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Three Essays on Moral Hazard and Federal Disaster Financing

A dissertation submitted in partial satisfaction
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Doctor of Philosophy
in
Economics

by

Daniel Anthony Szmurlo

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Three Essays on Moral Hazard and Federal Disaster Financing

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Abstract

Three Essays on Moral Hazard and Federal Disaster Financing

by

Daniel Anthony Szmurlo

Every year, the federal government provides billions of dollars in disaster assistance to homeowners in the form of subsidized insurance, post-disaster relief, and mitigation grants. Such transfer programs can have negative resource effects if they encourage moral hazard, either for the recipients of the transfers or for the elected officials who work with bureaucracies to oversee their allocation. This dissertation explores how federal disaster programs influence the choices of landowners, as well as how the incentives of elected officials influence the allocation of disaster-related expenditures. In doing so I am interested how policy that intends to decrease the long-term local costs of disasters can hinder adaptation and increase the asset base at risk.

The first chapter explores how federal subsidized disaster insurance influences land-use decisions. I use satellite land cover data spanning 1973-2000 and historical flood maps to measure how rate subsidies offered by the National Flood Insurance Program in the 1970s induced land to be converted to developed use, increasing the exposure of capital and households to flood risk. Using ordinary least squares and instrumental variables strategies, I find that a year of subsidy availability in the 1970s had a positive and growing effect on the probability of development for inland floodplains over time, suggesting that induced stocks of housing acted as coordination devices for new development funds in later years. For coastal floodplains, I find that subsidy availability increased development probability in the short-term, but overall development growth rates in the region tempered the overall legacy of the subsidies. Calculations focusing on the Mississippi

River Basin reveal an extra year of subsidy eligibility in the early stages of the NFIP increased expected flood costs by approximately \$250 million dollars per year for the entire Basin, measured at the year 2000.

In the second chapter, joint with Sahaab Bader Sheikh, we test for the presence of tactical redistribution in the allocation of post-disaster relief through the Federal Emergency Management Agency, a process that the President has strong discretion over. We find strong evidence that House electoral competition and Representative party alignment influence the amount of relief going to a region. We then calculate the resulting aggregate distortions from a baseline "politically-neutral" allocation and analyze which constituencies generally benefit from tactical redistribution. Relief packages increase by \$450,000-\$900,000 for zip codes in House districts with incumbent Representatives aligned with the President. Relief packages also increase for zip codes in House districts with incumbent Representatives unaligned with the President if the district is more competitive. We find that zip codes with more white, older homeowners tend to benefit from electoral influences, while more urban, nonwhite zip codes do not.

In the third chapter, joint with Sahaab Bader Sheikh, we investigate how elected federal officials at different levels influence the bureaucracies that allocate disaster-related expenditures. Using hazard mitigation grants from the Federal Emergency Management Agency from 1997-2020, we examine how the allocation of grants changes when the agency moves from being independent with direct Congressional oversight to being subsumed into the larger Department of Homeland Security in 2003. The restructuring represents an expansion of executive power over the operations of FEMA at the expense of Congressional influence. We that prior to the restructuring in 2003, Representatives successfully divert mitigation funds to their own constituencies to the order of 50%-150% of the median federal contribution per zip code. In addition, during this period they are also successful at using coalitions with Representatives within their state to secure hazard mitigation funds for other districts. Diverted funds through direct subcommittee membership and coalitions represent 7.2% of the total HMA budget. The 2003 restructuring of FEMA nullifies the benefits of both direct subcommittee membership and coalitions, showing how the expansion of executive oversight results in the preferences of the President dictating the allocation of grants at the expense of Congressional preferences.

Contents

Chapter 1

Federal Flood Insurance Subsidies and Induced Floodplain Development

1.1 Introduction

The use of subsidies by the federal government is a pervasive feature of the U.S. economy. For example housing, energy, and agriculture are all federally subsidized ([1, 2, 3]). Policymakers commonly justify subsidies to address market distortions, however, any efficiency gains from a corrective subsidy can be negated if the subsidy encourages moral hazard. Subsidized constituencies can act on the legislated incentives to increase their returns, and in doing so raise the overall cost of the subsidy program. These added resource costs are apparent in the agricultural sector - federal ethanol subsidies have been shown to increase the acreage devoted to corn, increasing C02 emissions and environmental degradation ([4, 5]). In addition, federal subsidized crop insurance has been shown to disincentivize adaption, increase land in production, and alter agricultural chemical use ([6, 7, 8]).

The federal government supports a wide range of subsidies related to natural disasters including hurricanes, floods, and wildfires. These phenomena are predicted to increase in severity and/or frequency due to climate change ([9]). The extent that adaptive responses will temper the economic losses from intensified disasters will depend on the cost of risk faced by individual landowners. Federal subsidy programs, such as insurance subsidies, post-disaster relief, or firefighting expenditures can create a moral hazard by encouraging development in disaster-prone regions. In the case of insurance subsidies, the cost of risk borne by the landowner is set below the actuarially fair rate, increasing the rents gained from converting to and keeping at-risk parcels at developed use. As a result subsidies send maladaptive signals that limit the ability of housing markets to efficiently adjust to changing risk levels.

I study the effects of National Flood Insurance Program (NFIP) subsidies on levels of floodplain development. The National Flood Insurance Program is a federal program and effectively the sole provider of flood insurance in the United States. Currently, 3.5 million structures inside floodplains are eligible for subsidized flood insurance through the NFIP, and 20% of all NFIP policyholders pay a subsidized rate ([10]). Flood insurance rate subsidies for NFIP policyholders are justified as increasing take-up rates, assisting lower-income households, and protecting property values ([11]).[1] Rate subsidies also create a moral hazard by encouraging occupancy of flood-prone regions. I show that NFIP rate subsidies induced land to be converted to developed use, increasing the exposure of capital and households to flood risk and the cost of flooding events in the United States.

The NFIP was established by Congress in 1968 to offer policies to home and business owners covering flood damages in the absence of private insurance. Private insurers had stopped covering flood damages over forty years prior due to lack of reinsurance and the

[1] Senators of coastal states are particularly in favor of keeping insurance premiums low ([12])

actuarial difficulties associated with covering floods ([13, 14]). Since its inception, the NFIP has offered a set of rate subsidies to structure owners in floodplains that provide the recipient with a 60 to 99 percent discount off of the full-risk rate. For example, in 1975 the owner of a subsidized structure in a coastal zone subject to storm surges would pay as little as 25 cents for $100 of coverage on the building. This amounted to 1% of the $25 for $100 worth of coverage that the structure would pay without subsidies ([15]).

Individuals cannot purchase flood insurance unless their community (municipality or unincorporated county) joins the NFIP, which entails the adoption of federally-approved zoning ordinances. To expand the policy base and ensure the Program's survival, Congress passed an amendment in late 1973 that mandated community participation in the NFIP. In the years following the amendment, the Flood Insurance Administration (FIA) within the Department of Housing and Urban Development struggled to complete final flood maps for all 20,000+ mandated communities. Due to the rules enacted by the NFIP regarding subsidy eligibility, while a community waited for its map, it experienced what the NFIP called an "Emergency Program," a duration in which all **new** construction was eligible to be grandfathered into a subsidized insurance policy, in addition to any existing structures ([16]). Communities varied in the number of years they waited, with some communities receiving their map in less than a year while others waited over a decade. After the conclusion of the Emergency Program, which was tied to the completion and publishing of the community's official flood map, new structures were no longer eligible for the subsidy, but existing structures still were. These rules for eligibility placed a premium on building early and building inside floodplains, allowing for savvy developers and landowners to capture rents by converting floodplain parcels to developed use. The subsidies for grandfathered structures still exist today.

This entry process into the NFIP for communities provides a "natural experiment" for investigating the effect of subsidies on development. I exploit variation in the length of a community's Emergency Program to identify the effect of subsidy availability on new development within floodplains. To measure the short and long-term effects of subsidy availability, I use high-resolution land cover data spanning five dates between 1973 and 2000 from the U.S. Geological Survey Land Cover Trends Project. This data provide 10 km^2 blocks of 60 m^2 pixels that allow me to observe when land converts from undeveloped use to developed use. I use the Federal Emergency Management Agency's (FEMA) Q3 Flood Data product, which outlines the extent of historical NFIP 100-year floodplains, to isolate land within floodplains. I focus on two samples of mandated communities located in flood-prone regions - a coastal sample of communities along the Gulf and Atlantic Coasts and an inland sample of communities in the Mississippi River and Ohio River Watersheds.

The role of subsidized insurance in influencing building decisions has been a long-held concern for policymakers ([17]). In the specific case of the NFIP and Emergency Programs, it is difficult however to identify causal effects on development outcomes because both the length and timing of the Emergency Program experienced by a community is unlikely to be exogenous to local economic indicators and is most likely subject to unobserved agency decisions. For example, development interests in a community with better waterfront amenities could engage in political rent-seeking to increase the time the subsidy is available, leading to an upwards bias on the effect of subsidy availability. Alternatively, the agency could devote more resources to the completion of the maps of the higher flood-risk communities, potentially leading to a downward bias on the impact of subsidies if flood-risk is correlated with waterfront amenities.

To address this endogeneity concern, this paper takes advantage of the 1973 amendment to the Program that mandated community participation and caused a large surge of communities joining the Program at roughly the same time. The amendment created a "queue" of community applications into the NFIP across America. This queue represents the number of nationwide communities already in the Emergency Program, waiting for their official flood map to be completed by the FIA, at the time any one community joins the Emergency Program. To instrument for Emergency Program duration, I isolate plausibly-random variation in the growth of this queue, captured by "size of stack" variables that calculate the number of communities that join the queue in a particular duration of time. This captures the bureaucratic congestion that could of occurred from relatively busier durations of time for the agency that influenced the timeliness of agency resources for a community's map. For example, I calculate the number of communities that join the Emergency Program in a particular month. A community that joins the Emergency Program during a month at which the agency received very few applications across the nation might be subject to a more organized and prompt map-completion process than a community that joins in a month along with many other communities.

I utilize the land-cover data in a series of two-date difference-in-differences equations that each use 1973, the first date of land-cover, as the base date. This strategy reveals the short-term effect of subsidies as well as the cumulative impact of subsidies over time. Ordinary least squares and instrumental variable strategies reveal that a year of insurance subsidy availability in the 1970s had a positive and persistent effect for floodplains in inland regions. The effect actually increases in both absolute value and relative value across ordinary least squares and instrumental variable specifications, going less than a half percentage point increase in development probability in the year 1980 to around a two percentage point increase in the year 2000, suggesting the presence of agglomeration effects. I conjecture that settlements and subdivisions induced by subsidies acted as coordination devices for new development funds in later years. For the coastal sample, OLS results are negative and insignificant, suggesting that the FIA targeted coastal communities with high development potential for quick map completions. IV results reveal that although the absolute effect is growing over time, going from 0.6

percentage points in 1980 to 4.5 percentage points in 2000, the absolute effect decreases, suggesting that a large portion of the induced stock was part of a "rush to build" and the development was temporally displaced (it would have occurred later in time in the absence of subsidies).

Finally, the paper uses back-of-the-envelope calculations to determine how much the National Flood Insurance Program's subsidy eligibility scheme increased the cost of flooding events. The increase in average annual flood cost in the year 2000 from an additional year of subsidy eligibility in the 1970s can be estimated using Census data to assume the contents of land cover pixels. Preliminary calculations focusing on the Mississippi River Basin reveal an extra year of subsidy eligibility in the early stages of the NFIP increased expected flood costs by approximately $250 million dollars per year for the entire Basin, measured at the year 2000.

Section 2 discusses the relationship between land use and the moral hazard generated by flood policy, as well as the existing literature on the issue. Section 3 discusses the relevant institutional background on the NFIP and the theoretical impacts of subsidies. Section 4 discusses the data. Section 5 discusses the econometric model and identification. Section 6 presents empirical results. Section 7 discusses extensions, and Section 8 concludes.

1.2 Background

Flood Insurance Provision

The nature of flood damages presents additional difficulties for any firm offering to insure them. The magnitude of losses and the high temporal and spatial correlation between claims require firms to have access to large pools of liquid capital or else bear risk of insolvency. This is true not only of flood but of catastrophes in general. For these reasons and more, catastrophes are often labeled as sources of "uninsurable risk" that government has a comparative advantage in insuring ([18, 19]).

Private firms in the U.S. began offering flood insurance in the 1890s, but immediately exited the market after the disastrous 1927 Mississippi River Floods ([20]). They were reluctant to reenter in the the years following - the American Insurance Association argued in 1956 "flood insurance covering fixed location properties in areas subject to recurrent floods cannot be feasibly written because of the virtual certainty of loss, its catastrophic nature and the reluctance or inability of the public to pay the premium charge required to make the insurance self sustaining." ([13]). After several failed attempts to establish federally-run programs in the 1950s, the National Flood Insurance Program was established through the National Flood Insurance Act of 1968.[2]

Government's comparative advantage in insuring catastrophes comes from its ability to tax or borrow from the treasury if required indemnity payments exceed collected premiums and other forms of financing. Under functioning markets, firms need to sufficiently compensate shareholders while minimizing the administrative costs of providing insurance to be competitive. Publicly provided insurance is not subject to this market discipline, but is however subject to the objectives of political actors ([21]). While a public agency is not required to turn a profit nor subject to the risk of insolvency in the same ways private firms are, its decisions are overseen by Congressional committees, who can threaten the agency with loss of funding or manpower if the agency executes a policy that hurts a committee member's constituents ([22]). By being shaped in the political sphere, insurance as much as a tool of risk management as it is a particularistic good to constituencies. Provision is then motivated by affordability and political inclusiveness

[2]For a history on the Federal Government's increasing role in disaster management and relief, see ([16]).

and likely to be subsidized by taxpayers.

Flood Risk

Flood risk alters the rents generated by a specific land use, as any risk of flooding will increase the expected cost associated with the use of a land parcel. The expected flood costs faced by a parcel will differ based on its specific use. Federal flood interventions such as subsidized flood insurance generate economic rents that factor into the market prices for land in the targeted regions. By altering the financial implications of flood risk for a parcel, federal flood interventions create ex-ante incentives that can affect the location and timing of conversion between land uses ([23]).

Subsidized flood insurance rates create a moral hazard by uncoupling the parcel's total expected cost of flooding and the expected cost of flooding borne by the parcel owner. Full-risk insurance rates roughly equal the expected cost of the flooding and force landowners to internalize any risk they take on from a particular land use.[3] In contrast, subsidized premiums allow the policy holder to only pay a fraction of the full-risk rate, allowing the landowner to internalize only a fraction of the risk while leaving the actual level of risk faced by the parcel unchanged.[4] This difference creates the potential for land conversions that are privately profitable but socially inefficient i.e. conversions that wouldn't have taken place at that time and place if the owner had to face the full expected cost of flood damages. Given that subsidized flood insurance policies through the NFIP cover flood damages on residential and commercial structures, this particular intervention could induce land conversions to developed use and increase the asset base at risk of flooding.

[3] In practice, an actuarially fair premium will also include added surcharges to cover administrative expenses and a risk premium.

[4] Expectations of ex-post aid does the same by disuniting the realized damages for the parcel and the realized damages borne by the owner of the parcel

Literature

Anecdotally, ex-ante moral hazard generated by federal flood interventions such as subsidized insurance rates is thought to be an important driver of floodplain development. Empirical settings that allow for the identification of land use conversions induced from these interventions have been elusive however, not just for flood interventions but across all natural disaster policies ([24]).

Concerning the NFIP, there have been few studies attempting to evaluate NFIP's performance in encouraging efficient floodplain usage. This is despite the fact that the Program's role in subsidizing floodplain development has been a concern for policymakers ever since its the Program's inception ([17]). Existing evaluations of the NFIP have offered suggestive evidence on whether the NFIP's influence in coastal regions has stood out apart from the greater demographic trends in the second half of the 20th century. [25] uses a panel of issued building permits for a collection of southeastern coastal communities and finds that being in the NFIP's Emergency Program had a small positive effect on the number of building permits issued that year. [26] uses survey data on home characteristics in the Florida Keys and postulates that flood insurance availability may have stimulated growth in development. [27] find that flood insurance availability through the NFIP is associated with a four to five percent increase in population levels in higher-risk counties. [28] examines NFIP membership in Florida and finds that participation in the NFIP increased housing starts for noncoastal counties but decreased starts for at-risk coastal counties.

As flood patterns change, housing markets can provide effective signals of the changing costs and guide adaptive responses. The hedonics literature has documented the resulting price differentials from flood risk in a variety of location and disaster circumstances (for examples see [29], [30], [31] or [32]). Subsidies can distort the information released by price signals by dampening any capitalized differences in flood risk ([33]). [34] provides evidence of this by demonstrating how property values New York City decrease in response to proposed cuts in subsidy levels.

Outside of the topic of induced development, recent economics literature on the NFIP has focused on the redistributive effects of the premium subsidies ([35, 36, 37], policy tenure [38]), the cognitive biases and other determinants that influence take-up of insurance ([39, 40]), program reforms ([41, 42]), and the modern-day potential for private provision ([43, 44]).

Climate change is predicted to increase the severity and frequency of wildfires ([45, 46]). Similar to flood interventions, both ex-ante fire prevention and fire-fighting expenditures create moral hazard by encouraging low-density development in riskier and more remote regions. Also similar to flood interventions, there is only a small literature examining this moral hazard. [24] finds that wildfire suppression on public lands has a small positive effect on the probability of development for nearby private lands. [47] measures the implicit subsidy of firefighting expenditures in housing values and as an extension discusses the role of firefighting expenditures in influencing patterns of development in high fire risk areas.

Of the subsidy programs in the US, federal agricultural policy has received the majority of the attention in the literature. This can be attributed to both the longevity and size of the set of federal programs that target agricultural producers. Crop insurance is the most prominent of the programs. The moral hazard produced by subsidized crop insurance has been shown to disincentivize adaptation to extreme heat ([6]), increase the land in production ([48, 7]), and alter agricultural chemical use ([49, 8]). Additionally, income support programs for farmers have been shown to increase soil erosion ([50]), and expectations of ex-post relief have been shown to decrease farmers' insurance expenditures as well as inputs, yields, and revenue ([51]).

Lastly, there is a small literature documenting induced conversions from the implementation of public works projects such as damns and levees. Like subsidized insurance or disaster aid, public works projects are typically transfers from taxpayers to the receiving regions. However, unlike these interventions, public works projects alter the actual flood risk rather than solely export it. Therefore, any land conversion that takes place after the implementation of a public works project is done with full internalization of flood costs. [52] surveys 17 cities that received public works projects from 1936 to 1957 and finds that despite the projects nearly all cities experienced increases in expected flood damages due to the fact that the projects induced land conversion to developed use. [53] provides evidence that federal-financed drainage projects contributed to the conversion of forested wetlands in the Mississippi Valley to agricultural use. [25] finds that funding of shore protection for coastal communities is associated with an increase in the number of new building permits filed.

1.3 NFIP and Regulatory Hypotheses

National Flood Insurance Program

Examining the early stages of the NFIP is essential to identifying how the Program induced floodplain development, for the occurrence of these distortions is tied to a community's entry into the Program. In the Appendix is a list of definitions and acronyms for the various terms relating to the NFIP that are used in this section.

In 1968 Congress passed the National Flood Insurance Act, establishing the National Flood Insurance Program.[5] The Act ordered for federally-provided flood insurance be made available to structures in communities that agreed to develop and enact land-use

[5] For a history on the Federal Government's increasing role in disaster management and relief, see [16].

regulations approved by the Department of Housing and Urban Development. NFIP-defined communities correspond to common jurisdictions/permit-issuing places such as towns, villages, cities, townships, or unincorporated counties.[6] The Act designated the "100-year floodplain," the area in which there is at least a 1% chance of flooding in a given year, as the regulatory standard.

Premium rates were set based on a structure's location in or outside the 100-year floodplain, the height of a structure, and the structure's characteristics. For example, a structure outside the floodplain would be rated on whether it had a basement, whether it was one-story, two-story, mobile, or a split-level home, and if it was for residential or commercial use. A structure inside the floodplain was rated along the aforementioned characteristics as well as the elevation of its first floor in relation to the base flood elevation i.e. the height of the 100-year flood and its vulnerability to storm surges.

A consequence of the 40 year lack of private provision was the absence of any market mechanism to price flood risk and guide land use decisions. Recognizing this, policymakers allowed for any structure built pre-1968 **or** any "Pre-FIRM" structures (structures built before the completion of its community's first Flood Insurance Rate Map (FIRM)) to qualify for subsidized insurance, with the intention to avoid "punishing" any owners who did not have full knowledge of the structure's flood risk at the time of purchase.

The subsidized rate, known as the "Chargeable Rate," was not calculated as a function of a structure's actual risk profile, but was a flat rate that was common for all structures in the United States, across all risk zones and communities.[7] All pre-1968 or Pre-FIRM structures, both inside and outside the 100-year floodplain were eligible for the Chargeable Rate. Because the rate was flat, its size relative to the full-risk rate varied across different risk zones. Policymakers set the rate to a level that made it beneficial

[6] For example, Santa Barbara, CA, an incorporated city, is a NFIP community. Unincorporated Santa Barbara County, which includes several unincorporated communities, is also a NFIP community.

[7] The Chargeable Rate started to vary by flood zone after 1998.

only for structures within the 100 year floodplain. For structures outside the 100-year floodplain, the unsubsidized full-risk rate was and still is lower than the Chargeable Rate.

For example, in 1975, the annual Chargeable Rate for flood coverage for a one-story residential structure (building, not contents) with a basement was \$0.25 for \$100 of coverage per year ([15]). For an identical structure within a 100-year floodplain at or below base flood elevation, the full-risk rate ranged from \$0.33 to \$25 for \$100 of coverage per year, depending on the height of the structure in relation to the base flood elevation. The full-risk rate for any structure outside the 100-year floodplain was only as high as \$0.15 per \$100 of coverage per year, below the Chargeable Rate. See Tables A.1 and A.2 in Appendix for full rates for non-floodplain and floodplain structures, taken from the 1975 NFIP Flood Insurance Manuel.

The NFIP was slow to take off - by 1970 only 16 individual policies were in force. By the end of 1973, only 2,885 of the 28,000 permit-issuing places across America had started the process to join the Program. Of those 2,885 communities, only 575 had been fully incorporated into the Program.

Communities were slow to join the Program for several reasons. Joining the Program required the community to develop land use regulations that had to be approved by Department of Housing and Urban Development. Any community wanting to maintain a strong tax base and support the local economy probably had reservations against zoning requirements that would inhibit floodplain development. Additionally, communities would open themselves up to lawsuits from disgruntled citizens. [54] argues that the modern takings doctrine was gaining momentum during the early stages of the NFIP. The costs of a takings lawsuit in response to land-use regulations would have imposed costs beyond the budget of many small communities, disincentivizing the community from joining the Program.[8]

[8] Ultimately the Program's zoning ordinances and mandates have had little efficacy. Throughout

The availability of post disaster aid may have prevented communities from participating as well. The original 1968 Act contained a clause that denied post-disaster relief to any persons in flood-prone communities that could have purchased flood insurance. The perverse incentive produced by this clause encouraged communities to not participate in the Program in order to receive free aid ([59]).[9]

Motivated by the low number of communities joining the Program and the resulting low number of policies in force, the government desired a broader, more spatially diverse policy base to finance the primarily high-risk communities that had chose to already join the Program. The Flood Disaster Protection Act was signed into law in December of 1973 and prohibited all federally regulated or insured banking institutions from extending mortgage loans to properties within 100-year floodplain unless flood insurance was acquired for the property.

The 1973 Act also penalized communities that did not join the NFIP by declaring them ineligible for a several forms of federal financial assistance (this overruled the 1968 clause banning relief for communities that did in fact join). Although post-disaster relief would be allowed for participating communities, policymakers believed increased insurance take-up would decrease post-disaster expenditures.

Before the 1973 Act, under 3,000 permit-issuing localities (towns, villages, cities, and unincorporated county) had started the process to join the Program. This amendment drew the majority of the 20,000+ remaining flood-prone localities into joining. As

the early stages of the NFIP, the agency had little ability to monitor whether communities actually abided by the floodplain management regulations ([17]). The Government Accountability Office wrote to Congress several times detailing issues with the Program, including the agency's inability to review and approve individual communities' floodplain management plans in a timely manner ([55]), the lack of federal support for the state agencies designated with coordinating information to stakeholders ([56]), heavy reliance on private citizens and newspapers for notifications of communal noncompliance with regulations ([57]), and infrequent and unorganized visits to participating communities by regional agency directors ([58]).

[9] Public reaction to the NFIP was mixed during this period. Individual landowners in the Northeast were quoted as calling the NFIP as "immoral," "just another program the federal government is trying to shove down our throats," and wanting to regulate if one would be able to "paint their house" ([60].)

explained in the next section, community participation in the NFIP started with the community's entry into the "Emergency Program." Figure A.1 displays the breakdown of communities that enrolled into the Emergency Program prior to the Amendment and enrolled into the Emergency Program after the Amendment. This corresponds to communities that joined the NFIP voluntarily verses communities that joined after the mandate. Communities are classified into counties that border the Atlantic Ocean ("coastal counties"), counties that border the Mississippi River, and counties that border neither ("inland counties"). Figure A.1 reveals that approximately 40% of communities in coastal counties joined voluntarily, as opposed to only 15% of communities in Mississippi River counties and 8% of communities in inland counties. This discrepancy between county types could be rooted in several differences between the types. For one, higher perceived flood risk in the coastal regions could have motivated coastal communities to join voluntarily. In addition, higher levels of development within floodplains in coastal regions could have motivated coastal communities to join early and gain flood coverage for existing structures.

Entry into Program

Recall the premium subsidy was available to both pre-1968 and pre-FIRM structures - this section will define what it means for a structure to be "pre-FIRM." Most communities' enrollment into the NFIP started with the FIA drawing the community's Flood Hazard Boundary Map, which outlined any 100-year floodplain in the community ([60]). The Flood Hazard Boundary Map would act as a preliminary map before risk zones inside and outside the floodplain could be delineated. It was the primary source of information for developers and landowners before the final map could be produced. For most communities, this occurred immediately after the 1973 amendment in 1974 and 1975.

Once the Flood Hazard Boundary Map was created, the community would have to choose if it was going to participate in the NFIP. If it didn't, no one in the community would be able to purchase flood insurance, and the community was not eligible for several forms of federal assistance, including post-disaster relief. If it did, it would enter what is called the Emergency Program. Most communities joined the Emergency Program in the months after their Flood Hazard Boundary Map was made available. During this time, a full flood insurance rate study would be conducted. This task took the bulk of the time and effort of the agency. This study, called the Flood Insurance Study, would evaluate the actuarial rates for each risk zone, flood-way elevations, and other topographic and hydrologic details. The Flood Insurance Study was used to produce the official Flood Insurance Rate Map (FIRM) for that community, and the community would then exit the Emergency Program and enter into what is called the Regular Program. At this point the community was fully integrated into the NFIP.

Detailed flood maps are not created quickly - they normally take one to two years to produce without delays ([61]). Given the thousands of FIRMs the Administration needed to complete after the 1973 amendment and the limited funds available to do so, this task took longer than one to two years for most communities. Some communities stayed in the Emergency Program for longer than five years, some over a decade or even longer. In 1969, HUD relied on a small in-house staff of engineers whose work would supplement any local information on flood risks communities possessed. As thousands of communities starting entering the program, many with little information on their local risks, the Administration did not have the resources to convert communities out of the Emergency Program into the Regular Program. Three engineering firms were hired in June of 1973 to undertake Flood Insurance Studies. Two more firms were hired in 1975, and two more in 1977. The Administration reduced the number of contracted outside engineering firms from seven to three in 1983 ([17]).

Figure A.2 displays the number of communities enrolled in the Emergency Program and the Regular Program over time. The histogram reveals a large mass of communities that entered the Emergency Program following the 1973 amendment mandating participation. Communities were then filed into the Regular Program gradually over the next 15 years.

Figure A.3 displays the number of communities enrolled in the Regular Program over time, separated by communities that joined pre-Amendment and post-Amendment. Nearly all pre-Amendment communities were enrolled into the Regular Program by the early 1980s, while it took until the early 1990s to enroll the majority of the post-Amendment communities.

Hypothesized Impacts of Subsidies

The date at which the FIRM was published was the date after which all newly built structures were no longer eligible for the subsidized Chargeable Rate. Structures that broke ground during a community's Emergency Program were considered Pre-FIRM and were grandfathered into paying the Chargeable Rate. Structures that broke ground after the FIRM was completed were required to pay the full-risk rate. The duration of the Emergency Program represents the time between the date at which communities signaled to investors, developers, and residents that it was joining the NFIP and the date at which and full-risk premiums became binding for new construction.

Although the subsidy was available to structures built before the start of the Emergency Program, as they too counted as "Pre-FIRM," uncertainty over the community's participation in the Program could have deterred agents from taking complete action before this crucial signal. If a developer broke ground on a floodplain project before the start of the Emergency Program, they risked the possibility of the community not joining the Program and ultimately being left without flood insurance as well as federal disaster aid. Of the 28,885 communities across the U.S., 6,568 do not participate in the NFIP. Of those 6,568, 2,051 had Flood Hazard Boundary Maps produced by HUD in the years after the 1973 Amendment but ultimately did not participate in the NFIP. Therefore the probability of a community receiving its FHBM and opting out of the Program was nontrivial. Because of this uncertainty that existed pre-Emergency Program, this paper considers the start of the Emergency Program has the start of the duration in which subsidies had full potential to impact development outcomes (and hence the start of the 'treatment period").

The timeline of a community's involvement in the NFIP can be broken into three segments: before the Emergency Program, during the Emergency Program, after the Emergency Program. The primary period in which the NFIP produced moral hazard that could have induced development is during the Emergency Program. Extending the duration of the Emergency Program allowed for more time to break ground on development that would be grandfathered into the Chargeable Rate. Therefore, a longer Emergency Program time could have resulted in more development inside the floodplain.

Two points must be addressed about the preceding statement - one, the necessary requirements for subsidies to have any impact at all on floodplain development, and two, the necessary requirements for additional years of subsidy availability beyond the initial year of Emergency Program duration to have an impact.

It is an open question whether subsidies had an economically relevant impact on development levels within floodplains. The marginal effect of a year of subsidies on development is not guaranteed to be strictly positive for all communities. For one, the rents from developed use of the floodplain land parcels, which are a function of amenities related to both location within the floodplain and the community, would have needed to be at a level so that a rate subsidy would push the development rents above those of undeveloped use. A non-trivial fraction of communities across America may not have contained any marginal plots during the span of the Emergency Program. For example, see Figure A.4 in the Appendix. The figure displays 1973 and 2000 land cover from the Land Cover Trends Database on top of a base topographic map. Red pixels represent development, dark blue pixels represent water, light blue pixels represent wetlands, and the orange pixels represent grassland. Figure A.4 displays land cover for unincorporated Vermillion Parish, Louisiana. For this region, very little development occurred between the two dates. Surrounded by wetland and remotely located, the rents from developed use were too low, and the availability of subsidies, no matter how long of Emergency Program, could not induce land conversions.

In addition, if floodplains were already built up at the start of the Emergency Program, subsidies would have had little effect on new development starts. See Figure A.5 in the Appendix, which displays land cover for Rock Island, Illinois and Davenport, Iowa. Rock Island and Davenport make up two of the “Quad Cities” that straddle the Mississippi River. For the Quad Cities, the corridor surrounding the river was nearly all developed by the time the Emergency Program started for either community, so the availability of subsidies had little effect on new starts as there was little land to be converted at that time.

Addressing the second point, a large share of communities across America did in fact have a number of undeveloped parcels in their floodplains that offered sufficiently high amenity value to be converted via subsidies. Consider Figure A.6 in the Appendix, which displays Mandeville, Lousiana. This community experiences substantial development growth between the years 1973 and 2000. If subsidies did have a role in inducing development, it is also an open question whether development responded to additional subsidy years beyond the initial year of Emergency Program. Recall the enrollment into the Regular Program displayed in Figure A.2 - most communities that entered the Emergency Program after the amendment in 1973 experienced at least one year of Emergency Program. The long, varied delays in map completion would not have proven distortionary if development only responded immediately at the onset of the Emergency Program.

Any additional year of subsidy availability would affect development if there were parcels that couldn't have been developed at the start of the Emergency Program but possessed sufficient amenity value later on to be converted. Heterogeneity of flood costs and floodplain amenities within the community, growth in the valuation of floodplain amenities, the size of the community, and the presence of local construction frictions and congestion effects would all impact the potential for this type of parcel to exist. For example, if a community contained a set of high-amenity and a set of low-amenity parcels, and the valuation of floodplain amenities was growing over time, a longer Emergency Program would have allowed for the high-amenity parcels to be developed at the beginning of the Emergency Program and the low-amenity parcels to be developed later on once it was profitable to do so.

Agglomeration

The eligibility rules and levels of the Pre-FIRM subsidy resulted in a policy with both temporally nonuniform and spatially nonuniform application. In other words, the flat subsidized rate was not directly beneficial for non-floodplain landowners and was not available for parcels developed after the publishing of the flood map. Looking at the development effects of these subsidies in the medium-to-long term *after* the map completion links this paper to a greater literature on the roles of natural advantages verses agglomeration economies in shaping the spatial distribution of economic activity.

For example, [62] examines how historical portage sites in the United States coordinate present day economic activity. [63] examines the long run effects of the Tennessee

Valley Authority on manufacturing and agricultural employment in the subsidized regions. [64] examines the long-run effects of a West German transfer program that targeted regions along the Iron Curtain. A central theme in the latter two papers is explaining the long-term persistence of economic gains after a place-based policy has expired, while [62] similarly look at the long-term persistence of economic gains after a natural advantage becomes obsolete.

This paper examines the development gains from a temporary subsidy regime in the medium- and long-term following the regime's expiration. Any development that was induced during the years of Emergency Program duration could have been temporally displaced- i.e. development that would have occurred in the following decades in the absence of treatment. If this was the case, the cumulative effect of the subsidy duration would be diminishing over time as the subsidized communities experience decreased development flows in the years following the subsidy duration. In addition, the communities that experienced shorter or near-zero Emergency Program durations would"catch up" in terms of development levels to the communities that experienced longer subsidy durations.

Alternatively, the initial induced stock of development could have spurred agglomeration effects that would have increased the cumulative legacy of the subsidy durations. Bleakley and Lin (2012) and Kline and Moretti (2014) identify agglomeration economies as the force behind persistent increases in economic densities, while von Ehrlich and Seidel (2015) suggest that the examined transfer program's caused local governments to invest in new roads, sewage systems, and electricity networks, solidifying the locational advantage of the subsidized region.

For the Emergency Program durations, settlements and subdivisions induced by subsidies could have acted as coordination devices for new development funds in later years, attracting new development even after new development is not eligible for subsidies. Any roads or utility networks that were established during the subsidy years might have created locational advantages for developed floodplains that attracted new construction, even after the Pre-FIRM subsidy was no longer available. In addition, its likely that adding to or expanding subdivisions would involve less new fixed costs such as permitting than creating entire new subdivisions.

1.4 Data and Sample

Data

To investigate the extent and evolution of the stock of induced development, I use fine-scale land cover data from the **Land Cover Trends Project**. The Land Cover Trends Project (LCT) provides land cover in select blocks across the continental United States for the years 1973, 1980, 1986, 1992, and 2000. It is currently the only source of land cover for the continental U.S that goes back to the 1970s. The blocks are either 10 kilometers by 10 kilometers or 20 kilometers by 20 kilometers, made up of 60 meter by 60 meter pixels. Each pixel is classified as one of 11 land covers types according to the Anderson Level 1 system. One of those classifications is “developed.” Others include agriculture, forest, wetland, water, mining, barren, grassland, ice, mechanically distributed, and non-mechanically distributed. The developed classification refers to “areas of intensive use with much of the land covered with structures (e.g., high density residential, commercial, industrial, transportation, mining, and confined livestock operations), or less intensive uses where the land cover matrix includes both vegetation and structures (e.g., low density residential, recreational facilities, cemeteries, etc.), including any land functionally attached to the urban or built-up activity” ([65]).

The data are not wall-to-wall coverage of the U.S. but are comprised of 2,688 blocks randomly selected within 84 EPA-defined ecoregions. For example, one of the ecoregions is the Central Corn Belt Plains ecoregion, which encompasses portions of Illinois, Wisconsin, Indiana, and Missouri. Within this ecoregion, there are 40 blocks of land that are measured at each of the five dates. The land cover in Figure A.5, which displays Rock Island, Illinois and Davenport, Iowa, is taken from Central Corn Belt Plains ecoregion. In addition, another ecoregion is the Western Gulf Coastal Plain, which encompasses coastal Texas and Louisiana. Within this ecoregion, there are 54 blocks of land that are measured at each of the five dates. The land cover displayed in Figure A.6, which shows Mandeville, Louisiana, is taken from the Mississippi Alluvial Plain ecoregion. Figure A.7 displays the full extent of the Land Cover Trends Database.

To isolate the pixels from the Land Cover Trends Project that are inside the 100-year floodplain, I use FEMA's **Q3 Flood Data Product**. The Q3 data were created in the 1990s by digitizing existing Flood Insurance Rate Maps (FIRMs) into GIS file formats. The Q3 data include the FIRMS of the communities in 1,368 of the 3,142 county and county equivalents in the United States (see Figure A.8 in the Appendix for a full map of data coverage). The data delineate boundaries of 100-year and 500-year floodplain. Recall that the 100-year floodplain is the area in which insurance purchase was made mandatory by the government in 1973.

Information on each community's entry into the NFIP, including the dates of the Emergency and Regular Programs, is provided by the NFIP's **Community Status Book Report**. Additional county-level data on disaster losses are provided by the Spatial Hazard Events and Losses Database for the United States (**SHELDUS**). SHELDUS provides count, property and crop losses, injuries, and fatalities data for flooding and hurricane events from 1960 to the present ([66]). County level and county subdivision level demographic data are taken from **IPUMS-USA** ([67]). All additional spatial data is provided by ESRI Data & Maps.

Sample

Neither the Land Cover Trends Project nor the Q3 Flood Data Product provide wall-to-wall coverage of the continental U.S, so the first step in constructing the sample is isolating the land cover blocks that intersect with the flood data. Second, of the 84 EPA ecoregions, I focus on the ecoregions that are subject to notable flood risk. This includes ecoregions along the Atlantic Coast, ecoregions comprising the Upper Mississippi River watershed, ecoregions comprising the Lower Mississiippi River Watershed, and ecoregions comprising the Ohio River Watershed. The eight ecoregions that cover the Atlantic Coast from Texas to Maine make up the **Coastal Sample**, while the 11 ecoregions that cover the Watersheds make up the **Inland Sample.**

Next, in each sample I eliminate the LCT-Q3 intersections that contain no human settlements at anytime between 1973 and 2000. These discarded blocks are mainly blocks composed of public land, agricultural land, wetlands, mountainous regions, and forest. I also eliminate the LCT-Q3 intersections that contain no 100-year floodplain.

Each of the remaining LCT-Q3 intersections contains at least one NFIP community. The final major reduction of the raw data involves isolating NFIP communities that joined the Emergency Program **after** the 1973 Amendment. This is done for two reasons. One, given that the first date of land cover available is 1973, any community that joined pre-Amendment would experience Emergency Program years that are not captured between two dates of land cover. Second, focusing on post-Amendment communities allows for the use of the resulting map queue from the Amendment as an exogenous determinant of Emergency Program duration.

I also restrict my sample to communities that joined the Regular Program before 1984. In 1984, FEMA changed the way it completed maps in order to enter more communities into the Regular Program. It sent out a survey to communities still in the Emergency

Program asking about expected development in the coming years. From the results of that survey, FEMA identified "low-growth communities" and hastily produced less-detailed FIRMS for 1,187 of them by the end of the year. This paper will not consider these communities nor any community that entered the Regular Program after this date.

This results in 152 communities represented in the Coastal sample, and 88 communities represented in the Inland sample. The Coastal sample consists of 174,750 unique pixels, each observed at all five dates in time. The Inland sample consists of 261,294 pixels, each observed at all five dates in time.

1.5 Econometric Framework

To identify the effect of subsidy availability on development within floodplains, consider a linear probability model, in which development is observed at 1973 (the first date of land cover provided by the Land Cover Trends Project) and at a second date for each community in the sample. This second date can be 1980, 1986, 1992, or 2000 (the following four dates of land cover provided by the Land Cover Trends Project). The outcome $Y_{ict} = \{0, 1\}$ indicates whether land cover pixel i in community c is developed or not at time t. $Emergency_c$ refers to the number of Emergency Program years that pixel i's community experienced between the two observed dates. For development in pixel i, at time t in community c, the estimating equation is :

$$(1) \qquad Y_{ict} = \alpha_0 + \gamma_t + \gamma_c + \alpha_1(Emergency_c \times \gamma_t) + \beta X_{it} + \epsilon_{ict}$$

γ_t is a year fixed effect corresponding to the second date. γ_c is a community fixed effect. X is a vector of additional time-varying and time-invariant pixel and community attributes that would influence the probability of development. Time-varying variables include cumulative number of flooding disasters since 1960, measured at the county level. Time invariant variables include county population, unemployment rate, and dwellings density at 1970, 1960, and 1950, as well as county population at 1940. Also included are pixel-level attributes - distance to water feature, distance to water feature squared, and distance to nearest highway. Demographics are measured at the county level and not at a finer level such as at the incorporated place, because the majority of the land covered by the LCT (and more generally, the majority of nonpublic land in the US) is located in unincorporated counties and no municipality-level statistics exist.

α_1 is the percentage point difference in the change of probability of development from 1973 to the later date for a floodplain parcel with one more year of Pre-FIRM eligibility under the Emergency Program. The paper's hypothesis is that $\alpha_1 > 0$; all stated factors held constant, an extra year a community is in the Emergency Program the higher the probability of a floodplain pixel being developed at the later date. γ_t is the baseline effect of going from 1973 to the later date on the probability of development.

Equation 1 can be modified to include alternative fixed effects specifications. The treatment variable in this model is continuous, which allows for an additional specification in which the un-interacted *Emergency* years variable is used rather than a community fixed effect to pick up baseline differences in development probability. State fixed effects can also be included when using the *Emergency* years variable. In addition, pixel-level fixed effects can capture time-invariant pixel-level characteristics that impact development probability. Therefore pixel-level fixed effects can also be used instead of the combination of community fixed effects or the *Emergency* term with time-invariant covariates. Therefore OLS results under Equation 1 are presented through three specifications that vary in how baseline differences are captured: community fixed effects, Emergency Program years, and pixel fixed effects.

As previously mentioned land cover is available through the LCT for years 1973, 1980, 1986, 1992, and 2000. Therefore, any one of the four dates following 1973 can be used as the later date in the two-date model. In addition, all five dates can also be pooled into one model. The subsidy year variable needs to be altered for the pooled model - now define $Emergency_{ct}$ as the number of Emergency Program years experienced by pixel i's community up to date t. Let $Dates = \{1980, 1986, 1992, 2000\}$.

$$(2) \qquad Y_{ict} = \alpha_0 + \sum_{Dates} \gamma_t + \gamma_c + \sum_{Dates} \alpha_{1t} \left(Emergency_{ct} \times \gamma_t\right) + \beta X_{it} + \epsilon_{ict}$$

$\alpha_{1,1}$ would give the percentage point difference in the change of probability of development from 1973 to 1980 for a floodplain parcel with one more year in the Emergency Program, while $\alpha_{1,2}$ would be the same percentage point difference from 1973 to 1986. Like Equation 1, Equation 2 can also be modified to include alternative fixed effects specifications. OLS results under Equation 2 are also presented through three specifications: community fixed effects, Emergency Program years, and pixel fixed effects.

The communities I include in the sample influence the interpretation of the α_1's, both in the two-date and pooled samples. Recall I only include communities that entered the Regular Program before 1984, due to the shift in how FEMA completed maps. Therefore $\alpha_{1,1}$, the 1980 treatment coefficient reveals a combination of the contemporaneous effect of the subsidies along with the cumulative, post-Regular Program effect of the subsidies. This is because there are communities in the sample already in the Regular Program at 1980, as well as communities that are still in the Emergency Program at 1980.

The same combination will be present in $\alpha_{1,2}$, the 1986 treatment coefficient. The sample consists of communities that experienced Emergency Program years after 1980 and before 1984. Therefore the 1986 treatment coefficient reveals a combination of the contemporaneous effect of the subsidies along with the cumulative, post-Regular Program effect of the subsidies. $\alpha_{1,3}$ and $\alpha_{1,4}$, the coefficients for the 1992 treatment effect and 2000 treatment effect, solely reveal that cumulative legacy and do not pick up contemporaneous effects, as no communities in the sample are still in the Emergency Program after 1984.

Identification

In general, there are many community, county, congressional district, and state-level factors that could potentially influence the time it takes to draw a flood map, including the size of the community, how topographically diverse a community is, political pressures on the local and federal level, unobserved agency decisions, and bureaucratic delays. A concern for identifying the effect of a year of Emergency Program on probability of development is that the length of the Emergency Program may be endogenous to past and contemporaneous community-level economic indicators and disaster history. Unfortunately, disaster history as well as population are measured at the county level and not at the community level. Ideally, the completion of the map would be a "black box" to the community. The community would be unable to alter the contents of the map nor the duration of the Emergency Program. We know both of these assumptions to be untrue.

Communities with more water amenities could have petitioned the results of any Flood Insurance Study or delayed the map-making process more intensively than communities with less amenities. Intensity of local politics seems to play a nontrivial role in the duration of the Emergency Program, but this is difficult to separate from inevitable delays resulting from the bureaucratic process.

If communities with more development pressures experienced longer Emergency Programs, the OLS estimate of the relevant coefficient would be biased upwards. Alternatively, the Flood Insurance Administration could have implicitly targeted communities with higher development pressures or more amenities earlier in order to minimize subsidized floodplain development, allowing for communities with weaker development interests to stay in the Emergency Program longer. If this is the case, OLS estimates of the relevant coefficient would be biased toward zero.

This paper will exploit the the bureaucratic processes of the Flood Insurance Administration during the initial years following the 1973 amendment. To my knowledge, there is little written evidence that the Administration resolved maps in any systematic way. Only one recorded instance of systematic completion is known to the author at the time of this paper: the 1984 initiative mentioned earlier to target low-growth communities.

The large spike in communities joining the Emergency Program after the December 1973 amendment created a natural "queue" of unfinished maps. Given the evidence that the Flood Insurance Administration could not address in a timely manner the maps of the thousands of communities that joined, it is plausible that the agency devoted map-completing resources in a way that was determined by the chronological ordering of communities joining after the amendment. Therefore, a community further down in the queue of unfinished maps could be subject to a longer wait time than a community closer to the top.

A community's place in the queue could potentially be correlated with omitted community factors however as communities could have strategically "dragged their feet" and joined the Emergency Program at a later time to potentially reap the benefits of a longer Emergency Program. Therefore instead of considering the size of the queue, this paper exploits variation in the size of the "slice" of the queue a community falls into when joining the Emergency Program as a exogenous determinant of Emergency Program length.

To capture the role that bureaucratic congestion had in Emergency Program duration, I construct a variables for each community that counts, at the time that community enters the Emergency Program, the number of other communities across the nation that joined the Program in the same 5-day, 10-day, and 1-month duration. This paper conjectures that the size of the cohort that a community was placed in dictated the timeliness or organization of the resources the agency devoted to completing a community's map. A community could not control or observe the size of the cohort it entered the Emergency Program in. For communities that joined the Emergency Program in 1974 and early 1975, they did not know how many other communities across the nation were joining the Emergency Program at the same time that they were. Although communities could have "dragged their feet" and joined the Emergency Program at a later time to potentially reap the benefits of a longer Emergency Program, they didn't know how many other communities were doing the same. Moreover, communities did not know how the agency would direct its resources for communities in larger cohorts verses communities in smaller cohorts.

Figure A.9 displays the size of the queue plotted against the time at which communities joined the Emergency Program post-amendment. The queue rises steadily from 1973 to the middle of 1975, where it increases dramatically. This large surge in the number of unfinished maps corresponds to a deadline set by the NFIP for communities to file their paperwork and join the Emergency Program. Figures A.10 through A.12 displays the size of the cohort each community experienced graphed against date of entry into Emergency Program. As size of cohort corresponds to entry into the queue, i.e the growth rate of the queue, it makes sense that the steady growth in queue from 1973 through the first half of 1975 corresponds to fairly even trend in cohort size through that duration. It shows that in the year and a half following the amendment up until some point in 1975, the size of the five-day cohort ranges from near 0 to over 150, but does not follow any systematic trend. The size of the one month cohort is slightly rising across 1974 and 1975. The large spike in cohort size in the middle of 1975 corresponds to a deadline set by the NFIP.

Because the first stages utilize cross-sectional variation in queue variables, unlike the least squares regression the two stage least squares regression cannot be done with pixel or community level fixed effects. Therefore the two-stage least squares regression is done using the *Emergency* years variable to pick up baseline differences in development probability, which needs to be instrumented for in addition to that variable's interaction with the appropriate time dummy. The two-stage least squares estimator is given by the following three equations using the two date model. Equations 3 and 4 comprise the first stage. Equation 5 represents the second stage utilizing the predicted values from Equations 3 and 4. The dependent variables in the first stages are the Emergency Program duration (Equation 3), and the interaction between Emergency Program duration in years and the appropriate time dummy for community c at time t (Equation 4). $5Day$, $10Day$, and $1Month$ measure the size of the community's 5 day, 10 day, and 1 month cohorts at the time of entry into the Emergency Program.

$$\begin{aligned} (3) \qquad Emergency_{ct} =& \omega_0 + \gamma_t + \omega_1 5Day_c + \omega_2 10Day_c + \omega_3 1Month_c + \beta X_{it} \\ &+ \omega_4(5Day_c \times \gamma_t) + \omega_5(10Day_c \times \gamma_t) + \omega_6(1Month_c \times \gamma_t) \\ &+ \gamma_s + \epsilon_{ict} \end{aligned}$$

$$\begin{aligned} (4) \qquad Emergency_{ct} \times \gamma_t =& \omega_7 + \gamma_t + \omega_8 5Day_c + \omega_9 10Day_c + \omega_{10} 1Month_c + \beta X_{it} \\ &+ \omega_{11}(5Day_c \times \gamma_t) + \omega_{12}(10Day_c \times \gamma_t) + \omega_{13}(1Month_c \times \gamma_t) \\ &+ \gamma_s + \epsilon_{ict} \end{aligned}$$

$$(5) \qquad Y_{ict} = \alpha_0 + +\gamma_t + \alpha_1 \widehat{Emergency}_c + \alpha_2 \widehat{Emergency}_c \gamma_t + \beta X_{it} + \gamma_s + \epsilon_{ict}$$

For the "size of stack" variables to be valid instruments, the size of the cohort of communities across the nation that joined the Emergency Program during a certain duration, conditional on the specified time-invariant variables, disaster history and group fixed effects, must be both correlated with the number of Emergency Program years and have no direct effect on changes in development patterns from 1973 onwards except through the its effect on the length of that community's Emergency Program. The first stage will be able to test the first requirement and show if place in queue created bureaucratic delays that lengthened the duration of the Emergency Program.

Pre-Trends

Unfortunately there is no land cover data available pre-1973. In addition, before 1970 there is little demographic data available at the level of the permit-issuing place (municipality or unincorporated county). To evaluate how communities that differed in subsidy intensity fared previous to the Emergency Programs, I use population counts measured at the county level.

Looking at how county populations evolve from 1940 to 1970 will give auxiliary evidence regarding the validity of the empirical strategy. Ideally, the counties of less-subsidized communities would evolve similar to counties with communities that experienced more subsidy years. To aggregate community-level Emergency Program lengths to the county level, I use two methods of aggregation for both the coastal and inland samples.

For the first, I classify communities based on the number of Emergency Program years they experienced, either 0-2, 2-4, 4-6, 6-8, 8-10, or 10+. Next, I take the population count for the county each community is in, and find the average county population for communities in each Emergency Program bin, weighted by the number of pixels each community contributes in the bin. Note that this may result in counties showing up in more than one Emergency Program bins if a county contains communities that fall in different bins.

Alternatively, I also find the average Emergency Program length experienced by communities within a county, weighted by the number of pixels each community contributes. This gives each county an average Emergency Program duration that can be classified as between 0-2, 2-4, 4-6, 6-8, 8-10, or 10+. Next, I find the average county population within each bin, weighted by the number of pixels each county contributes to the bin.

Pre-trends plots for both samples and both aggregation methods are displayed in the Appendix. Figure A.13 and A.14 show the inland sample. For both aggregation methods - the average county population by community Emergency Program (A.13), and the average county population by average Emergency Program within a county (A.14), the trends are fairly parallel, with the exception of the 4-6 Emergency Program year bin.

Figure A.15 and Figure A.16 display pre-trends for the coastal sample. Figure A.15 reveals that on average, the counties of communities that experienced more Emergency Program years were growing slower compared to counties of communities that fewer Emergency Program years. Figure A.16 corroborates this and shows that on average counties that had communities with more Emergency Program years were growing slower than counties that had communities with less Emergency Program years. This is auxiliary evidence that the NFIP may have targeted high-growth coastal communities for more expedient map-completion. If county-level trends correspond to development growth on the community level, this introduces a downward bias on any estimate of Emergency Program year on development outcome.

Table 1 Inland First Stage Equation 4	(1) EmerYears @1980 $\times \gamma_{1980}$	(2) EmerYears @1986 $\times \gamma_{1986}$	(3) EmerYears @1992 $\times \gamma_{1992}$	(4) EmerYears @2000 $\times \gamma_{2000}$
Cohort X Time				
1 Month Cohort $\times \gamma_t$	-0.00101***	-0.00183***	-0.00214***	-0.00173***
	(0.000144)	(0.000333)	(0.000370)	(0.000345)
10 Day Cohort $\times \gamma_t$	0.00847***	0.0117***	0.0123***	0.0119***
	(0.00114)	(0.00199)	(0.00201)	(0.00187)
5 Day Cohort $\times \gamma_t$	-0.0108***	-0.0152***	-0.0147***	-0.0174***
	(0.00178)	(0.00315)	(0.00305)	(0.00305)
Cohort				
1 Month Cohort	0.000253***	-0.0000364	-0.0000727	-0.0000272
	(0.0000931)	(0.000112)	(0.000110)	(0.000105)
10 Day Cohort	0.00246***	0.00204***	0.00257***	0.00219***
	(0.000477)	(0.000776)	(0.000764)	(0.000725)
5 Day Cohort	-0.00451***	-0.00177	-0.00244**	-0.00211**
	(0.000681)	(0.00115)	(0.00109)	(0.00106)
N	273314	273314	273314	273314
R^2	0.8933	0.8692	0.8731	0.8761
F	4.75	5.33	6.31	7.37
Appendix Table	A.4	A.4	A.4	A.4

Standard Errors clustered at community level
Standard Errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

1.6 Results

Tables 1 and 2 display the first stage results for the inland and coastal samples, respectively. Standard errors are in parentheses and are clustered at the community level. For each sample only the results for Equation 4 are displayed in the main section - Equation 4 is the equation corresponding to the interaction between Emergency Program years and the time dummy, which is the variable of interest. Results for Equation 3, the equation corresponding to Emergency Program years (without the interaction) are found in the Appendix in Tables A.3 and A.5.

The dependent variable in each column is the Emergency Program duration multiplied by the relevant time dummy. Results are displayed for the relevant instruments - the cohort terms multiplied by the relevant time dummies. To find the net effect of an additional community put into a certain sized cohort, one would need to add down each column the relevant coefficients. In both samples first stage results reveal ambiguous effects of cohort size on Emergency Program duration. The coefficient on the 10-day cohort size is positive, but the estimated coefficients for the 1-month and 5-day cohorts

Table 2 **Coastal First Stage** **Equation 4**	(1) EmerYears @1980 $\times \gamma_{1980}$	(2) EmerYears @1986$\times \gamma_{1986}$	(3) EmerYears @1992 $\times \gamma_{1992}$	(4) EmerYears @2000$\times \gamma_{2000}$
Cohort X Time				
1 Month Cohort $\times \gamma_t$	-0.00101***	-0.00183***	-0.00214***	-0.00173***
	(0.000144)	(0.000333)	(0.000370)	(0.000345)
10 Day Cohort $\times \gamma_t$	0.00847***	0.0117***	0.0123***	0.0119***
	(0.00114)	(0.00199)	(0.00201)	(0.00187)
5 Day Cohort $\times \gamma_t$	-0.0108***	-0.0152***	-0.0147***	-0.0174***
	(0.00178)	(0.00315)	(0.00305)	(0.00305)
Cohort				
1 Month Cohort	0.000253***	-0.0000364	-0.0000727	-0.0000272
	(0.0000931)	(0.000112)	(0.000110)	(0.000105)
10 Day Cohort	0.00246***	0.00204***	0.00257***	0.00219***
	(0.000477)	(0.000776)	(0.000764)	(0.000725)
5 Day Cohort	0.000455*	-0.00180***	-0.00171***	-0.00158***
	(0.000681)	(0.00115)	(0.00109)	(0.00106)
N	174,750	174,750	174,750	174,750
R^2	0.8798	0.8849	0.8847	0.8847
F	1.05	0.89	0.91	1.02
Appendix Table	A.6	A.6	A.6	A.6

Standard Errors clustered at community level
Standard Errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

are negative. Results in the Appendix for the other first stage equation, Equation 3, reveal generally similar results.

The 1-month cohort estimate is an order of magnitude smaller than the 10-day and 5-day terms. Therefore, an additional community within the 10-day cohort but not the 5-day cohort would increase Emergency Program duration by about a week. However, an additional community within the 5-day community would have a smaller negative net effect. This suggests communities that entered the Emergency Program before or after a busy week experienced bureaucratic delays more so than communities that joined during the actual busy week, which fits the idea of certain community applications being "buried" in the stack of paperwork after rush periods for the agency. Across most specifications the instruments are statistically significant with p values less than .01, however the F values are quite small, especially for the coastal sample. Therefore weak instrument bias is a concern - further diagnostics on the validity of the instrumental variable estimator will be presented later in the results section.

Ordinary least squares and two stage least squares second stage results for the Inland

Table 3 **Inland Results**	Ave OLS Time Trend	(1) P(Dev)	(2) P(Dev) Pooled	(3) P(Dev)	(4) P(Dev) Pooled	(5) P(Dev)	(6) P(Dev) Pooled	2SLS Time Trend	(7) P(Dev) 2SLS
EmerYear × γ_{1980}	0.0008	0.00573	0.00616	0.0170*	0.00725*	0.00358	0.00725*	0.0383	-0.00343
		(0.00431)	(0.00480)	(0.00864)	(0.00430)	(0.00328)	(0.00409)	(0.0319)	(0.0103)
EmerYear × γ_{1986}	0.0013	0.00852**	0.00778*	0.00835**	0.00783*	0.00795**	0.00740*	0.0341	0.00721
		(0.00400)	(0.00418)	(0.00400)	(0.00409)	(0.00394)	(0.00409)	(0.0232)	(0.00620)
EmerYear × γ_{1992}	-0.0126	0.0184*	0.0165*	0.0187*	0.0166*	0.0181*	0.0160*	0.0496*	0.00738
		(0.00971)	(0.00924)	(0.00967)	(0.00915)	(0.00970)	(0.00926)	(0.0258)	(0.00635)
EmerYear × γ_{2000}	-0.0206	0.0249*	0.0241*	0.0235*	0.0243*	0.0245*	0.0236*	0.0259	0.0184***
		(0.0141)	(0.0133)	(0.0138)	(0.0133)	(0.0141)	(0.0134)	(0.0270)	(0.00679)
FE		Comm	Comm	EmerYear	EmerYear	Pixel	Pixel	EmerYear	EmerYear
N		273,314	683,285	273,314	683,285	273,314	683,285	273,314	273,314
Appendix Table	A.7-A.9	A.7	A.7	A.8	A.8	A.9	A.9	A.10	A.10

Standard Errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
Standard Errors clustered at Community Level

sample are displayed in Table 3. Standard errors are in parentheses and are clustered at the community level. The dependent variable is the probability of development for pixel i at time t. Specifications 2, 4, and 6 display the pooled, five-date OLS models, while specifications 1,3, and 5 display results for the separate two date OLS models. More specifically, columns 1, 3, and 5 each display results for four separate regressions, 1973-1980, 1973-1986, 1973-1992, and 1973-2000. Specifications 1 and 2 use community fixed effects, Specifications 3 and 4 use Emergency Program Years as the baseline variable, and Specifications 5 and 6 use pixel fixed effects. The average of the time trends across all OLS specifications listed is display for reference. See Tables A.7 (Community Fixed Effects), A.8 (Emergency Program Years), A.9 (Pixel Fixed Effects), and A.10 (2SLS Second Stage) in the Appendix for complete inland results. Tables A.7, A.8, and A.10 display results with the time-invariant community, county, and pixel level controls. Table A.9 does not include these controls as the pixel-fixed effects preclude the use of additional time-invariant variables. Column 7 displays second stage results, and the time trends for the second stage are given for reference.

With a time trend of essentially zero, OLS results suggest that inland floodplains did not grow much at all in development (perhaps even experienced some floodplain retreat), but the presence of subsidies increased development probability through the year 2000. The absolute effect measures between 0.3-1.7 percentage points in 1980, and 2.3-2.5 percentage points in 2000. Compared to the time trends, this also corresponds to an increase in the relative effect over time, suggesting the presence of agglomeration effects for inland floodplains. This would mean that even after the Emergency Program ended, and new development was no longer subsidized, development was still attracted to the relatively longer subsidized floodplains.

Second stage results show similar, although slightly decreased, estimates for the effect of an Emergency Program year for the inland sample. Table A.10 in the Appendix) show full results for the inland second stage regressions. The effect of an additional subsidy year starts at effectively 0 at 1980 and grows to 1.85% in the year 2000. Unfortunately, all but one of the estimates is not statistically significant. The time trends are also increased from the OLS results, with only one time dummy having statistical significance.

These results suggests that Emergency Program length for inland communities was less susceptible to unobserved agency decisions regarding the targeting of certain communities. It is plausible that the NFIP put more focus on high-risk coastal communities, and less effort in the targeting of riparian communities based on development potential or risk. If anything, developers in inland communities with access to more marginal plots could have been able to lobby for longer Emergency Programs, which might explain why the 2SLS effect is smaller than the OLS effect.

Results for the coastal sample are on the next page in Table 4. Standard errors are in parentheses and are clustered at the community level. The dependent variable is the probability of development for pixel i at time t. Like in Table 3, odd specifications display the pooled, five-date models, while even specifications display results for the separate

Table 4 **Coastal Results**	Ave OLS Time Trend	(1) P(Dev)	(2) P(Dev) Pooled	(3) P(Dev)	(4) P(Dev) Pooled	(5) P(Dev)	(6) P(Dev) Pooled	2SLS Time Trend	(7) P(Dev) 2SLS
EmerYear × γ_{1980}	0.0129	-0.000162	-0.00505*	-0.00430	-0.00531*	0.00033	-0.00474	0.0122	0.00624
		(0.00208)	(0.00299)	(0.00570)	(0.00315)	(0.00193)	(0.00300)	(0.0796)	(0.0284)
EmerYear × γ_{1986}	0.06389	-0.0118	-0.00958	-0.0116	-0.00921	-0.0118	-0.00955	0.0790	0.0231
		(0.0111)	(0.00932)	(0.0111)	(0.00916)	(0.0111)	(0.00931)	(0.0950)	(0.0195)
EmerYear × γ_{1992}	0.1158	-0.0208	-0.0179	-0.0197	-0.0173	-0.0208	-0.0179	0.166	0.0418
		(0.0197)	(0.0173)	(0.0195)	(0.0173)	(0.0197)	(0.0173)	(0.159)	(0.0342)
EmerYear × γ_{2000}	0.1313	-0.0249	-0.0236	-0.0241	-0.0226	-0.0249	-0.0236	0.223	0.0495
		(0.0223)	(0.0213)	(0.0222)	(0.0210)	(0.0223)	(0.0212)	(0.215)	(0.0452)
FE		Comm	Comm	EmerYear	EmerYear	Pixel	Pixel	EmerYear	EmerYear
N		174,750	436,875	174,750	436,875	174,750	436,875	174,750	174,750
Appendix Table	A.11-A.13	A.11	A.11	A.12	A.12	A.13	A.13	A.14	A.14

Standard Errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
Standard Errors clustered at Community Level

two date models. Specifications 1 and 2 use Emergency Program years as the baseline variable, Specifications 3 and 4 use community fixed effects, and Specifications 5 and 6 use pixel fixed effects. The average of the time trends across all OLS specifications listed is display for reference. See Tables A.11 (Community Fixed Effects), A.12 (Emergency Program Years), and A.13 (Pixel Fixed Effects) in the Appendix for complete OLS inland results.

Unlike the time trends in the inland sample, coastal regions are growing fast in the sample period. The base time trend is on average 0.5 percentage points measured at 1980 and rises to 13 percentage points in 2000. Ordinary least squares results for the effect of an Emergency Program year are negative and insignificant, suggesting that Emergency Program years had little influence in this. It could be the case that amenity values in these floodplains were sufficiently high and increasing over time, eliminating the presence of marginal parcels that wouldn't have been developed without subsidies.

The negative and insignificant results could however be due to the agency targeting coastal communities that had a large amount of marginal parcels or high development potential. If this was the case and the Flood Insurance Administration worked to complete their maps quicker, than OLS estimates would be bias downwards and IV would be required. The pre-trends analysis at the county level in the last section suggest that this may be the case, as counties that experienced more Emergency Program years on average display slower population growth than less-subsidized counties.

Second stage results reveal a positive effect for an additional subsidy year, going from 0.006 percentage points to 4.9 in 2000. Unfortunately, no estimates are statistically significant. The difference between the OLS and IV estimates suggests instrumenting for Emergency Program duration can a least partially overcome the downward bias produced if certain communities were targeted for quicker Emergency Programs. Compared to the time trends, the relative effect is shrinking, suggesting that any induced development that did occur was done so in a "rush to build," and temporally displaced from later years. In other words, less-subsidized communities partially "caught up" in the years following the subsidies. This does not preclude the existence of agglomeration effects in the longer-subsidized floodplains, however, much of the development that did occur in the subsidized periods would have occurred anyways in the absence of subsidies. Full second stage results can be found in Table A.14 in the Appendix.

Diagnostics

Tables A.10 and A.14 present the second stage results for the inland and coastal samples, respectively, as well as diagnostics of the performance of the instrumental variables estimator. First, given that for each two-date specification (1980, 1986, 1992, 2000) there are six instruments for two endogenous variables, I can perform an overidentification test. Tables A.10 and A.14 present the p-value for the Hansen's J statistic. For almost all years across both samples, I fail to reject the null hypothesis that the instruments are validly excluded from the structural equation at standard levels of significance. This is the case for all but one regression - the 1980 regression with added variables for the coastal sample.

As Tables 3 and 4 in the Results section display (as well as corresponding A.3-A.6 in the Appendix), the F statistics for the instruments in both samples are below 10. They are especially small in the coastal sample, which suggest that weak instruments are a potential concern. The p-values for the Anderson-Rubin Wald test and the Stock-Wright LM statistic are displayed in Tables A.10 and A.14. Both statistics test the joint significance of the endogenous regressors in the main equation. For the inland sample, the Stock-Wright LM test rejects the null that our six instruments are insignificant, but the Anderson-Rubin Wald test fails to reject the null. Despite the small F statistic, both tests reject the null for every regression in the coastal sample.

1.7 Extensions

Added Cost of Flooding

I propose the following methodology to calculate the expected added costs of flooding from a year of subsidy availability. This can be done for the Coastal and Inland samples separately. Although I focus on the year 2000, this methodology can be applied any one of the four dates after 1973 (1980, 1986, 1992, and 2000). The expected added flood damages at 2000 can be expressed as:

$$\mathbb{E}[\text{Added Flood Damages at 2000}] =$$
$$\mathbb{E}[\text{Yearly Flood Costs per pixel at 2000}] \times \text{Extent of Induced Development at 2000}$$

Calculating the first term on the right-hand side, the Yearly Flood Costs per pixel in the year 2000, involves assuming the contents of each developed pixel in the Land Cover

Trends Project. Recall for a pixel to be developed it need not be capturing residential use, but potentially commercial or industrial use. Despite this, the best resource available to infer the contents of each pixel is measures of housing density published through the U.S. Census. Each housing density measure is taken at the state, county, municipality ,or unincorporated county level and is in terms of units per square mile. This term can be easily adjusted to reflect the number of structures per $60m^2$ pixel. This can be multiplied with estimates of the average yearly cost of flood damages per structure inside floodplains.

$$\begin{aligned}&\mathbb{E}[\text{Yearly Flood Costs per pixel at 2000}]\\&\qquad = \text{Number of Stuctures per Pixel} \times \mathbb{E}[\text{Yearly Cost of Flood per Structure}]\end{aligned}$$

For the second term on the right hand side, the extent of induced development can be calculated by first estimating the amount of undeveloped land within floodplains within human settlements, measured at 1973. This can be done by finding the number of undeveloped pixels within floodplains in human settlements measured at 1973 for the land cover blocks available, and then scaling the area up to reflect the entire region. Finally, this estimate for convertible land can be multiplied by the coefficient on the variable of interest, $Emergency_{ct} * \gamma_t$, to estimate the number of developed pixels in the region induced by subsidies.

$$\begin{aligned}&\text{Extent of Induced Development at 2000}\\&= (\text{amount of convertible land at 1973}) \times \text{coefficient}\end{aligned}$$

The primary difficulty with interpreting any cost estimate is that buildings developed after the Emergency Program duration were done so under full risk-based rates. Therefore, any coefficient estimate for the effect of a subsidy year actually picks up a combination of two effects - the contemporaneous effect that measures the direct impact of the subsidized rates, as well as the cumulative effect that includes any agglomeration forces that occurred after the close of the subsidy duration. The coefficients across the four later dates are going to differ on how much each is comprised of the contemporaneous effect verses the cumulative effect. In other words, the estimate of a subsidy year measured at 1980 is going to be mostly the contemporaneous effect, while the estimate measured at 2000 is going to pick up many years of development that occurred under full-internalization of flood costs (i.e. full-risk rates). Therefore it is difficult to make any statements regarding welfare. Using the effect of a subsidy year measured at the year 1980 instead better estimates the proportion of costs that represent a net welfare loss, as these coefficients better pick up solely the "contemporaneous" effect under subsidized rates.

Focusing on the Mississippi River Basin, 2.7% of the pixels of the land cover provided by the Land Cover Trends Project in 1973 were undeveloped, within floodplain, and located in human settlements. The Mississippi River Basin is roughly 1.15 million square miles. Scaling the 2.7% proportion to the entire Basin, converting that number to pixel units, and using the 1980 subsidy year coefficient gives us the amount of pixels "induced" to the developed state by subsidies within the region. Next, housing density measured at the year 2000 is averaged across the states in the Basin. Lastly, the average yearly cost of flooding per household in the 100-year floodplain is set at $5,000. These two numbers give us the average flood cost per pixel measured in the year 2000. With these estimated values, total added flood costs to come out to approximately $250 million per year.

1.8 Conclusion

Although the NFIP no longer subsidizes new housing starts, over 3.5 million existing structures located in floodplains are eligible for them today. These 3.5 billion were either built prior to the Emergency Programs analyzed in this paper, or in fact be the induced development identified in the empirical section. Recent pieces of passed legislation have demonstrated the difficulty in removing subsidies. The Biggert-Waters Flood Insurance Reform Act of 2012 dictated the phasing-out or immediate removal of most rate subsidies. After much contention, the 2012 Act was followed up the Homeowner Flood Insurance Affordability Act of 2014, which slowed down or nullified most of the subsidy phase-outs from the 2012 Act.

Climate change is predicted to increase the severity and/or intensity of natural disasters. Changing disaster patterns will most likely spur calls from the affected constituencies for additional homeowner assistance from the federal government, including post-disaster relief, firefighting expenditures, and affordable insurance. Although such policies are intended to minimize the long-term local economic costs of disasters, depending on how they are structured they can encourage opportunistic behavior on the part of landowners.

Grandfathering and subsidies are both often used as tools address goals of affordability or perceived "fairness." They are also used to gain political traction for a program. My paper shows how grandfathering and subsidies, together with limited bureaucratic resources that created generous deadlines, induced capital decisions by individual landowners that became costly in the aggregate and worked against the stated purpose of the policy (that is, to effectively manage flood risk). Utilizing land cover and historical floodmaps, I build a dataset that tracks land-use inside floodplains in coastal and inland regions over time. I develop a queue-based instrument to capture bureaucratic congestion and overcome any endogeneity concerns stemming from potential correlations between unobserved economic indicators and Emergency Program durations, as well as concerns from unobserved systematic procedures in the NFIP's map-making process.

I show how the availability of subsidies for new development increased the probability of development inside floodplains, effectively adding to the cost of flooding events in the United States. The evolution of this development was different for inland and coastal regions. Subsidy availability caused agglomeration effects in slower-growing inland regions, in which floodplains would not have "taken off" without the help of subsidies. I conjecture that the settlements and subdivisions induced by subsidies acted as coordination devices for new development funds in later years. For the coastal sample, the relative effect of a subsidy year decreases over time, suggesting that a "rush to build", resulting in temporally displaced development, was a major component of the induced stock. Finally, I estimate the total added flood costs from the pre-FIRM subsidies. Focusing on the Mississippi River Basin, I estimate the an additional year of subsidies added about a quarter billion of annual flood costs, measured at the year 2000.

Chapter 2

Disaster-Financing as Vote Buying

with Sahaab B. Sheikh

2.1 Introduction

Do House of Representative elections influence the allocation of post disaster relief? A central idea in the public choice literature is that politicians are inclined to use government expenditures as strategic transfers to constituencies to maximize votes [68, 69]. In recent years, victims of floods and other disasters have received substantial transfers from the federal government through relief programs. Since 2004, $22 billion has been paid out to individuals through the Federal Emergency Management Agency's (**FEMA**) Individuals and Households Program (**IHP**) for floods, hurricanes, levee breaks, tsunamis, typhoons, coastal storms, and severe storms.[1] This paper explores the electoral and political determinants of FEMA IHP grants. Focusing on Congressional House district elections, we test for tactical redistribution to highlight the role of electoral realities in the allocation of post-disaster relief.

[1] As of 02/05/2020.

Tactical redistribution refers to the use of general funds to court voters via economic favors and can take several forms ([70]). Politicians can use funds to alter electoral outcomes in competitive jurisdictions. Alternatively, politicians can reward jurisdictions politically aligned with their party. In this paper, we test for the presence of tactical redistribution with three electoral variables measured at the House district level - a variable for electoral competitiveness, a variable for House incumbent's alignment with the executive administration, and an interaction term between the two. Together, these three variables allow us to separately identify expenditures motivated by vote-buying in competitive districts verses expenditures that are motivated by political patronage, such as the rewarding of specific constituencies loyal to the President. They also enable to measure differential effects of House electoral competition conditional on party alignment.

As outlined by the Stafford Act of 1988, the allocation of post-disaster relief falls under the direct discretion of the President. Previous literature, including [71], [72], and [73], has emphasized the objectives of the President and his re-election hopes to explain the rate at which disasters are declared and federal funding furnished. However, elected officials other than the President can also benefit from vote-buying through disaster relief. House Representatives are often the federal policymaker most local to a disaster. With elections occurring every two years, House Representatives and their parties are constantly trying to court voters in their district. Needing to secure chamber majorities in order to advance their legislative agenda, Presidential administrations can use disaster funds to generate goodwill for the party in power and support the party's candidate in the upcoming House election.

We examine the universe of FEMA IHP grants aggregated to the zip code level from 2004-2020. Controlling for explicit measures of disaster severity as well as zip code and county level characteristics, we utilize a stacked-cross-section approach to identify the effect of electoral variables at the House level on the total aid package. We perform this analysis for multiple forms of hydrological disasters, including floods, hurricanes, mudslides, and severe storms.

Using both ordinary least squares and Tobit models with fixed effects, we find that electoral competition at the House level increases the amount of aid received by a House district if the incumbent is not aligned with the Presidential administration. Every percentage point increase towards 50% Democratic vote share in the previous election increases relief packages by up to $20,000 per zip code for each disaster. This is an economically significant effect, as the mean zip code grant total in our data is $440,000. This result shows that aid is used as a vote-buying resource by the political party in executive power, and administrations are confident that the goodwill from increased relief is attributed to the party in power and not the unaligned incumbent Representative.

We also find a strong party loyalty effect, indicating that the districts of House incumbents belonging to the party of the President benefit from additional aid by the order of $450,000-$900,000 per zip code for each disaster. We find that party loyalty funds crowd out competition funds in House districts aligned with the President, suggesting that parties prefer to blanket aligned districts with funds, regardless of competition.

We further the analysis by limiting the regression sample to particularly competitive districts, defined as those with Democratic vote shares between 40% and 60% in the previous election. We do this to examine the competition effect in highly contested districts. We find that for unaligned districts, a percentage point increase in Democratic vote share towards 50% now increases relief packages by up to $115,000 per zip code for each disaster. Compared to our full sample results, this suggests that the closer the district's previous election winner was to the 50% mark, the more the administration awards competition in an attempt to flip the district. Similar to the full sample results, aligned districts still benefit mainly through party loyalty and not through competition. As a robustness check, we further limit the sample to districts with Democratic vote shares between 47% and 53% in the previous election. We find even stronger competition effects for unaligned districts, although its harder to identify competition separately from party loyalty for aligned districts given the small sample. Focusing on limited samples also helps to address the potential for any unobserved economic or political indicators that are correlated with both district competitiveness and grant totals, as well as to examine the effect of competition exclusively in the most crucial districts.

We then extend this analysis to examine the overall distortions created by electoral and political factors, as well the constituents that benefit from them. We create hypothetical baseline amounts of relief for each zip code that would be allocated under a politically-neutral agency and compare them to the predicted values from the main regressions. We find that the differences between the predicted allocations and the political neutral allocations are in the hundreds of thousands for many zip codes. We find that white zip codes and zip codes with older homeowners benefit from these distortions, while non-white zip codes do not. In addition, zip codes that are benefactors of electoral influences don't tend to be hit harder by disasters, differ by flood zone, or be more prone to disasters.

This is among the first papers to examine how electoral realities at the House district level impact post-disaster relief packages.[2] While previous papers have examined how the President's election hopes impact relief amounts ([71, 72, 73]), this is the first paper to examine vote-buying expenditures purposed by the administration for lower level elections. More importantly, this is the first paper to differentiate how competition affects relief packages conditional on House incumbent and executive administration alignment. Finally, this is also first paper to estimate the overall distortions in relief allocation resulting from tactical redistribution.

[2]Only two previous papers have used electoral variation at the House level to explain aid and have done so only as a first stage in a two stage least squares strategy aiming to explain insurance purchase decisions ([51, 74]). To the authors' best knowledge, as of 6/24/2020.

The extent to which political objectives influence the operations of bureaucracies that administer disaster financing is important to test, as transfers can have real resource costs. The availability of post-disaster relief exports the cost of recovery to general taxpayers. This creates moral hazard by disincentivizing insurance purchase and incentivizing inefficient land-use and building decisions. Focusing on the impact of relief grants on future insurance purchases, we use estimates from [74] to quantify how electoral influences perpetuate a cycle in which insurance purchases decline, demand for post-disaster relief increases, and vote-buying is amplified. We also discuss the impact of relief on building and land-use decisions.

Section 2 reviews the relevant literature on disaster programs and public choice. Section 3 describes the data. Section 4 details the empirical strategy. Section 5 discusses main empirical results. Section 6 discusses the extensions, and Section 7 concludes.

2.2 Background & Literature Review

The role of the federal government in disaster relief and management has grown significantly in the past half century. The creation of the Federal Emergency Management Agency (FEMA) in 1979 and the passing of the Stafford Act in 1988 solidified the processes for the allocation of post-disaster relief by the federal government, a financial burden that was previously carried by the American Red Cross as well as local and state governments. Furthermore, since the creation of the National Flood Insurance Program (NFIP) in 1968, the federal government has effectively been the sole provider of flood insurance in the U.S., supplanting any private underwriting of flood risk.

In recent years, victims of floods and other disasters have received substantial transfers from the federal government through these outlets. Since 2004, the federal government has paid out nearly $22 billion in Individual and Households Program (IHP) grants as post-disaster relief for homeowners and renters. This is in addition to $56 billion in indemnity payments through the National Flood Insurance Program. IHP grants are available for both NFIP and non-NFIP policyholders, which NFIP policyholders can use to supplement any received indemnity payments. An additional $76 billion in post-disaster relief grants has been paid out to public entities during this time period for these disaster types.[3]

FEMA is a federal agency, created via executive order in 1979 by President Carter. FEMA began as an independent agency, but following a federal government reorganization plan in 2003 it was placed under the authority of the Department of Homeland Security. The majority of FEMA's operations are performed according to the guidelines established by the Robert T. Stafford Disaster Relief and Emergency Assistance Act passed in 1988. The Stafford Act outlines the procedures for presidential disaster declarations and the distribution of various types of aid.

The allocation of post-disaster relief funds follows a multi-step process sensitive to political influence. When a disaster strikes, the governor of the affected state will order an abbreviated Preliminary Damage Assessment conducted by state officials to gauge damages. If the governor considers the damage severe enough to warrant federal assistance, they will then order a full Preliminary Damage Assessment and request a disaster declaration from the President. The full Assessment is usually a cooperative effort by FEMA representatives, state officials, and local bureaucrats familiar with the area. The governor's request and the Preliminary Damage Assessments then go to the President, who then decides whether or not to declare a disaster. Upon declaring a disaster, the President consults the Preliminary Damage Assessments and determines the level of federal relief to be allocated to the state ([75]). The President has explicit authority over the declaration of a disaster and allocation of federal funds as further outlined by the Disas-

[3]Public entities being municipalities, counties, fire districts, school districts, etc.

ter Mitigation Act of 2007 ([73]). Given this authority, the President may deviate from the amount of relief suggested by FEMA through the Preliminary Damage Assessments, allowing for political and electoral factors to shape allocation.

Despite the potential for vote-buying and political favoritism, the political and electoral determinants of federal disaster relief in the United States are understudied in economics beyond examinations of state-level aggregates. Disaggregated data on FEMA grant allocation and NFIP policies only recently became public. In addition, FEMA provides no direct method to associate grant applications and amounts with NFIP policy coverage in a particular region.

On the other hand, agricultural insurance and relief, a public transfer program of similar magnitude and structure, has been heavily scrutinized for its role in redistributing income from taxpayers to agricultural producers ([76, 3, 77, 78, 79, 80]). Given the President's discretion in declaring disasters and allocating aid, existing literature on disaster financing has focused on the potential for vote-buying in Presidential elections. Specifically, previous papers have examined how state-level factors influence the the rate of Presidential Disaster Declarations in a state ([81, 82]), as well as the amount of post-disaster aid allocated once a declaration is made ([71, 72, 73]). These factors include gubernatorial and presidential party alignments, the competitiveness of recent presidential elections within a state, and the number of legislators within the state serving on Congressional oversight committees. They detect that the number of Presidential Disaster Declarations increases in election years, as do FEMA expenditures. In addition, states with more electoral votes and/or states that are historically competitive receive more Declarations. [83] conduct a similar analysis for Small Business Administration declarations, which operate separately from presidential declarations, and find similar results.

These previous studies are conducted at the state level and all make implicit assumptions about the politics of natural disasters. Natural disasters are often localized to a specific region within a state. Therefore, a constituency that might be politically important to the administration in the next Presidential or House election may not be located in the part of the state affected by the disaster.

This paper contributes to the literature by examining FEMA expenditures at the zip code level, a finer scale than the state level. The finer scale of analysis allows us to examine how electoral competition at the House Representative level influences the allocation of disaster financing. Besides the President, the House Representative is an office that may have much to gain electorally from the successful delivery of FEMA grants, as it is the federal policymaker most local to a disaster with the narrowest constituency. House elections occur every two years, so incumbents and challengers are constantly in "campaign mode." The Presidential administration also has reasons to focus on House elections when allocating relief packages. Securing a chamber majority is important for the President's policy agenda. The President must trade off between securing their re-election through vote-buying and trying to build a coalition to pass legislation. Given Presidential discretion, the President can reward incumbents or challengers from his own party over the opposition by allocating vote-buying resources on behalf of the party in power (Tactical Redistribution). Finally, the large number of House districts and frequency of elections provides sufficient variation to identify electoral and political factors independent of disaster severity.

Tactical Redistribution

Disaster relief is distributed by bureaucracies operating under elected officials. As a consequence, its allocation is subject to the political objectives of the agents involved. A central idea in the public choice literature is that politicians are inclined to use government expenditures as strategic transfers to constituencies to maximize votes ([68, 69]). The costs of such transfers are borne by general taxpayers. According to this model, relief grants are as much as particularistic goods to specific constituencies, or goods used to increase re-election odds, as they are tools of disaster recovery.

Allocation of disaster financing can be impacted by upcoming electoral challenges for elected officials representing disaster-affected jurisdictions. According to models of tactical redistribution, a governing political party will allocate scarce vote-buying resources to where the marginal dollar maximizes the number of jurisdictions it wins in the upcoming election ([70]). Jurisdictions that offer the most "bang for your buck" can be measured several ways. For example, the number of "swing voters" in a district can capture the amount of votes persuadable through political funds. The number of swing voters can be measured as the number of self-identified moderates, independents, or recent third-party voters ([51]). Alternatively, jurisdictions that offer the highest marginal return can be the ones that are most competitive. For a governing political party, a House district with a competitive upcoming election offers the chance to "flip" a district (or keep the district under party control) with relatively less funds than a more secure district for either party.

Our paper uses the results of the most recent election as an indicator of how competitive the upcoming election will be. Figure B.1 in the Appendix displays histograms of Democratic and Republican vote shares for all House elections from 2002 onwards. The histograms show the distributions of total vote share for each party in each House election for all elections between 2002 and 2020. The densities at 0 and 1 reveal elections in which one of the parties did not field a candidate. As Figure B.1 shows, the majority of elections garner a vote share between .3 and .7 for either party.

If the competitiveness of a district influences the allocation of FEMA grants, the interaction between competitiveness and party alignment of the incumbent Representative vis-a-vis the administration may also influence the size of the relief package. When allocating vote-buying resources to influence House elections, governing political parties may take into account the party of the incumbent Representative, as well as how the delivery of extra resources will be perceived by the voting public. The delivery of extra resources may be attributed to the political party in power, or, it may be attributed to the incumbent Representative (or both). If an upcoming House election is competitive and the incumbent is of the President's party, the party would devote extra resources to ensure that the incumbent Representative, the President, and the party all come off looking favorably. If an upcoming House election is competitive and the incumbent is not of the President's party, the party may hold off resources to make the incumbent look ineffective if the incumbent would receive the goodwill from any extra resources. Alternatively, the administration may be confident that the resources would be attributed to the party in power and not the incumbent Representative and send more relief to benefit the challenger to the incumbent.

There could also be a party loyalty channel, in which the governing party rewards constituents in districts represented by aligned incumbents, regardless of the competitiveness of the district's upcoming election. For example, the governing party could believe in "taking care of their base." Alternatively, the governing party could have a comfortable chamber majority and not have to worry about the potential for upheaval. This would allow them to focus on rewarding loyalty. Party loyalty spending could be a complement to or substitute of any competition-motivated expenditures within an aligned district. For example, if party-loyalty funds are sufficiently large, they may render any additional competition-motivated expenditures unneeded.

Testable Hypotheses

In Table 1 we present four hypothesis that represent the potential forms in which tactical redistribution can manifest. We measure tactical redistribution in this paper using three main variables. These terms will be the variables of interest in our regressions. First, *Comp* represents how close the Democratic share in the most recent House district election was to 50% (simply majority). Second, *Rep & Pres* is an indicator of the incumbent House Representative being of the same party as the President. Lastly, *Rep & Pres* × *Comp* or interaction for short, represents the closeness of the most recent House district election when the incumbent belongs to the President's party. With the presence of the interaction term, the *Comp* represents the level of competition when the incumbent is not of the same party as the President, while the sum of this term and the interaction represents the net effect of competition for an aligned incumbent.

Table 1

Hypothesis	*Comp*	*Rep & Pres*	*Rep & Pres* × *Comp*	What Matters
1	Null	Null	Null	No Electoral Influences
2	Positive	Null	Null	Competition
3	Null	Positive	Null	Party Loyalty
4	Positive/Negative	Positive	Positive/Negative	Comp & Loyalty

Politically-Neutral Allocation

Hypothesis 1 represents the null hypothesis, in which there is no effect of House-level electoral factors on relief packages. Hypothesis 1 states that competition does not influence the allocation of relief by the administration. In addition, the party alignment of the incumbent in relation to the President plays no part in relief allocation as well. This hypothesis represents a political system in which relief is allocated solely in a technocratic and politically neutral manner, or, the President solely uses relief for vote-buying for his or her own re-election but not for any downstream candidates.

Competition Matters

Hypothesis 2 represents a scenario in which the administration allocates vote-buying measures in competitive House districts, regardless of the party of the House incumbent. The administration is confident that in nonaligned districts, additional relief will be attributed to the party in power and not the incumbent Representative.

Party Loyalty Matters

Hypothesis 3 represents a scenario in which the administration rewards solely through a political patronage channel. Districts represented by House incumbents aligned with the party in power receive extra relief from the administration, regardless of the electoral competitiveness of the district.

Competition and Party Loyalty Matters

Hypothesis 4 represents a scenario in which competition matters in the allocation of relief, but differentially based on the alignment of the House incumbent. This hypothesis supports several different allocation strategies. For example, the administration could allocate vote-buying measures in competitive districts aligned with the President, but not in competitive districts held by incumbents of the other party. This could be the case if the administration is worried about relief funds (or lack thereof) being attributed to the incumbent instead of the administration. Alternatively, the administration could allocate vote-buying measures in competitive districts regardless of party alignment, and then reward aligned districts further through patronage.

2.3 Data

FEMA offers data on the universe of IHP post-disaster grants since 2004 aggregated at the zip code level. Variables include total grant amounts, the number of households that applied for aid, and the number of households that actually received aid. Rejected grant applications have a value of zero. Grant information is separately available for owners and renters. For our analysis, the outcome variable of interest is the sum of grant totals across owners and renters per zip code. IHP grants are only given out when a President declares a disaster and approves the allocation of IHP aid. In our analysis we consider FEMA disasters that are classified as floods, hurricanes, tsunamis, mudslides, typhoons, coastal storms, and severe storms.

Isolating the portion of a relief package that is driven by actual damages (and not political factors) is difficult, as any monetary-based measure of disaster severity that exists could consist of either type of disaster financing. As a result, commonly-used monetary measures such as those from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) or from the National Oceanic and Atmospheric Administration (NOAA) Storm Events Database would act as bad controls.

To control for disaster severity, we first use measures of daily rainfall. We use data from the PRISM (Paramater-elevation Relationships on Independent Slopes Model) Climate Group out of Oregon State University. This dataset uses 13,000 surface stations to generate 30 arc-sec grid cells for the lower 48 states. To assign a daily rainfall value to a zip code tabulation area, we match the centroid of the zip code tabulation area to the PRISM cell that it falls in. See Figure B.2 in the Appendix for an example daily map of the PRISM data. In our regressions, we use cumulative rainfall between the start and end dates of the declared disaster.

To further control for disaster severity, we include measures of vulnerability to storm-surges in hurricane-prone regions. The National Hurricane Center and Central Pacific Hurricane Center within NOAA provides Storm Surge Hazard Maps for the continental United States. We take the weighted average of wave height for a category 3 hurricane within the boundaries of each zip code tabulation area. For the regions of the United States not vulnerable to hurricanes, we assign a storm surge value of 0 for their zip code tabulation areas. Figure B.3 shows the map of storm surge heights corresponding to a category 3 hurricane.

Voting records for House elections are taken from the MIT Election Data and Science Lab, which list the total votes per candidate in every general and special House election since 1976. Demographic data at the zip code tabulation area level is taken from the 2000 and 2010 Censuses, and include white and nonwhite populations, age of household owner, number of housing units both rural and urban, number of renters and owners, housing units occupied by owners and units vacant. This data were extracted from the NHGIS dashboard on the IPUMS website. Monthly housing values at the zip code level are taken from the Zillow Housing Value Index and are averaged to the annual level. Yearly unemployment rate at the county level is taken from the Bureau of Labor Statistics. For both housing values and unemployment, we use the value of the year previous to the disaster date, as the contemporaneous year's measure could be influenced by the realization of the disaster itself.

In addition to the variables mentioned above, we also include purchased NFIP coverage to control for total amount of insurance financing. IHP grants are a secondary source of funding for structures that are covered by NFIP policies, so controlling for coverage allows us to measure both disaster severity faced by a zip code and its financial vulnerability to a given disaster.[4] FEMA offers a subset of the universe of policies that the NFIP has underwritten. This subset contains all policies that were active in 2009 onwards. More

[4]IHP grants are available for both NFIP insured and uninsured households.

specifically, we observe the universe of policies since 2009, but only observe the pre-2009 policies that were renewed through that year.[5] The dataset contains numerous household and policy-level covariates for 50 million policies dating back to 1984. Covariates include effective policy start date, end date, flood zone type, deductible, NFIP program type, policy term, and total building and contents coverage. We aggregate these variables to the zip code level. This includes summing the total coverage and finding the mode of the categorical variables. We do this to control for the amount of damages in a zip code that would be covered by insurance. For policyholders, IHP grants would be a secondary source of relief over and above any indemnity payments from the NFIP.

We also compile data on Congressional subcommittee representation. We do this to control for any Congressional influences that may impact FEMA's operations and the final grant allocation. We include indicators for both House and Senatorial subcommittees. These indicators refer to membership on the Homeland Security Subcommittee of the House Appropriations committee, the Emergency Preparation, Response, and Recovery Subcommittee of the House Homeland Security Committee, the Economic Development, Public Buildings, and Emergency Management Subcommittee of the House Transportation and Infrastructure Committee, the Homeland Security Subcommittee of the Senate Appropriations committee, and the Federal Spending Oversight and Emergency Management Subcommittee of the Senate Homeland Security Committee. Table 2 in the Appendix displays the Congressional subcommittees that oversee FEMA and the average number of members in each subcommittee from 2003-2020. There are 435 voting members in the House, and 100 voting members in the Senate. These rosters are taken from the Office of the Clerk of the U.S. House of Representatives and the Secretary of the Senate, respectively.

[5]For example, a policy that started in 2000 and was terminated in 2008 would not be included in the data but a similar policy that was terminated in 2012 would be included.

The data also include political variables to control for other political channels that may impact relief allocation. These include the party of the President, the party alignment of the governor vis-a-vis the Presidential administration, and the interaction of governor-President alignment with House electoral competition.

Table 3 in the Appendix displays summary statistics for the variables used in the grant regressions. Focusing on Total Grants, the average grant total at the zip code level is about $440,000 (the median is reported much lower at $11,200 but not shown in the table). This grant total is allocated to an average of 123 approved applicants (Approved Applicants Count) per zip code out of an average 288 valid registrations (Valid Registrations Count). A valid registration is an application for assistance filed during FEMA's designated registration period (60 days from the Disaster Declaration Date). The Total Damage variable shows that FEMA's initial assessment reports an average of $493,000 of damage in each affected zip code (the median is reported much lower at $11,900 but not shown in the table). This damage total corresponds to an average of 185 inspected structures (Inspected Structures Count) within a zip code.

The Democratic President dummy indicates that 27% of the zip code disaster pairs that occur during this 17 year period are filed under a Democratic administration.[6] The rainfall variable shows that the average cumulative rainfall between the start and end dates of a declared disaster, measured at its zip code centroid, is just over 268 millimeters (10.5 inches). The surge variable shows that on average, disaster-affected zip codes would experience 0.5 foot high storm surge waves during a Category 3 hurricane. The aggregate policy count and policy coverage variables show that on average, there is a total of 1,762 active policies in a zip code during a disaster totaling $ 428 million of coverage. The aggregate deductible variable shows that there is an average of $3.9 million in deductibles per zip code required before the NFIP makes any indemnity payments.

[6]We are excluding 2011 and 2012 from our analysis due to redistricting concerns.

2.4 Empirical Model

To capture the electoral competitiveness of a House district, we use the results from the most recent election in relation to the date of disaster. The measure of House electoral competition, called *Comp* in our regressions, is $Y = 2 * (.5 - |X - .5|)$, where X is the Democratic vote share in the most recent House election. A vote share of 50%, or $X = .5$, signifies high competition, and results in $Y = 1$. Values of $X = 1$ or $X = 0$ signify an uncontested race for one of the parties, and results in $Y = 0$.[7] This measure is symmetrical around .5 in the sense that it produces an identical value for Democratic vote shares of 0.4 and 0.6. It can also be used with Republican vote share with similar results.[8] To interpret the coefficient on the *Comp*, a one percentage point change in vote share corresponds to a two percentage point change in electoral competitiveness.

We use all House elections from 2002 onwards, excluding observations that occur in 2011 and 2012. The 2012 House elections were the first elections with newly drawn House districts after the 2010 Census and redistricting. Because district boundaries may have changed, the competitiveness of the district's last election in 2010 could be an inaccurate indicator of the 2012 election's competitiveness. The pre-redrawing district and the post-redrawing district, while sharing the same name, could be vastly different in shape, area, demographics, and political composition.

As mentioned in the Testable Hypotheses section, electoral competition may influence the allocation of disaster financing differently depending on whether the incumbent Representative is of the same party as the President. To capture these effects, we construct a "same-party" indicator for every district-year combination labeled as *Rep & Pres.* We then interact this indicator with our measure of electoral competitiveness to highlight any

[7] Prior literature uses a quadratic measure, to which our results are robust ([78, 72]).

[8] The presence of third parties does add noise to the measure as it designed for a two-party system, however, third-party vote shares tend to be small in most US House elections.

additional resources funneled to districts that are not only more competitive, but also have an incumbent from the President's party. This interaction term in our regression is called *Rep & Pres* × *Comp*.

FEMA Grants

FEMA grants for zip code z in Congressional district d at time t are specified in Equation 1.

$$\textbf{(1)}\ Grant_{zdt} = \alpha_0 + \sum_{1}^{N} \alpha_{1n} TactRedist_{ndt} + X\beta + \gamma_s + \gamma_c + \gamma_d + \gamma_y + \epsilon_{zdt}$$

$$TactRedist = \{Comp, Rep\ \&\ Pres, Comp \times Rep\ \&\ Pres\}$$

$TactRedist$ is a composite term which includes the measures from the Tactical Redistribution Model. It includes *Comp*, *Rep & Pres*, and the interaction between the two (*Rep & Pres* × *Comp*). The sign and significance of the coefficient estimates on these three terms will verify the validity of the hypotheses listed in Table 1.

γ_s and γ_d are state fixed effects and district fixed effects, respectively. γ_c and γ_y represent Congress and year fixed effects, respectively. Included in X are all the grant-level, political, disaster severity, demographic, and aggregate policy controls mentioned in summary Table 3.

NFIP insurance coverage in force at the time of the disaster (*Aggregate NFIP Policy Coverage (Zip)*) as well as NFIP policy count (*Aggregate NFIP Policy Count (Zip)*) are included in X because insurance would be the primary source of financing for an affected household, followed by grants. Therefore higher coverage would imply less need for grants. Aggregate NFIP Deductibles for the zip code is also included to control for damages not covered by NFIP.

Identification

Our identification comes from the variation in Democratic vote shares from 435 House district elections that occur every two years between 2002 and 2020. This corresponds to nine election cycles. Two channels of potential endogeneity must be addressed. First, we are trying to capture how the effect of electoral competition at the House district level influences the allocation of FEMA relief, a decision heavily influenced by the President. This must be separated from any incentives the President has to directly influence their own re-election. We are trying to identify aid channeled for the purpose of influencing House elections, not for the purpose of influencing the Presidential election. Every other House election cycle is concurrent to a Presidential election. In addition, House districts that are competitive may also be the regions which the President would like to target to increase their prospects of winning the state in the next Presidential election.

We first include district-fixed effects to capture any time-invariant district characteristics that would influence its desirability for the President. This allows us to compare zip codes within House districts, as opposed to comparing zip codes across House districts that may correspond to regions of different electoral importance for the President. Even so, we may be neglecting time-varying demographic trends that could change the electoral outlook for the President in a particular region and be correlated with House election returns. We use the Census to control for time-varying demographics such as population, racial make-up, and house-ownership at the zip code level. We also include yearly values of unemployment at the county level and housing values at the zip code level to further control for any local time-varying factors. In addition, we control for the ramped-up incentives brought forth by a Presidential election year by including year fixed effects. Finally, we control for whether the incumbent President is Democrat or Republican, as differences in ideological beliefs between parties may alter how opportunistically they allocate funding.

Second, despite all of the controls listed, the individual actions of a member of Congress can be correlated with the competitiveness of House elections and also the amount of relief going to a particular region. To help address this potential endogeneity, we use two limited samples that will restrict attention to different intervals of "closeness" around the 50% victory threshold. The X variable, i.e. share of Democratic votes in the most recent election, is limited to a range of $(0.4, 0.6)$ and then further to $(0.47, 0.53)$. By focusing on a narrower range of vote-shares, this allows us to eliminate any potential confounding factors associated with wide differences in electoral competitiveness. This also allows us to see if the relationship between our public choice variables and relief changes closer to the 50% threshold. While this approach limits our ability to measure a competition effect, one advantage of focusing on a narrow range is that we are able to better identify a pure patronage effect.[9] For these two limited vote share samples, the *Comp* variable is recalibrated to keep a range of Y between 0 to 1. For example, for the specification with votes shares in between (.4, .6), the variable becomes $Y = 2 \times (.5 - 5 * |X - .5|)$, and the interpretation becomes a one percentage point change in vote share results in a ten percentage point change in electoral competition. For the specification containing the (.47, .53) range, the variable becomes $Y = 2 \times (.5 - (\frac{100}{6} * |X - .5|)$, and the interpretation becomes a one percentage point change in vote share results in a 33.34 percentage point change in electoral competition.

Finally, a minor concern is the presence of unobserved agency preferences. Bureaucrats may have preferences on relief distribution that vary across regions and time. The

[9]Consider a vote-share range of .499 to .501. Within this range we are unable to discern any meaningful variation in competitiveness, however, the discontinuity that comes with victory at $.5 + \epsilon$ allows us to clearly identify a political patronage effect.

inclusion of Congress fixed effects captures any bureacratic changes that occur due to changing chamber composition and/or majorities. Furthermore, district and year fixed effects can also help address any unobserved bureaucratic influences. Ultimately, agency preferences are secondary to that of the President and can be trumped by his discretion.

2.5 Results and Discussion

Main Results

Table 4.1 in the Appendix shows full results for the grant analysis. Table 4.1 (Abbreviated) shows the main results focusing on the electoral variables. Six specifications are presented: Column 1 is the simplest specification and includes the tactical redistribution variables as well as controls for disaster severity, census demographics, political and electoral characteristics, NFIP policies aggregated at the zip code level, and district fixed effects. Column 2 adds in year and Congress fixed effects. Column 3 includes a control from the grant data; number of valid registrations per zip code. Columns 4-6 run the same specifications but under a Tobit model. We do this because of the large number of zeros (19% of observations) in the dataset. The coefficient on *Comp* variable ranges between 192,000 and 1,000,000 across all specifications and is significant in four of six specifications. The coefficient indicates that more competitive districts receive a larger amount of funding on average. A one percentage point change in a party's vote share towards 50% results in a \$4,000-\$20,000 increase in grant expenditures in the zip code (these numbers are found by multiplying the coefficient by .02). This is evidence that the administration allocates significant vote-buying measures for the purpose of influencing House elections, even if the incumbent of not from the party in power. This result is line with the prediction of Hypothesis 2: competition matters.

Table 4.1 (Abbreviated)	OLS			Tobit		
Full Vote Share Range	(1) Grant Total	(2) Grant Total	(3) Grant Total	(4) Grant Total	(5) Grant Total	(6) Grant Total
Tact. Redist. Variables						
Comp	653.0^{*}	657.8^{*}	192.5	1015.1^{**}	995.2^{**}	458.8
	(341.0)	(347.9)	(338.8)	(421.2)	(437.0)	(371.9)
Rep & Pres	496.5^{*}	480.0^{*}	869.6^{**}	$494.5^{\dagger}$	$468.1^{\dagger}$	893.2^{**}
	(282.6)	(275.1)	(386.8)	(309.6)	(296.6)	(393.5)
Rep & Pres × Comp	-783.3^{*}	-771.6^{*}	-893.7^{*}	-863.7^{*}	-820.7^{*}	-950.3^{*}
	(422.0)	(414.4)	(530.2)	(467.3)	(454.2)	(544.0)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
F Statistics						
Tact. Redist. Variables	2.27^{*}	1.60	3.29^{**}	2.58^{*}	$3.2e08^{***}$	3.21^{**}
Comp + Rep & Pres × Comp=0	0.10	0.08	3.35^{*}	0.10	0.14	1.51
N	15103	15103	15103	15103	15103	15103

Controls: grant information (Spec. 3 & 6 only), disaster severity, census demographics, aggregate NFIP information, electoral and political controls. Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

The coefficient on the *Rep & Pres* variable ranges between 450,000 and 900,000 across all specifications and is significant in five of six specifications. This result is line with the prediction of Hypothesis 3: party loyalty matters. The coefficients reveal a strong party loyalty effect, regardless of the competitiveness of the House district race.

The *Rep & Pres* × *Comp* variable is the one of most interest. The *Comp* variable by itself measures the competition effect when the incumbent is of different party than the administration. The interaction coefficient summed with the *Comp* coefficient (i.e. the net of the two) indicates the competition effect when the incumbent is of the same party as the administration. The coefficient on the *Rep & Pres* × *Comp* variable ranges between -770,000 and -950,000 across all specifications and is significant in all specifications. Note that in all specifications it is close in absolute value to the coefficients on the *Comp* variable. In specifications 4 and 5, the net effect is positive. In the others, the net effect is negative. For an incumbent aligned with the administration, to find the effect of moving one percentage point toward 50%, multiply the net effect by 0.02. Regardless of the sign of the net effect, the competition effect for an incumbent is an order of magnitude smaller than the patronage effect (the coefficient on *Rep & Pres*). For example, in specification 5

), a zip code in a House district aligned with the administration receives a patronage effect of \$468,083. For every additional percentage point of vote share closer to 50% (increasing electoral competition), the aid package increases by $(996,163 - 820,724.7) * .02 = \$3,449$.

The first reported F statistics test the joint significance of the three main tactical redistribution variables. We reject the null hypothesis of no electoral influence on relief packages, in five out of the six specifications. The second reported F statistics test the sum of the *Comp* and *Rep & Pres* × *Comp.* variables, the net effect of which measures how competition impacts relief allocations in aligned districts. We are unable to reject the null that the sum is equal to zero in five of the six specifications, suggesting that aligned districts are rewarded primary though party loyalty and not competition. Overall, results suggest competition when the incumbent is of the same party as the administration matters less than when the incumbent is of opposite party. The administration prefers to reward the bulk of vote-buying resources via patronage, and does not attribute many additional resources to party incumbents in competitive districts. This result is line with the prediction of Hypothesis 4: both party loyalty and competition matter. As per our findings, we reject the null Hypothesis 1 in favor of Hypothesis 4.

Limited Sample Results

Table 4.2/4.3 (Abbreviated) shows the same set of regressions for more competitive elections, defined as those zip codes within districts wherein the Democratic vote share is between 0.40 and 0.60 (Panel A) and 0.47 and 0.53 (Panel B), respectively. Tables 4.2 and 4.3 in the Appendix display full results. This is done to further control for any unobserved economic or political indicators that could be correlated with vote share and the amount of disaster resources flowing into the district.

Coefficients on the *Comp* variable in Panel A are significant and between 650,000

Table 4.2/4.3 (Abbreviated)	OLS			Tobit		
Limited Vote Share Range	(1) Grant Total	(2) Grant Total	(3) Grant Total	(4) Grant Total	(5) Grant Total	(6) Grant Total
Panel A: Vote Share $\in (0.4, 0.6)$						
Tact. Redist. Variables						
Comp	878.2***	899.1***	899.1***	972.5***	1040.7***	770.3**
	(319.6)	(320.5)	(320.5)	(323.1)	(351.8)	(327.1)
Rep & Pres	267.5**	281.1*	281.1*	326.7**	372.4**	361.5*
	(126.4)	(144.8)	(144.8)	(157.9)	(181.0)	(189.2)
Rep & Pres × Comp	-854.5***	-819.4**	-819.4**	-1071.3***	-1142.4***	-901.9**
	(277.8)	(346.4)	(346.4)	(327.0)	(405.5)	(408.2)
F Statistic						
Tact. Redist. Variables	3.89**	3.04**	2.28*	4.63***	3.69**	2.24*
Comp + Rep & Pres × Comp=0	0.01	0.06	0.01	0.09	0.08	0.14
N	3502	3502	3502	3502	3502	3502
Panel B: Vote Share $\in (0.47, 0.53)$						
Tact. Redist. Variables						
Comp	6723.3**	6425.7***	3025.8***	14715.3***	14245.4***	9857.3***
	(3021.8)	(2223.2)	(771.6)	(5488.5)	(14.11)	(13.49)
Rep & Pres	-259.8	633.5	1812.0**	-1921.8**	-11800.2***	-8631.0***
	(366.2)	(1086.5)	(714.6)	(864.1)	(10.31)	(5.563)
Rep & Pres × Comp	-6190.8**	-6953.5**	-5273.7***	-11625.3***	512.3***	862.1***
	(2527.4)	(2782.2)	(1204.7)	(4291.4)	(14.46)	(8.167)
F Statistic						
Tact. Redist. Variables	10.78***	2.89**	8.48***	5.24***	1.e08***	6.8e06***
Comp + Rep & Pres × Comp=0	0.61	0.21	4.00*	6.36**	2.75e05***	2.6e05***
N	1036	1036	1036	1036	1036	1036
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓

Controls: grant information (Spec. 3 & 6 only), disaster severity, census demographics, aggregate NFIP information, electoral and political controls. Standard errors are clustered at the Congressional District Level. All Grant Totals are in \$ '000
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

and 1,050,000, meaning a one percentage change in vote share towards 50% results in an additional \$65,000-\$105,000 additional funds in the zip code for an incumbent not in the administration's party.[10] These results are larger than the main sample results, suggesting a heightened competition effect when a district is more "in striking range" for a challenger. The *Rep & Pres* coefficient reveals a slightly more conservative patronage effect than the main results, ranging between \$267,000 and \$372,000. The coefficient on the interaction term is also negative and of similar magnitude to the *Comp* effect.

[10]Because we recalibrate the competition variable to maintain a range of 0 and 1, a one percentage point change in vote share now corresponds to a ten percentage point change in electoral competition.

The F statistics on the sum of *Comp* and the interaction term are insignificant in all six specifications. We cannot reject the null that the net effect of competition in aligned districts is equal to zero. These three coefficients together suggest that despite the heightened focus on competition funds for unaligned districts, districts aligned with the administration are still primarily rewarded via patronage. In other words, the effect of competition is stronger in this limited range, however, like the main results competition matters less for aligned districts than unaligned districts.

Coefficients on the *Comp* variable in Panel B are significant and between 3,000,000 and 14,700,000 meaning a one percentage change in vote share towards 50% results in an additional \$1,000,000-\$4,900,000 additional funds in the zip code for an incumbent not in the administration's party.[11] This suggests vote-buying is strongest in zip codes within the tightest of battleground districts. It is however difficult to differentiate between competition and patronage at such a tight range in vote shares. Therefore, while the signs on *Rep & Pres* and *Rep & Pres* × *Comp* flip, the net effect still suggests that aligned districts get paid more and tighter competition matters.

2.6 Extensions

Distortions

The main results presented in section five measure the role of electoral and political factors in influencing relief packages. In this section, we quantify the size of the distortion produced by these influences by contrasting our results with a baseline, i.e., how relief packages would look in a world with purely technocratic and politically-neutral allocation. This process necessitates constructing a counterfactual that would represent a

[11] A one percentage point change in vote share now corresponds to a $33.\bar{3}$ percentage point change in competition.

world in which Hypotheses 1 is accurate. In this world, many factors could hypothetically be different - what cooperation looks like between political actors, the role of merit and competence, and the incentives and objectives of bureaucrats among others. Within the limitations of our data, a politically-neutral world will be constructed by solely altering the values of our main tactical redistribution variables. FEMA states that the agency considers concentration of damages, demographics, insurance coverage, special populations, and voluntary agency assistance when determining relief amounts in an area ([75]). Therefore our politically neutral allocation is one that abides by those guidelines without any Presidential interference on behalf of House elections.

To calculate the magnitude of the distortion for each zip code-disaster pair, we must address ex-ante uncertainty regarding whether the estimated differentials in grant totals stemming from electoral and political factors are primarily, reward-based, punishment-based, or a combination of both. For example, in specification 2 from Table 4.1 of our main regression, the same party indicator has an estimated coefficient of \$480,000. This estimate could be the result of the administration rewarding aligned districts \$480,00 on top of a baseline amount, the administration subtracting \$480,000 from a baseline amount for unaligned districts, or some combination of reward and punishment that nets \$480,000 (for example, rewarding the aligned district \$240,000 and subtracting \$240,000 from the unaligned district). One could conduct the same inquiry with the competition variables, although it is a more difficult exercise.

In order to create a baseline amount of aid that would occur in the politically-neutral world, we need to make assumptions about the nature of these differentials. Going from the observed political world to this counterfactual politically-neutral world, would aligned districts receive less, bringing them down to a baseline level? Or would unaligned districts receive more, bringing them back up to the baseline level? Or a combination of both? As mentioned above, all three cases are observationally equivalent in our regression results.

To calculate distortions, we take the coefficient estimates from our main regression specifications, and then adjust the electoral and political variables to mimic a counter-factual world ($\widetilde{PublicChoice}$). Every other right-hand-side variable from our regressions stays the same. We then find the predicted grant amounts in the counter-factual world ($\tilde{Y}$), and compare them to the original predicted values from our regressions ($\widehat{Y}$) to quantify the distortion ($\widehat{Distortion}$) for zip code z in district d at time t:

$$\textbf{(2)} \qquad \widehat{Distortion}_{zdt} = \widehat{Y}_{zdt}(PublicChoice_{dt}, X_{zdt}|\hat{\alpha}, \hat{\beta}) - \tilde{Y}_{zdt}(\widetilde{PublicChoice}_{dt}, X_{zdt}|\hat{\alpha}, \hat{\beta})$$

We present two different counterfactuals, each resulting from a different set of assumptions regarding how to "neutralize" political and electoral influence. In our first counterfactual $\tilde{Y}_1$, we assume a politically-neutral world is best constructed by assigning the average of the electoral variables' values to each observation. The averages are taken from the sample of observations used in the main regressions. $\widetilde{PublicChoice}$ consists of the average of *Comp*, the average of *Rep & Pres*, and a counterfactual interaction term of the two. We also take the average values for all three House subcommittee indicators: *Homeland*, *Appropriations*, and *Transportation*.

The average of *Comp* is 61%, which corresponds to a Democratic vote share of either .305 or .695. The average of *Rep & Pres* is 54.45 %, corresponding to an interaction term with a value of 33%. These three values create a baseline amount $\tilde{Y}_1$ that supports both **rewards** and **punishments**. In other words, it assumes that in the observed world, zip codes are both rewarded and punished based on electoral and political factors.

Consider the same party indicator *Rep & Pres*. Setting the indicator to the average of 54.45% reveals that aligned districts are receiving more than the baseline (by possessing a value of $1 \times \hat{\alpha}$) and unaligned districts are receiving less than the baseline (by possessing a value of $0 \times \hat{\alpha}$). The same logic can be applied to the competition variables. For example, districts with an electoral importance less than 61% (Democratic vote share greater then 69.5% or less than 30.5%) are **punished** relative to the baseline, while districts with an electoral importance greater than 61% (Democratic vote share less than 69.5% and greater than 30.5%) are **rewarded** relative to the baseline.

In our second counterfactual $\tilde{Y}_2$, we assume a politically-neutral world is best constructed by assigning a value of zero to every electoral variable: *Comp*, Rep & Pres, and their interaction term. We keep the subcommittee indicators at the averages (same as the $\tilde{Y}_1$) because the majority of our specifications and previous literature show that these subcommittee influences are minimal. These values create a baseline amount $\tilde{Y}_2$ that only supports **rewards**. In other words, it assumes that in the observed world, zip codes are only rewarded based on electoral factors and never punished.

We construct $\widehat{Y}$, $\tilde{Y}_1$, and $\tilde{Y}_2$ using regression coefficients taken from specifications 3 (OLS) and 6 (Tobit) in our main regressions. For each specification we present the univariate kernel densities of the three predicted values in Figure B.4 in the Appendix. We also present in Figure B.5 of the Appendix histograms of the distortion sizes under each of the two counterfactuals.

The blue density in Figure B.4 displays how politically-neutral allocation would look in our sample, assuming that a politically-neutral world resembles our first counterfactual $\tilde{Y}_1$. Across both specifications 3 and 6, comparing the blue density to the black density reveals that electoral factors result in both punishments and rewards. The blue histogram in Figure B.5 displays the sizes of the actual differences between the two values for each zip code-disaster pair in our sample ($\hat{Y} - \tilde{Y}_1$). The differences are distributed around zero, with some zip codes benefiting from electoral factors and other zip codes being punished. Figure B.5 reveals that for many zip codes, the distortions are in the hundreds of thousands of dollars. This suggests that non-trivial amounts of relief are diverted from zip codes that meet FEMA's definition of deserving to zip codes in which electoral incentives align on their behalf.

The red density in Figure B.4 displays how politically-neutral allocation would look in our sample, assuming that a politically-neutral world resembles our second counterfactual $\tilde{Y}_2$. Across both specifications 3 and 6, comparing the red density to the black density reveals that electoral factors only reward zip codes and never punish. The red histogram in Figure B.5 displays the sizes of the actual differences between the two values for each zip code-disaster pair in our sample ($\hat{Y} - \tilde{Y}_1$). By construction, the differences are strictly positive for all observations, with most zip codes receiving an additional \$300,000 to \$700,000 in relief.

Note that the distribution of distortions $\hat{Y} - \tilde{Y}_1$ and $\hat{Y} - \tilde{Y}_2$ are identical, with the only difference being a shift factor. This stems from the fact that the counterfactuals $\tilde{Y}_1$ and $\tilde{Y}_2$ are identical in their construction except for the three electoral variables, which are constant for every predicted value within the counterfactual group.

Given the distribution of distortion size, we investigate if the size and sign of distortions are correlated with any key demographic factors. To do this, we run a regression of the distortion on demographic, disaster, grant-level, and political controls that we use in the main regression section. We also include the same set of district, year, and Congress fixed effects. Given that the distributions of $\tilde{Y}_1$ and $\tilde{Y}_2$ are identical besides a shift factor, running an OLS regression for both would yield identical results except for different constants.[12] Distortion size for zip code z in congressional district d at time t are specified in Equation 3:

$$\textbf{(3)}\ \widehat{Distortion}_{zdt} = \alpha_0 + X\beta + \gamma_s + \gamma_d + \gamma_c + \gamma_y + \epsilon_{zdt}$$

[12] This would be untrue if we choose to use a Tobit Regression for $\tilde{Y}_2$ given that distortions are strictly non-negative, although the values are still very similar.

Table 5 (Abbreviated)	(1) Distortion Spec. 3	(2) Distortion Spec. 6
Electoral Control		
Democratic President	-296.4***	-278.4***
	(47.46)	(48.62)
Disaster Controls		
Rainfall during Disaster (mm)	0.0184	0.0153
	(0.0497)	(0.0502)
Cat3 Storm Surge Height (ft)	2.695†	3.213*
	(1.798)	(1.896)
Census Demo. Controls (Zip)		
Nonwhite Population	-0.0135***	-0.0153***
	(0.00353)	(0.00398)
White Population	0.00542***	0.00616***
	(0.00206)	(0.00191)
Households with Owner over 64	0.0277**	0.0295**
	(0.0126)	(0.0131)
Households with Owner under 64	0.0246**	0.0271**
	(0.0109)	(0.0114)
Aggregate Policy Controls (Zip)		
Mode Flood Zone: D	-28.77*	-25.74
	(14.85)	(18.97)
Mode Flood Zone: V	-31.58	-33.15
	(27.49)	(29.30)
Mode Flood Zone: X	-4.562†	-3.886
	(3.152)	(3.379)
Grant Controls		
Valid Registrations Count	-0.0122**	-0.00994†
	(0.00605)	(0.00640)
Fixed Effects		
State	✓	✓
Congressional District	✓	✓
Year and Congress	✓	✓
N	15103	15103

Standard errors are clustered at the Congressional District Level. Disaster totals are in $ '000
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5 (Abbreviated) displays select coefficient estimates for demographic variables of interest. Full results are displayed in Table 5 in the Appendix. Column 1 in Table 5 reports OLS coefficient estimates when distortions are calculated using the estimates from specification 3 from Table 4.1. Column 2 in Table 5 reports OLS coefficient estimates when distortions are calculated using the estimates from specification 6 from Table 4.1.

The coefficients on Democratic President reveals that distortions tend to be smaller during Democratic administrations by about \$280-\$300 thousand dollars. This suggests Republican administrations tend to act more strategically in their relief allocations than Democratic administrations. The insignificance of the Rainfall variable, the weak significance of the mode flood zone variables, and the weak significance of the Storm Surge variable suggests that the zip codes that are the benefactors of tactical redistribution don't tend to be hit harder by disasters, differ by flood zone or be more prone to disasters. If one considers zip codes hit by more rainfall to be relatively "more deserving" of additional aid, then it does not appear that tactical redistribution creates distortions that benefit more "deserving" disaster victims. This is also supported by the negative coefficient on the Valid Registrations variable, which is another proxy for disaster severity, as it captures the number of people who applied for aid. Zip codes in which more people applied for aid are either not rewarded via electoral influences or punished.

Predominantly white zip codes tend to be the benefactors of electoral distortions, while predominantly non-white zip codes tend to either not be rewarded or be punished. This could be for several reasons, for example urban areas tend to have higher non-white populations and also tend be safely Democratic. The lack of electoral competition in these House districts combined with the efficacy of tactical redistribution in Republican administrations could help explain this result. The positive coefficients for both household owner variables suggest that electoral influences tend to benefit older homeowners slightly more than younger homeowners. Although the difference is small, it could suggest that the wealth and voting-frequency of older homeowners attracts more vote-buying expenditures from administrations. Overall, both coefficients suggest that distortions tend be larger for zip codes better represented by owners rather than renters.

Moral Hazard

Post-disaster relief can create a moral hazard by exporting disaster damage to taxpayers and decreasing the expected value of disaster damages borne by a property owner. We consider two individual decisions that could be influenced by expectations of post-disaster relief: insurance-purchase decisions and land-use decisions.

Government spending in response to hurricanes and floods may decrease demand for flood insurance. If this is the case, when electoral influences increase grant totals, they also lessen insurance demand in the future, which in turn increases the amount of property that would use grants as a primary source of funding after disasters. Increasing the population in need of grants may increase the overall effectiveness of vote-buying and the incentive for an administration to pursue it. This series of mechanisms creates a cycle that increases overall taxpayer costs and benefits the elected officials in power. Disaster relief is targeted to a small number of homeowners in affected jurisdictions, while the costs are diffuse across the entire taxbase.

Using a dataset of household insurance purchases, Individual Assistance grants, and SBA low interest disaster loans from 2000-2011, [74] empirically analyze whether federal disaster aid crowds out household purchases of NFIP coverage. They find no effect of disaster relief on the extensive margin of insurance purchase, however, they do find an economically modest negative effect on the intensive margin. In their main Instrumental Variable specification, they find that a $1000 increase in the average Individual Assistance grant in a zip code decreases insurance coverage by $1600 per policy.

Applying their analysis to a hypothetical representative household that incorporates an entire zip code, we can estimate the added moral hazard produced by electoral influences. In the main regression results, we estimate the patronage effect i.e. when a House district is aligned with the party of the President to be $470,000-$900,000 of additional grants. Combining these estimates with the estimate from [74], we find that patronage spending decreases insurance coverage by $752,000-$1,450,000 in a zip code in the following year. In our main regression results we also estimate a competition effect of $4,000-$20,000 for every percentage point change in vote share towards 50%. Combining these estimates tells us that a one percentage point change in vote share towards 50% in turn decreases insurance coverage in a zip code by $6,400-$32,000.

There is not much literature measuring the development effects of disaster relief, as empirical settings that allow for identification of the moral hazard are rare. Among the previous papers on this subject, none have focused on FEMA post-disaster aid.[13] If post-disaster relief, or expectations of post-disaster relief, do impact building decisions in flood- or hurricane-prone regions, we apriori expect such effects to be small. For one, IHP grants are a secondary source of funding after NFIP coverage. Additionally, nearly twenty percent of zip codes in our dataset receive zero dollars in IHP, suggesting that allocation can not be taken as given by homeowners. Also, the grant totals we examine in the paper are aggregated at the zip code level. Estimating grants per household would reveal a much smaller transfer, one which is less likely to distort price signals and influence housing values. The average grant per household is about $4000 ([85]). That being said, if any development effect is in fact non-zero and economically significant, our distortion results in this paper reveals that electoral influences don't necessarily concentrate this moral hazard to higher risk regions. The weak significance of the storm surge variable, the negative sign on the valid registrations variable, and the insignificance of the rainfall variable in Table 5 all suggest that electoral influences don't benefit risk-prone regions at a higher rate than less-vulnerable regions. If there is induced housing development from IHP, tactical redistribution wouldn't necessarily be amplifying this moral hazard in the places most vulnerable to floods or hurricanes.

[13] [84] uses satellite land cover data to measure the development effects of NFIP subsidies in floodplains and finds that subsidy availability results in both short- and long-run increases in development probability. [47] measures the implicit subsidy going to homes in wildfire-prone areas from public fire-fighting expenditures. As an extension they run back-of-envelope calculations to determine the additional homes in high-risk wildland-urban-interface areas caused by the subsidy. [24] use satellite land-cover data and find that wildfire suppression on public lands has a small positive effect on the probability of development for nearby private lands.

2.7 Conclusion

In this paper, we investigate the role of electoral and political factors in determining post-disaster relief. While previous papers have highlighted the use of FEMA expenditures as vote-buying to influence Presidential elections, we use zip code level grant data to investigate its role in influencing Congressional House elections, a channel of vote-buying previously unexplored. Testing the Tactical Redistribution Model, we find significant and robust results pointing towards strong patronage by the Presidential administration towards aligned incumbents in House districts. We also find strong evidence that the competitiveness of a district's race results in a large increase in funding for challengers in unaligned districts. Results suggest that administrations are confident that grants in unaligned districts will be attributed to the party in power and not the unaligned incumbent. Despite this, the bulk of vote-buying resources are funneled through the party loyalty channel, suggesting that parties like to ensure ownership of currently aligned districts first and foremost. Our findings are robust when the sample is restricted to more competitive House districts. The limited vote-share samples reveal that as we narrow the sample to the most contested districts, the effect of competition increases significantly.

We estimate the distortions from tactical redistribution to be in the hundreds of thousands of dollars. We find that tactical redistribution benefits zip codes with more white, older, home-owning constituents at the expense of zip codes with more non-white constituents. We also find that the distortions from tactical redistribution do not benefit zip codes that receive more rain during a disaster or are more vulnerable to floods or hurricanes.

Chapter 3

Congressional Dominance of Federal Hazard Mitigation Assistance

with Sahaab B. Sheikh

3.1 Introduction

Are the recipients of federal disaster assistance the same constituencies the original legislation intended to help? This paper examines the Federal Emergency Management Agency's (**FEMA**) Hazard Mitigation Assistance program from 1997-2020 and tests the role of Congressional subcommittees in influencing the allocation of mitigation grants. Hazard Mitigation Assistance (**HMA**) grants fund property acquisitions, demolitions, retrofittings, and family relocations. We find that prior to FEMA's restructuring into the newly created Department of Homeland Security in 2003, Representatives with seats on Congressional subcommittees that oversee FEMA successfully lean on the agency to supply more mitigation expenditures to their districts. This influence likely steers expenditures from a limited budget to projects of lower marginal value, while giving less money to unrepresented, more deserving regions in need of "floodplain retreat." We also find evidence for the presence of intra-state coalitions to distribute the gains of Congressional pressure to districts within a state but not represented on a FEMA subcommittee. Congressional influence ceases after FEMA's 2003 restructuring into the Department of Homeland Security, showing how the expansion of executive power and refocus to a national constituency base lessened the power of individual members of Congress to secure expenditures for their constituencies.

Government expenditures are distributed via bureaucracies operating under the oversight of elected officials. A central theme in the public choice literature is that the incentives faced by elected officials influence the crafting and execution of public policy ([68, 69]). According to models of public choice, elected officials will bend bureaucracies to supply strategic transfers to constituencies in order to maximize votes.

As per the Constitution, while federal bureaucracies are created and operated under the executive branch, they are overseen by the legislative branch, which has the power to authorize bureaucratic programs and set agency budget. This power is largely exercised through the Congressional committee system ([86]). Overseeing members of the two branches of government have different constituent bases - the President represents the whole country, while members of Congress only represent single districts. This produces two different sets of electoral incentives that motivate two different allocations of government expenditures. In both allocations, the beneficiaries of transfers may be different than the intended constituencies nominally targeted in the original legislation. This results from elected officials trying to maximize votes while taking advantage of the transactions costs that inhibit voters from obtaining information about transfer recipients ([87]).

The Federal Emergency Management Agency is the primary federal agency charged with assisting homeowners, communities, and states in responding to and preparing for natural disasters. Since 1997, $20 billion has been paid out to homeowners, communities and states through FEMA's Hazard Mitigation Assistance grant programs to mitigate future disaster damages through property acquisitions, demolitions, and retrofitting.[1] The approval of HMA grants applications are subject to the discretion of FEMA bureaucrats, who in turn may operate under the pressures of both the executive branch and from members of Congress with seats on FEMA subcommittees.

We use the universe of FEMA HMA grants from 1997-2020 to examine how the differential incentives across branches of government influence allocations of expenditures. Models in public choice, most notably the Congressional Dominance Model, predict that Representatives will use subcommittee assignments to secure additional transfers for their constituencies in order to boost their chances of re-election ([86, 88]). This however is only possible if the organization of the bureaucracy allows for channels of Congressional influence. We exploit the 2003 FEMA restructuring into the new Department of Homeland Security (**DHS**) to test how allocations change with the expansion of executive operations. The 2003 restructuring, by elevating emergencies to a national scale, adding layers of bureaucracies, and splintering oversight into new Congressional committees, represents a potential change in Congress's ability to lean on FEMA's operations, and hence a potential change in the allocation of HMA grants.

Controlling for disaster severity and both zip code and county level characteristics, we use a stacked-cross-section approach to identify the effect of Congressional factors on HMA totals. We perform this analysis for multiple forms of hydrological disasters, including floods, hurricanes, mudslides, and severe storms. Using both ordinary least squares and Tobit models with fixed effects, we find that from 1997-2002, representation on a House FEMA subcommittee increases the amount of HMA grants received by a

[1]For comparison, the National Flood Insurance Program paid $61 billion in claims during the same period.

zip code in that district by \$30,000-\$90,000 depending on the specific subcommittee. These values represent an additional amount that is 50-150% of the median HMA zip code grant total. Summed across the entire period, we estimate Representatives with seats on subcommittees are able to bring in \$82.4 million in transfers to their districts directly. This sum represents around 4.12% of FEMA's HMA budget of \$2.03 billion for the period 1997-2002. Out of 435 districts across three Congresses, only 22 House districts receive these funds. This evidence suggests that prior to the 2003 restructuring, Representatives are successful at pressuring bureaucracies to benefit their constituencies. These dollars amounts also underestimate the total benefits of HMA projects to the district, as projects such as acquisitions, demolitions, and retrofitting theoretically benefit neighboring property owners as well. This "direct representation" effect ceases post-2003 after FEMA's restructuring into the larger DHS, corroborating previous literature that states the restructuring hindered Congress's ability to exert pressure on the agency in favor of executive objectives ([72, 89]).

We also test for the presence of coalitions. Coalitions can exist to maintain vote-trading agreements between different Representatives, who face high transactions costs in securing particularistic benefits to their districts ([90]). Since the districts within a state often share similar disaster experiences, economic drivers, and culture, one would expect intra-state cooperation between Representatives. We find that having a Representative within one's state but outside one's own district on a House FEMA subcommittee increases HMA grants by \$25,000-\$40,000 depending on the specific subcommittee. Summed across the entire period of 1997-2002, we estimate Representatives with seats on subcommittees are able to bring in \$62.4 million in transfers to the districts of their intra-state coalition members. This "indirect representation" effect is positive and significant but less than the direct representation effect, suggesting that Representatives within a state build coalitions to aid their respective constituencies. This sum represents around 3.12% of FEMA's HMA budget of $2.03 billion for the period 1997-2002. Totaled with the summed transfers from direct representation, we estimate that subcommittee members divert $142.8 million to districts of their states in this period. This indirect representation effect, like the direct effect of having one's own Representative on a subcommittee, becomes nullified after the 2003 restructuring and expansion of executive power.

Finally, we test for distortions produced by heterogeneous effects of demographic and disaster controls based on subcommittee representation. If Congressional influence lowered the "disaster severity threshold" for zip codes to receive HMA, welfare-improving expenditures are then transferred to less-deserving areas where the marginal value of mitigation is lower. We find suggestive evidence that from 1997-2002, a given level of disaster severity results in more HMA grants for zip codes represented on FEMA subcommittees as compared to unrepresented zip codes, but we lack statistical power to estimate coefficients precisely.

This paper contributes to the analysis of Congressional dominance of FEMA by offering three refinements to the previous literature. Our paper is the first in economics to examine the determinants of HMA expenditures.[2] Most previous literature on Congressional dominance of FEMA focuses on post-disaster relief, which is subject to explicit Presidential authority ([71, 72, 73]). HMA grants are not only a consequential public policy tool, but are allocated by bureaucratic discretion more so than post-disaster relief. Therefore, examining HMA better enables us to measure how "soft political power" influences bureaucratic decision-making allocations rather than explicit Presidential authority.

Second, we examine HMA grants at the zip code level. Existing papers have examined FEMA expenditures at the state level. When measuring Congressional influence,

[2]To the best of our knowledge. As of 6/03/2020.

[71, 72] and [73] sum the number of Representatives within a state with seats on the relevant Congressional FEMA subcommittees. This strategy ignores the fact that within an affected state, House Representatives with seats on FEMA subcommittees may serve in different districts than the district affected by disasters and in need of FEMA assistance. By summing to the state level, any estimated effect of subcommittee membership is therefore a convex combination of direct Congressional subcommittee representation and indirect representation by a member of Congress within one's state but in a different district. Our finer geographic scale of analysis enables us to isolate the effect of Congressional subcommittee membership on the allocation of disaster goods without making implicit assumptions about the politics of disasters in a state, as each zip code can be matched to a Congressional district and Representative. It also allows us to identify coalition effects.

Third, we study Congressional dominance of HMA grants from 1997-2020, both before and after FEMA's restructuring. This allows us to examine the role of FEMA's restructuring in changing Congressional influence. Existing papers have either only used data from the pre-restructuring period or post-restructuring period or not explicitly controlled for the restructuring's effect on committee influence. Most notably, [71] only examines 1991-1999, while [71] only examines 2003-2005. [73] examines expenditures from 1969-2005, although does not include any specific committee variables.

The rest of the paper is organized as follows: Section 2 reviews the relevant literature on disaster programs and public choice and outlines testable hypotheses. Section 3 describes the data. Section 4 details the empirical strategy. Section 5 discusses main empirical results. Section 6 discusses aggregate effects and distortions. Section 7 concludes.

3.2 Background & Literature Review

Despite being large transfers of economic consequence, disaster expenditures and their political and electoral determinants are understudied in economics beyond examinations of state-level aggregates. Disaggregated data on FEMA grant allocation and National Flood Insurance Program policies and claims only recently became public. In addition, FEMA provides no direct method to associate grant amounts with insurance policy coverage in a particular region. On the other hand, agricultural insurance and relief, a public transfer program of similar magnitude and structure, has been heavily scrutinized for its role in redistributing income from taxpayers to agricultural producers ([76, 3, 77, 78, 79, 80]). Given the President's discretion in declaring disasters and allocating post-disaster relief, existing literature on disaster financing has focused on aid allocation in Presidential election years. Specifically, previous papers have examined how state-level factors influence the rate of presidential disaster declarations in a state ([81, 82]), as well as the amount of post-disaster relief allocated once a declaration is made ([71, 72, 73]). [83] conduct a similar analysis for Small Business Administration declarations, which operate separately from presidential declarations.

FEMA was created as an independent federal agency via executive order in 1979 by President Carter. Being an independent agency, FEMA existed outside the Executive Office of the President, limiting the President's ability to appoint or dismiss the agency's head. Its creation merged together several existing independent agencies as well as subsumed several programs from the Department of Housing and Urban Development, including the National Flood Insurance Program. Congress continued to assign additional responsibilities to FEMA during its time as an independent agency, culminating with the passage of the Robert T. Stafford Disaster Relief and Emergency Assistance Act in 1988. The majority of FEMA's relief programs are performed according to the guidelines established by the Stafford Act. The Stafford Act also outlines the procedures for Presidential disaster declarations and the distribution of various types of aid, including Hazard Mitigation Grants. Prior to the 2003 restructuring, FEMA had direct Congressional committee oversight and an independent budget. Congress would fund FEMA through regular and emergency appropriations.

In response to the September 11 attacks, FEMA was subsumed into the newly created Department of Homeland Security (DHS) by the Homeland Security Act of 2002. The nominal goal of the Act was to increase the ability for federal leadership to respond to future disasters and terrorist attacks. Overall, the move emphasized a "national all-hazards approach", amplifying FEMA's focus on national response and recovery, as opposed to the more decentralized coordination role with state governments that it had in the 1990s.[3]

The DHS houses over 22 major federal agencies, including Immigration and Customs Enforcement, U.S. Customs and Border Protection, the Transportation Security Administration, the United States Coast Guard, the Cybersecurity and Infrastructure Security Agency, and the Secret Service. Starting with the 108th Congress (2003-2005), FEMA is neither independent nor under direct Congressional oversight. Its leading official is now a member of the Cabinet, who is nominated by the President and confirmed by the Senate. In addition, FEMA is now overseen in Congress along with other DHS matters. For example, the House Homeland Security subcommittee that oversees FEMA operations also oversees topics including bioterrorism, school security, and counterterrorism preparedness grants. Restructuring also increased the number of subcommittees in the House with FEMA oversight. Previously, FEMA operations were overseen by a House Appropriations subcommittee and a House Transportation and Infrastructure subcommittee.

[3] The restructuring was not without controversy. Significant infighting and turf-wars among bureaucrats occurred during the restructuring. Many believe the restructuring broke long-standing relationships between FEMA and states and other stakeholders ([91]).

After the restructuring, oversight was split from the Transportation and Infrastructure subcommittee and added to the new Homeland Security subcommittee, increasing the number of FEMA subcommittees from two to three.

FEMA's budget is no longer stand-alone but a part of the larger DHS budget. For example, in the 2020 fiscal year, $50.5 billion is allocated to the DHS. $12.5 billion of that amount is allocated to the Disaster Relief Fund, which funds the majority of FEMA's pre- and post-disaster assistance programs ([92]). Housed in FEMA are multiple programs designed to assist homeowners, municipalities, and states with both pre-and post-disaster grants. This includes the Individual Assistance and Public Assistance programs, which administer post-disaster relief, as well as the National Flood Insurance Program and the Hazard Mitigation Assistance program.

FEMA's budget is controlled by the House and Senate Appropriations Committees. Pre-2003, the Subcommittee on Veteran's Affairs, HUD, and Independent Agencies within the House Appropriations Committee was responsible for writing the bill that funds FEMA in the House. Since the restructuring of FEMA, the Subcommittee on Homeland Security within the House Appropriations Committee has been responsible. Similarly in the Senate, the Subcommittee on Veteran's Affairs, HUD, and Independent Agencies within the Senate Appropriations Committee had funding responsibilities over FEMA, but was replaced by the Subcommittee on Homeland Security in 2003.

In addition, several committees within the House and Senate oversee various aspects of FEMA. Pre-2003, the Subcommittee on Economic Development, Public Buildings, and Emergency Management within the Transportation and Infrastructure Committee oversaw issues related to emergency management in the House. Since the restructuring of FEMA, oversight has been split - both the Subcommittee on Economic Development, Public Buildings, and Emergency Management within the House Transportation and Infrastructure Committee as well as the Subcommittee on Emergency Preparedness, Response, and Communications within the House Homeland Security Committee oversee issues related to emergency management.

In the Senate, the Subcommittee on Clean Air, Wetlands, Private Property, and Nuclear Safety within the Environment and Public Works committee had jurisdiction over FEMA operations pre-2003. Since the restructuring, the Subcommittee on Federal Spending Oversight and Emergency Management within the Senate Committee on Homeland Security and Governmental Affairs has jurisdiction over FEMA operations. Table 1 in the Appendix displays the Congressional subcommittees that oversee FEMA and the average number of members in each subcommittee from 1997-2020. There are 435 voting members in the House, and 100 voting members in the Senate.

Hazard Mitigation Assistance

HMA is one of FEMA's primary assistance programs and the only one that addresses long-term vulnerability reduction goals by working to reduce the amount of housing capital and public infrastructure at risk following a disaster ([93]). HMA grants are used to fund mitigation projects such as property acquisitions, structure demolitions/relocations/elevations, flood-proofings, retrofittings, soil stabilization, and other flood risk reduction projects.

HMA provides an ex-ante measure to communities to decrease the asset base at risk of future disasters. The potential economic benefits are twofold. First, such interventions decrease the number of future flood insurance claims, which can help stabilize the NFIP's solvency and reduce the cost borne by taxpayers and policyholders who cross-subsidize the premiums of high-risk structures. Secondly, such interventions provide economic benefits to the community. Reducing the number of properties exposed to repeat-flooding and producing green space/wetlands in their place can produce positive hedonic spillovers for neighboring homeowners. Recipients of HMA need not be NFIP policyholders. By funding projects for everyone, the savings from avoided damages are transferred to uninsured households, local charities within the community who may have raised funds for the household, and the general taxpayer.

HMA grants are discretionary, in that FEMA need not automatically accept applications for funds. Bureaucrats within the agency review applications based on cost-benefit ratio and other program guidelines. This bureaucratic discretion is then subject to the preferences of the elected officials that oversee the agency. The allocation of HMA grants follows a multi-step process that is sensitive to political influence. First, property owners contact their local municipality. Municipalities are considered sub-applicants and must filter the requests of local property owners and then submit an application to their state government on behalf of the property owners. The state agency in charge of HMA then weighs mitigation priorities, consolidates all submitted projects from municipalities, and submits the primary application to FEMA.

The cost of HMA projects is shared between the federal government and non-federal sources. The non-federal share may be provided by the state, the local government, non-profit organizations, or private donations, although most often the state provides the non-federal share. The federal government generally pays up to 75% of the cost of the HMA project. Once the application reaches FEMA, the agency can either approve, ask for more information, or reject it. If the application is approved, the state becomes the recipient of the funds and then delineates funds for property grants to the municipality.

HMA grants offer a great opportunity to investigate Congressional dominance of FEMA due to data availability for both pre-and post-restructuring.[4] Also, there is no written evidence that the rules regarding HMA allocations changed with the restructur-

[4][94] investigates post-disaster relief in the form of Individual Assistance (IA) grants. Data on IA is only publicly available post-2003. The authors find null effects of Congressional dominance on the allocation of IA grants for the period of 2003-2020.

ing. In addition, HMA grants are more insulated from the discretion of the President than post-disaster relief. Despite FEMA being an independent agency pre-2003, the President has always had a unique role in declaring disasters and determining post-disaster relief packages, a role that continued post-restructuring. While post-disaster relief totals are determined by Presidential preference in the weeks after the disaster, HMA grants are allocated on a case by case basis over time by the discretion of FEMA bureaucrats. Members of Congress have the ability to intervene not only during FEMA's review of the application, but also help shape and prioritize projects as the application develops.

The role state governments have in the allocation of HMA introduces additional channels of political influence. States can pick and choose which municipalities they would like to prioritize and are the main source of non-federal funding. Together, these channels introduce a potential source of selection in our data. We revisit these topics when we discuss our empirical strategy and identification.

Congressional Dominance

It is useful to look to the institutional design of Congress to understand how committee membership and coalition-building may impact the allocation of HMA. Representatives are elected on the promise of bringing particularistic goods to their districts. They however face great transaction costs in doing so. According to [88], while logrolling agreements with other Representatives can help secure votes for one's own piece of legislation, non-contemporaneous benefit flows and non-simultaneous exchange of votes threaten the durability of such agreements, making them difficult to execute. In addition, the formation of new coalitions to create guaranteed blocks of "yes" or "no" votes is difficult due to the costs of enforcement, monitoring, and verification required to ensure coalition stability. Elected officials, who would rather spend time on district issues, campaigning, or fund-raising, prefer to remain in pre-existing, long-term coalitions and maintain delicate legislative deals spread across a wide breadth of programs rather than defect to accommodate new requests ([90]).

According to [88], the formation of Congressional committees economizes upon the transaction costs of securing benefits. Congressional members can self-select into committees that oversee policy topics that most directly impact their constituents, trading off membership on committees of lesser relevance. Committee membership offers a near monopoly over legislative proposals in that policy area. Committees set the legislative agenda and can veto or push through pieces of legislation to the rest of the Chamber. Legislators also have oversight responsibilities over the bureaucracies that execute passed legislation. Legislators have the ability to control budgets, reject internal projects, and threaten bureaucratic careers.

Operating under Congressional oversight, federal agencies are responsive to the wishes of the overseeing Congressional subcommittees. Agency bureaucrats are incentivized to adopt the policy preferences of their subcommittee members, for doing so and appeasing them would be favorable for the agency's budget and their own careers in the years following.[5] Subcommittee members are incentivized to influence the operations of the agency, for securing added benefits for their constituents can increase the probability of re-election. In the case of FEMA, we would expect subcommittee members to "lean" on the bureaucratic discretion of the agency in order to secure additional HMA for their home district or state.

Committees enable members of Congress to oversee policy topics and bureaucracies most important to their constituents. Bureaucracies however also face varying levels of executive pressure. Agencies operate under the Executive branch, and face varying

[5] Empirical tests have demonstrated that this is the case for other forms of federal spending and policy. Changes in Federal Trade Commission (FTC) policy making in the late 1970's can be traced to changes in the ideological composition of the members on the FTC Oversight sub-committee ([86]).

levels of Presidential pressure depending on the agency's degree of independence. For location-specific discretionary expenditures such as HMA, Representatives with seats on FEMA subcommittees have constituent bases and preferred allocations which differ from that of the President. Bureaucracies represent environments in which these two sets of electoral incentives clash. We present a simple political model to demonstrate these two incentives.

Suppose there are only two districts within a country: A and B. Presidential elections are determined by voters from both districts, while district elections for Representatives are determined by voters of the respective jurisdiction only. An incumbent Representative or President would like to maximize votes V with respect to expenditure budget E_{total}. Expenditure totals E_A and E_B are allocated to each of the districts respectively such that $E_A + E_B = E_{total}$. Let the probability that constituents vote for an incumbent in district A be $f(E_A)$ such $f'(E_A) \geq 0$ and $f''(E_A) < 0$. Let the probability that voters vote for an incumbent in district B be $g(E_B)$ such that $g'(E_B) \geq 0$ and $g''(E_B) < 0$. Each vote function applies to both Representatives and Presidents - assume that both offices are judged on the delivery of a single good which is a function of the expenditures the district receives.

Let n_A denote the voting population of district A. The voting population of district B is denoted as n_B or $N - n_A$. For a single Representative, his or her vote total is only affected by the expenditure total within the district. Given that the rest of the nation does not matter for the Representative's re-election, voters most likely to disapprove of any location-specific benefits to preferential groups within the district are ineligible to vote against the Representative ([87]). Within districts, the homogeneity of demographics, economic indicators, disaster profile, and culture help determine the shape of f and g.

Therefore, the Representative from district A would like to set E_A such that $f'(E_A) = 0$, and the Representative from district B would like set E_B such that $g'(E_B) = 0$. These two Representative-preferred allocations are labeled as $\tilde{E}_A$ and $\tilde{E}_B$, respectively. Also assume that $\tilde{E}_A + \tilde{E}_B > E_{total}$, in that the budget is not large enough to accommodate both Representatives' preferred allocations. The President however, has to maximize votes across both districts. The administration must attempt to satisfy both constituencies through transfers. The President's vote maximizing function is the following:

$$V_{Pres} = n_A f(E_A) + (1 - n_A) g(1 - E_A)$$

The President selects E_A to maximize V_{Pres}:

$$\frac{\partial V_{Pres}}{\partial E_A} = n_A f'(E_A) - (1 - n_A) g'(1 - E_A) = 0$$

The President would select the level of E_A such that $\frac{f'(E_A)}{g'(1-E_A)} = \frac{1-n_A}{n_A}$. Therefore, the only instance in which the preferred allocation of the President for a district is equal to the preferred allocation of the Representative is when the other district's probability function is non-increasing in E. We denote the President's preferred allocations as E_A^* and E_B^*, whose sum is equal to E_{total}.

Suppose expenditures are distributed on an discretionary basis via bureaucracies. Representatives with seats on subcommittees that oversee the bureaucracy can attempt to secure resources for a district beyond the President's preferred allocation. If the Representative on the subcommittee has the ability to influence budgets and programs enough to impact a bureaucrat's career in the following years, then the bureaucrat will grant the preferential treatment. The President however will attempt to keep bureaucracies functioning with a national focus and weigh their re-election chances when leading the bureaucracy.[6]

[6] Prior literature has demonstrated cases in which both branches have a hand in influencing allocation. Examining the large increase in federal spending associated with the New Deal, [95], [96], and [97] find that expenditures correlated with Congressional tenure, subcommittee membership, and a state's

Prior to the restructuring in 2003, FEMA was an independent agency with direct Congressional oversight. Compared to agencies established within the Executive Office of the President, the President had limited abilities to remove the head of FEMA if agency preferences conflicted with that of the President. Under this regime we would expect strong Congressional influence. If district A is represented on a subcommittee and district B is not, then we would expect $E^*_A < E_A \leq \tilde{E_A}$ - transfers to district A are greater than the President's preferred amount. Due to limited budgets, this results in $E_B < E^*_B < \tilde{E_B}$ - transfers to district B are less than the President's preferred amount.

The 2003 restructuring disrupted stakeholder relationships and placed FEMA within a larger federal bureaucracy. It's top official is now the Secretary of Homeland Security, a Cabinet position nominated by the President. The move weakens the relationships between subcommittee members and the bureaucrats they oversee. Therefore we would expect both E_A and E_B to be closer to E^*_A and E^*_B in the post-restructuring period, respectively. In addition if district A is represented on a subcommittee and district B is not, both pre and post- restructuring, we would expect $E^{Post}_A < E^{Pre}_A$ and $E^{Post}_B > E^{Pre}_B$.

In reality, there are many types of expenditures and many districts. While subcommittees offer certain privileges, they are small in membership, and appropriations bills require approval by the entire chamber, not just members of a subcommittee. Coalitions can help secure approval for programs such as HMA. Coalitions arise when the costs of ensuring coalition stability are less than the transaction costs associated with individual vote-buying deals ([90]). Districts within a state tend to experience similar disaster types and frequencies. They also tend to be closer in demographics and culture than districts across states, and may have common economic interests. We confidently conjecture that intra-state coalitions exist to help secure project expenditures for districts not directly

electoral importance in the upcoming Presidential election. [98] show how Internal Revenue Service audits occur less in states that have representation on key Congressional subcommittees or are important to the president electorally.

represented on FEMA subcommittees.

Members of subcommittees overseeing one type of expenditure may choose to benefit the constituencies of another district within the state, in return the promise of benefits from some other particularistic good (for example transportation funds or public health grants). Therefore, Representatives on subcommittees may choose a level of E less than $\tilde{E}$ for their own district, and divert the remaining resources to a neighboring, unrepresented district in exchange for another good. A Representative would do this if the marginal effect on votes of the new good exceeds that of the diverted expenditures from the original good.

Prior literature has offered mixed results over whether committee representation influences FEMA expenditures both pre- and post-restructuring, but has not commented on coalition effects. [71] examine post-disaster relief in the 1990s, prior to the 2003 restructuring of FEMA, and find that having a House Representative on an FEMA subcommittee increases the size of a state's relief package by \$10-\$15 million in 1996 dollars. In addition, they find that states that are electorally important for the President benefit from a higher rate of Presidential Disaster Declarations. [72] re-examine Congressional influence on relief in the first years after the restructuring from 2003-2005 (the 108th Congress) and find inconsistent and insignificant results. They also report that the states electorally important for the President continue to enjoy higher rates of disaster declarations post-restructuring, suggesting that the expansion of executive operations and splintering of oversight reduced Congress's ability to exert pressure while boosting Presidential control.

Both of these papers look at post-disaster relief at the state level, and are unable to identify the effect of direct representation on a subcommittee separately from indirect representation through coalitions. Although we conduct our analysis at the zip code level primarily to identify the effect of direct representation on FEMA subcommittees, indirect representation may have a role in directing disaster assistance to unrepresented areas. We test for the effect of indirect representation by measuring the number of Representatives on FEMA subcommittees within a state but outside one's own district.[7]

Testable Hypotheses

In Table 2 we present five hypotheses to represent potential scenarios of subcommittee influence (direct representation) and the presence of coalitions (indirect representation).

In our regressions, we measure congressional dominance using four variables for each FEMA subcommittee type. The first variable is an indicator for any FEMA subcommittee membership. The second variable is the interaction between the membership indicator and an indicator for the post-restructuring period - *Post*. The sum of these two variables represents the net effect of subcommittee membership post-2002, and determines how restructuring impacted Congressional dominance. For example, for a constituency represented on a subcommittee in 2005, both the *Subcomm* and the *Subcomm***Post* indicators would be equal to one.

The remaining two variables are coalition terms which measure the size of a district's intra-state cohort represented on the FEMA subcommittee type. We use the size of the district's coalition and its interaction with *Post*. The sum of these two variables represents the net effect of subcommittee coalitions post-2002.

Each of the testable hypotheses applies to a single subcommittee type. In our regressions we test each hypothesis for both the *Appropriations* and *Oversight* subcommittees.

[7] Vote records on the act that created the DHS offer suggestive evidence of the existence of intra-state coalitions. During the deliberations in crafting the Homeland Security Act of 2002, there was an amendment introduced by Representative James Oberstar (D) of Minnesota that would have kept FEMA as an independent agency (House Amendment 574 within the 107th Republican-controlled Congress). This amendment was defeated by a count of 165 in favor of to 261 against ([99]). Although the vote was firmly split across party lines (with Democrats voting for the amendment, and hence weakening the Republican-sponsored legislation), Republican House members from disaster-prone states such as Florida and Mississippi broke across party lines and voted in favor of the amendment, suggesting the presence of intra-state coalitions to maintain the role Representatives had on FEMA's oversight.

Table 2					
Hypothesis	*Subcomm*	*Subcomm*Post*	*Coalition*	*Coalition*Post*	What Matters:
1	Null	Null	Null	Null	No Cong Influence
2	Positive	Null	Null	Null	Cong Influence, No Coalitions
3	Positive	Negative	Null	Null	Cong Influence pre-2003, No Coalitions
4	Positive	Null	Positive	Null	Cong Influence & Coalitions
5	Positive	Negative	Positive	Negative	Cong Influence & Coalitions pre-2003

Table 2: Testable Hypotheses representing five scenarios of Congressional dominance capturing direct and indirect representation on FEMA subcommittees.

No Congressional Influence

Hypothesis 1 is the null hypothesis, in that it represents no effect of subcommittee representation on HMA grants throughout the study period. Members of subcommittees inform the agencies of constituent needs and give local knowledge regarding disaster-affected areas, but do not or are not successful at leaning on bureaucrats to influence their operations. Furthermore, FEMAs' restructuring in 2003 has no impact. In addition, the lack of subcommittee influence negates the possibility of intra-state coalitions aiding each others' constituencies through HMA grants.

Membership Matters

Hypothesis 2 represents a scenario in which subcommittee members are successful at securing additional HMA funds to their home districts, both pre- and post-FEMA restructuring. FEMA's restructuring does not impact Congressional influence through subcommittees. Although direct representation yields benefits to constituencies, coalitions do not impact HMA grant allocations. Constituencies in different districts within the state do not benefit from indirect representation.

Only Pre-2003 Membership Matters

Hypothesis 3 represents a scenario in which subcommittee members are successful at securing additional HMA funds to their home districts, pre-FEMA restructuring. The restructuring of FEMA into DHS nullifies their ability to lean on bureaucrats to influence their operations. Although direct representation yields benefits to constituencies pre-2003, coalitions do not impact HMA grant allocations. Constituencies in different districts within the state do not benefit from indirect representation.

Coalitions Matter

Hypothesis 4 represents a scenario in which subcommittee members are successful at securing additional HMA funds for their home districts as well as other districts within the state, both pre- and post-FEMA restructuring. FEMA's restructuring does not impact Congressional influence through subcommittees. Direct representation yields benefits to constituencies, and subcommittee members are also successful at building coalitions with their intra-state cohort to allocate HMA grants. Cohort members with seats on different subcommittees trade off favors for HMA, yielding additional benefits for their constituencies from indirect representation.

Only Pre-2003 Coalitions Matter

Hypothesis 5 represents a scenario in which subcommittee members are successful at securing additional HMA funds for their home districts as well as other districts within the state, pre-FEMA restructuring. Constituencies in the state from both within and outside the represented district benefit from representation pre-2003, however the restructuring of FEMA into DHS nullifies the ability to reap direct or indirect benefits.

3.3 Data

FEMA offers data on the universe of properties funded by HMA grants since 1989. Variables include total grant amounts, federal share of grant amounts and number of properties under each HMA grant. For our analysis, the outcome variable of interest is the total federal share of HMA grants per zip code. We use total federal share instead of total grants because HMA is often a cost-sharing agreement between the federal government and the states, and the size of the grant as well as the federal cost share percentage may both be susceptible to Congressional influence. HMA grants can only be allocated in areas that have recently received a Presidential Disaster Declaration. In our analysis we consider disasters that are classified as floods, hurricanes, tsunamis, mudslides, typhoons, coastal storms, and severe storms.

Nominally, HMA grants are determined not only by damages suffered during the most recent disaster but also the region's historical disaster experience. Therefore, we include data on contemporaneous disaster severity, historical disaster profile, and past HMA grant awards. Our analysis looks at HMA grants from 1997-2020. Therefore we collect data on contemporaneous disaster severity spanning 1997-2020, and data on historical disaster profile and past HMA grant awards from before 1997.

Isolating the portion of a HMA grant that is driven by damages from the contemporaneous disaster is difficult, as any monetary-based measure of disaster severity that exists could itself consist of totals of federal disaster expenditures. As a result, commonly-used monetary measures such as those from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) or from the National Oceanic and Atmospheric Administration (NOAA) Storm Events Database would act as bad controls.

To control for contemporaneous disaster severity, we first use measures of daily rainfall. We use data from the PRISM (Paramater-elevation Relationships on Independent

Slopes Model) Climate Group out of Oregon State University. This dataset uses 13,000 surface stations to generate 30 arc-sec grid cells for the lower 48 states. To assign a daily rainfall value to a zip code, we match the centroid of the zip code to the PRISM cell that it falls in. See Figure C.1 in the Appendix for an example daily map of the PRISM data. For the regression, we use cumulative rainfall between the start and end dates of the declared disaster.

To capture the historical disaster profile for each zip code, we construct three additional measures. First, using the PRISM data, we calculate each zip code's "rainfall history". For each zip code - disaster date combination, rainfall history gives the sum of rainfall that occurred in all previous declared disasters since 1997. Second, we include measures of vulnerability to storm-surges in hurricane-prone regions. The National Hurricane Center and Central Pacific Hurricane Center within NOAA provides Storm Surge Hazard Maps for the continental United States. We take the weighted average of wave height (ft.) for a category 3 hurricane within the boundaries of each zip code. For the regions of the United States not vulnerable to hurricanes, we assign a storm surge value of 0 for their zip code. Figure C.2 shows the map of storm surge heights corresponding to a category 3 hurricane. Lastly, from SHELDUS we include county level cumulative property damage totals from 1960-1996 ([66]). These three measures control for the effect that reoccurring disasters, such as repeated flooding or annual hurricanes, have on contemporaneous grant amounts.[8]

The receipt of HMA grants in the past may influence the demand for grants in the future. In order to control for the effect of past HMA grants on contemporaneous HMA totals, we construct sums of HMA grants for each county from the program's inception in 1988 to 1996, right before the start of our study period.

[8] Note that although we believe SHELDUS data to be problematic when controlling for contemporaneous disaster severity, the data can used as a control for past disaster experience.

House subcommittee rosters are taken from the Office of the Clerk of the U.S. House of Representatives. Senate subcommittee rosters are taken from the Secretary of the Senate. In addition to subcommittee representation, we include controls for political and electoral factors, including House election competition, gubernatorial party representation, and Presidential party representation. Gubernatorial representation is taken from the website of the National Governors Association. Voting records for House elections, which we use as a measure for competition, are taken from the MIT Election Data and Science Lab, which list the total votes per candidate in every general and special House election since 1976.[9]

We include demographic data at the zip code level, which will help capture the socioeconomic factors that will influence both demand of HMA grants and the probability that FEMA will award them. Demographic data at the zip code level is taken from the 2000 and 2010 Censuses, and include nonwhite and white populations, age of household owner, number of housing units both rural and urban, number of renters and owners, and housing units occupied by owners. This data was extracted from the NHGIS dashboard on the IPUMS website on February 3, 2020 ([67]). Monthly housing values at the zip code level are taken from the Zillow Housing Value Index and are averaged to the annual level. Yearly unemployment rate at the county level is taken from the Bureau of Labor Statistics. For both housing values and unemployment, we use the value of the year previous to the disaster date, as the contemporaneous year's measure could be influenced by the realization of the disaster itself.

FEMA also offers a subset of the universe of policies that the National Flood Insurance Program has underwritten. This subset contains all policies that were active in 2009 onwards. More specifically, we observe the universe of policies since 2009, but only

[9] See [94] for details on the construction and use of House electoral competition as a Public Choice variable.

observe the pre-2009 policies that were renewed through that year.[10] The dataset contains numerous household and policy-level covariates for 50 million policies dating back to 1984. Covariates include effective policy start date, end date, flood zone type, deductible, and total building and contents coverage. We aggregate these variables to the zip code level. We include these variables because insurance coverage is a good proxy for demand for HMA. A major goal of the HMA program is to reduce future NFIP claims.

Table 3 in the Appendix displays summary statistics for the variables within the grant regressions. All dollar values are listed in thousands ('000). Focusing on federal share of total grants, the average federal contribution at the zip code level is about $1.08 million (the median is reported much lower at $64,000 but not shown in the table). This grant total is allocated to an average of eight properties in each zip code (*Properties Count*). The average federal cost share is 74%, meaning on average other entities (states, counties, nonprofits) contribute 26% of the cost of HMA projects on top of the federal contribution. There are a handful of negative values of federal share in the data, perhaps due to data entry mistakes.

Focusing on our subcommittee variables, *Appro. Sub.* and *Oversi. Sub.*, only four and five percent of zip codes receiving HMA funds in our sample are represented on a House FEMA Appropriations or Oversight subcommittee, respectively. These percentages are low but expected as there are 435 Representatives and only an average of around 14 seats on each of these subcommittees. The *Post-Restructure* variable indicates that 70% of zip codes in our sample receive HMA grants post-restructuring (2003 +). The coalition variables, *Appro. Coa.* and *Oversi. Coa.*, indicate that the average zip code has .37 and .43 Representatives within the state but not of its district with seats on Appropriations and Oversight subcommittees, respectively.

[10] For example, a policy that started in 2000 and was terminated in 2008 would not be included in the data but a similar policy that was terminated in 2012 would be included.

The rainfall variable shows that the average cumulative rainfall between the start and end dates of the contemporaneous disaster, measured at its zip code centroid, is just over 170 millimeters (6.69 inches). The surge variable shows that on average, disaster-affected zip codes experience 0.68 foot high storm surge waves during a Category 3 hurricane. The cumulative disaster rainfall variable indicates that a zip code experiences an average of 126 millimeters of rainfall during declared disaster dates previous to the disaster contemporaneous to the HMA grant. The aggregate policy count and policy coverage variables show that on average, there is a total of 1,307 active policies in a zip code that receives HMA during a disaster with a combined $306 million of coverage.

3.4 Empirical Model

To test the Congressional Dominance Model, we construct indicators for whether a zip code falls in a Congressional district represented on the relevant Congressional subcommittee.[11] In order to compare committee influence pre-2003 and post-2003, we synthesize the different House subcommittees into harmonized indicators; *Appro. Sub.* and *Oversi. Sub.*. The *Appro. Sub.* variable represents both the pre-2003 Subcommittee on Veteran's Affairs, HUD, and Independent Agencies or the post-2003 Subcommittee on Homeland Security. The *Oversi. Sub.* variable represents both the pre-2003 and post-2003 Transportation and Infrastructure Subcommittee on Emergency Management as well as the post-2003 Homeland Security Subcommittee on Emergency Preparedness, Response, and Recovery.

Next we construct a dummy variable *Post*, which is an indicator equal to one for all dates after the restructuring of FEMA into DHS (2003 onwards). We then interact both

[11] Around 37% of zip codes in the US (represented as zip code tabulation areas by the Census) overlap more than one Congressional district. We only include a zip code in the analysis if at least 95% of its area is covered by an individual Congressional district.

of the House subcommittee indicators, *Appro. Sub.* and *Oversi. Sub.*, with *Post*. These interaction terms enable us to compare subcommittee influence in the pre- and post-restructuring periods. For example, the net effect of Oversight subcommittee influence post-2003 is measured by summing *Oversi. Sub.* and *Oversi. Sub.* × *Post*.

We construct similar indicators for Senatorial representation. In this paper we focus on the politics of relief at the district-level as opposed to state-level, but we include Senatorial representation as controls. Senators may act similarly to House Representatives in pressuring FEMA bureaucrats to secure more funds for the state, and therefore this is an important channel of political influence to control for.

HMA Grants

HMA grants for zip code z in congressional district d at time t are specified in Equation 1.

$$\textbf{(1)}\ Grant_{zdt} = \alpha_0 + \sum_{1}^{N} \alpha_{1n} CongDom_{ndt} + \alpha_2 Post + X\beta + \gamma_s + \gamma_d + \gamma_c + \gamma_y + \epsilon_{zdt}$$

$$CongDom = \{Appropriations, Oversight, Appropriations \times Post, Oversight \times Post\}$$

$CongDom$ (short for Congressional Dominance) is a composite term which includes the two subcommittee indicators that measure House subcommittee influence on HMA grants as well as their interactions with $Post$. The interaction terms measure the change in subcommittee influence post-FEMA restructuring in 2003. The sign and significance of the coefficient estimates on these four terms will verify the validity of the hypotheses listed in Table 2.

γ_s and γ_d are state and district fixed effects, respectively.[12] γ_c and γ_y are Congress fixed effects (for example, the 114th Congress) and year fixed effects, respectively. Included in X are all the grant, electoral, political, disaster severity, demographic, and aggregate policy controls mentioned in summary Table 3.

Identification

Our identification comes from variation in subcommittee rosters drawn from 435 House members. House elections occur every two years, changing chamber composition and hence subcommittee rosters as well. Our data cover Congresses 105 through 116, with three Congresses before the FEMA restructuring into DHS and nine Congresses after 2002.

Two channels of potential selection must be addressed. First, we only observe HMA projects that are accepted and funded by FEMA. It is unclear whether the dataset contains applications from states to FEMA that are ultimately rejected. Neither the data nor FEMA's website offers an indication of how often rejections occur. There are less than 100 grant observations in our dataset with a value of 0, although we believe this to be coding errors and not representative of the true rejection rate. If the true rejection rate is higher among zip codes in districts not represented on a FEMA subcommittee, than our estimated coefficients will represent a lower bound of the true effect of representation. If instead the true rejection rate is lower among zip codes in districts not represented on a FEMA subcommittee, than our estimated coefficients will represent a upper bound to the true effect of Congressional influence. If the Congressional Dominance Model is an accurate description of how members of Congress interact with the bureaucrats in the agencies they oversee, the rejection rate should be higher in zip codes not represented on a subcommittee. To help recover the true rejection rate, we would need to know every

[12]When included, γ_d absorbs γ_s.

zip code in the nation that was eligible for HMA funds, which would be every disaster-affected zip code covered by a Presidential Disaster Declaration. This information would still remain incomplete, as we still do not observe rejected applications, just zip codes that could have had rejected applications.

The role of state governments introduce another selection issue. States aggregate the applications from municipalities, and theoretically could elect to remove a municipality's project from the final application to FEMA. State governors have electoral incentives of their own which may influence the region they choose to support. In addition, states may select projects that they believe have the highest probability of being accepted by FEMA, and may consider the electoral incentives of the federal policymakers. Although we only observe the zip codes that receive funds, state politics could impact the intensive margin of a zip code's grant amount. We include an indicator, *Gov & House Rep Same Party*, for whether the party of the governor matches the party of the House Representative.[13] The party of the House Representative provides a good proxy for the general political alignment of the zip code. Therefore the interaction term between the party alignments of the two politicians provides a good indication of whether the zip code would support the governor or not. District fixed effects also capture time-invariant characteristics of the region that may impact the relationship with the state government.

In addition, the cost share component may amplify this selection effect. State governments usually provide the non-federal source of funds. Having "skin in the game" amplifies a state government's desire to pick the projects that best meet its objectives, whether the objectives are political or centered on mitigation of disaster damages. State and district fixed effects capture any time-invariant state characteristics that explain the each state's preferred allocation of HMA. In addition, indicators for governor party

[13] We also include an indicator for whether the party of the House Representative matches the party of the President, another indicator for whether the party of the governor matches the party of the President, and interactions of these indicators with the House Electoral Competition variable.

alignment capture any differences between Democratic and Republic governors in their willingness to fund the non-federal share of HMA.

A minor concern is the presence of unobserved agency preferences. Bureaucrats may have preferences on HMA allocation that vary across regions and time. The inclusion of Congress fixed effects captures any bureaucratic changes that occur due to changing chamber composition. Furthermore, district effects can also help address any unobserved bureaucratic influences.

Finally, the geographic boundaries of zip codes may change over time. We only observe the map of zip codes across the U.S. in 2000 and 2010. Ideally, we would observe the map every year. This may be an issue if the zip code boundary changes and subsequently intersects another Congressional district. To address this issue, we first only include a zip code in the analysis if at least 95% of its area is covered by a single Congressional district. This eliminates zip codes that overlap more than one Congressional district in any meaningful way. In addition, we run a regression only using zip codes that meet the above condition but also remain in the same district both pre- and post-redistricting. These two conditions give us a good indication that the zip code did not change very much geographically. Results are near identical to what we have in the paper, except the sample size is smaller.

3.5 Results

Direct Representation

Table 4 in the appendix shows full results for the HMA grant analysis. Table 4 (Abbrev.) shows the main results focusing on the subcommittee variables. Six specifications are presented: Column 1 is the simplest specification and includes the public choice

Table 4 (Abbrev.)	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
House Subcommittee Variables						
Appro. Sub.	33.12*	74.67**	72.71**	32.94*	74.50**	73.39**
	(19.61)	(32.63)	(33.07)	(19.48)	(31.72)	(32.10)
Oversi. Sub.	92.68*	88.67*	66.79	92.23*	88.40*	66.95†
	(48.05)	(47.94)	(46.75)	(47.86)	(46.71)	(45.54)
Appro. Sub. × Post	-52.00*	-92.83**	-98.40**	-51.64*	-92.18**	-98.52***
	(27.40)	(40.51)	(39.27)	(27.21)	(39.41)	(38.04)
Oversi. Sub. × Post	-114.6**	-112.5**	-99.62**	-113.7**	-111.2**	-99.05**
	(47.67)	(48.00)	(47.81)	(47.58)	(46.92)	(46.62)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District		✓	✓		✓	✓
Year and Congress			✓			✓
F Statistics						
Appro. Sub. + Appro. Sub. × Post=0	0.93	0.76	1.69	0.93	0.76	1.70
Oversi. Sub. + Oversi. Sub. × Post=0	2.72†	2.12†	4.28**	2.56†	1.98	4.24**
Subcommittee Variables	2.53*	2.97**	3.33**	2.48**	2.99**	3.44***
N	6236	6236	6236	6236	6236	6236

Controls: grant information, disaster severity, census demographics, aggregate NFIP information, electoral controls, and political controls.
Standard errors are clustered at the Congressional District Level. All Grant Totals are in \$ '000.
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$
Full results are displayed in Table 4 in the Appendix.

variables and controls for contemporaneous disaster severity, historical disaster profile, census demographics, NFIP variables aggregated at the zip code level, electoral factors, political factors, grant characteristics, as well as state fixed effects. Column 2 adds in Congressional district fixed effects. Column 3 adds in year and Congress fixed effects. Columns 4-6 run the same specifications but under a Tobit model. We do this because grant amounts are theoretically bounded from below at zero (negative grant amounts shouldn't exist).

The estimated coefficient on the *Appro. Sub.* variable ranges between 32.94 and 74.67, and is significant across all six specifications. This implies membership on the Appropriations Subcommittee on Veteran's Affairs, HUD, and Independent Agencies (pre-2003) increases HMA federal grant contributions in a zip code by \$33,000 to \$75,000 for a disaster.

The estimated coefficient on the *Oversi. Sub.* variable ranges between 66.79 and

92.68, and is significant across five of six specifications. This implies membership on the Transportation and Infrastructure Subcommittee on Emergency Management (pre-2003) increases HMA federal grant contributions in a zip code by \$66,000 to \$93,000.

The unconditional median of HMA federal grant share per zip code in our data is \$64,000. The estimated coefficients imply direct representation on the Appropriations subcommittee increases federal funds to a zip code by 50-115% of the median zip code federal contribution, and direct representation on the Oversight subcommittee increases funds by 100-143% of the median zip code federal contribution.[14] Although these effects dollar-wise are economically modest compared to FEMA's HMA budget, the findings suggest prior to FEMA's restructuring, Representatives with seats on the subcommittees that oversee FEMA's budget and operations are successful at leaning on bureaucratic functions in order to benefit their constituencies. These findings are in line with Hypothesis 2 from Table 2 for both subcommittees and suggest that Congress exerts dominance over FEMA prior to the 2003 restructuring.

The interactions of the Subcommittee variables with the *Post* indicator reveal the effect FEMA's restructuring into the larger DHS has on Congressional influence. The *Appro. Sub.* $\times$ *Post* variable indicates membership on the Appropriations Subcommittee on Homeland Security post-2002, and the *Oversi. Sub.* $\times$ *Post* indicates membership on the Transportation and Infrastructure Subcommittee on Emergency Management **or** membership on the Homeland Security Subcommittee on Emergency Management post-2002. The estimated coefficients on both interaction terms are negative and significant across all six specifications. The interaction terms cancel out the positive effect of the pre-2003 Subcommittee variables, suggesting that Congressional influence is nullified after the restructuring takes place. We fail to reject the null hypothesis that the sum of the

[14]The median for pre-2003 observations is \$25,000 and the median for post-2003 observations is \$100,000.

two terms is zero in all specifications for the Appropriations subcommittee and in four specifications for the Oversight subcommittee (not considering the 15% significance level denoted by the dagger symbol †).

These results are consistent with the findings of [72], which examines the influence of subcommittee assignments in the 108th Congress on state-level FEMA expenditures and find no evidence of Congressional dominance. Both the aforementioned paper and our paper support the idea that FEMA's restructuring into the Department of Homeland Security splintered Congressional oversight across several different committees, resulting in decreased ability for committee members to exert pressure on the agency. It also lumped FEMA with other matters of national security, resulting in committee members joining oversight committees for purposes other than influencing FEMA operations.

The reported F statistic reveals joint significance of the four House subcommittee variables in all six specifications, which adds confidence to our results. Overall, these results are in line with the prediction of Hypothesis 3 for both subcommittees types. As per our findings, we reject both the null Hypothesis 1 as well as Hypothesis 2 in favor of Hypothesis 3 - only pre-2003 membership matters.

Among the controls, a few of the estimated coefficients stand out. The coefficient for cumulative rainfall history is positive and highly significant, revealing that an additional centimeter of rainfall experienced in a disaster previous to the one that prompted the grant adds an an additional \$250-\$400 in HMA grants for a zip code. Areas with more older homeowners (64+) tend to receive more funds, while the coefficient on urban housing units reveals that urban zip codes tend to receive less.

Indirect Representation

We also test for the presence of intra-state coalitions. Previous papers on FEMA expenditures and Congressional influence ([78, 72]) conduct their analysis at the state level and do not differentiate between Representatives with seats on subcommittees from one's own district verses Representatives within the state but outside one's own district. We test for whether Representatives within the same state use their position(s) on subcommittees to aid each others' constituencies. For example, a member of Congress on a subcommittee may attempt to secure additional HMA grants for a neighboring Congressional district. This "indirect representation" is one strategy that Representatives can engage in to minimize the transaction costs of securing particularistic goods for their constituencies ([86, 88]).

In addition to the main Congressional Dominance variables, we include coalition terms that measure the number of members of Congress within a state with seats on a FEMA subcommittee, outside of one's own district. The *Appro. Coa.* and *Oversi. Coa.* variables measure the size of the coalitions on the *Appropriations* and *Oversight* subcommittees, respectively. Each of these coalition terms are then interacted with *Post* (*Oversi. Coa.* $\times$ *Post* and *Appro. Coa.* $\times$ *Post*) to measure the change in coalition influence post-2003. HMA grants for zip code z in congressional district d at time t are now specified in Equation 2 as the following:

$$\textbf{(2)}\ Grant_{zdt} = \alpha_0 + \sum_{1}^{N} \alpha_{1n} CongDom_{ndt} + \alpha_2 Post + X\beta + \gamma_s + \gamma_c + \gamma_y + \gamma_d + \epsilon_{zdt}$$

$$\begin{aligned} CongDom = \{&Appropriations, Oversight, Appropriations \times Post, Oversight \times Post \\ &Appropriations\ Coalition, Oversight\ Coalition, \\ &Appropriations\ Coalition \times Post, Oversight\ Coalition \times Post\} \end{aligned}$$

CongDom (short for Congressional Dominance) is a composite term which includes the two subcommittees indicators that measure House subcommittee influence on HMA grants as well as their interactions with *Post*. It also includes the two coalition terms as well as their interactions with *Post*. The sign and significance of the coefficient estimates on these eight terms will verify the validity of the hypotheses listed in Table 2.

Table 5 in the appendix reports full results for the HMA coalition analysis. Table 5 (Abbrev.) shows the main results focusing on the coalition variables. Six specifications are presented. Column 1 includes all controls from the main regression and state fixed effects. Column 2 adds in year and Congress fixed effects. Column 3 adds in Congressional district fixed effects. Columns 4-6 run the same specifications but under a Tobit model.

The estimated coefficient on the *Appro. Coa.* variable ranges between 25.90 and 34.55, and is significant across four out of six specifications. This implies that indirect representation on the Appropriations Subcommittee on Veteran's Affairs, HUD, and Independent Agencies (pre-2003) by a single Representative increases HMA federal grant contributions in a zip code by \$26,000 to \$35,000. This effect is smaller than the effect of direct representation, as displayed by the estimated coefficient for the *Appro. Sub.* variable. As reported by the first F statistic in Table 5 (Abbrev.), we fail to reject the null that the Appropriations subcommittee variable is greater than or equal to the Appropriations coalition variable in all six specifications.

The estimated coefficient on the *Overs. Coa.* variable ranges between 25.33 and 40.74,

Table 5 (Abbrev.)	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
House Coalition Variables						
Appro. Coa.	34.55*	30.10*	25.90	34.55*	30.31*	26.21
	(17.78)	(17.44)	(21.21)	(17.70)	(17.33)	(20.59)
Oversi. Coa.	25.61†	30.82**	40.74*	25.33†	30.74**	40.67*
	(15.66)	(15.62)	(23.23)	(15.55)	(15.50)	(22.55)
Appro. Coa. × Post	-44.20**	-34.52*	-27.61	-44.03**	-34.40*	-27.59
	(18.89)	(19.34)	(22.82)	(18.79)	(19.21)	(22.15)
Oversi. Coa. × Post	-19.11	-23.74	-31.82	-18.23	-23.24	-31.32
	(18.10)	(18.00)	(25.65)	(17.99)	(17.84)	(24.84)
House Subcommittee Variables						
Appro. Sub.	35.67*	36.19*	58.44*	35.54*	36.62*	59.19*
	(19.98)	(20.00)	(32.99)	(19.86)	(19.94)	(31.99)
Oversi. Sub.	75.11*	70.80†	50.89	74.71*	70.69†	50.93
	(42.59)	(43.19)	(44.74)	(42.41)	(42.99)	(43.49)
Appro. Sub. × Post	-55.03*	-57.11**	-84.07**	-54.72*	-57.04**	-84.19**
	(28.26)	(27.24)	(38.50)	(28.09)	(27.01)	(37.28)
Oversi. Sub. × Post	-97.91**	-95.45**	-80.76*	-96.71**	-94.85**	-79.88*
	(42.72)	(43.13)	(45.75)	(42.67)	(42.97)	(44.56)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
Congressional District			✓			✓
F Statistics						
Appro. Sub. ≥ Appro. Coa.	0.00	0.10	0.95	0.00	0.10	1.03
Oversi. Sub. ≥ Oversi. Coa.	1.42	0.83	0.06	1.42	0.84	0.06
Appro. Coa. + Appro. Coa. × Post=0	0.84	0.18	0.02	0.82	0.15	0.01
Oversi. Coa. + Oversi. Coa. × Post=0	0.51	0.56	0.64	0.61	0.64	0.76
Coalition Variables	2.97**	3.22**	2.18*	2.98**	3.28**	2.34*
Subcommittee Variables	2.68**	2.86**	2.79**	2.60**	2.84**	2.85**
Coalition and Subcommittee Variables	2.59***	2.93***	2.78***	2.57***	2.97***	2.92***
N	6236	6236	6236	6236	6236	6236

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Controls: grant information, disaster severity, census demographics, aggregate NFIP information, electoral controls, and political controls.

Full results are displayed in Table 5 in the Appendix.

and is significant across all six specifications. This implies that indirect representation on the Transportation and Infrastructure Subcommittee on Emergency Management (pre-2003) increases HMA federal grant contributions in a zip code by $25,000 to $41,000 per Representative. This effect is smaller than the effect of direct representation, as displayed by the estimated coefficient for the *Oversi. Sub.* variable. As reported by the second F statistic in Table 5 (Abbrev.), we fail to reject the null that the Oversight subcommittee variable is greater than or equal to the Oversight coalition variable in all six specifications.

These findings suggest that prior to FEMA's restructuring in 2003, in addition to securing funds for their own constituencies, Representatives engage in "deals" to benefit the constituencies of their intra-state cohort members. These bestowed funds are smaller than the funds gained by direct representation, suggesting that Representatives choose to benefit the constituencies of their home district more so than those of neighboring districts. This makes sense given that Representatives should pay more attention to the constituencies that can vote for their re-election. These results are in line with Hypothesis 4 from Table 2 for both subcommittees, which states that subcommittee membership and coalitions matter throughout the study period.

Much like the *Appro. Sub.* $\times$ *Post* and *Oversi. Sub.* $\times$ *Post* interaction terms, the interaction of the coalition variables with *Post* are negative and are of similar absolute value to the uninteracted coalition variables. As reported by the third and fourth F statistics in Table 5, we fail to reject the null hypothesis that the sum of the two terms is zero in all six specifications for the both subcommittees. These results suggest that any benefits of indirect representation that exist pre-2003 are also nullified after the restructuring along with the benefits of direct representation. Table 5 also reports F statistics for the joint significance for the group of coalition variables, the group of subcommittee variables, and both variable groups together. The F statistics are significant across all specifications for each grouping. Overall, these results are in line with the prediction of Hypothesis 5 from Table 2 for both subcommittees. As per our findings, we reject Hypotheses 1-4 in favor of Hypothesis 5 - only pre-2003 membership and coalitions matter.

3.6 Pre-Restructuring Aggregates and Distortions

We estimate the total amount of HMA Representatives on FEMA subcommittees are able to divert to their own districts prior to the 2003 FEMA restructuring. To do so we multiply the estimated coefficients for pre-2003 subcommittee representation from Table 4 with the number of zip codes that receive HMA and are directly represented on FEMA subcommittees during the 1997-2002 period. There are 51 zip codes represented on the Oversight subcommittee and 98 zip codes represented on the Appropriations subcommittee that receive HMA expenditures during this period. In our sample, about $12 million is diverted through direct representation to 22 unique Congressional districts. This sum represents 4.12% of total federal HMA contributions in our data for the 1997-2002 period. Applying this percentage to the universe of HMA grants reveals that an estimated $82.4 million out of the $2 billion budget for HMA expenditures across the six years was obtained via direct Congressional representation.[15]

We also estimate the amount of HMA expenditures Representatives on FEMA subcommittees are able to divert to the districts of intra-state coalition members prior to the restructuring. We multiply the estimated coefficients for pre-2003 coalition representation from Table 5 with the number of zip codes that receive funds and are indirectly represented on a FEMA subcommittee. There are 133 zip codes indirectly represented on the Oversight subcommittee and 107 zip codes indirectly represented on the Appropriations subcommittee that receive HMA from 1997-2002. In our sample, about $9.1 million is diverted through indirect representation. We scale this figure up to represent the universe of HMA expenditures for the time period. We find that an additional $62.4 million out of the $2 billion budget for HMA expenditures across the six years is obtained via intra-state coalitions. Overall the combined diverted funds from indirect and direct

[15] We do this scaling exercise because our regression analysis contains 63% of all zip codes that receive HMA grants for our sample period of 1997-2020.

representation total \$144.8 million, 7.24% of all HMA expenditures for the 1997-2002 period.

Next, we limit the regression to only pre-2003 observations and interact subcommittee representation with select demographic, disaster, and grant controls. The interaction terms allow us to measure any heterogeneity via Congressional influence that impacts the relationship between grant amounts and demographic and disaster measures. For example, rainfall interacted with subcommittee representation measures how every additional millimeter of rainfall impacts HMA totals conditional on subcommittee representation.

Equation 3 tests if demographic and disaster controls have heterogeneous effects based on subcommittee representation. HMA grants for zip code z in congressional district d at time t are now specified in Equation 3 as the following:

$$\textbf{(3)}\ Grant_{zdt} = \alpha_0 + \alpha_1(\textit{Rep on Sub}) + \sum_i \alpha_i(\textit{Rep on Sub} \times Control_i) + X\beta + \gamma_s + \gamma_c + \gamma_y + \gamma_d + \epsilon_{zdt}$$

To simplify the analysis and cut down on the number of interactions, we synthesize the two subcommittee indicators, *Appro. Sub.* and *Oversi. Sub.*, into a single common indicator for subcommittee representation - *Rep on Sub.*. We then interact *Rep on Sub.* with each of the demographic, disaster, and grant variables of interest represented by $Control_i$ - rainfall, disaster rainfall history, Category 3 storm surge height, cumulative county property damage 1960-1997, cumulative county HMA grants 1989-1997, nonwhite population, white population, housing owners over 64, housing owners under 64, county unemployment rate, housing value, and party of President.

Table 6 in the Appendix displays full regression results for the distortion analysis. Table 6 (Abbrev.) displays the interactions of the four disaster controls and subcommit-

Table 6 (Abbrev)	OLS (Specification 3) Grant Total	Tobit (Specification 6) Grant Total
Rep on Sub.	99.98†	99.98†
	(67.72)	(63.14)
Rainfall during Disaster (mm) × Rep on Sub.	0.120	0.120
	(0.274)	(0.255)
Cum. Disaster Rain 1997+ (mm) × Rep on Sub.	0.788	0.788†
	(0.564)	(0.525)
Cat3 Storm Surge Height (ft) × Rep on Sub.	52.21†	52.21*
	(32.05)	(29.88)
Disaster Prop. Damage 1960-96 ($) × Rep on Sub.	-0.001*	-0.001*
	(0.0008)	(0.0007)
Fixed Effects		
State	✓	✓
Year and Congress	✓	✓
Congressional District	✓	✓
N	1969	1969

Data is from 1997-2002. Specifications 3 and 6 are similar to Table 4. All Grant Totals are in $ '000
Standard errors are clustered at the Congressional District Level.
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$
Controls: grant information, disaster severity, census demographics, aggregate NFIP information, electoral controls, and political controls.

tee representation. We focus on the four disaster interactions because they are a direct measure of actual need for HMA. Any heterogeneous effects would suggest that represented zip codes, regardless of need, receive funds at a lower disaster "threshold" or more dollars per unit of disaster severity.

The first column is identical to Specification 3 in Table 4 except for the inclusion of the additional interactions. The second column is identical to Specification 6 in Table 4 except for the inclusion of the additional interactions. The estimated coefficient on the synthesized *Rep on Sub.* variable is 99.98, representing an additional $99,980 of HMA from having a Representative with a seat on a FEMA subcommittee (Oversight or Appropriations). This coefficient is a weighted average of the effect of Appropriations representation and the effect of Oversight representation.

For the disaster interactions, all coefficient estimates (except *Disaster Prop. Damage 1960-96* × *Rep on Sub.*) are consistent with the Congressional Dominance Model.[16] Ad-

[16]The property damage interaction is significant and negative, however, the magnitude is very small.

ditional disaster severity results in more HMA funds for zip codes represented by members of Congress on FEMA subcommittees compared to those that are not represented. An additional centimeter of rainfall during the contemporaneous disaster yields $1,200 more in HMA for a represented zip code than for an unrepresented zip code. In addition, an extra centimeter of rainfall in any disaster previous to the contemporaneous disaster yields $7,800 more in HMA for a represented zip code. Most notably, an additional foot (30.48 centimeters) of Category 3 storm surge height yields $52,000 more in a represented zip code. We do lack power to measure estimates with precision for this analysis , most likely because of a lack of observations (three Congresses and 22 represented House districts that receive HMA funds).

These results offer suggestive evidence that either represented zip codes are given more expenditures conditional on a certain disaster profile/experience, or, represented zip codes with less severe disaster profiles are given similar expenditures to unrepresented zip codes with more severe profiles. If this is the case, expenditures are funneled to projects of lower marginal value to the community. HMA funds potentially increase property values by removing "problem" structures and creating greenspace. They reduce community trauma from disasters, and reduce future flood insurance claims. Congressional influences from 1997-2002, by diverting funds from limited budgets to preferred zip codes, remove the benefits of HMA from others.

We do not run the distortions extension on the post-2002 data, as there is no evidence of Congressional influence on HMA grants after the restructuring of FEMA into DHS. Although none of the requirements or application processes changed for HMA with the restructuring, the added layers of bureaucracy, weakening of stakeholder relationships, and shift to a national focus hindered the ability of members of Congress to influence where HMA funds end up.

3.7 Conclusion

Hazard Mitigation Assistance is potentially an effective federal tool in mitigating future climate damages. HMA grants can accelerate floodplain retreat, remove properties from the housing stock that would not otherwise be purchased, and reduce future flood damages. According to the Federal Emergency Management Agency, it is the only form of FEMA financing dedicated to "breaking the cycle of damage, reconstruction, and repeated damage" ([93]). Therefore these transfers to at-risk homeowners can have positive efficiency effects if executed appropriately. They can reduce the number of future flood insurance claims, helping to solidify the solvency of the National Flood Insurance Program and reduce the burden on taxpayers and other policyholders. Within the community, they can produce positive hedonic spillovers, reduce the burden on local charities and disaster services, and lessen community trauma. As flood patterns change and produce hedonic effects, homeowners may have trouble selling their properties in floodplains and migrating to safer ground. HMA provides a "buyer of last resort" by the federal government and reduces the asset base at risk of future disasters.

HMA grants, like any transfer, are subject to the incentives of elected officials who oversee their allocation. If HMA is "hijacked" by members of Congress with seats on FEMA subcommittees in order to boost re-election prospects, funds may be directed towards constituents who have less financial need for such transfers, reducing the net benefits of the program. Therefore it is important to measure the degree to which HMA is influenced by elected officials.

We test for Congressional dominance of FEMA from 1997-2020, both before and after FEMA's restructuring into the larger Department of Homeland Security in 2003. We find that prior to the restructuring, Representatives on FEMA subcommittees successfully divert HMA funds to their own constituencies to the order of 50%-150% of the median federal contribution per zip code. This influence ceases after the restructuring, suggesting that the additional bureaucratic layers of the DHS, expansion of executive power, and splintering of oversight across committees nullify the ability of Representatives to influence the bureaucratic discretion of FEMA officials.

We also detect the presence of intra-state coalitions pre-2003, suggesting that Representatives on FEMA subcommittees engage in deals with other Representatives from their state to deliver HMA grants to unrepresented districts. This indirect representation effect also ceases after the 2003 restructuring of FEMA into DHS.

We also test for heterogeneous effects of disaster and demographic controls conditional on subcommittee representation. We find suggestive evidence of distortions from the transfers in the pre-restructuring period. Not only do represented zip codes with similar "disaster profiles" receive more HMA funding than unrepresented zip codes, but each unit of disaster severity reaps higher funding for represented zip codes. By diverting funds from limited budgets to less at-risk properties, Congressional influence stunts floodplain retreat in unrepresented zip codes for the benefit of represented zip codes, reducing the positive pecuniary and hedonic effects of the transfer.

Acknowledgments

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Storm Surge Hazard Maps from the National Hurricane Center and the Central Pacific Hurricane Center are accessible at https://www.nhc.noaa.gov/nationalsurge/.

Rainfall data comes from the PRISM Climate Group hosted out of Oregon State University. The Prism Climate data were accessed on November 21, 2020 and is accessible at http://prism.oregonstate.edu/.

The HMA data was downloaded from the FEMA website. This product uses the Federal Emergency Management Agency's API, but is not endorsed by FEMA." FEMA cannot verify the quality and/or timeliness of any data or any analysis derived after the data has been retrieved from FEMA.gov The logos and/or seals of FEMA and the Department of Homeland Security (DHS) may not be used without prior written authorization from FEMA or DHS. Data were accessed on May 23, 2020.

Appendix A

Federal Flood Insurance Subsidies and Induced Floodplain Development

Acronyms and Definitions in Section 3

National Flood Insurance Program (NFIP) - Federal program established by the National Flood Insurance Act of 1968

Flood Disaster Protection Act - Amendment to the original 1968 act, signed into law December of 1973. Prohibited all federally regulated or insured banking institutions from extending mortgage loans to properties within a 100-year floodplain unless flood insurance was acquired for the property. Resulted in the majority of permit-issuing places in the United States joining the NFIP.

Chargeable Rate - the subsidized, flat insurance rate that was available to all pre-1968 and pre-FIRM structures.

Flood Hazard Boundary Map (FHBM) - The preliminary map used to outline the boundaries of any 100-year floodplain within a community. Was used as preliminary information until the full flood map was drawn.

Flood Insurance Rate Map (FIRM) - The full, official flood map that delineated zones both within and outside the 100-year floodplain and set the rates within each zone.

Emergency Program - The amount of time between the completion of the FHBM and the completion of the FIRM. New structures are eligible for the Chargeable Rate

Regular Program - The time following the completion of the FIRM and full entry of the community into the NFIP. All new structures are only eligible for the full-risk rate.

Pre-FIRM Any structure built before the completion of the community's FIRM

Flood Insurance Administration - The bureaucratic unit within the United States Department of Housing and Urban Development that managed the NFIP previous to the establishment of FEMA

Federal Emergency Management Administration (FEMA) - Centralized agency created in 1979 that replaced the Flood Insurance Administration in running the NFIP

Section A—Structure—One- to Four-Family Residential

Type of Structure	A	AO	Zone B	C	D
One story—no basement	.35	.30*	.03	.01	.20
Two or more stories—no basement	.30	.25*	.02	.01	.15
Split level—no basement	.30	.25*	.02	.01	.15
One story—with basement	2.05	2.00	.15	.10	1.10
Two or more stories—with basement	1.30	1.35	.10	.10	.70
Split level—with basement	1.30	1.35	.10	.10	.70
Mobile home on foundation	1.40	.65*	.15	.15	.80

Table A.1 Full-risk rates for non-floodplain structures. Zones B and C are areas designated as outside the 100-year floodplain. Taken from the 1975 National Flood Insurance Program Flood Insurance Manual.

FIA ELEVATION RATE TABLE III
SECTION A—ONE TO FOUR FAMILY RESIDENTIAL STRUCTURE
ONE STORY

ELEVATION OF FIRST FLOOR ABOVE OR BELOW BASE FLOOD ELEVATION	NO BASEMENT ZONES				WITH BASEMENT ZONES			
	A1—A7	A8—A14	A15—A17	A18—A30	A1—A3	A4—A7	A8—A9	A10—A30
+ 5 OR MORE	.01	.01	.01	.01	.10	.10	.10	.10
+ 4	.01	.01	.01	.01	.10	.10	.10	.10
+ 3	.01	.01	.02	.04	.10	.10	.10	.10
+ 2	.01	.02	.05	.08	.10	.10	.11	.13
+ 1	.01	.07	.10	.15	.90	.30	.24	.22
0	.12	.16	.19	.23	4.78	.84	.49	.33
− 1	.48	.31	.31	.34	13.13	2.13	.95	.49
− 2	1.59	.55	.47	.48	*	4.95	1.77	.71
− 3	*	.93	.70	.64	*	6.73	3.15	.98
− 4	*	1.48	1.00	.83	*	*	5.16	1.36
− 5	*	2.34	1.40	1.07	*	*	*	1.87
− 6	*	2.86	1.91	1.34	*	*	*	2.52
− 7	*	*	2.62	1.66	*	*	*	3.40
− 8	*	*	3.53	2.02	*	*	*	4.56
− 9	*	*	*	2.48	*	*	*	5.21
−10	*	*	*	3.03	*	*	*	*
−11 OR LOWER	*	*	*	*	*	*	*	*
ZONE RATE	.35	.55	.73	.95	7.36	2.01	1.33	1.12

*USE $25.00 RATE.

Table A.2: Full-Risk Rates for a one-story residential structure inside the 100-year floodplain. Taken from the 1975 National Flood Insurance Program Flood Insurance Manual.

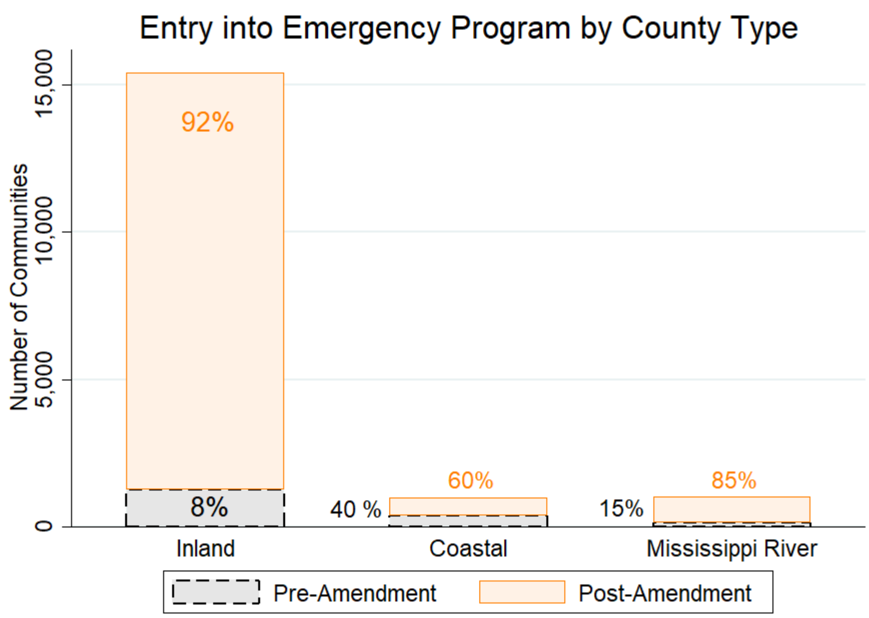

Figure A.1: Entry into Emergency Program by County-Type. Each bar measures the number of communities within the specific county-type that joined the Emergency Program pre-Amendment and post-Amendment. Due to data limitations, this figure only includes municipalities and excludes unincorporated counties.

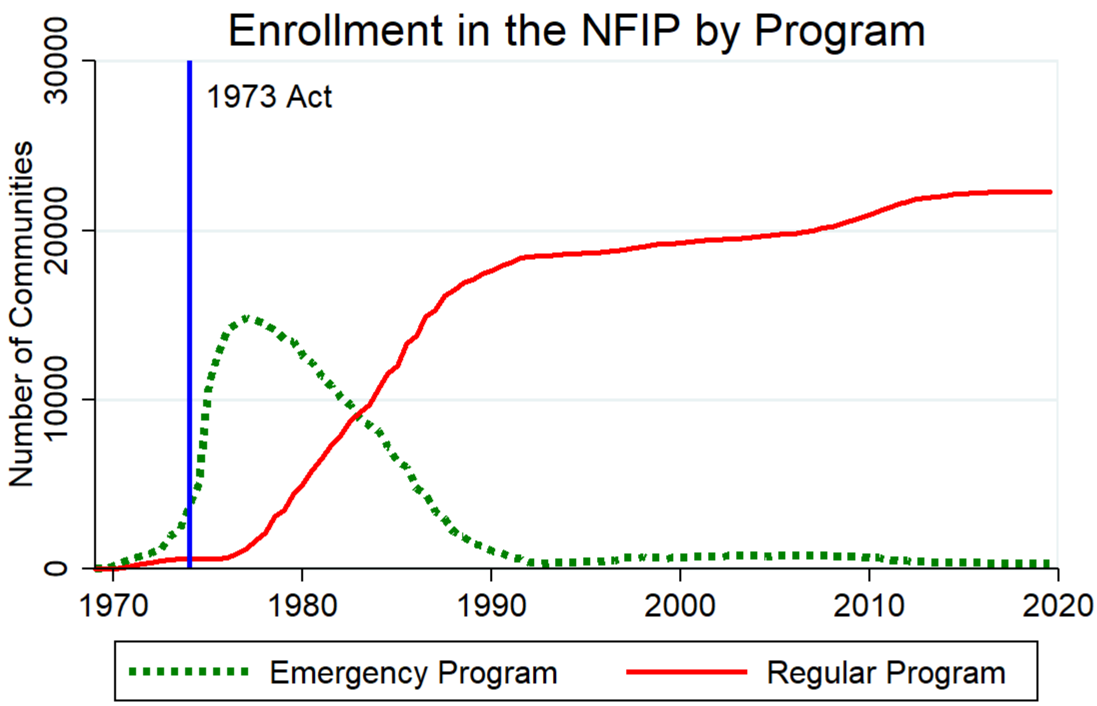

Figure A.2: Cumulative Entry into (and Exit out of) Emergency Program and Entry into Regular Program. The green line displays the number of communities across the nation that were enrolled the Emergency Program. The red line displays the number of communities across the nation that were entered the Regular Program.

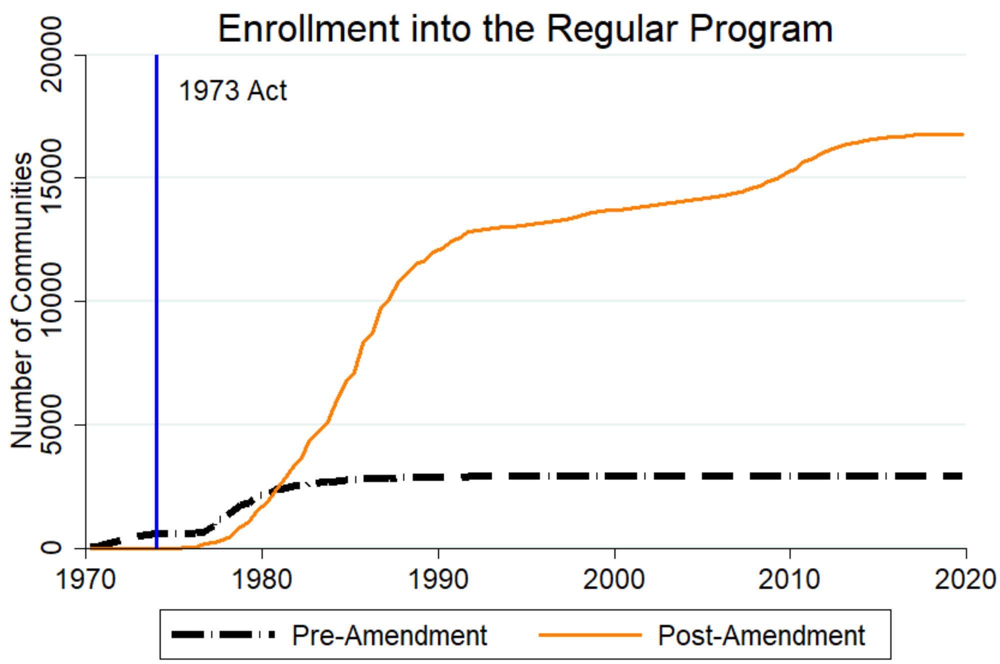

Figure A.3: Cumulative Entry into Regular Program by Amendment-Type. The dashed black line represents communities that joined the Emergency Program previous to the 1973 Amendment. The solid orange line represents communities the joined the Emergency Program after the 1973 Amendment.

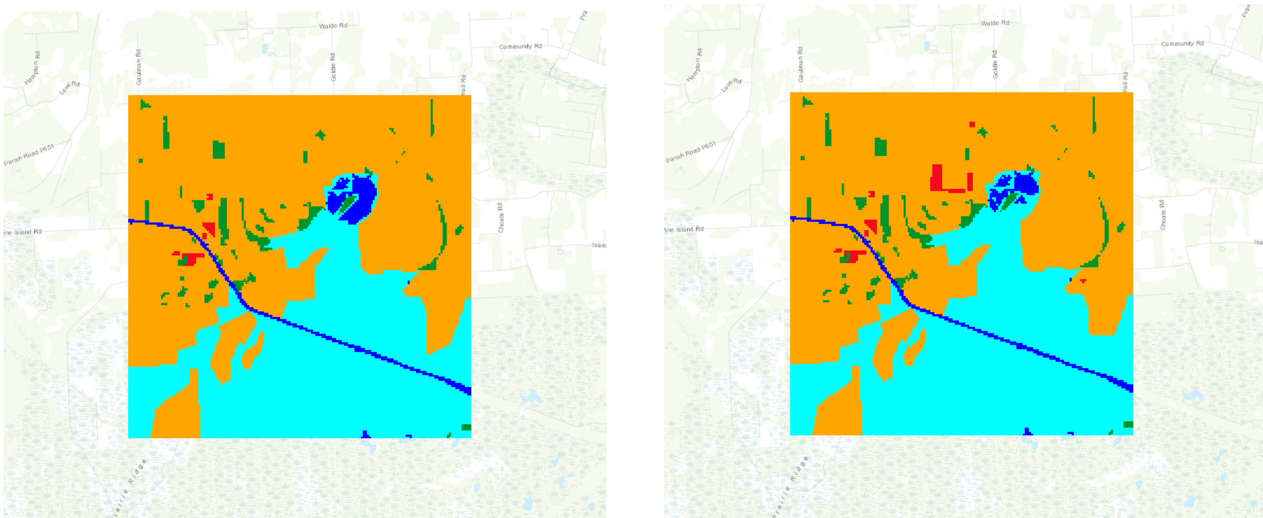

Figure A.4: Vermillion Parish, Louisiana in 1973 and in 2000. Red pixels indicate development.

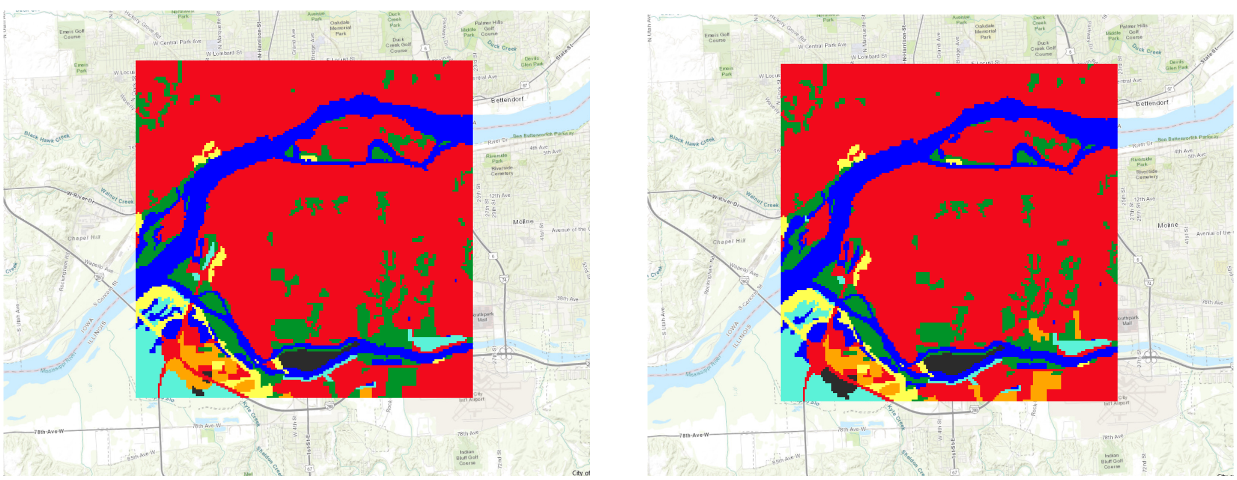

Figure A.5: Rock Island, Illinois (south bank of river) and Davenport, Iowa (north bank of river) in 1973 and in 2000. Red pixels indicate development.

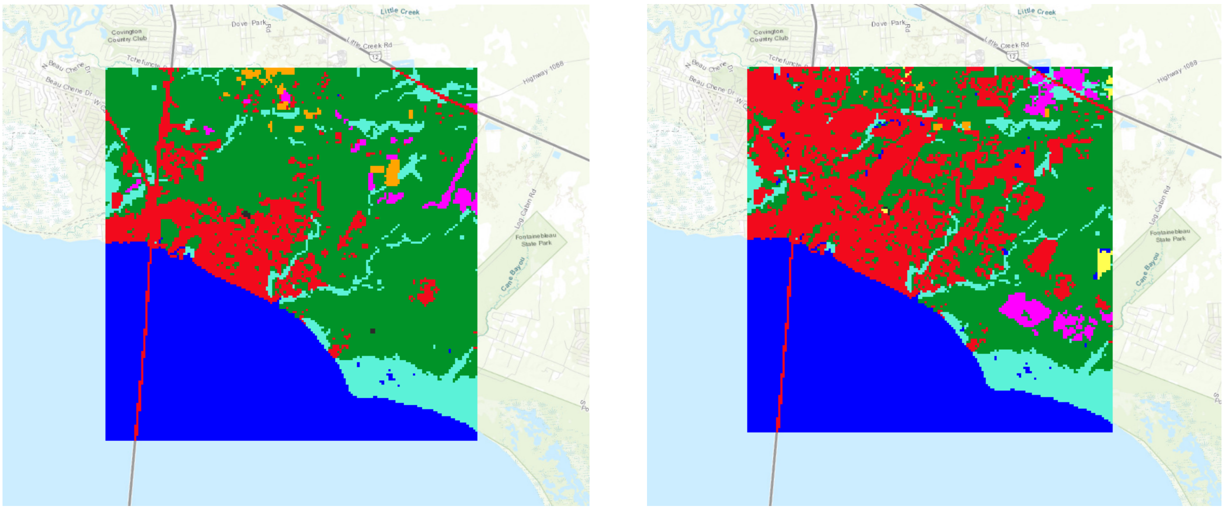

Figure A.6: Mandeville, Louisiana in 1973 and in 2000. Red pixels indicate development.

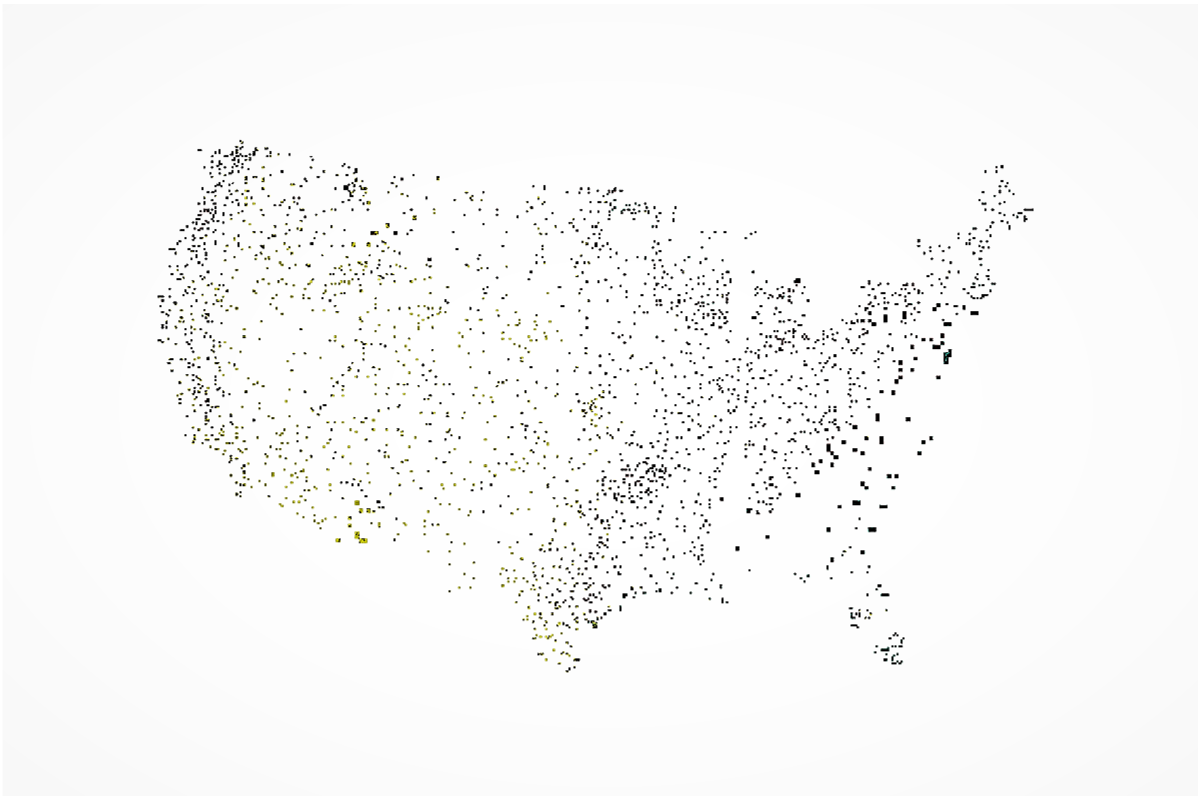

Figure A.7: Full Coverage of the Land Cover Trends Database

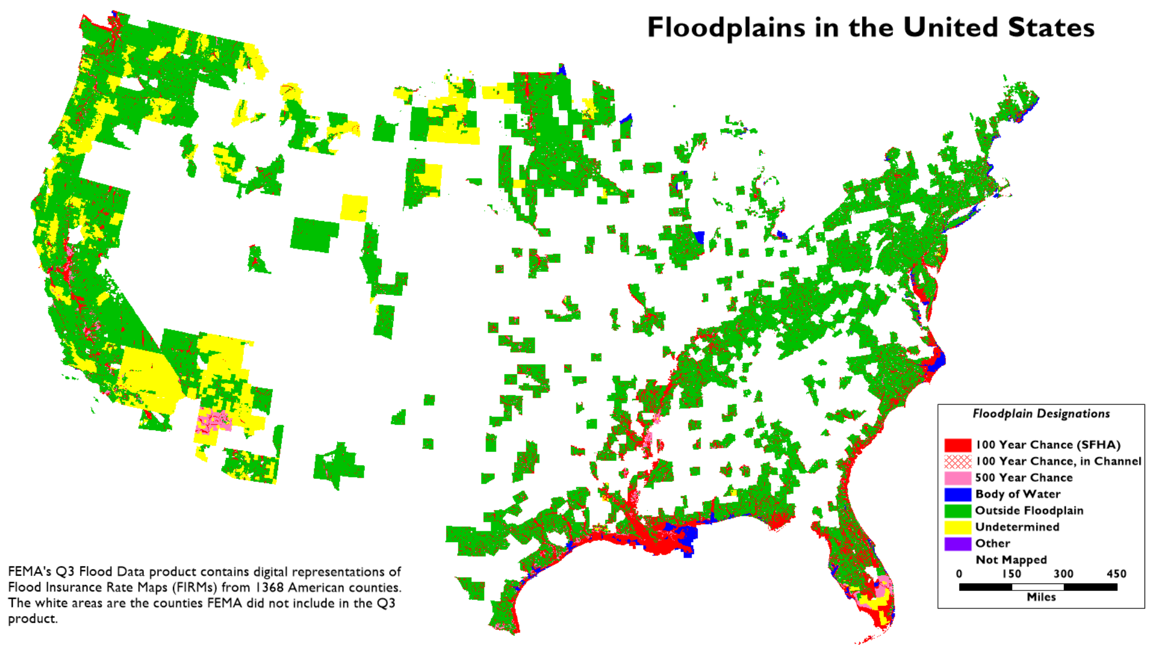

Figure A.8: Floodplains in the United States

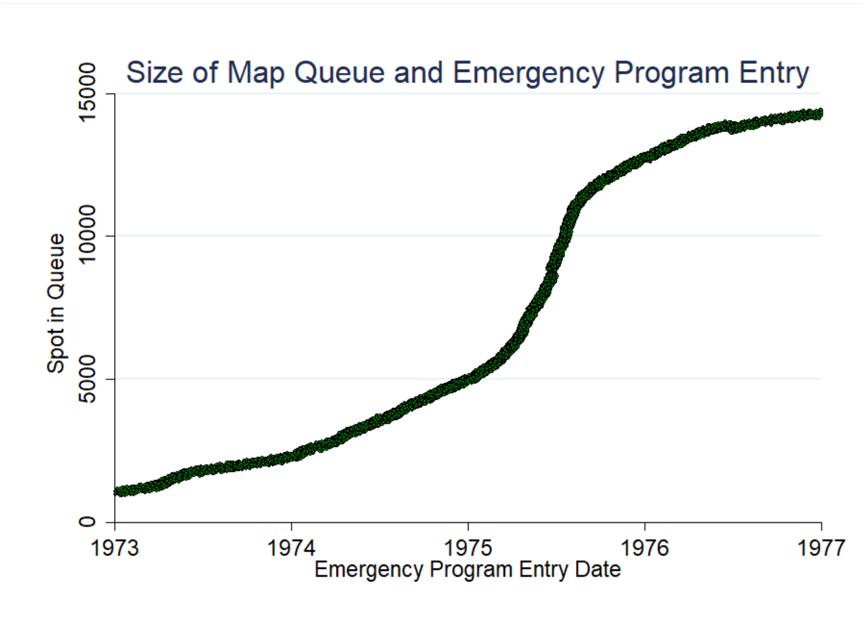

Figure A.9: Entry into Emergency Program and Size of Queue

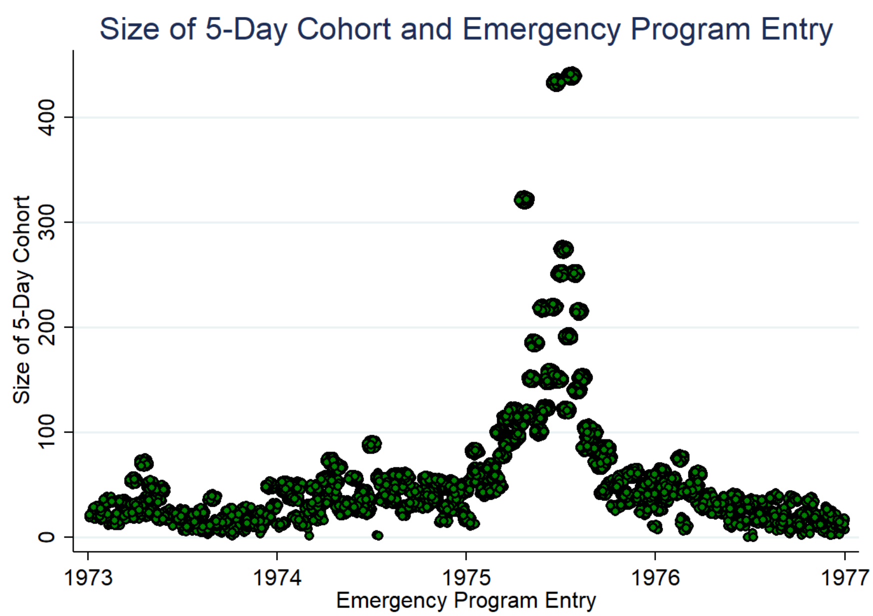

Figure A.10: Entry into Emergency Program and Size of 5-day Emergency Program cohort

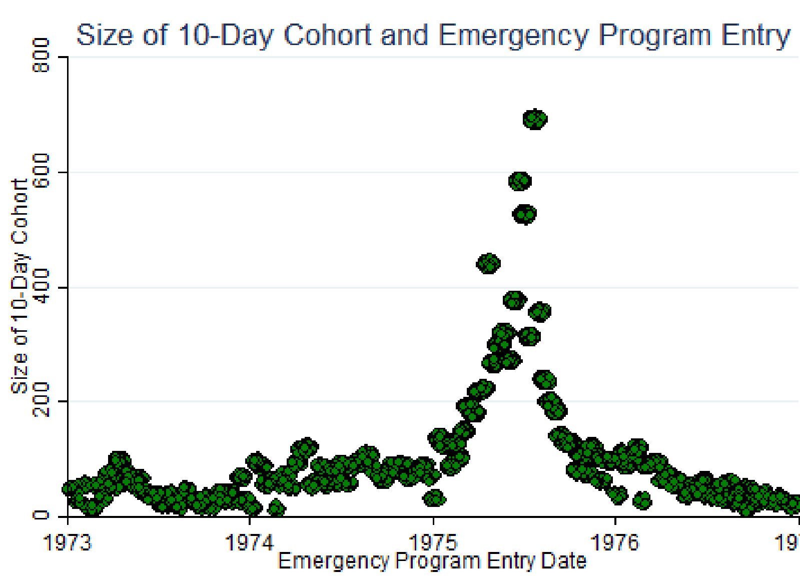

Figure A.11: Entry into Emergency Program and Size of 10-day Emergency Program cohort

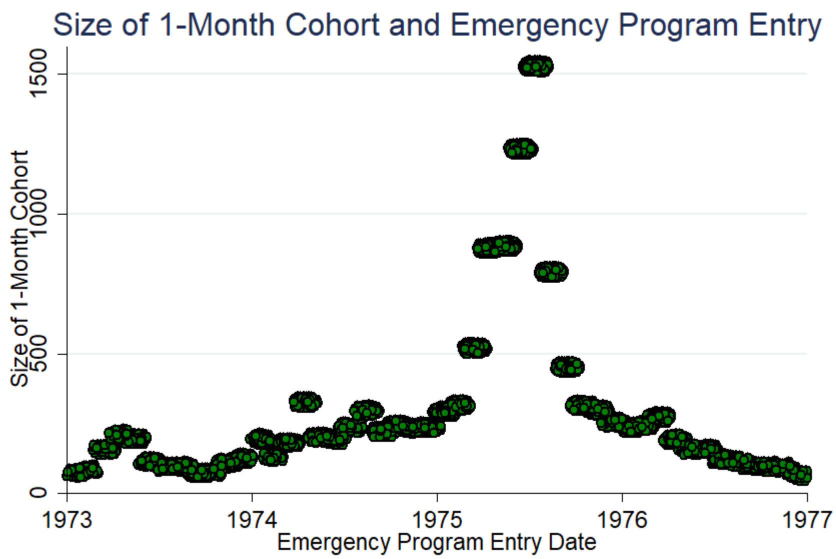

Figure A.12: Entry into Emergency Program and Size of 1-month Emergency Program cohort

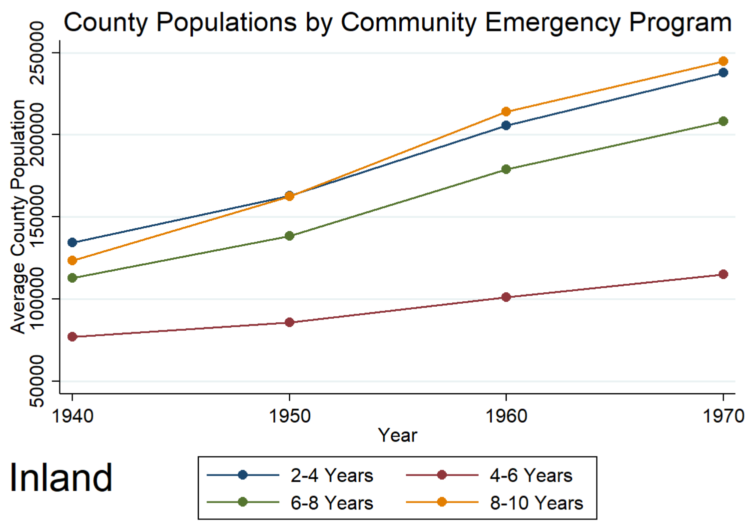

Figure A.13: Average County Population over Time for Communities in Emergency Program Bins. Inland Sample. Communities are binned by Emergency Program, either 0-2, 2-4, 4-6, 6-8, 8-10, or 10+. Average county population is found for each bin for 1940, 1950, 1960, and 1970, weighted by the number of pixels each community contributes to the bin. Note that this may result in counties showing up in more than one Emergency Program bins if a county contains communities that fall in different bins.

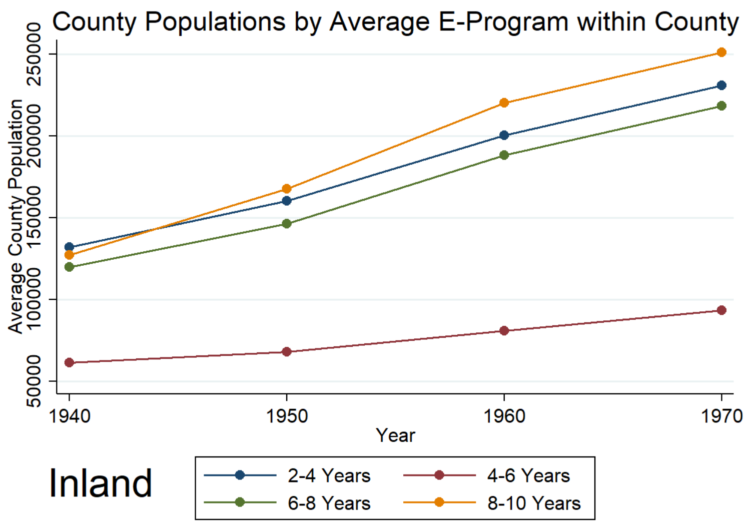

Figure A.14: County Population Over Time by Average Emergency Program Duration. Inland Sample. The figure is an alternative to Figure A.13. For the figure, I find the average Emergency Program duration for communities within a county, weighted by the number of pixels in each community. I then classify each county average into one of 6 bins. Next, I find the average county population across counties in each bin, weighted by the number of pixels each county contributes to the bin.

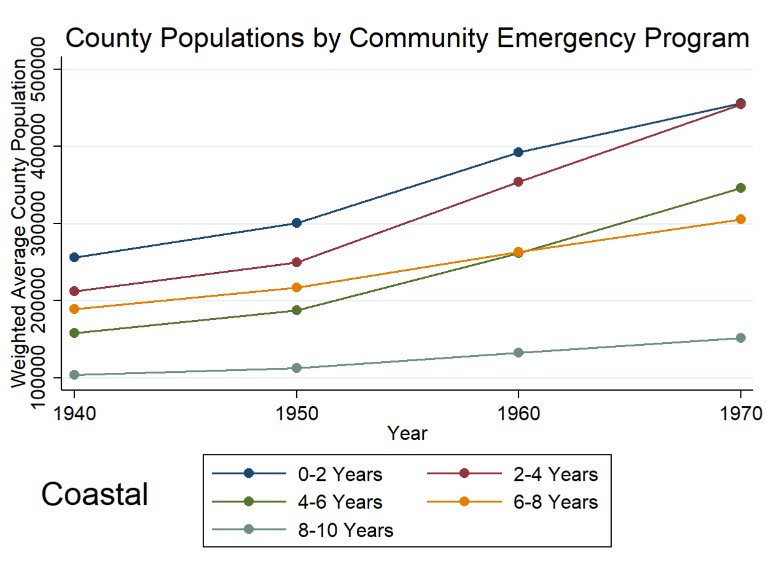

Figure A.15: Average County Population over Time for Communities in Emergency Program Bins. Coastal Sample. Communities are binned by Emergency Program, either 0-2, 2-4, 4-6, 6-8, 8-10, or 10+. Average county population is found for each bin for 1940, 1950, 1960, and 1970, weighted by the number of pixels each community contributes to the bin. Note that this may result in counties showing up in more than one Emergency Program bins if a county contains communities that fall in different bins.

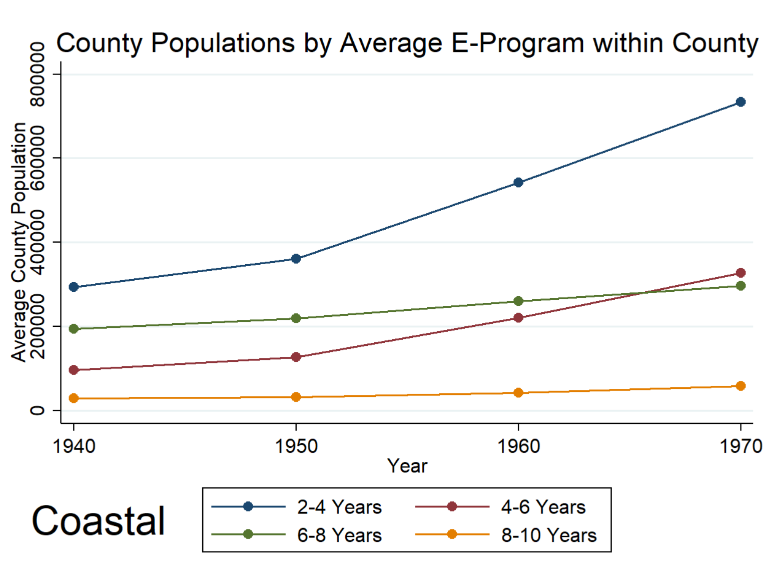

Figure A.16: County Population Over Time by Average Emergency Program Duration. Coastal Sample. The figure is an alternative to Figure A.15. For the figure, I find the average Emergency Program duration for communities within a county, weighted by the number of pixels in each community. I then classify each county average into one of 6 bins. Next, I find the average county population across counties in each bin, weighted by the number of pixels each county contributes to the bin.

First Stage Results

Table A.3 **Inland First Stage** **Equation 3**	(1) EmerYears @ 1980	(2) EmerYears @ 1986	(3) EmerYears @ 1992	(4) EmerYears @ 2000
Cohort				
1 Month Cohort	-0.000398**	-0.00162***	-0.00165***	-0.00161***
	(0.000172)	(0.000290)	(0.000292)	(0.000297)
10 Day Cohort	0.0116***	0.0151***	0.0156***	0.0153***
	(0.00124)	(0.00278)	(0.00273)	(0.00272)
5 Day Cohort	-0.0179***	-0.0198***	-0.0204***	-0.0201***
	(0.00214)	(0.00376)	(0.00370)	(0.00369)
Cohort X Time				
1 Month Cohort × γ_t	-0.000356***	-0.000436***	-0.000726***	-0.000321
	(0.0000545)	(0.0000817)	(0.000136)	(0.000238)
10 Day Cohort × γ_t	0.00289***	0.00170***	0.00225***	0.00180***
	(0.000526)	(0.000491)	(0.000523)	(0.000475)
5 Day Cohort × γ_t	-0.00195***	0.000906	0.00136	-0.000726***
	(0.000666)	(0.000876)	(0.000876)	(0.000136)
Time Dummies				
γ_{1980}	-0.414***			
	(0.0492)			
γ_{1986}		-0.559***		
		(0.0776)		
γ_{1992}			-0.632***	
			(0.0626)	
γ_{2000}				-0.606***
				(0.106)
Pixel Controls				
Dist. to Highway	-0.00000431	-0.000101***	-0.0000971***	-0.000101***
	(0.0000151)	(0.0000317)	(0.0000309)	(0.0000317)
Dist. to Water	0.0000601	0.000221***	0.000221***	0.000247***
	(0.0000388)	(0.0000844)	(0.0000826)	(0.0000852)
(Dist. to Water)^2	-1.89e-08***	-7.59e-08***	-7.46e-08***	-8.06e-08***
	(6.11e-09)	(1.51e-08)	(1.48e-08)	(1.52e-08)

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.3 (Cont) **Inland First Stage** **Equation 3**	(1) EmerYears @ 1980	(2) EmerYears @ 1986	(3) EmerYears @ 1992	(4) EmerYears @ 2000
County Controls				
Cum. Dis. Days 1960-	0.00817***	0.00587***	0.00525***	0.00450***
	(0.000746)	(0.000694)	(0.000514)	(0.000521)
County Pop 1970	0.0000390***	0.0000130**	0.0000126**	0.0000132**
	(0.00000382)	(0.00000629)	(0.00000608)	(0.00000612)
County Pop 1960	-0.0000660***	-0.0000113	-0.0000112	-0.00001000
	(0.00000809)	(0.0000134)	(0.0000123)	(0.0000125)
County Pop 1950	0.0000679***	0.0000421***	0.0000453***	0.0000412***
	(0.00000767)	(0.0000142)	(0.0000133)	(0.0000132)
County Pop 1940	-0.0000396***	-0.0000455***	-0.0000483***	-0.0000461***
	(0.00000323)	(0.00000597)	(0.00000580)	(0.00000575)
County Unemploy. 1970	-0.162***	0.0374	0.0373	0.000764
	(0.0546)	(0.143)	(0.142)	(0.138)
County Unemploy. 1960	0.0280	0.564***	0.543***	0.570***
	(0.0459)	(0.0974)	(0.0968)	(0.0985)
County Unemploy. 1950	-18.60***	-38.18***	-36.58***	-38.59***
	(3.168)	(6.627)	(6.749)	(6.417)
County Dwell. Den. 1970	-0.0170*	0.0225*	0.0290***	0.0299***
	(0.00918)	(0.0116)	(0.0111)	(0.0112)
County Dwell. Den. 1960	-0.0279	-0.102***	-0.119***	-0.122***
	(0.0230)	(0.0254)	(0.0237)	(0.0240)
County Dwell. Den. 1950	0.0434***	0.0814***	0.0930***	0.0947***
	(0.0152)	(0.0165)	(0.0151)	(0.0153)
Constant	3.108***	2.104***	2.133***	2.213***
	(0.223)	(0.346)	(0.336)	(0.326)
N	273314	273314	273314	273314
R^2	0.5204	0.4790	0.4877	0.4897
F	4.44	5.89	5.80	5.07

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.4 **Inland First Stage** **Equation 4**	(1) EmerYears @ 1980 × γ_{1980}	(2) EmerYears @ 1986 × γ_{1986}	(3) EmerYears @ 1992 × γ_{1992}	(4) EmerYears @ 2000 × γ_{2000}
Cohort X Time				
1 Month Cohort × γ_t	-0.00101***	-0.00183***	-0.00214***	-0.00173***
	(0.000144)	(0.000333)	(0.000370)	(0.000345)
10 Day Cohort × γ_t	0.00847***	0.0117***	0.0123***	0.0119***
	(0.00114)	(0.00199)	(0.00201)	(0.00187)
5 Day Cohort × γ_t	-0.0108***	-0.0152***	-0.0147***	-0.0174***
	(0.00178)	(0.00315)	(0.00305)	(0.00305)
Cohort				
1 Month Cohort	0.000253***	-0.0000364	-0.0000727	-0.0000272
	(0.0000931)	(0.000112)	(0.000110)	(0.000105)
10 Day Cohort	0.00246***	0.00204***	0.00257***	0.00219***
	(0.000477)	(0.000776)	(0.000764)	(0.000725)
5 Day Cohort	-0.00451***	-0.00177	-0.00244**	-0.00211**
	(0.000681)	(0.00115)	(0.00109)	(0.00106)
Time Dummies				
γ_{1980}	2.101***			
	(0.0734)			
γ_{1986}		3.573***		
		(0.153)		
γ_{1992}			3.498***	
			(0.131)	
γ_{2000}			3.499***	
				(0.142)
Pixel Controls				
Dist. to Highway	0.00000727	-0.0000412***	-0.0000368***	-0.0000405***
	(0.00000740)	(0.0000152)	(0.0000142)	(0.0000151)
Dist. to Water	0.0000328*	0.000113***	0.000113***	0.000142***
	(0.0000184)	(0.0000410)	(0.0000391)	(0.0000417)
(Dist. to Water)^2	-6.59e-09**	-3.67e-08***	-3.53e-08***	-4.17e-08***
	(3.06e-09)	(7.83e-09)	(7.45e-09)	(7.96e-09)

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
This table represents specifications displayed in Table 1 of the main section.

Table A.4 (Cont.) **Inland First Stage** **Equation 4**	(1) EmerYears @ 1980 × γ_{1980}	(2) EmerYears @ 1986 × γ_{1986}	(3) EmerYears @ 1992 × γ_{1992}	(4) EmerYears @ 2000 × γ_{2000}
County Controls				
Cum. Dis Days 1960-	0.0109***	0.00620***	0.00553***	0.00494***
	(0.000754)	(0.000782)	(0.000515)	(0.000522)
County Pop 1970	0.0000316***	0.0000109***	0.0000105***	0.0000115***
	(0.00000268)	(0.00000344)	(0.00000303)	(0.00000305)
County Pop 1960	-0.0000683***	-0.0000235***	-0.0000233***	-0.0000234***
	(0.00000583)	(0.00000859)	(0.00000671)	(0.00000693)
County Pop 1950	0.0000626***	0.0000406***	0.0000438***	0.0000411***
	(0.00000493)	(0.00000910)	(0.00000734)	(0.00000726)
County Pop 1940	-0.0000254***	-0.0000289***	-0.0000318***	-0.0000300***
	(0.00000187)	(0.00000348)	(0.00000302)	(0.00000295)
County Unemploy. 1970	-0.0795**	0.0264	0.0262	-0.0132
	(0.0341)	(0.0729)	(0.0715)	(0.0673)
County Unemploy. 1960	0.0615**	0.325***	0.303***	0.335***
	(0.0282)	(0.0495)	(0.0488)	(0.0500)
County Unemploy. 1950	-8.430***	-19.02***	-17.34***	-19.47***
	(1.804)	(3.285)	(3.376)	(3.069)
County Dwell. Density 1970	-0.0239***	0.00879*	0.0157***	0.0168***
	(0.00387)	(0.00504)	(0.00465)	(0.00463)
County Dwell. Density 1960	0.0423***	-0.0324***	-0.0511***	-0.0530***
	(0.0103)	(0.0114)	(0.00977)	(0.00989)
County Dwell. Density 1950	-0.0209***	0.0241***	0.0365***	0.0375***
	(0.00718)	(0.00778)	(0.00635)	(0.00647)
Constant	-0.148	-1.304***	-1.272***	-1.204***
	(0.109)	(0.157)	(0.156)	(0.153)
N	273314	273314	273314	273314
R^2	0.8933	0.8692	0.8731	0.8761
F	4.75	5.33	6.31	7.37

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
This table represents specifications displayed in Table 1 of the main section.

Table A.5 **Coastal First Stage** **Equation 3**	(1) EmerYears @ 1980	(2) EmerYears @ 1986	(3) EmerYears @ 1992	(4) EmerYears @ 2000
Cohort				
1 Month Cohort	-0.000398**	-0.00162***	-0.00165***	-0.00161***
	(0.000172)	(0.000290)	(0.000292)	(0.000297)
10 Day Cohort	0.0116***	0.0151***	0.0156***	0.0153***
	(0.00124)	(0.00278)	(0.00273)	(0.00272)
5 Day Cohort	-0.0179***	-0.0198***	-0.0204***	-0.0201***
	(0.00214)	(0.00376)	(0.00370)	(0.00369)
Cohort X Time				
1 Month Cohort × γ_t	-0.000356***	-0.000436***	-0.000726***	-0.000321
	(0.0000545)	(0.0000817)	(0.000136)	(0.000238)
10 Day Cohort × γ_t	0.00289***	0.00170***	0.00225***	0.00180***
	(0.000526)	(0.000491)	(0.000523)	(0.000475)
5 Day Cohort × γ_t	-0.00195***	0.000906	0.00136	-0.00118
	(0.000666)	(0.000876)	(0.000876)	(0.00163)
Time Dummies				
γ_{1980}	-0.414***			
	(0.0492)			
γ_{1986}		-0.559***		
		(0.0776)		
γ_{1992}			-0.632***	
			(0.0626)	
γ_{2000}				-0.606***
				(0.106)
Pixel Controls				
Dist. to Highway	-0.00000431	-0.000101***	-0.0000971***	-0.000101***
	(0.0000151)	(0.0000317)	(0.0000309)	(0.0000317)
Dist. to Water	0.0000601	0.000221***	0.000221***	0.000247***
	(0.0000388)	(0.0000844)	(0.0000826)	(0.0000852)
(Dist. to Water)^2	-1.89e-08***	-7.59e-08***	-7.46e-08***	-8.06e-08***
	(6.11e-09)	(1.51e-08)	(1.48e-08)	(1.52e-08)

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.5 (Cont)	(1)	(2)	(3)	(4)
Coastal First Stage	EmerYears @ 1980	EmerYears @ 1986	EmerYears @ 1992	EmerYears @ 2000
Equation 3				
County Controls				
Cum. Dis. Days 1960-	0.00817***	0.00587***	0.00525***	0.00450***
	(0.000746)	(0.000694)	(0.000514)	(0.000521)
County Pop 1970	0.0000390***	0.0000130**	0.0000126**	0.0000132**
	(0.00000382)	(0.00000629)	(0.00000608)	(0.00000612)
County Pop 1960	-0.0000660***	-0.0000113	-0.0000112	-0.00001000
	(0.00000809)	(0.0000134)	(0.0000123)	(0.0000125)
County Pop 1950	0.0000679***	0.0000421***	0.0000453***	0.0000412***
	(0.00000767)	(0.0000142)	(0.0000133)	(0.0000132)
County Pop 1940	-0.0000396***	-0.0000455***	-0.0000483***	-0.0000461***
	(0.00000323)	(0.00000597)	(0.00000580)	(0.00000575)
County Unemploy. 1970	-0.162***	0.0374	0.0373	0.000764
	(0.0546)	(0.143)	(0.142)	(0.138)
County Unemploy. 1960	0.0280	0.564***	0.543***	0.570***
	(0.0459)	(0.0974)	(0.0968)	(0.0985)
County Unemploy. 1950	-18.60***	-38.18***	-36.58***	-38.59***
	(3.168)	(6.627)	(6.749)	(6.417)
County Dwell. Den. 1970	-0.0170*	0.0225*	0.0290***	0.0299***
	(0.00918)	(0.0116)	(0.0111)	(0.0112)
County Dwell. Den. 1960	-0.0279	-0.102***	-0.119***	-0.122***
	(0.0230)	(0.0254)	(0.0237)	(0.0240)
County Dwell. Den. 1950	0.0434***	0.0814***	0.0930***	0.0947***
	(0.0152)	(0.0165)	(0.0151)	(0.0153)
Constant	3.108***	2.104***	2.133***	2.213***
	(0.223)	(0.346)	(0.336)	(0.326)
N	174750	174750	174750	174750
R^2	0.3174	0.3874	0.3875	0.3874
F	0.93	1.23	1.09	1.02

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.6	(1)	(2)	(3)	(4)
Coastal First Stage	EmerYears	EmerYears	EmerYears	EmerYears
Equation 4	@ 1980 × γ_{1980}	@ 1986 × γ_{1986}	@ 1992 × γ_{1992}	@ 2000 × γ_{2000}
Cohort X Time				
1 Month Cohort × γ_t	-0.00101***	-0.00183***	-0.00214***	-0.00173***
	(0.000144)	(0.000333)	(0.000370)	(0.000345)
10 Day Cohort × γ_t	0.00847***	0.0117***	0.0123***	0.0119***
	(0.00114)	(0.00199)	(0.00201)	(0.00187)
5 Day Cohort × γ_t	-0.0108***	-0.0152***	-0.0147***	-0.0174***
	(0.00178)	(0.00315)	(0.00305)	(0.00305)
Cohort				
1 Month Cohort	0.000253***	-0.0000364	-0.0000727	-0.0000272
	(0.0000931)	(0.000112)	(0.000110)	(0.000105)
10 Day Cohort	0.00246***	0.00204***	0.00257***	0.00219***
	(0.000477)	(0.000776)	(0.000764)	(0.000725)
5 Day Cohort	0.000455*	-0.00180***	-0.00171***	-0.00158***
	(0.000681)	(0.00115)	(0.00109)	(0.00106)
Time Dummies				
γ_{1980}	2.101***			
	(0.0734)			
γ_{1986}		3.573***		
		(0.153)		
γ_{1992}			3.498***	
			(0.131)	
γ_{2000}				3.499***
				(0.142)
Pixel Controls				
Dist. to Highway	0.00000727	-0.0000412***	-0.0000368***	-0.0000405***
	(0.00000740)	(0.0000152)	(0.0000142)	(0.0000151)
Dist. to Water	0.0000328*	0.000113***	0.000113***	0.000142***
	(0.0000184)	(0.0000410)	(0.0000391)	(0.0000417)
(Dist. to Water)^2	-6.59e-09**	-3.67e-08***	-3.53e-08***	-4.17e-08***
	(3.06e-09)	(7.83e-09)	(7.45e-09)	(7.96e-09)

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
Columns 1-4 represent specifications displayed in Table 2 of the main section.

Table A.6 (Cont)	(1)	(2)	(3)	(4)
Coastal First Stage	EmerYears	EmerYears	EmerYears	EmerYears
Equation 4	@ 1980 × γ_{1980}	@ 1986 × γ_{1986}	@ 1992 × γ_{1992}	@ 2000 × γ_{2000}
County Controls				
Cum. Dis. Days 1960-	0.0109***	0.00620***	0.00553***	0.00494***
	(0.000754)	(0.000782)	(0.000515)	(0.000522)
County Pop 1970	0.0000316***	0.0000109***	0.0000105***	0.0000115***
	(0.00000268)	(0.00000344)	(0.00000303)	(0.00000305)
County Pop 1960	-0.0000683***	-0.0000235***	-0.0000233***	-0.0000234***
	(0.00000583)	(0.00000859)	(0.00000671)	(0.00000693)
County Pop 1950	0.0000626***	0.0000406***	0.0000438***	0.0000411***
	(0.00000493)	(0.00000910)	(0.00000734)	(0.00000726)
County Pop 1940	-0.0000254***	-0.0000289***	-0.0000318***	-0.0000300***
	(0.00000187)	(0.00000348)	(0.00000302)	(0.00000295)
County Unemploy. 1970	-0.0795**	0.0264	0.0262	-0.0132
	(0.0341)	(0.0729)	(0.0715)	(0.0673)
County Unemploy. 1960	0.0615**	0.325***	0.303***	0.335***
	(0.0282)	(0.0495)	(0.0488)	(0.0500)
County Unemploy. 1950	-8.430***	-19.02***	-17.34***	-19.47***
	(1.804)	(3.285)	(3.376)	(3.069)
County Dwell. Den. 1970	-0.0239***	0.00879*	0.0157***	0.0168***
	(0.00387)	(0.00504)	(0.00465)	(0.00463)
County Dwell. Den. 1960	0.0423***	-0.0324***	-0.0511***	-0.0530***
	(0.0103)	(0.0114)	(0.00977)	(0.00989)
County Dwell. Den. 1950	-0.0209***	0.0241***	0.0365***	0.0375***
	(0.00718)	(0.00778)	(0.00635)	(0.00647)
Constant	-0.148	-1.304***	-1.272***	-1.204***
	(0.109)	(0.157)	(0.156)	(0.153)
N	174,750	174,750	174,750	174,750
R^2	0.8798	0.8849	0.8847	0.8847
F	1.05	0.89	0.91	1.02

Standard Errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
Columns 1-4 represent specifications displayed in Table 2 of the main section.

Second Stage and OLS Results

Table A.7 **Inland OLS** **Community F.E Spec.**	(1) P(Dev) @ 1980	(2) P(Dev) @ 1986	(3) P(Dev) @ 1992	(4) P(Dev) @ 2000	(5) P(Dev) Pooled
Emer.Prog. Years					
Emer Year$\times$ γ_{1980}	0.00573				0.00616
	(0.00431)				(0.00480)
Emer Year $\times$ γ_{1986}		0.00852**			0.00778*
		(0.00400)			(0.00418)
Emer Year $\times$ γ_{1992}			0.0184*		0.0165*
			(0.00971)		(0.00924)
Emer Year $\times$ γ_{2000}				0.0249*	0.0241*
				(0.0141)	(0.0133)
Time Dummies					
γ_{1980}	0.00361				0.000480
	(0.00761)				(0.0124)
γ_{1986}		-0.000555			0.00219
		(0.0116)			(0.0108)
γ_{1992}			-0.0141		-0.0125
			(0.0213)		(0.0208)
γ_{2000}				-0.0222	-0.0196
				(0.0301)	(0.0302)
Pixel Controls					
Dist. to Highway	-0.0000142	-0.0000149	-0.0000158	-0.0000168	-0.0000161
	(0.0000106)	(0.0000109)	(0.0000112)	(0.0000115)	(0.0000118)
Dist. to Water	-0.0000325	-0.0000369*	-0.0000403**	-0.0000469**	-0.0000452**
	(0.0000203)	(0.0000200)	(0.0000203)	(0.0000200)	(0.0000205)
(Dist. to Water)^2	1.59e-09	1.96e-09	2.44e-09	3.20e-09	2.79e-09
	(2.72e-09)	(2.68e-09)	(2.69e-09)	(2.66e-09)	(2.69e-09)
Country Controls					
Cum. Dis. Days 1960-	-0.000140	-0.0000852	-0.000141	-0.0000870	-0.0000848
	(0.000171)	(0.0000663)	(0.000105)	(0.000123)	(0.0000913)
County Pop 1970	-0.00000413	-0.00000454*	-0.00000426	-0.00000435*	-0.00000439*
	(0.00000252)	(0.00000258)	(0.00000257)	(0.00000251)	(0.00000259)
County Pop 1960	-0.00000167	-0.00000126	-0.00000432	-0.00000334	-0.00000306
	(0.00000525)	(0.00000559)	(0.00000557)	(0.00000553)	(0.00000570)
County Pop 1950	0.0000177***	0.0000186***	0.0000223***	0.0000204***	0.0000206***
	(0.00000558)	(0.00000637)	(0.00000653)	(0.00000654)	(0.00000664)

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 1 and 2 of Table 3 of the main section.

Table A.7 (Cont)	(1)	(2)	(3)	(4)	(5)
Inland OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Community F.E Spec.					Pooled
County Controls (Cont)					
County Pop 1940	-0.00000861***	-0.00000955**	-0.0000102***	-0.00000923**	-0.00000976**
	(0.00000323)	(0.00000373)	(0.00000369)	(0.00000367)	(0.00000378)
County Unemploy. 1970	0.0298	0.0290	0.0467	0.0415	0.0392
	(0.0396)	(0.0430)	(0.0433)	(0.0423)	(0.0440)
County Unemploy. 1960	0.0377	0.0511	0.0524	0.0502	0.0538
	(0.0354)	(0.0427)	(0.0411)	(0.0404)	(0.0424)
County Unemploy. 1950	-2.039	-2.249	-3.029	-2.885	-2.833
	(1.715)	(1.980)	(2.073)	(2.048)	(2.096)
County Dwell. Den. 1970	-0.00181	-0.000288	-0.00480	-0.00470	-0.00340
	(0.00440)	(0.00451)	(0.00430)	(0.00416)	(0.00435)
County Dwell. Den. 1960	0.00107	-0.00154	0.0106	0.0103	0.00691
	(0.0118)	(0.0121)	(0.0115)	(0.0112)	(0.0118)
County Dwell. Den. 1950	-0.000319	0.000904	-0.00725	-0.00699	-0.00478
	(0.00781)	(0.00810)	(0.00769)	(0.00748)	(0.00784)
Constant	-0.234	-0.291	-0.340	-0.311	-0.323
	(0.227)	(0.260)	(0.256)	(0.252)	(0.262)
Fixed Effects	Comm.	Comm.	Comm.	Comm.	Comm.
N	273314	273314	273314	273314	683285
R^2	0.309	0.312	0.309	0.307	0.307

Standard Errors clustered at community level.

Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.

This table represents specifications displayed in columns 1 and 2 of Table 3 of the main section.

Table A.8	(1)	(2)	(3)	(4)	(5)
Inland OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Emer. Years Spec.					Pooled
Emer. Prog. Years					
Emer Year × γ_{1980}	0.0170*				0.00725*
	(0.00864)				(0.00430)
Emer Year × γ_{1986}		0.00835**			0.00783*
		(0.00400)			(0.00409)
Emer Year × γ_{1992}			0.0187*		0.0166*
			(0.00967)		(0.00915)
Emer Year × γ_{2000}				0.0235*	0.0243*
				(0.0138)	(0.0133)
Time Dummies					
γ_{1980}	-0.00759				-0.00159
	(0.0118)				(0.0114)
γ_{1986}		0.000620			0.00320
		(0.0114)			(0.0109)
γ_{1992}			-0.0123		-0.0114
			(0.0214)		(0.0209)
γ_{2000}				-0.0227	-0.0182
				(0.0310)	(0.0306)
Community Controls					
Emer Years (1973-1980)	-0.0126				0.0505
	(0.0307)				(0.0415)
Emer Years (1973-1986)		-0.0372**	-0.0418**	-0.0458**	-0.0669***
		(0.0179)	(0.0188)	(0.0196)	(0.0237)
Pixel Controls					
Dist. to Highway	-0.0000257**	-0.0000260**	-0.0000269**	-0.0000281**	-0.0000270**
	(0.0000122)	(0.0000125)	(0.0000127)	(0.0000129)	(0.0000130)
Dist. to Water	-0.0000503***	-0.0000511***	-0.0000551***	-0.0000627***	-0.0000616***
	(0.0000170)	(0.0000174)	(0.0000177)	(0.0000175)	(0.0000172)
(Dist. to Water)^2	4.67e-09	4.02e-09	4.62e-09*	5.55e-09**	5.47e-09*
	(0.116)	(0.136)	(0.093)	(0.046)	(0.052)
County Controls					
Cum. Dis. Days 1960-	-0.000579**	-0.0000924	-0.000171	-0.0000410	-0.000101
	(0.000256)	(0.0000675)	(0.000105)	(0.000119)	(0.0000959)
County Pop 1970	-0.00000125	-0.000000991	-0.000000658	-0.000000412	-0.00000134
	(0.00000297)	(0.00000292)	(0.00000318)	(0.00000335)	(0.00000370)

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 3 and 4 of Table 3 of the main section.

Table A.8 (Cont) **Inland OLS** **Emer. Years Spec.**	(1) P(Dev) @ 1980	(2) P(Dev) @ 1986	(3) P(Dev) @ 1992	(4) P(Dev) @ 2000	(5) P(Dev) Pooled
County Controls (Cont)					
County Pop 1960	-1.86e-08	-0.000000947	-0.00000151	-0.00000185	0.00000113
	(0.00000735)	(0.00000683)	(0.00000751)	(0.00000788)	(0.00000895)
County Pop 1950	-0.00000117	0.00000269	0.00000404	0.00000501	0.00000143
	(0.00000737)	(0.00000658)	(0.00000689)	(0.00000716)	(0.00000824)
County Pop 1940	0.00000217	-0.00000106	-0.00000225	-0.00000319	-0.00000160
	(0.00000332)	(0.00000317)	(0.00000318)	(0.00000325)	(0.00000357)
County Unemploy. 1970	0.0294	0.00438	0.00604	0.00265	0.00459
	(0.0345)	(0.0321)	(0.0318)	(0.0326)	(0.0332)
County Unemploy. 1960	0.0616***	0.0707***	0.0738***	0.0758***	0.0879***
	(0.0217)	(0.0200)	(0.0206)	(0.0207)	(0.0231)
County Unemploy. 1950	-4.132**	-3.749**	-3.712**	-3.331**	-4.758**
	(1.945)	(1.474)	(1.491)	(1.472)	(1.971)
County Dwell. Den. 1970	0.0108**	0.0142**	0.0137**	0.0134**	0.0163**
	(0.00503)	(0.00566)	(0.00600)	(0.00620)	(0.00676)
County Dwell. Den. 1960	-0.0107	-0.0201	-0.0196	-0.0197	-0.0268
	(0.0150)	(0.0163)	(0.0174)	(0.0179)	(0.0194)
County Dwell. Den. 1950	0.00107	0.00741	0.00741	0.00769	0.0122
	(0.0106)	(0.0113)	(0.0121)	(0.0124)	(0.0134)
Constant	0.519**	0.622***	0.609***	0.605**	0.567**
	(0.231)	(0.224)	(0.223)	(0.237)	(0.246)
Fixed Effects	State	State	State	State	State
N	273314	273314	273314	273314	683285
R^2	0.214	0.221	0.220	0.220	0.219

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 3 and 4 of Table 3 of the main section.

Table A.9	(1)	(2)	(3)	(4)	(5)
Inland OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Pixel F.E. Spec.					Pooled
Emer. Prog. Years					
Emer Year × γ_{1980}	0.00358				0.00725*
	(0.00328)				(0.00409)
Emer Year × γ_{1986}		0.00795**			0.00740*
		(0.00394)			(0.00409)
Emer Year × γ_{1992}			0.0181*		0.0160*
			(0.00970)		(0.00926)
Emer Year × γ_{2000}				0.0245*	0.0236*
				(0.0141)	(0.0134)
Time Dummies					
γ_{1980}	0.00323				-0.00286
	(0.00607)				(0.0109)
γ_{1986}		0.000330			0.00229
		(0.0114)			(0.0105)
γ_{1992}			-0.0129		-0.0126
			(0.0213)		(0.0206)
γ_{2000}				-0.0213	-0.0199
				(0.0302)	(0.0302)
Primary Controls					
Cum. Dis. Days 1960-	0.0000105	-0.0000684	-0.000141	-0.0000817	-0.0000668
	(0.0000928)	(0.0000650)	(0.000105)	(0.000123)	(0.0000908)
Constant	0.138***	0.140***	0.143***	0.141***	0.140***
	(0.00334)	(0.00261)	(0.00400)	(0.00620)	(0.00488)
Fixed Effects	Pixel	Pixel	Pixel	Pixel	Pixel
N	273314	273314	273314	273314	683285
R^2	0.013	0.030	0.056	0.077	0.049

Standard Errors clustered at community level
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$
This table represents specifications displayed in columns 5 and 6 of Table 3 of the main section.

Table A.10 **Inland IV Second Stage**	(1) P(Dev) @ 1980	(2) P(Dev) @ 1986	(3) P(Dev) @ 1992	(4) P(Dev) @ 2000
Emer. Prog. Years				
Emer Year × γ_{1980}	-0.00343			
	(0.0103)			
Emer Year × γ_{1986}		0.00721		
		(0.00620)		
Emer Year × γ_{1992}			0.00738	
			(0.00635)	
Emer Year × γ_{2000}				0.0184***
				(0.00679)
Time Dummies				
γ_{1980}	0.0383			
	(0.0319)			
γ_{1986}		0.0341		
		(0.0232)		
γ_{1992}			0.0496*	
			(0.0258)	
γ_{2000}				0.0259
				(0.0270)
Community Controls				
Emer Years (1973-1980)	-0.0262			
	(0.0245)			
Emer Years (1973-1986)		0.00565	0.0469***	0.00181
		(0.0153)	(0.0149)	(0.0153)
Pixel Controls				
Dist. to Highway	-0.0000161***	-0.0000161**	-0.0000174**	-0.0000172**
	(0.00000620)	(0.00000689)	(0.00000686)	(0.00000729)
Dist. to Water	-0.0000655***	-0.0000724***	-0.0000749***	-0.0000854***
	(0.0000152)	(0.0000161)	(0.0000158)	(0.0000166)
(Dist. to Water)^2	9.95e-09***	1.17e-08***	1.18e-08***	1.37e-08***
	(3.35e-09)	(3.64e-09)	(3.54e-09)	(3.71e-09)

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 7 and 8 of Table 3 of the main section.

Table A.10 (Cont)	(1)	(2)	(3)	(4)
Inland IV Second Stage	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000
County Controls				
Cum. Dis. Days 1960-	-0.000463	-0.000484***	-0.000367***	-0.000272**
	(0.000335)	(0.000170)	(0.000111)	(0.000108)
County Pop 1970	0.00000264	0.00000268*	0.00000349**	0.00000433***
	(0.00000168)	(0.00000148)	(0.00000147)	(0.00000151)
County Pop 1960	-0.000000543	-0.00000117	-0.00000323	-0.00000550*
	(0.00000326)	(0.00000283)	(0.00000278)	(0.00000285)
County Pop 1950	-0.00000561**	-0.00000576**	-0.00000285	-0.000000368
	(0.00000285)	(0.00000271)	(0.00000257)	(0.00000258)
County Pop 1940	0.00000357***	0.00000427***	0.00000251**	0.00000143
	(0.00000126)	(0.00000134)	(0.00000125)	(0.00000125)
County Unemploy. 1970	0.0178	0.0236	0.0242	0.0255
	(0.0164)	(0.0162)	(0.0163)	(0.0167)
County Unemploy. 1960	0.0341**	0.0291	0.0384*	0.0352*
	(0.0166)	(0.0203)	(0.0202)	(0.0209)
County Unemploy. 1950	-2.298*	-1.563	-1.789	-1.339
	(1.212)	(1.409)	(1.389)	(1.435)
County Dwell. Dens. 1970	0.0179***	0.0172***	0.0163***	0.0152***
	(0.00220)	(0.00215)	(0.00213)	(0.00217)
County Dwell. Dens. 1960	-0.0383***	-0.0342***	-0.0319***	-0.0289***
	(0.00568)	(0.00530)	(0.00525)	(0.00531)
County Dwell. Dens. 1950	0.0223***	0.0190***	0.0175***	0.0155***
	(0.00401)	(0.00362)	(0.00357)	(0.00358)
Constant	0.0424	-0.0610	-0.0840*	-0.0896*
	(0.0742)	(0.0481)	(0.0501)	(0.0509)
Fixed Effects	State	State	State	State
N	273314	273314	273314	273314
R^2	0.138	0.141	0.147	0.150
Hansen's J	0.3826	0.3564	0.2005	0.2081
Anderson-Rubin Wald Test	0.2709	0.2554	0.2188	0.2328
Stock-Wright LM Test	0.0124	0.0089	0.0014	0.0052

Standard Errors clustered at community level
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 7 and 8 of Table 3 of the main section.

Table A.11	(1)	(2)	(3)	(4)	(5)
Coastal OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Community F.E Spec.					Pooled
Emer. Prog. Years					
Emer Year × γ_{1980}	-0.000162				-0.00505*
	(0.00208)				(0.00299)
Emer Year × γ_{1986}		-0.0118			-0.00958
		(0.0111)			(0.00932)
Emer Year × γ_{1992}			-0.0208		-0.0179
			(0.0197)		(0.0173)
Emer Year × γ_{2000}				-0.0249	-0.0236
				(0.0223)	(0.0213)
Time Dummies					
γ_{1980}	0.0134				0.0162**
	(0.00967)				(0.00736)
γ_{1986}		0.0748			0.0582
		(0.0572)			(0.0452)
γ_{1992}			0.138		0.107
			(0.111)		(0.0865)
γ_{2000}				0.136	0.140
				(0.101)	(0.102)
Pixel Controls					
Dist. to Highway	-0.0000609***	-0.0000630***	-0.0000609***	-0.0000592***	-0.0000611***
	(0.0000120)	(0.0000119)	(0.0000119)	(0.0000123)	(0.0000120)
Dist. to Water	-0.0000967**	-0.0000976***	-0.000108***	-0.000104***	-0.000103***
	(0.0000379)	(0.0000364)	(0.0000349)	(0.0000351)	(0.0000350)
(Dist. to Water)^2	3.25e-08**	3.23e-08**	3.45e-08***	3.39e-08***	3.33e-08***
	(1.33e-08)	(1.26e-08)	(1.24e-08)	(1.27e-08)	(1.25e-08)
County Controls					
Cum. Dis. Days 1960-	-0.000324	0.000368	0.000112	0.000839	0.000640
	(0.000371)	(0.000501)	(0.000286)	(0.000700)	(0.000551)
County Pop 1970	0.00000125	0.00000135	0.00000133	0.00000174*	0.00000162*
	(0.000000900)	(0.000000949)	(0.000000952)	(0.000000954)	(0.000000973)
County Pop 1960	-0.00000183	-0.00000219	-0.00000202	-0.00000360	-0.00000289
	(0.00000292)	(0.00000300)	(0.00000302)	(0.00000292)	(0.00000303)
County Pop 1950	0.000000187	0.000000904	0.000000447	0.00000278	0.00000131
	(0.00000648)	(0.00000653)	(0.00000663)	(0.00000618)	(0.00000653)

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 1 and 2 of Table 4 of the main section.

Table A.11 (Cont)	(1)	(2)	(3)	(4)	(5)
Coastal OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Community F.E Spec.					Pooled
County Controls (Cont)					
County Pop 1940	0.000000271	-0.000000205	0.000000104	-0.00000100	-0.000000148
	(0.00000431)	(0.00000435)	(0.00000444)	(0.00000416)	(0.00000440)
County Unemploy. 1970	-0.0674	-0.0458	-0.0664	-0.00382	-0.0382
	(0.125)	(0.129)	(0.130)	(0.128)	(0.132)
County Unemploy. 1960	-0.0335	-0.0374	-0.0359	-0.0442	-0.0363
	(0.0473)	(0.0480)	(0.0495)	(0.0480)	(0.0503)
County Unemploy. 1950	7.702	6.984	7.262	4.257	6.219
	(7.241)	(7.319)	(7.413)	(7.004)	(7.286)
County Dwell. Den. 1970	0.000281	0.000313	0.000304	0.000441	0.000505
	(0.000440)	(0.000440)	(0.000439)	(0.000423)	(0.000427)
County Dwell. Den. 1960	-0.0000395	-0.000504	-0.000246	-0.000558	-0.000438
	(0.00306)	(0.00312)	(0.00321)	(0.00312)	(0.00325)
County Dwell. Den. 1950	-0.000544	0.0000421	-0.000289	-0.0000143	-0.000265
	(0.00438)	(0.00447)	(0.00461)	(0.00447)	(0.00467)
Constant	0.388**	0.326	0.372*	0.295	0.326
	(0.196)	(0.213)	(0.211)	(0.219)	(0.229)
Fixed Effects	Comm.	Comm.	Comm.	Comm.	Comm.
N	174750	174750	174750	174750	436875
R^2	0.336	0.327	0.324	0.324	0.324

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 1 and 2 of Table 4 of the main section.

Table A.12 **Coastal OLS** **Emer. Years Spec.**	(1) P(Dev) @ 1980	(2) P(Dev) @ 1986	(3) P(Dev) @ 1992	(4) P(Dev) @ 2000	(5) P(Dev) Pooled
Emer. Prog. Years					
Emer Year × γ_{1980}	-0.00430				-0.00531*
	(0.00570)				(0.00315)
Emer Year × γ_{1986}		-0.0116			-0.00921
		(0.0111)			(0.00916)
Emer Year × γ_{1992}			-0.0197		-0.0173
			(0.0195)		(0.0173)
Emer Year × γ_{2000}				-0.0241	-0.0226
				(0.0222)	(0.0210)
Time Dummies					
γ_{1980}	0.00742				0.0131
	(0.0104)				(0.00857)
γ_{1986}		0.0693			0.0483
		(0.0588)			(0.0463)
γ_{1992}			0.113		0.0918
			(0.111)		(0.0891)
γ_{2000}				0.119	0.117
				(0.104)	(0.104)
Community Controls					
Emer Years (1973-1980)	0.0514				0.0363
	(0.0537)				(0.0635)
Emer Years (1973-1986)		0.0246*	0.0298*	0.0326**	0.0235
		(0.0146)	(0.0158)	(0.0164)	(0.0209)
Pixel Controls					
Dist. to Highway	-0.0000561***	-0.0000555***	-0.0000554***	-0.0000558***	-0.0000584***
	(0.0000102)	(0.0000103)	(0.0000102)	(0.0000102)	(0.00000965)
Dist. to Water	-0.0000609	-0.0000669	-0.0000736*	-0.0000691	-0.0000663
	(0.0000429)	(0.0000448)	(0.0000439)	(0.0000438)	(0.0000421)
(Dist. to Water)^2	3.15e-10	1.02e-09	2.70e-09	2.02e-09	2.33e-09
	(1.40e-08)	(1.41e-08)	(1.39e-08)	(1.41e-08)	(1.41e-08)
County Controls					
Cum. Dis. Days 1960-	0.00131	0.000564	0.000698	0.00111	0.00101
	(0.00165)	(0.000705)	(0.000626)	(0.000684)	(0.000612)
County Pop 1970	-0.00000380**	-0.00000304*	-0.00000250	-0.00000210	-0.0000463
	(0.00000146)	(0.00000157)	(0.00000152)	(0.00000147)	(0.0000742)

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 3 and 4 of Table 4 of the main section.

Table A.12 (Cont)	(1)	(2)	(3)	(4)	(5)
Coastal OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Emer. Years Spec.					Pooled
County Controls (Cont)					
County Pop 1960	0.00000564*	0.00000394	0.00000304	0.00000223	0.00000242
	(0.00000338)	(0.00000353)	(0.00000343)	(0.00000335)	(0.00000337)
County Pop 1950	0.00000135	0.00000371	0.00000394	0.00000495	0.00000340
	(0.00000367)	(0.00000336)	(0.00000325)	(0.00000324)	(0.00000431)
County Pop 1940	-0.00000307	-0.00000468***	-0.00000459***	-0.00000525***	-0.00000376
	(0.00000217)	(0.00000177)	(0.00000169)	(0.00000170)	(0.00000283)
County Unemploy. 1970	-0.0443	-0.0300	-0.0296	-0.0447	-0.0416
	(0.0603)	(0.0656)	(0.0642)	(0.0635)	(0.0629)
County Unemploy. 1960	-0.0106	0.000333	0.00261	0.00446	0.00382
	(0.0309)	(0.0345)	(0.0337)	(0.0332)	(0.0343)
County Unemploy. 1950	2.733	1.728	2.065	3.251	3.121
	(2.351)	(2.473)	(2.450)	(2.431)	(2.230)
County Dwell. Den. 1970	-0.00147**	-0.00140**	-0.00126**	-0.00134**	-0.00116*
	(0.000592)	(0.000564)	(0.000539)	(0.000531)	(0.000595)
County Dwell. Den. 1960	0.00167	0.00171	0.00173	0.00188	0.00176
	(0.00153)	(0.00159)	(0.00155)	(0.00153)	(0.00148)
County Dwell. Den. 1950	-0.000163	-0.000264	-0.000426	-0.000507	-0.000598
	(0.00130)	(0.00139)	(0.00135)	(0.00133)	(0.00125)
Constant	0.178	0.169	0.0965	0.0483	-0.0225
	(0.215)	(0.141)	(0.140)	(0.136)	(0.197)
Fixed Effects	State	State	State	State	State
N	174750	174750	174750	174750	436875
R^2	0.168	0.165	0.171	0.178	0.177

Standard Errors clustered at community level.
Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$.
This table represents specifications displayed in columns 3 and 4 of Table 4 of the main section.

Table A.13	(1)	(2)	(3)	(4)	(5)
Coastal OLS	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000	P(Dev)
Pixel F.E. Spec.					Pooled
Emer. Prog. Years					
Emer Year × γ_{1980}	0.000330				-0.00474
	(0.00193)				(0.00300)
Emer Year × γ_{1986}		-0.0118			-0.00955
		(0.0111)			(0.00931)
Emer Year × γ_{1992}			-0.0208		-0.0179
			(0.0197)		(0.0173)
Emer Year × γ_{2000}				-0.0249	-0.0236
				(0.0223)	(0.0212)
Time Dummies					
γ_{1980}	0.0121				0.0154**
	(0.00954)				(0.00739)
γ_{1986}		0.0747			0.0580
		(0.0572)			(0.0451)
γ_{1992}			0.138		0.107
			(0.111)		(0.0864)
γ_{2000}				0.136	0.140
				(0.101)	(0.102)
Primary Controls					
Cum. Dis. Days 1960-	-0.000318	0.000369	0.000107	0.000840	0.000643
	(0.000372)	(0.000502)	(0.000284)	(0.000700)	(0.000552)
Constant	0.233***	0.214***	0.221***	0.201***	0.206***
	(0.00911)	(0.0189)	(0.0148)	(0.0311)	(0.0270)
Fixed Effects	Pixel	Pixel	Pixel	Pixel	Pixel
N	174,750	174,750	174,750	174,750	436875
R^2	0.010	0.039	0.063	0.096	0.058

Standard Errors clustered at community level

Standard Errors in parentheses, * $p < .1$, ** $p < .05$, *** $p < .01$

This table represents specifications displayed in columns 5 and 6 of Table 4 of the main section.

Table A.14 **Coastal IV Second Stage**	(1) P(Dev) @ 1980	(2) P(Dev) @ 1986	(3) P(Dev) @ 1992	(4) P(Dev) @ 2000
Emer. Prog. Years				
Emer Years × γ_{1980}	0.00624			
	(0.0284)			
Emer Years × γ_{1986}		0.0231		
		(0.0195)		
Emer Years × γ_{1992}			0.0418	
			(0.0342)	
Emer Years × γ_{2000}				0.0495
				(0.0452)
Time Dummies				
γ_{1980}	0.0122			
	(0.0796)			
γ_{1986}		0.0790		
		(0.0950)		
γ_{1992}			0.166	
			(0.159)	
γ_{2000}				0.223
				(0.215)
Community Controls				
Emer Years (1973-1980)	0.105			
	(0.0753)			
Emer Years (1973-1986)		-0.154***	-0.167***	-0.162***
		(0.0447)	(0.0465)	(0.0470)
Pixel Controls				
Dist. to Highway	-0.0000376***	-0.0000437***	-0.0000446***	-0.0000454***
	(0.00000843)	(0.00000973)	(0.00000990)	(0.0000100)
Dist. to Water	-0.0000866***	-0.000135***	-0.000137***	-0.000132***
	(0.0000176)	(0.0000191)	(0.0000188)	(0.0000186)
(Dist to Water)^2	8.27e-09***	1.10e-08***	1.13e-08***	1.09e-08***
	(1.73e-09)	(1.59e-09)	(1.55e-09)	(1.52e-09)

Standard errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
This table represents specifications displayed in columns 7 and 8 of Table 4 of the main section.

Table A.14	(1)	(2)	(3)	(4)
Coastal IV Second Stage	P(Dev) @ 1980	P(Dev) @ 1986	P(Dev) @ 1992	P(Dev) @ 2000
County Controls				
Cum. Dis. Days 1960-	0.000600	-0.000399	-0.000130	0.000503
	(0.000664)	(0.000537)	(0.000519)	(0.000472)
County Pop 1970	-0.00000128	-0.00000342**	-0.00000335*	-0.00000309*
	(0.00000102)	(0.00000172)	(0.00000171)	(0.00000168)
County Pop 1960	0.00000303	0.00000661*	0.00000679*	0.00000643*
	(0.00000237)	(0.00000372)	(0.00000372)	(0.00000367)
County Pop 1950	-0.00000663**	-0.0000123**	-0.0000130**	-0.0000125**
	(0.00000327)	(0.00000542)	(0.00000540)	(0.00000531)
County Pop 1940	0.00000509**	0.00000963***	0.0000101***	0.00000957***
	(0.00000213)	(0.00000370)	(0.00000368)	(0.00000361)
County Unemploy. 1970	0.0585*	0.0196	0.0146	0.0162
	(0.0306)	(0.0233)	(0.0226)	(0.0224)
County Unemploy. 1960	-0.0339*	0.0540***	0.0557***	0.0469**
	(0.0203)	(0.0203)	(0.0199)	(0.0209)
County Unemploy. 1950	-0.0108	-1.113	-0.707	-0.430
	(2.563)	(1.096)	(1.080)	(1.076)
County Dwell. Dens. 1970	0.000795***	0.00162***	0.00157***	0.00147***
	(0.000221)	(0.000315)	(0.000313)	(0.000315)
County Dwell. Density 1960	-0.0000346	-0.000715	-0.000667	-0.000597
	(0.000700)	(0.000514)	(0.000509)	(0.000509)
County Dwell. Density 1950	-0.000781	-0.00102***	-0.00103***	-0.000985***
	(0.000609)	(0.000371)	(0.000368)	(0.000360)
Constant	-0.0410	0.941***	0.989***	0.962***
	(0.267)	(0.221)	(0.229)	(0.228)
N	174750	174750	174750	232948
R^2	0.104	0.104	0.108	0.080
Hansen's J	0.0449	0.1254	0.1093	0.1017
Anderson-Rubin Wald Test	0.0002	0.0001	0.0001	0.0001
Stock-Wright LM Test	0.0000	0.0000	0.0000	0.0003

Standard errors clustered at community level.
Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$
This table represents specifications displayed in columns 7 and 8 of Table 4 of the main section.

Appendix B

Disaster-Financing as Vote Buying

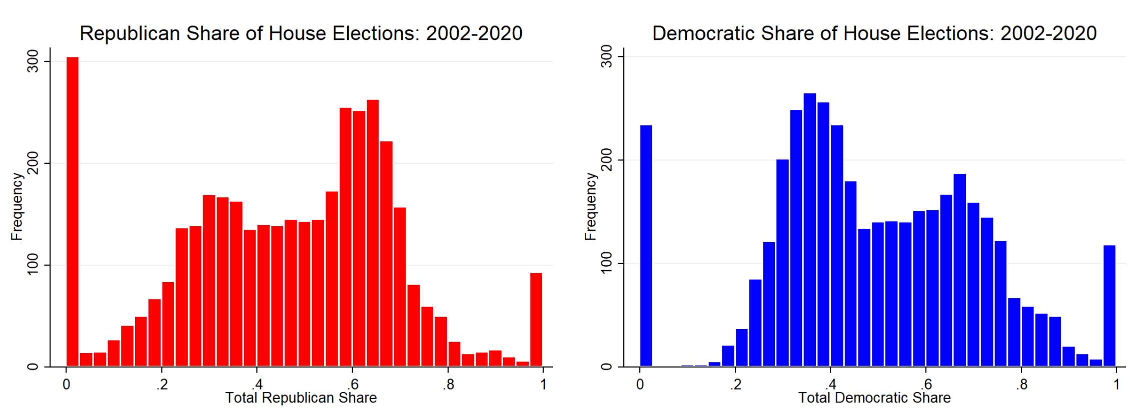

Figure B.1: Republican and Democratic Shares in House Elections: 2002-2020. Back To Text.

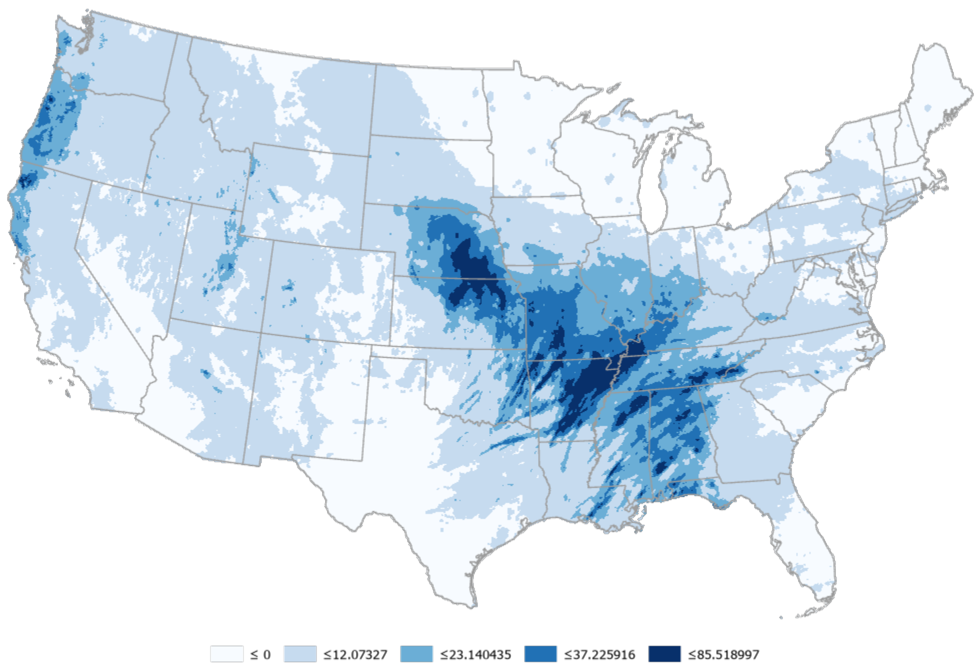

Figure B.2: Rainfall on 12/01/2018 represented by the PRISM dataset. Rainfall is measured in millmeters. Class widths were created using the "Natural Breaks" method in ArcGIS. Back To Text.

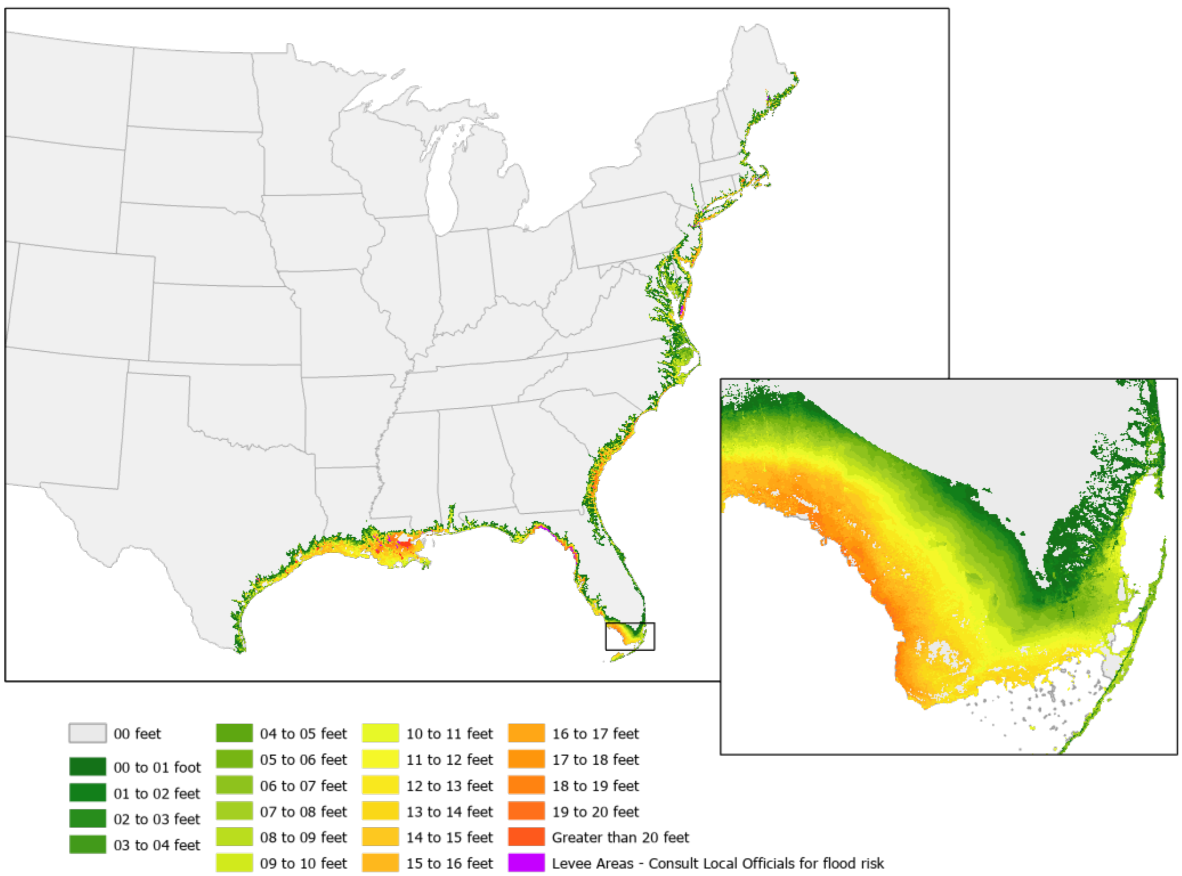

Figure B.3: Storm Surge Wave Height for Category 3 Hurricanes. The inset map displays southern Florida as an example to reveal the gradient of storm surge height going from the coast to inland. Back To Text.

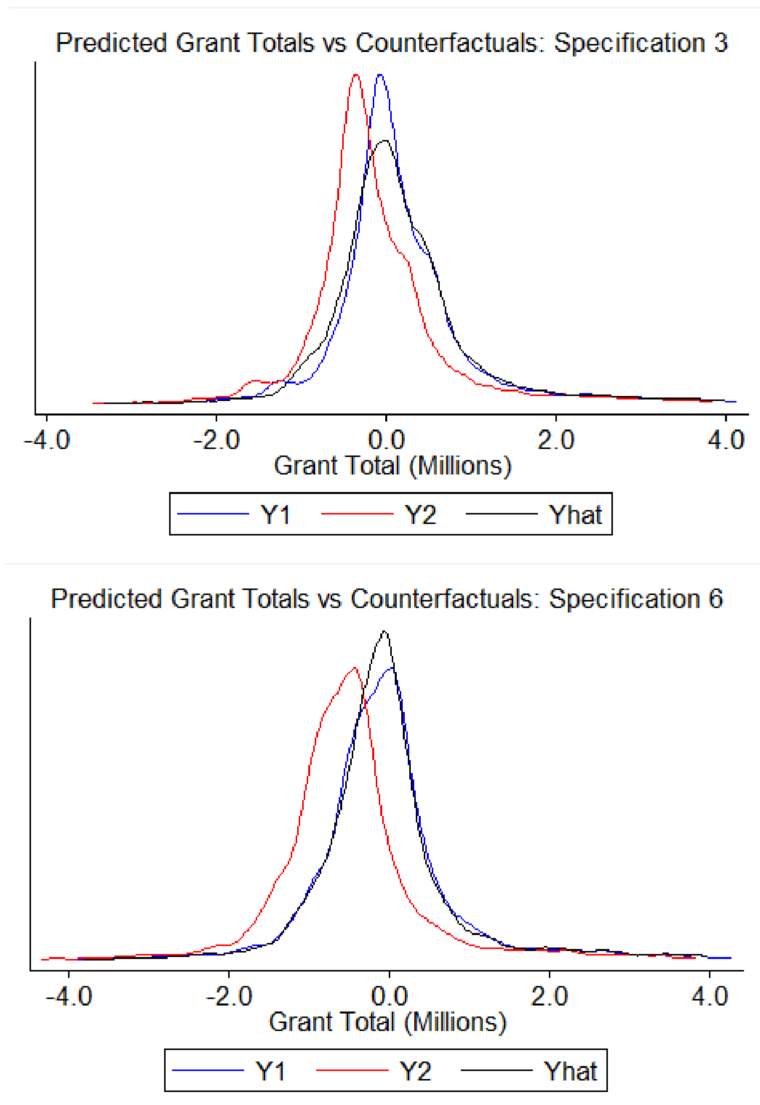

Figure B.4: Densities for Predicted Values. $Y1$ and $Y2$ represent the counterfactual predicted values $\tilde{Y}_1$ and $\tilde{Y}_2$, respectively. Back To Text.

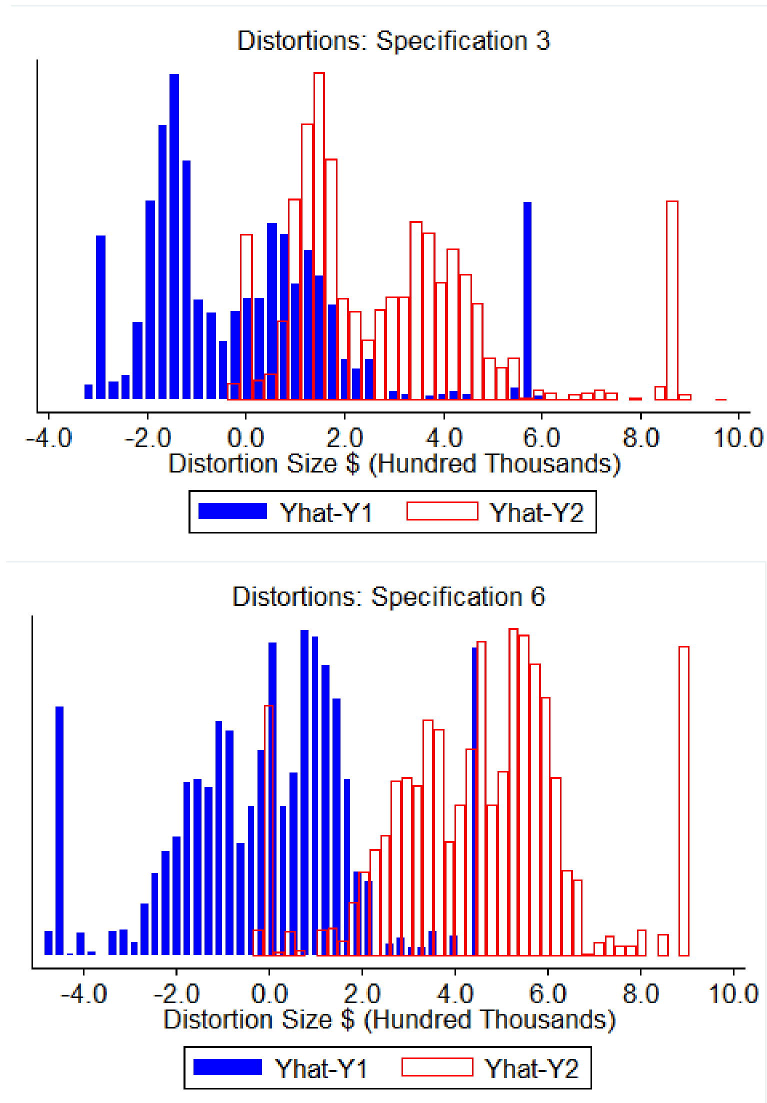

Figure B.5: Histograms of Distortions $(\hat{Y} - \tilde{Y})$. Back To Text.

Table 2: Committee Summaries		
Subcommittee	Committee	#
House		
Homeland Security	Appropriations	13
Emer Prep, Response, and Recovery	Homeland Security	11
Econ Dev, Public Buildings, and Emer Mngt	Transportation and Infrastructure	14
Senate		
Homeland Security	Appropriations	14
Federal Spending Oversight and Emer Mngt	Homeland Security	9

Table 21: Average Number of Members on FEMA Committees - 2003-2020. Years correspond to the 108-116 Congresses. The House consists of 435 voting members, the Senate consists of 100 voting members. Back To Text.

Table 3: Summary Statistics	Count	Mean	SD	Min	Max
Dependent Variable					
Total Grants ($ '000)	40070	439.4	5100.1	0.00	418069.9
Grant Variables					
Total Damage ($ '000)	40070	492.9	5424.2	0.00	407976.6
Inspected Structures Count	40070	185.3	763.9	0.00	24879
Approved Applicants Count	40070	123.7	691.1	0.00	32116
Valid Registrations Count	40070	288.8	1220.4	1.00	42427
Tact. Redist. Variables					
Comp	25105	0.612	0.280	0.00	1.00
Rep & Pres	40070	0.347	0.476	0.00	1.00
Rep & Pres × Comp	25105	0.334	0.362	0.00	1.00
Disaster Severity Variables					
Rainfall during Disaster (mm)	35097	268.5	158.7	0.00	1527.9
Cat3 Storm Surge Height (ft)	39679	0.510	1.97	0.00	19.2
Demographic Variables					
Nonwhite Population (Zip)	35173	3734.9	7117.3	0.00	99296
White Population (Zip)	35173	9797.3	10662.5	0.00	91748
Owner-Occupied Housing Units (Zip)	35173	3378.7	3517.8	0.00	24843
Urban Housing Units (Zip)	35173	4723.7	6145.0	0.00	41575
Rural Housing Units (Zip)	35173	974.8	1273.6	0.00	13249
Households with Owner over 64 (Zip)	35173	1114.7	1283.3	0.00	14165
Household with Owner under 64 (Zip)	35173	3992.8	4455.0	0.00	35980
Housing Value, Prev. Year (Zip) ($ '000)	29855	176.5	139.5	8.91	3211.5
Unemp. Rate, Prev. Year (County)	39502	5.79	1.99	1.70	23.1
Electoral and Political Variables					
House Homeland Security	25104	0.015	0.120	0.00	1.00
House Transportation	25104	0.027	0.163	0.00	1.00
House Appro. Homeland Security	25104	0.029	0.168	0.00	1.00
Senate Homeland Security	40070	0.156	0.368	0.00	2.00
Senate Appro. Homeland Security	40070	0.257	0.440	0.00	2.00

Table 3: Summary Statistics Continued	Count	Mean	SD	Min	Max
Electoral and Political Variables					
Gov & Pres	40070	0.593	0.491	0.00	1.00
Gov & Pres × Comp	25105	0.339	0.366	0.00	1.00
Democratic President	40070	0.272	0.445	0.00	1.00
Aggregate Policy Variables					
NFIP Policy Count (Zip)	30547	1761.6	5276.0	1.00	156585
NFIP Policy Coverage (Zip) ($ '000)	30547	424308.6	1248811.0	1.10	3.07e+07
NFIP Deductibles (Zip) ($ '000)	30547	3893.5	10472.1	0.00	278501.5
Policy Term Mode (Zip)	30547	1.00	0.043	1.00	3.00
Regular Program Dummy	30541	2.00	0.039	1.00	2.00
Policy Duration (Zip)	30547	3628.3	1272.1	216	10777

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Table 4.1	OLS			Tobit		
Full Vote Share Range	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Tact. Redist. Variables						
Comp	653.0*	657.8*	192.5	1015.1**	995.2**	458.8
	(341.0)	(347.9)	(338.8)	(421.2)	(437.0)	(371.9)
Rep & Pres	496.5*	480.0*	869.6**	494.5†	468.1†	893.2**
	(282.6)	(275.1)	(386.8)	(309.6)	(296.6)	(393.5)
Rep & Pres × Comp	-783.3*	-771.6*	-893.7*	-863.7*	-820.7*	-950.3*
	(422.0)	(414.4)	(530.2)	(467.3)	(454.2)	(544.0)
Electoral and Political Controls						
House Homeland Security	-243.1†	-155.0	108.9	-362.5	-315.2	7.350
	(148.1)	(193.9)	(166.7)	(253.9)	(305.8)	(230.5)
House Transportation	32.80	21.82	10.17	-235.7	-216.6	-158.3
	(238.4)	(216.7)	(148.7)	(288.3)	(242.3)	(183.6)
House Approp. Homeland Security	3.865	23.01	-34.88	-36.51	-14.50	-39.97
	(118.2)	(121.0)	(240.2)	(183.4)	(172.5)	(276.9)
Sen. Homeland Security	-362.5†	-291.5†	-468.4**	-383.5†	-228.1	-434.6**
	(234.6)	(199.0)	(195.6)	(239.7)	(190.3)	(184.0)
Sen. Approp. Homeland Security	-598.3	-458.7	52.81	-554.1	-404.9	155.4
	(509.1)	(462.8)	(292.9)	(535.8)	(485.5)	(328.2)
Gov & Pres	156.5	98.51	-158.7	323.0	236.7	-79.95
	(393.3)	(459.0)	(336.3)	(434.1)	(504.1)	(359.1)
Gov & Pres × Comp.	100.3	165.0	443.4	-133.7	-87.14	285.1
	(382.2)	(374.9)	(424.9)	(425.0)	(411.7)	(433.5)
Democratic President	-302.2	-247.8	136.4	-380.2†	-401.6	22.82
	(225.1)	(391.2)	(287.4)	(239.0)	(437.5)	(302.7)
Disaster Severity Controls						
Rainfall during Disaster (mm)	2.271***	2.364***	0.837***	3.317***	3.587***	1.848***
	(0.592)	(0.683)	(0.315)	(0.799)	(0.940)	(0.444)
Cat3 Surge Height (ft)	13.35	13.28	33.76	16.74	18.10	39.59†
	(31.81)	(32.13)	(26.18)	(32.49)	(32.91)	(26.35)
Census Demo. Controls						
Nonwhite Population	-0.0469	-0.0460	-0.0608	-0.0534	-0.0528	-0.0695
	(0.0488)	(0.0480)	(0.0500)	(0.0531)	(0.0523)	(0.0552)
White Population	0.0282	0.0292	-0.0195	0.0347	0.0363	-0.0209
	(0.0218)	(0.0217)	(0.0299)	(0.0256)	(0.0255)	(0.0355)
Owner-Occupied Housing Units	0.0358	0.0336	0.0192	0.0512*	0.0485†	0.0331
	(0.0268)	(0.0262)	(0.0362)	(0.0306)	(0.0298)	(0.0438)
Housing Units White Owner	-0.341*	-0.342*	-0.0504	-0.393**	-0.396**	-0.0706
	(0.179)	(0.178)	(0.151)	(0.197)	(0.196)	(0.173)
Urban Housing Units	-0.0777	-0.0858†	-0.106*	-0.0779	-0.0804	-0.0998†
	(0.0606)	(0.0589)	(0.0566)	(0.0657)	(0.0623)	(0.0608)
Rural Housing Units	-0.0209	-0.0276	-0.0448	0.0136	0.0114	-0.0105
	(0.0582)	(0.0569)	(0.0542)	(0.0589)	(0.0564)	(0.0558)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in \$ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4.1 (Continued)	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Census Demo. Controls (Zip)						
Households Owners over 64	0.397**	0.407**	0.150	0.451**	0.459**	0.169
	(0.189)	(0.187)	(0.151)	(0.205)	(0.202)	(0.178)
Households Owners under 64	0.337*	0.344*	0.205	0.364*	0.367*	0.214
	(0.193)	(0.191)	(0.143)	(0.205)	(0.201)	(0.159)
Housing Value, Prev. Year	-0.245	-0.182	0.587**	-0.626**	-0.549*	0.380
	(0.228)	(0.209)	(0.265)	(0.315)	(0.286)	(0.314)
Unemp. Rate, Prev. Year (County)	28.69	37.30†	1.142	48.06	49.77*	5.080
	(35.95)	(23.31)	(20.48)	(39.40)	(26.40)	(21.31)
Aggregate Policy Controls (Zip)						
NFIP Policy Coverage	-0.000542	-0.000548	-0.000105	-0.000563	-0.000569	-0.000111
	(0.000386)	(0.000392)	(0.000292)	(0.000395)	(0.000401)	(0.000303)
NFIP Policy Count	0.0786	0.0821	0.0298	0.0825	0.0837	0.0290
	(0.0592)	(0.0606)	(0.0423)	(0.0615)	(0.0624)	(0.0442)
NFIP Deductibles	0.0456†	0.0454†	0.00619	0.0457†	0.0464†	0.00566
	(0.0285)	(0.0285)	(0.0200)	(0.0289)	(0.0290)	(0.0205)
Policy Term Mode	-360.5	-376.1	84.46	-273.6	-251.3	224.3
	(297.6)	(302.3)	(156.8)	(288.3)	(311.7)	(181.8)
Regular Program Dummy	327.9*	279.3*	-66.48	8.819	-19.41	-317.0*
	(186.7)	(162.8)	(160.5)	(268.5)	(241.5)	(191.6)
Average Policy Duration	0.0104	0.00418	0.00881	0.0334	0.00569	0.00959
	(0.0270)	(0.0123)	(0.0106)	(0.0335)	(0.0163)	(0.0138)
Flood Zone Mode: D	61.02	34.92	112.8	471.7***	422.9***	407.2**
	(101.9)	(93.32)	(119.4)	(148.8)	(137.2)	(162.9)
Flood Zone Mode: V	57.60	51.19	228.3	267.3	249.9	387.4
	(495.9)	(486.2)	(497.2)	(495.0)	(481.8)	(492.9)
Flood Zone Mode: X	-1.939	-0.395	-49.63*	-54.42	-50.57	-93.12***
	(35.48)	(34.23)	(29.21)	(39.52)	(37.72)	(33.70)
Grant Controls (Zip)						
Valid Registrations Count			1.485***			1.538***
			(0.319)			(0.323)
Constant	-1298.4*	-1183.8*	-255.3	-154.6	-390.1	-1145.0*
	(708.4)	(663.4)	(511.1)	(1035.5)	(977.0)	(643.3)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
F Statistics						
Tact. Redist. Variables	2.27*	1.60	3.29**	2.58*	3.2e08***	3.21**
Comp + Rep & Pres × Comp=0	0.10	0.08	3.35*	0.10	0.14	1.51
N	15103	15103	15103	15103	15103	15103

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 4.2	OLS			Tobit		
Limited Vote Share Range [0.40,0.60]	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Tact. Redist. Variables						
Comp	878.2***	899.1***	899.1***	972.5***	1040.7***	770.3**
	(319.6)	(320.5)	(320.5)	(323.1)	(351.8)	(327.1)
Rep & Pres	267.5**	281.1*	281.1*	326.7**	372.4**	361.5*
	(126.4)	(144.8)	(144.8)	(157.9)	(181.0)	(189.2)
Rep & Pres × Comp	-854.5***	-819.4**	-819.4**	-1071.3***	-1142.4***	-901.9**
	(277.8)	(346.4)	(346.4)	(327.0)	(405.5)	(408.2)
Electoral and Political Controls						
House Homeland Security	-285.2†	-537.4**	-537.4**	-947.1***	-1368.6***	-1099.8***
	(185.2)	(238.3)	(238.3)	(238.2)	(350.2)	(294.9)
House Transportation	-163.0	-90.35	-90.35	-257.0†	-287.3	-330.7†
	(138.6)	(169.8)	(169.8)	(163.7)	(202.5)	(213.6)
House Approp. Homeland Security	-315.1**	-38.38	-38.38	-500.6	-246.4	-243.0
	(138.4)	(199.3)	(199.3)	(349.6)	(346.9)	(234.2)
Sen. Homeland Security	-368.3***	-292.1**	-292.1**	-470.7***	-310.5**	-308.2**
	(128.5)	(143.4)	(143.4)	(103.3)	(143.2)	(127.1)
Sen. Approp. Homeland Security	-461.4**	-168.2	-168.2	-416.9***	68.09	-38.39
	(177.8)	(173.8)	(173.8)	(157.0)	(213.0)	(218.7)
Gov & Pres	70.78	35.93	35.93	43.74	-8.834	125.3
	(150.5)	(149.8)	(149.8)	(184.4)	(183.4)	(208.0)
Gov & Pres × Comp	-244.0	-312.5	-312.5	-144.1	-312.6	-567.5†
	(339.7)	(321.0)	(321.0)	(384.6)	(364.1)	(377.7)
Democratic President	-292.0***	-179.9	-179.9	-398.0***	-321.2	-624.9**
	(111.7)	(185.3)	(185.3)	(135.9)	(258.1)	(295.6)
Disaster Severity Controls						
Rainfall during Disaster (mm)	1.481***	1.523***	1.523***	1.803***	2.058***	1.381***
	(0.427)	(0.428)	(0.428)	(0.512)	(0.520)	(0.535)
Cat3 Surge Height (ft)	-23.86	-29.47	-29.47	-27.59	-33.44	13.02
	(23.03)	(23.70)	(23.70)	(23.78)	(24.33)	(21.30)
Census Demo. Controls (Zip)						
Nonwhite Population	0.115*	0.117*	0.117*	0.0932†	0.0938†	0.0887*
	(0.0629)	(0.0643)	(0.0643)	(0.0612)	(0.0628)	(0.0518)
White Population	-0.0610*	-0.0574†	-0.0574†	-0.0563†	-0.0519	-0.0756
	(0.0343)	(0.0356)	(0.0356)	(0.0350)	(0.0363)	(0.0548)
Owner-Occupied Housing Units	-0.00197	-0.00807	-0.00807	-0.00407	-0.0111	0.0137
	(0.0375)	(0.0366)	(0.0366)	(0.0390)	(0.0377)	(0.0442)
Housing Units White Owner	0.410*	0.405†	0.405†	0.323	0.316	0.469*
	(0.242)	(0.246)	(0.246)	(0.238)	(0.243)	(0.239)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4.2 (Continued)	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Census Demo. Controls (Zip)						
Urban Housing Units	0.00843	-0.0225	-0.0225	0.00508	-0.0251	0.0337
	(0.108)	(0.111)	(0.111)	(0.109)	(0.112)	(0.110)
Rural Housing Units	0.0715	0.0449	0.0449	0.0967	0.0696	0.119
	(0.123)	(0.126)	(0.126)	(0.122)	(0.125)	(0.120)
Households with Owners over 64	-0.253	-0.210	-0.210	-0.147	-0.102	-0.343
	(0.322)	(0.329)	(0.329)	(0.321)	(0.328)	(0.294)
Households with Owners under 64	-0.253	-0.225	-0.225	-0.174	-0.145	-0.321
	(0.253)	(0.261)	(0.261)	(0.251)	(0.258)	(0.230)
Housing Value, Previous Year	0.00912	0.0301	0.0301	-0.0194	0.00441	0.712**
	(0.239)	(0.240)	(0.240)	(0.240)	(0.240)	(0.284)
Unemployment Rate, Previous Year (County)	52.24*	60.22*	60.22*	74.76**	85.46**	51.23**
	(27.73)	(33.30)	(33.30)	(30.21)	(35.99)	(23.64)
Aggregate Policy Controls (Zip)						
NFIP Policy Coverage	-0.000307	-0.000253	-0.000253	-0.000284	-0.000217	0.00000778
	(0.000335)	(0.000350)	(0.000350)	(0.000341)	(0.000359)	(0.000278)
NFIP Policy Count	0.00849	0.00674	0.00674	0.00449	0.000952	-0.0239
	(0.0576)	(0.0579)	(0.0579)	(0.0583)	(0.0591)	(0.0405)
NFIP Deductibles	0.0426*	0.0392†	0.0392†	0.0425*	0.0386†	0.00509
	(0.0233)	(0.0242)	(0.0242)	(0.0230)	(0.0239)	(0.0163)
Regular Program Dummy	174.8	153.8	153.8	32.90	25.88	-58.89
	(256.4)	(221.6)	(221.6)	(345.3)	(300.5)	(222.0)
Average Policy Duration	-0.0144	0.00513	0.00513	-0.0153	0.00547	0.0275*
	(0.0148)	(0.0126)	(0.0126)	(0.0191)	(0.0159)	(0.0162)
Flood Zone Mode: V	200.9	247.6	247.6	388.4	445.9†	-134.0
	(259.9)	(262.2)	(262.2)	(291.9)	(295.9)	(216.8)
Flood Zone Mode: X	12.84	9.942	9.942	-21.05	-24.68	-91.99**
	(35.97)	(36.18)	(36.18)	(38.58)	(39.04)	(38.85)
Grant Controls (Zip)						
Valid Registrations Count			0.790***			0.793***
			(0.187)			(0.183)
Constant	-1030.7*	-1166.9**	-1166.9**	-670.8	-1441.0*	-676.4
	(603.4)	(587.8)	(587.8)	(815.7)	(858.8)	(641.0)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
F Statistics						
Tact. Redist. Variables	3.89**	3.04**	2.28*	4.63***	3.69**	2.24*
Comp + Rep & Pres × Comp=0	0.01	0.06	0.01	0.09	0.08	0.14
N	3502	3502	3502	3502	3502	3502

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 4.3	OLS			Tobit		
Limited Vote Share Range [0.47,0.53]	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Tact. Redist. Variables						
Comp	6723.3**	6425.7***	3025.8***	14715.3***	14245.4***	9857.3***
	(3021.8)	(2223.2)	(771.6)	(5488.5)	(14.11)	(13.49)
Rep & Pres	-259.8	633.5	1812.0**	-1921.8**	-11800.2***	-8631.0***
	(366.2)	(1086.5)	(714.6)	(864.1)	(10.31)	(5.563)
Rep & Pres × Comp	-6190.8**	-6953.5**	-5273.7***	-11625.3***	512.3***	862.1***
	(2527.4)	(2782.2)	(1204.7)	(4291.4)	(14.46)	(8.167)
Electoral and Political Variables						
House Homeland Security	-799.0*	-1138.2	10.34	-1515.1**	-1523.7***	-245.8***
	(449.1)	(809.4)	(376.1)	(705.4)	(42.90)	(35.25)
House Transportation	0	0	0	-2863.5**	-9147.5***	-7032.5***
	(.)	(.)	(.)	(1271.5)	(2.493)	(3.310)
House Approp. Homeland Security	-2476.3**	-2271.4**	-294.7	-6054.9***	-8516.8***	-5583.5***
	(1214.0)	(904.4)	(443.9)	(2239.3)	(26.21)	(15.62)
Sen. Homeland Security	103.0	-29.28	-208.5*	510.3**	1918.4***	1453.2***
	(109.7)	(146.7)	(103.8)	(225.0)	(9.207)	(6.570)
Sen. Approp. Homeland Security	0	0	0	1877.0**	7877.7***	7356.2***
	(.)	(.)	(.)	(897.9)	(8.795)	(5.450)
Gov & Pres	-230.0	-93.31	-85.85	-1254.8	-469.5***	-391.8***
	(514.1)	(366.1)	(303.0)	(871.5)	(9.332)	(12.13)
Gov & Pres × Comp	-1551.2†	-1493.6*	-167.9	-4033.8***	-4853.0***	-3150.3***
	(963.9)	(880.1)	(635.5)	(1473.5)	(12.79)	(13.80)
Democratic President	-1378.9†	-1325.9	742.4	-4139.8**	-6506.2***	-3536.4***
	(848.6)	(1034.3)	(631.5)	(1726.4)	(6.173)	(5.260)
Disaster Severity Controls						
Rainfall during Disaster (mm)	2.628†	2.800†	1.675†	3.444*	3.514***	2.303***
	(1.568)	(1.675)	(1.033)	(1.955)	(0.0303)	(0.0390)
Cat3 Surge Height (ft)	-45.48	-44.99	77.87	-72.38	-72.16***	72.14***
	(80.63)	(82.00)	(107.4)	(91.21)	(2.569)	(2.393)
Census Demo. Controls (Zip)						
Nonwhite Population	-0.00241	-0.000927	0.0883	-0.00955	-0.00970*	0.0903***
	(0.0988)	(0.0997)	(0.0963)	(0.100)	(0.00569)	(0.00675)
White Population	-0.0624	-0.0652	-0.0167	-0.0713	-0.0676***	-0.0167***
	(0.0714)	(0.0708)	(0.0473)	(0.0723)	(0.00284)	(0.00284)
Owner-Occupied Housing Units	0.0723	0.0710	-0.0203	0.0524	0.0529***	-0.0387***
	(0.0777)	(0.0785)	(0.0505)	(0.0726)	(0.00864)	(0.00896)
Housing Units White Owner	0.164	0.172	0.525	0.156	0.149***	0.536***
	(0.538)	(0.539)	(0.521)	(0.540)	(0.00720)	(0.00732)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4.3 (Continued)	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Census Demo. Controls (Zip)						
Urban Housing Units	-0.0396	-0.0385	0.0756	-0.0883	-0.0852***	0.0263***
	(0.148)	(0.149)	(0.165)	(0.144)	(0.00629)	(0.00665)
Rural Housing Units	-0.0878	-0.0868	0.0279	-0.101	-0.0986***	0.00754**
	(0.101)	(0.101)	(0.110)	(0.103)	(0.00496)	(0.00313)
Households with Owner over 64	-0.0474	-0.0475	-0.568	0.0500	0.0490*	-0.510***
	(0.575)	(0.578)	(0.624)	(0.563)	(0.0251)	(0.0262)
Households with Owner under 64	0.0551	0.0516	-0.491	0.158	0.152***	-0.429***
	(0.431)	(0.435)	(0.511)	(0.424)	(0.00768)	(0.00807)
Housing Value, Prev. Year	-0.217	-0.203	-0.129	-0.148	-0.166***	-0.116***
	(0.287)	(0.290)	(0.190)	(0.241)	(0.0263)	(0.0204)
Unemployment Rate, Previous Year (County)	81.05	103.2	0.744	177.5***	153.3***	39.43***
	(67.66)	(75.22)	(59.53)	(68.52)	(1.758)	(1.668)
Aggregate Policy Controls (Zip)						
NFIP Policy Coverage	-0.0000188	-0.00000469	0.000414	0.000144	0.000127***	0.000606***
	(0.000125)	(0.000105)	(0.000306)	(0.000161)	(0.00000996)	(0.00000909)
NFIP Policy Count	0.00750	0.00396	-0.0714	-0.0245	-0.0215***	-0.106***
	(0.0577)	(0.0546)	(0.0707)	(0.0622)	(0.00255)	(0.00246)
NFIP Deductibles	0.00570†	0.00547†	-0.0199†	0.00564	0.00611***	-0.0218***
	(0.00358)	(0.00349)	(0.0132)	(0.00439)	(0.000977)	(0.00104)
Regular Program Dummy	172.3	165.9	18.30	107.6	117.0***	-32.20***
	(168.5)	(158.4)	(131.8)	(280.1)	(4.897)	(4.937)
Average Policy Duration	-0.0110	-0.0126	-0.00396	-0.0160	-0.0211***	-0.0113***
	(0.0222)	(0.0229)	(0.0211)	(0.0273)	(0.00245)	(0.00210)
Flood Zone Mode: V	719.7	727.8	-870.2	1494.4	1468.9***	-497.9***
	(879.9)	(900.9)	(1052.8)	(1054.4)	(87.30)	(110.1)
Flood Zone Mode: X	-90.39	-90.68	-77.62†	-136.3*	-137.2***	-110.8***
	(70.46)	(71.76)	(48.12)	(78.78)	(10.70)	(11.97)
Grant Controls (Zip)						
Valid Registrations Count			1.130***			1.143***
			(0.393)			(0.00960)
Constant	-2937.3**	-3091.8**	-1933.0***	-1014.6	1503.9***	483.1***
	(1318.6)	(1489.0)	(505.8)	(877.7)	(9.746)	(9.818)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
F Statistics						
Tact. Redist. Variables	10.78***	2.89**	8.48***	5.24***	1.6e08***	6.8e06***
Comp + Rep & Pres × Comp=0	0.61	0.21	4.00*	6.36**	2.75e05***	2.6e05***
N	1036	1036	1036	1036	1036	1036

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 5: Distortion Results	OLS	
	(1)	(2)
	Distortion Spec. 3	Distortion Spec. 6
Disaster Controls		
Rainfall during Disaster (mm)	0.0184	0.0153
	(0.0497)	(0.0502)
Cat3 Storm Surge Height (ft)	2.695†	3.213*
	(1.798)	(1.896)
Electoral and Political Controls		
Democratic President	-296.4***	-278.4***
	(47.46)	(48.62)
Sen. Homeland Security	-23.16	5.177
	(32.53)	(35.30)
Sen. Appro. Homeland Security	3.516	-24.89
	(43.87)	(43.86)
Census Demo. Controls (Zip)		
Nonwhite Population	-0.0135***	-0.0153***
	(0.00353)	(0.00398)
White Population	0.00542***	0.00616***
	(0.00206)	(0.00191)
Owner-Occupied Housing Units	0.00301	0.00223
	(0.00286)	(0.00314)
Housing Units White Owner	-0.0490***	-0.0541***
	(0.0134)	(0.0138)
Urban Housing Units	0.00880*	0.0104**
	(0.00463)	(0.00493)
Rural Housing Units	0.00818*	0.00998**
	(0.00443)	(0.00473)
Households with Owner over 64	0.0277**	0.0295**
	(0.0126)	(0.0131)
Households with Owner under 64	0.0246**	0.0271**
	(0.0109)	(0.0114)
Housing Value, Prev. Year	-0.0867***	-0.0741***
	(0.0255)	(0.0279)
Unemp. Rate, Prev. Year (County)	1.978	5.908
	(5.112)	(5.264)

Standard errors are clustered at the Congressional District Level. Distortion totals in \$ '000
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5 (Continued)	OLS	
	(1)	(2)
	Distortion Spec. 3	Distortion Spec.6
Aggregate Policy Controls (Zip)		
NFIP Policy Coverage (Zip)	-0.0000127	-0.00000658
	(0.0000114)	(0.0000119)
NFIP Policy Count (Zip)	0.00268	0.00143
	(0.00279)	(0.00288)
NFIP Deductibles (Zip)	-0.000700	-0.000871
	(0.000684)	(0.000693)
Policy Term Mode (Zip)	68.42†	47.66
	(47.08)	(41.73)
Mode Program Type (Reg Program Dummy)	-7.426	24.81
	(35.65)	(40.79)
Average Policy Duration (Zip)	0.00157	0.000988
	(0.00212)	(0.00207)
Mode Flood Zone: D	-28.77*	-25.74
	(14.85)	(18.97)
Mode Flood Zone: V	-31.58	-33.15
	(27.49)	(29.30)
Mode Flood Zone: X	-4.562†	-3.886
	(3.152)	(3.379)
Grant Controls		
Valid Registrations Count	-0.0122**	-0.00994†
	(0.00605)	(0.00640)
Constant	68.48	-17.85
	(102.8)	(104.5)
Fixed Effects		
State	✓	✓
Congressional District	✓	✓
Year and Congress	✓	✓
N	15103	15103
R^2	0.547	0.528

Standard errors are clustered at the Congressional District Level. Distortion totals in $ '000
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$
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Appendix C

Congressional Dominance of Federal Hazard Mitigation Assistance

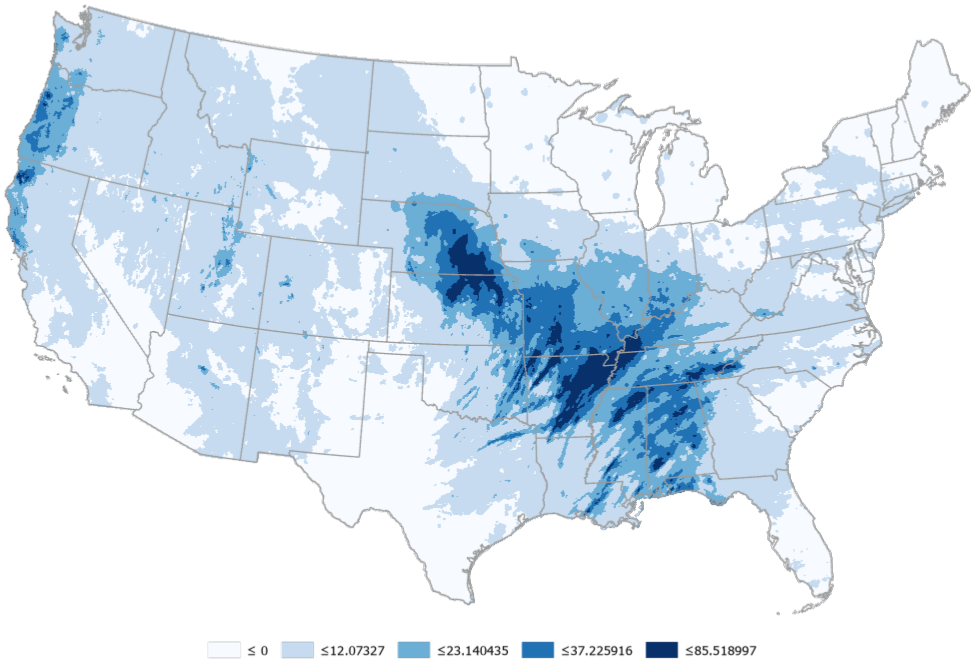

Figure C.1: Rainfall on 12/01/2018 represented by the PRISM dataset. Rainfall is measured in millmeters. Class widths were created using the “Natural Breaks” method in ArcGIS. Back To Text.

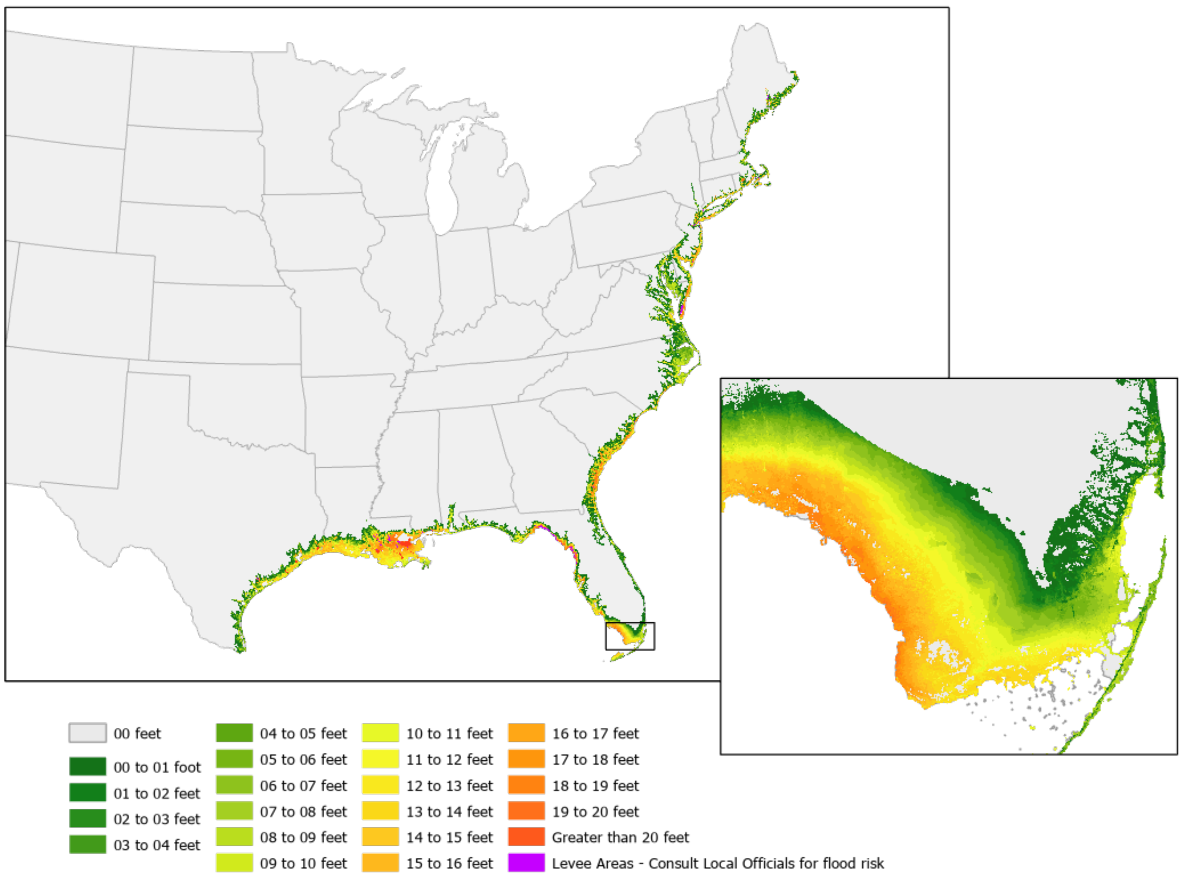

Figure C.2: Storm Surge Wave Height for Category 3 Hurricanes. The inset map displays southern Florida as an example to reveal the gradient of storm surge height going from the coast to inland. Back To Text.

Table 1: Committee Summaries

Type	Subcommittee	Committee	Period	#
House				
Appropriations				
	VA, HUD, and Independent Agencies	Appropriations	Pre-2003	14
	Homeland Security	Appropriations	Post-2003	13
Oversight				
	Emergency Management*	Trans. and Infrst.	Pre-2003	16
	Emer Prep, Response, and Recovery*	Homeland Security	Post-2003	11
	Emergency Management*	Trans. and Infrst.	Post-2003	14
Senate				
Appropriations				
	VA, HUD, and Independent Agencies	Appropriations	Pre-2003	11
	Homeland Security	Appropriations	Post-2003	14
Oversight				
	Clean Air, Wetlands, Priv. Prop. and Nuc. Safety*	Env. and Public Works	Pre-2003	7
	Federal Spending Oversight and Emer Mngt	Homeland Security	Post-2003	9

Table 1: Average Number of Members on FEMA subcommittees - 1997-2020. Years correspond to the 105-116 Congresses. The House consists of 435 voting members, the Senate consists of 100 voting members. * The name of the subcommittee changes across both study periods. Back To Text.

Table 3: Summary Statistics	Count	Mean	SD	Min	Max
Dependent Variable					
Grants (Fed Share) ($'000)	10150	1073.0	16373.4	-2.56	756587.9
Grant Variables					
Properties Count	10150	8.18	31.7	0.00	913.0
Federal Cost Share	10121	0.74	0.11	0.00	1.00
Cul. HMA Grants 1989-97 ($'000)	10120	2628.2	25931.4	0.00	659711.3
House Subcommittee Variables					
Appro. Sub.	7135	0.04	0.19	0.00	1.00
Oversi. Sub.	7135	0.05	0.21	0.00	1.00
Post-Restructure	10150	0.70	0.46	0.00	1.00
House Coalition Variables					
Appro. Coa.	7135	0.37	0.72	0.00	5.00
Oversi. Coa.	7135	0.43	0.69	0.00	5.00
Disaster Severity Variables					
Rainfall during Disaster (mm)	10133	170.6	174.5	0.00	1279.6
Cum. Disaster Rain 1997+ (mm)	10133	125.5	248.5	0.00	2281.5
Cat3 Storm Surge Height (ft)	10017	0.68	2.29	0.00	19.2
Disaster Prop. Damage 1960-96 ($'000)	10124	409430.5	1850254.3	1.59	10952708.0
Demographic Variables (Zip)					
Nonwhite Population	9425	3617.8	5697.7	0.00	57827.0
White Population	9425	10941.6	10642.4	0.00	86186.0
Owner-Occupied Housing Units	9425	3785.7	3463.58	0.00	24474.0
Urban Housing Units	9425	4894.6	5873.8	0.00	38565.0
Rural Housing Units	9425	1367.7	1452.7	0.00	12755.0
Households Owners over 64	9425	1237.2	1212.9	0.00	10293.0
Households Owners under 64	9425	4354.5	4254.6	0.00	28926.0
Housing Value, Prev. Year ($'000)	10084	130.1	96.4	17.4	2029.9
Unemp. Rate, Prev. Year (County)	10131	6.01	2.49	1.30	30.0

Table 3: Summary Statistics (Cont.)	Count	Mean	SD	Min	Max
Electoral Variables					
Democratic President	10150	0.57	0.49	0.00	1.00
Democratic Governor	10150	0.32	0.47	0.00	1.00
Gov & President Same Party	10150	0.45	0.50	0.00	1.00
Gov & Rep Same Party	10150	0.44	0.50	0.00	1.00
House Rep & President Same Party	10150	0.31	0.46	0.00	1.00
House Electoral Competition	7155	0.59	0.31	0.00	1.00
H&P Same Party × Elec. Comp.	7155	0.25	0.35	0.00	1.00
G&P Same Party × Elec. Comp.	7155	0.25	0.35	0.00	1.00
Senate Subcommittee Variables					
Sen. Appro. Sub.	10150	0.28	0.46	0.00	2.00
Sen. Oversi. Sub.	10150	0.20	0.40	0.00	2.00
Aggregate Policy Variables (Zip)					
NFIP Policy Count	10150	1307.3	5002.2	0.00	98419.0
NFIP Policy Coverage ($'000)	10150	306495.4	1179353.3	0.00	21730248.0
Aggregate NFIP Deductibles ($'000)	10150	2864.9	10385.4	0.00	253381.3

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Table 4	OLS			Tobit		
Results: Direct Rep	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
House Subcommittee Variables						
Appro. Sub.	33.12*	74.67**	72.71**	32.94*	74.50**	73.39**
	(19.61)	(32.63)	(33.07)	(19.48)	(31.72)	(32.10)
Oversi. Sub.	92.68*	88.67*	66.79	92.23*	88.40*	66.95†
	(48.05)	(47.94)	(46.75)	(47.86)	(46.71)	(45.54)
Appro. Sub. × Post	-52.00*	-92.83**	-98.40**	-51.64*	-92.18**	-98.52***
	(27.40)	(40.51)	(39.27)	(27.21)	(39.41)	(38.04)
Oversi. Sub. × Post	-114.6**	-112.5**	-99.62**	-113.7**	-111.2**	-99.05**
	(47.67)	(48.00)	(47.81)	(47.58)	(46.92)	(46.62)
Disaster Severity Controls						
Rainfall during Disaster (mm)	0.042†	0.023	0.009	0.043†	0.024	0.010
	(0.027)	(0.029)	(0.030)	(0.027)	(0.028)	(0.030)
Cum. Disaster Rain 1997+ (mm)	0.037**	0.041***	0.026**	0.037**	0.041***	0.026**
	(0.015)	(0.014)	(0.013)	(0.015)	(0.014)	(0.012)
Cat3 Storm Surge Height (ft)	1.786	2.992	2.931	1.800	2.993	2.927
	(1.933)	(2.434)	(2.402)	(1.918)	(2.367)	(2.331)
Disaster Prop. Damage 1960-96 ($)	-2.27e-6	-8.25e-6	-6.42e-6	-2.29e-6	-8.24e-6	-6.45e-6
	(2.7e-6)	(6.47e-6)	(5.78e-6)	(2.68e-6)	(6.28e-6)	(5.60e-6)
Demographic Controls (Zip)						
Nonwhite Population	0.002	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
White Population	0.0010	-0.0003	0.0004	0.0011	-0.0003	0.0004
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Owner-Occupied Housing Units	-0.005	-0.004	-0.005	-0.005	-0.004	-0.005
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Urban Housing Units	-0.009***	-0.008**	-0.008***	-0.009***	-0.008***	-0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Rural Housing Units	-0.004	-0.003	-0.003	-0.004	-0.003	-0.003
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Households Owners over 64	0.024***	0.020**	0.019**	0.024***	0.020**	0.019**
	(0.0082)	(0.0086)	(0.0087)	(0.0082)	(0.0083)	(0.0085)
Households Owners under 64	0.010	0.012	0.011	0.0097	0.012	0.011
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
Unemp. Rate, Prev. Year (County)	-1.342	-0.382	-0.416	-1.317	-0.321	-0.436
	(1.806)	(1.996)	(1.960)	(1.802)	(1.962)	(1.895)
Housing Value, Prev. Year ($)	0.0740*	0.0598	0.0289	0.0744*	0.0599	0.0293
	(0.0390)	(0.0440)	(0.0426)	(0.0387)	(0.0428)	(0.0413)
Aggregate Policy Controls (Zip)						
NFIP Policy Count	0.001	0.003	0.004	0.001	0.003	0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
NFIP Policy Coverage ($)	-3.31e-5*	-3.91e-5*	-4.40e-5**	-3.32e-5*	-3.91e-5*	-4.39e-5**
	(1.90e-5)	(2.27e-5)	(2.14e-5)	(1.89e-5)	(2.21e-5)	(2.08e-5)
NFIP Deductibles ($)	0.0045***	0.0043***	0.0043***	0.0045***	0.0043***	0.0043***
	(0.0010)	(0.0012)	(0.0011)	(0.00098)	(0.0011)	(0.0011)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4 (Cont.)	OLS			Tobit		
Results: Direct Rep	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Electoral Controls						
Democratic President	-3.684	-5.742	115.1**	-2.499	-4.410	115.6**
	(9.549)	(10.50)	(53.63)	(9.314)	(10.01)	(51.94)
Democratic Governor	32.34**	37.01**	36.46***	31.17**	35.76**	34.91***
	(12.94)	(14.92)	(13.01)	(13.13)	(14.91)	(13.02)
Gov & President Same Party	11.50	17.53	12.71	11.94	18.19	13.78
	(21.07)	(25.68)	(22.19)	(20.93)	(24.97)	(21.68)
Gov & House Rep Same Party	-3.920	4.264	4.532	-4.491	3.548	3.913
	(8.925)	(11.98)	(11.24)	(8.941)	(11.89)	(11.11)
House Rep & President Same Party	6.031	12.25	6.957	6.860	13.68	7.831
	(16.46)	(17.39)	(16.74)	(16.51)	(17.26)	(16.35)
House Electoral Competition	-11.40	-4.147	-1.360	-11.65	-3.500	-0.670
	(16.72)	(27.60)	(26.35)	(16.57)	(27.03)	(25.89)
H&P Same Party × Elec. Comp.	-3.186	-13.01	-10.73	-3.514	-13.73	-10.68
	(28.34)	(31.17)	(30.36)	(28.19)	(30.45)	(29.45)
G&P Same Party × Elec. Comp.	6.786	0.609	-4.313	7.018	0.462	-4.716
	(30.56)	(39.95)	(34.77)	(30.27)	(38.71)	(33.78)
Senate Subcommittee Controls						
Sen. Appro. Sub.	-68.81***	-67.14***	-81.95***	-68.86***	-67.11***	-81.84***
	(20.56)	(24.72)	(25.27)	(20.51)	(24.15)	(24.54)
Sen. Oversi. Sub.	89.55***	92.39***	110.3***	90.25***	92.95***	111.6***
	(21.55)	(24.29)	(26.20)	(21.47)	(23.68)	(25.62)
Sen. Appro. Sub. × Post	29.26	22.06	35.41	29.29	22.18	35.45
	(22.09)	(27.28)	(28.26)	(21.99)	(26.62)	(27.43)
Sen. Oversi. Sub. × Post	-67.50***	-73.17***	-86.14***	-67.63***	-72.91***	-86.12***
	(21.97)	(23.96)	(27.84)	(21.83)	(23.25)	(26.93)
Grant Controls						
Properties Count	1.892**	1.972**	2.063**	1.891**	1.970**	2.061**
	(0.883)	(0.989)	(1.002)	(0.877)	(0.961)	(0.971)
Cul. HMA Grants 1989-97 ($)	-2.17e-4*	1.92e-5	2.64e-4*	-2.18e-4**	1.62e-5	2.60e-4*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Post-Restructure	60.58***	66.31***	21.99	60.38***	65.91***	21.11
	(18.58)	(22.22)	(68.95)	(18.51)	(21.69)	(67.06)
Constant	49.44*	34.47	-65.56	11.73	109.2***	9.169
	(25.64)	(30.70)	(71.84)	(29.34)	(30.59)	(69.01)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Congressional District		✓	✓		✓	✓
Year and Congress			✓			✓
F Statistics						
Appro. Sub. + Appro. Sub. × Post=0	0.93	0.76	1.69	0.93	0.76	1.70
Oversi. Sub. + Oversi. Sub. × Post=0	2.72†	2.12†	4.28**	2.56†	1.98	4.24**
Subcommittee Variables	2.53*	2.97**	3.33**	2.48**	2.99**	3.44***
N	6236	6236	6236	6236	6236	6236

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 5	OLS			Tobit		
Results: Indirect Rep	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
House Coalition Variables						
Appro. Coa.	34.55*	30.10*	25.90	34.55*	30.31*	26.21
	(17.78)	(17.44)	(21.21)	(17.70)	(17.33)	(20.59)
Oversi. Coa.	25.61†	30.82**	40.74*	25.33†	30.74**	40.67*
	(15.66)	(15.62)	(23.23)	(15.55)	(15.50)	(22.55)
Appro. Coa. × Post	-44.20**	-34.52*	-27.61	-44.03**	-34.40*	-27.59
	(18.89)	(19.34)	(22.82)	(18.79)	(19.21)	(22.15)
Oversi. Coa. × Post	-19.11	-23.74	-31.82	-18.23	-23.24	-31.32
	(18.10)	(18.00)	(25.65)	(17.99)	(17.84)	(24.84)
House Subcommittee Variables						
Appro. Sub.	35.67*	36.19*	58.44*	35.54*	36.62*	59.19*
	(19.98)	(20.00)	(32.99)	(19.86)	(19.94)	(31.99)
Oversi. Sub.	75.11*	70.80†	50.89	74.71*	70.69†	50.93
	(42.59)	(43.19)	(44.74)	(42.41)	(42.99)	(43.49)
Appro. Sub. × Post	-55.03*	-57.11**	-84.07**	-54.72*	-57.04**	-84.19**
	(28.26)	(27.24)	(38.50)	(28.09)	(27.01)	(37.28)
Oversi. Sub. × Post	-97.91**	-95.45**	-80.76*	-96.71**	-94.85**	-79.88*
	(42.72)	(43.13)	(45.75)	(42.67)	(42.97)	(44.56)
Disaster Severity Controls						
Rainfall during Disaster (mm)	0.0414†	0.0258	0.0146	0.0420*	0.0261	0.0150
	(0.0257)	(0.0267)	(0.0285)	(0.0255)	(0.0265)	(0.0277)
Cum. Disaster Rain 1997+	0.0392***	0.0291**	0.0302**	0.0396***	0.0297**	0.0307**
	(0.0143)	(0.0118)	(0.0124)	(0.0142)	(0.0117)	(0.0120)
Cat3 Storm Surge Height (ft)	1.415	1.316	2.662	1.428	1.325	2.657
	(1.933)	(1.922)	(2.402)	(1.916)	(1.902)	(2.329)
Disaster Prop. Damage 1960-96 ($)	-2.09e-6	-8.91e-7	-5.99e-6	-2.10e-7	-9.52e-7	-6.02e-6
	(2.68e-6)	(2.76e-6)	(5.92e-6)	(2.66e-6)	(2.73e-6)	(5.73e-6)
Demographic Controls (Zip)						
Nonwhite Population	0.002	0.001	0.001	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
White Population	0.001	0.0089	0.0004	0.001	0.001	0.0004
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Owner-Occupied Housing Units	-0.006	-0.006	-0.005	-0.006	-0.006	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Urban Housing Units	-0.009***	-0.010***	-0.009***	-0.009***	-0.010sym***	-0.009***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Rural Housing Units	-0.004	-0.00508†	-0.004	-0.005	-0.005†	-0.005
	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Households Owners over 64	0.03***	0.03***	0.02**	0.03***	0.02***	0.02**
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Households Owners under 64	0.01	0.01†	0.01†	0.01	0.01†	0.01†
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5 (Cont.)	OLS			Tobit		
Results: Indirect Rep	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Demographic Controls (Zip)						
Unemp. Rate, Prev. Year (County)	-1.055	-1.349	-0.522	-1.022	-1.424	-0.537
	(1.817)	(1.836)	(2.007)	(1.815)	(1.804)	(1.941)
Housing Value, Prev. Year ($)	0.0739*	0.0467	0.0341	0.0744*	0.0473	0.0345
	(0.0389)	(0.0379)	(0.0426)	(0.0387)	(0.0376)	(0.0412)
Aggregate Policy Controls (Zip)						
NFIP Policy Count	0.001	0.003	0.005	0.001	0.003	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
NFIP Policy Coverage ($)	-3.26e-5*	-3.83e-5**	-4.46e-5**	-3.28e-5*	-3.83e-5**	-4.45e-5**
	(1.88e-5)	(1.75e-5)	(2.13e-5)	(1.87e-5)	(1.74e-5)	(2.07e-5)
NFIP Deductibles ($)	0.004***	0.004***	0.004***	0.004***	0.004***	0.004***
	(0.000985)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Electoral Controls						
Democratic President	-9.368	159.9***	121.7**	-8.161	160.4***	121.9**
	(9.625)	(61.22)	(59.35)	(9.331)	(60.75)	(57.54)
Democratic Governor	28.97**	31.72***	36.82***	27.85**	30.31***	35.32***
	(12.80)	(11.30)	(12.73)	(13.02)	(11.52)	(12.74)
House Rep & President Same Party	9.421	8.178	11.33	10.27	8.674	12.25
	(15.69)	(15.73)	(15.82)	(15.73)	(15.60)	(15.43)
Gov & President Same Party	11.17	7.592	14.88	11.48	8.281	15.94
	(20.68)	(18.24)	(21.49)	(20.53)	(18.13)	(21.00)
Gov & Rep Same Party	-3.031	-3.136	3.078	-3.641	-3.628	2.434
	(9.007)	(8.700)	(11.30)	(9.042)	(8.713)	(11.19)
House Electoral Competition	-5.538	-1.629	8.594	-5.872	-1.783	9.237
	(15.89)	(16.77)	(25.34)	(15.74)	(16.64)	(24.89)
H&P Same Party × Elec. Comp.	-9.908	-11.37	-18.85	-10.23	-11.22	-18.83
	(27.58)	(27.74)	(29.40)	(27.42)	(27.45)	(28.49)
G&P Same Party × Elec. Comp.	10.65	8.200	-0.210	10.94	8.312	-0.557
	(29.20)	(25.98)	(33.77)	(28.92)	(25.74)	(32.81)
Senate Subcommittee Controls						
Sen. Appro. Sub.	-69.81***	-73.61***	-74.48***	-69.91***	-73.51***	-74.36***
	(19.36)	(20.52)	(23.44)	(19.22)	(20.30)	(22.66)
Sen. Oversi. Sub.	102.2***	116.5***	123.1***	102.9***	117.8***	124.4***
	(23.10)	(24.32)	(26.33)	(22.98)	(24.28)	(25.72)
Sen. Appro. Sub. × Post	25.06	24.56	24.29	25.27	24.64	24.38
	(21.19)	(22.10)	(26.00)	(21.09)	(21.87)	(25.19)
Sen. Oversi. Sub. × Post	-82.87***	-92.42***	-99.47***	-82.80***	-92.55***	-99.39***
	(23.95)	(26.15)	(27.90)	(23.73)	(25.87)	(26.95)

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5 (Cont.)	OLS			Tobit		
Results: Indirect Rep	(1)	(2)	(3)	(4)	(5)	(6)
	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total	Grant Total
Grant Controls						
Properties Count	1.915**	2.010**	2.054**	1.915**	2.009**	2.052**
	(0.897)	(0.910)	(1.003)	(0.890)	(0.901)	(0.972)
Cul. HMA Grants 1989-97 ($)	-1.60e-5*	-9.56e-5	1.26e-5	-1.62e-4*	-9.85e-5	9.11e-6
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Post-Restructure	87.29***	114.5†	40.73	86.55***	111.9†	39.32
	(22.71)	(77.74)	(73.30)	(22.70)	(77.58)	(71.34)
Constant	24.18	-123.0*	-111.9†	-7.720	-154.9**	-37.46
	(26.50)	(72.11)	(72.81)	(29.17)	(74.79)	(70.22)
Fixed Effects						
State	✓	✓	✓	✓	✓	✓
Year and Congress		✓	✓		✓	✓
Congressional District			✓			✓
F Statistics						
Appro. Sub. $\geq$ Appro. Coa.	0.00	0.10	0.95	0.00	0.10	1.03
Oversi.Sub. $\geq$ Oversi. Coa.	1.42	0.83	0.06	1.42	0.84	0.06
Appro. Coa. + Appro. Coa.$\times$ Post=0	0.84	0.18	0.02	0.82	0.15	0.01
Oversi. Coa. + Oversi. Coa.$\times$ Post=0	0.51	0.56	0.64	0.61	0.64	0.76
Coalition Variables	2.97**	3.22**	2.18*	2.98**	3.28**	2.34*
Subcommittee Variables	2.68**	2.86**	2.79**	2.60**	2.84**	2.85**
Coalition and Subcommittee Variables	2.59***	2.93***	2.78***	2.57***	2.97***	2.92***
N	6236	6236	6236	6236	6236	6236

Standard errors are clustered at the Congressional District Level. All Grant Totals are in $ '000.

† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

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Table 6 **Results: Distortion Interactions**	OLS (Specification 3) Grant Total	Tobit (Specification 6) Grant Total
Subcommittee Variable		
Rep on Sub.	99.98†	99.98†
	(67.72)	(63.14)
Disaster Interactions		
Rainfall during Disaster (mm) × Rep on Sub.	0.120	0.120
	(0.274)	(0.255)
Cum. Disaster Rain 1997+ (mm) × Rep on Sub.	0.788	0.788†
	(0.564)	(0.525)
Cat3 Storm Surge Height (ft) × Rep on Sub.	52.21†	52.21*
	(32.05)	(29.88)
Disaster Prop. Damage 1960-96 ($) × Rep on Sub.	-0.001*	-0.001*
	(0.0008)	(0.0007)
Demographic Interactions		
Nonwhite Population × Rep on Sub.	-0.004	-0.004
	(0.01)	(0.01)
White Population × Rep on Sub.	0.001	0.001
	(0.01)	(0.01)
Households Owners over 64 × Rep on Sub.	0.01	0.01
	(0.04)	(0.04)
Households Owners under 64 × Rep on Sub.	-0.01	-0.01
	(0.03)	(0.03)
Democratic President × Rep on Sub.	53.16	53.16
	(93.30)	(87.00)
Housing Value Prev. Year ($) × Rep on Sub.	-0.804	-0.804†
	(0.592)	(0.552)
Unemploy. Rate Prev Year (County) × Rep on Sub.	6.42	6.42
	(6.68)	(6.23)
Grant Interactions		
Cul. HMA Grants 1989-97 ($) × Rep on Sub.	0.02	0.02
	(0.063)	(0.059)
Disaster Controls		
Rainfall during Disaster (mm)	-0.01	-0.01
	(0.063)	(0.058)
Cum. Disaster Rain 1997+ (mm)	0.028	0.028
	(0.04)	(0.04)
Cat3 Storm Surge Height (ft)	-3.05	-3.05
	(2.67)	(2.48)
Disaster Prop. Damage 1960-96 ($)	3.16e-6	3.16e-6
	(4.58e-6)	(4.27e-6)
Demographic Controls (Zip)		
Nonwhite Population	-0.002	-0.002
	(0.005)	(0.005)
White Population	-0.004	-0.004
	(0.007)	(0.007)
Owner-Occupied Housing Units	-0.01	-0.01
	(0.01)	(0.01)
Urban Housing Units	-0.01*	-0.01**
	(0.007)	(0.006)
Rural Housing Units	-0.006	-0.006
	(0.007)	(0.006)

Data is from 1997-2002. Specifications 3 and 6 are similar to Table 4. All Grant Totals are in $ '000
Standard errors are clustered at the Congressional District Level.
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$

Table 6 (Cont.) **Results: Distortion Interactions**	OLS (Specification 3) Grant Total	Tobit (Specification 6) Grant Total
Demographic Controls (Cont.)		
Households Owners over 64	0.02	0.02
	(0.02)	(0.02)
Households Owners under 64	0.04^{*}	0.04^{**}
	(0.02)	(0.02)
Unemploy. Rate Prev Year (County)	-2.08	-2.08
	(2.73)	(2.55)
Housing Value Prev. Year ($)	0.05	0.05
	(0.16)	(0.15)
Aggregate Policy Controls (Zip)		
NFIP Policy Count	0.03^{***}	0.03^{***}
	(0.01)	(0.01)
NFIP Policy Coverage ($)	-1.41e-4*	-1.41e-4*
	(7.72e-5)	(7.19e-5)
Aggregate NFIP Deductibles ($)	0.003	0.003
	(0.006)	(0.005)
Electoral Controls		
Democratic President	-52.57^{*}	-52.57^{*}
	(31.47)	(29.35)
Democratic Governor	-8.409	-8.409
	(23.92)	(22.31)
Gov & President Same Party	-18.34	-18.34
	(30.01)	(27.98)
Gov & House Rep Same Party	$32.23^{\dagger}$	32.23^{*}
	(19.60)	(18.28)
House Rep & President Same Party	104.8^{***}	104.8^{***}
	(34.96)	(32.59)
House Electoral Competition	-8.298	-8.298
	(48.70)	(45.41)
H&P Same Party $\times$ Elec. Comp.	-128.5^{**}	-128.5^{**}
	(58.91)	(54.93)
G&P Same Party $\times$ Elec. Comp.	18.62	18.62
	(45.48)	(42.41)
Senate Subcommittee Controls		
Sen. Appro. Sub.	-176.7^{***}	-176.7^{***}
	(62.45)	(58.23)
Sen. Oversi. Sub.	$43.70^{\dagger}$	43.70^{*}
	(27.00)	(25.17)
Grant Controls		
Properties Count	2.409^{***}	2.409^{***}
	(0.840)	(0.784)
Cul. HMA Grants 1989-97 ($)	8.24e-5	8.24e-5
	(0.003)	(0.002)
Constant	168.2^{***}	362.4^{***}
	(46.63)	(65.98)
Fixed Effects		
State	✓	✓
Year and Congress	✓	✓
Congressional District	✓	✓
N	1969	1969

Data is from 1997-2002. Specifications 3 and 6 are similar to Table 4. All Grant Totals are in $ '000
Standard errors are clustered at the Congressional District Level.
† $p < .15$, * $p < .1$, ** $p < .05$, *** $p < .01$. Back To Text.

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