $ATT(g,t)$ in event time. This allows me to understand how the treatment effect evolves over time and also compare pre-treatment trends between treated and control gruops.[14] Callaway and Sant'Anna (2020) estimator not only compares the treated group to a "clean" set of control grups–namely never-treated and/or not-yet-treated groups–instead of all possible control groups but also allows flexible and robust ways to aggregate the estimates.

3.6 Results

3.6.1 Effect of Individual Pricing on Per Unit Food Waste Quantity

Table 3.2 presents the effect of introducing individual pricing on per unit food waste quantity at the apartment complex level. Estimates in columns (1) to (3) come from aggregating $ATT(g,t)$ from equation (3.4), first to the average effect for each group (i.e., a collection of apartments that was treated in the same calendar month) then averages these effects across groups to summarize the overall

[14] These correspond to $\theta_{sel}(\tilde{g})$, θ_{sel}^{O}, and $\theta_{es}(e)$ from Callaway and Sant'Anna (2020), respectively.

effect of participating in the treatment.[15] The aggregated treatment effect represents the ATT experienced by all ever-treated units, which shares the same interpretation as the ATT in the canonical DiD setup (Callaway and Sant'Anna 2020).

In column (1), never-treated apartments are used as a control group. Also, the entire apartments, some of which are newly built or demolished during the sample period, are used for the analysis as long as it has observations for more than 50% of the sample period.[16] The estimated coefficient indicate that the individual pricing reduces per unit food waste quantity by 19 percent.[17] The estimate in column (1) is likely to be attenuated given the measurement error that caused unbalanced panel.

In column (2), I repeated column (1) using the balanced sample. As expected, the magnitude gets larger at 26 percent reduction in per unit food waste quantity after the treatment. In column (3), the same balanced panel as column (2) is used, but I include not-yet-treated observations as an additional control group. As the smaller standard error in column (3) suggests, increasing the control group size improve precision. At the same time, the two point estimates are almost identical. This alleviates a potential concern that the never-treated apartments might not be a good comparison group to the ever-treated apartments as shown in Table **??**.

Finally, in column (4), I present $\hat{\beta}$ from equation (3.3). Note that the number of observations are larger in column (4) when compared to columns (2) and (3), though the same balanced panel has been used for the analysis. It is because in column (4), the always-treated apartments are not dropped and served as a control group. As Goodman-Bacon (2018) has pointed out using always treated observations as a control gruop could bias the treatment effect estimate and the estimate in column (4) suggests that the magnitude of bias in this application is

[15] The first and second parts correspond to $\theta_{sel}(\tilde{g})$ and θ^{O}_{sel}, respectively from Callaway and Sant'Anna (2020). The estimates in Table 3.2 are thus θ^{O}_{sel}.

[16] In some cases, billing information is missing but I was not able to confirm whether it was random error or not.

[17] Since the magnitude of the change is large, I exponentiate the coefficient: $e^{-0.21} - 1 = -0.189$.

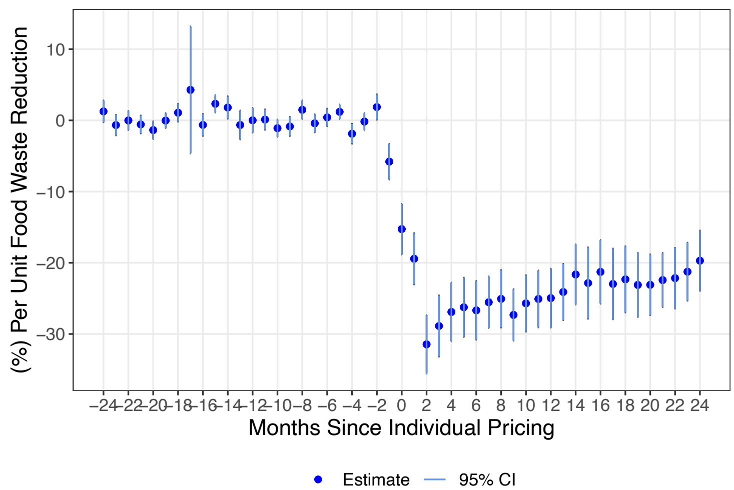


Figure 3.3: The Effect of the Individual Pricing on Waste Quantity

substantial.

In Figure 3.3, I present an event-study style graph for 18 months before and after the treatment date. These estimates are produced by aggregating $ATT(g,t)$ in equation (3.4) for each event time $e = t - g$. The figure shows that before the individual pricing, the treatment effect is almost constantly zero, suggesting that there is no pre-trend. Once the individual pricing policy kicks in, the policy effect is about 20% reduction in per unit food waste quantity and the effect grows to 30-40% as time elapses.

In Figure **??**, I repeat the same plot as Figure 3.3 with ever-treated apartments only. Here, the identifying variation comes from the timing of the treatment and as early vs. late treated apartments are more alike than ever vs. never treated apartments, this could alleviate potential concern over selection into treatment. Observe that while the magnitude of the treatment effect is slightly smaller in months after 15, overall the trajectory is very similar to Figure 3.3, suggesting that the pricing conversion is plausibly exogenous for most households.

It is worth pointing out that the policy effect kicks in a month prior to the treatment. As discussed in section 3.3, a typical apartment goes through a pilot period for 2-4 weeks before starting the individual pricing. During the period, residents are required to use the RFID system although they are still being charged based on the group pricing. Because the system informs users how much waste they have generated, the awareness on food waste is likely to be elevated even in the pilot period. Thus, the 10% reduction in the -1 month is likely to reflect the information treatment effect from the RFID system, separately from the pricing effect.

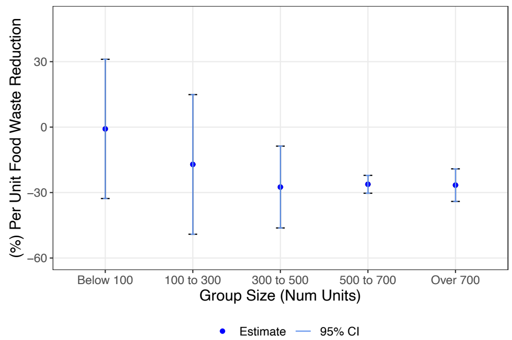
In appendix Figure **??**, I plot the treatment effect by treatment timing where the number on the Y-axis indicate the number of months from Febuary 2014. Thus this figure informs if and how the treatment effect differs conditional on the early vs. late treatment. If there is a selection into the treatment–for instance, those who would benefit the most have signed up for the individual pricing first– then the effect will be larger for early-treated groups. However, the distribution of the treatment effect suggests that there is no such selection effect.

3.6.2 What Determines the Magnitude of the Free-Riding Behavior?

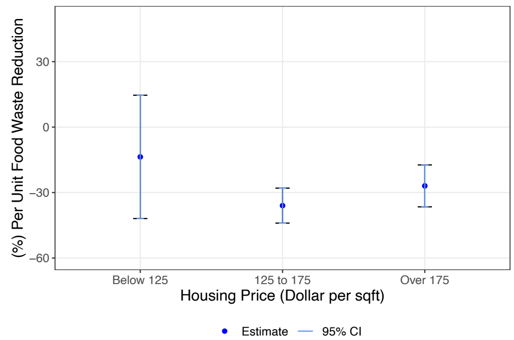
Discussion in sectoin 3.2 indicates that the effect of individual pricing, which elliminates the incentive to free-ride, depends on various factors including the group size, income level, and the tax rate. In this section, I explore heterogeneous policy effects by estimating θ^{O}_{sel}, which corresponds to column (2) of Table 3.2 on a list of subsamples.

The first dimension I consider is the group size. It is one of the most studied relationship in the literature, but most of the empirical works rely on lab experiments, which can have only limited variation in the group size (Ledyard 1995). By exploiting a wide range of apartment complex size in terms of the number of units, I create five subsamples. They are below 100 units, 100 to 300 units, 300 to 500 units, 500 units to 700 units, and above 700 units. The first three group corresponds to roughly 25% of, while the last two are about 12.5% of the entire sample. Figure 3.4 (a) plots the treatment effect for each subgroup.

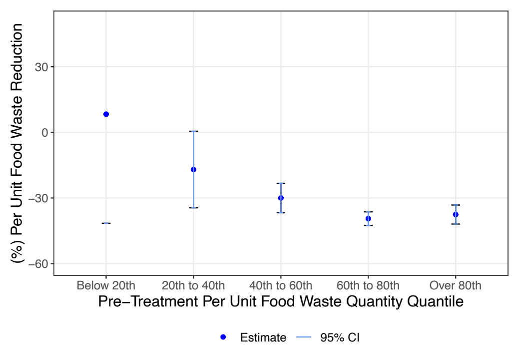
Two observations are worth pointing out. First, the effect size grows over the group size. For instance, for the below 100 units subsample, the point estimate



(a) Treatment Effect by Group Size



(b) Treatment Effect by Wealth



(c) Treatment Effect by Pre-Treatment Food Waste Quantity

Figure 3.4: The Heterogeneous Effect of the Individual Pricing

is near zero, indicating that the per unit waste quantity remained the same even after the individual pricing, whereas for a 300 to 500 units group, the treatment effect was a 20 percent reduction. As the reduction size corresponds to the extent of free-riding behavior before the individual pricing, the result suggests that free-riding gets worse as the group size increases. Second, and more importantly, the group size effect on free-riding plateaus as the group size reaches 300 to 500. The treatment effects for between 500 to 700 and over 700 units groups are almost identical to the 300 to 500 units group at 20%. This suggests that when the group size is "large enough", the factors that could mitigate free-riding behavior such as reputation might not work.

In panel (b), I repeat the same exercise using the per square foot housing price, which can be seen as a proxy for wealth level. As about 20% of the observations do not have transaction price data, I create only three subgroups. The results show that the treatment effect is the largest in the middle-price group. One possible explanation is that the low-price group was not wasting too much food waste even before the individual pricing–presumably not because of the waste fee but because they are more resource-constrained when it comes to grocery purchases. Also, the high-price group might have smaller incentive to respond to the individual pricing that the middle-price group.

Finally, panel (c) shows the heterogeneity based on pre-treatment per unit food waste quantity. I created five subsamples based on the quantile of per unit waste quantity at the apartment level so an apartment belonging to the "below 20%" subsample means that this apartment created smallest amount of waste before the treatment. The results show that the policy effect is larger in apartments with higher pre-treatment per unit waste quantity. This coincides with the free-riding story: namely, if an apartment is experiencing serious free-riding, implementing the individual pricing can have a large reduction in waste quantity.

3.7 Conclusion

In this paper, I study how setting the right pricing incentive can reduce the overusage of public services by exploiting a staggered expansion of individual level unit-based food waste pricing in South Korea. Because food waste quantity is a decreasing function of household effort, per household food waste quantity is a proxy for an average effort level at the apartment complex level. I find that switching from group pricing to individual pricing while holding the per KG price constant can reduce per household food waste quantity by 20%. The finding suggests that under the group pricing, substantial number of households were not putting enough effor and free-riding on other households. The policy effect is driven by apartments with larger number of households, indicating that free-riding is more rampant when a group size is larger. The findings of this paper have important policy implication for public service policies. Specifically, investing in individual household level measurement is costly, but it is worth investing given the reduction in overuse of public services. It can save government finances, reduce environmental externality, and also benefit a large chunk of end users who were effectively subsidizing a subset of free-riders.

Appendix A

Flood History Data

A.1 Background and Construction Procedure

Background

One of the major difficulties in studying the economic impact of floods is that no single repository or database objectively documents past flood events in the US. For instance, the National Weather Service (NWS) Storm Events Database, one of the most widely used databases, documents county-level flood incidents with information on the meteorological environment, monetary damage estimates, and the number of fatalities. A critical feature of these data–which many other flood datasets share–is that they are self-reported by local offices.[1] In other words, two events of the same magnitude could and could not be included in the dataset, depending on the local officer's judgment. This feature creates potential bias from missing data (Gallagher 2013, Gourley et al. 2013).

Earlier studies focused on a single event, relatively well-documented flood type, used the Presidential Disaster Declaration (PDD) floods, or water-depth data from the flood insurance claims data (Bin and Landry 2013, Gallagher 2014, Deryugina 2017, Wagner 2019, Muller and Hopkins 2019). However, these measures do not cover the entire extent of past floods in an objective manner. For instance, focusing on Hurricanes only leaves out devastating inland floods like The Great Flood of 1993. Using PDD floods is also problematic because the declaration, which is the discretion of the president, not only reflects the size of the flood, but also political interests (Reeves 2011). Water-depth from the flood insur-

[1]See Gall et al. (2009) and Saharia et al. (2017a) for review of the existing natural disaster data.

ance claims data is a function of flood size and any property level flood protection measures, thus can be different from the physical size of a flood.

An alternative is to construct flood history data by combining objective records with physical characteristics of floods. Floods, like other natural disasters, can be characterized by their magnitude, which can be measured by the likelihood of exceedance, such as a flood having a 10% chance of being exceeded in any year (Task Committee on Hydrology Handbook of Management Group D of ASCE 1996).[2] The reciprocal of annual exceedance probability, which is also called the recurrence interval, defines an average length of time in years, between the occurrences of floods of a specified magnitude or larger. Therefore, referring to a 10% AEP as a 10-year flood and flood with a 1% AEP as a 100-year flood has become common. To know the quantile, we need to calibrate a distribution.

The newly constructed flood history data have two important advantages. First, they provide an objective and consistent documentation of flood events at the national level for a long period. Second, the measure of flood size–the recurrence interval–is flexible enough to allow for building flood events data of varying size.

Procedure

Following England Jr et al. (2019), I implemented the following steps using USGS discharge data from 3,507 gauge stations in the 27 contiguous ever-disclosed states retrieved using the USGS official R package "dataRetrieval" (Cicco et al. 2018). First, I estimate the parameters of Log-Pearson Type III Distribution using the annual peak discharge data at each gauge station. I keep stations with at least 10 or more annual peak observations following the USGS guideline (England Jr et al. 2019). Also, I use annual peak data until 1990 to reflect flood thresholds around the disclosure policy change.

Second, I estimate the maximum daily flow from the mean daily flow data. Conceptually, maximum daily flow is an appropriate discharge measure to identify

[2]Another common measure is discharge, that is the volume of water flowing at a point for instantaneous time. However, the absolute value of discharge is not useful for comparing floods using different stations, because the baseline discharge is much different from station to station.

Table A.1: Number of MDF Stations vs. IPF Stations in Iowa

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

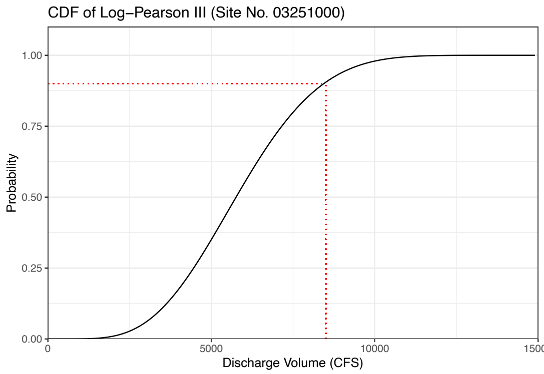
flood events (as opposed to the mean daily flow), because the maximum could be significantly higher than the mean, especially for gauges with a smaller basin area (Chen et al. 2017). However, for most of the stations, the maximum daily flow (or more precisely the instantaneous peak flow which enables calculating maximum daily flow) data have too many missing values. Table A.1 illustrates this point. It compares the number of stations that have records for at least 80% of the days (i.e., 292 days or more) for a given year in Iowa and the number of mean daily flow sites outnumber maximum daily flow sites substantially for most years.

This is problematic because, with many missing observations, flood events will be significantly under-recorded. To solve this problem, I estimate IPF from MDF by constructing a relationship between the ratio of instantaneous peak and the corresponding mean daily flow, with physiographic characteristics of the basin such as the size of the drainage area (Fuller 1913).[3] Third, I compare the estimated daily maximum discharge volumes with fitted distribution to identify their quantile in terms of the annual peak flow. This has an intuitive interpretation. Suppose, the maximum discharge volume for Oct 1, 1995, is at the 99%th quantile of the fitted distribution. It means that this day's discharge volume is large enough to exceed 99 *annual* maximum volumes out of 100 observations and thus interpreted as (once in) a 100-year flood.

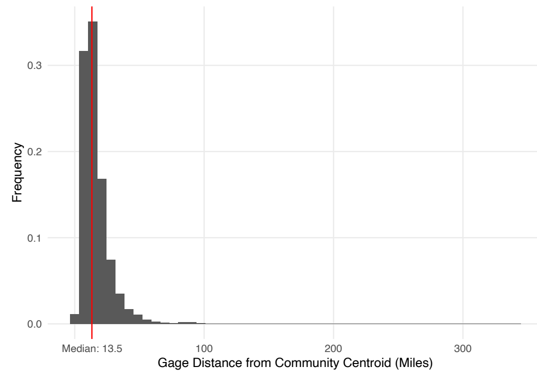
Figure A.1 (a) shows an example of flood frequency analysis. The black solid line represents the CDF of the fitted Log-Pearson III distribution from the USGS site 03251000. If daily discharge volume is 8,500 CFS, it corresponds to the 90th quantile or a 10-year flood.

Note, because the USGS gauge stations rarely cover coastal areas, I add 45

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



(a) Flood Frequency Analysis



(b) Distance between Gauge and Community Centroids

Figure A.1: Flood Frequency Analysis and Gauge Matching

additional NOAA sites in the gauge station data. Zervas (2013) documents the flood threshold for the entire NOAA sites, so I adopt them directly. NOAA water level data are retrieved using the R package “Rnoaa” (Edmund et al. 2014).

Finally, to translate gauge-level flood events to the community-level floods, I match each community to the three nearest gauges using the distance between a centroid of community and gauge stations. Then, I calculated the average flood size for a community using the inverse distance as a weight. Figure A.1 (b) presents the distribution of the average distance between gauges and community centroid. Over 90% of them are within 20 miles with a median distance of 13.5 miles.

Unified Flash Flood Database

The Unified Flash Flood Database (Gourley et al. 2013) is an existing database that is constructed by the procedure outlined above using the USGS gauge records. It is a comprehensive and objective measure of flood events that can present the overall trend of flood events for the contiguous US. I decided not to use this database for a couple of reasons. First, the primary flood threshold used in the unified data is the NWS flood thresholds, which have four categories:

action, minor, moderate, and major.[4] These categories are defined by NWS in collaboration with local stakeholders, which makes comparisons across different stations harder. Second, the data are constructed based on the instantaneous peak flow data thus, a potential bias arises due to the missing records, which can be especially problematic for the years before the 2000s.

A.2 Validation and Summary Statistics

To validate the data, I check the number of the average 10-year flood events over 10 years for the 8,194 communities. These communities are from the 27 ever-disclosed states that are on the Q3 map. By definition, a 10-year flood is going to happen once in a 10-year period around the disclosure policy change date on average. Figure **??** (a) shows that most communities had 0, 1, or 2 10-year floods over the 10 years whereas the average number of the 10-year flood is 0.99.

Figure **??** (b) shows the distribution of flood event size (i.e., recurrence interval), where flood size is truncated at 100 for readability. Note the frequency of low-intensity events dominates the entire distribution. This fact is well-documented in the literature. Jackson (2013) reads "the magnitude of a natural hazard event and its frequency is often depicted as log-normal where the magnitude increases linearly (e.g., 1, 2, 3, . . .) whereas the frequency decreases as an inverse power function (e.g., 1/3, 1/9, 1/81) with increasing magnitude."

[4] Each is defined as minor: minimal or no property damage, but possibly some public threat (e.g., inundation of roads); moderate: some inundation of structures and roads near the stream, evacuations of people and/or transfer of property to higher elevations; major: extensive inundation of structures and roads, significant evacuations of people and/or transfer of property to higher elevations (National Weather Service and Mullusky 2019).

Appendix B

Additional Tables and Figures

Table B.1: Balance Table (Tracts With/Without the SFHA)

	No SFHA		With SFHA		Difference	
Variables	Mean	SE	Mean	SE	Mean	t-stat
Population	3510	15	3492	9.99	-17	-0.2311
Median Inc	168946	811	180352	537	11407	2.3
(%) 65+	12.13	0.063	11.97	0.0395	-0.1557	-0.2662
(%) BA	19.25	0.1421	20.89	0.091	1.64	2.69
(%) Black	20.73	0.2971	9.77	0.1178	-11	-3.26
N Housing Unit	1377	6.31	1408	4.2	32	1.2
(%) Home Age Below 6	0.078	0.0012	0.1296	0.0009	0.0517	4.1
(%) Home Age Above 42	0.3961	0.0028	0.2337	0.0014	-0.1624	-3.8
N Home Age Below 6	92	1.58	160	1.14	68	4.64
N Home Age Above 42	558	5.09	343	2.44	-216	-3.7

Note:
For each variable, I show the mean and standard error for tracts with and without the SFHA border. The last column shows the difference in the mean with the standard error of the difference. I cluster standard error at the state level.

Table B.2: Balance Table (Properties in/out the SFHA)

	No SFHA		With SFHA		Difference	
Variables	Mean	SE	Mean	SE	Mean	t-stat
ITAStaggered Adoption Sample						
log(Price)	12.29	0.0046	12.21	0.0009	0.0734	0.8201
House Age	33.44	0.1803	35.89	0.0354	-2.45	-0.6939
Sqft	3222	14.5	3310	3.15	-87.94	-0.3079
N Stories	1.52	0.0029	1.55	0.0006	-0.0313	-1.21
ITADiff-in-Disc Sample						
log(Price)	11.58	0.01	11.65	0.0052	-0.0746	0
Sqft$	1868	16.77	1919	7.96	-51.24	0

Note:
Note: For each variable, I show the mean and standard error for properties inside and outside of the SFHA. The last column shows the difference in the mean with the standard error of the difference. I cluster standard error at the state level.

Table B.3: Effect of Disclosure Requirement on Demographics (Diff-in-Disc)

	(1)	(2)	(3)
SFHA × Disc	−139.8	−2.0	1214.1
	(27.4)	(.9)	(1676.5)
D.V	Population	(%) BA	Median Income
Avg D.V.	1,182	24.7	68,185
Bandwidth	1KM	1KM	1KM
Num. obs.	40252	40202	40232
Num. groups: comm_id	2300	2300	2300

Note: This table is produced from equation (1.6). Columns (1) to (3) are estimated using the decennial census block group data right before and right after the disclosure policy change. All standard errors are clustered at the state level.

Table B.4: Effect of Discosure Requirement on Flood Insurance Policy Counts

	(1)	(2)
Disclosure	−.022	−.031
	(.012)	(.017)
Year FE	X	X
Community FE	X	X
Sample	Above Median SFHA	Below Median SFHA
Num. obs.	37610	37620

Note: This table is produced by regressing the disclosure dummy from equation (1.5) on the inverse hyperbolic sine transformed policy counts per community. Columns (1) and (2) are estimated using communities above and below the median proportion of the SFHA level. All standard errors are clustered at the state level.

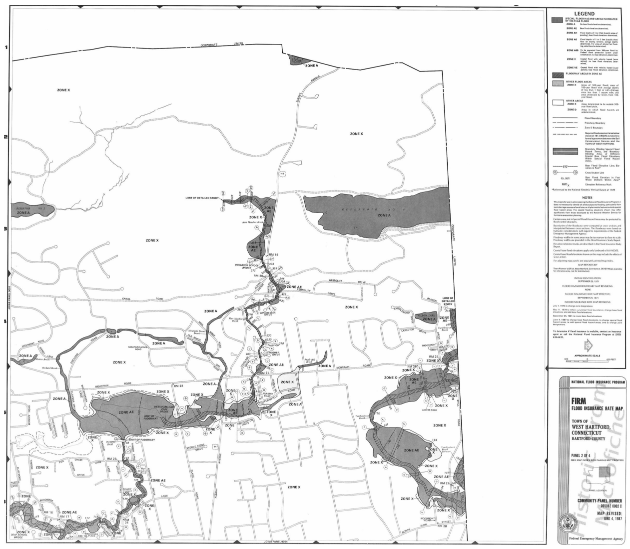


Figure B.1: Sample Flood Insurance Rate Map (West Hartford, CT)

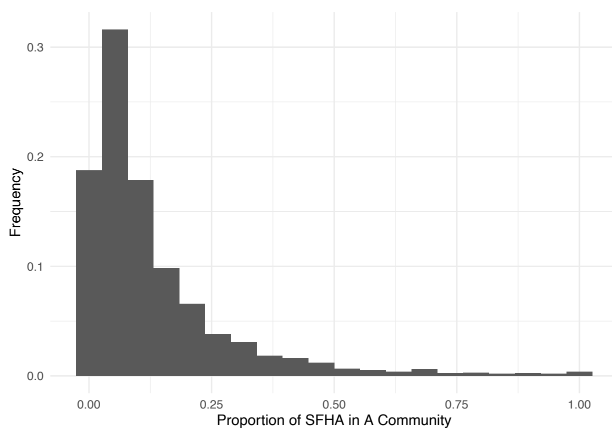


Figure B.2: Histogram of the Proportion of the SFHA for Each Community

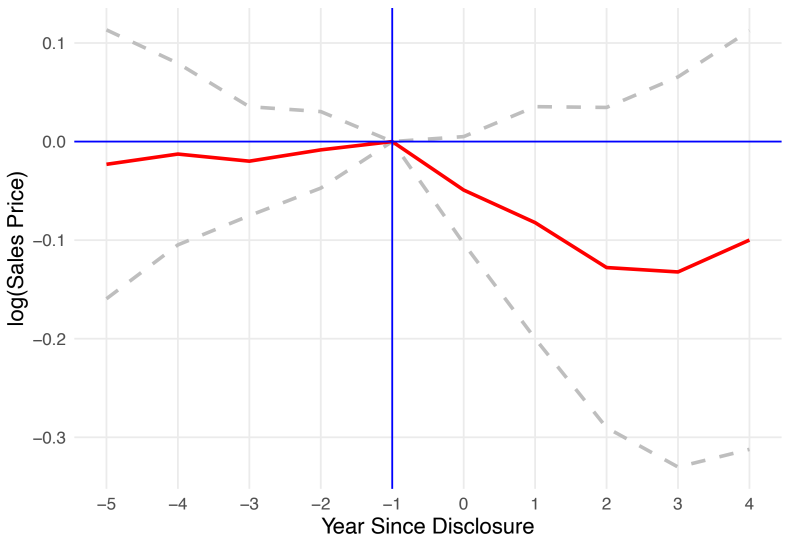


Figure B.3: Effect of Disclosure on Housing Price (Yearly)

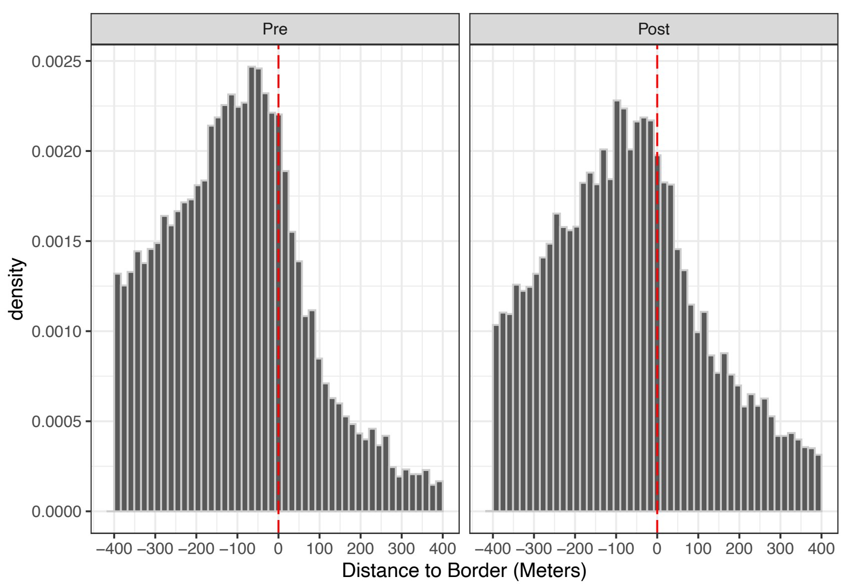
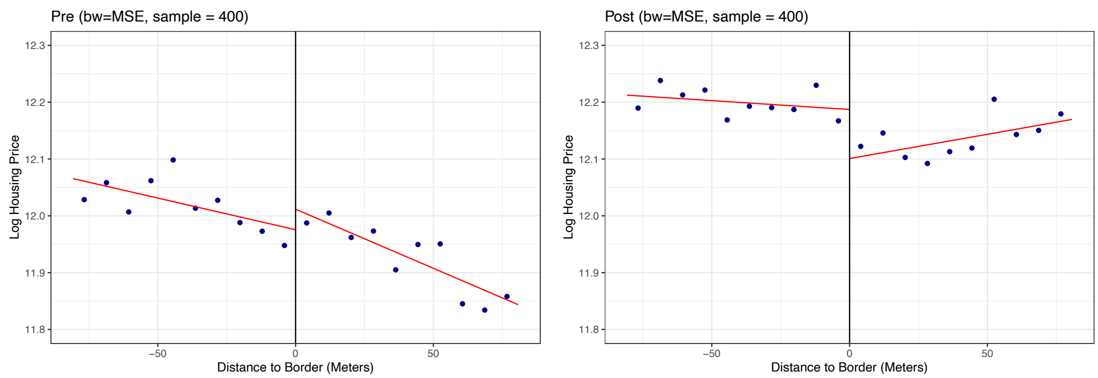


Figure B.4: Density of Houses Regarding Distance to the SFHA Border



(a) Pre (b) Post

Figure B.5: The Effect of the Disclosure Requirement on Flood Damage

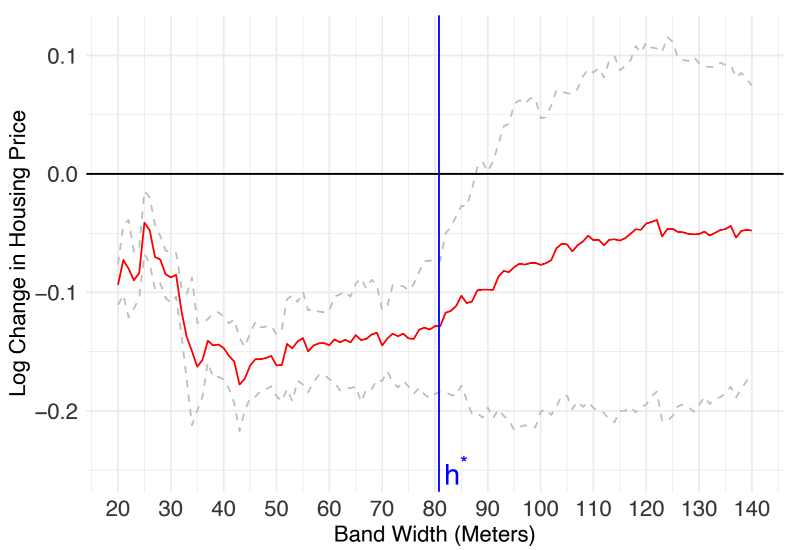


Figure B.6: The Effect of the Disclosure Requirement on Housing Price for Different Bandwidth

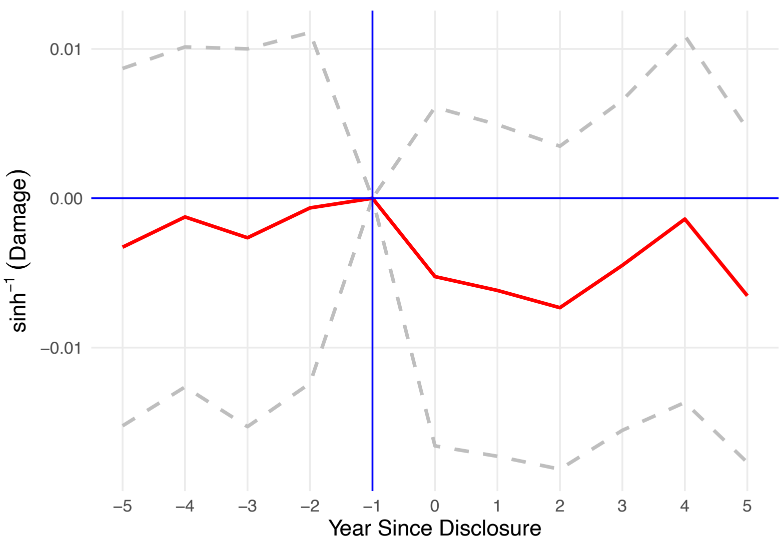


Figure B.7: The Effect of the Disclosure Requirement on Flood Damage

REFERENCES

Acuff, K., and D. T. Kaffine. 2013. Greenhouse gas emissions, waste and recycling policy. Journal of Environmental Economics and Management 65:74–86.

Aldy, J. E., and R. Zeckhauser. 2020. Three Prongs for Prudent Climate Policy. HKS Faculty Research Working Paper Series RWP 20-009.

Allers, M. A., and C. Hoeben. 2010. Effects of Unit-Based Garbage Pricing: A Differences-in-Differences Approach. Environmental and Resource Economics 45:405–428.

Andersson, J. J. 2019. Carbon Taxes and CO2 Emissions: Sweden as a Case Study. American Economic Journal: Economic Policy 11:1–30.

Auffhammer, M. 2018. Quantifying Economic Damages from Climate Change. Journal of Economic Perspectives 32:33–52.

Bajželj, B., K. S. Richards, J. M. Allwood, P. Smith, J. S. Dennis, E. Curmi, and C. A. Gilligan. 2014. Importance of food-demand management for climate mitigation. Nature Climate Change 4:924–929.

Baker, A. C., D. F. Larcker, and C. C. Y. Wang. 2021. How Much Should We Trust Staggered Differences-In-Differences Estimates?59.

Bakkensen, L. A., and L. Ma. 2019. Sorting Over Flood Risk and Implications for Policy Reform. Working Paper:66.

Bakkensen, L., and L. Barrage. 2017. Flood Risk Belief Heterogeneity and Coastal Home Price Dynamics: Going Under Water? National Bureau of Economic Research, Cambridge, MA.

Baland, J.-M., and J.-P. Platteau. 2003. Economics of Common Property Management Regimes. Pages 127–190 *in* Handbook of Environmental Economics.

Elsevier.

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

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

Beltrán, A., D. Maddison, and R. J. R. Elliott. 2018. Is Flood Risk Capitalised Into Property Values? Ecological Economics 146:668–685.

Berlemann, M. 2016. Does hurricane risk affect individual well-being? Empirical evidence on the indirect effects of natural disasters. Ecological Economics 124:99–113.

Bernstein, A., M. T. Gustafson, and R. Lewis. 2019. Disaster on the horizon: The price effect of sea level rise. Journal of Financial Economics 134:253–272.

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

Bloom, E., L. F. Grimsley, C. Pehrson, J. Lewis, and L. Larsson. 2009. Molds and mycotoxins in dust from water-damaged homes in New Orleans after hurricane Katrina. Indoor Air 19:153–158.

Bollinger, B., P. Leslie, and A. Sorensen. 2011. Calorie Posting in Chain Restaurants. American Economic Journal: Economic Policy 3:91–128.

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

Calcott, P., and M. Walls. 2000. Can Downstream Waste Disposal Policies Encourage Upstream "Design for Environment"? American Economic Review 90:9.

Callaway, B., and P. H. C. Sant'Anna. 2020. Difference in Differences with Multiple Time Periods:37.

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

Carpenter, J. P. 2007. Punishing free-riders: How group size affects mutual monitoring and the provision of public goods. Games and Economic Behavior 60:31–51.

Cattaneo, M. D., N. Idrobo, and R. Titiunik. 2019. A Practical Introduction to Regression Discontinuity Designs: Foundations. Cambridge University Press.

CBO. 2016. Potential Increases in Hurricane Damage in the United States: Implications for the Federal Budget. Congressional Budget Office:46.

Changnon, S. A., R. A. Pielke Jr, D. Changnon, R. T. Sylves, and R. Pulwarty. 2000. Human Factors Explain the Increased Losses from Weather and Climate Extremes. Bulletin of the American Meteorological Society 81:437–442.

Chen, B., W. F. Krajewski, F. Liu, W. Fang, and Z. Xu. 2017. Estimating instantaneous peak flow from mean daily flow. Hydrology Research 48:1474–1488.

Chivers, J., and N. E. Flores. 2002. Market Failure in Information: The National Flood Insurance Program. Land Economics 78:515–521.

Cicco, L. A. D., D. Lorenz, R. M. Hirsch, and W. Watkins. 2018. dataRetrieval: R packages for discovering and retrieving water data available from U.S. Federal hydrologic web services. U.S. Geological Survey, Reston, VA.

Collenteur, R. A., H. de Moel, B. Jongman, and G. Di Baldassarre. 2015. The failed-levee effect: Do societies learn from flood disasters? Natural Hazards

76:373–388.

Committee on Financial Services. 2017. Report: 21st Century Flood Reform Act. House of Representatives.

Constible, J. 2018. The Emerging Public Health Consequences of Hurricane Harvey.

Davies, B., and M. Doble. 2004. The Development and Implementation of a Landfill Tax in the UK. *in* Addressing the Economics of Waste. OECD.

Davis, L. W. 2008. The Effect of Driving Restrictions on Air Quality in Mexico City. Journal of Political Economy 116:38–81.

Davis, L. W., and L. Kilian. 2011. Estimating the effect of a gasoline tax on carbon emissions. Journal of Applied Econometrics 26:1187–1214.

Dellavigna, S., and J. M. Pollet. 2009. Investor Inattention and Friday Earnings Announcements. The Journal of Finance 64:709–749.

Deryugina, T. 2017. The Fiscal Cost of Hurricanes: Disaster Aid versus Social Insurance. American Economic Journal: Economic Policy 9:168–198.

DHS Office of Inspector General. 2017. FEMA Needs to Improve Management of Its Flood Mapping Programs. Department of Homeland Security.

Diederich, J., T. Goeschl, and I. Waichman. 2016. Group size and the (in)efficiency of pure public good provision. European Economic Review 85:272–287.

Dranove, D., and G. Z. Jin. 2010. Quality Disclosure and Certification: Theory and Practice. Journal of Economic Literature 48:935–963.

Edmund, H., S. Chamberlain, and K. Ram. 2014. Rnoaa: NOAA climate data from R.

Elinder, M., S. Escobar, and I. Petré. 2017. Consequences of a price incentive on free riding and electric energy consumption. Proceedings of the National Academy of Sciences 114:3091–3096.

England Jr, J. F., T. A. Cohn, B. A. Faber, J. R. Stedinger, W. O. Thomas Jr, A. G. Veilleux, J. E. Kiang, and R. R. Mason Jr. 2019. Guidelines for determining flood flow frequency—Bulletin 17C. US Geological Survey.

EPA. 2016. Inventory of US Greenhouse Gas Emissions and Sinks: 1990-2014. EPA.

Epple, D., and G. J. Platt. 1998. Equilibrium and Local Redistribution in an Urban Economy when Households Differ in both Preferences and Incomes. Journal of Urban Economics 43:23–51.

FEMA. 1996. Q3 Flood Data Users Guide, Draft. FEMA.

FEMA. 2005. National Flood Insurance Program (NFIP) Floodplain Management Requirements: A Study Guide and Desk Reference for Local Officials. FEMA.

FEMA. 2014. Transaction Record Reporting and Processing (TRRP) Plan. FEMA.

Field, C. B., V. Barros, T. F. Stocker, and Q. Dahe (Eds.). 2012. Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation: Special Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge.

Figlio, D. N., and M. E. Lucas. 2004. What's in a Grade? School Report Cards and the Housing Market. American Economic Review 94:591–604.

Flavelle, C. 2017. The Nightmare Scenario for Florida's Coastal Homeowners. Bloomberg.

Foley, J. A., N. Ramankutty, K. A. Brauman, E. S. Cassidy, J. S. Gerber, M. Johnston, N. D. Mueller, C. O'Connell, D. K. Ray, P. C. West, C. Balzer, E. M. Bennett, S. R. Carpenter, J. Hill, C. Monfreda, S. Polasky, J. Rockström,

J. Sheehan, S. Siebert, D. Tilman, and D. P. M. Zaks. 2011. Solutions for a cultivated planet. Nature 478:337–342.

Fowlie, M. L. 2009. Incomplete Environmental Regulation, Imperfect Competition, and Emissions Leakage. American Economic Journal: Economic Policy 1:72–112.

Fuller, W. E. 1913. Flood Flows. American Society of Civil Engineers.

Fullerton, D., and T. C. Kinnaman. 1995. Garbage, Recycling, and Illicit Burning or Dumping. Journal of Environmental Economics and Management 29:78–91.

Fullerton, D., and T. C. Kinnaman. 1996. Household Responses to Pricing Garbage by the Bag. American Economic Review 86:971–984.

Fullerton, D., A. Leicester, and S. Smith. 2008. Environmental Taxes. NBER working paper 14197:1–61.

Gall, M., K. A. Borden, and S. L. Cutter. 2009. When Do Losses Count?: Six Fallacies of Natural Hazards Loss Data. Bulletin of the American Meteorological Society 90:799–810.

Gall, M., K. A. Borden, C. T. Emrich, and S. L. Cutter. 2011. The Unsustainable Trend of Natural Hazard Losses in the United States. Sustainability 3:2157–2181.

Gallagher, J. 2013. Learning About an Infrequent Event: Evidence from Flood Insurance Take-Up in the US (Online Appendix). SSRN Electronic Journal.

Gallagher, J. 2014. Learning about an Infrequent Event: Evidence from Flood Insurance Take-Up in the United States. American Economic Journal: Applied Economics 6:206–233.

GAO. 2013. Flood Insurance: More Information Needed on Subsidized Properties.

Gayer, T., J. T. Hamilton, and W. K. Viscusi. 2000. Private Values of Risk Tradeoffs at Superfund Sites: Housing Market Evidence on Learning about Risk. The Review of Economics and Statistics 82:439–451.

GGC. 2010. Initiative for Less Food Waste. Green Growth Committee.

GGIRC. 2015. 2014 national greenhouse gas inventory. Greenhouse Gas Inventory; Research Center.

Ghanbari, M., M. Arabi, J. Obeysekera, and W. Sweet. 2019. A Coherent Statistical Model for Coastal Flood Frequency Analysis Under Nonstationary Sea Level Conditions. Earth's Future 7:162–177.

Goeree, J. K., C. A. Holt, and S. K. Laury. 2002. Private costs and public benefits: Unraveling the effects of altruism and noisy behavior. Journal of Public Economics 83:255–276.

Goodman-Bacon, A. 2018. Difference in Differences with Variation in Treatment Timing. National Bureau of Economic Research, Cambridge, MA.

Gourley, J. J., Y. Hong, Zach ary L. Flamig, Ami Arthur, Robert Clark, Martin Calianno, Isabelle Ruin, Terry Ortel, Mich ael E. Wieczorek, and Kirstetter. 2013. A Unified Flash Flood Database over the US. AMERICAN METEOROLOGICAL SOCIETY:799–805.

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

Grossman, S. J. 1981. The Informational Role of Warranties and Private Disclosure about Product Quality. The Journal of Law and Economics 24:461–483.

Gwangju Metropolitan City. 2016. Cutting Down Food Waste by 30%.

Hallstrom, D. G., and V. K. Smith. 2005. Market responses to hurricanes. Journal of Environmental Economics and Management 50:541–561.

Harbaugh, R., J. W. Maxwell, and B. Roussillon. 2011. Label Confusion: The

Groucho Effect of Uncertain Standards. Management Science 57:1512–1527.

Hiç, C., P. Pradhan, D. Rybski, and J. P. Kropp. 2016. Food Surplus and Its Climate Burdens. Environmental Science & Technology 50:4269–4277.

Hino, M., and M. Burke. 2020. Does information about climate risk affect property values? NBER Working Paper:1–30.

Hiraishi, T., B. Nyenzi, J. D. Miguez, A. D. Bhide, and R. Pipatti. 2000. Waste (Ch5). Page 32 *in* Good Practice Guidance and Uncertainty Management in National Greenhouse Gas Inventories. IPCC.

Horn, D. P., and J. T. Brown. 2018. Introduction to the National Flood Insurance Program (NFIP). Pages 1–29. Congressional Research Service.

IPCC. 2014. Climate Change 2014 Mitigation of Climate Change: Working Group III Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge.

IPCC. 2018. Global warming of 1.5°C.

Isaac, R. M., and J. M. Walker. 1988. Group Size Effects in Public Goods Provision: The Voluntary Contributions Mechanism. The Quarterly Journal of Economics 103:179.

Isaac, R. M., J. M. Walker, and W. Williams. 1994. Group size and the voluntary provision of public goods. Journal of Public Economics 54:1–36.

Ito, K., and S. Zhang. 2020. Reforming Inefficient Energy Pricing: Evidence from China. Pages 1–33. National Bureau of Economic Research, Cambridge, MA.

Jack, B. K., S. Jayachandran, and S. Rao. 2020. Environmental externalities and free-riding in the household. Working Paper:41.

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

Jessoe, K., M. Papineau, and D. Rapson. 2020. Utilities Included: Split Incentives in Commercial Electricity Contracts. The Energy Journal 41.

Jin, G. Z., and P. Leslie. 2003. The Effect of Information on Product Quality: Evidence from Restaurant Hygiene Grade Cards. QUARTERLY JOURNAL OF ECONOMICS 118:409–451.

Kahn, M. E. 2005. The Death Toll from Natural Disasters: The Role of Income, Geography, and Institutions. THE REVIEW OF ECONOMICS AND STATISTICS 87:271–284.

Katare, B., D. Serebrennikov, H. H. Wang, and M. Wetzstein. 2017. Social-Optimal Household Food Waste: Taxes and Government Incentives. American Journal of Agricultural Economics 99:499–509.

Kaza, S., L. Yao, P. Bhada-Tata, and F. V. Woerden. 2018. What a Waste 2.0: A Global Snapshot of Solid Waste Management to 2050. World Bank Group.

Kim, B., T. Park, and K. Choi. 2010. A study on unit based food waste tax and other reduction policies. Pages 1–389. Ministry of Environment.

Kinnaman, T. C. 2006. Policy Watch: Examining the Justification for Residential Recycling. Journal of Economic Perspectives 20:219–232.

Kline, P. 2012. The Impact of Juvenile Curfew Laws on Arrests of Youth and Adults. American Law and Economics Review 14:44–67.

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

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

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

Lahey, K. E., and D. A. Redle. 1997. The Ohio Experience: The Effectiveness of Mandatory Real Estate Disclosure Forms. Real Estate Law Journal 25:319–344.

LARSON, L. W. 1996. The Great USA Flood of 1993. Presented at IAHS Conference, Anaheim, CA.

Ledyard, J. O. 1995. Public Goods: A Survey of Experimental Research. The Handbook of Experimental Economics:111–194.

Lee, Y., and M. Choi. 2015. A Study on the Lmprovement of the Apartment House Management. Journal of the Korean Cadastre Information Association 17:193–206.

Lefcoe, G. 2004. PROPERTY CONDITION DISCLOSURE FORMS: HOW THE REAL ESTATE INDUSTRY EASED THE TRANSITION FROM CAVEAT EMPTOR TO "SELLER TELL ALL". Real Property, Probate and Trust Journal 39:193–250.

Levinson, A., and S. Niemann. 2004. Energy use by apartment tenants when landlords pay for utilities. Resource and Energy Economics 26:51–75.

Levitt, S. D., and J. A. List. 2007. What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World? Journal of Economic Perspectives 21:153–174.

Liao, K.-H. 2014. From flood control to flood adaptation: A case study on the Lower Green River Valley and the City of Kent in King County, Washington. Natural Hazards 71:723–750.

MacKinnon, J. G., and L. Magee. 1990. Transforming the Dependent Variable in Regression Models. International Economic Review 31:315.

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

Marshall, M. N., P. G. Shekelle, S. Leatherman, and R. H. Brook. 2000. The

Public Release of Performance Data: What Do We Expect to Gain? A Review of the Evidence. Journal of the American Medical Association 283:1866–74.

Miao, Q., and D. Popp. 2014. Necessity as the mother of invention: Innovative responses to natural disasters. Journal of Environmental Economics and Management 68:280–295.

Michel-Kerjan, E. O. 2010. Catastrophe Economics: The National Flood Insurance Program. Journal of Economic Perspectives 24:165–186.

Milgrom, P. R. 1981. Good News and Bad News: Representation Theorems and Applications. The Bell Journal of Economics 12:380.

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

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

Muller, N. Z., and C. A. Hopkins. 2019. HURRICANE KATRINA FLOODS NEW JERSEY: THE ROLE OF INFORMATION IN THE MARKET RESPONSE TO FLOOD RISK. NBER working paper w25984:1–96.

Myers, E., S. Puller, and J. West. 2019. Effects of Mandatory Energy Efficiency Disclosure in Housing Markets. Page w26436. National Bureau of Economic Research, Cambridge, MA.

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

National Weather Service, and M. Mullusky. 2019. Natinal Weather Service Manual 10-950: Definitions and General Terminology. NATIONAL WEATHER SERVICE.

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

OECD. 2000. STRATEGIC WASTE PREVENTION: OECD Reference Manual.

OECD.

OECD. 2007. Instrument Mixes Addressing Household Waste. OECD.

OECD. 2013. New Modes of Water Supply and Sanitation Management and Emerging Business Models. OECD.

Ortiz-Bobea, A., and J. Tack. 2018. Is another genetic revolution needed to offset climate change impacts for US maize yields? Environmental Research Letters 13:124009.

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

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

Pinter, N. 2005. One Step Forward, Two Steps Back on U.S. Floodplains. Science 308:207–208.

Pinter, N., F. Huthoff, J. Dierauer, J. W. F. Remo, and A. Damptz. 2016. Modeling residual flood risk behind levees, Upper Mississippi River, USA. Environmental Science & Policy 58:131–140.

Poore, J., and T. Nemecek. 2018. Reducing food's environmental impacts through producers and consumers. Science 360:987–992.

Pope, D. G. 2009. Reacting to rankings: Evidence from "America's Best Hospitals". Journal of Health Economics:12.

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

Pretis, F. 2019. Does a Carbon Tax Reduce CO2 Emissions? Evidence From

British Columbia. SSRN Electronic Journal.

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

Rehdanz, K., H. Welsch, D. Narita, and T. Okubo. 2015. Well-being effects of a major natural disaster: The case of Fukushima. Journal of Economic Behavior & Organization 116:500–517.

Repetto, R. C., R. C. Dower, R. Jenkins, and J. Geoghegan (Eds.). 1992. Green fees: How a tax shift can work for the environment and the economy. World Resources Institute, Washington, DC.

Reschovsky, J. D., and S. E. Stone. 1994. Market Incentives to Encourage Household Waste Recycling: Paying for What You Throw Away. Journal of Policy Analysis and Management 13:120–139.

Roberts, F. Y. 2006. Off-Site Conditions and Disclosure Duties: Drawing the Line at the Property Line. BRIGHAM YOUNG UNIVERSITY LAW REVIEW 957:39.

Saharia, M., P.-E. Kirstetter, H. Vergara, J. J. Gourley, and Y. Hong. 2017a. Characterization of floods in the United States. Journal of Hydrology 548:524–535.

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

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

Satija, N., K. Collier, and A. Shaw. 2017. Houston officials let developers build homes inside reservoirs. But no one warned buyers. The Texas Tribune.

Sieg, H., V. K. Smith, H. S. Banzhaf, and R. Walsh. 2004. ESTIMATING THE

GENERAL EQUILIBRIUM BENEFITS OF LARGE CHANGES IN SPATIALLY DELINEATED PUBLIC GOODS*. International Economic Review 45:1047–1077.

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

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

Tarlock, A. D. 2012. United States Flood Control Policy: The Incomplete Transition From the Illusion of Total Protection to Risk Management. Duke Environmental Law & Policy Forum 23:151–183.

Task Committee on Hydrology Handbook of Management Group D of ASCE. 1996. Floods. *in* Hydrology Handbook. Second Edition. American Society of Civil Engineers Publications, Reston, VA.

Taylor, R. L. C. 2019. Bag leakage: The effect of disposable carryout bag regulations on unregulated bags. Journal of Environmental Economics and Management 93:254–271.

TEXAS PROPERTY CODE. 2019, September..

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

Wagner, K. 2019. Adaptation and Adverse Selection in Markets for Natural Disaster Insurance. SSRN Electronic Journal.

Walls, M., and K. Palmer. 2001. Upstream Pollution, Downstream Waste Disposal, and the Design of Comprehensive Environmental Policies. Journal of Environmental Economics and Management 41:94–108.

Washburn, R. M. 1995. Residential Real Estate Condition Disclosure Legislation.

DePaul L. Rev. 44:381–459.

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

White, A. M., and C. L. Mansfield. 2002. Literacy and Contract. Stanford Law & Policy Review 13:233–266.

Wooldridge, J. M. 2006. Introductory Econometrics: A Modern Approach. 3rd editions. Thomson, New York.

Zervas, C. 2013. EXTREME WATER LEVELS OF THE UNITED STATES 1893-2010. National Oceanic; Atmospheric Administration, Silver Spring, Maryland.

Zhang, X., and F. Zhu. 2011. Group Size and Incentives to Contribute: A Natural Experiment at Chinese Wikipedia. American Economic Review 101:1601–1615.