Essays in Infrastructure Economics

By

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Contents

Abstract

Chapter 1 examines the long-term economic impact of levee systems, which mitigate flood damage, on the local economy and optimal policy targeting for levee systems. First, I estimate the causal effects of levee systems using a standard difference-in-differences approach. Second, I develop a framework for computing ex-post optimal treatment assignments using a changes-in-changes approach and a statistical treatment rule. I find that the levee systems decreased the flood risks in counties with more enormous flood risks. From the optimal targeting exercise, I show that while there could be a better treatment allocation, the externality of levee construction makes it challenging to achieve the optimal allocation. Moreover, I show that an additional 20% over the current total investment in U.S. levee systems is necessary to reduce the number of disasters by at least one per decade.

Chapter 2 examines the causal impact of the levee investment projects under the American Recovery and Reinvestment Act (ARRA) of 2009 on employment at the county level. To address endogeneity, I use a new instrument: the length of levees. As the length of levees is a persistent stock variable and is unrelated to the magnitude of the recession, it constitutes an ideal instrument to measure the effect of levee projects. I find that a county's receipt of a marginal \$100,000 investment in levees resulted in an additional 4.2 job-years, 1.8 of which

were in the construction sector. I also find that the projects showed short implementation lags and seasonal cycles in employment gains.

Chapter 3 examines the impact of Hurricanes Katrina and Rita on firms' bidding behavior using data on highway procurement auctions in Louisiana. Using a high-dimensional sparse regression technique to account for auction-specific observable heterogeneity and a difference-in-differences approach, I find that parishes affected by the two hurricanes faced higher costs and a less competitive environment than those not affected. I use a standard first-price auction model with endogenous entry to analyze the mechanism and simulate the counterfactual scenarios. I show that (i) the government would save 10% on procurement if the hurricanes did not occur, and (ii) subsidizing firms' bids may not be an optimal post-hurricane strategy.

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Chapter 1

Effects of Levee Systems and Optimal Policy Targeting

1.1 Introduction

Floods cause damage to people and property, and the expected losses may become more severe with climate change. Levees are a type of infrastructure that mitigates such damage. While an abundance of research has emphasized the importance of physical infrastructure,[1] there has been little empirical analysis of levee systems. How do levees affect the local economy in the long term? How appropriate is the treatment assignment in the past, given the recent surge of floods? How much will it cost to improve a misallocated condition?

This paper examines the long-term impacts of levee construction in the United States on local economies, whether past treatment assignments in levees have been optimally targeted,

[1] For example, agricultural dams: Duflo and Pande (2007) , multipurpose dams: Kline and Moretti (2013), electricity: Dinkelman (2011), highways: Duranton and Turner (2012), railways: Donaldson (2018), and BRT: Tsivanidis (2019)

and how much it will take to mitigate a specific level of floods. To identify the causal effects of levee systems, I need to find variation in levees and an exogenous shock to levee construction. I use survey data on river improvement called the 308 reports, a foundation for basin development in the United States. The 308 Program selected 190 rivers from the whole United States river systems based on water trade, irrigation, and power development. Using counties along such rivers is desirable to control for unobserved characteristics such as productivity and amenities. The federal government performed a cost-benefit analysis to select projects in each report. The federal government prioritized projects which (i) included more populated areas, (ii) experienced recent severe floods, and (iii) had the potential for development. As a result of the selection process, including a random factor such as recent flood records, there are variations in levee investment among these counties. I use such variation to identify the causal effect of levee systems. The Flood Control Act in 1936 was enacted based on the survey result, and the federal government started levee projects around the U.S. I use this event as an exogenous shock for building the U.S. levee systems.

Since the federal government used the cost-benefit analysis to select the projects, there is an imbalance between treatment and control counties regarding local economic indicators in the pre-treatment period. To adjust the imbalance, I use a propensity score reweighting method. After reweighting, the balance improved dramatically. Point estimates from my main specifications suggest four points. First, although one may conjecture that levees increase the population because they reduce the risk of disasters, I cannot find clear evidence of the point. Second, while a theoretical implication about levee construction in Kousky et al. (2006) shows that government protection such as levees could increase land value, my estimate is inconsistent with the prediction, though the confidence interval is wide. Third, while the effect of levees on the cumulative number of disasters is positive in leveed counties during the main construction periods (from 1940 to 1970), the estimate becomes negative after the main construction period (from 1970 to 2000), though the estimates are statistically insignificant. Fourth, I cannot obtain clear evidence that the presence of levees decreases severe disasters, though most of the point estimates are negative.

I check the robustness of my results in two ways. The first method is a placebo analysis. I estimate the "effect" of the levee on 1900-1930 changes in outcomes. If I observe a different trend in leveed and non-leveed counties in the decades leading up to the policy intervention conditional on the reweighting process, this would be suggestive evidence of selection bias. I find that all outcome variables have only minor insignificant estimated differences. The second method is to examine whether a confounding factor affects my result. In particular, I check whether the impact of the Mississippi river in 1927 biases my result. Hornbeck and Naidu (2014) shows that the Great Mississippi Flood in 1927 caused migration and increased firm capital. This event might affect my analysis. To check this possibility, I add a dummy variable for counties impacted by the Great Mississippi Flood in 1927 and then reexamine the same difference-in-differences approach. After controlling for the effect of the disaster, I find that my estimates are similar to the main result.

The second goal of this paper is to examine whether past treatment allocations were optimal in light of recent floods and provide a policy proposal about the cost of improving a misallocated condition. I analyze the counterfactual scenario of what would have happened to counties with levees if they did not have levees and what would have happened to counties without levees if they did have levees. To obtain the counterfactual distribution, I use the changes-in-changes (CIC) approach proposed by Athey and Imbens (2006) (henceforth 'A&I'), which provides a generalization of the DID approach to the entire distribution of potential outcomes. A&I present a method to recover the hypothetical distribution of the treated group in the post-treatment period without treatment, using the difference in the untreated group's distribution functions before and after treatment and the distribution function of the treated group before treatment. This approach can also recover the counterfactual distribution function of treatment effects on the untreated. Moreover, A&I proposed three CIC approaches when the outcome variable is discrete. The approaches are (i) partial identification, (ii) using a conditional independence assumption for point identification, (iii) using covariates for point identification. Since my outcome variable, the number of disasters, is discrete, I can use these approaches for my analysis.

To examine the optimal treatment allocation, I need to estimate the value of each county from the counterfactual distribution obtained by the CIC approach. To do so, I propose a framework for connecting the statistical decision rule and the counterfactual distribution. Specifically, I discuss two assumptions for estimating each county's effects. The first is that the ranks of unobservable variables such as geographical vulnerability in the prior and posterior of the treatment are the same, called perfect correlation. The second is the condition that counterfactual and posterior observed ranks are the same, called rank invariance. Based on these concepts and the observed rank information of each county, I show at least three cases to derive the outcome value of each county from the distribution. I also discuss how to check whether these assumptions are satisfied or not.

Using observed data, counterfactual values, and assumptions, I can formulate objective functions for allocating levees to mitigate the effect of water-related disasters. Although there are many types of social objectives to assign levees across counties, I set three social goals: decreasing (i) the total number of disasters, (ii) the total population affected by the disasters, and (iii) the total economic effect of the disasters using county-level GDP. Since there are three CIC approaches to identifying the counterfactual distributions, including partial identification, I formulate three objective functions corresponding to each CIC case. Moreover, since each counterfactual value is a random variable, I propose a new framework using an idea from asset pricing theory to consider such randomness. The result of CIC approaches showa that the levees decreased the number of water-related disasters in counties with relatively more disasters in the 1930s, especially in the 75th and 90th percentiles in the treated group.

Based on the three CIC approaches, ranking conditions, and policy objectives on disasters, I find that the matching ratio between the optimal investment counties and the counties that actually built levees is 33% to 63%. One possible reason the percentage is low is that the optimal allocation exercise does not consider the externality of floods. If a county has a levee, the county will decrease the flood risk. However, floodwaters that had previously overflowed could overflow in a different location. A simple exercise shows that the allocation taking into account the spillover effect of floods is close to the government's actual allocation, suggesting that the externality of floods makes it challenging to achieve the optimal allocation.

Moreover, there are two findings from additional analyses. First, after presenting the geographical distribution of the optimal investment, I show that the optimal investment group has (1) fewer farms, more large farm areas, and high farm value, (2) more disasters in 1930. Second, I show that, under the assumption that the levee building construction cost in each county is constant, an additional 20% of the total amount invested so far in levees in the U.S. is necessary to reduce the number of disasters by at least one per decade.

This paper contributes in three main areas. First, it adds to the existing literature on the economics of infrastructure investment, especially in nation-building. Kline and Moretti (2013), for example, study the Tennessee Valley Authority (TVA), including multipurpose dams. Donaldson and Hornbeck (2016) and Hornbeck and Rotemberg (2019) show that the U.S. railroad system yielded benefits. Glaeser and Poterba (2020) review this literature. While many authors have conducted empirical studies on various types of infrastructure development, this is the first study to focus on levees to the best of my knowledge.

This paper also contributes to the literature on climate adaptation, especially flood disaster risk management. International organizations such as the World Bank and governments take various measures to mitigate the flood damage caused by climate change. There are two broad categories of measurements: (1) those that reduce ex-post financial damage, such as flood insurance and bailouts after a disaster, and (2) those that reduce the probability of disaster damage. Many researchers (e.g., Wagner, forthcoming) have examined the first category, providing empirical evidence on the effects of flood insurance. Gregory (2017) and Fu and Gregory (2019) conduct empirical analyses of the Louisiana Road Home rebuilding grant program. In the second category, facilities such as dams and levees mitigate the damage directly. Although these facilities are crucial, there are few empirical studies on the effects of an investment in such infrastructure, especially levees. While Kousky et al. (2006) provide theoretical implications for levee investment to maximize social welfare, this paper offers the first evidence of the effects of levees on the local economy in the long run.[2]

[2] Shirai (2021) examines the causal impact of levee investment projects under the American Recovery and Reinvestment Act of 2009 on employment at the county level. Wang (2021) studies the spillover effects of levee building.

Finally, this paper contributes to the literature on statistical treatment rules in econometrics (e.g., Manski, 2005; Hirano and Porter, 2009; Kitagawa and Tetenov, 2018; Athey and Wager, 2021). There have been few empirical applications for these rules thus far, except those presented as exemplars in theoretical research. Assunção et al. (2019) use this approach to examine optimal environmental targeting in the Amazon rainforest. My paper is the first to apply this empirical approach to infrastructure development and propose a detailed crucial discussion to connect counterfactual distributions and statistical treatment rules.

The rest of the paper is organized as follows: The next section provides background on levee construction in the United States. Section 1.3 introduces the empirical framework for the difference-in-differences approach. Section 1.4 describes the data used, and Section 1.5 provides the results for the first goal and robustness checks. Section 1.6 explains a framework for optimal targeting exercise, and Section 1.7 shows its result. Section 1.8 concludes.

1.2 Background

Levee systems are designed to reduce flood damage. While private companies and households may sometimes provide protection efforts from floods, the government provides the primary protection. The federal government began flood protection efforts in the 20th century. In 1917, Congress appropriated funds for flood control through the Flood Control Act. The Act was the first time Congress allocated a budget for flood protection. In 1927, persistent heavy rains across the Mississippi River basin caused a catastrophic river flood. Following this devastation, the federal government amended the Flood Control Act and authorized the Mississippi River & Tributaries (MR&T) project, creating one of the world's largest levee systems. At that time, federal flood control was limited to the lower Mississippi and the Sacramento rivers (Arnold, 1988) and the construction process took a long time.[3]

The program known as the *308 Program* played a crucial role in selecting and constructing levee systems across the United States (U.S. Congress, 1926). In 1927, Congress instructed the United States Army Corps of Engineers (hereafter USACE) to prepare a nationwide series of river surveys to determine the feasibility of developing hydroelectric power combined with navigation, irrigation, and flood control measures. USACE produced and transmitted 190 river surveys to Congress under the 308 Program. These reports include feasibility studies for projects to improve water trade, irrigation, power development, and flood control. Federal, state, and local officials have used these results to make decisions about proposed projects.[4]

The cost-benefit analysis is a vital part of the 308 Program to check the feasibility of the project. Pearce (1983) states that the Flood Control Act in 1936, which reflected the result of the 308 Program, is called the origin of cost-benefit analysis. Each survey report of the rivers includes a cost-benefit analysis, though the quality of the examination varies by river. For instance, the report of the Mississippi River below Cape Girardeau discusses the construction of levees, comparing the benefit of the land value per acre with the construction cost of levees per acre. USACE calculates the benefit of land value using the frequency of overflow and the depth of floods. If the average future losses of land to be protected are higher than the levee construction cost, the report concludes that the construction of the levee is economically justified.

[3]For example, the AR-LA MS River levee system is part of the MR&T project. The year constructed was 1967.

[4]A proposed system in the 308 reports on the Tennessee Valley is similar to the design of multipurpose reservoirs developed by the Tennessee Valley Authority.

While the Secretary of War submitted most of the reports to Congress in the early 1930s, it took some time for the flood control measures to be legislated. A White House meeting was held on 31 January 1934 about flood control. President Roosevelt told reporters: “It was preliminary; we are going to meet again. We talked about flood control from the point of view of national planning with the general thought that we would try to work out a national plan in the larger aspect that would list the various rivers and flood control projects in the order of their necessity; that is, on the order of damage done, human beings affected, property affected, et cetera. But that is as far as we got, discussing national planning for flood control and all the things that go with it, power, reclamation, submarginal lands and everything else.”

In June 1935, Representative Riley J. Wilson introduced H.R. 8455, which listed 285 specific flood control projects authorized by Congress at the cost of $370 million. The 285 flood control projects were located in 34 states. The projects had been investigated by USACE and had a favorable cost/benefit ratio. In 1935 disastrous floods happened across the United States, drawing attention to the bill. The amendment process added several more flood control works (e.g., upstate New York and Yazoo River project). As a result, the cost of H.R. 8455 became approximately $500 million. Although the House passed H.R. 8455, the Senate didn’t. Senator Arthur H. Vandenberg (Michigan) and Millard E. Tydings (Maryland) denounced the bill because the amendment projects had not been thoroughly considered.

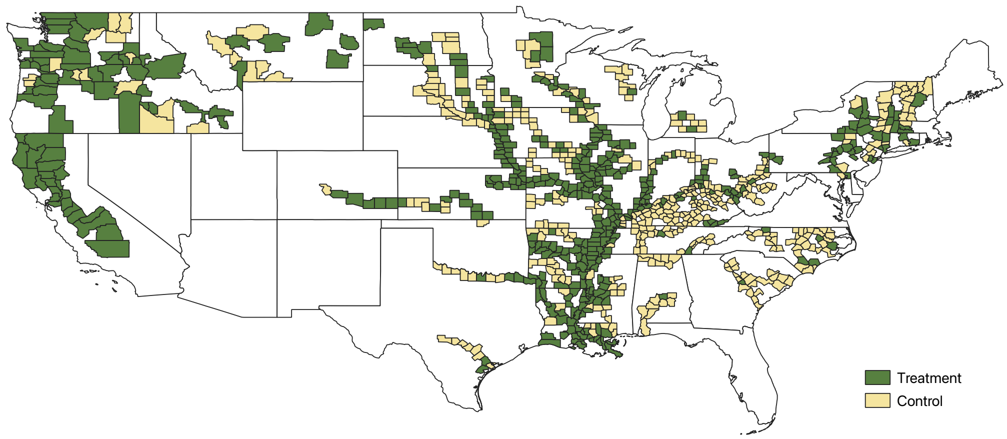
In March 1936, a severe flood across a wide area of the Northeast happened and inundated Maryland, West Virginia, Ohio, Pennsylvania, and New York. The flood encouraged the passage of national flood control legislation. Projects were selected primarily on USACE recommendations from the 308 reports and various emergency studies (such as for New York State and New England). In June 1936, the President signed H.R. 8455. A national program of flood control had become the official policy of the federal government. The 1936 Flood Control Act expanded flood control works to 41 states in total. Since then, the federal government has amended the Act eight times. Based on these amendments, the federal government invested in flood control infrastructure across the United States.

Figure 1.1 shows counties with and without levees based on the sample in rivers with flood risks based on the 308 Program reports. Since each 308 report does not show counties' names, I select counties along these rivers and use the National Levee Database (NLD) data to define counties with levees. The counties with green have areas protected by levees, and counties with yellow do not. I omit counties colored white because (1) the reports did not target these counties, or (2) the reports state that these counties do not have serious flood risks. Among 869 counties, 451 counties have levees, and 418 counties don't. While counties along the Mississippi River and in California likely have levees, counties in the East Coast region are not likely to have levees.

Figure 1.2 shows the years of completion for levees in the United States from 1930 to 2000. Samples are based on Figure 1.1. I use 1930 as a pre-treatment period because (i) the 1936 Flood Protection Act initiated many projects around the United States, and (ii) governments completed many projects after 1940. I set 2000 as the baseline post-treatment year to examine the long-term effect of levees on urban development and agricultural development.[5] The bulk of completion occurred between 1950-1970, with 1962 the mean year of completion.

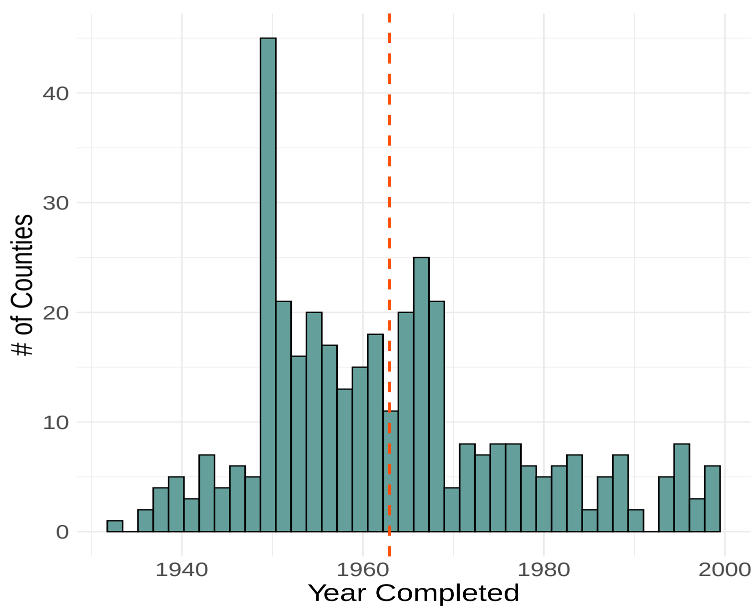
[5] Kline and Moretti (2013) uses the time window from 1940 to 2000 to analyze the long-term impact of TVA.

Figure 1.1: Counties with and without levees based on the 308 program



Notes: USACE surveyed 190 rivers based on the 308 program. I restrict the figure to rivers with flood risks. Colored green and yellow are counties along these rivers. Data from the protected area from levees is derived from the National Levee Database.

Figure 1.2: Constructed Year of Levees



Notes: Counties are defined in Figure 1.1. Data from year is derived from National Levee Database. The red vertical dot line is the mean of the completion year.

1.3 Framework: Difference-in-Differences

My research design will compare the characteristics in leveed counties to ones in non-leveed counties. To select treatment and control counties, I use surveyed data of the 308 Program, considering flood control measures in 190 rivers. Each report recommends whether the government should take flood control measures or not based on the cost-benefit analysis. While the federal government and local government constructed levees in some counties based on this report's recommendations, there are counties without levees along these rivers. I use this variation for the identification of the effects of levee construction. U.S. Congress (1926) defined the scope of this survey and ordered USACE to focus on navigable streams with power development feasibility. If the survey's nomination process was similar in rivers, this yields a set of control counties with both observable and unobservable characteristics such as productivity and amenities.[6]

Moreover, the reports provide descriptive evidence about flood risks. I focus on counties with flood risks based on this information. Specifically, each report includes comments from the Major General of USACE. I include counties along the rivers with comments such as "Floods frequently occur on the mainstream" and remove counties along the rivers with comments such as "Flood damages have been small". The exclusion process brings the number of eligible rivers from 190 to 79.

Though the use of these counties as controls has advantages, one may still be concerned that leveed counties are fundamentally different from non-leveed counties. This is because (i) the selection of the projects was based on the cost-benefit analysis, and (ii) counties with

[6] Busso et al. (2013) use tracts nominated by the local government but rejected as controls to evaluate Round I of the federal urban Empowerment Zone (EZ) program. Kline and Moretti (2013) use authorities that were proposed but never approved by Congress as controls to study the TVA.

levees are located around the major rivers. For example, Figure 1.1 indicates that leveed counties include ones along the south part of the Mississippi River. These counties are surrounding the Mississippi—Yazoo Delta, which has been dubbed as the "most southern place on earth" (Cobb, 1994). Moreover, these counties experienced the Great Mississippi Flood in 1927, an event which led to agricultural development (Hornbeck and Naidu, 2014).

To mitigate the imbalance, I use a propensity score reweighting method, which I will explain in the next section. Moreover, I carefully examine fundamental differences in the robustness section in two ways. In particular, first, I perform a placebo analysis, where I estimate the "effect" of the levee on 1900-1930 changes in outcomes. This experiment tests whether my outcome variables are trending differently in leveed and non-leveed counties in the decades leading up to the policy intervention. Because the period 1900-1930 is just before the levee treatment, the finding of differences between leveed counties and controls would be evidence of selection bias. Second, to eliminate the effect of the Great Mississippi Flood in 1927, I use a dummy variable that equals 1 when a county experienced the Flood in 1927 in the main econometric model. Then, reexamine the impact of levees on the local economic development.

As a first goal, I examine the causal impact of levee investment on various outcomes. To achieve this goal, I will rely on a simple standard difference-in-differences estimator to compare leveed counties to non-leveed counties. Specifically, I estimate program impacts using county-level regressions of the form:

$$\Delta Y_c = \beta_0 + \beta_1 Levee_c + \beta_3 X_c + \epsilon_c \tag{1.1}$$

where ΔY_c is the change in some outcome (e.g., log population) in county c from 1930 to 2000, $Levee_c$ is an indicator for whether a county c includes leveed area in 2000, and X_c is control variables. The coefficient β_1 is my interest. Since the characteristics within rivers may be correlated (e.g., the construction of levees in one area affects the building in other areas.[7]), I report the standard error clustered by the river in the approach.

1.4 Data

In order to identify counties related to the 308 Program, I use river GIS data in the U.S. from the National Weather Service. Then, I use a QGIS function to determine counties along these rivers. Next, to check whether counties have leveed areas, I use data from the National Levee Database. The National Levee Database provides location data on which levees protect specific areas. Finally, I define counties with levees as the treatment group and counties without levees as the control group. Although I can describe the treatment variable (leveed area) as continuous, my analysis involves methods, namely the CIC approach and the optimal targeting exercise, in which a continuous treatment variable complicates the calculations significantly. Thus, this paper uses a binary treatment variable.

There are ten outcome variables: population, employment, number of houses, number of farms, farmland acres, average farm size, farm value per acre, agricultural employment rate, manufacturing employment rate, and the number of water-related disasters. I use these variables to answer the following questions: how levees affect (i) the location choice of people

[7] Note that though levee construction may increase damage in other areas, I ignore this point in the exercise because the model becomes complicated. However, I will discuss the characteristics using a simple exercise in section 1.7.3.

(population and number of houses), (ii) the local labor market (employment, agricultural employment rate, manufacturing employment rate), (iii) the agricultural development (number of farms, farmland acres, average farm size, farm value per acre), and (iv) the number of water-related disasters (number of disasters). These data are from the Population Census, the Manufacturing Census, and the Agricultural Census. The data on the number of disasters is compiled based on the disaster declarations from the American National Red Cross (ARC) and FEMA using in Boustan et al. (2020).[8] Since the government undergoes a political process to declare an official catastrophe after a given weather event, one may be concerned that this data may not be appropriate for evaluating the impact of levees on disasters. To deal with this issue, I conduct an analysis using data on disaster fatalities.

I use four types of control variables. First, I use state dummy variables to control for political differences. Second, I use the monthly average precipitation of each county to control climate characteristics. Third, I use the pre-treatment lagged outcome variable from 1920 to 1930 to control for the convergence trend. Fourth, to control for the effect of flood control dams, I use the storage volumes of dams for flood control measured in 2020. This data is from the National Inventory of Dams. Since dams have many purposes,[9] I selected dams with flood control listed as the purpose. Then, I need to distinguish the role of flood control from irrigation and hydroelectricity in the case of multipurpose dams. To do this, I calculate maximum storage volume minus normal storage volume for each dam and sum up the volume by the river. I construct a panel and remove counties with missing data based on these variables, yielding a sample of 869 counties.

[8] I thank Leah Platt Boustan and Maria Lucia Yanguas for providing the dataset.

[9] For example, the database uses I: for Irrigation, H: for Hydroelectric, C: for Flood Control, and Storm Water Management.

Table 1.1 summarizes the average characteristics of leveed counties and control counties in 1930 along with changes in these characteristics over the period of 1920-1930. Column 5 of Table 1.1 provides p-values for the test of the null hypothesis that means pre-treatment levels and trends are equal across the leveed and control samples.

There are three main facts evident in Columns 1,2, and 5. First, leveed counties were more populated and had higher valued farms. Leveed counties passed the cost-benefit analysis based on these factors. Thus, the results are consistent with the selection based on the investigation. On the other hand, the number of farms and average farm size in leveed counties was similar to non-leveed counties. Second, the local economic structure was more industrialized in leveed counties than non-leveed counties because the manufacturing employment rate was higher, though the trend was similar. Finally, leveed counties were more likely to experience water-related disasters.

To deal with these imbalances, I rely on propensity score adjustments to control a wide array of pre-period county characteristics. There are two steps to select covariates. First, I set pre-treatment variables as covariates based on the method suggested in Imbens and Rubin (2015).[10] I calculate each county's weight and weighted outcomes using the estimated propensity score.[11] The third column of Table 1.1 shows the weighted mean after reweighting the controls to mimic the covariate distribution of the treated observations using the same covariates. Although both pre-treatment levels and trends in characteristics improved after

[10]In particular, I used the following variables as the covariates based on the likelihood ratio statistics:
(1) Level in 1920: Log population, Log # of houses, Log # of farms, Log farmland acres, Log farm value per acre, Agricultural employment rate, # of disaster.
(2) Change from 1910-1920: log population, log employment, log # of houses, log # of farms, log farm value per acre, Agricultural employment rate, Manufacturing employment rate.
(3) Geography: minimum elevation, monthly average precipitation (1910-1936)

[11]In particular, $weight_c = \frac{p(x_c)}{1-p(x_c)}(1 - Levee_c) + Levee_c$, where $p(x_c)$ is a propensity score in a county c. The weighted outcome $\tilde{Y_c}$ is calculated by $\tilde{Y_c} = weight_c Y_c$, where Y_c is an observed outcome.

reweighting, Column 6 shows that there are still imbalances between treatment and control counties, especially for the lagged outcome variables. Because the pre-treatment variables based on the 1910s are not enough to remove the imbalances, I also use five outcome values in level and lagged.[12] Columns 1, 4, and 7 show that adding these variables exhibits dramatically improved balance in the pre-treatment period.

To illustrate these facts visually, Figure 1.3 shows the mean behavior of my ten variables in the leveed and control counties before and after reweighting across the one hundred years in my sample. After reweighting, treated and control counties exhibit similar patterns in economic activity prior to the start of the program in 1936.

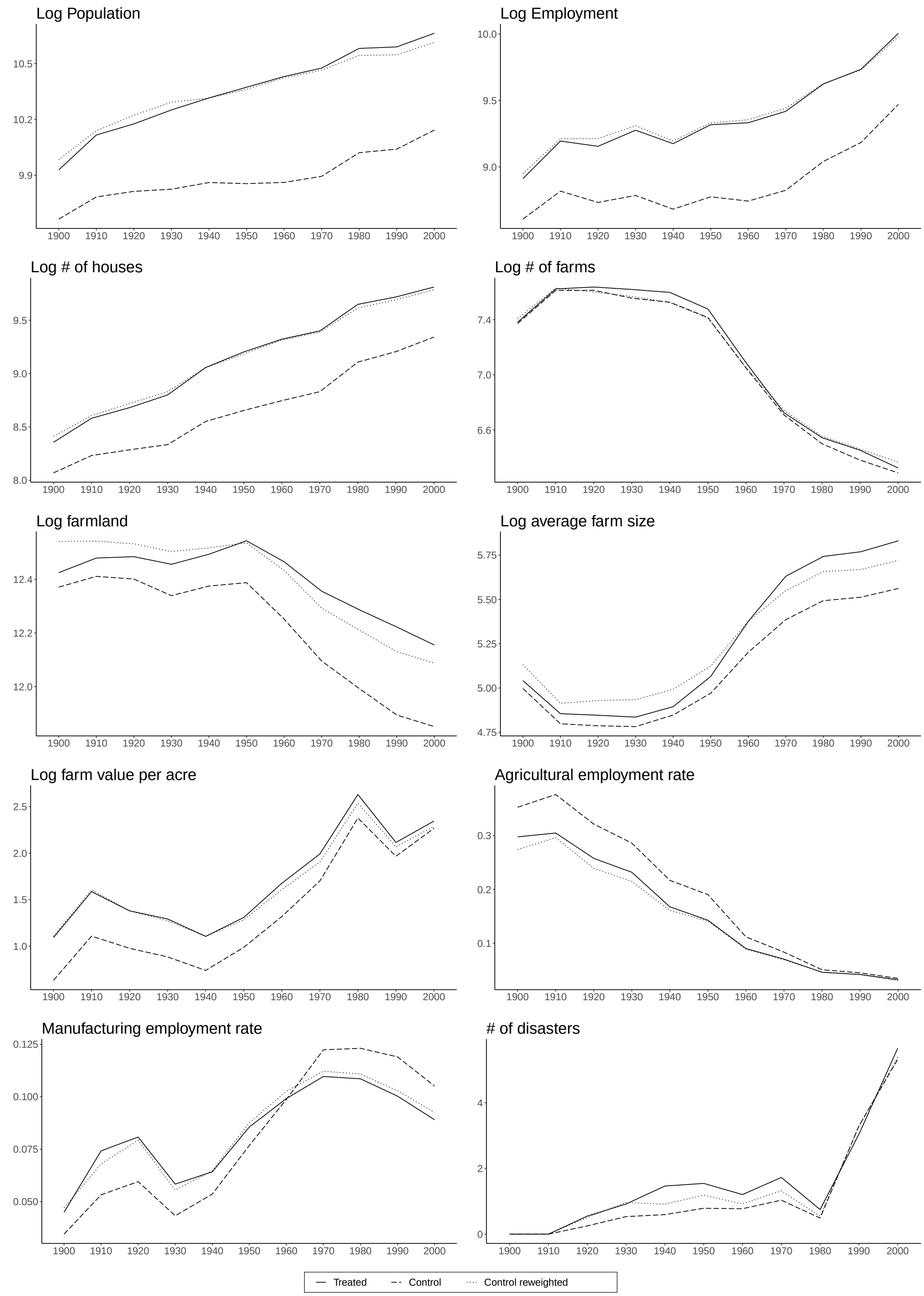
[12] In particular,
(4) Level in 1930: Agricultural employment rate, Log average farm size,
(5) Change from 1920-1930: log employment, log number of farms, # of disaster.

Table 1.1: Pre-Treatment Sample Means

	Leveed	Ctrls	Ctrls weight	Ctrls weight ver 2	p-value btw (1) and (2)	p-value btw (1) and (3)	p-value btw (1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Level in 1930							
Log population	10.251	9.825	10.338	10.292	0.000	0.173	0.509
Log employment	9.276	8.785	9.367	9.310	0.000	0.178	0.610
Log # of houses	8.800	8.334	8.877	8.831	0.000	0.214	0.612
Log # of farms	7.620	7.557	7.422	7.569	0.154	0.000	0.298
Log farmland	12.456	12.339	12.374	12.503	0.009	0.113	0.341
Log average farm size	4.836	4.782	4.951	4.934	0.283	0.022	0.059
Log farm value per acre	1.292	0.885	1.212	1.273	0.000	0.074	0.668
Ag employment rate	0.232	0.286	0.202	0.215	0.000	0.002	0.080
Manuf employment rate	0.058	0.043	0.056	0.056	0.000	0.538	0.506
# of disasters	0.918	0.533	0.700	0.962	0.000	0.005	0.602
Changes 1920-1930							
Log population	0.075	0.012	0.032	0.071	0.000	0.000	0.686
Log employment	0.121	0.053	0.052	0.097	0.000	0.000	0.146
Log # of houses	0.119	0.049	0.083	0.112	0.000	0.000	0.519
Log # of farms	−0.018	−0.057	−0.113	−0.034	0.002	0.000	0.233
Log farmland	−0.028	−0.062	−0.115	−0.029	0.003	0.000	0.934
Log average farm size	−0.011	−0.005	−0.002	0.005	0.617	0.495	0.204
Log farm value per acre	−0.087	−0.093	−0.071	−0.107	0.816	0.525	0.405
Ag employment rate	−0.026	−0.035	−0.025	−0.024	0.136	0.878	0.784
Manuf employment rate	−0.022	−0.016	−0.025	−0.024	0.062	0.491	0.673
# of disasters	0.368	0.282	0.080	0.456	0.226	0.001	0.305
Observations	451	418	418	418	869	869	869

Notes: The unit of observation is a county. Column 1 reports sample means for leveed counties. Column 2 shows the means for control counties. Column 3 reports the means for control after propensity score reweighting. Column 4 reports the means for control after propensity score reweighting with additional covariates. Column 5 presents p-values for a null hypothesis test that the mean in Column 1 equals the mean in Column 2. Similarly, Columns 6 and 7 reports p-values for the equality of the means in Columns 1 and indicated columns.

Figure 1.3: Means By Year and Treatment Status



Notes: Figures depicts means of the listed variables in Leveed counties and controls. Reweighted lines correspond to weighted means using propensity score weights described in Section 1.4.

1.5 Results: Difference-in-Differences

I turn now to my baseline difference-in-differences estimates of the impact of levee systems. Table 1.2 provides estimates of the effects of levee systems on long-run local economic development. Column 1 reports within-state differences between leveed counties and non-leveed counties in the period from 1930-2000. Column 2 adds control variables to Column 1. Column 3 takes into account the propensity score reweighting discussed in the previous section. Column 3 is my preferred specification.

To examine the effect of levee systems, I discuss the answers to the four questions mentioned in the previous section. First, the effect of the levee on the population was negative and not statistically significant. Although people may choose to live in leveed areas because they reduce the risk of flooding on their land, I cannot find such evidence in this exercise.

Second, the effect on acres of farmland is positive and statistically insignificant, and the effect on farm value per acre is negative and statistically insignificant after the reweighing process. Kousky et al. (2006) shows that the benefits of government protection, such as levees, fall entirely on land value. My estimate is inconsistent with the prediction, though the confidence interval is wide.

Third, Column 3 indicates that the 1930-2000 growth rate for agricultural employment was higher in leveed. On the other hand, the manufacturing employment growth rate was lower in leveed counties than in non-leveed counties. These results are statistically insignificant. Although counties along rivers with levees might be suitable for agriculture because this industry needs much water, and levees might prevent soil erosion, I cannot find clear evidence here.

Fourth, I find that the impact of levee systems decreases the number of water-related disasters, though the result is imprecise. While Column 2 shows that levees have a statistically significant impact on the number of disasters, Column 3 shows the result becomes insignificant. I note that I compare the number of disasters from 1930-1940 with 2000-2010, which may partially explain my limited findings.

In Table 1.3, I present the change from 1930 to 1970 since most of the levee construction finished before 1970. Though the overall sign and magnitude are similar to Table 1.2, Column 3 shows that the number of firms, the average farm size, and the number of disasters become statistically significant. The results suggest that the levees invested in earlier decades caused a decrease in the water-related disasters and larger farm size per firm.

Unlike other variables, I can assess the number of disasters cumulatively. Table 1.4 shows this exercise. I compare the total number of disasters from 1910 to 1940 (before the main construction) with the total number of disasters shown in each panel title. Column 1 shows that the estimate for the total number of disasters is negative but insignificant from 1940 to 2010. To check the effect before and after the main construction period, I divide the overall period into two-time windows from 1940 to 1970 and from 1970 to 2000. The results show that while the number of disasters increased during the main construction period, the number of disasters decreased after finishing the main construction. However, these results are insignificant, and the difference between the two estimates is statistically insignificant.

As mentioned in the data section, the disaster counts are compiled based on the disaster declarations by the American National Red Cross or various federal agencies, which undergo a political process to declare an official catastrophe after a given weather event. Although I use a state dummy variable to control for this effect, one may be concerned that this data may not be appropriate for evaluating the impact of levees on disasters. Ideally, I would use detailed climate data to measure the flood intensities. However, it is not possible to gather such data over an entire century. Alternatively, I can use the fatalities associated with water-related disasters as an outcome. Using this variable as the primary outcome for my framework, especially the optimal policy exercise that requires enough variation to build distributions, is challenging because of the lack of variation (e.g., 86 percent of the observations were zero in 1930). However, the variable is helpful to mitigate the effect of the political process and provide more clear implications about the relationship between levees and water-related disasters. I set various fatality thresholds, starting with a threshold of only ten fatalities and increasing to an extreme threshold of one hundred fatalities.

Table 1.5 provides two implications. First, estimates are negative except for one. Second, the magnitudes in Column 3 are higher than those in Column 2. Although these results imply that there are areas where levees have reduced catastrophic disasters since the government constructed levees, I lace the statistical power to detect effects of reasonable size.

Overall, I don't see significant evidence that the presence of levees increased the population or economic development of the area. Also, I cannot find clear evidence that levees have reduced flood risks. I provide additional heterogenous evidence related to disasters in the following section using the CIC approach. The limitation of this method is that the analysis does not include the benefits considered in the usual cost-benefit analysis for levee construction. Specifically, though the DID approach can compare the growth rate of economic outcomes in treated with control counties after the treatment, such an approach cannot consider the benefits of levees for the pre-existing life and property in treated areas. Integrating this in the study is an interesting future research topic.

Table 1.2: Economic Effects of Levee Infrastrcture, 1930-2000

Outcome	naive (1)	control (2)	reweighted & control (3)
Log population	0.095	−0.018	−0.116
	(0.072)	(0.061)	(0.162)
Log employment	0.044	−0.010	−0.218
	(0.075)	(0.069)	(0.218)
Log # of houses	0.003	−0.064	−0.218
	(0.068)	(0.061)	(0.181)
Log # of farms	−0.028	−0.223	−0.028
	(0.090)	(0.064)	(0.201)
Log farmland	0.187	0.052	0.092
	(0.056)	(0.072)	(0.066)
Log average farm size	0.215	0.276	0.077
	(0.103)	(0.095)	(0.176)
Log farm value per acre	−0.330	−0.234	−0.151
	(0.067)	(0.049)	(0.151)
Agricultural employment rate	0.052	0.028	0.012
	(0.017)	(0.015)	(0.031)
Manufacturing employment rate	−0.031	−0.021	−0.008
	(0.008)	(0.009)	(0.009)
# of disasters	−0.046	−0.384	−0.466
	(0.367)	(0.177)	(0.552)
Observations	869	869	869

Notes: Each entry gives the 1930-2000 difference-in-differences (DID) estimate of levee on the outcome presented in each row. Column 1 reports DID estimates without controls; column 2 reports DID estimates using control variables; column 3 reports DID estimates using propensity score reweighting and control variables. Robust standard errors are in parentheses. The standard errors are clustered by the river.

Table 1.3: Economic Effects of Levee Infrastrcture, 1930-1970

Outcome	naive (1)	control (2)	reweighted & control (3)
Log population	0.040	0.002	−0.076
	(0.067)	(0.046)	(0.089)
Log employment	−0.030	−0.013	−0.142
	(0.079)	(0.059)	(0.102)
Log # of houses	0.004	−0.018	−0.064
	(0.073)	(0.051)	(0.094)
Log # of farms	−0.154	−0.151	−0.302
	(0.061)	(0.043)	(0.156)
Log farmland	0.143	0.068	0.049
	(0.067)	(0.061)	(0.049)
Log average farm size	0.297	0.222	0.341
	(0.099)	(0.089)	(0.184)
Log farm value per acre	−0.040	−0.067	0.094
	(0.058)	(0.017)	(0.089)
Agricultural employment rate	0.011	0.005	−0.054
	(0.016)	(0.010)	(0.033)
Manufacturing employment rate	−0.020	−0.015	0.006
	(0.007)	(0.004)	(0.010)
# of disasters	−0.206	−0.393	−0.552
	(0.205)	(0.156)	(0.174)
Observations	869	869	869

Notes: Each entry gives the 1930-2000 difference-in-differences (DID) estimate of levee on the outcome presented in each row. Column 1 reports DID estimates without controls; column 2 reports DID estimates using control variables; column 3 reports DID estimates using propensity score reweighting and control variables. Robust standard errors are in parentheses. The standard errors are clustered by the river.

Table 1.4: Effects for Total Number of Disasters of Levee Infrastructure

Outcome	1940-2010 (1)	1940-1970 (2)	1970-2010 (3)
Cumulative Number of Disasters	−0.816	0.223	−1.446
	(1.698)	(0.375)	(1.240)

Notes: Robust standard errors are in parentheses. The standard errors are clustered by the river. I aggregate the total number of disasters by indicated periods in the column for the post-treatment outcome and the total number of disasters from 1910 to 1940 for the pre-treatment outcome. Propensity score reweighting method and control variables are used for the estimation.

Table 1.5: Effects for Severe Disaster of Levee Infrastructure

Fatality Threshold	1940-2010 (1)	1940-1970 (2)	1970-2010 (3)
10	−0.295	0.017	−0.269
	(0.327)	(0.071)	(0.284)
20	−0.276	−0.086	−0.186
	(0.249)	(0.061)	(0.221)
30	−0.231	−0.041	−0.174
	(0.236)	(0.061)	(0.216)
40	−0.232	−0.037	−0.173
	(0.234)	(0.061)	(0.213)
50	−0.075	0.010	−0.065
	(0.165)	(0.054)	(0.143)
60	−0.043	0.000	−0.022
	(0.118)	(0.052)	(0.096)
70	−0.060	−0.004	−0.040
	(0.119)	(0.058)	(0.098)
80	−0.017	−0.004	0.003
	(0.105)	(0.058)	(0.081)
90	−0.113	−0.004	−0.093
	(0.109)	(0.058)	(0.086)
100	−0.117	−0.011	−0.097
	(0.109)	(0.058)	(0.087)

Notes: Robust standard errors are in parentheses. The standard errors are clustered by the river. I aggregate the total number of disasters by indicated periods in the column for the post-treatment outcome and the total number of disasters from 1910 to 1940 for the pre-treatment outcome. Propensity score reweighting method and control variables are used for the estimation.

1.5.1 Robustness

To evaluate the effectiveness of my controls in matching the pre-treatment growth patterns of leveed counties, Table 1.6 shows the results of a placebo analysis. In particular, I estimate the "effect" of the levee on the 1900-1930 changes in population, employment, housing units, agricultural characters, and industry structure. This false experiment tests whether my outcome variables have different trends in leveed and non-leveed counties in the decades leading up to the policy intervention. Because the period 1900-1930 is just before the federal main levee construction across the U.S., the finding of differences between leveed counties and controls would be evidence of selection bias.

Column 1 shows the unconditional difference between leveed counties and non-leveed counties. Column 2 adds control variables. Column 3 uses propensity score reweighting. Column 1 indicates that population, employment, and the number of houses are significantly different between treatment and control. However, after adding control variables and reweighting, Column 3 shows that none of the outcomes register statistically significant differences across treatment and control counties.

The second robustness check examines whether the Mississippi river in 1927 affected the result. Hornbeck and Naidu (2014) shows that the Great Mississippi Flood in 1927 caused migration and increased farm capital. Since the catastrophic flood affected the design of levee systems along the lower Mississippi River and the economic outcomes, the event would be considered a confounding variable. To control for this effect, I add a dummy variable of counties with the Great Mississippi Flood in 1927 from Hornbeck and Naidu (2014), then I examine the same difference-in-differences approach. Table 1.7 shows that the result has a similar trend even after controlling for this disaster. Thus, this confounding factor does not affect my outcomes.

Table 1.6: Placebo Test: 1900-1930

Outcome	naive	control	reweighted & control
	(1)	(2)	(3)
Log population	0.161	0.143	−0.031
	(0.060)	(0.047)	(0.062)
Log employment	0.186	0.150	−0.034
	(0.060)	(0.048)	(0.077)
Log # of houses	0.179	0.140	−0.042
	(0.056)	(0.046)	(0.057)
Log # of farms	0.054	−0.020	−0.011
	(0.049)	(0.039)	(0.044)
Log farmland	0.063	−0.026	0.042
	(0.037)	(0.036)	(0.032)
Log average farm size	0.009	−0.006	0.078
	(0.036)	(0.035)	(0.053)
Log farm value per acre	−0.053	−0.016	0.029
	(0.055)	(0.046)	(0.066)
Agricultural employment rate	0.001	−0.009	−0.004
	(0.009)	(0.009)	(0.016)
Manufacturing employment rate	0.005	0.007	0.004
	(0.004)	(0.004)	(0.004)
Observations	869	869	869

Notes: Robust standard errors are in parentheses. The standard errors are clustered by the river.

Table 1.7: Robustness: Controlling for the Great Mississppi Flood in 1927

Outcome	naive	control	reweighted & control
	(1)	(2)	(3)
Log population	0.118	0.072	−0.083
	(0.069)	(0.054)	(0.175)
Log employment	0.084	0.034	−0.197
	(0.066)	(0.060)	(0.247)
Log # of houses	0.027	0.029	−0.193
	(0.063)	(0.053)	(0.245)
Log # of farms	0.063	−0.201	−0.004
	(0.068)	(0.052)	(0.205)
Log farmland	0.182	0.052	0.056
	(0.057)	(0.079)	(0.079)
Log average farm size	0.119	0.253	0.059
	(0.071)	(0.085)	(0.188)
Log farm value per acre	−0.342	−0.227	−0.157
	(0.068)	(0.049)	(0.148)
Agricultural employment rate	0.065	0.028	0.017
	(0.015)	(0.016)	(0.033)
Manufacturing employment rate	−0.032	−0.020	−0.006
	(0.009)	(0.009)	(0.009)
# of disasters	−0.157	−0.491	−0.440
	(0.382)	(0.159)	(0.548)
Observations	869	869	869

Notes: Robust standard errors are in parentheses. The standard errors are clustered by the river.

1.6 Framework: Optimal Policy Targeting

This section provides a framework for optimal targeting of levees in the U.S. to decrease the losses associated with the water-related disasters. There are three subsections to discuss this topic. First, I explain the discrete CIC approach to obtain an estimator for the entire counterfactual distribution of effects that the treated (untreated) group would have experienced without (with) the treatment. Second, I propose a critical assumption to convert the counterfactual distribution into individual values. Third, I develop an optimal targeting framework based on the estimated counterfactual values.

1.6.1 Changes-in-Changes with Discrete Outcomes

Assumptions

The CIC approach is a nonlinear generalization of the DID approach. Since my outcome (the number of water-related disasters) is discrete, I must consider a variant of the standard CIC approach. Section 4 in Athey and Imbens (2006) provides explanations about this topic. I explain my framework based on their section. Note that I drop the subscript for the county to simplify the notation.

A county belongs to a group $G \in \{0, 1\}$, where group 0 is the control group and group 1 is the treatment group. The values are observed in time period $T \in \{0, 1\}$. A county is characterized by a random variable U. The random variable U represent unobserved characteristics such as geographical vulnerability for floods. Let D_{gt} denote the number of water-related disasters observed in a county of group $G = g$ in time period $T = t$. Let D^N_{g1} denote the number of water-related disasters for a county in period $T = 1$ if that county does not have levees and D^I_{g1} if that county has levees. For example, when D^I_{11} are observed values, D^N_{11} are counterfactual values. To use the discrete CIC approach, I impose six assumptions.

Assumption 1. *The number of water-related disasters without levees satisfies the relationship* $D^N_{gt} = h\,(U, T)$.

Assumption 1 requires that the number of water-related disasters without levees is a function of an unobservable variable U and time T, but not of the group indicator G.

Assumption 2. *The function* $h^j(u, t)$ *is nondecreasing in* u *for* $j \in \{I, N\}$.

Assumption 2 means that counties with high values of unobservable characteristics such as geographical vulnerability are likely to have many disasters. This assumption allows me to invert the function h^j and derive bounds on the average effect of the treatment.

Assumption 3. *Within each group, the distribution of* U *is the same over time:* $U \perp T|G$.

Assumption 3 requires that any unobservable differences between leveed and non-leveed counties be stable over time. This assumption is called time invariance. This assumption allows us to estimate the trend in one group by evaluating the trend in the other group, playing a similar role to the standard DID method's common trends assumption. Since the distribution of unobservables does not have to be the same across treatment and control groups, this assumption allows for policy interventions targeted at a group with potentially higher average benefits. This characteristic is desirable for my framework because there are some differences between treated and control counties regarding the number of water-related disasters in the pre-treatment period (see Table 1.1). Although the assumption is inherently untestable, I test the assumption in the following section using a pre-treatment setting because the assumption is critical to using the CIC approach.

Assumption 4. *Supp* $(U|G = 1) \subseteq$ *Supp* $(U|G = 0)$

Assumption 4 requires that for every value of U in the $G = 1$, we need to infer the counterfactual outcome in the $t = 1$ period, which can only be obtained from the $G = 0$.

Assumption 5. *The variables* $U|G = 1$ *and* $U|G = 0$ *are continuously distributed.*

While the number of water-related disasters is a discrete value, Assumption 5 requires that the unobservables such as geographical vulnerability for floods are continuous. The assumption allows us to compare the distribution of the unobservable across the groups.

Approach I: Partial Identification

There are three approaches for identification of the CIC with a discrete outcome. First, I discuss partial identification. The reason why point identification is lost is that we cannot invert the distribution to obtain the outcome value. Let us denote by $F_{D^j_{gt}}$ the conditional distribution function of D^j_{gt} for $j \in \{I, N\}$. For any quantile $q \in [0, 1]$, I define the following inverse distribution functions:

$$\begin{aligned} F_D^{-1}(q) &= \inf\{d \in \mathbb{D} | F_D(d) \geq q\} \\ F_D^{(-1)}(q) &= \sup\{d \in \mathbb{D} \cup \{-\infty\} | F_D(d) \leq q\}. \end{aligned} \tag{1.2}$$

These equations are expressed in Figure 1.4. When we invert q, we only know that D lies in the interval between $F_D^{(-1)}(q)$ and $F_D^{-1}(q)$.

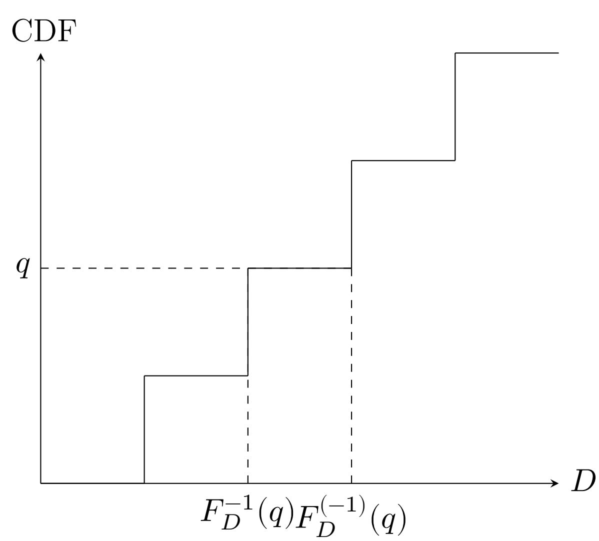
Using Equation (1.2), the bounds in the discreate CIC approach are defined as follows. This is from Theorem 4.1 from Athey and Imbens (2006).

$$F^{LB}_{D^N_{11}}(d) \leq F_{D^N_{11}}(d) \leq F^{UB}_{D^N_{11}}(d) \tag{1.3}$$

where $F^{LB}_{D^N_{11}}(d) = F_{D_{10}}\left(F^{(-1)}_{D_{00}}\left(F_{D_{01}}(d)\right)\right)$, $F^{UB}_{D^N_{11}}(d) = F_{D_{10}}\left(F^{-1}_{D_{00}}\left(F_{D_{01}}(d)\right)\right)$ and D_{gt} are observed values.

In words, the bounds of the counterfactual distribution can be calculated based on the distribution of outcomes: for the same group before the treatment, the control group before the treatment, and the control group after the treatment. By comparing the observed with the counterfactual, we can obtain estimates of various treatment effects on the treated. Also, we can get treatment effects on the untreated using a similar method.

Figure 1.4: An illustration of Equation (1.2)



Approach II: Conditional Independence Assumption

The second approach is to use the conditional independence assumption. If I use the conditional independence assumption, the distribution is point identified. Specifically, we need the following assumption:

Assumption 6. *Conditional on D and T, the distribution of U is independently distributed between groups: $U \perp G|D,T$*

Assumption 6 requires that for observations with the same value of D in time T, the distributions of the unobservable characteristics are the same in the $G = 1$ as in the $G = 0$. This assumption allows us to identify counties in the first-period control group with the same outcome d when we look at the treated county in the first period with outcome d. After identifying control counties, we can use the distribution of unobservables in the $G = 0$ to infer the counterfactual distribution of the discrete outcome. The distributions of unobservables are continuous (Assumption 6), so I can infer the distribution between discrete outcome values. Using the monotonicity assumption (Assumption 2), I can derive the distribution of their second-period outcomes. Also, I use these values to derive the counterfactual distribution for the second period treated in the absence of the intervention.

Using these assumptions, the counterfactual distribution is given by the following equation. The equation is from Theorem 4.2 in Athey and Imbens (2006).

$$\begin{aligned} F^{DCIC}_{D^N_{11}}(d) =& F_{D_{10}}\left(F^{(-1)}_{D_{00}}\left(F_{D_{01}}(d)\right)\right) \\ &+ \left(F_{D_{10}}\left(F^{-1}_{D_{00}}\left(F_{D_{01}}(d)\right)\right) - F_{D_{10}}\left(F^{(-1)}_{D_{00}}\left(F_{D_{01}}(d)\right)\right)\right) \\ &\times \frac{F_{D_{01}}(d) - F_{D_{00}}\left(F^{(-1)}_{D_{00}}\left(F_{D_{01}}(d)\right)\right)}{F_{D_{00}}\left(F^{-1}_{D_{00}}\left(F_{D_{01}}(d)\right)\right) - F_{D_{00}}\left(F^{(-1)}_{D_{00}}\left(F_{D_{01}}(d)\right)\right)}. \end{aligned} \tag{1.4}$$

There are two parts to this equation. The first part is related to the lower bound. The second part is derived from the difference between a counterfactual distribution and the lower bound multiplied by the term inferred from the conditional independence assumption.

Approach III: Covariates

The third method is to use covariates for point identification. Although Athey and Imbens (2006) suggest an approach to obtain point estimates by incorporating covariates in the discrete CIC approach, the empirical estimation in this context is not well established.[13] Athey and Imbens (2002) also suggest a CIC approach with covariates using an additive separability for the effects of covariates. I use their method for the point identification. In particular, let I be the four-dimensional vector $((1-T)(1-G), T(1-G), (1-T)G, TG)'$. In the first stage, I estimate

$$D_c = I_c'\delta + X_c'\beta + \epsilon_c. \tag{1.5}$$

Then construct the residuals with the group/time effects added back in:

$$\tilde{D_c} = D_c - X'\hat{\beta} \tag{1.6}$$

Then, I use the usual CIC approach, which yields a point-identified estimate.[14]

[13] See p.40 in the working paper version (Athey and Imbens, 2002).

[14] I use the same covariates in the DID setting. In particular, state dummy variables, monthly average precipitation, pre-treatment lagged outcome and storage volumes of dams for flood control.

1.6.2 A Critical Assumption to Connect between CIC and Optimal Targeting

This subsection discusses a critical assumption to bridge between the CIC approach and the optimal targeting exercise. While the CIC approach allows us to identify the counterfactual distribution, it does not provide the individual counterfactual value. Assumption 3 in the previous subsection permits changing each county's rank. However, the assumption is not enough to discuss optimal targeting because the exercise requires the individual outcome effect. One strategy is to assume that each county needs a fixed rank over time. Note that Athey and Imbens (2006) does not require this assumption in the CIC approach.

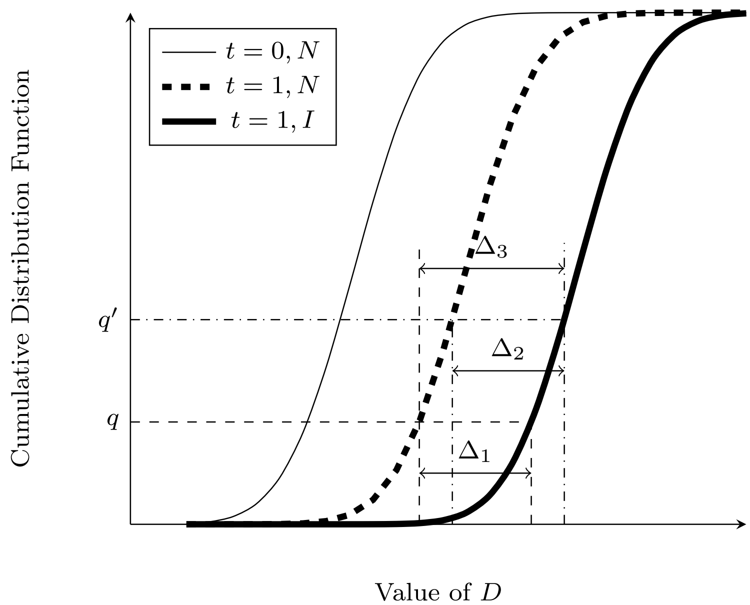
Assumption 7. *U^j_{ct} is constant over time for all counties for $j \in \{I, N\}$.*

To emphasize that each county has the same rank, I use county c as a subscript in this assumption. There are two definitions related to this assumption. The first concept is perfect correlation. Athey and Imbens (2006) defines the condition as $U_{c0} = U_{c1}$. This requires that the unobservables stay the same in county c across time. The second concept is rank invariance. The typical definition of rank invariance can be written as $U^N_{c1} = U^I_{c1}$, where N means control and I means treatment (Chernozhukov and Hansen, 2005). This requires that the observed rank in $t = 1$ and the counterfactual rank in $t = 1$ are the same. Using these definition, I can write Assumption 7 as follows: $U^N_{c0} = U^N_{c1} = U^I_{c1}$.

This assumption seems unrealistic because the rank of unobservables such as geographical flood vulnerability in each county, would change from 1930 to 2000. To deal with this issue, I define the following three cases. Figure 1.5 illustrates the cases. The red line is the cumulative distribution function at $t = 0$, the blue line is the counterfactual distribution at

$t = 1$, and the orange line is the observed distribution at $t = 1$. Let q be a rank of a county at $t = 0$ and q' at $t = 1$. If $U^N_{c0} = U^N_{c1} = U^I_{c1}$ holds, then the individual effect equals to Δ_1. On the other hand, when rank invariance $U^N_{c1} = U^I_{c1}$ holds, the effect is Δ_2. There are many possibilities for setting the counterfactual rank of each county.[15] One reasonable assumption is to use the same rank in $t = 0$ for the counterfactual value. This setting implies that while the unobservable rank does not change without levees, the rank changes when the county has a levee. This setting yields Δ_3. Assumption 7 is a critical assumption for the CIC approach and the optimal targeting exercise, so I examine these assumptions in the next section.[16]

Figure 1.5: Illustration of Rank Conditions and County's Effects



[15]See for instance the discussion in Heckman et al. (1997)

[16]To avoid more discussion about the assumption, I can use the conditional average treatment effect for each county used in Assunção et al. (2019). However, while the approach can consider county heterogeneity using observables, the approach ignores the heterogeneity of unobservables within the treatment group. As I show in the next section, unobservable heterogeneity within the treatment group affects the rank over time. Thus, I decide not to use this approach.

1.6.3 Statistical Decision Theory

Based on the previous discussion, I can allocate levees to counties. Although there are many social objectives, I set three to assign the counties subject to the constraint that the total number of leveed counties stays the same. The first objective is to minimize the total number of disasters by allocating levees to counties. The second objective is to minimize the number of disasters weighted by the population in 1930. The third objective is to minimize the economic effect of disasters using county-level GDP in 2016.[17]

Denote the counterfactual assignment rule by ϕ_c. If I use Approach II (conditional independence assumption) or Approach III (covariates) in the previous subsection, the counterfactual distribution can be point-identified. In this case, the policymaker solves the problem:

$$\min_{\phi_c \in \{0,1\}} \sum_{c=1}^{C} \left[\phi_c D_{1,c}^{I} + (1-\phi_c) D_{1,c}^{N} \right], \tag{1.7}$$

where $D_{1,c}^{I}$ are treatment value and $D_{1,c}^{N}$ are non-treatment value. For example, when a county c is actually treated, $D_{1,c}^{I}$ is an observed value and $D_{1,c}^{N}$ is a counterfactual value. One constraint applies to the total number of counties $\bar{C}$:

$$\sum_{c=1}^{C} \phi_c = \bar{C} \tag{1.8}$$

I can solve this problem using an integer linear programming method. I call the problem *the minimum criterion* hereafter.

When I use the second objective I use the county's population as a weight. The objective

[17]The county-level GDP is available from 2016.

function becomes

$$\min_{\phi_c \in \{0,1\}} \sum_{c=1}^{C} Pop_{1930,c} \left[\phi_c D_{1,c}^{I} + (1-\phi_c) D_{1,c}^{N} \right], \tag{1.9}$$

where $Pop_{1930,c}$ is the population at county c in 1930. When I use the third objective, the objective function is

$$\min_{\phi_c \in \{0,1\}^M} \sum_{c=1}^{C} GDP_{2016,c} \left[\phi_c D_{1,c}^{I} + (1-\phi_c) D_{1,c}^{N} \right], \tag{1.10}$$

where $GDP_{2016,c}$ is the county's GDP in 2016. Equation (1.8) is applied to both settings.

If I use Approach I, the model is not point-identified. I can only partially identify a counterfactual number of water-related disasters. In this case, an ex-post policy evaluation must be analyzed as a treatment choice problem under ambiguity (Manski, 2005). I consider the minimax criterion, assuming the policymaker chooses counties to minimize the negative effects of disasters in the worst-case scenario.

There are two possible cases when the policymaker thinks about the worst-case. First, the policymaker supposes the water-related disasters happen a lot, and the policymaker wants to mitigate the effect as much as possible. Second, the construction of levees does not work well (e.g., levees increase disasters). I use the first scenario in my exercise because the second case results in a trivial pattern (e.g., all counties should not have levees). In the treatment effect on the treated, the policymaker considers the worst number of disasters if he does not construct levees. This value is from the *upper* bound of the counterfactual number of water-related disasters. On the other hand, I use the lower bound case when examining treatment effects on the untreated. Suppose that if the government constructs levees, the impact of disasters decreases as much as possible. The value is from the *lower* bound of the counterfactual number of water-related disasters.[18]

The problem can be written as follows:

$$\begin{aligned}\min_{\phi_c \in \{0,1\}} \sum_{c=1}^{C} \phi_c & \left[\{G_c = 1\} D^I_{11,c} + \{G_c = 0\} \underline{D}^I_{01,c} \right] \\ & + (1-\phi_c) \left[\{G_c = 1\} \overline{D}^N_{11,c} + \{G_c = 0\} D^N_{01,c} \right],\end{aligned} \tag{1.11}$$

subject to the number of counties constraint (1.8). $\overline{D}^N_{01,c}$ is the upper bound on the number of disasters and $\underline{D}^I_{11,c}$ denotes the lower bound. This minimization problem subject to the constraint is also a linear programming problem that is straightforward to solve numerically. When I use other objectives, I use population or GDP as the weights such as Equations (1.9) and (1.10).

The minimization problem formulated above uses an estimate of the counterfactual value. Since the counterfactual value has the variance from the CIC approach, I need to consider the property to develop the problem. In particular, I incorporate the variance into the minimization problem in the following way.

$$\begin{aligned}\min_{\phi_c \in \{0,1\}} \sum_{c=1}^{C} \phi_c & \left[\{G_c = 1\} D^I_{11,c} + \{G_c = 0\} (D^I_{01,c} - \frac{\gamma}{2}\sigma_c^2)) \right] \\ & + (1-\phi_c) \left[\{G_c = 1\} (D^N_{11,c} - \frac{\gamma}{2}\sigma_c^2) + \{G_c = 0\} D^N_{01,c} \right],\end{aligned} \tag{1.12}$$

where γ is an absolute risk aversion parameter. This approach uses an idea from portfolio theory. To derive this equation, I use the following steps: I assume that the counterfactual

[18] While Equation (1.3) uses the outcome as the domain, Figure 1.6 below uses the quantile as the domain. Thus, the lower bound distribution yields the upper bound outcomes and vice versa.

value for each county is normally distributed around the estimate, with a specific variance parameter:

$$D^{I,\sigma}_{11,c} = D^{I}_{11,c} + \epsilon_c, \quad D^{N,\sigma}_{01,c} = D^{N}_{01,c} + \epsilon_c, \text{where } \ \epsilon \sim \mathcal{N}(0,\, \sigma_c^2) \tag{1.13}$$

In addition, I assume the policymaker is risk-averse with a standard utility function over the gain from the project and a typical constant coefficient of absolute risk aversion γ:

$$u(\pi) = 1 - \exp(-\gamma\pi), \tag{1.14}$$

where

$$\pi = \sum_{c=1}^{C} \phi_c \left[\{G_c = 1\}\, D^{I}_{11,c} + \{G_c = 0\}\, D^{I,\sigma}_{01,c} \right] + (1 - \phi_c) \left[\{G_c = 1\}\, D^{N,\sigma}_{11,c} + \{G_c = 0\}\, D^{N}_{01,c} \right]. \tag{1.15}$$

Substituting Equation 1.13 and taking the expectation, the expected utility is given by:

$$\begin{aligned} 1 - \exp(-\gamma \sum_{c=1}^{C} \phi_c & \left[\{G_c = 1\}\, D^{I}_{11,c} + \{G_c = 0\}\, (D^{I}_{01,c} - \frac{\gamma}{2}\sigma_c^2) \right] \\ & + (1 - \phi_c) \left[\{G_c = 1\}\, (D^{N}_{11,c} - \frac{\gamma}{2}\sigma_c^2) + \{G_c = 0\}\, D^{N}_{01,c} \right]), \end{aligned} \tag{1.16}$$

Applying a monotone transformation, I can reduce this problem to a quadratic problem similar to those studied in standard asset pricing texts.[19] To obtain different objective functions and a minimax criteria version, I use the same manipulation as discussed previously.

[19]See Campbell (2018) for a survey.

1.7 Results: Optimal Policy Targeting

1.7.1 CIC approaches

To illustrate how the CIC approach works, Figure 1.6 shows the relationship between the observed values and the counterfactual values of treated and control counties, respectively. The left panel shows the various effects on treated counties. The dotted orange line is the value observed at $t = 2000$ and the solid orange line is the point-identified counterfactual value estimated using the conditional independence assumption, named "FDCI". The green lines show the upper and lower bounds derived from the partial identification approach. For example, if Assumption 7 in the previous subsection holds, a county of $Quantile = 0.25$ for a treated county in 1930 have three disasters with the levee in 2000 but would have four without the levee. Therefore, the treatment effect of the levee in the county is estimated to be one. The dashed green line indicates that the number of disasters could range from 0 to 6 if there were no levees. I use the upper case to evaluate the optimal targeting exercise based on the worst-case scenario.

The right panel of Figure 1.6 shows the results of the case in control counties if counties have levees. For example, a county of $Quantile = 0.5$ for control counties in 1930 would have five water-related disasters if there were no levees but four if there were levees. The dashed green line indicates that if there were levees, the number of disasters could range from 0 to 5. When evaluating control counties based on the minimax criteria, I use the lower case to assess the optimal policy targeting.

Table 1.8 reports the estimates from the CIC approach. I present the results for five estimators: (i) the discrete CIC approach with conditional independence (ci), (ii) the discrete

CIC approach lower bound, (iii) the discrete CIC approach upper bound, (iv) the CIC approach with covariates, and (v) the CIC approach with covariates and the propensity score reweighting. I present five statistics for each estimator and their standard errors based on 1,000 bootstrap replications. The first statistic is (i) the average effect. The following four statistics are the difference in quantiles of the distribution of outcomes for the second-period treatment group and the counterfactual distribution at the four quantiles (ii) 0.25, (iii) 0.50, (iv) 0.75, (v) 0.90.

In the top panel, I present the estimated effects of levees on the number of water-related disasters in the treated group. My preferred specification is a model using propensity score reweighting and covariates labeled ”reweighted & covariates” because this model controls for the county's pre-treatment characteristics. The effect on the treated group yields an estimate of -0.648 on average, with a standard error of 0.727. The estimates at the 75th and the 90th percentiles are negative and statistically significant. The results suggest that levees effectively reduced disasters in counties where the disasters were relatively frequent in the 1930s.

I also present the estimated effects in the control group in the bottom panel. The last row of the preferred specification shows that the average estimate is -0.178, with a standard error of 0.487. The estimate at the 90th percentile is negative and statistically significant at the 95% level. The average effect for the treated group is larger than that for the control group, though the difference between the two estimates is statistically insignificant. These results suggest that the government targets counties that more effectively reduce the flood risks.

There are two important notes about Table 1.8. The first note is the relationship between the partial identification results and the confidence intervals. Consider the bounds on the CIC-discrete estimates. Imbens and Manski (2004) show how to construct confidence intervals for the average treatment effect in cases where only bounds are identified. Their approach leads to a 95% confidence interval for the average treatment effect on the treated group of [-2.60, 2.46] and the control group of [-3.80, 1.58]. Thus, under the partial identification approach I fail to reject that the levees have no effect.

The second note is about the meaning of positive values in the Table. There are positive values in the row labeled "lower." These results indicate that some counties might have *more* floods even after levees are constructed. Although it is difficult to interpret this result, the possible scenarios are as follows. The government designs levees to have a particular size and shape to withstand the corresponding floodwater level. Thus, non-targeted floods may occur in this area. Moreover, the construction of levees may prevent water that used to flow into rivers from flowing out due to the levees.

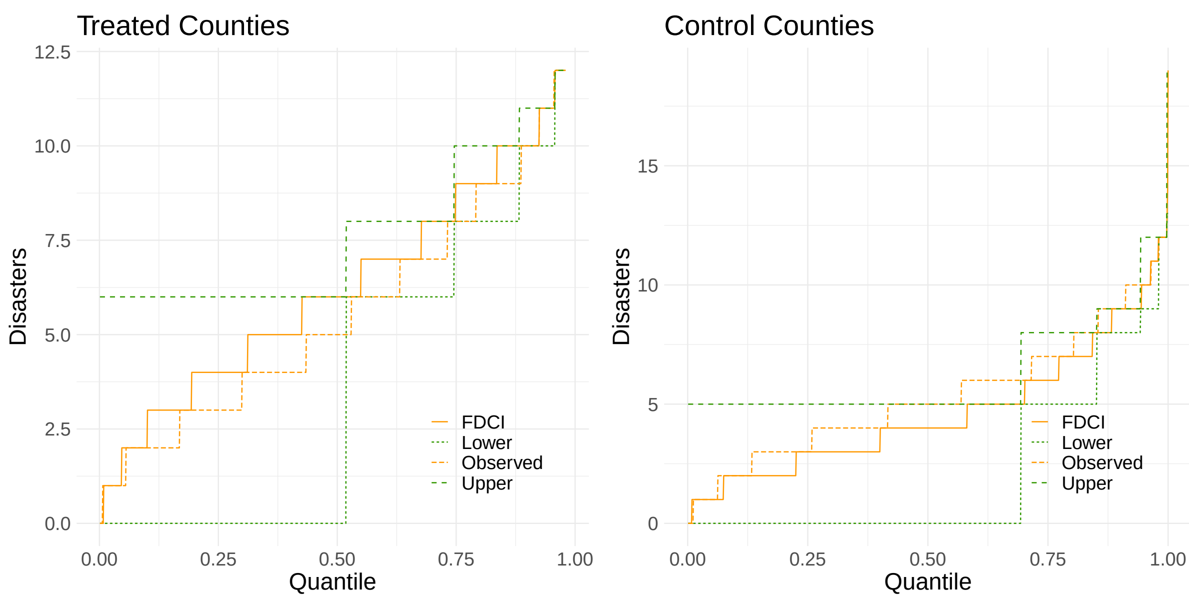
To compare the CIC results with an alternative approach of heterogeneous treatment effects, I provide a subgroup analysis. In particular, I divide the sample into two groups based on the number of disasters in 1920's: $D_{1920} = 0$ and $D_{1920} > 0$.[20] Ideally, I would use each number and divide by group size (e.g., $D_{1920} = 1, 2, \cdots$). However, since the sample sizes in these categories are small, I use the category for the subgroup analysis. Then, I use the difference-in-differences approach from the 1930s to the 2000s.

Table 1.9 shows that the effects are negative but insignificant in the group of $D_{1920} = 0$, and negative and statistically significant in the group of $D_{1920} > 0$. The results suggest that

[20] If I use D_{1930} to select the sample, this may yield a biased result because D_{1930} is a pre-treatment outcome.

counties that experienced a relatively higher number of disasters benefit more from levees. Recall that the CIC approach with covariates and the propensity score reweighting in ATT show that levees decrease disasters in the 75th and 90th percentile. Thus, the result of subgroup analysis is consistent with the CIC result, though the subgroup analysis provides the "average" treatment effect in each group.

Figure 1.6: Quantile ($t = 1930$) and Disasters ($t = 2000$)



Notes: This figure shows the relationship between the original quantile and ex-post disasters

Table 1.8: Estimates using the CIC approaches

Model	Est.	25th	50th	75th	90th
Treated Group (ATT)					
ci	−0.565	−1.000	−1.000	−1.000	0.000
	(0.345)	(0.000)	(0.510)	(0.510)	(0.765)
lower	1.940	3.000	5.000	0.000	0.000
	(0.303)	(0.000)	(2.041)	(1.020)	(0.765)
upper	−1.976	−3.000	−1.000	−2.000	−1.000
	(0.354)	(0.255)	(1.020)	(1.020)	(0.765)
covariates	−0.368	−0.008	−0.679	−0.917	−0.673
	(0.287)	(0.275)	(0.283)	(0.400)	(0.645)
reweighted & covariates	−0.648	3.020	−1.001	−3.459	−4.881
	(0.727)	(0.098)	(1.105)	(1.475)	(1.963)
Control Group (ATU)					
ci	−0.688	0.000	−1.000	−1.000	0.000
	(0.251)	(0.510)	(0.000)	(0.510)	(0.510)
lower	0.938	2.000	0.000	1.000	0.000
	(0.404)	(0.510)	(0.255)	(0.510)	(0.765)
upper	−3.225	−3.000	−5.000	−2.000	−1.000
	(0.294)	(0.255)	(0.255)	(0.255)	(0.765)
covariates	−0.362	0.033	−0.484	−0.609	−0.931
	(0.164)	(0.185)	(0.193)	(0.310)	(0.412)
reweighted & covariates	−0.178	2.995	1.788	−0.894	−4.902
	(0.487)	(0.171)	(0.230)	(0.651)	(1.412)

Notes: Each row shows the result of (i) the discrete CIC approach with conditional independence (ci), (ii) the discrete CIC approach lower bound, (iii) the discrete CIC approach upper bound, (iv) the CIC approach with covariates, (v) the CIC approach with covariates and propensity score reweighting. The column named "Est" shows the average effect. The following four statistics are the difference in quantiles of the outcome distribution for the second-period treatment group and the counterfactual distribution at the four quantiles. Bootstrap standard errors computed using 1000 replications in parentheses.

Table 1.9: Subgroup Analysis of the Number of Disasters

Outcome	Naive	OLS	reweighted & control
	(1)	(2)	(3)
$D_{1920} = 0$	-0.086	0.133	-0.085
	(0.474)	(0.167)	(0.557)
Observations	636	636	636
$D_{1920} > 0$	-0.136	-0.760	-3.075
	(0.456)	(0.397)	(1.689)
Observations	233	233	233

Notes: Robust standard errors are in parentheses. The standard errors are clustered by the river.

1.7.2 Tests for Time Invariance, Rank Invariance, and Perfect Correlation

I now discuss tests for crucial assumptions in my framework. First, I test Assumption 3 (time invariance). I use a test for the validity of the time invariance based on Melly and Santangelo (2015). Although the time invariance assumption is not testable with only two periods, I can test the assumption if the data includes more than one pre-treatment period because both groups are non-treated. This setting allows me to focus on the effect of unobservables by groups. If there are differences between counterfactual and actual distributions, the assumption may not hold. To do that, I assess whether the observed distribution equals the counterfactual distribution when imposing the policy intervention (falsely) in 1920, ten years early. The assessment serves as a placebo test.

Figure 1.7 shows the result of the exercise. The left figure shows the result from the treated group, and the right figure is from the control group. The base data used in the figure is the observed number of disasters adjusted with propensity score reweighting and the control variables discussed in the previous section. The data in 1930 yields the line named "Actual." The data in 1920 and 1930 and the CIC approach generates counterfactual lines named "CF." The "upper" and "lower" lines are the 95 percent confidence interval of the counterfactual line.

The figure shows that the "Actual" lines locate within the confidence interval on the support from the dashed red line to the solid red line. While the lower bound is 0.252 and the upper bound is 4.18 on the left figure, the lower is 0.252 and the upper is 1.65 on the right figure. Since samples outside the window may not satisfy the time invariance assumption, I perform additional analysis. In particular, I use the samples in 1930 within $D = [0.252, 1.650]$[21] and perform the CIC analysis. Table 1.10 shows the result. Since the sample size becomes small, the standard errors become large, and all estimates are statistically insignificant. Although the magnitudes become smaller than Table 1.6, most of the signs do not change. The results imply that levees decrease disaster in counties with relatively high flood risk even within the trimmed sample, though the result is imprecise.

Second, I provide tests for Assumption 7 to connect the result of the CIC estimation and the optimal targeting exercise. As I mentioned earlier, there are two concepts for the assumption: (i) rank invariance and (ii) perfect correlation. In this subsection, I check the concepts. We cannot test for rank invariance and perfect correlation directly because we do not have the joint distribution of potential outcomes. Even though there is such a restriction, researchers propose test procedures to check rank invariance. For example, Bitler et al. (2005) offers a test using the treatment and control distributions of demographic characteristics. Based on this concept, Dong and Shen (2018) and Frandsen and Lefgren (2018) extend the test procedure in endogenous situation.

I use the method proposed by Bitler et al. (2005), comparing the covariate characteristics conditional on outcome ranks, to check rank invariance for two reasons. First, the methods proposed by Dong and Shen (2018) and Frandsen and Lefgren (2018) require me to consider the outcome distribution of observationally equivalent counties across treatment status. It is challenging to construct such distributions from my sample because of the limited sample size.[22] Second, the previous empirical papers use the method to test rank invariance

[21] I use $D = 1.650$ in the treated group because Assumption 4 implies $Supp\,(Y|G=1) \subseteq Supp\,(Y|G=0)$.

[22] See Lemma 1 in Dong and Shen (2018) to understand the difference between Bitler et al. (2005) and Dong and Shen (2018) and Frandsen and Lefgren (2018).

(Djebbari and Smith, 2008; Buhl-Wiggers et al., forthcoming). According to the method by Bitler et al. (2005), under rank invariance, characteristics of units should look the same at corresponding quantiles of the treatment and control outcome distributions. For example, under rank invariance, the characteristics of counties at the 75th percentile of the control outcome distribution should mirror those of counties at the 75th percentile of the treated outcome distribution.

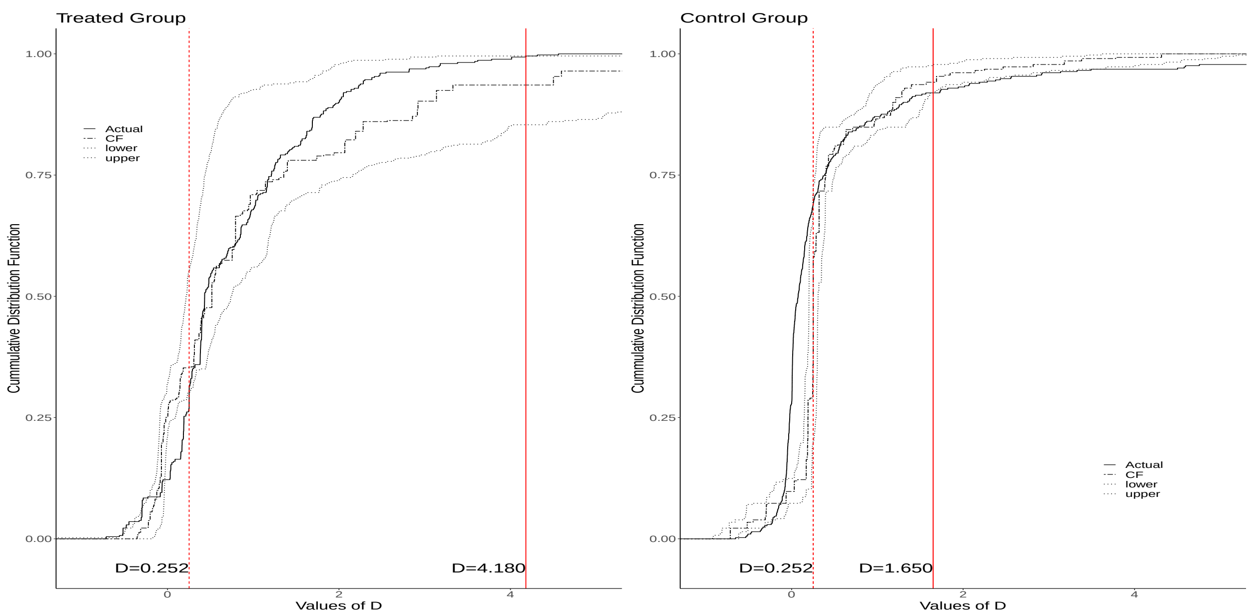
Table 1.11 shows the results of characteristic variables for population, employment, and industry using the method proposed by Bitler et al. (2005). The table classifies counties by their position (quantile) in the distribution of the number of disasters. Column 1 presents the difference in the characteristic variable between two samples within a given quartile.[23] Columns 2 and 3 show the confidence intervals calculated by the bootstrap. I reject the null in 21 out of 32 tests. The results suggest some evidence of rank reversal based on these characteristics.

Since the method checks rank invariance, the test cannot apply to perfect correlation. While the equality of unobservables over time ($U_{c0} = U_{c1}$) is not testable, one possible way to check this condition is to compare covariates similar to Bitler et al. (2005). In particular, I use population as a proxy. If the population increases, disaster risk might increase because the soil penetration rate of water decreases. I divide the sample by treatment and year, and I use quartiles to separate each group. Then, I check whether the quartile group in 1930 matches in 2000.

[23] Bitler et al. (2005), Djebbari and Smith (2008) and Buhl-Wiggers et al. (forthcoming) use quartile for the test. There is a trade-off to setting the size of sub-intervals. If I put many sub-intervals, the setting yields low power because each sub-interval has a small sample size. On the other hand, relying on a few tests runs the risk of missing departures from equality of distributions within small sub-intervals of the outcome distributions. Literature uses a quartile setting, and I also follow the strategy.

This exercise yields a mean of 0.55 for treatment and 0.57 for control. This evidence suggests the violation of perfect correlation. Since the rank invariance and perfect correlation conditions appear to be violated, I have to consider such violations in the optimal targeting exercise.

Figure 1.7: Test for the time invariance assumption



Notes: The base data used in the figure is the observed number of disasters adjusted with propensity score reweighting and control variables. The data in 1930 yields the line named "Actual." The data in 1920 and 1930 and the CIC approach generates counterfactual lines named "CF." The "upper" and "lower" lines define the 95 percent confidence interval of the counterfactual line using the bootstrap standard with 1000 replications. The dashed red lines and solid lines are the lower and upper bound that the "Actual" lines are within the counterfactual confidence interval.

Table 1.10: Estimates using the CIC approaches (trimmed sample)

Model	Est.	25th	50th	75th	90th
Treated Group (ATT)					
reweighted & covariates	−0.179	0.021	0.079	−0.147	−1.621
	(0.299)	(0.099)	(0.071)	(0.866)	(1.639)
Control Group (ATU)					
reweighted & covariates	−0.114	0.016	0.063	−0.050	−0.140
	(0.193)	(0.091)	(0.042)	(0.291)	(0.942)

Notes: Bootstrap standard errors computed using 1000 replications in parentheses. The sample size is 258 (Treated:189, Control:69). The percentiles are based on the trimmed sample.

Table 1.11: Treatment-control differences at quantiles of the distribution of the number of disasters

Variables	Quartiles	Mean Differences (1)	Lower of CI (2)	Upper of CI (3)
Log population	0-25th Perc.	0.679	−0.314	0.279
	25-50th Perc.	0.239	−0.275	0.312
	50-75th Perc.	0.487	−0.298	0.186
	75-100th Perc.	0.694	−0.215	0.249
Log employment	0-25th Perc.	0.666	−0.334	0.309
	25-50th Perc.	0.254	−0.289	0.325
	50-75th Perc.	0.513	−0.328	0.222
	75-100th Perc.	0.731	−0.224	0.277
Log # of houses	0-25th Perc.	0.597	−0.301	0.274
	25-50th Perc.	0.215	−0.263	0.300
	50-75th Perc.	0.465	−0.306	0.206
	75-100th Perc.	0.621	−0.199	0.248
Log # of farms	0-25th Perc.	0.284	−0.195	0.154
	25-50th Perc.	0.007	−0.132	0.204
	50-75th Perc.	−0.063	−0.174	0.111
	75-100th Perc.	−0.063	−0.162	0.149
Log # of average farm size	0-25th Perc.	0.236	−0.251	0.173
	25-50th Perc.	0.250	−0.318	0.087
	50-75th Perc.	0.230	−0.141	0.154
	75-100th Perc.	0.302	−0.119	0.149
Log # of farm land value per acre	0-25th Perc.	0.101	−0.169	0.203
	25-50th Perc.	0.053	−0.145	0.221
	50-75th Perc.	0.034	−0.159	0.115
	75-100th Perc.	0.138	−0.112	0.087
Agricultural employment rate	0-25th Perc.	−0.005	−0.010	0.008
	25-50th Perc.	0.005	−0.011	0.005
	50-75th Perc.	−0.001	−0.005	0.007
	75-100th Perc.	−0.007	−0.005	0.005
Manufacturing employment rate	0-25th Perc.	−0.025	−0.009	0.013
	25-50th Perc.	−0.022	−0.007	0.013
	50-75th Perc.	−0.014	−0.011	0.009
	75-100th Perc.	−0.004	−0.010	0.008

Notes: Estimates use the main analysis sample. Column 1 represents the unconditional treatment-control mean differences in the value of a given variable. Columns 2 and 3 present bootstrapped 90% confidence intervals.

1.7.3 Optimal Policy Targeting

To compare the simulation result of optimal targeting and each county's observed treatment status, I define a term named the matching ratio. To explain the term, I use Table 1.12. The table compares the observed county's treatment status with the ex-post optimal obtained by solving the constrained minimization problems. I use the minimum criteria case from Equation (1.7) and set the total number of counties $\bar{C} = 451$ as the constraint. The government strategy is less than half of the ex-post optimal: 39.2 percent in the minimum criteria case. This result suggests that when the policymaker is constrained to invest by the $\bar{C} = 451$, it can reduce the population affected by the disasters by replacing 264 treatment counties with 264 control counties in the minimum criterion case.

Table 1.13 shows the results of the optimal allocation using different policy objectives, criteria, and rank conditions. Each panel shows the results of three policy objectives for minimizing the impact of water hazards. In each panel, nine estimation results are reported based on a combination of three approaches and three ranks. I use Equation (1.12) and its variants of different policy objectives and minimax criteria and report three risk-aversion coefficient cases: $\gamma = 0, 0.5, 1$.[24]

The result indicates the following four points. First, the matching ratio varies from 33% to 63%, suggesting that investing in the control counties might have more efficiently reduced the impact of water hazards on society. Second, the weighted approach has a higher matching

[24] Previous literature estimates various values of the absolute risk aversion coefficient. Barseghyan et al. (2018) survey the literature. For example, Beetsma and Schotman (2001) use a television game show and estimate a coefficient of absolute risk aversion of 0.12. Handel (2013) yields an average coefficient of absolute risk aversion of 0.0019 from health insurance data. Bolotnyy and Vasserman (2021) estimate 0.071 from infrastructure procurement. Although these estimates are from individual or bid-level data, not from policymakers, these values are smaller than my setting. Since the small γ did not affect the matching ratios very much, I use relatively large γ to check the variance sensitivity to the matching ratios.

ratio in each panel, implying policymakers take these regional characteristics into account when constructing levees because the weighted approach considers the factors of each region using a propensity score. Third, when I consider population and GDP for targeting, the matching ratio of the conditional independence approach and partial identification improves. In contrast, the matching ratio of the weighted approach does not. This evidence also indicates that policymakers consider regional characteristics to build levees. Finally, the matching ratio decreases when I consider variance, implying that when their risk aversion is high policymakers become more skeptical about the effectiveness of levee construction. This skepticism leads to increased cases where levees should not be constructed.

Why is the percentage low? One possible reason is that the optimal allocation exercise does not consider the externality of floods. For example, if a county has a levee, the county will decrease the flood risk. However, floodwaters that had previously overflowed could overflow in a different location.[25] To consider this principle, I examine the case with a fixed actual number of counties invested in each river. I use this assumption because each area would have construction capacities such as firms and laborers to construct levees. Then I allocate these resources from downstream counties to upstream counties. The exercise yields 75 percent. The result highlights that the government considers this principle when allocating resources.

To see the geographical distribution, Figure 1.8 shows the geographical allocation of the weighted approach in Panel A as a model case. To draw this figure, I calculate the mitigation effects of each county using three different rank conditions and take the average

[25] Levee construction upper stream may increase the flood damage downstream. To mitigate the additional damages, the governments of the downstream area also build levees or increase the levee height. Wang (2021) shows that a 1% increase in the upstream levee elevation increased the downstream levee height by 0.73%.

of three numbers. Then, I allocate targets to minimize the impact of disasters. There are mainly four categories. The first is control counties that the government should not invest in, colored yellow. The second is control counties that the government should invest in, colored pink. The third one is treated counties in which levees do not work well, colored green. Finally, the fourth one is treated counties that levees work well, colored blue. While blue counties are located around California, many pink and green colored counties are along with the Mississippi River System.

Why are such counties targeted? To answer this question and shed light on the mechanism of the optimal targeting exercise, I use a logit regression where the dependent variable is equal to one if a county should invest leveed in Figure 1.8. I use economic variables in 1930 as the independent variables. Table 1.14 reports this analysis. Columns 1, 2, and 3 show that the number of farms is negative, the farmland is positive, and the farm value per acre is positive. The results suggest that counties with relatively large agricultural areas and high farmland value should have levees. Second, the number of disasters is positive, suggesting that the government should invest in levees in counties if the number of disasters is large, consistent with one's intuition. On the other hand, Column 4 uses the actual treatment assignment as the dependent variable. Although the result has similar characteristics to other columns, the magnitude of the number of disasters becomes small. The difference implies that the actual investment is based not only on the number of disasters but also on externalities and other factors.

While I have considered the optimal allocation problem by exogenously imposing constraints, we can calculate how many counties should invest to achieve a policy goal. For example, suppose the policymaker wants to eliminate one disaster once ten years. I calculated each county's outcomes using observed and counterfactual values. If the calculated outcome is minus one, the levee eliminates one disaster once ten years. I pick up counties with outcomes less than minus one. If we focus on the control counties with no levee systems in the National Levee Database, we need to invest in 91 counties to achieve this goal. Assuming that the construction cost per county is constant and that the total amount invested is 1, a 20 percent higher budget is necessary to achieve this goal. Although the construction of levees in the control counties requires detailed verification of topographical conditions and hydraulics, it can serve as a reference point for discussing the cost of physical infrastructure for water-related disasters, which have been increasing rapidly in recent years.[26]

Table 1.12: Matching Ratio

	Optimal		
Observed	0	1	$MatchingRatio$
0	154	264	41.5
1	264	187	36.8
		Overall	39.2

Notes: Observed "0" means a county did not have levees. Optimal "0" means a county should not have levees.

[26]Although estimating the construction cost per county and the total amount invested from historical records is challenging because of data limitations, there are some references on this point. For example, Aerts (2018) surveys cost estimates for different categories of flood adaptation. The cost of new river levees is $12.1-18.2 million/km in the United States. According to Shirai (2021), the average levee length per county related to USACE is 31km per county (sd: 81km). Thus, about $34-51 billion (e.g., $12.1 million/km × 31 km/per county × 91 county) would be necessary to achieve this goal.

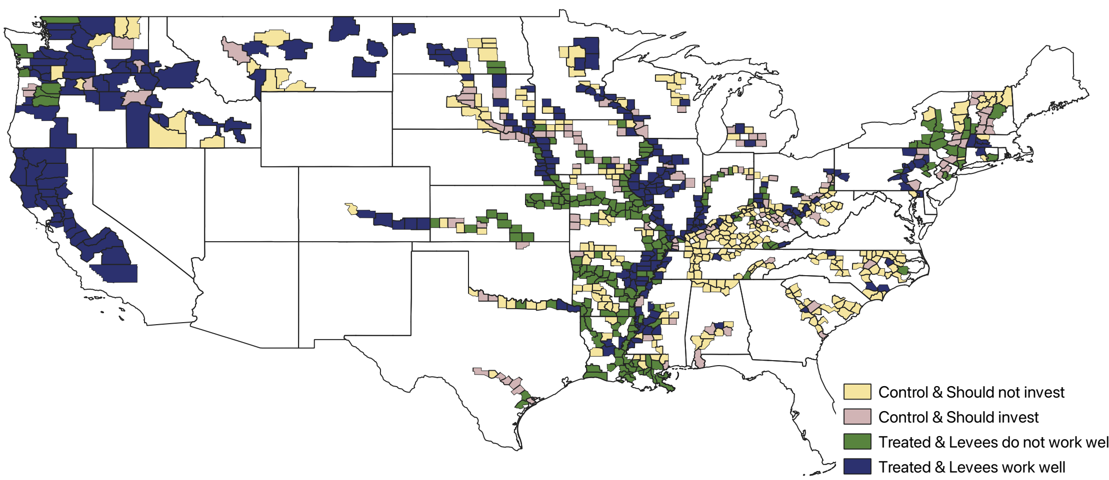
In terms of the monetary damage of a disaster, Federal Emergency Management Agency (2022) assists individuals and households through the delivery of Individual Assistance (IA) programs and local communities using Public Assistance (PA) programs. According to the data from Individual Assistance and Public Assistance programs, The average damage that a flood disaster cause is $56 million (standard deviation: $171 million) for individuals and $44 million (standard deviation: $118 million) for local communities. In my sample of 869 counties from 2000 to 2010, the average disaster declaration is 5.49 (sd: 2.97). Thus, these control counties may suffer about $49 billion (e.g., $(44 + 56) million/disaster × 5.49 disaster/county × 91 county) per decade.

Table 1.13: Matching Ratios by Policy Objectives, Criteria, and Rank Conditions

Models	Criteria	Rank	$\gamma = 0$	$\gamma = 0.5$	$\gamma = 1$
Panel A. non-weight					
naive	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.392	0.333	0.333
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.507	0.441	0.441
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.517	0.510	0.510
	Minmax	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.333	0.333	0.333
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.314	0.273	0.273
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.415	0.404	0.404
weighted	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.628	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.629	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.632	0.625	0.609
Panel B. Population in 1930-weight					
naive	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.448	0.425	0.411
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.507	0.441	0.441
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.530	0.526	0.514
	Minmax	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.333	0.333	0.333
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.367	0.346	0.330
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.443	0.434	0.432
weighted	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.628	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.629	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.632	0.625	0.609
Panel C. GDP in 2016-weight					
naive	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.459	0.443	0.429
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.507	0.427	0.420
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.528	0.526	0.524
	Minmax	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.503	0.494	0.491
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.498	0.491	0.487
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.524	0.519	0.517
weighted	Minimum	$F^{obs} = q_{1930}$ & $F^{cf} = q_{1930}$	0.628	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{2000}$	0.629	0.627	0.624
		$F^{obs} = q_{2000}$ & $F^{cf} = q_{1930}$	0.632	0.625	0.609

Notes: F^{obs} is an observed distribution in 2000. F^{cf} is a counterfactual distribution in 2000. q_t is the observed rank in time t. γ is an absolute risk averse parameter.

Figure 1.8: Geographical allocations for optimal targeting



Notes: This figure compares the simulation result of optimal targeting with observed treatment status. I use the weighted approach in Panel A of Table 1.13 and set $\gamma = 0$ as a model case. To calculate each county's outcome, I calculate the mitigation effects of each county using three different rank conditions and take the average of the three values. Then I allocate targets to minimize the impact of disasters.

Table 1.14: Determinants of the optimal targeting

	All (1)	Treated (2)	Control (3)	Observed (4)
Constant	−13.310	−10.528	−26.953	−3.056
	(2.710)	(3.387)	(5.831)	(2.482)
Log population	−3.627	−2.051	−4.381	−4.986
	(1.074)	(1.394)	(2.093)	(1.009)
Log employment	0.725	0.508	−0.420	2.498
	(0.997)	(1.222)	(2.046)	(0.936)
Log # of houses	3.520	1.635	7.136	3.372
	(1.097)	(1.419)	(2.064)	(1.023)
Log # of farms	−1.566	−1.588	−2.495	−1.005
	(0.276)	(0.355)	(0.578)	(0.264)
Log farmland	1.799	1.810	2.326	0.637
	(0.181)	(0.247)	(0.357)	(0.150)
Log farm value per acre	0.982	0.687	1.535	0.751
	(0.168)	(0.224)	(0.311)	(0.155)
Agricultural employment rate	1.044	1.530	2.710	1.465
	(1.227)	(1.518)	(2.422)	(1.147)
Manufacturing employment rate	1.749	4.162	−1.473	−1.347
	(1.896)	(2.305)	(3.584)	(1.780)
# of disasters	0.888	0.891	1.024	0.451
	(0.090)	(0.126)	(0.166)	(0.077)
Observations	869	451	418	869

Notes: Columns 1, 2, and 3 report results from a logit regression where the dependent variable is equal to one if a county should have levees based on the setting in Figure 4. Column 2 uses only treated counties, and Column 3 uses control counties. Column 4 reports the result from a logit regression where the dependent variable equals one if a county is treated. The independent variables are measured in 1930. Standard errors are in parentheses.

1.8 Conclusion

Infrastructure planning is becoming more important to the political agenda. Evaluating adaptive measures for climate change is also essential. There have been few studies on levees, a type of infrastructure designed to mitigate flood risks, and their impact on the local economy. Given the increase in disasters in recent years, it is an important policy issue to consider whether the investment allocation was appropriate and the cost needed to achieve a given policy goal.

This paper examines (i) the effect of levee construction on local economic development and (ii) the optimal targeting exercises for mitigating the total effect of water-related disasters. For the first question, I use the Flood Control Act in 1936 as an exogenous shock for the whole U.S. levee systems construction and the 308 program, a foundation survey for the U.S river system development, to define the control. Then, using a standard difference-in-differences approach with propensity score reweighting, I cannot find clear evidence that the presence of levees leads to an increase of population, economic development, or a decrease of flood risks. For the second part, I use the Changes-in-Changes approach and find heterogeneous effects across treatment and control groups. Moreover, I apply statistical decision theory. I find the matching ratio, an index comparing actual versus optimal treatment allocation, from 33 percent to 63 percent, though the allocation exercise does not consider the characteristics of floods. Finally, for a future policy implication, I show 20 percent more budgets are needed to reduce one disaster every ten years.

Chapter 2

The Employment Effects of Levee Investments

Infrastructure can play an important role in increasing long-run output and standards of living. In the short run, infrastructure investment is used to stimulate local labor markets. Despite its widespread use, researchers and policymakers debate whether infrastructure investment constitutes an effective tool in counter-cyclical fiscal policy. Ramey (2020) points out that there is scant empirical evidence that infrastructure investment has a short-run stimulus effect. While there are many types of infrastructure, recent studies have primarily focused on highway construction. The employment effects of infrastructure are likely to be heterogeneous because of differences in government contracting processes, the size of businesses targeted, and the type of location targeted. Moreover, while previous literature has evaluated employment multipliers and implementation lags in infrastructure stimulus programs, evidence of seasonal cycles is still scarce. Tschetter and Lukasiewicz (1983) and Geremew and Gourio (2018) point out that seasonality is a well-known characteristic of the construction industry, which is a primary target sector for infrastructure investment. To design a counter-cyclical fiscal infrastructure policy, such evidence would be essential.

This paper examines the impact of the levee investment projects under the American Recovery and Reinvestment Act (ARRA) of 2009 on local labor markets, focusing on the following factors: (1) employment multipliers, (2) implementation lags, and (3) seasonal cycles. I use levee projects to examine the employment effects of the ARRA for three reasons. First, levee projects have a different institutional design to highway projects. Previous empirical analyses (Wilson, 2012; Conley and Dupor, 2013; Leduc and Wilson, 2013, 2017) have exploited formula-based mechanisms to examine the effects of highway projects. Unlike highways, the majority of federal funds for levees are not distributed by a formula to states or through competitive grant programs (Carter, 2018). Second, the transaction amount per levee project is likely to be small. While the mean transaction amount in highway projects is $2.1 million, in levee projects this amount is $1.2 million. This means that levee projects may target smaller businesses. Third, levee construction is likely to be seasonal. For example, it may be difficult to construct levees when the water level is high. Such difficulties may lead to implementation lags and seasonal cycles. These three differences between levee and highway projects provide new interpretations of fiscal multiplier.

The main challenge in obtaining accurate results is that the amount of investment a county receives depends on the county's economic conditions. Policymakers deliberately selected infrastructure projects that would result in immediate employment. One may expect that counties with severe economic conditions (e.g., a high unemployment rate) received more investment compared to counties with less severe economic conditions. If so, the simple linear relationship between investment and changes in employment ratio understates the true effect of levee investment. To address this concern, one approach is to use instrumental variables. Previous literature on highway investment has used pre-recession formulas as instrumental variables to eliminate endogeneity problems related to highway funding. These formulas include pre-recession total lane miles of federal highway, total vehicle miles traveled on federal highways, tax payments paid into the federal highway trust fund, and Federal Highway Administration obligation limitations.[1] Since policymakers for levees do not use formulas for the distribution of funds, it is not possible to use formula-based approaches to constructing instruments to evaluate levee projects.

To insure against the concern that the apportionment of grants may be correlated with pre-existing labor market outcomes, I use an instrumental variable to isolate the component of the investment unrelated to changes in economic circumstances. In particular, I address this endogeneity problem by exploiting the length of levees from the National Levee Database (NLD). The NLD provides the exact locations of levees in the U.S. Levee length is necessarily correlated with investment but uncorrelated with economic conditions. County-level levee length is correlated with investment because governments must maintain these levees. On the other hand, the length of levees is not correlated with short-run fluctuations in economic circumstances. These assumptions allow me to use the length variable as an ideal instrument for local investments.

There are three main findings. First, the levee investment projects had a positive effect on employment. My dynamic specification shows that the positive effects continued until the end of the program. The instrumental variable (IV) estimates imply that an additional

[1] The obligation limitations are a ceiling on the obligation of contract authority that can be made within a fiscal year.

investment of $100,000 led to a gain of 4.2 job years in total employment, of which 1.8 were in the construction sector. This point estimate corresponds with a cost per job of $24,000 for total employment and $55,000 for the construction sector. There are a number of recent academic studies analyzing the effect of ARRA spending on employment. Chodorow-Reich (2019) summarizes empirical cross-sectional multipliers, comparing recent empirical studies in this field. He finds that the 90 percent confidence interval for the "cost-per job" is ($25,500, $73,900). While his estimates are based on cross-state regressions, my estimates use cross-county regressions. Though the difference of approaches may yield different results, levee projects may be a particularly low-cost means to support employment during a recession.

Second, the projects had short implementation lags. Policymakers and researchers may be concerned that the implementation of the program might show a lag between the date that budget plans were approved and the timing of the positive employment effects. Typical infrastructure projects might experience implementation lags for a number of reasons. For example, construction companies need to bid to be awarded a project. After they win the bid, they must survey local geological conditions for the project. Although such processes take time, they do not require many workers. Following this, construction companies can start major construction work and hire many workers. However, in this study levee projects had immediate effects on employment. This finding alleviates common concerns about the implementation lags of levee projects, suggesting that levee projects are an efficient way to create employment effects quickly.

Finally, the findings take seasonal cycles into account. Seasonality is a well-known characteristic of the construction industry. While constructing highways in the summer is suitable, building levees in the summer would be challenging because the higher water level disturbs safe and efficient construction conditions. I find that the estimates for winter are relatively large and statistically different from zero in terms of both total employment and construction employment. Moreover, the estimate for winter in the construction sector is statistically different from those for autumn and summer. This means that seasonal factors should be considered in levee construction. My result suggests that the seasonal cycle is an important characteristic to consider in the design of a public stimulus package related to infrastructure projects.

As a robustness check, I perform two falsification tests to assess whether the instrument satisfies the exclusion restriction—that is, that the instrument is not correlated with the error term in the equation of interest. The first test is a reduced-form regression, using data from between January 2000 and August 2009. My IV strategy rests on the assumption that levee investment is the only channel through which the length of levees affects employment outcomes. If this assumption is correct, then a positive relationship between the length of levees and employment outcomes should not exist before the commencement of levee investment. There is little evidence that in the years before the ARRA was passed, counties experienced systematically different employment trends. The second test is using an observable variable, housing prices, as a proxy for unobservables, such as geographic conditions, to check the correlation between the instrument and unobservables. There is a possibility that levee length may be correlated with other unobservables. If this were the case, the correlation would violate the exclusion restriction. There is little evidence that the instrument is correlated with the housing price data proxy. The results of these falsification tests strengthen the case that the IV estimates in this study reflect the causal impact of the ARRA investment on subsequent employment change, rather than a spurious correlation due to omitted factors.

I provide two extensions of the baseline results. First, I check the heterogeneity of the effect of levee projects. I divide counties into two subgroups by using the median population and apply a regression analysis to each subgroup. I find that the estimate in smaller counties for construction employment is positive and statistically significant, though the differences between the two subgroups are not statistically significant, Second, I provide the effect of levee projects on high school completion. A model of human capital predicts that an increase in the wages of workers with low education relative to wages of workers with high education will reduce investment in schooling because the returns of additional years of schooling are diminished. I find little evidence that this infrastructure investment affects high school completion rates.

My analysis contributes to a growing empirical literature that studies local economies in order to estimate fiscal multipliers, leveraging cross-state or cross-county variation in government spending. This paper is the first, to my knowledge, to explore the cross-county variation in levee investment to estimate its employment effects by using monthly data. A number of papers have estimated the combined effects of the different stimulus measures (e.g., transfers to individuals, financing of public employment, and infrastructure investments) included in the ARRA (Feyrer and Sacerdote, 2011; Chodorow-Reich et al., 2012; Wilson, 2012; Conley and Dupor, 2013). While meaningful cross-sectional variation in spending is confined to the state level because of a lack of identifying variation in the data, some studies have examined the employment effects of investment with county-level data (Dupor and Mehkari, 2016; Dupor and McCrory, 2017; Dube et al., 2018; Garin, 2019; Popp et al., 2020). Chodorow-Reich (2019) and Ramey (2020) survey this literature in detail. The most closely related work to this study is that of Garin (2019). I use a new instrumental variable to examine the effect of levee projects, while Garin (2019) uses a difference-in-difference approach to analyze highway projects. Moreover, while he examines the yearly effects on employment, I examine monthly effects on employment to evaluate detailed implementation lags and seasonal cycles in the short run.

The remainder of this paper proceeds as follows. In Section 2.1, I provide background information about the ARRA stimulus package. Section 2.2 describes data sources. Section 2.3 contains the econometric methodology and identification strategy for the instrumental variable. Section 2.4 presents and discusses the results. Section 2.5 provides analyses of robustness and extensions. Section 2.6 offers some concluding remarks.

2.1 Background

The financial crisis between 2007 and 2009 caused a recession (the Great Recession) in the U.S. and world economies. The crisis began on Wall Street, pushing the U.S. economy into recession in December 2007. From a low of 4.4% in March of that year, the unemployment rate rose steadily, peaking at 10.0% in October 2009. The employment-population ratio dropped from 63% in 2007 to 58% in 2009, a loss of 8.6 million jobs. At the bottom of the recession, real GDP was more than 7% below potential. Some local areas experienced more Great Recession shocks than other local areas. For example, local areas that had previously experienced housing booms were disproportionately affected (Mian and Sufi, 2014).

After this rapid decline in economic activity, the United States Congress passed the ARRA on January 6, 2009. On February 17, 2009, President Obama signed into law the ARRA, a $787 billion package designed to stimulate aggregate demand in the economy. The final plan included more than $250 billion in tax cuts and more than $500 billion in new government spending on such things as unemployment benefits, infrastructure, education, health care, and aid to state and local governments. According to the Congressional Budget Office, about $185 billion of the stimulus was disbursed in 2009, followed by another $400 billion in 2010. There were two main purposes for this law: the first one was to save or create at least three million jobs by the end of 2010 (Romer and Bernstein, 2009), and the second was to invest in transportation, environmental protection, and other infrastructure that would provide long-term economic benefits. Proponents of the Recovery Act emphasized the bill's supplemental funding for "shovel-ready" infrastructure projects, or construction work that could begin immediately once funded. Increasing labor demand in the construction sector was a priority, as the housing bust precipitating the recession had particularly affected construction-sector spending and employment.

The ARRA provided funding to the U.S. Army Corps of Engineers (USACE) to accomplish these goals through the improvement of water-related infrastructure. The legislation appropriated $4.6 billion to USACE for a program known as Civil Works projects. Civil Works projects utilized several kinds of accounts. For example, $2 billion was allocated for the construction account and $2.075 billion was provided for the operation and maintenance account. The Mississippi River and Tributaries account received $375 million in appropriations. USACE identified many potential Civil Works projects that met the criteria of the legislation for funding. Selected projects were distributed across the U.S. and across USACE programs to provide the nation with inland and coastal navigation, environmental, flood risk management, hydropower, and recreational resources. This paper focuses on levee projects because the institutional design underlying the geographical distribution of levees helps to identify shocks to county-level investment.[2]

USACE is directly engaged in planning and construction projects, with the agency's appropriations used to perform work on geographically specific projects. Additionally, the ARRA announced project selection criteria for projects that would: (1) be obligated/executed quickly; (2) result in high and immediate employment; (3) have little schedule risk; (4) be executed by contract or direct hire of temporary labor; and (5) perform its functions, such as flood protection, in the local areas without additional funding. The second and fourth factors raise concerns over endogeneity, motivating the instrumental variable approach.

According to USACE (2010), spending plans were approved on August 19, 2009. Therefore, the data points used for analysis were from August 2009 onwards. Although the estimates evolve over time (see Section 2.4), the estimates of changes between August 2009 and February 2010 are reported as a baseline result because February 2010 was one year after the ARRA was enacted. The program was expected to finish by September 30, 2011. My primary outcome data source, the Quarterly Census of Employment and Wages (QCEW), provides monthly employment data on the 12th day of the month. The final data point used for analysis was October 2011.

[2] The levee projects in ARRA included building new levees, improving levee systems (e.g., controlling stability and leakage against floods), and safety inspection. For example, the most expensive project was the Central and South Florida site one impoundment project. The budget was about $44 million. This project strengthened the existing l40 levee with borrowed material and armored the top and banks with soil cement and turf reinforcement mat. Also, this project included removing existing structures and borrowing the construction site from land owners.

2.2 Data

Baseline Setting.— The main analysis is based on counties with levee projects related to the ARRA. Since the ARRA does not require reports of trade less than \$25,000[3] and the focus of this study is the intensive margin of the labor demand, I exclude counties with less than \$25,000 in outlays. The main drawback to limiting the data is that my sample size is small, so I also report results from all counties in the next section. I normalize variables by working-age population (between 15 and 65 years of age) from the dataset of the Survey of Epidemiology and End Results[4] for three reasons. First, levee projects in populated areas are likely to have increased costs. For example, the wages for construction workers and the cost of renting land for projects in populated areas would be higher than in rural areas. Additionally, it likely takes more time to reach a consensus on projects in populated areas because project stakeholders are likely to be large, and project management is high. As a result, the project length is likely to be longer, leading to higher costs because the firms are likely to rent construction machines longer. To control for these effects, I use normalization. Second, when I regress the change of employment on the level of government investment, my instrument is weak, which may lead to biased results. Third, previous literature on empirical cross-sectional multipliers (e.g., Chodorow-Reich, 2019) typically uses normalization.

Outcome Variable.— My primary outcome variable is the change in private-sector employment, normalized by working-age population. I obtained monthly county-by-sector employment totals from the Quarterly Census of Employment and Wages (QCEW), a product

[3]Recipients who received more than \$25,000 are required to report information on the use of funds on Recovery.gov.

[4]The tractable data is available at https://www.nber.org/research/data/survey-epidemiology-and-end-results-seer-us-state-and-county-population-data-age-race-sex-hispanic

of the Bureau of Labor Statistics (BLS). The QCEW data is compiled from administrative establishment-level records collected by state unemployment insurance systems. The resulting dataset includes monthly average employment (full-time and part-time) and salary levels, broken down by county and industry. Infrastructure projects may affect economic activity in other industries. On the one hand, it is possible that an increase in production in the construction industry boosts local demand for intermediate goods and services (i.e., purchasing construction materials). On the other hand, the addition of jobs in the construction sector may crowd out jobs in other industries. I select industries related to construction, including Mining, Manufacturing, Wholesale Trade, Finance and Insurance, and Other services, and define the sum of these industries as total employment.[5] I analyze the effect of levee projects on employment in terms of both (1) total employment and (2) employment in the construction industry.

Endogenous Variable.— My primary endogenous variable is the county-level levee investment, normalized by working-age population. I identify levee projects and locations using reports through Recovery.gov.[6] I retrieved data from FedSpending.org, selected the "Department of Defense" category, and filtered by "U.S. Army Corps of Engineers - civil program financing only". There are many types of Civil Works projects, including navigation, environmental, and recreational programs. To select only levee projects, I used keywords in the description of each project. First, I selected projects that had keywords such as "flood", "dike", "pump", and "levee". Second, since the first selection process was not sufficiently nar-

[5] Carrington (1996) focuses on the short-run multiplier generated by the construction of the Trans-Alaskan Pipeline System. He finds evidence that the increase in construction jobs caused by the System had strong employment growth for jobs in other parts of the sector in Alaska.

[6] This website is no longer available, but archived data are available at `http://data.nber.org/data/ARRA/` and `https://www.fedspending.org/rcv/index.php?reptype=a`.

row to select only levee projects, I excluded projects with keywords such as "dam", "port", "lock", and "harbor".[7] Although the dataset does not include county name, I pinpointed the counties in which these projects were located by using "Place of Performance Latitude" and "Place of Performance Longitude". Based on the project locations, I aggregated data at the county level. Although Wilson (2012) uses actual payments reported in weekly Financial and Activity Reports and cumulates the payment to analyze the impact of the investment, most of the literature (e.g., Chodorow-Reich et al., 2012; Leduc and Wilson, 2017) uses total outlays as a one-time shock. I also use levee investment as a one-time shock, because weekly reports are not available at the county level. Given this, the identification of the effect of investment on employment depends on cross-sectional variation in investments across counties. Figure 2.1 plots the geographical variation in investments, with counties shaded according to investments. It shows that there is ample variation in investments across counties. In particular, investments were focused around (1) the confluence of the Mississippi River and the Ohio River, (2) Louisiana, and (3) Florida.

Instrumental Variable.— The instrumental variable is county-level levee length, normalized by working-age population. This data comes from the National Levee Database (NLD).

[7] All keywords to select levee projects were "flood", "levee", "river", "pump", "revetment", "floodway", "dike", "bank", "seepage", "improvement", "excavation", "dredg*", "maintenance", "well", "inspection", "stabilization", "spillway", and "erosion". The process yielded 1495 projects. However, this included projects unrelated to flood projects. Thus, I checked projects one by one and excluded other kinds of projects were "dam", "port", "bay", "lock", "harbor", "reservoir", "navigation", "lake", "jetty", "gulf", "traffic", "waterway", "inland", "ship", "boat", "vessel", "water", "savannah", "garrison", "Atlantic", "canaveral", "treatment", "sewer", "pipe", "alaska", "Hawaii", "erie", "Galveston", "jacksonville", "intracoastal", "environmental", "restoration", "roof", "herbicide", "Saugatuck", "clinical", "channel", "picnic", "keys", "street", "park", "road", "grounds", "irrigation", "aquatic", "wetland", "recreational", "tractor", "hospital", "building", "vehicle", "estuary", "oxygen", "tuckahoe", "storage", "museum", and "willamette". I exclude these words because some projects are unrelated to the levee project. For example, I exclude "water" because all projects that include water were related to improving drinking water facilities or water trade improvement. I also exclude some area names, such as "Galveston", because the project is related to water navigation improvement, not related to flood control improvement. I get 384 projects using the exclusion process.

The NLD is developed by USACE and includes comprehensive information about levees in the U.S. In particular, the database contains levee location, levee length, leveed area (e.g., areas protected by levees from floods), and the year in which each project was completed. The database contains information to facilitate and link activities, such as flood risk communication, levee system evaluation for the National Flood Insurance Program (NFIP), levee system inspections, flood plain management, and risk assessments. There are 9,084 levee systems in the U.S. The NLD provides levee systems data in many formats, including GeoJSON and Shapefiles, a common format for geographical vector data. I downloaded GeoJSON files because QGIS plugins do not work with Shapefiles from the NLD. I restricted the data to the categories "USACE federally constructed and USACE federally operated" and "USACE federally constructed, turned over to public sponsor operations and maintenance". I also excluded systems completed by August 1, 2009, the first day of the month in which the spending plan was approved. This restriction yields 1,246 levee systems. The dataset includes polygon data and inspection data for levees. Unfortunately, this dataset is not available at the county level. To address this issue, I used GIS software (QGIS) plugins, including "dissolve", "intersection", and "measure length" on PyQGIS, to calculate county-level data.

Figure 2.2 shows the levee systems related to USACE. Blue objects represent the locations of levees. There are two geographical features in this figure. First, counties along the Mississippi River and the Sacramento River have greater total levee length because the Flood Control Act of 1928 authorized USACE to design and construct projects for flood control on the Mississippi River and its tributaries, as well as the Sacramento River. Second, counties located in southern Florida have levee systems because of the Central and Southern Florida Project in this region. Therefore, the length of levees related to the federal government in these regions is greater than in other regions.

Control Variables.— My baseline specification includes two control variables that may be simultaneously correlated with employment outcomes and levee investment. First, I control for employment level normalized by working-age population, because one would expect the baseline employment level to affect subsequent employment change. In particular, I use the level of employment normalized by working-age population in February 2009, when the ARRA was enacted. As mentioned in the last section, this spending plan was approved in August 2009. I use the employment conditions in February 2009 because this data would have been used by policymakers to make a project list. In fact, lobbyist communication occurred between March 2009 and June 2009.[8] Thus, this employment variable should be correlated with levee investment and employment outcomes. Second, I control for employment trend normalized by working-age population because employment changes are highly persistent. I use the previous year's employment trend, from August 2008 to August 2009.

Table 2.1 presents summary statistics for the main variables used in the study. The average changes in total employment and construction employment were negative, though there is considerable cross-county variation in this pattern. The average investment was about $2.3 million and the average total levee length was 31 kilometers per county. These variables also showed considerable variation.

[8]The records are available at `https://www.usace.army.mil/Recovery/Lobbyist-Communication/`.

Figure 2.1: County-level Levee Investments via the ARRA ($)

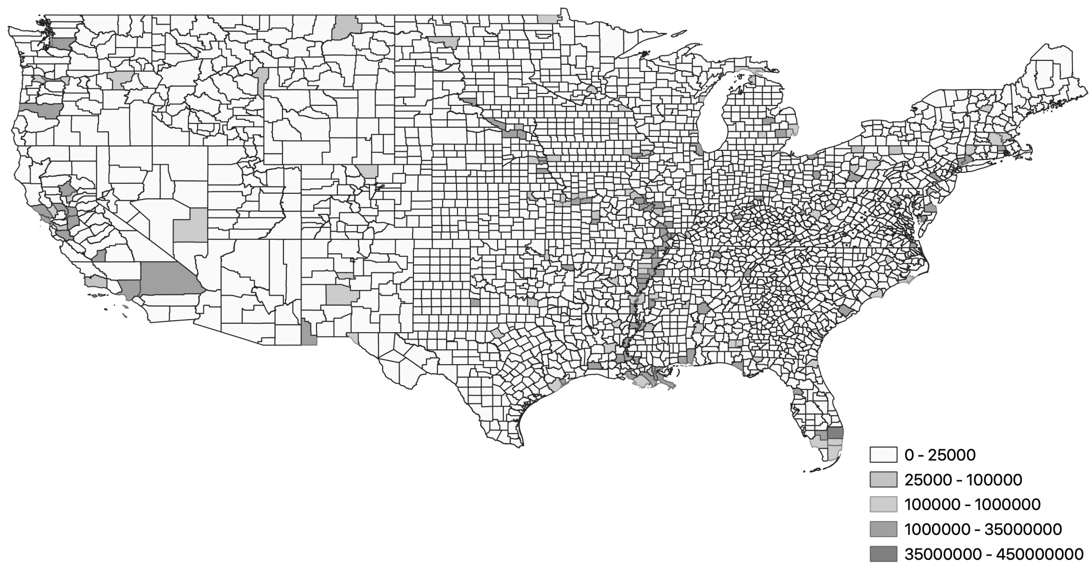


Figure 2.2: Levees related to USACE

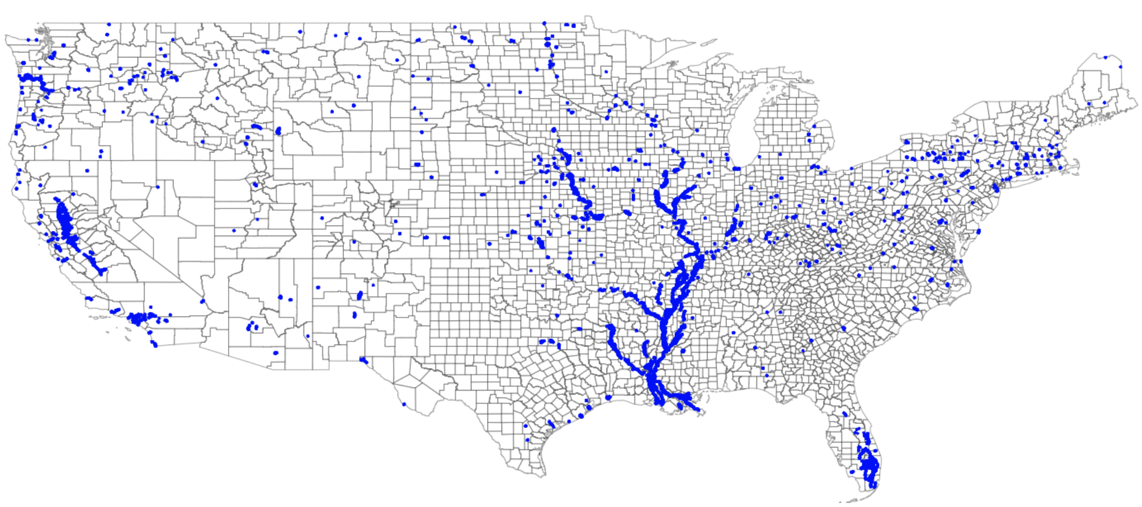


Table 2.1: Summary Statistics

	Mean	SD	Min.	Median	Max.
Outcome variables					
Δ Const Related Emp, Aug 09 $\rightarrow$ Feb 10	−2,149	4,753	−40,814	−451	5,614
Δ Const Emp, Aug 09 $\rightarrow$ Feb 10	−1,067	2,291	−21,273	−269	8,995
Endogenous variable					
Investment in Levees (M$)	2.275	4.236	0.027	0.880	44.125
Instrumental variable					
Length of Levees (kilometers)	31	81	0	0	780
Control variables					
Const Related Emp Level, Feb 09	44,631	105,961	0	10,005	1,158,294
Const Emp Level, Feb 09	7,010	13,187	0	1,834	123,508
Δ Const Related Emp, Aug 08 $\rightarrow$ Aug 09	−4,890	11,556	−116,374	−971	5,770
Δ Const Emp, Aug 08 $\rightarrow$ Aug 09	−1,519	3,770	−31,593	−248	12,275

Notes: All variables are the nominal term. See text for sources. Note that employment is from the private sector.

2.3 Methodology

The goal of the empirical strategy is to assess both the dynamic employment response and the overall employment effect of the investment projects. To this end, I use an instrumental variable approach to estimate investment-induced employment gains. To understand the framework and concerns, I begin with a simple linear framework that relates levee investment to employment. The change in the ratio of employment in industries to potential workers in a county, c, depends on levee investment that the county receives, a series of controls that capture differential trends, and a county-specific shock:

$$\frac{E_{t,c} - E_{Aug\ 2009,c}}{N_c} = \beta_{0,t} + \beta_{1,t}\frac{G_c}{N_c} + \beta_{2,t}Controls_c + \epsilon_{c,t}, \tag{2.1}$$

where $E_{t,c}$ is the employment in the construction industry in county c in period t, N_c is the number of potential workers in the county c, $\beta_{0,t}$ is national-level shock, G_c is the investment in levees received by county c, $Controls_c$ are county-level controls in the county c, and $\epsilon_{c,t}$ is a county-level mean-zero shock. As discussed in the data section, I use $t = February$ 2010 for my baseline results. I also report the dynamic effects of levee investment on employment using 26 different months.

If county levee investment per potential worker, $\frac{G_c}{N_c}$, were uncorrelated with the error term, ϵ_c, then Equation (2.1) could be estimated with bivariate OLS. However, this assumption may not be valid. As mentioned above, the ARRA levee projects depended on five factors: projects needed to (1) be obligated/executed quickly; (2) result in high and immediate employment; (3) have little schedule risk; (4) be executed by contract or direct hire of temporary labor; and (5) complete a project phase, complete a project, or provide a useful service that did not require additional funding. These factors, especially (2) and (4), may be correlated with post-stimulus economic conditions. For example, to result in high and immediate employment, counties with the most severe economic conditions may have received more investment through USACE compared to counties with less severe economic conditions. If so, the OLS relationship between infrastructure investment and changes in employment ratio would understate the true effect of infrastructure investment.

I address this concern by using an instrument that isolates the component of the investment unrelated to changes in economic circumstances. Specifically, I implement a two-stage least squares estimation, using the total levee length per capita as an instrument to evaluate the ARRA investment. The instrument satisfies the following conditions: (1) instrument relevance and (2) instrument exogeneity. Instrument relevance means that the total levee length is correlated with spending. If a county has a longer total levee length, the investment should be higher to maintain this physical capital. Instrument exogeneity means that the length of levees is not correlated with the error term of Equation (2.1). The existing total levee length is not affected by short-run economic fluctuations. For these reasons, it constitutes an ideal instrument for local investments. I report empirical evidence for these assumptions in the next section.

2.4 Results

2.4.1 Relevance and Exclusion Restriction

One main assumption of the IV strategy is that the instrument is relevant; that is, that the total existing levee length is a predictor of investment. Typically, the relevance of the instrument is tested using the first stage of the IV model. Table 2.2 shows the results of first-stage regressions. Panel A uses my preferred sample set: counties with positive investment. Panel B uses all counties as a sample. The outcome variable is the investment in levees, normalized by working-age population and measured in $100,000 increments. The coefficients in Table 2.2 represent the average increase in investment (in increments of $100,000) due to one additional kilometer of levees. One additional kilometer of levees is associated with an increase in investment of around $29,000. That is, counties with longer total levee length have to spend more to maintain them. The positive coefficients are consistent with this pattern.

Column 1 in Panel A presents a simple bivariate regression. The coefficient for the first instrument (levee length) is 0.29, with a partial F-statistic of 13.41. The coefficient is precisely estimated. Columns 2 to 5 show that this positive and precisely estimated relationship between the instrument and my main endogenous variable is robust to the inclusion of a number of covariates. As argued by Stock and Yogo (2005), we do not need to worry about a weak instrument if the first-stage F-statistic exceeds 10.[9] Since this partial F-statistic is above the conventional critical value of ten, the first stage in Panel A is strong when I use my preferred sample.

[9] Lee et al. (2021) discuss the statistical characteristics of the threshold.

On the other hand, Panel B shows that the instrument becomes weak when all counties are used as a sample, because the robust standard errors become relatively large. While some counties received levee investments, other counties with similar total levee lengths did not. One possible reason is that factors other than the length of levees are considered when policymakers accept the project. The ARRA required that projects be obligated quickly and have little schedule risk. This means that policymakers would have reviewed factors such as the progress of the ground survey for the construction site and the consensus in neighborhoods for the project, both of which are unobservable to researchers. When all counties, including zero investment, are used as the sample, these components affect the residual in the first stage and the F-statistic becomes low.[10]

The second main assumption of the IV strategy is the exclusion restriction, which requires the errors $\epsilon_{c,t}$ to be independent of the instrument. This implies that levees may be correlated with employment outcomes only via their effect on investment. The length of levees in the United States is very persistent over time, and therefore is unlikely to be correlated with economic conditions in the short or medium run. Figure 2.3 illustrates this persistence by plotting the length of levees in 2009 against the length of levees in 1989. The right-hand graph is normalized by working-age population. The data is tightly clustered around the 45-degree line. This demonstrates that there are at most minor changes in the length of

[10] Using a dummy variable in Equation (2.1) provides a different view of this discussion. In particular, consider an analogous version of Equation (2.1):

$$\frac{E_{t,c} - E_{Aug\ 2009,c}}{N_c} = \beta_{0,t} + \beta_{1,t} T_c + \beta_{2,t} Controls_c + \epsilon_{c,t},$$

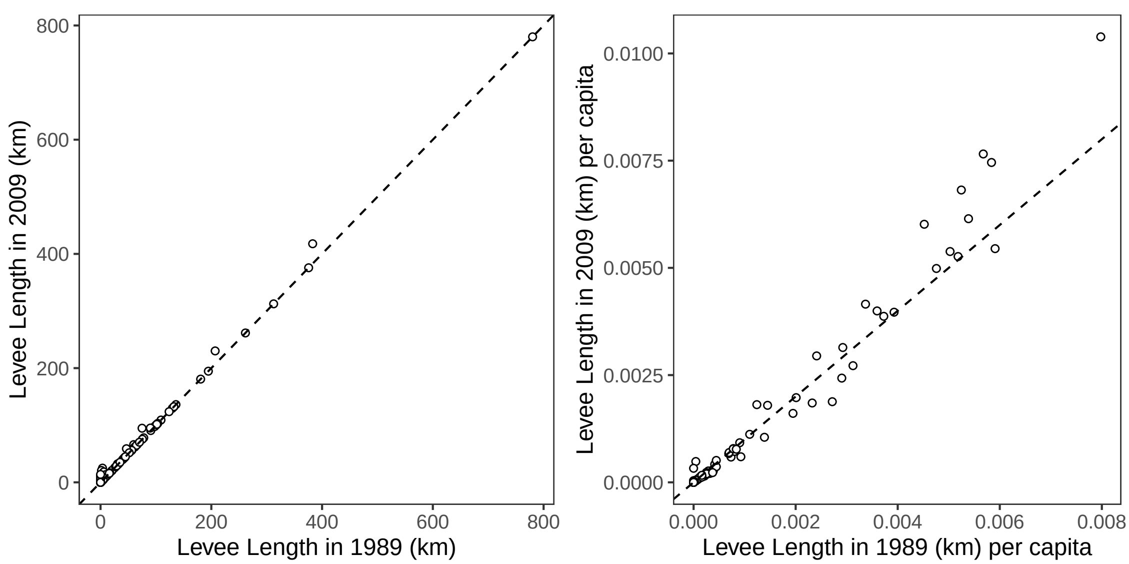
where T_c is a dummy variable for whether or not county c has levee investment. When I use the baseline setting ($t = February$ 2010) in the next subsection, the OLS shows that the coefficient $\beta_{1,t}$ is positive and statistically significant, which means that the existence of a levee project and the subsequent employment growth show a positive correlation.

levees over time.

The age distribution of levee systems from the NLD offers another view of this persistence. Figure 2.4 displays the age distribution of levee systems. The mean is 1967, with the data indicating that the existing stock of levees was constructed during the 1950s and 1970s. The current demand for levees is declining and maintenance costs are increasing. The total levee length is therefore predominantly determined by policy decisions made before the 1970s. This means it is likely that the total levee length is independent of employment outcomes during the recession of 2009.

Section 2.5 reports further evidence on the exclusion restriction.

Figure 2.3: The Autocorrelation of Levee Length between 1989 and 2009



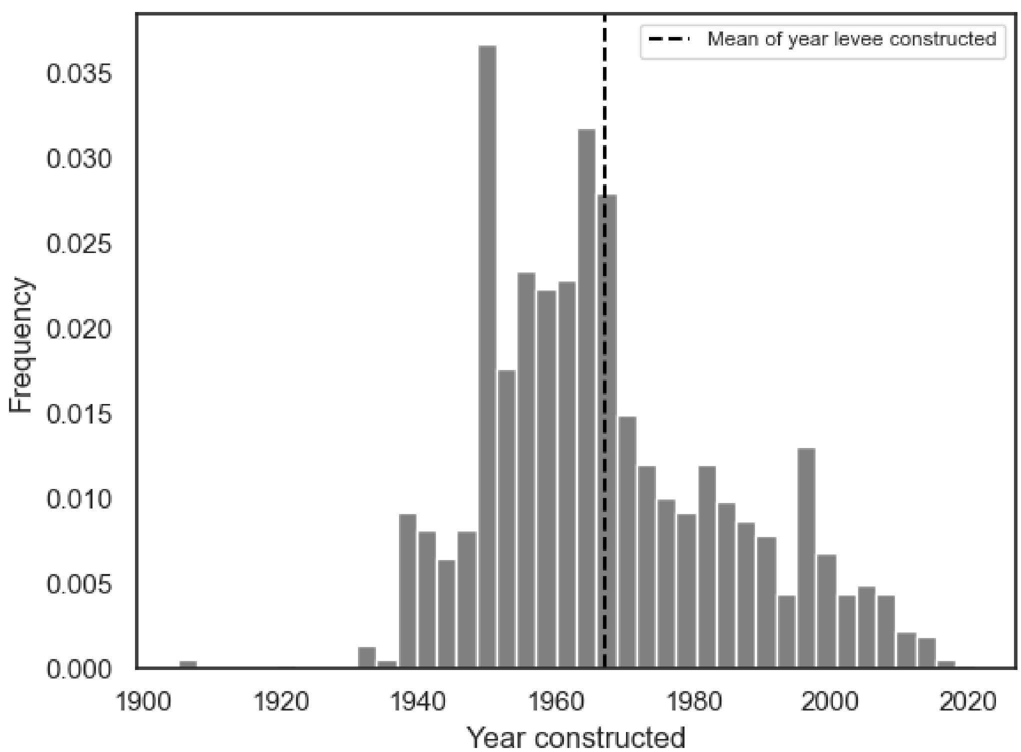
Notes: This figure displays, for each county, the length of levees in 2009 against the length of levees in 1989. The left figure shows the result of the nominal term and the right figure display the result of per capita term. The dashed lines are 45 degree lines.

Table 2.2: First Stage Regressions

	(1)	(2)	(3)	(4)	(5)
Panel A. Counties with positive investment					
Total Levee Length (instrument)	0.29	0.27	0.27	0.27	0.27
	(0.08)	(0.08)	(0.07)	(0.08)	(0.08)
Employment Level (Total)		X	X		
Employment Trend (Total)			X		
Employment Level (Construction)				X	X
Employment Trend (Construction)					X
Partial F-statistic	13.41	10.93	13.50	10.71	10.54
Observations	199	199	199	199	199
Adjusted R^2	0.22	0.25	0.26	0.23	0.23
Panel B. All Counties					
Total Levee Length (instrument)	0.03	0.03	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Employment Level (Total)		X	X		
Employment Trend (Total)			X		
Employment Level (Construction)				X	X
Employment Trend (Construction)					X
Partial F-statistic	3.79	3.74	3.72	3.77	3.76
Observations	3,107	3,107	3,107	3,107	3,107
Adjusted R^2	0.02	0.02	0.03	0.02	0.02

Notes: All variables are normalized by working-age population. The outcome variable for each regression is the investment in levees per working-age population in a county. Robust standard errors are in parentheses.

Figure 2.4: Constructed year of levees



Notes: This figure displays constructed year of levees. The vertical line show the mean of year (1967).

2.4.2 Baseline Results

Table 2.3 presents baseline results for the effect of levee investment on employment between August 2009 and February 2010. Panel A shows the effect on total employment in construction-related sectors. Panel B presents the effect on employment in the construction sector. Columns 1–3 of Panel A report OLS estimates that show positive effects on the change in employment. Column 3 shows that after controlling for the level and trend of employment, the estimated impact on employment becomes somewhat smaller. Columns 3–6 present the IV results, which yield imprecise but much larger impacts than OLS. The fact that the OLS estimate is lower than the IV estimate suggests that levee investment and economic conditions are negatively correlated: counties experiencing worse economic outcomes were likely to receive more investment. Column 6, the preferred specification, suggests that for every $100,000 in investment per working-age population that a county received, the county's total employment increased by 1.81 per working-age population between August 2009 and February 2010. The next subsection provides further discussion of how to interpret this result.

Panel B in Table 2.3 uses the change in construction employment as the outcome variable. The OLS coefficients (Columns 1–3) are positive, relatively small in magnitude, and not statistically significant. The IV results (Columns 3–6) suggest a positive and strong relationship between levee investment and change in employment in the construction sector. Column 6, the preferred specification, suggests that for every $100,000 in investment per working-age population, counties' construction employment increased by 0.96 per working-age population from August 2009 to February 2010.

It is also worth mentioning that the estimated coefficients on the control variables relate to employment conditions in the pre-stimulus period. All coefficients are negative. This result suggests that counties with worse initial employment conditions were likely to grow faster.

Table 2.4 presents the results of all counties, including zero investment, which is not my preferred sample set. The coefficients are unstable and statistically insignificant.

My preferred specifications suggest that the increase in labor demand from levee investment had positive impacts on the change in employment between August 2009 and February 2010. The coefficients in Panel B of Table 2.3 have a lower magnitude than those in Panel A of Table 2.3, suggesting that the "indirect" employment gains in construction-related sectors were notable. To look at this more clearly, I examine employment effects by sector. Table 2.5 reports the results of estimating the empirical model via IV with the full set of controls and with the six groups of employment by industry as dependent variables. Column 1 shows that the estimate is positive and statistically insignificant. One interpretation consistent with the finding in Column 1 is that jobs in the mining industry include the mining of construction sand. This element is one of the input factors in the production of levees, and the transportation cost of construction sand is high. Thus, levee investment may have led to benefits for the mining industry. Although manufacturing creates pieces of construction machinery, Column 3 shows no evidence that levee investment affected employment in the manufacturing sector. One possible interpretation is that relevant manufacturing companies may not be located in the counties in which projects were undertaken. Columns 4–6 show that this result is very unstable; thus, I find little evidence that investment affected employment in these industries.

To understand the dynamic effects of levee investment on employment, I explore how my estimates evolve as I move toward the month that marks the end of my sample. Specifically, I rerun the cross-sectional regression for changes in employment from August 2009 until every month from September 2009 to October 2011 and report the second-stage coefficients on levee investment from my preferred specification with the full set of control variables. That is, I rerun the estimate from August 2009 to September 2009, August 2009 to October 2009, August 2009 to November 2009, and so on, reporting each of these 26 coefficients.[11]

Figure 2.5 presents these results for total employment and employment in the construction sector. The solid line represents the point estimate and the dashed lines indicate the 90 percent confidence interval. Three important patterns emerge. First, levee investment results in immediate positive effects on counties' local labor markets, with the effect particularly large after the winter of the second year. Second, the effect on the construction industry is likely to be statistically significant, though the indirect effect is positive but not statistically significant. Third, the construction industry shows a seasonal cycle in which the effect is larger in winter. I discuss these implementation lags and seasonal cycles in the last two subsections.

2.4.3 Employment Multipliers

My results indicate a positive relationship between investment and relative employment outcomes. To interpret the magnitude of the estimates, I can translate the regression coefficients into the increase in job-years from $100,000 of marginal county investment. This requires two assumptions. First, I assume that investment received up until October 2011

[11] Chodorow-Reich et al. (2012) also use this approach to examine the dynamic effects.

Table 2.3: Employment Effects of Levee Investments (Aug 2009-Feb 2010)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Construction related employment						
Levee Investment (\$100,000)	1.16	0.80	0.89	2.41	1.97	1.81
	(0.50)	(0.60)	(0.64)	(1.00)	(1.19)	(1.06)
Employment Level (Total)		−0.03	−0.04		−0.02	−0.03
		(0.01)	(0.01)		(0.01)	(0.02)
Employment Trend (Total)			−0.09			−0.10
			(0.19)			(0.18)
First-stage F-statistics				13.41	10.93	13.50
Observations	199	199	199	199	199	199
Panel B. Construction employment						
Levee Investment (\$100,000)	0.50	0.29	0.44	1.29	0.80	0.96
	(0.20)	(0.22)	(0.23)	(0.32)	(0.35)	(0.38)
Employment Level (Construction)		−0.08	−0.13		−0.07	−0.12
		(0.03)	(0.04)		(0.03)	(0.04)
Employment Trend (Construction)			−0.37			−0.37
			(0.22)			(0.22)
First-stage F-statistics				13.41	10.71	10.54
Observations	199	199	199	199	199	199

Notes: The outcome variable for each regression is the changes in employment per working-age population in a county, from August 2009 to February 2010. Panel A uses total employment in construction-related sectors and Panel B uses employment in the construction sector. The main variable of interest is the investment in levees per working-age population. Columns 4-6 instrument using the total existing levee length. Robust standard errors are in parentheses.

Table 2.4: Employment Effects of Levee Investments (Aug 2009-Feb 2010)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Construction related employment						
Levee Investment (\$100,000)	0.88	0.57	0.91	3.66	0.29	2.09
	(0.48)	(0.54)	(0.49)	(3.75)	(3.62)	(3.34)
Employment Level (Total)		−0.05	−0.07		−0.05	−0.07
		(0.01)	(0.01)		(0.01)	(0.01)
Employment Trend (Total)			−0.21			−0.21
			(0.07)			(0.07)
First-stage F-statistics				3.79	3.74	3.72
Observations	3107	3107	3107	3107	3107	3107
Panel B. Construction employment						
Levee Investment (\$100,000)	0.47	0.17	0.47	2.91	−1.92	−0.27
	(0.18)	(0.21)	(0.19)	(2.56)	(2.77)	(2.65)
Employment Level (Construction)		−0.29	−0.32		−0.29	−0.32
		(0.10)	(0.08)		(0.10)	(0.08)
Employment Trend (Construction)			−0.45			−0.45
			(0.13)			(0.13)
First-stage F-statistics				3.79	3.77	3.76
Observations	3094	3094	3094	3094	3094	3094

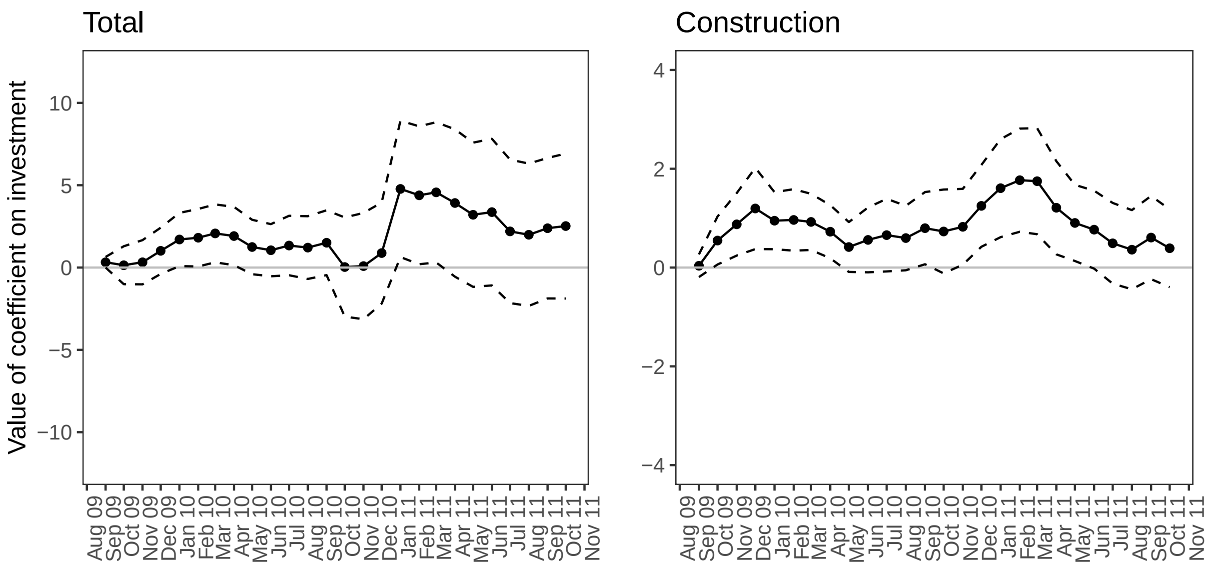
Notes: The outcome variable for each regression is the change in employment per working-age population in a county, from August 2009 to February 2010. Panel A uses total employment in construction-related sectors and Panel B uses employment in the construction sector. The main variable of interest is the investment in levees per working-age population. Columns 4-6 instrument the length of levees. Robust standard errors are in parentheses.

Table 2.5: Employment Effects of Levee Investments by Industry

	Mining	Const.	Manuf.	Wholesale	Fin and Ins	Oth. serv.
	(1)	(2)	(3)	(4)	(5)	(6)
Levee Investment (\$100,000)	0.19	0.96	0.07	0.58	0.01	−0.12
	(0.16)	(0.38)	(0.91)	(0.78)	(0.08)	(0.29)
Observations	191	199	199	199	199	199

Notes: The outcome variable for each regression is the change in employment of each indicated sector per working-age population in a county, from August 2009 to February 2010. The main variable of interest is the investment in levees per working-age population. Robust standard errors are in parentheses.

Figure 2.5: Employment Dynamics



Notes: These charts display the second-stage coefficient for regressions where the outcome variable is the change in employment between August 2009 and the months indicated on the x-axis. The left-hand graph shows the results in total employment of construction-related sectors, and the right-hand graph shows the results in the construction sector. The variable of interest is the investment in levees per working-age population. Regressions include the full set of controls. The 90 percent confidence interval, derived from robust standard errors, is plotted using dashed lines.

has no employment effects beyond October 2011. If the employment effects remain beyond October 2011, then my estimate of job-years is a lower bound. Second, I assume that investment made after October 2011 do not influence employment changes before October 2011.

Under these assumptions, the increase in job-years from $100,000 of investment can be calculated by taking the integral under the dynamic charts (Figure 2.5) and dividing this by 12 to convert job-months to job-years (Chodorow-Reich et al., 2012). My point estimates suggest that $100,000 of marginal investment increased county employment by 4.2 job years, 1.8 of which were in the construction sector. While the estimate is not significant in the total employment case, in the construction sector the effect is significant. Dividing $100,000 by 4.2 job-years yields a cost per job year of $24,000. Dividing $100,000 by 1.8 job-years yields a cost per job year of $55,000. The sum of awarded grants in my sample is 453 million dollars. Thus, this program would have created 19,000 jobs in total, including 8,300 jobs in the construction sector.

In the context of the costs and benefits of fiscal stimulus, levee investment may be a particularly low-cost means of supporting employment during a recession: a cost per job of $24,000 is at the lower end of cost-per-job-year estimates for the ARRA. Chodorow-Reich (2019) reviews empirical studies of fiscal multipliers and provides an updated analysis of the ARRA. He shows that the 90 percent confidence interval for the "cost-per-job" is ($25,500, $73,900). While his estimates are based on cross-state regressions, my estimates use cross-county regressions, and this difference in approaches may yield different results. The magnitude of the employment effect can be interpreted by comparing the estimate of the cost per job year with the wage in construction, the main target industry of the investment program. To do so, I compare my result with the annual wage. In my sample counties, the average annual wage of the construction sector in 2008 was $34,000. Calculated on this basis, the "wage multiplier," the ratio between the wage and the cost per job, is 0.61.[12]

When I compare my results with highway investment, the difference is notable. For

[12] The additional effects of distortionary taxes should be taken into account when evaluating the figure. Samuelson (1954) shows that the sum of the marginal rates of substitution must be equal to the marginal rate of transformation. He uses the case in which the government is financed entirely by lump-sum taxation. His analysis is extended by Stiglitz and Dasgupta (1971) and Atkinson and Stern (1974) to account for the more realistic situation in which revenue has to be raised by distortionary taxation. These papers show that a crucial factor in the optimal size of government is the marginal welfare cost of raising revenue through distortionary taxes, subsequently labeled the marginal cost of public funds (MCF) by Browning (1976). Ballard and Fullerton (1992), Dahlby (2008) and Kreiner and Verdelin (2012) provide a recent reviews of this literature.

Also, it is crucial to account for both the benefits to the public good itself (long-run) and the benefit in terms of mitigating the unemployment (short-run). Michaillat and Saez (2018) propose a theory of optimal public expenditure when unemployment is inefficient. When unemployment is inefficiently high, too many workers are idle. They introduce inefficient unemployment into Samuelson's theory.

example, Garin (2019) shows a cost per job year of $150,000. There are two possible reasons why the employment effects of levee investment are larger. First, the transaction amounts for levee projects are more likely to be small. In particular, while the mean transaction amount in highway projects is $2.1 million, for levee projects it is $1.2 million in my sample. The bidding process is also different. While USACE, a Federal government agency, performs procurement for levees, states do this for highways. These differences may also have affected the results. Smaller projects may also be more efficient in hiring labor, because these projects are not likely to use large physical capital. Buchheim and Watzinger (2019) use school buildings in Germany to estimate the fiscal multiplier. Although school buildings are not an infrastructure project, this is a good comparison with levees because it is mainly related to construction. They show a cost per job year of $33,000, which is similar to my result. Generally, the transaction scale for building a school is also small, which might also influence the result.

2.4.4 Implementation Lags

Policymakers and researchers may be concerned that the program could have implementation lags after the date upon which budget plans were approved. Even though the projects related to the ARRA are "shovel-ready", it is possible that the public procurement process may take time. Even after a construction company wins a project, it has to survey local geological conditions based on the characteristics of the project. After such procedures, construction companies can start major construction work and hire more workers. Summers (2008) states that the implementation of investment programs may take such a long time that they are an ill-suited policy tool for a downturn when quick reactions are required. Becker (2009) also expresses concerns with implementation lags. Both emphasize that properly designed infrastructure projects have the virtue of being helpful as short-run stimulus. Summers proposes the benchmark of attaining this goal within a year. The fact that levee investment had immediate effects on employment alleviates common concerns and suggests that levee projects are an effective way to gain employment effects quickly.[13]

2.4.5 Seasonal Cycles

Another potential concern is that employment in the construction sector may show a seasonal cycle. While seasonal fluctuations are crucial in designing a short-run stimulus package, they are typically ignored by researchers. For example, building highways in the winter is difficult in cold regions. Geremew and Gourio (2018) present some facts about U.S. employment seasonality. They document that construction employment reaches a minimum in February and a peak around August. There is considerable heterogeneity in the amplitude of the seasonal cycle across states. For example, Florida is less seasonal than Minnesota.

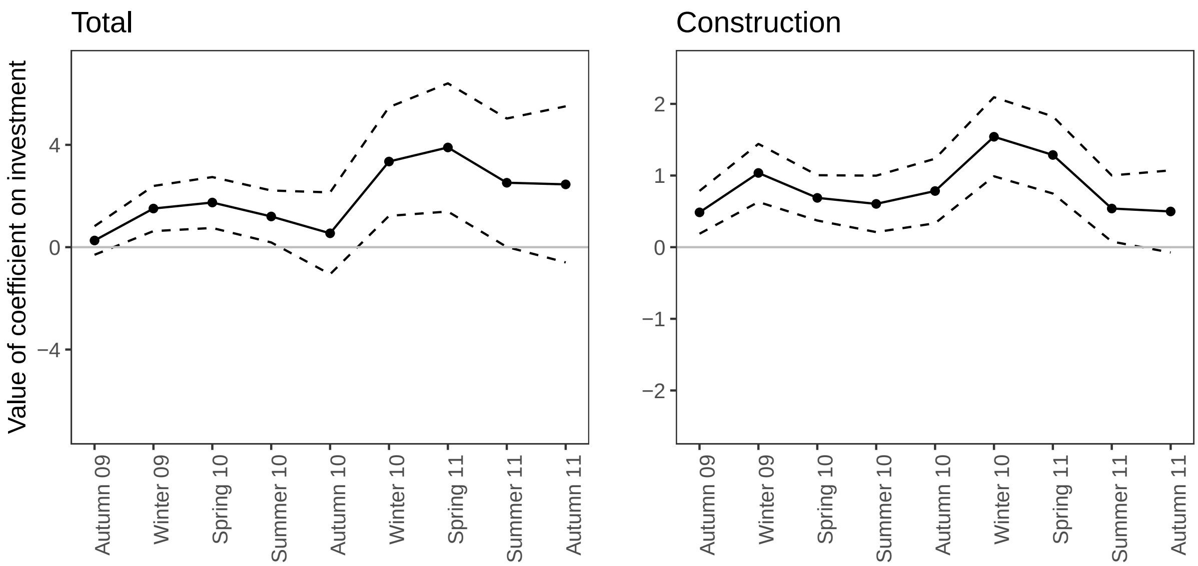
Figure 2.6 parallels the results in Figure 2.5, using each year's samples aggregated by season. Table 2.6 aggregates samples by season and uses regression by season. The estimates for winter are relatively large and are statistically different from zero in terms of both total employment and construction employment. Moreover, Table 2.6 shows that the estimate for winter in the construction sector is statistically different to those for autumn and summer (the difference between estimates in winter and autumn is $1.29 - 0.60 = 0.69$ and the

[13]The ARRA requires federal agencies to report actual weekly outlays, though the data is not available at the county level. By August 2010 (one year after the spending plan was approved), total gross outlays in the Mississippi River and Tributary account, a project heavily reliant on levees, were \$143 million, whereas the total obligation was \$267 million. This fact supports the statement that implementation lags are short.

standard error is $\sqrt{(0.21)^2 + (0.15)^2} \approx 0.26$). One possible interpretation is that seasonal factors should be considered in levee construction. Figure 2.1 plots the geographic variation in investment. Investment is high in Florida, California, Louisiana, and Missouri. These states have a relatively higher average temperature in January. This fact allows construction firms to build levees in winter. Moreover, high water levels delay levee construction. USACE (2006a) points out that damaged structures should be updated prior to flood season. Baldwin and Lall (1999) investigate the seasonality of the upper Mississippi River streamflow, showing that the streamflow reaches a minimum around January and a peak around June. Such facts suggest that building levees in the winter would be ideal. Thus, the employment effect may be larger in the winter.

While previous literature does not examine seasonal cycles in highway construction, such cycles would exist even in highway construction. For example, installing concrete and asphalt, major materials for highways, are challenging in the winter because these materials require dry and relatively high temperatures. On the other hand, filling soil, a major material for levees, does not have such a seasonal limitation. Therefore, levee construction may be an efficient policy tool to stimulate construction labor demand in the winter.

Figure 2.6: Seasonal Cycles of Employment



Notes: These charts display the second-stage coefficient for regressions where the outcome variable is the change in employment, using each year's samples aggregated by season. The left-hand graph shows the results in total employment of construction-related sectors, and the right-hand graph shows the results in the construction sector. The variable of interest is the investment in levees per working-age population. Regressions include the full set of controls. The 90 percent confidence interval, derived from robust standard errors, is plotted in the dashed lines.

Table 2.6: Employment Effects of Levee Investments by Season

	Autumn	Winter	Spring	Summer
Panel A. Construction related employment				
Levee Investment ($100,000)	0.92	2.43	2.82	1.86
	(0.61)	(0.71)	(0.83)	(0.83)
Observations (# of counties)	199	199	199	199
Panel B. Construction employment				
Levee Investment ($100,000)	0.60	1.29	0.99	0.57
	(0.15)	(0.21)	(0.19)	(0.18)
Observations (# of counties)	199	199	199	199

Notes: The outcome variable for each regression is the change in employment per working-age population in a county, using samples aggregated by season. Robust standard errors are in parentheses.

2.5 Robustness Checks and Extensions

2.5.1 Falsification Tests

One potential concern is that counties with longer total levee length might have been experiencing employment growth before the investment. To address this concern, I estimate the effect of levee length on the change in employment (i.e., I estimate the reduced form equation) prior to the period of interest. This also allows me to check whether the investment is the only channel through which the instrument affects the change in employment. If the positive and significant coefficient found on the ARRA investment in the baseline regressions does indeed reflect a causal effect on employment growth, then the coefficient should be zero when the dependent variable is the length of levees before the commencement of the ARRA.

Figure 2.7 reports the reduced-form coefficients for this test using data from January 2003 to August 2009. I consider six-month changes in both total employment and employment in the construction sector. I then run my reduced-form estimates on each overlapping six-month period. I rank the coefficients based on their magnitude and report empirical cumulative distribution function. For comparison, I also show the reduced-form estimate for the baseline period, August 2009 to February 2010, with a vertical line.

The results show two key patterns. First, the estimates are centered around 0; the empirical median of the estimate is -0.12 for total employment and -0.01 for construction. That is, the evidence suggests that in the years before the investment related to the ARRA, counties did not experience systematically different employment trends. Second, my baseline reduced-form estimate is large relative to the coefficients in the period before the investment. Both pieces of evidence increase my confidence that the estimates reported above capture the effect of the ARRA rather than underlying differences between counties with longer or shorter levees.

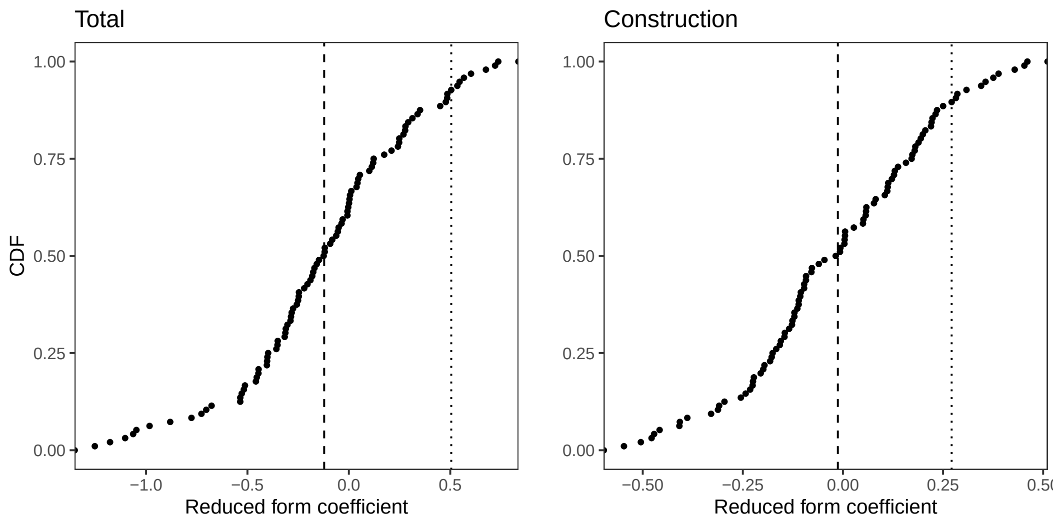
Another test is used to check whether the total existing levee length is correlated with other county-level unobservables. For instance, labor demands might be affected by geological characteristics. Projects for levees might be challenging if geological conditions are not favorable (i.e., productivity would decrease). More skilled workers might be required to deal with these issues. These conditions would be correlated with the characteristics of levees. Unfortunately, these variables related to productivity are unobservable at the county level. To address this issue, I use county-level house price data in 2009 from Zillow to check the correlation between the instrument, levee length, and typical home value. Housing prices can be used as a proxy for geographic productivity because they are related to geological characteristics. Because the correlation between levee length and home value is 0.02, I find little evidence that these variables are related to one another. This result supports my assumption that the instrument affects the outcome only through investment.[14]

2.5.2 Heterogeneity

The effects of investment on employment may not be the same in all places. In particular, a given amount of per-capita infrastructure investment might constitute a larger shock to local demand in smaller counties. Even if the underlying effects are constant across counties, it may be easier to detect an effect in smaller labor markets if infrastructure projects constituted larger shocks in such locales. Thus, I test for heterogeneity using the 2009 pop-

[14]One might be concerned that house prices could be correlated with the investment. Because the correlation between investment and local house prices is 0.1, I find little evidence that these variables are related.

Figure 2.7: Falsification Tests



Notes: Plots results of reduced-form regressions, where the outcome variable is change in employment for each overlapping one year period, starting in January 2000 and ending in August 2009. All regressions include the full set of control variables. Coefficient from August 2009 to February 2010 is indicated with the vertical dotted line. The median of the coefficients is indicated with the vertical dash-dotted line.

ulation. To do so, I use the median to divide counties into two subgroups. I then regress each subgroup using the baseline setting for regression.

These regressions yield coefficients of (1) 2.99 (SE = 1.54) for total employment in smaller-population counties, (2) 1.39 (SE = 0.60) for construction employment in smaller-population counties, (3) −55.2 (SE = 66.0) for total employment in larger-population counties, and (4) −7.86 (SE = 9.33) for construction employment in larger-population counties. Although the differences between the two subgroups are not statistically significant, the estimates in smaller counties are positive, reaching statistical significance in the construction sector. A potential explanation for these observations is that construction markets in smaller counties may be small, so increases in aggregate labor demand will tend to increase the employment level.

2.5.3 High School Completion

A model of human capital predicts that increases in the wages of workers with low education relative to wages of workers with high education will reduce investment in schooling because the returns to additional years of schooling are diminished. Black et al. (2005) study the effect of the Appalachian coal boom on high school enrollments. Because the fiscal stimulus program might affect high school enrollments, I examine the effect of the investment on high school dropout rates.

To determine these effects, I use enrollment data from the October Supplement of the U.S. Bureau of Labor Statistics' Current Population Survey (CPS). The data includes (1) whether the sample is enrolled at a regular school and (2) whether the sample has finished high school. The U.S Department of Education's National Center for Education Statistics defines dropout rates as follows: the percentage of the population in a given age range who have not finished high school or are not enrolled in school at one point in time. Since most enrollment data are not available at the county level, I aggregate data at the state level. I check the change in dropout rates (1) from 2009 to 2010 and (2) from 2009 to 2011 as an outcome of the equation. These regressions yield coefficients of (1) 7.95 (SE = 4.42) and (2) −2.95 (SE = 2.04). Since these results are very noisy, I find little evidence that levee investment affects high school completion rates.

2.6 Conclusion

This study estimates the employment effects of a previously unstudied form of government intervention in ARRA: levee investment. Using a novel and unique instrumental variable, the total existing levee length, IV results indicate that this ARRA spending had a positive impact on employment. My preferred specifications suggest that $100,000 of marginal investment increased employment by 4.2 job-years, 1.8 of which were in the construction sector. Moreover, levee projects had immediate effects on employment and showed a seasonal cycle. It should be emphasized that the stimulus effects estimated here relate to the effects of one particular stimulus program enacted in a unique economic environment.

One question for future research is whether levee investment has an economic effect in the long run. Investing in infrastructure has two main roles: (1) to preserve and create jobs, and (2) to provide long-term economic benefits by improving productivity. In relation to the second point, many studies have focused on transportation infrastructure. In contrast, there is little literature focusing on flood control infrastructure. Shirai (2022) examines the long-term economic impact of levee systems, which mitigate flood damage, on the local economy and optimal policy targeting for levee systems. More research is needed to inform policymakers about the impact of infrastructure investments on local economies.

Chapter 3

Natural Disasters and Public Procurements: Evidence from Hurricanes Katrina and Rita

3.1 Introduction

Natural disasters affect society. Climate change is causing an increase in the intensity of these disasters (Wing et al., 2022), and quantifying their effects is a crucial task for researchers and policymakers. While natural disasters directly impact human lives and capital, they also have indirect effects. During the recovery process, construction demands increase dramatically, leading to a significant shock in the local construction market. How do natural disasters affect local construction markets? How much do these disasters increase procurement costs? What mechanism is helpful to mitigate these shocks to the market? The answers to these questions are critical for infrastructure planning.

This paper provides new evidence on the effect of natural disasters on public procurement. I focus on highway procurement auctions in Louisiana before and after Hurricanes Katrina and Rita. These storms, which devastated the Gulf Coast in 2005, are the costliest in US history. This setting is suitable for my research design because the Louisiana Department of Transportation and Development (LADOTD) provides bid letting information for periods before and after the hurricanes, allowing me to compare the local construction market. I need to identify exogenous variation related to construction demand to examine the effect of these hurricanes. The Federal Emergency Management Agency (FEMA) has an Individual and Households Program for the recovery of homes from disasters and a Public Assistance Program to repair infrastructure such as hospitals and schools. Since these datasets are available at the parish level, they constitute an excellent proxy to explain the exogenous variation in the local construction markets. I use this variation to explore bidder entry and competition.

Each highway procurement project includes various jobs such as pavement, excavation, and marking, and I need to control for this heterogeneity. Typically, the highway procurement literature uses work-related dummies (e.g., the main task is mowing) to control for heterogeneity. However, few studies use details about each item in their assessment, though the data provides more detailed characteristics of auction heterogeneity. A possible reason for researchers not using this characteristic is that the data is high dimensional. For example, there are 1,391 item categories in my dataset, though the total number of firm bid observations is 1,152. I thus need to select appropriate variables from the high-dimensional data to control for heterogeneity. To normalize the firms' bids, I use a Least Absolute Shrinkage and Selection Operator (LASSO), which adds a penalty function in the sum of the absolute values of the coefficient estimates to the standard Ordinary Least Squares (OLS) objective function.

Using a LASSO and a continuous difference-in-differences approach, I find that hurricanes mainly affect two aspects of highway procurement auctions. First, the effects of hurricanes on bids, winning bids, and second bids are positive and statistically significant, suggesting that hurricanes increased the cost of highway construction. Second, the number of bidders decreased after the hurricanes in the affected area, meaning that affected parishes experienced a less competitive environment because some construction firms did not participate in the auctions. Since the firm behaviors may be different based on the firm's status, I perform a difference-in-differences approach with triple interaction and find that the estimated coefficients are imprecise. I thus find clear evidence that firm size affects bidding behavior.

I provide three robustness checks. First, I perform a placebo analysis to check the parallel trend assumption in the difference-in-differences approach. In particular, I assume a false hurricane occurred in February 2005, six months before the hurricanes, and then check for structural differences among parishes. If this test reveals a difference, the main result may be biased. I find no evidence of a difference in the normalized bid setting; thus, the placebo test is passed in my dataset. Second, I consider the existence of potential bidders. I use a Heckman selection model to check for evidence of selection, finding that only bidders with low costs enter the auction. This implies that the distribution of realized bids is a truncated version of the distribution of potential bids, suggesting that I should consider this characteristic to identify the private cost. Finally, policymakers are concerned about collusion among firms after catastrophic disasters. I use the approach proposed in Bajari and Ye (2003) to check for collusion but find no evidence of this.

The results from the design-based approach have practical implications for highway procurement after catastrophic disasters. There are three possible reasons why firms change their bidding strategy. First, firms expect increasing exogenous demand for construction labor and material for the recovery projects and rising labor, materials, and bidding prices. The factor requires me to consider exogenous demand shocks in my bidding model. Second, the opportunity cost for participation may increase because of the exogenous demand shock, suggesting that an endogenous entry model should be used. Third, collusion may be a factor in increasing the bid; however, I do not find any evidence of collusion. Thus, I do not consider this characteristic in my model. The evidence also raises questions about the auction market. For example, how much could the government save on procurement if the hurricanes had not occurred? How would the overall procurement surplus change if the government subsidized bids? These questions motivated the use of a structural approach to analyze auctions.

I use a standard first-price auction model, which takes into account the procurement and entry costs in respect of the natural disaster based on Guerre et al. (2000) and Athey et al. (2011). Using a first-price sealed-bid auction model with endogenous entry, I find that the exogenous construction-demand shock increases bids and bidders' costs. Since firms use a higher bid for the same private value, the auction profits for firms would increase after the disaster. I also find that entry costs, calculated based on profit, also increase. The result suggests that the opportunity cost of participating in the highway bidding process increases after the disaster.

The estimates of the primitives of my structural model allow me to examine counterfactual exercises. First, I examine the additional cost of procuring each road project in the face of increasing construction demand by calculating the cost of no-hurricane recovery projects. I find that the government-paid prices for highway projects are, on average, 10% higher than for the no-hurricane case. Total project costs would increase by about $72 million, based on the observed data. Since the average project cost is $3.5 million, the government could undertake about 20 additional projects if the hurricanes had not occurred.

Second, I analyze the effects of subsidy procurement policies after natural disasters. The expected profit increases with subsidies, and some firms are able to enter the auction. This entry may cause procurement costs to decrease. I show that the government can save on procurement costs because firms that can provide projects inexpensively are able to enter the auction, while firm ex-post profits decrease if subsidies increase. However, the government has to raise taxes, and social surplus falls when the government introduces a subsidy policy after the hurricane. Thus, this is not the best method for post-hurricane highway procurement auctions.

I contribute to three main strands in the literature. The first focuses on the economic impacts of natural disasters. For example, Deryugina (2017) shows that US hurricanes lead to substantial increases in non-disaster government transfers. Deryugina et al. (2018) use tax returns to estimate the economic impact of Hurricane Katrina. Gregory (2017), and Fu and Gregory (2019) conduct empirical analyses of Louisiana's Road Home rebuilding grant program. Cheng and Wilmot (2009), in the study most similar to my own but in the area of construction management, compare the cost trend of highway procurement auctions after Hurricanes Katrina and Rita. They compare representative bid-item prices to examine the effects of hurricanes on the construction cost, whereas I use a difference-in-differences approach for causal inference and a sealed first-price auction model with endogenous entry.

My methods have an advantage because I can consider (1) competition effects such as the number of bidders, (2) the relationship between private costs and bids, and (3) subsidy effects (through counterfactual exercises).

I also contribute to the literature on the structural estimation of auctions, especially in respect of highways (Krasnokutskaya, 2011; Krasnokutskaya and Seim, 2011; Lewis and Bajari, 2011; Bajari et al., 2014; Lewis and Bajari, 2014). There are studies of auctions that focus on changes in construction demand due to external shocks similar to natural disasters, for example, those examining the bidding behavior of companies during economic crises. Balat (2017) examines the cost of projects by focusing on the increase in construction demand following the adoption of the American Recovery and Reinvestment Act. Gugler et al. (2015) examines the impact of the 2008 global financial crisis and the impact of Austria's subsequent economic stimulus measures on the markup. My paper provides insights into how the increase in construction demand during the disaster recovery process changes the bidding behavior of firms.

Finally, my paper is related to a large literature using high-dimensional economic data. Belloni et al. (2014) and Athey and Imbens (2019) provide an overview of the methodology. To deal with the high-dimensional issue, many researchers use LASSO. For example, in their study on asset pricing, Freyberger et al. (2020) use a LASSO to select characteristics and to estimate how these affect expected returns nonparametrically. Using a LASSO, Knaus et al. (2020) investigate the effect of heterogeneity of job-search programs for unemployed workers, and Buhl-Wiggers et al. (forthcoming) examine the effects of an educational intervention in Uganda. As far as I know, only one paper applies the technique to the literature on highway procurement auctions. Kim and Jung (2019) select variables to forecast bidder's bids using machine learning methods, including a LASSO. Although I use the same idea to normalize bids, I add LASSO to the well-established framework, including a difference-in-differences approach and a sealed-first-price auction model with endogeneity entry.

The remainder of the paper is organized as follows. Section 3.2 provides background information on Hurricanes Katrina and Rita and the institution of highway procurement in Louisiana. In Section 3.3 I set out the data and in Section 3.4 present and discuss the design-based estimation and results. Section 3.5 describes the model and results of structural estimation. Section 3.6 provides two counterfactual exercises. Section 3.7 concludes.

3.2 Background

3.2.1 Hurricanes Katrina and Rita

Hurricanes Katrina and Rita were intense Category 5 hurricanes that lasted for several days in the Caribbean and Gulf of Mexico. Hurricane Katrina struck the US Gulf Coast on August 29, 2005. Approximately 80% of New Orleans was flooded to depths exceeding 15 feet in many areas. Surge and wave action caused 50 major levee breaches. Thirty-four of the city's 71 pumping stations were damaged. The storm and subsequent flooding left two-thirds of the city's housing stock uninhabitable without extensive repairs, the costs of which significantly exceeded insurance payouts for many pre-Katrina homeowners in New Orleans. Twenty-eight days after Katrina, in the early morning hours of September 24, 2005, Hurricane Rita hit southwestern Louisiana, causing further devastation (GAO, 2006).

After the hurricanes, residents, business owners, and the government had to reconstruct homes and infrastructures. According to Spader (2015), Hurricane Katrina significantly damaged more than 1,000 blocks in three parishes (Orleans, Jefferson, and St. Tammany). Hurricane Rita destroyed about half of the homes in Cameron Parish (Waugh and Smith, 2006). Individuals affected by such disasters may be eligible for FEMA's Individuals and Households Program (IHP). The total spent by these programs to recover homes following the disasters was $453 million. In addition, the state of Louisiana created the Louisiana Road Home program, which provided cash grants for rebuilding or relocating pre-Katrina Louisiana homeowners with uninsured damages.

Related to the rebuilding demand of small businesses following such disasters, the US Small Business Administration (SBA) offers disaster loans to businesses and individuals for disaster-related losses not covered by insurance. A firm may apply for such loans to repair or replace entities damaged or destroyed in a declared disaster. Although SBA loans were available following Hurricane Katrina, evidence suggests that the process was mismanaged and that many eligible applicants were rejected. Associated Press (2010) examining SBA data, finds that 55% of homeowners and businesses that applied for help did not receive assistance.

USACE (2006b) undertook extensive safety-related studies to improve the flood control system after the hurricanes. Congress authorized and funded the development of the 100-year level risk reduction system, known as the Hurricane and Storm Damage Risk Reduction System (HSDRRS). According to USACE (2018), the HSDRRS cost $14 billion, includes five parishes (Orleans, Jefferson, St. Bernard, St. Charles, and Plaquemines), and consists of 350 miles of levees and floodwalls, 73 non-Federal pumping stations, three canal closure structures with pumps, and four gated outlets. In addition, the local government rebuilt other infrastructures such as schools and hospitals. For example, 110 out of 126 public schools were destroyed. FEMA provides public assistance programs to cover the repairs or replacement of infrastructure (roads, bridges, public buildings, etc.), debris removal, and emergency protective measures.

These reconstruction activities increased construction demands, putting immediate pressure on the construction industry in the region. Figure 3.1 shows the repairing demands of damaged housing and infrastructures from Hurricanes Katrina and Rita. I use a publicly available dataset of FEMA's Individual Assistance Program and Public Assistance Funded Project. The construction demand in the south of Louisiana increased dramatically.

Figure 3.2 is a depiction of the Louisiana construction market before and after the hurricanes to appreciate the full effects of the post-hurricanes recovery demand. The graph is constructed using an event study approach, grouping construction demand as median or greater and less than the median.[1] Panels A and B show that although there was no pre-existing statistically significant difference in employment and wages between more damaged counties and less damaged counties, there was a statistically significant difference after the hurricanes. Panels C, D, and E show the number of firms in the Louisiana construction industry. After the disaster, many small companies (those with fewer than 100 employees) entered the disaster area, while the effect on entry for larger companies (those with more than 100 employees) was small. Panel F presents data on the entry and exit of firms in highway, street, and bridge construction, a more granular sector of the construction industry.

[1] In particular, I estimate the following equation and report β_j: $Y_{pt} = \sum_{j=-\underline{j}}^{+\bar{j}} \beta_j b_p^j + \mu_p + \lambda_t + \epsilon_{pt}$, where Y_{pt} is an outcome variable (e.g., employment) for time t in parish p, b_p^j is a dummy variable for time j in parish p where the recovery demand is greater than the median, μ_p and λ_t are parish and time fixed effects, and ϵ_{pt} is an unobserved error term.

There are no statistically significant differences between the pre- and post-disaster periods for this category.

3.2.2 Highway Procurement in Louisiana

Licensed firms can participate in the typical highway auction.[2] The LADOTD provides a list of active bidders and approved contractors who submitted bids in the preceding year. Although there is no archive data for active bidders for 2005, there are 170 active bidders in 2022. An alternative definition of the firms in this market is provided by County Business Patterns (CBP) data, which indicates that in 2005 199 firms specialized in "Highway, Street, and Bridge Construction (NAICS code: 2373)." My dataset includes 117 firms that enter the auction, which I describe in detail in my data section.

Highway procurement covers various projects, including removing existing debris and constructing drainage structures and pavements. A typical project consists of a combination of these construction types. LADOTD (2013) shows that a project requires pre-procurement feasibility studies, planning and environmental impact assessment, and budgeting. Therefore, a typical project takes several years to reach the procurement stage, and the procurement portfolio in a given year would not change significantly as a result of a disaster. However, procurement for some types of projects is expected to emerge after hurricanes. For example, the I-10 Twin Span Bridges crossing Lake Pontchartrain between Slidell and New Orleans were severely damaged (Gill, 2007) in the hurricanes and were repaired in approximately four and a half months. Another example is the repair of pavement that deteriorated after submersion for long periods; about 2,000 miles of roadway in the Greater New Orleans

[2] If the estimated project value is greater than $50,000, a license is required (LADOTD, 2016).

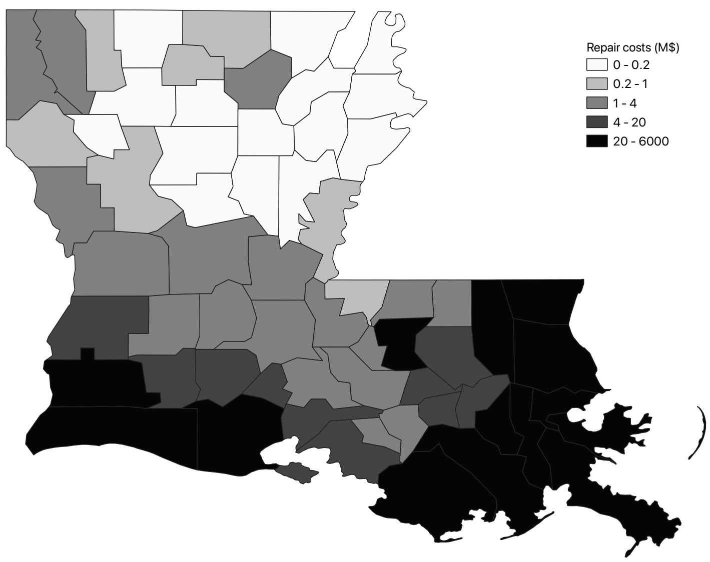
area were submerged by floodwaters for up to 5 weeks. Zhang et al. (2008) show that pavement structures are affected by the floods. These projects are not included in my sample because they are unique, and it is challenging to find comparable pre-hurricane projects.

The procurement process for projects involves three steps: First, the LADOTD announces a project. The project advertisement usually contains only limited information, such as type of work, location, and completion time. Second, potential bidders may collect proposals explaining the plans and specifications of the work required. Based on a proposal, bidders may submit a sealed bid. Since preparing bid documents requires time and effort, I treat the costs of such bid preparation as entry costs. Third, the bids received are unsealed and ranked on the letting day. The project is awarded to the lowest bidder if the LADOTD can identify that the bidder has sufficient equipment and labor.

The LADOTD uses unit-price contracts for highway construction. Government engineers prepare a list of items that describe the tasks and materials required for the project. Each construction item has a number, description, quantity, unit of measure, unit price, and the bid amount. For example, item number "502-01", description "Superpave asphaltic concrete", quantity "28753.0", unit of measure "TON", unit price "56.20", and bid amount "1,615,918.60".[3] The itemized list is publicly advertised, along with a detailed set of plans and specifications describing how the project will be completed. The bidders propose the unit prices when preparing their contract estimate. The sealed bid is determined by the sum of the item bid amounts.

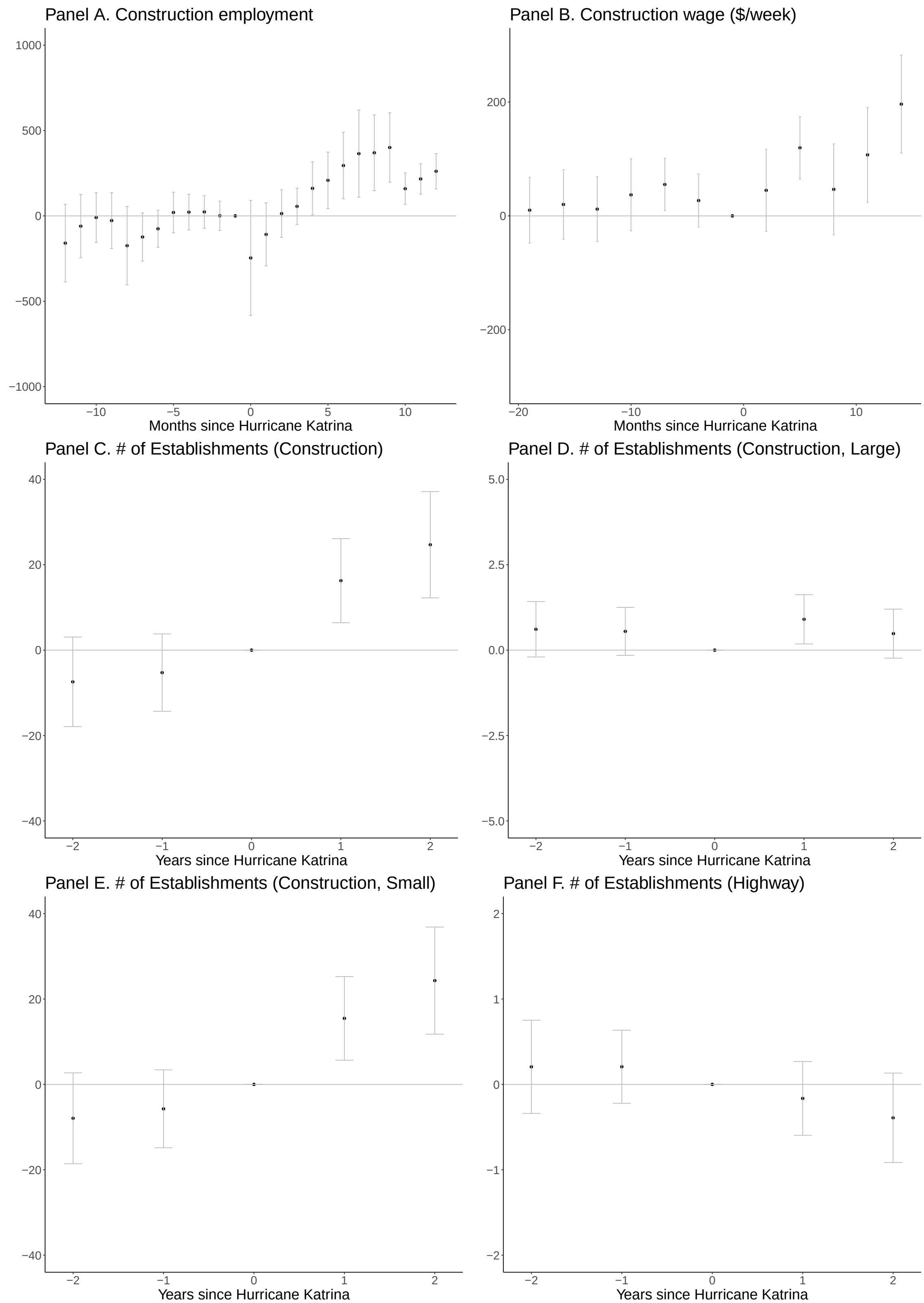
[3]This example is from the following auction: "http://wwwapps.dotd.la.gov/engineering/lettings/bidstabs/tabulations/BtDetails.aspx?page=bt05101201.shtml"

Figure 3.1: Total Repair Costs of Housing and Infrastructures of Hurricanes Katrina and Rita



Notes: The data is from OpenFEMA webpage. I use (1) the “repair and replace amount” from the Individual Assistance-Owners dataset, and (2) “project amount” from the Public Assistance dataset.

Figure 3.2: Local Construction Markets in Louisiana before and after the hurricanes



Notes: The data is from the Quarterly Census of Employment and Wages (Panels A and B) and the County Business Patterns data series (Panels C, D, E, and F). Each dot shows the estimate of the event study design, and the error bar is the 95 percent confidence interval. See text for the definition of counties and methodologies.

3.3 Data

My observation unit is a highway contract procured by the LADOTD between August 2004 and August 2006. I index the contract's projects by $n = 1, \cdots, N$. I mainly focus on civil engineering work such as paving because this yields a larger sample than other projects. The procurement includes repairing electronics such as signals, mowing, and constructing large bridgework. The procurement auction also includes hiring professionals to oversee projects, called construction management. These auctions are few in number, and I omit them to control for unobservable auction heterogeneity.[4] The sample includes $N = 364$ projects with a total awarded value of $1.13 billion. There are a total of 1,152 bids submitted by 117 general contractors.

For each project, I collected information from the publicly available bid summaries that include letting date, contract number, job location, route name, pay-item information, and bidder identities with their itemized bids. Pay items are individual construction components for which the contractor proposes prices when preparing a contract estimate. I have the unit prices for all bidders and the estimated quantity for each item. I use the item category to normalize bids using the method explained in the next section. I note that the engineer's estimate and reservation price are missing.

The hurricanes destroyed houses and infrastructure, and this destruction and the subsequent recovery caused construction demand to fluctuate. I analyze the effect of Hurricanes Katrina and Rita on highway procurement using rebuilding-demand variables. I use the demand for housing and infrastructure rebuilding as the continuous treatment variable.

[4] In particular, each bid tabulation provides descriptions of the relevant project. I exclude projects including certain words (e.g, signals, mowing). The full dataset is available upon request.

FEMA's Individual and Public Assistance programs provide parish-level rebuilding data categorized by disasters. Figure 3.1 in the preceding section shows the geographical distribution of the relevant demand. Although I admit the dataset may include rebuilding unrelated to construction demand, such as repairing electric facilities, the dataset is a good proxy for parish-level rebuilding demand.[5]

I use three variables to control for firm heterogeneity. First, I complement the LADOTD data by constructing the backlog variable using previously won but incomplete projects. For each firm, the backlog is defined as the dollar value of work left outstanding from previously won contracts. Since my dataset does not include information on the construction period, I define the inventory variable according to the definition in Porter and Zona (1993): the jobs are to be completed within three months, with firms working at a constant pace. Second, I collect data on the number of plants and their locations for each firm and then calculate the driving distance to the job location. I use the minimum distance, that is, the distance to the closest plant, to consider comparative advantage. Finally, I control for the market share of the firms. Table 3.1 lists the top 20 contractors in my dataset and their market share. I define market size based on the values of the winning bids for the projects in my dataset. In my sample, 117 firms participate in the auctions. Sixty-seven firms won contracts during the period, with four firms having a market share of over 10%. To account for asymmetry in size and experience, I use a dummy variable equal to one if firm i is a "fringe" firm, defined as a firm that won less than 1% of the value of contracts awarded (Bajari et al., 2014).

Potential bidders need to be defined to detect any change in their bidding-participation

[5] Although I can infer rebuilding costs from other datasets, such as flood insurance by parish, the dataset of flood insurance does not provide information about the corresponding level of disaster.

behavior from before to after a disaster. In typical studies on highway procurement, potential bidders are described as acquirers of plans distributed during the advertisement process. In the absence of published data in the LADOTD, I define potential bidders as firms participating in projects that are similar in (i) size, (ii) geography, and (iii) timing. First, I use the eighth quantile of normalized bids, which I define in the next section, to determine similarity in size. Second, I use the nine LADOTD districts and target businesses in the same district for geographic similarity. Finally, I target projects in the 45 days before and after hurricanes for similarity in timing. This definition yields, on average, 18 potential bidders per project, and approximately 20% of the potential bidders participate in the bidding process.

Table 3.2 presents summary statistics for the data. Although I use a continuous treatment variable, I divide the data into two groups to capture the parish characteristics. Specifically, I use the median of rebuilding demand, and parishes for which the demand is less than the median are a "small rebuild," and parishes for which the demand is higher than the median are a "large rebuild" in the table. Columns 1 and 2 show the heterogeneity of project size: the mean value of the winning bid is \$2.5 million with a standard deviation of \$4.5 million before the hurricanes and \$3.2 million with a standard deviation of \$5.8 million. The number of bidders decreased after the hurricanes, especially in the large-rebuild group. The distance from the project site to the site of a firm's plant is not different for the before- and after-hurricane periods, although the distance is higher for the small rebuilding group than for the large rebuilding group. I cannot find a clear difference in the backlog in the sample.

Table 3.1: Identities of Top 20 Firms

Name	Share	Name	Share
Gilchrist Construction Co.	16.5%	Madden Contracting Co.	1.8%
James Construction Group	12.6%	Byron E. Talbot Contractor	1.7%
Denton-James	11.7%	Best-Yet Builders	1.3%
Diamond B Construction Co.	11.2%	H & S Construction Co.	1.3%
Barriere Construction Co.	8.8%	F. G. Sullivan, Jr. Contractor	1.2%
D & J Construction Co.	5.3%	R. E. Heidt Construction Co.	1.1%
Boh Bros. Construction Co.	4.7%	Prairie Contractors	0.9%
Coastal Bridge Co.	3.5%	American Contracting & Services	0.9%
Barber Bros. Contracting Co.	3.4%	Covington Paving Co.	0.9%
W. E. Mcdonald & Son	2.1%	Hard Rock Construction Co.	0.8%

Notes: There were 117 total active bidders for road improvement contracts in my sample between 2004 and 2006. The firms listed above are the top 20 firms from my sample, ranked according to their market share. I designate the firms with less than 1 percent market share as fringe firms.

Table 3.2: Summary Statistics

	All (Before) (1)	All (After) (2)	Small Rebuild (Before) (3)	Small Rebuild (After) (4)	Large Rebuild (Before) (5)	Large Rebuild (After) (6)
Bid (M$)	2.80	3.54	3.81	2.58	2.17	4.18
	(4.98)	(6.44)	(6.42)	(5.19)	(3.68)	(7.10)
Win Bid (M$)	2.55	3.21	3.47	2.34	1.97	3.79
	(4.54)	(5.83)	(5.78)	(4.74)	(3.43)	(6.40)
Bidders	4.14	3.34	3.66	3.26	4.44	3.40
	(1.71)	(1.08)	(1.36)	(1.01)	(1.84)	(1.12)
Miles	56.67	56.46	68.77	70.98	49.04	46.74
	(52.25)	(48.90)	(59.76)	(54.83)	(45.34)	(41.84)
Backlog (M$)	4.53	4.77	5.01	4.26	4.24	5.12
	(9.72)	(8.09)	(10.47)	(7.59)	(9.21)	(8.41)
Observations	641	511	248	205	393	306

Notes: Data include highway procurements in Louisiana between Aug 2004 and Aug 2006. A procurement is a "Small Rebuild" if the rebuilding demand in Figure 1 is lower than the median. The parentheses contain the standard deviation.

3.4 Design-Based Estimation

3.4.1 Difference-in-Differences Approaches for high-dimensional controls

Since each auction consists of different construction types, I need to control for auction heterogeneity. To account for auction-specific observable item heterogeneity in my framework, I normalize the bid price through the procedure in Haile et al. (2003) before performing the design-based and structural estimations. In particular, I estimate a linear regression of the log-bid price on observables and calculate a normalized log-bid price by subtracting the predicted log-bid price from the actual log-bid price. My regression equation is

$$log(b_{ia}) = z_a'\delta + I_a'\eta + \xi_{ia}, \tag{3.1}$$

where b_{ia} is the bid from the firm i in the auction a, z_a is a vector of auction-specific item-category observables, and I_a is a vector of dummies for the different values of the actual bidders. It is desirable to use the information about quantity for z_a to control for project heterogeneity. Unfortunately, some items have different units of measure (e.g., some projects use "ton," and others use "lump sum"), and the dataset's characteristics do not allow me to align the units. Thus, I use dummy variables of each item category for z_a. Fixed effects for the number of observed bidders are included in the regression because I expect participation to affect bid price.

To control for auction characteristics, typical highway procurement studies use type-of-work and location dummies (Krasnokutskaya and Seim, 2011). While highway procurement includes a large number of items, few studies use this to control for auction heterogeneity.[6] The possible reason that researchers have not used this characteristic is that the data is high dimensional. For example, there are $p = 1,391$ item categories in my dataset, though the total number of auction-specific observables is $n = 1,152$. When the dataset has high-dimensional variables, that is, $p > n$, the least-squares estimator is not uniquely defined because of rank deficiency. Thus, I need to use methods other than least squares to control project heterogeneity.

I focus on a LASSO to deal with the issue of high dimensionality.[7] LASSO solves the minimization problem of the sum of squared errors and adds a 1-norm penalty to the estimation. I choose the penalty parameter λ for the LASSO by 10-fold cross-validation to minimize the mean-squared error.[8] The popular computational implementation for LASSO is the `glmnet` package in `R`. The penalty parameter is $\lambda = 0.009$ and the number of explanatory variables z_a^* is 220.[9] I then use $log(b_{ia}) - z_a'^{*}\delta_{Lasso}$ as the normalized log bidding price.[10]

[6] Using the data from the Massachusetts Department of Transportation, Bolotnyy and Vasserman (2021) study data for "scaling" auctions, in which private construction firms submit unit price bids for each piece of material required to complete a project. They find bidders respond strategically to uncertainty in respect of quantities by skewing their bids. Luo and Takahashi (2021) also study scaling auctions using data from the Florida Department of Transportation.

[7] A classic study is Hastie et al. (2009). In a highway procurement literature, Kim and Jung (2019) use a LASSO to predict the winning bid. They choose a set of crucial tasks that determine a bidder's bid amounts.

[8] Specifically, the method involves the following steps: (1) Randomly sort the observations; (2) Split the observation $k = 10$ into folds of (roughly) equal size $n_k \simeq n/K$. Let I_k denote the observations in fold k; (3) For $k = 1, \cdots, 10$, (a) exclude fold I_k from the dataset, (b) calculate the estimator $\hat{\beta}_{(-k)}$, (c) calculate the prediction errors $\tilde{e}_i = Y_i - X'\hat{\beta}_{(-k)}$, and (d) calculate $CV_k = n_k^{-1}\sum_{i\in I_k}\tilde{e}_i^2$; and (4) Calculate $CV = 10^{-1}\sum_{k=1}^{10} CV_k$.

[9] Among the selected variables, the top five affected item categories (e.g., large δ) are "701-07-F: yard drain pipe", "302-02-F: class ii base course", "713-06-F: temporary pavement legends and symbols", "730-09: electrical system", and "701-14-A: cleaning existing pipes."

[10] The hurricanes may affect the number of actual bidders and the bid price. Partialing out only the item-category information allows the normalized log bidding price to include information about the number of actual bidders. When evaluating the bid outcomes, I use the number of actual bidders in auction a as a control variable in the following difference-in-differences approach. The normalization procedure is also helpful to consider the structural first-price auction model because the model uses the number of actual bidders as an explicit factor to express the competitive environment between firms.

After I obtain the normalized bid using LASSO, I use a difference-in-differences approach with a continuous treatment variable to examine the causal effect of Hurricanes Katrina and Rita on highway procurement outcomes. Specifically, I use the following equation:

$$y_{iajp} = \beta_0 + \beta_1 Disaster_p \cdot d_j + \beta_2 Disaster_p + \beta_3 d_j + x'_{iaj}\gamma + \epsilon_{iajp}, \tag{3.2}$$

where y_{iajp} is the outcome variables of firm i in auction a at time j and parish p (e.g., log bid, including normalized value, and the number of actual bidders). The variable $Disaster_p$ is a continuous variable of rebuilding demand for housing and infrastructure in parish p, as defined in the last section. The variable d_j is binary; if the bid is related to a project procured after the hurricane, the term is equal to 1. The control variables x_{iaj} include (i) the backlog, (ii) the distance from the firm's plant to the project site, (iii) the dummy variable related to market share, and (iv) the number of actual bidders when I evaluate the bid outcomes. The variable ϵ_{iajp} is the error term. The coefficient β_1 provides the average effect of the intervention on the treatment group.[11]

The above two-step method is convenient because the normalized bid can be easily applied to the difference-in-differences approach and structural first-price auction model. However, there are two alternative approaches to deal with the high-dimensional controls. First, I can combine Equations (3.1) and (3.2). Specifically,

$$y_{iajp} = \beta_1 \underbrace{Disaster_p \cdot d_j}_{treatment} + \underbrace{\beta_0 + \beta_2 Disaster_p + \beta_3 d_j + x'_{iaj}\gamma + z'_a\delta + I'_t\eta}_{\text{a set of control variables}} + \epsilon_{iajp}, \tag{3.3}$$

[11] There may be a time-varying parish-level control variable related to the auction outcomes and construction demands after the disasters. In that case, it could be helpful to obtain a more precise estimate. However, I have been unable to determine such a variable despite my best efforts.

Belloni et al. (2013) proposes "post-double-selection" to select controls. The approach involves the selection of a set of control variables that are useful for predicting the treatment in the first step and selecting additional control variables that predict outcomes. Since the treatment variable is exogenous in my paper, I use the "post-single-selection" referenced in Belloni et al. (2013) to select the control variables. That is, I select a set of control variables that predict outcomes.

Second, I can aggregate to reduce the dimension of control variables, although the approach has drawbacks because a control variable must be selected arbitrarily. In particular, I can use aggregate categories of construction items. LADOTD (2016) classifies the specific highway-project construction types. A three-digit number defines the most extensive aggregation level. For example, "202: Removing or Relocating Structures and Obstructions", "501: Thin Asphalt Concrete Applications", "601: Portland Cement Concrete Pavement." Each category includes specific items with high-dimensional characteristics such as "202-01: Removal of Structures and Obstructions" and "202-04: Excavation, Disposal and Backfilling of Non-Contaminated Overburden." To reduce the dimensionality, I can use three-digit level categories to define z_t using dummy variables if an auction t includes that three-digit number. Since there are 66 categories, I can obtain the average treatment effect on the treated using OLS estimator.

3.4.2 Results

Table 3.3 shows the results of the difference-in-differences approach. Column 1 provides a standard difference-in-differences estimate. Column 2 includes distance, backlog, and firms' fringe status as control variables, and Column 3 uses normalized bids and control variables. Column 3 is my preferred specification. There are two points. First, the effects of hurricanes on bids, winning bids, and second bids are positive and significant, suggesting the hurricanes increased the costs of highway construction. Second, the number of bidders in the affected area decreased after the hurricanes, indicating that some bidders did not participate in the usual highway procurement.

One possible concern regarding the decrease in the number of bidders after the hurricanes in the damaged parishes is that project size might affect the result. If the projects become larger after the disasters, the number of bidders might become smaller. To check the channel, I use the quantity for Superpave asphaltic concrete, one of the most common items, to proxy the project size. A difference-in-differences estimation with three interaction terms shows that the effect of the quantity of Superpave asphaltic concrete after the disaster in the damaged parish on the number of bidders is positive but statistically insignificant.[12] I find little evidence that the project size affects the number of bidders after the hurricanes in the damaged parishes.

Firm behavior may differ depending on status. For example, firms with a relatively small market share in highway procurement may have much construction works from private markets such as housing. Thus, these firms may be affected more by the increased demands of recovery projects such as rebuilding houses. On the other hand, large firms may have market power, and they might increase the bid using the market power more after the shock of the natural disaster. I perform a difference-in-differences estimation with triple interaction

[12]Specifically, I use the following regression: $n_t = \gamma_0 + \gamma_1 Disaster_p + \gamma_2 d_j + \gamma_3 Superpave_t + \gamma_4 Disaster_p \cdot d_t + \gamma_5 Disaster_p \cdot Superpave_t + \gamma_6 d_j \cdot Superpave_t + \gamma_7 Disaster_p \cdot d_j \cdot Superpave_t$, where n_t is the number of bidders in auction t and $Superpave_t$ is the quantity of Superpave asphaltic concrete. γ_7 is my interest in the analysis.

to analyze the heterogeneous impacts of the hurricanes.[13] I check two cases. First, I use all samples. Second, I focus on the auction that includes non-fringe and fringe firms to check whether firm behavior changes within an auction. Both estimated coefficients are imprecise. Thus, I find little evidence that the firm size affects bidding behavior.

Table 3.4 compares the results of the four methods for controlling high-dimensional variables discussed in the previous section. Model 1 restates the results obtained in Table 3. Model 2 uses Equation (3.3) and selects controls using post-single-selection methods. All Model 2 estimates are larger than those for Model 1 and are statistically significant. Models 3 and 4 concern the case when the item category is aggregated to a lower dimension. Model 1 is my preferred specification because the approach is consistent with the structural approach discussed later. Model 3 shows that the estimates are very imprecise. This may be because 68 control variables are considered for 1,157 observations. By contrast, when I use a post-single selection method, Model 4 represents the bid increase after the hurricanes, though the estimates of bids are statistically insignificant.

The bids can only be observed if the firms enter the auction. This causes a selection problem, motivating me to use a Heckman selection model. In using this approach, I need to find an independent variable related to the selection process that has no direct effect on the bid value (exclusion restriction). I use the number of potential bidders as such a variable for two reasons. First, each firm can expect the number of potential bidders at the entry point because the LADOTD publishes a list. Thus, the variable affects the selection process. After the entry decision, each firm is able to observe the number of actual bidders and thereafter

[13] In particular, I use $log(b_{itjp}) = \gamma_0 + \gamma_1 Disaster_p + \gamma_2 d_j + \gamma_3 Fringe_i + \gamma_4 Disaster_p \cdot d_j + \gamma_5 Disaster_p \cdot Fringe_i + \gamma_6 d_t \cdot Fringe_i + \gamma_7 Disaster_p \cdot d_j \cdot Fringe_i$, where $Fringe_i$ is binary if the firm is a "fringe" firm; γ_7 is my interest

ignore the number of potential bidders. Thus, the number of potential bidders should not affect the bid value. Second, the existing literature uses this variable to discuss the selection (c.f., Roberts and Sweeting (2011)). In the first stage, I estimate a probit model of the decision to enter as a function of regressors in the difference-in-differences approach and the number of potential bidders. Then, I use the predicted probabilities to form an estimate of the inverse Mills ratio and include it in a second-stage bid regression.

The results are set out in Table 3.5. Column 1 shows the second-stage results from regressing the log of observed bids on auction covariates. Column 2 uses normalized bids. The negative and significant coefficient on the inverse Mills ratio is consistent with bidders being a negatively selected sample of potential entrants. There is potentially a concern that the exclusion restriction may not be valid because the potential bidders may be related to unobserved firm characteristics. For example, firm engineering skills, which are unobservable to researchers, are related to the bid value; the number of potential bidders is related to that variable. To check the validity of the exclusion restriction, I use the backlog variable as a proxy for firm engineering skills and study the correlation between the variable and the backlog variable. The value of the correlation coefficient is -0.15, reinforcing the validity of the exclusion restriction.

I conduct a placebo analysis to evaluate the effectiveness of my controls in matching the pre-treatment patterns of parishes; Table 3.6 shows the results of this analysis. In particular, I restrict the analysis to data before August 2005 and estimate the "effect" of the hurricanes had they occurred in February 2005. This false experiment tests whether my outcome variables have different trends in the treatment and control counties in the decades leading up to the disaster intervention. Because the period examined is immediately before the hurricanes, the finding of non-zero differences between leveed counties and controls would be evidence of selection bias.

Column 1 shows the unconditional difference between treatment and control counties. Column 2 adds control variables. Column 3 uses a normalized bid. Columns 1 and 2 indicate that outcome variables related to bids are significantly different between the treatment and control counties. The evidence suggests that if I use a non-normalized bid in the main analysis, the approach may violate the parallel trend assumption of the difference-in-differences approach. However, Column 3 shows no statistically significant differences and the small magnitude of the coefficients across treatment and control counties after normalizing bids using a LASSO, suggesting that the placebo test passes when I use normalized bids.

Collusion is a serious problem after natural disasters. Federal Trade Commission and Department of Justice (2006) states, "the Antitrust Division of the Department of Justice and the Federal Trade Commission ("the Agencies") will not tolerate any attempt by competing businesses to undertake naked price-fixing or market allocation agreements and thereby prey on those affected by Hurricanes Katrina and Rita." To check whether or not the collision occurred, I use a method proposed by Bajari and Ye (2003). In particular, I test the conditional independence assumption; conditional on the firm's characteristics, firm i's bid and firm j's bid are independently distributed.

Assuming the coefficient of correlation between the residual of firm i's bid function and firm j's bid function, ϵ_{it} and ϵ_{jt} is ρ_{ij}, then the test of conditional independence is equivalent to the test of the following null hypothesis: $H_0 : \rho_{ij} = 0$. Let r be the correlation coefficient calculated from the sample data, the Fisher Z transformation is given by $Z = \frac{1}{2} \ln \frac{1+r}{1-r}$. The test statistic is $z = Z\sqrt{n-3}$ for each pair of firms whenever $n > 3$. I divide the sample into two groups based on the demand, with a focus on the large demand group, and compare the number of firm pairs for which the null hypothesis can be rejected at a 5% significance level. I cannot find such pairs. Thus, I cannot find evidence of collusion in the highway auction before and after the hurricanes.

Overall, the difference-in-differences approach shows that rebuilding demand increases the bid, leading to a rise in procurement costs. Moreover, I find that the number of bidders decreases after the hurricanes. Based on the results, there are three possible explanations for why firms change their bidding strategy; these are used to construct a bidding model. First, as a result of the recovery projects, the firm expects increasing exogenous demand for construction labor and material, increasing labor, material, prices, and thereby increasing bids. Such a channel requires me to consider exogenous demand shocks in my bidding model. Second, the opportunity cost for participation may increase because of the exogenous demand shock, suggesting that I should use an endogenous entry model to examine the opportunity costs. Third, collusion may be a factor in increasing the bid, but I do not find any evidence of collusion. Thus, I do not consider this characteristic in my model.

The evidence also raises questions about the auction market. For example, how much could the government save in procurement costs if the hurricanes had not occurred? If the government subsidized the bids, how would the overall procurement surplus change? These questions motivate me to use a structural approach to auctions.

Table 3.3: The Effect of Hurricanes on Highway Auction

	naive (1)	control (2)	normalized (3)
Log Bid	0.061	0.052	0.017
	(0.031)	(0.030)	(0.007)
Log Winning Bid	0.060	0.047	0.012
	(0.032)	(0.030)	(0.007)
Log Second Bid	0.065	0.051	0.016
	(0.031)	(0.030)	(0.007)
Number of Bidders	−0.148	−0.141	–
	(0.036)	(0.035)	–
Observations	1152	1152	1152

Notes: Robust standard errors are in parentheses. Model (1) provides a standard difference-in-differences estimate. Model (2) uses control variables. Model (3) uses normalized bids.

Table 3.4: The Effect of Hurricanes on Highway Auction by Method

	2-Step (1)	1-Step (2)	Agg (OLS) (3)	Agg (LASSO) (4)
Log Bid	0.017	0.033	−0.003	0.019
	(0.007)	(0.014)	(0.018)	(0.016)
Log Winning Bid	0.012	0.056	−0.008	0.016
	(0.007)	(0.015)	(0.018)	(0.016)
Log Second Bid	0.016	0.053	0.001	0.024
	(0.007)	(0.014)	(0.018)	(0.016)

Notes: Standard errors are in parentheses. Model (1) uses a standard difference-in-differences approach after LASSO normalizes the bids. Model (2) uses a difference-in-differences approach with high-dimensional controls. Model (3) uses a difference-in-differences approach with aggregated item categories. Model (4) uses a difference-in-differences approach with the variable selection technique applied to the aggregated item categories.

Table 3.5: Selection Evidence

	Naive (1)	Normalized (2)
Constant	15.769	12.280
	(0.279)	(0.051)
Log(demand)	0.040	−0.001
	(0.027)	(0.005)
Time dummy	0.154	0.017
	(0.147)	(0.027)
Log(demand) Time dummy	0.101	0.042
	(0.026)	(0.007)
Actual # of bidders	0.101	0.021
	(0.041)	(0.008)
Inverse Mills Ratio	−2.265	−0.176
	(0.262)	(0.050)
Observations	3469	3469

Notes: Standard errors are in parentheses. Model (1) uses log bids as the dependent variables. Model (2) uses normalized bids.

Table 3.6: Placebo Analysis

	naive (1)	control (2)	Normalized (3)
Log Bid	−0.086	−0.075	0.002
	(0.039)	(0.038)	(0.009)
Log Winning Bid	−0.080	−0.066	0.011
	(0.040)	(0.039)	(0.008)
Log Second Bid	−0.085	−0.070	0.006
	(0.039)	(0.038)	(0.008)
Number of Bidders	0.088	0.093	–
	(0.061)	(0.058)	–
Observations	641	641	641

Notes: Robust standard errors are in parentheses. Model (1) provides a standard difference-in-differences estimate. Model (2) uses control variables. Model (3) uses normalized bids.

3.5 Structural Estimation

This section describes my model of firm participation and bidding decisions and calibrates the model based on the motivations mentioned in the previous section. First, I briefly describe the standard first-price auction model with endogeneity entry. Second, I estimate the cost distributions as a function of auction characteristics from the bid distributions in sealed auctions. From these cost distributions, I obtain bidder profits conditional on entry. I then estimate equilibrium entry probabilities using data from all of the unrestricted auctions as a function of auction characteristics. These entry probabilities allow me to infer entry costs. Note that throughout this section, I use normalized bids based on the LASSO as described in the previous section.

3.5.1 Model of Firms' Participation and Bidding Decisions

Suppose there are N potential bidders and n actual bidders participate in the auction. I model a potential bidder's decision as a two-stage process. In the first stage, each potential bidder decides whether to participate in the auction. In the second stage, actual bidders prepare and submit their bids. The potential bidders have private cost information that is independently distributed F with density f. A potential bidder does not observe their project cost realization at the time of their participation decision but establishes this through the investment of bid preparation costs K. After the entry decisions are made but before bids are submitted, each participant learns the identities of the other participants.[14] The lowest bidder wins and pays their bid in a sealed-bid auction.

[14] This assumption is from Levin and Smith (1994). Athey et al. (2011), and Krasnokutskaya and Seim (2011) also rely on the assumption.

I begin with an analysis of the bidding stage and then use the results to analyze the participation stage. Suppose i is a participating bidder with c_i, and the set of participants is n. The expected profit of Bidder i is

$$\max_{b_i}(b_i - c_i) \prod_{j \in n \setminus i} [1 - G_j(b; n)], \tag{3.4}$$

where $G_j(b; n) = F_j(b_j^{-1}(b; n))$ is the probability that j will bid less than b. The lowest bidder wins the contract in the first-price sealed-bid auction and makes a profit equal to $b_i - c_i$ and all other bidders make zero profits. The first order condition for i's bidding problem is

$$\frac{1}{b_i - c_i} = \sum_{j \in n \setminus i} \frac{g_j(b_i; n)}{1 - G_j(b_i; n)}. \tag{3.5}$$

The equation provides a basis for estimating the bidder's cost distribution.

At the participation stage, firms compare the ex-ante expected profit conditional on entry to their entry cost K. Firms with entry costs below their expected profit decide to incur the entry fee to discover the cost of completing the project. Bidder i's ex-ante expected profit from participating is then

$$\Pi_i(p) = \sum_{n-1 \subset N-1} \left(\int \pi(c; n-1) dF_C(c) \right) Pr[n-1|N], \tag{3.6}$$

where $Pr(n-1|N)$ is the probability of observing $(n-1)$ firm competitors, given the number of potential bidders of N. Let $\pi(c; n-1)$ denote the expected equilibrium profit of a bidder with cost realization c. This reflects the fact that at the participation stage, the firm is uncertain about both its own project costs and the competitive environment it will face upon entry. The firm assess the probability that there will be $(n-1)$ competitors in the auction as

$$Pr[n-1|N] = \binom{N-1}{n-1} p^{n-1}(1-p)^{N-n}, \tag{3.7}$$

where $\binom{N}{n}$ denotes the binomial coefficient of choosing n firms out of N potential bidders.

3.5.2 Bidder's Bid Distributions

My approach to estimating the bidders' cost distribution follows Guerre et al. (2000), Krasnokutskaya and Seim (2011), and Athey et al. (2011). The first step fits a parametric model of the bid distributions in sealed auctions, allowing these distributions to depend on observed exogenous conditions. The second step uses nonparametric methods to estimate the implied cost distributions. Estimates of these primitives allow me to compute expected bidder profits conditional on entry and, from this, to infer costs.

Let $F(\cdot|X, Z)$ denote the cost distribution conditional on the observed auction conditions X (e.g., exogenous shock construction demands) and the observed firm conditions Z. I assume that X, Z, and the number of actual participants n are common knowledge among the bidders when they submit their bids. I assume that bidder costs are independent conditional on X, Z and that participants use equilibrium bidding strategies. I write the equilibrium bid distributions as $G(\cdot|X, Z, n)$.

In the estimation, I make parametric assumptions about the distribution of interest because of the relatively small size of my dataset. I assume that the log of the individual normalized bid component $\ln(b)$ follows a normal distribution, specified as:

$$G(b|X,Z,n) = \frac{1}{2}\left[1 + \text{erf}\left(\frac{\ln(b) - \mu\left(X,Z,n\right)}{\sigma}\right)\right], \tag{3.8}$$

where $\mu(X,Z,n) = X\beta_X + Z\beta_Z + n\beta_n + \beta_0$ and $\text{erf}(\cdot)$ is the Gauss error function.

I match the distribution to data using a generalized method of moments estimator. Here I summarize the moment conditions that I use to estimate the parameters of the firm-specific bid component.

To estimate the parameters of the mean of log-bids, $\ln(b)$, I exploit that

$$m_1 = E[X(\ln(b) - \mu(X,Z,n))] = 0. \tag{3.9}$$

I also assume that the number of potential bidders does not depend on the exact bids, but is related to the auction and firm characteristics. That is,

$$m_2 = E[N(\ln(b) - \mu(X,Z,n))] = 0. \tag{3.10}$$

Finally, since the actual bid residual distribution does not match well when the only two moment conditions in (3.9) and (3.10) are used.[15] To match the kurtosis moment, I use the following fourth moment condition.

$$m_3 = E[X(\ln(b) - \mu(X,Z,n))^4]. \tag{3.11}$$

[15]The result is available upon request.

Table 3.7 reports estimated coefficients of the bid-distribution parameters, and Figure 3.3 plots the distribution of sealed bids in my sample and the distribution predicted by my fitted models. To draw the actual-bid residual distribution, I calculate the residuals based on the estimates and observed covariates. I then simulate 1,000 observations using the estimated variance for the estimated density, which is used to draw each kernel density distribution. The estimated bid distributions fit the data well.

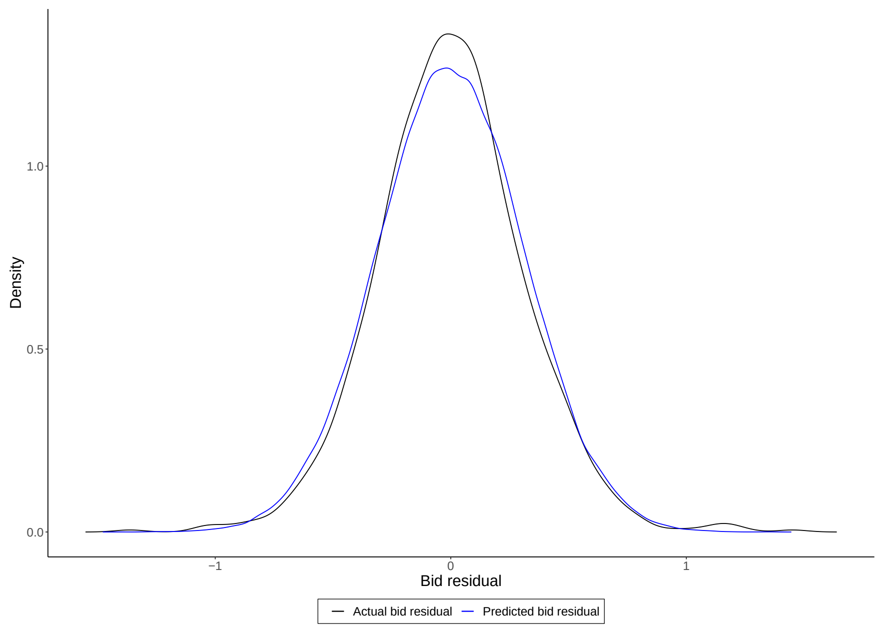
From Table 3.7, several points about the estimated bid distributions deserve mention. First, the row named "Log(demand)*Disaster Time(dummy)" is significantly positive, suggesting that bids affected by the hurricanes increased. Second, the effect of the number of actual bidders on bids is positive and statistically significant. This result is contrary to theoretical intuition since, holding other conditions constant, the assumption is that as the number of bidders increases, the value of bids decreases. Third, distance from firm plants to project sites positively affects bid prices. This result suggests that the firms near the project sites have comparative advantages. Forth, firms with backlog and fringe firms bid less aggressively.

Table 3.7: Estimated Parameters of Log-Normal Distributions of Bids

	Coefficient	Standard error
Ln(demand)	−0.011	0.005
Disaster Time (dummy)	−0.190	0.026
Ln(demand) * Disaster Time (dummy)	0.047	0.007
No actual bidders	0.037	0.006
Ln(miles)	0.072	0.010
Ln(Backlog)	−0.003	0.001
Fringe	−0.135	0.022
Constant	12.046	0.048

Notes: Bid distribution estimates are obtained using only auctions with two or more bidders. Bid distribution estimates are based on maximum likelihood as described in the text. The standard errors are in the parentheses.

Figure 3.3: Actual versus Estimated Density of Sealed Bid



3.5.3 Bidder's Cost Distributions

I turn to recovering the bidders' cost distributions. Assuming that bidders behave as predicted by the model, each bid must be optimal against the opponents' bid distributions. Given estimates of the equilibrium bid distributions, a bidder's cost c_i can be expressed as

$$c_i = \phi_i(b_i; X, Z, n) = b_i - \frac{1}{\sum_{j \in n \setminus i} \frac{\hat{g}_j(b_i|X,Z,n)}{1-\hat{G}_j(b_i|X,Z,n)}}, \tag{3.12}$$

where $\hat{G}_j$ and $\hat{g}_j$ are estimates of the distribution and the density, respectively, of the bids made by bidder j. I am able to calculate the distribution of bidders' costs as follows:

$$F(c|X, Z) = G(\phi^{-1}(c; X, Z, n)|X, Z, n). \tag{3.13}$$

Note that the distribution of bidders' costs does not depend on n; I deal with this by using a typical entry number of bidders, $n = 4$, to estimate the distribution following Athey et al. (2013).

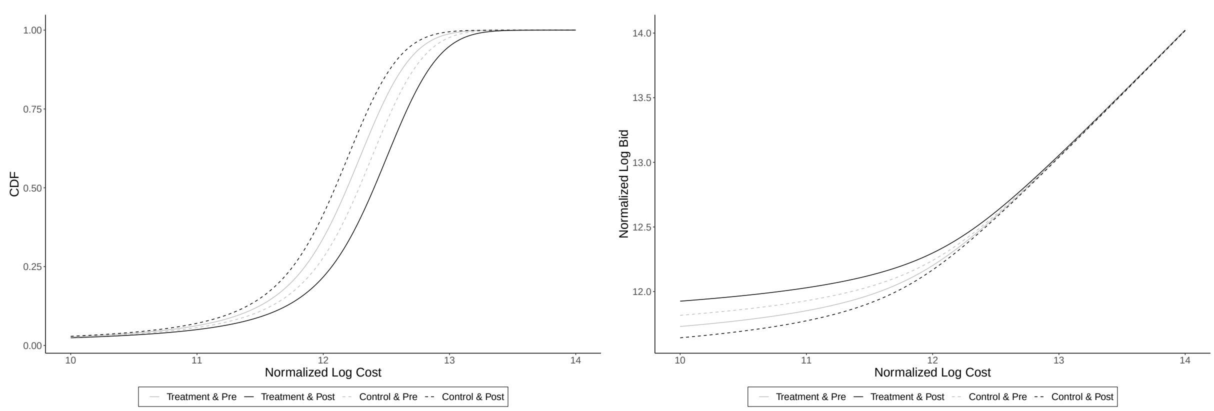
Figure 3.4 plots the cumulative distribution functions. I use the following conditions to draw these graphs: (1) rebuilding demand in Orleans Parish, the most damaged parish, as the treatment and West Carroll Parish, the least damaged parish, as the control, (2) average number of actual participating and potential bidders, (3) average firm characteristics (e.g., distance and backlog), (4) non-fringe firms. As Figure 3.4 indicates, the distribution of costs in the damaged parish is substantially shifted rightward as compared to the other distribution. Moreover, the damaged parish's estimated bid function is higher than that of other parishes. Thus, firms related to damaged parishes bid more, holding constant other conditions such as the number of bidders, project distance, and backlog.

Since I have the cost distributions and bid function conditional on (X, Z, n), I can define the ex-ante expected profit as follows.

$$\pi(X,Z,n) = \exp\left(\underbrace{\int \left(\phi^{-1}(c;X,Z,n)\right) f(c|X,Z)dc}_{\text{expected log bid}}\right) - \exp\left(\underbrace{\int cf(c|X,Z)dc}_{\text{expected log cost}}\right), \quad (3.14)$$

where $f(c|X,Z) = \frac{dF(c|X,Z)}{dc} = \frac{dG(\phi^{-1}(c;X,Z,n)|X,Z,n)}{d\phi^{-1}(c;X,Z,n)} \frac{d\phi^{-1}(c;X,Z,n)}{dc} = g(b|X,Z,n)\frac{d\phi^{-1}(c;X,Z,n)}{dc}$.

Figure 3.4: Estimated Cost Distributions $F(c|X,Z)$ and Bid Functions $\phi^{-1}(c|X,Z,n)$



Notes: I use the average auction and firm characteristics in my sample. I use rebuilding demand in Orleans Parish as the treatment and West Carroll Parish as the control.

3.5.4 Entry

I estimate the bidder entry costs for each auction, using the observed entry patterns. I first need to estimate the number of potential entrants at each auction. Denoting the number of potential entrants as N, and writing the entry cost as $K(X, Z, N)$, the indifference condition is

$$\sum_{n \subset N} \pi(X, Z, n) Pr[n|X, Z, N, i \in n] = K(X, Z, N). \tag{3.15}$$

Here $Pr[n|X, Z, N, i \in n]$ is the probability that n bidders enter given that i enters. Specifically,

$$Pr[n|X, Z, N, i \in n] = \binom{N-1}{n-1} p(X, Z, N)^n (1 - p(X, Z, N))^{N-n}. \tag{3.16}$$

I estimate the equilibrium entry probability p using the observed data and the following parametric model:

$$p(X, Z, N) = \frac{\exp(X\alpha_X + Z\alpha_Z + N\alpha_N)}{1 + \exp(X\alpha_X + Z\alpha_Z + N\alpha_N)}. \tag{3.17}$$

Table 3.8 reports the maximum likelihood estimates of the entry-probability parameters. Column 1 reports the estimates using all observations, Column 2 only uses observations of fringe firms, and Column 3 uses regular (non-fringe)-firm observations. There are four notable findings. First, Column 1 shows that firm entry after hurricanes insignificantly decreases for the sample of all firm observations. Second, the analysis reveals a negative, statistically significant effect of the number of potential competitors on a firm's participation decision. Third, an increase in distance from a firm's plant to the project site has a negative impact on entry, especially for the regular firms. Fourth, the extent of the backlog does not affect the probability of entry. Although the backlog should negatively affect the entry process because of capacity restrictions, I do not find evidence of this in my sample.

Using my estimate of $p(X, Z, N)$, combined with the estimated profit $\pi(X, Z, n)$, I can use equation (3.15) to infer entry costs for all auctions as a function of the covariates. Entry costs can be estimated using a typical auction setting, with an entry cost of $\$63,103$ in

Orleans Parish and $\$42,304$ in West Carroll Parish. In other words, the opportunity cost for auction entry increased after the hurricanes.

Table 3.8: Entry Probabilities

	All (1)	Fringe (2)	Regular (3)
Ln(demand)	−0.027	0.004	−0.056
	(0.021)	(0.028)	(0.032)
Disaster Time (dummy)	−0.163	−0.135	−0.190
	(0.111)	(0.152)	(0.163)
Ln(demand) * Disaster Time (dummy)	−0.042	−0.075	−0.011
	(0.030)	(0.042)	(0.044)
No potential bidders	−0.095	−0.085	−0.113
	(0.010)	(0.014)	(0.015)
Ln(miles)	−0.195	−0.069	−0.378
	(0.041)	(0.055)	(0.065)
Ln(Backlog)	−0.001	0.007	−0.009
	(0.006)	(0.009)	(0.009)
Fringe	−0.811	-	-
	(0.097)	-	-
Constant	1.705	0.216	2.718
	(0.223)	(0.290)	(0.350)
Observations	3469	2028	1441

Notes: The table presents the estimates from the binomial entry model. Column 1 uses all observations. Columns 2 and 3 use observations of fringe and regular firms, respectively. Standard errors are in parenthese.

3.6 Counterfactual Analysis

In this section, I use the estimates of the primitives of my structural model in two counterfactual simulations. First, I examine the additional cost of procuring each road project in the face of increasing construction demand by calculating the cost of no-hurricane recovery projects. Second, I assess the impact of subsidies. For the first simulation, I use 175 projects ordered in the year after the hurricane. I first estimate the winning bid using the estimated log-normal distribution under the established auction and firm characteristics. Next, I estimate the winning bid when the recovery demand, which is a characteristic of the auction, is set to zero and compare the winning bids when there is recovery demand and when there is no recovery demand.

I find that the government-paid prices for highway projects are, on average, 10% higher than for the no-hurricane case. The total project costs would increase by about $72 million based on the observed data. Since the average cost of one project is $3.5 million, the government could perform about 20 additional projects if the hurricanes had not occurred. One effective way to avoid these additional costs is to delay the timing of procurement. Lewis and Bajari (2011) discuss the effect of a scoring auction on the time of procurement. They find that a scoring auction is faster than other forms of auction. The welfare gain exceeds the increase in procurement costs. Bock et al. (2021) provides evidence that road damage caused an increase in vehicle crash rates and decreased vehicle speed. Comparing the costs of delaying the procurement against the benefits of quick delivery is a challenging and exciting question for future research.

Next, I analyze the effects of subsidy procurement policies after natural disasters. The subsidy here means that the government intends to pay a surcharge of several percent on the company's bid.[16] The subsidy improves each firm's profit and may allow firms offering public works with cheaper costs to enter the market. The simulation steps are the following: (i) I draw $N = 10$ from the estimated cost distribution. (ii) Using the bid function and the private cost information, each firm can calculate the expected profit.[17] (iii) If the expected profit is higher than the entry threshold, the firm participates in the bidding. Finally, (iv) each participating firm can calculate their bid, and a minimum bidding firm wins the auction. When considering subsidy policy, I inflate bids by $\alpha\%$ in step (ii). As a result, the expected profits increase, and some firms decide to enter the auction. This entry may cause a decrease in procurement costs.

Table 3.9 presents the exercise for an Orleans Parish auction after the hurricanes and using average firm characteristics. There are three notable points from the analysis. First, while firm profits decrease if subsidies increase, the government can save on procurement costs because firms that can provide projects inexpensively can enter the auction. Second, to provide participation incentives, the government has to raise taxes. Third, these factors lead to a negative surplus if the government introduced a subsidy policy after the hurricane. Thus, while subsidies encourage firms to participate in the auction, the method is not the best for highway procurement through auction after the hurricanes. Note that this analysis does not consider the reserved prices and engineer estimates because of data limitations. Increasing reserved prices and engineer estimates may have implications; this is an interesting area for future research on procurement following a natural disaster.

[16] Krasnokutskaya and Seim (2011) examine the effect of subsidies on the participation of small firms in highway projects.

[17] Firms have to pay the entry cost to understand their cost in the model. I weaken this assumption in the exercise.

Table 3.9: Comparison of subsidies

	No subsidy (1)	3% subsidy (2)	5% subsidy (3)
(i): Δ Firm Profits ($)	0.0	-9074.5	-15044.2
(ii): Δ Procurement Cost ($)	0.0	12943.3	21365.2
(iii): Tax ($)	0.0	9420.6	15388.5
(i) + (ii) - (iii): Surplus ($)	0.0	-5551.9	-9067.6
Bidder entry	3.6	4.0	4.2

Notes: The table reports the implications of subsidy policies. The actual data is Orleans Parish and average firm characteristics. The potential bidder is ten and simulates 5000 auctions in each column. The dollar term is a normalized value by Lasso.

3.7 Conclusion

This study examines how an increase in the number of construction-related natural-disaster-recovery projects affects the bidding behavior of construction firms for regular highway improvement projects. I first normalized the observed bids using a high-dimensional sparse regression technique. I then used a difference-in-differences approach to establish that the bid prices significantly increased and the number of bidding firms decreased after the hurricanes. Next, I analyzed the results using the standard method of structural estimation of first-price sealed-bid auctions. The results showed that the opportunity cost of bidding increased and that the procurement cost increased by 10% compared to the hypothetical case where no disaster occurred. Moreover, I set out the implications of subsidy policies and showed that the optimal strategy in terms of social welfare is to not provide a subsidy.

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