Does Flood Insurance Alter Location Incentives? Evidence from the National Flood Insurance Program

Abigail Peralta*

Jonathan B. Scott†

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Abstract

Despite the large costs of covering flood losses, little is known about whether the National Flood Insurance Program (NFIP) affects households' decisions to sort into more flood-prone locations. In this paper, we leverage the Federal Emergency Management Agency's lengthy, plausibly exogenous process of mapping risky communities as a necessary determinant of full entry into the NFIP, thereby granting eligibility to homeowners in these communities for highly subsidized flood insurance. We find that the NFIP had an overall positive effect on the population size of communities enrolling into the program, but a significantly larger impact on the relatively more flood-prone locations—causing an additional 5 percent increase in population per one standard deviation increase in historical flood risk. Our findings highlight the potential for nationally subsidized flood insurance to contribute to flood damages by altering incentives to reside in risky areas.

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*Louisiana State University; **aperalta@lsu.edu**

†Binghamton University; **jbscott@binghamton.edu**

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1 Introduction

Since the creation of the National Flood Insurance Program (NFIP), the U.S. government has paid out over $51 billion to cover flood losses. Almost half of these payouts went to just 25 counties, which are also among the fastest growing counties by population (Kane and Puentes, 2015). There are a number of potential explanations for this. The aesthetic appeal of coastal living may encourage households to take unnecessary risk by residing in flood-prone locations (Kahn, 2005; Boustan, Kahn, and Rhode, 2012). As most of US economic activity is concentrated on its ocean and Great Lakes coasts (Rappaport and Sachs, 2003), there are likely labor market incentives to locate in these areas. We focus on a previously unstudied factor: that insuring people against potential flood losses contributes directly to population growth in flood-prone areas.

The motivation to provide insurance against the consequences of flooding is clear. Globally, the costs of flooding are projected to increase over time, from $6 billion per year in the largest cities in 2005, to $52 billion per year by 2050 (Hallegatte, Green, Nicholls, and Corfee-Morlot, 2013). Nationally, severe weather-related disasters appear to be linked with increased out-migration, poverty, and lower home prices (Boustan, Kahn, Rhode, and Yanguas, 2020). Moreover, the economic effects of these disasters persist for years after. For example, Strobl (2011) finds that counties affected by hurricanes experience a significant reduction in their annual economic growth rate. In response, the federal government has offered significant financial assistance to victims of flooding. Deryugina (2017) finds that direct disaster aid provided to affected counties amounts to $155-$160 per capita, in addition to $780-$1,150 per capita from non-disaster social insurance programs in the ten years following a hurricane.

Given the amount of federal aid targeted toward flood victims, a natural question to ask is whether this coverage has encouraged households to take on unnecessary risk. In this paper, we examine whether NFIP insurance directly increases a household's propensity to locate in flood-prone areas, relative to less risky locations. This distortionary behavior would increase the burden on taxpayers, adding to the program's already existing inefficiencies (Kahn and Smith, 2017).

While the existence of these perverse incentives is an important policy question, isolating the causal channel requires that access to coverage was independent of confounding factors additionally impacting migration decisions in these communities. Doing so is difficult because a community's decision of whether to join, and when to join the NFIP is likely based on local-level factors. The resulting potential for selection into the program means that a naïve, direct estimation of the impact of NFIP adoption on population growth may produce biased estimates.

To overcome these issues, we leverage the Federal Insurance Administration's (FIA) emergency program and Federal Emergency Management Agency's (FEMA) subsequent roll-out of upgraded flood maps, which allowed these communities to enter the regular program, granting access to flood insurance at subsidized rates.[1] In the 1970s, the FIA hired multiple engineering firms to identify flood-prone communities, and had Flood Hazard Boundary Maps (FHBMs) prepared for them. These early maps indicate the existence of special flood hazard areas and allowed communities into the emergency program—providing them with a limited amount of coverage. Once FEMA was created in 1979, the process of updating these maps to Flood Insurance Rate Maps (FIRMs)—which mapped premium zones, in addition to previously identified special flood hazard areas—began.

We leverage this roll-out of initial FIRMs for high priority FHBM communities during our analytic period, 1990 to 2011, to estimate the impact of a community's entrance into the NFIP on household migration. Specifically, we examine how household migration behavior changes over time, exploiting the publication timing of the map upgrades, which granted entrance into the regular flood insurance program. We exploit the roll-out of initial FIRMs as an intervention that determines access to subsidized flood insurance for households, thereby allowing us to pin down the causal effects of the NFIP.[2]

Results indicate that households are sufficiently mobile such that the NFIP has a significant

[1] Because of the potential for households to endogenously purchase insurance, throughout, we examine enrollment at the community-level (ultimately aggregating to county), which is required before a household in that community can receive NFIP insurance. We discuss the difference between the emergency and regular programs of the NFIP, as well as mapping and enrollment processes in Section 2 and examine insurance take-up in Appendix E.

[2] We discuss in greater detail the roll-out of initial FIRMs in Section 2.

impact on a household's decision to locate in flood-prone areas. We estimate that population in communities who are encouraged to join the NFIP increases by 4 to 5 percent. This is in line with the increased out-migration following severe disasters estimated by Boustan et al. (2020), and comparable in magnitude to the 5 percent inter-county migration rates from Molloy, Smith, and Wozniak (2011). Our estimated effect is primarily driven by existing residents choosing to remain in flood-prone locations when flood insurance becomes available, where the counterfactual response would have been to migrate to less risky areas. By comparison, we find weaker evidence of increases in migration *into* the NFIP areas. This pattern of results is consistent with existing residents being more informed about the need for NFIP insurance and, thus, more responsive to its availability.[3]

As our estimates of the impact of NFIP insurance availability on migration reflect an average effect on the FHBM communities targeted by the NFIP—where the counterfactual is no insurance—these results say little about the relative role of flood risk. Building on our estimates of the average treatment effect, our heterogeneous estimates explain the relative effect of the NFIP on migration in communities across varying flood histories. This specifically allows us to address whether the NFIP has increased households' willingness to take on more risk. Results suggest that the NFIP produces an additional 5 percent increase in population for a one standard deviation increase in flood risk. As before, we attribute most of this effect to current residents choosing to stay in, rather than move out of more flood-prone areas.

This study contributes to the literature in several ways. Our study is closely related to Browne, Dehring, Eckles, and Lastrapes (2019), who find that housing development in Florida shifted from coastal to non-coastal counties following NFIP enrollment, in contrast with our main results. The authors theorize that their findings reflect a negative supply side response due to coastal counties having higher costs of compliance with flood mitigation requirements that offset any potential increased demand for housing from subsidized flood insurance. Although we focus on the residency effects of *initial* flood maps, our paper is also related to Gibson and Mullins (2020), who exploit updates to existing flood maps to examine

[3] In Section 2.3, we discuss institutional reasons against interpreting our results as being driven by amenity values of joining the NFIP.

the role of beliefs about flood risks in housing prices in New York City, and to Ben-Shahar and Logue (2016), who estimate perverse effects of subsidies to homeowners insurance for properties in Florida that face greater threat of severe weather. We complement these studies by providing comprehensive evidence that NFIP insurance affects population flows across the U.S. In complementing these studies, our findings provide causal evidence on questions posed by older studies (Government Accountability Office, 1982; Cordes and Yezer, 1998).

This paper also complements existing studies that document a positive relationship between flood risk and flood insurance demand (Kriesel and Landry, 2004; Landry and Jahan-Parvar, 2011; Gallagher, 2014; Kousky, 2017; Browne, Dehring, Eckles, and Lastrapes, 2019). Gallagher (2014) and Kousky (2017) find that flood insurance take-up increases in the year following hurricanes and large flooding events. Kousky (2017) attributes most of her result to the requirement that federal disaster aid recipients subsequently purchase flood insurance, while Gallagher (2014) interprets his result as increased flood risk salience, which is additionally documented in housing prices by Bakkensen, Ding, and Ma (2019).

More broadly, because our results indicate that people take on more risk following the introduction of a market for subsidized flood insurance, we contribute to the larger body of research on moral hazard in insurance markets. Such responses have been found to occur with health insurance (Spenkuch, 2012; Einav, Finkelstein, Ryan, Schrimpf, and Cullen, 2013; Keane and Stavrunova, 2016), life insurance (Cawley and Philipson, 1999), and automobile insurance (Dionne, Michaud, and Dahchour, 2013; Weisburd, 2015). In contrast to these papers that examine the mis-pricing of insurance in the presence of an agent's perverse, "hidden actions," we emphasize the government's role in intentionally pricing premiums below marginal damages to encourage take-up.

In addition to the increased costs incurred from past major disasters, the perverse incentives created by the NFIP play a major role in inhibiting adaptation to the future risks of climate change. We show that NFIP insurance adoption is a strong driver of population growth in high flood risk areas, adding to the already growing costs of increasingly frequent, climate change-driven natural disasters. Our findings provide strong evidence that household migration patterns are responsive to insurance markets, suggesting that flood insurance rates priced below actuarially fair levels will produce inefficient sorting to flood-prone locations—further hindering climate change adaptation.[4]

The remainder of the paper proceeds as follows: Section 2 discusses the relevant background of the National Flood Insurance Pogram. In Section 3, we discuss the data used in this paper. In Section 4, we lay out the theoretical framework for the mechanism in which national flood insurance may lead to increased risk taking. In Section 5, we present our empirical strategy to test for perverse incentives from flood insurance. In Section 6, we present estimates of the impact of the flood insurance program on migration. In Section 7, we test for relative risk taking by estimating heterogeneous effects of the flood insurance program across counties with different historical flood frequencies. Finally, in Section 8, we conclude with a brief discussion of our findings.

2 Background of the NFIP and Flood Maps

Our empirical strategy relies on an understanding of the institutional details behind the historical rollout of flood maps. In this section, we describe the relevant information surrounding the creation of the National Flood Insurance Program (NFIP) and early flood mapping efforts. We then describe the process by which the first flood maps, the Flood Hazard Boundary Maps (FHBMs) were upgraded to Flood Insurance Rate Maps (FIRMs), and how this upgrading process provides plausibly exogenous variation in access to NFIP insurance.

[4]Note that FEMA's flood insurance rate maps (FIRMs) currently map flood risks (often defined discretely by 1% and 0.5% annual flood risk and minimal flood risk zones) into insurance premiums. However, given that rates are still highly subsidized in these areas, our results argue that these below actuarially fair premiums only exacerbate risk taking behavior, beyond efficient levels. Due to endogeneity concerns (and data constraints) related to flood insurance premiums, we are not able to speak to the private benefit households receive from reductions in risk, and thus, we cannot speak to the NFIP's overall efficiency. Therefore, this paper focuses on the social marginal cost of the NFIP, in the form of induced population growth in flood-prone locations. We discuss this in more detail in Section 4.

2.1 Objectives of the National Flood Insurance Program

The NFIP was created through the National Flood Insurance Act of 1968. Before the NFIP, private insurers were largely unwilling to offer flood insurance, both because the necessary flood risk maps did not exist and because actuarially fair premiums were thought to be too expensive for prospective buyers (Anderson, 1974). The NFIP has two main goals: (1) provide access to flood insurance, and (2) to develop and enforce flood risk mitigation measures to reduce overall flood risk (National Research Council, 2015).

In 1973, the Flood Disaster Protection Act required the NFIP, through the Federal Insurance Administration (FIA), to identify and notify all communities at risk of severe flooding. For the purposes of the NFIP, a community is any governmental body that can pass and implement local development regulations. This definition includes cities, townships, and villages (Federal Emergency Management Agency, 2001). The NFIP communicated risks to these communities by publishing flood maps, a communication tool it still uses to this day (Kousky, 2018).

2.2 Flood Hazard Boundary Maps: The First Flood Maps

Initially, the NFIP planned to map an estimated 13,600 communities at risk of severe flooding by mid-1974 (American Institutes for Research, 2005).[5] With the help of private engineering firms, the NFIP was able to finish identifying flood hazards in these communities in 1978. The NFIP issued FHBMs, based on approximate data, to map flood risk in these riskiest communities. Compared to modern flood maps, FHBMs were relatively rudimentary and only indicated the existence of special flood hazard areas, or 100-year floodplains, as well as approximate flood boundaries, mudflow, and other hazardous areas within communities. We discuss this early mapping effort in further detail in Appendix A.

As provided for in the Housing and Urban Development Act of 1969, once given their FHBM, communities were allowed to enroll in the emergency phase of the NFIP. While a few of these communities may have entered the emergency phase as a result of a prior major flooding

[5]We requested this list of 13,600 communities from FEMA, but we were informed that those records are no longer available.

event, the main difference between the emergency phase and the regular phase is that coverage limits in the emergency phase are much lower than in the regular program. Today, the emergency program coverage limit for a single-family residential dwelling is $35,000, compared to $250,000 in the regular phase (Federal Emergency Management Agency, 2011). To become eligible for the regular phase—which grants full access to expanded coverage—communities had to wait for the FIA to complete a comprehensive flood insurance study and upgrade their flood maps to FIRMs. This responsibility was transferred to the newly created Federal Emergency Management Agency (FEMA) in 1979.

2.3 Upgraded Flood Map Publication as a Precursor to NFIP Enrollment

The process of upgrading FHBMs to FIRMs started in earnest during the 1980s. Because FIRMs are an upgrade, they are issued to the same jurisdiction that has a FHBM. About half of the flood-prone FHBM communities identified in the 1970s were given their initial FIRMs later in the 1970s, while many communities received their initial FIRMs much later (during our analytic period) delaying their full participation in the NFIP regular phase.[6] Depite the lag, FEMA consistently targeted FHBM communities to receive FIRM upgrades. By 2011—the end of our analytic period—less than 3 percent of FHBM communities were still waiting for their FIRM upgrade (Federal Emergency Management Agency, 2011).

Our empirical strategy leverages the timing of the initial FIRM upgrade to existing FHBM communities. In particular, communities that were upgraded to FIRMs had a strong incentive to enroll in the regular phase of the NFIP. Enrolling in the NFIP meant committing to floodplain management regulations set by the NFIP. These requirements are listed in Title 44 of the Code of Federal Regulations.[7] For individual properties, compliance means elevating the lowest part of the structure above the base flood level specified by the FIRM. Communities must also commit to limiting development within the floodplain. Data on community compliance with NFIP requirements is scarce, but is estimated to be about 70

[6] The data for our analysis begins in 1990. In addition, some FIRMs were created before 1979, but these were simply hand-drafted emergency maps made after a major flood (Morrissey, 2006).

[7] The current version can be found at: https://www.ecfr.gov/current/title-44

percent (Adler et al., 2019). As discussed by Browne et al. (2019), this has the effect of increasing property costs, especially for those required to purchase flood insurance. Since higher costs would deter households from residing in these areas, we do not expect our estimates to reflect amenities arising from NFIP participation.

Communities that did not wish to comply were sanctioned, mainly by being cut off from federal disaster aid.[8] These incentives make two types of communities more likely to join the NFIP: those with low costs of floodplain management, and those that anticipate the need for disaster aid, especially subsidized flood insurance. In Section 6.1, we show that most existing FHBM communities that received their initial FIRM upgrade enrolled in the regular phase of the NFIP within one year.

The criteria for updating maps were often inconsistent across FEMA regions, and only loosely dependent on emergency status and flood risk (Browne et al., 2019). As we discuss in more detail in Appendix G, the Government Accountability Office (GAO) in 1983 illustrated the lack of coordination in setting priorities for map upgrades:

> *"...we found that FEMA has not set any priorities for its mapping effort, allowing its various regions to select communities for mapping based on widely different criteria. This resulted in some undeveloped, relatively less flood-prone communities receiving rate maps, while other more flood-prone areas remained in the emergency program."*

Upgrading FHBM communities to receive FIRMs—a time and labor-intensive process due to the comprehensive flood insurance studies required—was constrained by available funds. Figure 1 plots the distribution of communities by the year of their FHBM against the average number of years before they were upgraded to FIRM. Figure 1 illustrates a natural backlog, where communities that received their initial FHBM at the peak of FHBM assignments waited the longest for their FIRM upgrade. For instance, communities that received their

[8] In the 1990s, two policy changes occurred that made flood map upgrades an even stronger predictor of future community NFIP enrollment and household take-up of NFIP insurance. In 1994, the Riegel Community Development Regulatory Improvement Act, which penalizes mortgage lenders that do not verify whether borrowers required to carry NFIP insurance actually do so, was passed. In addition, FEMA conducted Cover America, which was an extensive information campaign from 1994 to 2000.

FHBM just before 1975 waited almost 10 years on average to be upgraded to FIRMs, while communities that received their FHBM in 1970 only waited 3 years.

Compared to the old flood maps, FIRMs were detailed enough to divide communities into zones according to the level of flood risk.[9] This allowed premiums to vary by the risk-level of the zone in which the property is located. FIRMs also identified which properties are required to carry NFIP insurance (Morrissey, 2006), as NFIP insurance is mandatory for federally-backed mortgaged properties in the 100-year floodplain (Federal Deposit Insurance Corporation, 2019).

The upgraded flood maps resulted in greater awareness about the flood risks and sanctions from not joining the NFIP (Chivers and Flores, 2002). Figure 2 illustrates how community enrollment in NFIP increased from the 1990s onward. In Appendix E, we also show that the number of NFIP insurance policies in force significantly increased within communities following enrollment into the NFIP regular phase. By 1998, over 60 percent of homeowners with mortgages residing in the 100-year floodplain carried NFIP insurance.

The final push to create and update flood maps relevant to our analytic period began in 1997, when FEMA started the Flood Map Modernization Initiative, with the goal of transitioning from paper maps to digital flood maps. Combined with the National Flood Insurance Reform Act of 1994, which required that FEMA assess the need to revise and update flood maps once every five years, these two pieces of legislation motivate and finance much of the map upgrading process. Today, the NFIP covers about $1.2 trillion worth of property (Michel-Kerjan and Kunreuther, 2011). The coverage limits for communities in the regular phase of the NFIP are $250,000 for residential buildings and $500,000 for non-residential buildings (Burby, 2001).

[9] Data used in these studies included historical geophysical and environmental data, land and aerial surveys, and interviews with the local population. By law, future flood projections or factors that affect future flood risk such as expected population growth and development were not to be used (Technical Mapping Advisory Council, 2015; Pralle, 2017).

2.4 The Extent of Subsidies in the NFIP

NFIP insurance remains highly subsidized due to a combination of grandfathering and outdated flood maps (Congressional Budget Office, 2009, 2017; Kunreuther et al., 2017; Lee and Wessel, 2017; Horn, 2021; Wagner, forthcoming). In general, grandfathering rules allowed structures that were built to the correct standard at the time of construction to be insured at lower rates.[10] By the early 2000s, about 21 percent of all NFIP policies were subsidized, paying premiums 40 to 45 percent below what the NFIP considers full risk rates (Government Accountability Office, 2013).

Compounding the subsidization of premiums is that even what the NFIP considers full-risk rates are below what private insurers might charge. The Congressional Budget Office (CBO) attributes this to FEMA's reliance on historical data, rather than current and projections of future flood risk, and outdated data that do not reflect important topographical changes such as shoreline erosion and loss of wetlands (Congressional Budget Office, 2009, 2017). Estimates indicate that eliminating all subsidies, or allowing the private sector to price insurance, may result in an increase in the average premium for properties in flood-prone areas from around \$500 to over \$2,000 (American Institutes for Research, 2005; Lee and Wessel, 2017).

Several laws have since been passed to reform and ensure the fiscal soundness of the NFIP. In 2012, Congress passed the Biggert–Waters Flood Insurance Reform Act with the goals of further updating flood maps to more accurately reflect current conditions, and gradually increasing insurance premiums to full actuarial risk rates, especially for high-risk properties. Gibson and Mullins (2020) examine some of the impacts of these rate changes. However, public pressure soon led to the Homeowner Flood Insurance Affordability and the Consolidated Appropriations Acts of 2014, which replaced the insurance premium increases with a surcharge paid annually by all policyholders (Miller, Dixon, and Clancy, 2019). That efforts to reform the NFIP are still ongoing is one of the reasons we focus on the period 1990 to 2011, excluding 2012 onward, in this paper. We explore the implications of these reforms in

[10]The specific grandfathering rule that applies depends on when exactly the building was constructed. See https://www.fema.gov/node/404682 for details.

Section 8, in the context of our findings.

3 Data

3.1 Population

We obtain data on population flows for years 1990 through 2011 from county-to-county migration files published by the Internal Revenue Service, which they construct from individual tax returns received each year.[11] By tracking changes in addresses, the IRS is able to track the number of people making inter-county moves between two filing years, as well as the number of people that stay in the same county. Because tax returns are filed every year, the IRS data are arguably the best source of data on movers over a long time period (Hauer and Byars, 2019). As is standard, we construct all of our population outcomes using the number of exemptions to proxy for the number of people.[12] From these files, we construct our proxy for county-level population, which is the sum of non-migrants (residents that did not change counties) and inflow migrants (new residents who moved from another county). Importantly, although our data do not cover the first two decades of the NFIP, they do cover the entire period when flood map publication became a stronger predictor of NFIP adoption (see Figure 2).

There are some key limitations of the IRS data. First, for confidentiality the IRS does not report totals based on fewer than 10 tax returns. While this likely does not significantly affect the main results, it prevents us from conducting additional analyses, such as examining where all in-migrants are coming from, or where all out-migrants are moving to. For similar reasons, the IRS also does not report data for geographic units smaller than counties. Second, there were methodological changes after 2011 that led to an increase in the number of tax returns that were being counted in the county-to-county migration files. Because the resulting increase in tax returns was not uniformly allocated across counties (Pierce, 2015), we exclude the entire affected period from our main results. Third, these data will not reflect moves by those individuals not required to file an income tax return. Finally, the IRS

[11] See https://www.irs.gov/statistics/soi-tax-stats-migration-data. Data after 2011 are available, but generated under a different methodology.

[12] As per IRS documentation, the number of exemptions is often used to proxy for the number of individuals, whereas tax filings are used to proxy for the number of households. See https://www.irs.gov/pub/irs-soi/99gross_update.doc for more information.

data will not account for a household's potential choice of multiple counties, for example by purchasing vacation homes or other secondary residences. This is important to note given that vacation homes in high risk areas—such as waterfront properties—likely contribute to a significant share of flood damage payouts. Given this limitation, our results should only be interpreted as the causal effects on *primary* residency choice. We share this focus on primary residency choice with other papers that examine the effect of aggregate shocks on migration (e.g., Curtis, Fussell, and DeWaard, 2015; Wilson, 2020; and Saks and Wozniak, 2011 also use IRS data). In Appendix K, we show that we obtain similar results when using BEA population data, though these data likely suffer from the same shortcoming.

3.2 National Flood Insurance Program

Our empirical strategy requires that we know which communities were identified as risky at the start of the NFIP, and when they were pushed by FEMA to join. We obtain this information from the Community Status Book published by FEMA.[13] The data contain information on the publication dates of all community-level FHBMs and FIRMs, as well as the dates that communities adopted the NFIP or were sanctioned by FEMA for not joining the NFIP. Knowing the publication date of the FHBM is important because it allows us to identify the communities that were initially identified during the watershed analyses of the 1970s as having elevated flood risk. Likewise, the publication date of the initial FIRM tells us when the community was next encouraged by FEMA to join the NFIP.

We aggregate community-level information on map coverage to the county-level by constructing a variable for the fraction of a county's communities that have a FHBM.[14] An issue which sometimes arises when aggregating up to county is that communities and counties can overlap.[15] That is, counties may contain multiple communities, and communities may contain multiple counties. For simplicity, we treat each community as identical and aggregate communities within counties. In doing so, it is possible that one community appears in multiple counties. We expect any aggregation error to be small and, likely, only enter our

[13] See https://www.fema.gov/national-flood-insurance-program-community-status-book.

[14] FEMA defines relevant areas as "communities," and NFIP enrollment occurs at this level. Therefore, we maintain this terminology in this paper; however, these "communities" are simply towns and cities, or local municipals.

[15] This happens about 5.6 percent of the time.

explanatory variable as classical measurement error.

The lack of relevant population or migration data at the community level necessitates aggregation to county level. Further, without community-level population data, we are unable to weight communities by their population. Aggregating from communities up to counties also implies that we will have fractional treatments at the county-level. This means that our *FHBM* indicator will be a value between 0 and 1, indicating the fraction of communities within a given county with a FHBM. Similarly, our *post-FIRM* indicator will describe the fraction of a county that is in a period following FIRM assignment.

Figure 3 presents the distribution of counties by the fraction of their communities with a FHBM. More than 70 percent of all US counties have at least one community under FHBM, and 45 percent of counties are comprised entirely of FHBM communities. It is important to note that the FHBM classification only indicates the presence of a 100-year floodplain, not its size relative to the community. According to FEMA (1983), only 4% of the total U.S. land area is within the 100-year floodplain (Robinson, 2004; Maantay and Maroko, 2009). Nevertheless, a county's position in the distribution is correlated with its assessed flood risk relative to other counties. Table 1 presents a summary of statistics for our sample, suggesting that by the end of our sample period, 88 percent of counties are enrolled in the NFIP; exceeding that of the 78 percent originally assigned a FHBM.

3.3 Other Data

In addition to the primary data sources in this paper, we use a set of time-varying county-level controls to test the robustness of our estimates. We use county-level data on natural disasters from the FEMA Disaster Declarations Summary.[16] The data contain a list of counties for which the state governor requested and was granted a federal disaster declaration. A federal disaster declaration allows counties to receive disaster assistance. Over 75% of flood-related requests are approved, and the request for a federal disaster declaration requires documentation of damage assessments. The lack of information on declined requests

[16] See https://www.fema.gov/openfema-dataset-disaster-declarations-summaries-v1.

for disaster declarations is a potential limitation of using these data.[17] We also use data on historical flood episodes published by FEMA for our analysis in Section 7. FEMA generate the data by processing multiple datasets from the National Oceanic and Atmospheric Administration (NOAA), FEMA Individual Assistance (IA) and the NFIP.[18] FEMA's main source of flood records is Storm Data, a publication of NOAA, which reports the geographic coordinates of floods. FEMA then aggregates this to the county-level, making it arguably one of the most comprehensive sources of historical floods data. The distribution of mean annual flood episodes is plotted in Figure 4 for the full sample and for the set of counties with 100 percent FHBM coverage. The positions of Orleans Parish (of New Orleans, LA) and Harris County (of Houston, TX)—known for recent hurricanes Katrina and Harvey, respectively—are plotted for illustration.

We collect additional county-level data to conduct robustness checks. Building permits data, which include counts and dollar value, come from the U.S. Census Bureau and are used as a leading indicator of construction activity. Finally, we use county-level income and employment data from the Bureau of Economic Analysis. A summary of these data are also presented in Table 1.

4 Conceptual Framework

We begin with a stylized model of residency choice to illustrate the manner in which perverse incentives may arise in the context of a flood insurance program. The general framework is similar to those discussed in work on decisions involving a moral hazard, inherent in insurance coverage (e.g., Cutler and Zeckhauser, 2000; Chetty, 2008; Einav, Finkelstein, Ryan, Schrimpf, and Cullen, 2013; Bajari, Hong, Khwaja, and Marsh, 2014; Kowalski, 2015), but differs to the extent to which this becomes a sorting model, and to which utilization of the insurance becomes (by assumption) exogenous, conditional on enrollment (i.e., utilization

[17]See Gallagher (2014) for a discussion of other data on flooding events.

[18]See https://www.fema.gov/data-visualization/historical-flood-risk-and-costs. FEMA and the Federal Government cannot vouch for the data or analyses derived from these data after the data have been retrieved from the Agency's website(s) and/or Data.gov. NOAA defines a flood as occurring when water surface level rises to a gage height in which it begins to create a hazard to lives, property, or commerce. More information for what NOAA constitutes as a flood can be found here: https://www.weather.gov/aprfc/terminology.

comes from exogenous flood damages). To simplify the framework, we assume exogenous take-up of flood insurance. For an extended framework which accounts for the insurance take-up decision, see Appendix B.

Consider a possible choice of county, j, from a set of all possible counties $0, 1, ...J$, where $j = 0$ indicates the outside option of maintaining current residence. The household has income y and, conditional on insurance availability in county j, faces a premium of p_j. When insurance is not available, the household bears the full costs of monetized flood damages, r. Suppose r is non-deterministic, and the household forms their expectations according to the county-specific distribution of possible flood damages, $F_j(\cdot)$. Whether the household currently holds a flood insurance policy is defined by the variable $\eta \in \{0, 1\}$.

Sorting decision. We focus specifically on the household's choice of residence, in the framework of a residential sorting model. For a given vector of flood risks ($r_j \in \mathbf{r},\ \forall j \in \{0, 1, ...J\}$), the household maximizes utility across counties and a continuous composite good, x, subject to a budget constraint. That is,

$$\begin{aligned}
&\max_{j,x}\ u(x, j), \qquad\qquad j \in \{0, 1, ...J\} \\
&subject\ to \\
&p_x \cdot x + \eta \cdot p_j + (1 - \eta) \cdot r_j = y
\end{aligned} \tag{1}$$

where we assume that $u(\cdot, \cdot)$ is a continuous, quasi-concave function of its first argument, x. The budget constraint states that the household incurs the full cost of flood damages, r, when uninsured, or pays a premium, p, for insurance with certainty, if insured. For fixed choice of residence, j (and corresponding fixed flood-risk, r_j), the problem becomes a continuous problem in the composite good. Denote the argument that solves this problem $x^*\big(p_x, \tilde{y}(p_j, \eta, r_j), j\big)$, where $\tilde{y}(p_j, \eta, r_j)$ is the residual income function. For simplicity, we normalize the price of the composite good to one. Plugging the demand function for the composite good back into the utility function, and allowing r_j to enter as a random variable, we obtain the following modified optimization problem.[19]

$$\max_{j\in\{0,1,\dots J\}} \int \nu\Big(y - \eta \cdot p_j - (1-\eta)\cdot r,\ j\Big) dF_j(r) \tag{2}$$

where the j subscript is omitted from flood damages, r, to denote a random variable and $\nu(\cdot)$ is the indirect utility function. This paper specifically aims to test whether access to federal flood insurance, η, increases the household's preferences for flood prone locations. From a revealed preference perspective, a household increases their relative preference for flood prone locations when their marginal loss from additional flood damages is less when granted flood insurance. This implies the following inequality holds.

$$\frac{\partial}{\partial r}\left\{\nu\big(\tilde{y}(p_j,\eta,r),j\big)\Big|_{\eta=1} - \nu\big(\tilde{y}(p_j,\eta,r),j\big)\Big|_{\eta=0}\right\} > 0, \quad \forall j \in \{0,1,\dots J\} \tag{3}$$

Equation 3 illustrates an unintended result of flood insurance, whereby simply being insured makes flood prone locations relatively more appealing. However, when the rate structure fully incorporates the social costs of locating in these areas, the external costs associated with living in these areas are completely offset by the private benefits of insurance. In the context of the NFIP, where premiums are priced well below actuarially fair rates, these location incentives are beyond efficient levels. We illustrate this point in Figure 5.

Figure 5 depicts the inequality in Equation 3 for two locations, which have two discretely different levels of expected flood damages, $\bar{r} < \bar{r}'$. Suppose these expected flood losses

[19] We estimate the significant effect of NFIP introduction on household purchases of insurance in Appendix E, though not explicitly accounted for in this model is the possibility that NFIP community enrollment alters risk exposure for households that do not purchase a policy. For example, households might perceive a different risk distribution after the community enters the NFIP because disaster aid becomes available. In the context of this model, an altered *perceived* risk distribution would enter the household's decision by integrating utilities over the distribution of risk, conditional on NFIP: $F(r|\eta)$. We expect that this may play only a modest role due to the substantial differences in payouts. Disaster aid to households is capped at \$33,000, while flood insurance covers up to \$250,000 (Burby, 2001; Federal Emergency Management Agency, 2017a). Furthermore, after a household receives disaster aid, they are *required* to purchase flood insurance (Federal Emergency Management Agency, 2017b).

come from two symmetric distributions—represented here by bounds defined by the diagonal, dashed lines. In a setting where there is no market for flood insurance, the household maximizes expected utility over the distribution of flood damages. The household's expected utility under the two levels of expected risk, $\bar{r}$ and $\bar{r}'$, are represented by points A and A', respectively.

Points B and B' illustrate the introduction of a flood insurance market, where rates are set to the expected cost of flood damages. Under risk-aversion, there is a positive increase in utility between points A and B. Additionally, the household will receive a higher marginal utility of flood insurance in the greater risk zones (i.e., $B' - A' > B - A$). The marginal household deciding between the high risk and low risk locations (e.g., an attractive beach-front property versus a lower flood risk property with less amenities) chooses the high risk property. This is a perverse incentive created by the introduction of flood insurance, however, at actuarially fair rates, the accepted risk level is fully priced into premiums. Our setting differs in that the NFIP offers discounted rates and, thus, we should expect an even stronger response in the current market.

Consider the rate structure under the NFIP. Suppose premiums are set for each location at a fraction, $\theta \in (0, 1)$, of expected flood damages. Because residual income from this discounted insurance is greater than in the actuarially fair case (i.e., $y - \theta\bar{r} > y - \bar{r}$), the household receives a higher marginal utility of flood insurance under the lower rate. The mechanism is identical to a movement *along* the utility curve to a location of risk level $\theta\bar{r}$. Holding the household's risk exposure constant at this location, the reduced premium puts the household on a higher utility curve. The level of utility the household receives in the low-risk (with discount $\bar{r} - \theta\bar{r}$) and high-risk (with discount $\bar{r}' - \theta\bar{r}'$) locations are represented as points C and C', respectively. Thus, while risk averse preferences increase the incentive to locate in flood prone areas even under actuarially fair rates, at discounted rates the same mechanism suggests an amplified response.

The main focus of this paper is on the impact of the NFIP on migration in flood-prone locations. Importantly, as discussed in Section 2, flood insurance was often unavailable to communities prior to the NFIP. Therefore, this paper looks to identify the revealed preference analogs to the difference $A - C$, while points B are generally unobserved. These effects will, therefore, encompass the full effects of the NFIP. Section 5 discusses how we seek to uncover these estimates.

5 Empirical Strategy

This paper examines the perverse incentives created by the NFIP by testing whether households choose to live in locations with higher flood risk, following enrollment into the NFIP. This effect is largely motivated by Equation 3 (or $(C' - A') > (C - A)$ in Figure 5), which proposes that the marginal utility of flood insurance is largest on the most flood-prone locations. To test this inequality in a revealed preference setting, we focus on the extent to which historical risk level plays into a household's response to the NFIP. However, before examining these heterogeneous effects, we narrow our focus to identification of the average causal effects of the NFIP.

A community's entry (and timing of entry) into the program, is likely endogenous and correlated with other factors that might drive population growth. To overcome this problem, we exploit FEMA's direct targeting of specific communities following the Flood Disaster Protection Act of 1973. We use the FHBM assignments in the 1970s to isolate variation from areas with a higher propensity to enter the program. FHBM assignments allow entry into the emergency program of the NFIP. Years (and often decades) later, FEMA followed up with largely the same group of communities by upgrading them to FIRMs, which describe the rate structure communities would face upon enrollment into the regular program. In our sample, 99% of counties with at least one FHBM ultimately received a FIRM, providing evidence that this targeted group remained consistent over time.

Following rollout of initial FIRMs, communities were given one year to join the NFIP before being sanctioned. This combination is what we exploit as a plausibly exogenous incentive that induced many of these communities to enroll into the NFIP. This strategy should be robust to community influence and unobservables driving future changes in population, as available information suggests that initial FIRM assignment was independent of such factors (see Section 2). We demonstrate that our proposed instrument strongly predicts NFIP entrance by estimating the following first stage equation:

$$postNFIP_{cst} = \alpha \cdot postFIRM_{cst} \times FHBM_{cs} + x_{cst}\tilde{\beta} + \mu_t \times FHBM_{cs} + \tilde{\lambda}_{st} + \tilde{\gamma}_{cs} + \tilde{\varepsilon}_{cst} \tag{4}$$

where $postNFIP_{cst}$ indicates actual enrollment into the NFIP for county, c, in state, s, at year, t. This variable represents the fraction of communities in a county which have been enrolled and is, thus, between zero and one. $postFIRM_{cst} \times FHBM_{cs}$ describes the fraction of communities within a county assigned an FHBM (before the start of our data), that have also been assigned a FIRM at some time t or earlier. As we are interested in the average effect on the FHBM group, we omit a constant term for FIRM timing.[20] α estimates the relationship between FIRM issuance and actual take-up. x_{cst} is a set of time-varying, county-level controls, including information on building permits, employment, income, and natural disasters. The inclusion of these covariates empirically tests whether they are significant drivers of FIRMs, which in turn could bias our estimates. We expect that the extent to which these specific variables would be driven by FIRM assignment are derived from their impact on migration decisions, as the characteristics of a community are often determined by the population residing there. We allow counties with higher FHBM shares to follow different trends by including year-specific controls for FHBM, $\mu_t \times FHBM_{cs}$. Finally, we control for a set of state-by-year and county fixed effects, λ_{st} and γ_{cs}, respectively.

In the reduced form, we estimate the impact of the FIRM update for the emergency program communities, initially assigned a FHBM. As FIRM assignment for FHBM communities only partially encompasses enrollment into the NFIP, our approach is an instrumental variable design. Identification of the causal effects of the NFIP requires that changes in population over time in the FHBM counties would track closely with non-FHBM counties, absent

[20]Conditioning directly on FHBM communities is not feasible in our setting as we aggregate to county-level, producing fractions rather than binary indicators. In Appendix J, we condition on only counties with at least one FHBM and, additionally, weight observations by fraction of a county with a FHBM. In Appendix L, we examine an alternative to this approach which uses FIRM timing on the entire sample. Each alternative produces both qualitatively and quantitatively similar results.

FIRM assignment, *and* that these FEMA interventions affect our primary outcomes only through NFIP insurance enrollment. Controlling for time-specific effects on FHBM communities weakens our assumption about trends, but assumes the timing of the map update for the FHBM group was conditionally independent of confounding factors associated with population growth. These interactions allow identification to come from deviations between FHBM communities updated and those not currently updated to FIRMs. Additionally, since we control for county fixed effects, our approach should be robust to selection into the emergency program, as this process took place prior to the start of our data (1970s). An alternative to this approach, which makes use of FIRM assignment for the entire sample of communities produces similar estimates (see Appendix L).

As our primary outcome includes the migration decisions of households within a community, our identifying assumption might be violated if the FHBM communities have sufficient influence over the upgrade to FIRMs. If this were the case, communities may endogenously time their FIRM assignment with other factors that drive population growth, such as new construction and infrastructure projects.[21] The result would produce changes in migration outcomes, even in absence of the FIRM assignment, leading us to misinterpret our results as causal. As a robustness check, we control for building permits in some specifications—in case construction and community decisions linked to new construction, play a meaningful role in the FIRM assignment. This does not significantly alter our estimate. In addition, in Appendix J, we produce estimates that identify strictly off of the initial set of communities originally flagged by FEMA as flood-prone.[22] This approach also produces similar results.

Our primary estimating equation for the intent-to-treat (ITT) effect of the NFIP on population flows is the following.

[21]Through numerous discussions with FEMA national and regional representatives, as well as extensive review of the Federal Register and related documents, we have found no (anecdotal) evidence that communities have this level of influence over when FEMA updates FHBMs to initial FIRMs. A description of the process involved in upgrading the flood maps can be found in Section 2.

[22]We thank an anonymous referee for this suggestion.

$$migration_{cst} = \delta \cdot postFIRM_{cst} \times FHBM_{cs} + x_{cst}\beta + \mu_t \times FHBM_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \tag{5}$$

Our main outcome is the natural logarithm of population. We also decompose the estimated changes in population into the log- number of non-migrants each year, as well as the log-number of inflow migrants. δ is our coefficient of interest, estimating the ITT effect of the NFIP on the population outcomes. Causal interpretations for NFIP enrollment should be made on the scaled coefficient, δ/α.[23]

We interpret FHBM assignment as indicating the relatively riskier counties with a higher propensity to be treated. We identify effects on the FHBM group in order to isolate the plausibly exogenous wait time to initial FIRM assignment for these prioritized communities. A positive effect on population ($\delta > 0$) would suggest that NFIP insurance program increased the incentive to reside in the flood prone FHBM communities. Note, however, that this approach does not inform us of the causal effects of the program on the non-"treated", non-FHBM communities. To test whether this is purely a homogenous response to the NFIP program as opposed to one that depends on risk level, in Section 7, we examine heterogenous effects by historical risk level.

6 Baseline Effects of NFIP Enrollment

6.1 First Stage Estimates

We begin our analysis by testing the relevance of our instrument for NFIP enrollment. Conditional on receiving a FIRM, a community may endogenously time their entry into the NFIP. We mitigate these concerns by examining NFIP enrollment, only indirectly, through the influence of FEMA's assignment of FIRMs. As FHBM communities were strongly encouraged to join the NFIP following map upgrades, we should expect a near one-to-one

[23] We implement two-stage least squares estimation for our heterogeneous effects, presented in Section 7. While this portion of the paper focuses on the reduced-form effect of the flood maps, the second stage estimates for the NFIP can be obtained from the scaled coefficient. The following is the implied second stage specification, where δ and α are the coefficients in Equations 5 and 4, respectively.

$$migration_{cst} = \frac{\delta}{\alpha} \cdot postNFIP_{cst} + x_{cst}\beta + \mu_t \times FHBM_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst}$$

relationship.

Estimates for Equation 4 can be seen in Table 2. Standard errors are clustered by county to account for the possibility of serially correlated, within-county shocks (Angrist and Pischke, 2009). The estimates are highly significant, producing an F-statistic exceeding 100. This result assures us that our instrument is relevant. The estimates are not sensitive to time-varying effects on FHBM communities (Column 2), the inclusion of additional controls, where estimates do not change at the thousandths decimal place after controlling for building permits, employment, and income-related variables (Column 3), or declared disaster controls (Column 4). The first stage estimates suggest that 95 percent of counties targeted by FEMA enroll into the NFIP. As our first stage estimates are close to 1, we will interpret our reduced form estimates from Equations 5 as if they were estimates from the structural equations, though the scaled estimates of the effect of the NFIP will be slightly larger.

The dynamics of the first stage can be seen in Figure 6, where we regress NFIP on a series of lagged and leading terms of our instrument. The final lagged term represents the effect through the remaining periods of the data. These estimates illustrate the strong incentives that FEMA imposed on affected communities, inducing the majority of program enrollment in the first year following the intervention. This is to be expected as communities risk being cut off from federal disaster aid if they do not join the NFIP within one year of the map upgrade (see Section 2).

Insurance take-up is the most likely underlying mechanism for flood insurance encouraging residency in flood-prone areas; however, it is not the most obvious first stage outcome for our analysis. While it is likely that the NFIP induced flood insurance take-up by households, residency choice and insurance take-up are likely simultaneous actions. A household may choose its residency based on their insurance coverage, and will choose enrollment into insurance coverage conditional on residency. For this reason, we see the reduced-form effect of the NFIP program as the most obvious parameter of interest. In Appendix E, we establish a strong positive impact of the NFIP on the number of flood insurance policies.

6.2 Establishing the Validity of the Research Design

Before presenting our main results, we present evidence to support our parallel trends assumption. We do this by estimating a fully dynamic version of Equation 5, with several lagged and leading terms for FIRM assignment. Specifically, we estimate the following event study specification:

$$\begin{aligned} migration_{cst} = &\sum_{l=-\underline{L}}^{\bar{L}-1} \delta_l \cdot newFIRM_{cst-l} \times FHBM_{cs} + \delta_{\bar{L}} \cdot postFIRM_{cst-\bar{L}} \times FHBM_{cs} \\ &+ x_{cst}\beta + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \end{aligned} \tag{6}$$

where $\bar{L}$ is the number of lags and $\underline{L}$ is the number of leads. This event study specification estimates the dynamic effects at each point of FIRM assignment for FHBM counties, $newFIRM \times FHBM$, which indicates the fraction of communities who were in the emergency program (FHBM) and are updated to FIRMs at a given point in time. The final lagged term estimates the average effect for the remaining post-FIRM periods in the data. The leading terms serve as placebos, as we should not expect to see responses to future period treatments. Estimates from Equation 6 are presented in Figure 7 for three outcomes: population (Panel A), non-migration (Panel B), and out-migration (Panel C).[24]

The results do not suggest any significant evidence of anticipatory effects that would violate our identifying assumption. For example, if households in FHBM communities anticipated the map update, we would expect to observe an increase in population level—relative to the control—prior to the initial FIRM date.[25]

6.3 The Effect of the NFIP on Population Flows

Table 3 reports the reduced form estimates from Equation 5. Column 1 presents the estimates for our base specification, which only controls for county and state-by-year fixed effects. From Panel A, the reduced form estimates imply an effect on population of about

[24] In Appendix C, we present results from a naïve specification, which directly regresses migration on NFIP enrollment.

[25] Though the majority of the dynamic coefficients are not statistically significant in the event study specifications, this does not imply that they are not jointly significant. Our main specification—estimated in Section 6.3—will focus on the average effect in the post periods.

5%, or about a 5.25% when scaling by the first stage.[26] In Column 2, we allow FHBM communities to follow different trends by including year-specific FHBM controls, which do not seem to affect our estimates. In Column 3, we add time-varying county-level controls, including building permits, per-capita income, and unemployment rates. We do not see evidence of endogenous selection into FIRM assignment correlated with these observables. In Appendix F, we extend this sensitivity analysis to additionally include lags of these controls.

It is possible that, rather than responding to flood insurance availability, households are directly responding to previous major disasters, which encouraged entry into the NFIP. For example, Gallagher (2014) finds an increase in insurance take-up following a flood. In Column 4, we test whether this is a potential confounder by controlling for all water-related nationally-declared disasters. Since we obtain similar estimates, we conclude that such omitted natural disasters should not significantly bias our estimates.

In Column 5, we add a one year lead of our treatment variable as a falsification check. This directly tests whether counties diverge in population outcomes in the year before the NFIP. Consistent with the evidence we show in Figure 7, we see no effect prior to treatment. In Column 6 we introduce county-specific linear time trends. This specification serves as an additional robustness check and will account for any linear trends in unobservables which might be correlated with our instrument and the migration outcomes.

Though not statistically different from the estimates in our base specification in Column 2, including county-trends attenuates our estimate. This attenuation can arise if *post-FIRM* variation in population changes do not simply exhibit a sharp increase in levels, but additionally act upon this trend. In this case, county-trends will not only control for pre-FIRM variation in population, but also absorb some of the post-FIRM, treatment effect. This point is raised by Wolfers (2006) who notes that "a major difficulty in difference-in-differences analyses involves separating out trends from dynamic effects of a policy shock." This problem

[26] One might expect this effect to attenuate over time, as migrants move to areas who initially enrolled into the NFIP early on in the program. Estimates suggest that, if anything, the effect slightly grows. Interacting our treatment with year suggests that the marginal effect increases by almost 0.3% per year in the study period. This may be attributed to the time it takes households to adjust to a policy change, generating a growing dynamic response.

has also been discussed informally by Baum-Snow and Lutz (2011), Lee and Saez (2012), Williams (2014), Meer and West (2015), and more formally by Borusyak and Jaravel (2018).

In Column 7, we include two additional lags of FIRM timing to account for some potential dynamics in the treatment effect, which might be partially absorbed in the county-trend controls. The second lag in this specification is a lagged indicator of *post-FIRM* and, thus, estimates the treatment effect, two years following FIRM assignment. This specification additionally controls for an indicator representing the time of FIRM assignment, and its one year lag. To the extent to which the first two periods after FIRM assignment exhibit a growing treatment effect, separating the trending portion might help overcome some of the concerns that coincide with the inclusion of unit-specific trends. As expected, the magnitude increases for the final lagged term, approaching the estimate in our base specification.

Recent literature on difference-in-differences approaches with staggered treatment suggests that biased estimates may arise when treatment effects are heterogeneous across groups and time (Sun and Abraham, 2021; Goodman-Bacon, 2021; Sant'Anna and Zhao, 2020; de Chaisemartin and D'Haultfœuille, 2020; Wooldridge, 2021; Callaway and Sant'Anna, 2021; Borusyak, Jaravel, and Spiess, 2022). To address these concerns, in Appendix I, we utilize a new method developed by Borusyak, Jaravel, and Spiess (2022). This approach produces estimates on par with the estimates reported in Table 3.

Next, we decompose this effect on total population into two sources of variation: residents deciding not to move from one year to the next (non-migrants) and individuals moving into a new county (migrants). These results can be seen in Panels B and C, respectively. Our estimates suggest that most of the effect on population is coming from residents deciding not to move, where the counterfactual—absent the NFIP—would have been to move out. After scaling, this amounts to a 5.6% effect of the NFIP on non-migrants. Although we estimate a 3% effect of the NFIP on in-migration, these coefficients are estimated with less precision than our results for population and non-migrants. To put this into perspective, if inflow migrants were driving the entirety of the measured effect on population, we would expect an estimate of 71%.[27] This is because in-migration encompasses a much lower baseline level of variation in population than that of non-migration; i.e., it is a "flow" measure rather than a "stock" measure.[28] In contrast, an estimate of 5.4% is needed on non-migration in order to fully account for the changes in population, which is similar to our main results.

Our estimates illustrate a large impact of the NFIP on the long-run population of a community—estimated to increase by a magnitude of 5 percent beyond a similar non-NFIP community. In Appendix E we document a large reduced-form effect of FIRM assignment on the corresponding number of flood insurance policies in a county—illustrating the primary mechanism for these population increases. We also confirm these estimates using an alternative dataset—population counts from the BEA—in Appendix K. Given that the majority of the effect we see comes from a household's higher propensity to stay in their county of residence, we can compare these to the observed average year-to-year "non-migration" rate. From the IRS migration data, we estimate the average number of residents who stay in their county, as a fraction of the prior year's population, at approximately 94%.[29] This suggests year-to-year migration on par with our long-run impact of the NFIP.

7 Increased Risk-Taking as a Heterogeneous Response to the NFIP

In this section, we exploit further variation in historical flood risk within treated groups in order to measure heterogeneous effects of the NFIP. We interact NFIP enrollment with flood intensity within FHBM communities to detect the *additional* effect that the NFIP has in areas bearing higher risk. This approach estimates an effect analogous to the expression in Equation 3, which argues that an increased willingness to take on risk occurs from a disproportionate response to insurance in locations with higher relative flood risk.

In Figure 8, we split our data into below- and above-median number of annual flood episodes

[27] On average, non-migrants make up 93% of annual population levels and in-migrants make up the remaining 7%. Thus, to fully explain the estimated 5% increase in population, we would require a magnitude of $5/7 = 71\%$ increase in in-migration to occur.

[28] We explore the implications of estimates on population flow versus stock terms in Appendix G.

[29] Similarly, Molloy, Smith, and Wozniak (2011) document a 5 percent inter-county migration rate.

and plot the reduced form coefficients of Equation 6 for each subsample.[30] As with Figure 7, we see little evidence that either the low or high historical risk-level, treated counties diverge from the control before treatment. Figure 8 demonstrates that relatively higher-risk FHBM counties exhibit a greater divergence after map assignment, indicating that the majority of the effect of the NFIP comes from the highest-risk counties in our sample. This result is consistent with the condition in Equation 3.

Table 4 reports the heterogeneous effects of the NFIP across historical flood risk. Specifically, the following equation is estimated by two-stage least squares, instrumenting NFIP enrollment with map assignments.

$$\begin{aligned} \mathit{migration}_{cst} = &\delta_0 \cdot \mathit{postNFIP}_{cst} \\ &+ \delta_1 \cdot \mathit{postNFIP}_{cst} \times \mathit{flood\ risk}_{cs} \\ &+ x_{cst}\beta + \mu_t \cdot \mathit{flood\ risk}_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \end{aligned} \tag{7}$$

where $\mathit{flood\ risk}_{cs}$ is our measure of average annual flood episodes for a county-state, cs. We control for time-varying confounders specific to flood-prone areas, $\mu_t \cdot \mathit{flood\ risk}_{cs}$. All other pair-wise interaction terms are absorbed into state-by-year and county fixed effects, λ_{st} and γ_{cs}, respectively. δ_0 measures the constant treatment effect of the NFIP when *flood risk* is equal to zero.

In this specification, δ_1 is our coefficient of interest and is analogous to the term in Equation 3 (and differential effects depicted in Figure 5). Specifically, δ_1 measures the additional impact of the NFIP coming from one additional flood per year. That is, while the primary estimates presented in Section 6 report that households generally favor communities enrolled in the NFIP, δ_1 tests whether households receive a larger benefit in the relatively riskier areas. Note that while the main results estimate a treatment effect on FHBM communities, which tend to be of higher flood risk, identification of δ_1 will come from variation in flood risk, *within* these FHBM communities.

The two-stage least squares estimates of Equation 7 are reported in Table 4. Column 1 presents our base specification. Demographic controls are added in Column 2 and declared

[30] The median annual flood episode is about 0.82.

disaster controls in Column 3. The results in Panel A suggest that areas with one additional flood per year have an additional effect of about 3.6% on population. With an annual flood risk standard deviation of 1.45, these estimates suggest that the NFIP has an additional impact of about 5 percent for a one standard deviation increase in flood risk.[31] This result fully characterizes the types of incentives that flood insurance produces, illustrating that the NFIP yields its largest effects in the riskiest of counties.

As with our main results, we decompose the population into the number of non-migrants, in Panel B, and in-migrants, in Panel C. Similar to our results in Table 3, most of the effect seems to come from the increased propensity of existing residents to stay in risky counties, as opposed to the outside influence of in-migrants. For our primary specification, we estimate significant effects of 4-4.5% on the number of non-migrants. Though we lack statistical power, our estimates still imply a meaningful 1.6% effect on the number of in-migrants per additional average floods per year. Importantly, our estimates also suggest that there is no statistically significant effect when a community's expected flood risk is zero (indicated by the estimated coefficient on $postNFIP$).

In Table 5, we introduce interactions with the year 1990 values of various county characteristics; mainly, per-capita income, job counts, and building permits. Each specification controls for a year-specific interaction with the characteristics included in the regression. This exercise attempts to test the extent that other observable attributes may be driving the differential effects between low- and high-risk locations. Some characteristics exhibit evidence of their own effects. For instance, counties with a higher baseline number of building permits had a higher response to flood insurance availability. This is not surprising given the potential constraints on housing in some communities. Overall, however, there is no indication that these characteristics are significantly driving the disparities between high- and low-risk communities. This finding argues in favor of actual internalization of flood risk on the part of households.

[31] Note that the standard deviation of flood risk differs here from the standard deviation presented in Table 1, since we are using the average floods over time in this specification, whereas Table 1 presents summary statistics for the county-by-year panel.

Assuming flood damages are proportional to population size, our estimates suggest that this NFIP-induced migration has been responsible for significant costs from major historical floods, such as those coming from Hurricanes Katrina and Harvey. Given that Orleans Parish (New Orleans), Louisiana ranks in the 75th percentile in historical flood risk (see Figure 4) in our sample, our estimates suggest that the NFIP contributed to a 6.6 percent increase in costs attributed to Hurricane Katrina. As for Harris County (Houston), Texas, which ranks outside the 90th percentile in historical flood risk, we calculate that the NFIP was responsible for a 14 percent increase in damages from Hurricane Harvey.

To the extent to which the counterfactual of nationally subsidized flood insurance is private flood insurance, priced at actuarially fair prices, our results also speak to the impact of under-priced insurance. However, because flood insurance was largely unavailable in many of these communities prior to the NFIP, we are unable to directly disentangle the efficiency impact of reduced risk from the additional distortionary behavior produced from inefficient pricing (Knowles and Kunreuther, 2014). Our results can only suggest that households are sufficiently mobile to respond to the incentives of flood insurance in their sorting decisions and that subsidizing premiums (unintentionally) exacerbates the risky behavior we uncover.[32] Further, if the intention is to provide the right incentives to adapt to the future risks of climate change, it is important that policymakers account for these altered location incentives produced by the NFIP when managing rates.

8 Discussion and Conclusion

This paper presents evidence of costly, unintended consequences produced by the NFIP. This program provides highly subsidized flood insurance, securing households against expensive damages from future floods. Our findings show that population increases in flood-prone areas as a direct response to community enrollment into the NFIP. Moreover, we provide evidence of induced risk taking by demonstrating that the NFIP causes larger population increases in historically riskier areas. Thus, our findings suggest that the private benefit households receive in the form of a reduction in potential risks produces adverse behavior,

[32] A direct examination of how NFIP disproportionately affects counties with above average discounts on premiums is covered in Appendix A.2.

imposing significant external costs.

The growth of communities in flood-prone regions of the U.S. produces significant costs following major disasters. This type of behavior has large implications in the presence of climate change and rising sea levels. Shorelines in the U.S. account for only 10% of land area, yet the populations residing there make up nearly 39% of the total U.S. population (NOAA). As climate change risks inevitably increase the occurrences of future floods, the population will need to adapt in an effort to mitigate these risks. This may mean developing in less risky areas.

Maintaining inefficiently low rates for flood insurance provides consistent incentives to rebuild and reside in areas with high risk. Our results show that households are mobile, producing costly behavior from NFIP insurance which may only be exacerbated by the low rates offered through the program. Given our estimates on population growth, our results suggest the external costs produced by the NFIP may have contributed to a 6.6 percent increase in damages from Hurricane Katrina, and up to a 14 percent increase in damages from Hurricane Harvey.

Adaptation will certainly be a necessary component of the response to climate change, as the number of major disasters and flood losses are anticipated to increase (Michel-Kerjan and Kunreuther, 2011). This means accounting for some of the perverse incentives created by the NFIP. If policy is intent on providing the right incentives to encourage adaptation to future risks of climate change, it must consider the unintended behavioral responses to national flood insurance. With growing concerns over the financial sustainability of the NFIP, this may mean restructuring the program sooner rather than later.

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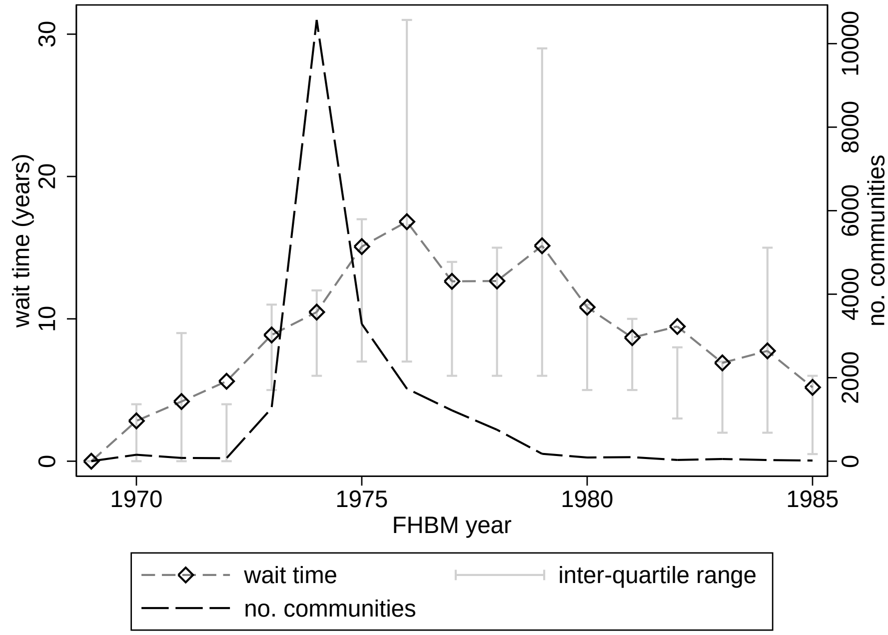
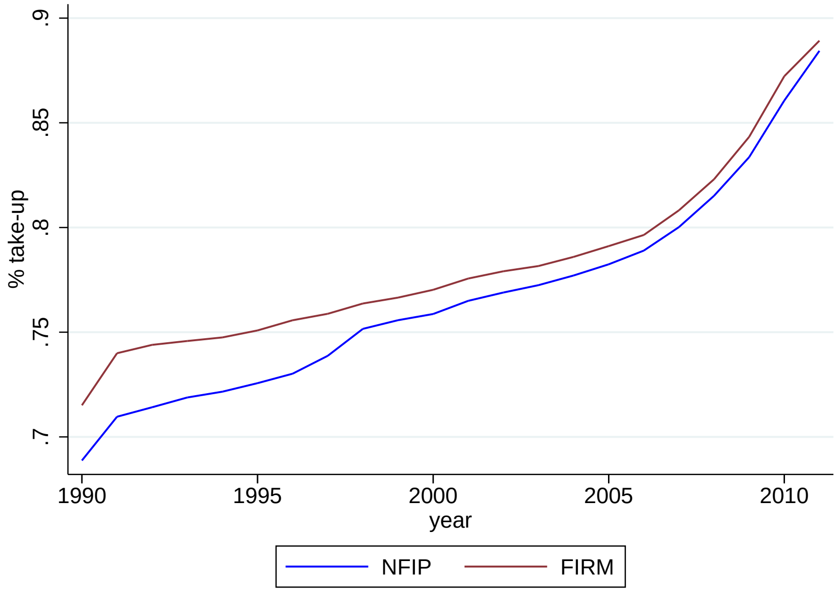
Figures

Figure 1: FIRM Wait Time and Distribution of Pending FIRMS by FHBM Year



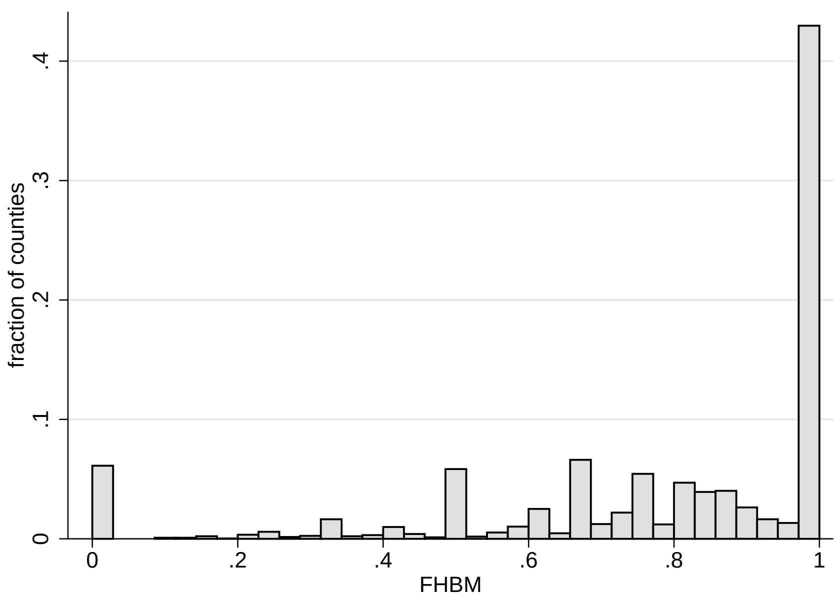
Note: This figure plots the average number of years that a FHBM community waited to be assigned their FIRM upgrade, plotted over their FHBM assignment year (wait time). Vertical bars indicate the inter-quartile range of wait time. The long dashed line represents the number of communities within each FHBM year bin.

Figure 2: Timing of FIRM Assignment and NFIP Enrollment



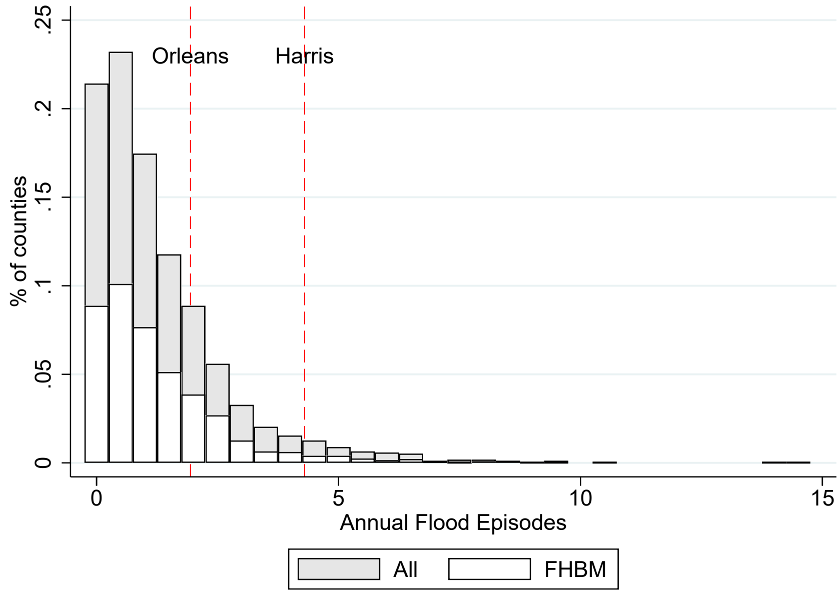
Note: This figure plots the cumulative fraction of counties enrolled in the National Flood Insurance Program (NFIP) and assigned a flood insurance rate map (FIRM) during the timespan of our data.

Figure 3: Distribution of FHBM Counties



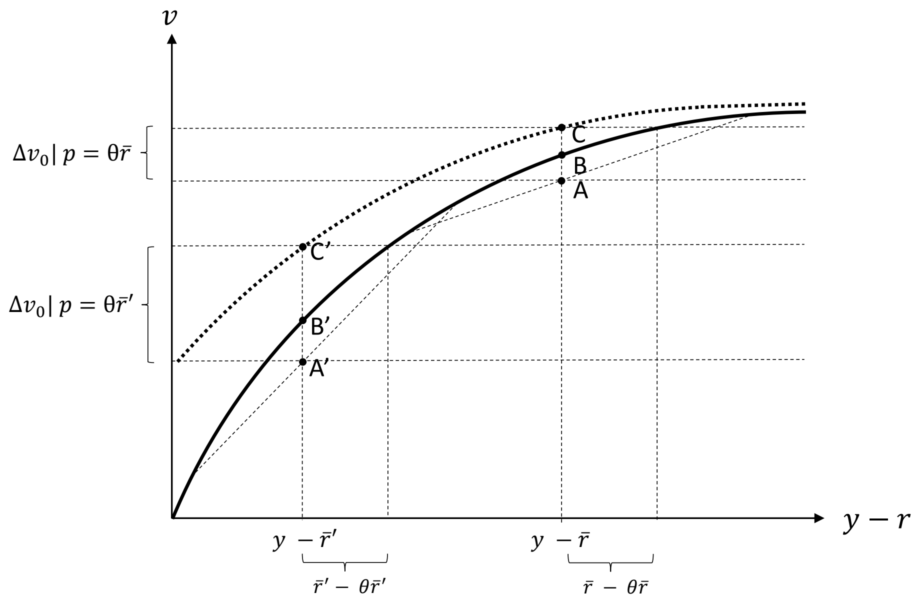
Note: This figure plots the distribution of fractions of a county (communities within a county) which FEMA has identified as flood-prone by publishing a Flood Hazard Boundary MAP (FHBM) for them. The vast majority of these assignments occurred in the 1970s. Our analytic sample include all 50 states.

Figure 4: Distribution of Flood Risk Across Counties



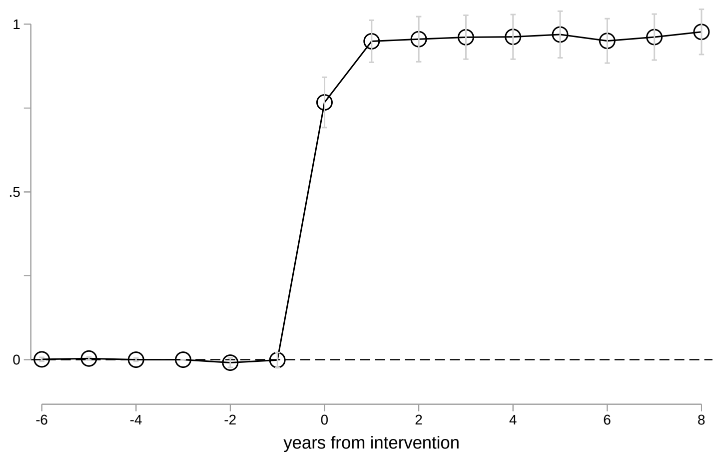
Note: This figure plots the distribution of county flood risk, defined as historical annual flood episodes (reported by the National Oceanic and Atmospheric Administration (NOAA)). Frequencies are plotted for counties that are 100% covered under an FHBM, and for the full sample. The data are derived from the years 1996-2011. NOAA defines a flood as occurring when water surface level rises to a gage height in which it begins to create a hazard to lives, property, or commerce. For illustration, the position of Orleans Parish (of New Orleans, LA) and Harris County (of Houston, TX) are denoted in the figure. These counties are prominently known for the major disasters of Hurricane Katrina and Harvey, which occurred in each county, respectively.

Figure 5: Marginal Utilities from Flood Insurance



Note: A utility function for household location choice is illustrated, depicting the marginal effect of flood insurance under two discrete levels of expected risk, $\bar{r} < \bar{r}'$, coming from two different distributions of flood risk. The figure depicts a representative household's level of (expected) risk along the horizontal axis, under three different scenarios: (A & A') no market for flood insurance, (B & B') flood insurance under actuarially fair premiums, and (C & C') flood insurance under subsidized rates. The marginal effect of interest is the marginal utility from no insurance, to subsidized flood insurance, at a discount of $1 - \theta \in (0, 1)$.

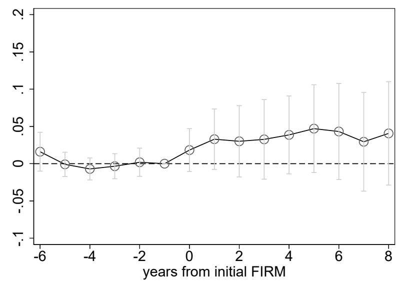
Figure 6: First Stage: The Effect of FEMA Intervention on NFIP Enrollment



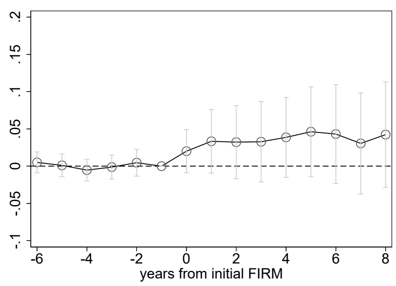
Note: This figure presents the dynamic coefficients from our first stage regression of the effect of post-FIRM*FHBM intervention by FEMA—which granted affected areas a 1 year grace period before sanctions were imposed—on actual NFIP enrollment. 95 percent confidence interval bars are presented. Standard errors are clustered by county.

Figure 7: Event Study Specification for Intent to Treat of NFIP

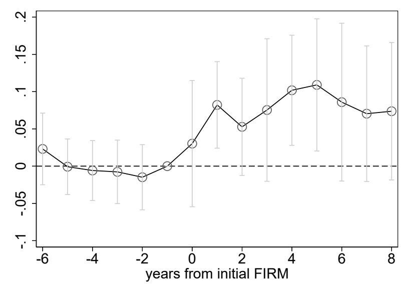
(a) Effect of NFIP (ITT) on (log) Population



(b) Effect of NFIP (ITT) on (log) Non-Migrants



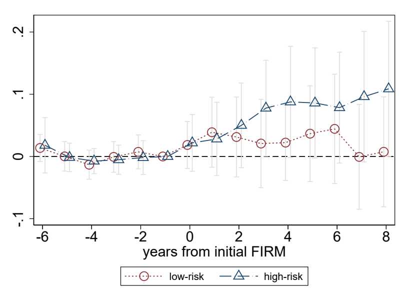
(c) Effect of NFIP (ITT) on (log) In-Migrants



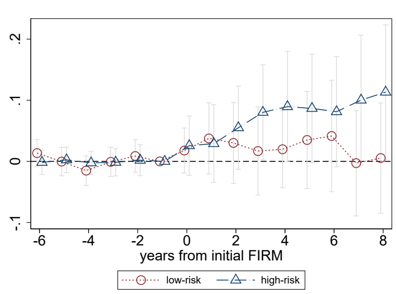
Note: This figure plots the reduced-form coefficients from Equation 6, using lagged and leading Flood Insurance Rate Map terms in our instrument. Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented.

Figure 8: Heterogeneous Effect of NFIP (ITT), by Flood Risk

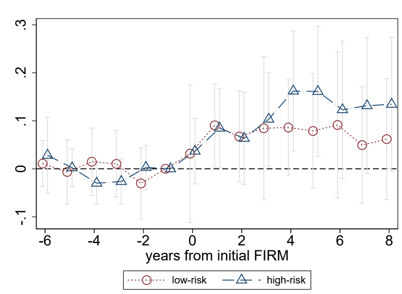
(a) Effect of NFIP (ITT) on (log) Population



(b) Effect of NFIP (ITT) on (log) Non-Migrants



(c) Effect of NFIP (ITT) on (log) In-Migrants



Note: This figure plots the reduced-form coefficients from Equation 6, using lagged and leading Flood Insurance Rate Map terms in our instrument, from two separate samples—below (low-risk) and above (high-risk) median risk, defined by annual historical flood episodes. Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented.

Tables

Table 1: County Characteristics

NFIP	0.884
	(0.241)
FHBM	0.780
	(0.280)
Total Exemptions	69,429.4
	(213,308.7)
Non-Migrant Tax Exemptions	65,243.4
	(204,297.8)
Migrant Tax Exemptions	4,184.4
	(10,385.3)
Annual Flood Episodes	1.506
	(2.411)
Water-Related Declared Disasters	0.148
	(0.460)
Building Permits (Housing Units)	394.3
	(1471.5)
Total Value of Units ($ mil)	48.870
	(193.998)
Per-Capita Income ($)	24,710.6
	(8,982.1)
Unemployment Rate	6.439
	(3.354)

Note: Sample means and standard deviations (in parentheses) are presented for our full sample of counties (1990-2011). NFIP represents the proportion of communities that ultimately enrolled in the National Flood Insurance Program. FHBM represents the proportion of communities assigned a flood hazard boundary map.

Table 2: First Stage: The NFIP enrollment on FIRM and FHBM assignment

Post-NFIP	(1)	(2)	(3)	(4)
postFIRM × FHBM	0.950***	0.952***	0.952***	0.952***
	(0.0163)	(0.0157)	(0.0157)	(0.0157)
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year-by-FHBM		Yes	Yes	Yes
Controls			Yes	Yes
Declared Disaster Controls				Yes
N	64472	64472	64472	64472

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Note: Above are the first stage estimates which regress National Flood Insurance Program (NFIP) enrollment on Flood Hazard Boundary Map (FHBM) and Flood Insurance Rate Map (FIRM) assignment. Standard errors in parentheses are clustered by county.

Table 3: Effect of Flood Insurance on Migration (Reduced Form)

Migration Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Log- Population							
$\text{postFIRM}_t \times \text{FHBM}$	0.0503***	0.0506***	0.0480***	0.0481***	0.0498***	0.0262*	
	(0.0173)	(0.0173)	(0.0163)	(0.0163)	(0.0183)	(0.0141)	
$\text{newFIRM}_{t+1} \times \text{FHBM}$					0.00815	0.00237	0.00330
					(0.0130)	(0.0100)	(0.00902)
$\text{newFIRM}_t \times \text{FHBM}$							0.0159
							(0.0119)
$\text{newFIRM}_{t-1} \times \text{FHBM}$							0.0188
							(0.0167)
$\text{postFIRM}_{t-2} \times \text{FHBM}$							0.0413**
							(0.0201)
Panel B: Log- Non-Migrants							
$\text{postFIRM}_t \times \text{FHBM}$	0.0533***	0.0537***	0.0508***	0.0509***	0.0533***	0.0300**	
	(0.0177)	(0.0178)	(0.0167)	(0.0167)	(0.0188)	(0.0141)	
$\text{newFIRM}_{t+1} \times \text{FHBM}$					0.0113	0.00616	0.00549
					(0.0133)	(0.0101)	(0.00903)
$\text{newFIRM}_t \times \text{FHBM}$							0.0182
							(0.0117)
$\text{newFIRM}_{t-1} \times \text{FHBM}$							0.0229
							(0.0167)
$\text{postFIRM}_{t-2} \times \text{FHBM}$							0.0420**
							(0.0200)
Panel C: Log- Migrants							
$\text{postFIRM} \times \text{FHBM}$	0.0340*	0.0342*	0.0335*	0.0336*	0.0303	0.00709	
	(0.0188)	(0.0189)	(0.0181)	(0.0181)	(0.0204)	(0.0233)	
$\text{newFIRM}_{t+1} \times \text{FHBM}$					-0.0156	-0.0235	-0.0117
					(0.0176)	(0.0182)	(0.0173)
$\text{newFIRM}_t \times \text{FHBM}$							0.00537
							(0.0233)
$\text{newFIRM}_{t-1} \times \text{FHBM}$							-0.0112
							(0.0276)
$\text{postFIRM}_{t-2} \times \text{FHBM}$							0.0509
							(0.0326)
N	64473	64473	64473	64473	64473	64473	59397
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × FHBM		Yes	Yes	Yes	Yes	Yes	Yes
Controls			Yes	Yes	Yes		
Declared Disaster Controls				Yes	Yes		
County Time Trend						Yes	Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Note: Above are the OLS estimates of reduced-form Equation 5 of flood hazard boundary map (FHBM) and flood insurance rate map (FIRM) assignment on outcomes log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c). Column 2, includes time-specific constants on our FHBM measure. County-level demographic controls in Columns 2-5 include building permits, total value of permitted housing units, per-capita income, unemployment rate, and job counts. County-level water-related declared natural disasters are included in Columns 4-5. Leading terms of treatment are included in Columns 5-7 as a falsification, and county-specific time trends are included in Column 6 and 7, with lagged treatment in Column 7. Standard errors in parentheses are clustered by county.

Table 4: Heterogeneous Effect of NFIP, by Flood Risk

	Migration Outcome		
	(1)	(2)	(3)
Panel A: Log- Population			
postNFIP	0.00428	-0.00107	-0.00103
	(0.0285)	(0.0275)	(0.0275)
postNFIP × Annual Floods	0.0360**	0.0393**	0.0393**
	(0.0181)	(0.0169)	(0.0169)
Panel B: Log- Non-Migrants			
postNFIP	0.000467	-0.00566	-0.00564
	(0.0301)	(0.0292)	(0.0292)
postNFIP × Annual Floods	0.0411**	0.0448**	0.0449**
	(0.0197)	(0.0187)	(0.0187)
Panel C: Log- Migrants			
postNFIP	0.0163	0.0174	0.0175
	(0.0304)	(0.0295)	(0.0295)
postNFIP × Annual Floods	0.0148	0.0139	0.0139
	(0.0189)	(0.0180)	(0.0180)
N	64472	64472	64472
County FE	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes
Year X Floods	Yes	Yes	Yes
Controls		Yes	Yes
Declared Disaster Controls			Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Note: Above are two-stage least squares estimates of the heterogeneous effects from Equation 7. Coefficients of interest are on the additional impact of the NFIP from one additional flood per year ($postNFIP \times Annual\ Floods$). *Annual Floods* refers to the average number of flood episodes per year, within a county. Standard errors in parentheses are clustered by county.

Table 5: Heterogeneous Effect of NFIP from Other Characteristics

Outcome: Log- Population	(1)	(2)	(3)	(4)
postNFIP $\times$ Annual Floods	0.0393**	0.0354**	0.0406**	0.0465***
	(0.0169)	(0.0166)	(0.0168)	(0.0162)
postNFIP $\times$ Income		0.0544*	0.0584**	0.0444
		(0.0295)	(0.0293)	(0.0279)
postNFIP $\times$ Jobs			-0.0617	-0.519*
			(0.0933)	(0.308)
postNFIP $\times$ Building Permits				0.369*
				(0.208)
N	64472	64472	64472	64472
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year X Floods	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Declared Disaster Controls	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Note: Each column introduces an additional interaction with a *baseline* (i.e., 1990) county characteristic. All characteristics, except floods, are in standard deviations. Interactions with year and the baseline characteristic are controlled for in all specifications. Standard errors in parentheses are clustered by county.

Appendix

A Flood Maps and the Emergency and Regular Phases of the National Flood Insurance Program

A.1 Institutional Details

This appendix provides further details about the process by which the NFIP created and upgraded flood maps, and how that relates to the flood insurance available. As discussed in Section 2, the FHBMs only identified communities that contain areas with high flood risk. Once assigned their original FHBM, and thus qualifying for limited forms of flood insurance, a community was required to wait for a comprehensive flood insurance study to take place before receiving their initial FIRMs, thereby qualifying them for the regular program.

Upgrading flood maps took many years because of the extensive data requirement, which included geophysical and environmental data, land and aerial surveys, and interviews with the local population. Available documents from present-day mapping initiatives indicate that the priority-setting process for map upgrades took into account topographic and flood hazard data, in addition to the age of any existing flood maps,[33] though the General Accounting Office (1983) suggested that the process was often sporadic (emphasis added):

> *"If the mapping effort is extended, we believe that the Congress, either through legislation or committee report, could require FEMA to review each community and select the optimum conversion method which balances the extra information obtained by detailed mapping against the need for that information when less costly alternatives are available. This action is important because we found that* ***FEMA has not set any priorities for its mapping effort, allowing its various regions to select communities for mapping based on widely different criteria. This resulted in some undeveloped, relatively less flood-prone communities receiving rate maps, while other more flood-prone areas remained in the emergency program."***

[33] (e.g., North Carolina State Mapping Program , 2001; California Department of Water Resources, 2002; Indiana Department of Natural Resources, 2004; Dudley and Schalk, 2005; Vermont Agency of Natural Resources, 2005)

The flood insurance studies relied on historical data and by law could not be based on future flood projections, or factors that affect future flood risk such as expected population growth and development (Technical Mapping Advisory Council, 2015; Pralle, 2017).[34]

A.2 Relationship Between FHBM and FIRM Publication Timing

In this section, we use flood map publication dates and document the backlog of pending FIRM upgrades. Figure A.1 depicts the distribution of the communities' FHBM assignment year over their subsequent FIRM assignment year, during the time-frame of our data. Scatter points are proportional in size to the number of observations within a FHBM-year/FIRM-year bin. Though there appears to be no significant correlation between the timing of these two dates (depicted by the fitted line with slope of 0.001 and t-stat of 0.43), this figure does illustrate a negative relationship between the FHBM to FIRM "wait time" and the community's FHBM assignment year.

Because FHBMs are essentially preliminary flood maps that FEMA intends to upgrade to FIRMs, it is possible that this observed decreasing wait time is related to the timing in which communities "enter" and "exit" the "queue." Figure 1 plots the distribution of communities by the year of their FHBM and the corresponding mean number of years waited before receiving their FIRM. This indicates that communities receiving their FHBMs just after the peak in assignments had the longest wait times, while wait times dropped for FHBMs issued even later. This resembles a natural backlog, indicative of communities in queue for map assignment, where the bulk of our FIRM assignments fall on the decline in wait time. This is unsurprising given the constraints on the number of studies FEMA was able to conduct per year (Comptroller General of the United States, 1976).[35] The General Accounting Office (1983) describes the reasoning for this backlog in more detail:

"Given that the 1968 act provided 15 years for developing the rate maps and that

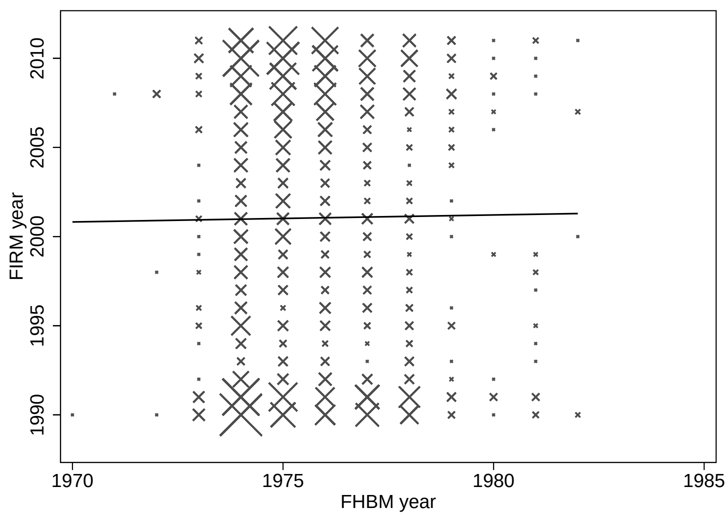
[34]The Technical Mapping Advisory Council recommended that FEMA incorporate future flood projections into mapping in late 2015, meaning any changes would have been made in 2016 or later.

[35]The Comptroller General of the United States (1976) also discusses the backlog in FHBM assignments, where the FIA stated that "the primary reason all identified flood-prone communities were not identified by the June 30, 1974 deadline was the increasing number of communities identified with flooding problems."

over $606 million has been appropriated for mapping, the question arises: What has prevented FEMA from developing flood insurance rate maps for all the Nation's flood-prone communities? Our review has suggested several factors.

An initial factor was the unexpected magnitude of the undertaking. When the 1968 act was passed, it was estimated that there were about 5,000 flood-prone communities in the Nation. However, as the process of identifying flood-prone communities proceeded, the total proved to be over 20,000, or four times the original estimate. Eighty-seven percent of those communities elected to join the program."

Figure A.1: Relationship between FHBM and FIRM Timing



Note: This figure depicts the relationship between the timing of community map assignments for FIRMs and FHBMs. Scatter points are binned by year, with size proportional to frequency observed in the data. The fitted line estimates the correlation between the two mapping times, with a resulting slope of 0.001 and corresponding t-stat of 0.43.

B Extended Conceptual Framework

This section expands on the framework in Section 4 by allowing for endogenous take-up of flood insurance. We develop a two-period framework. In the first period, a utility-maximizing household chooses a county of residence, conditional on their expectation of potential flood damages and coverage availability. In the second period—conditional on the availability of NFIP insurance—the household then decides whether to enroll. If the household does not enroll in NFIP insurance, or if NFIP insurance is not available in the county of choice, the household bears the full cost of flood risk.

As this is a household-specific decision, we omit individual-level subscripts for convenience. Consider a possible choice of county, j, from a set of all possible counties $0, 1, ...J$, where $j = 0$ indicates the outside option of maintaining their current residence. The household has income y, and, conditional on insurance availability in county j, faces a premium of p_j, and out-of-pocket expenditures defined as a function of the potential (monetized) flood damages, $c(r)$. That is, under the scenario of no insurance, the household bears the full costs of potential floods, r, but is only subject to a co-payment of $c(r)$ when enrolled in the program. In the simplest case, this co-payment is a linear function (i.e., $c(r) = c \cdot r$, where $c \in (0, 1)$). Suppose r is non-deterministic, and the household forms their expectations according to the county-specific function $F_j(\cdot)$. The *availability* of NFIP insurance in county j is defined by the variable $\eta_j \in \{0, 1\}$, and the enrollment decision—conditional on availability—is defined by $e_j(\eta_j)$.

Insurance enrollment. In the second period, taking the county of choice as given, and conditional on insurance availability, the household decides whether to enroll into insurance coverage.[36] Note that this is no longer a choice variable, but exogenously given if insurance is not available. Suppose the consumer is risk averse with respect to residual income, and makes their enrollment decision according to function $\nu_1(\cdot)$, which is concave and strictly increasing in its monetary arguments. For counties enrolled into the NFIP insurance program ($\eta_j = 1$), we assume the household enrolls into coverage if the following inequality holds.

$$\int \nu_1(y - p_j - c(r))dF_j(r) > \int \nu_1(y - r)dF_j(r) \tag{8}$$

Under the case in which NFIP insurance is not available ($\eta_j = 0$), enrollment is exogenously determined. Note that differences in demand for insurance come directly from the uncertainty

[36] We do not directly estimate the insurance enrollment decision. Estimating the response to a (plausibly endogenous) rate change would require conditioning on endogenous enrollment of a community into the NFIP insurance program, potentially biasing our estimates. Moreover, FEMA informed us that data from most of our study period is no longer available.

of flood-risk. Define the enrollment choice, conditional on availability, as the following.[37]

$$e_j(\eta) = \begin{cases} \arg\max_{e\in\{0,1\}} \; e \cdot \int \nu_1(y - p_j - c(r))dF_j(r) & \\ \qquad\qquad +(1-e) \cdot \int \nu_1(y - r)dF_j(r), & \textit{if } \eta = 1 \\ 0, & \textit{if } \eta = 0 \end{cases} \tag{9}$$

Sorting decision. In the first period, the household optimally chooses a county of residence, while taking information of insurance availability, η, into account. This is an optimal sorting problem. For a given vector of flood risks ($r_j \in \mathbf{r}$, $\forall j \in \{0, 1, ...J\}$), the household maximizes utility across counties and a continuous composite good, x, subject to a budget constraint. That is,

$$\begin{aligned} &\max_{j,x} \; u(x, j), \qquad\qquad j \in \{0, 1, ...J\} \\ &\textit{subject to} \\ &p_x \cdot x + e_j(\eta_j) \cdot (p_j + c(r_j)) + (1 - e_j(\eta_j)) \cdot r_j = y \end{aligned} \tag{10}$$

where we assume that $u(\cdot, \cdot)$ is a continuous, quasi-concave function of its first argument, x. For fixed choice of residence, j (and corresponding fixed flood-risk, r_j), the problem becomes a continuous problem in the composite good. Denote the argument that solves this problem $x^*\big(p_x, \tilde{y}(p_j, \eta_j, r_j), j\big)$, where $\tilde{y}(p_j, \eta_j, r_j)$ is the residual income function. For simplicity, we normalize the price of the composite good to one. Plugging the demand function for the composite good back into the utility function, and unfixing r_j, we attain the

[37] For simplicity, we do not include the potential for enrollment to be exogenously mandated, under the scenario in which the county has enrolled into the program, and the household takes residency in a flood plain.

following modified optimization problem.[38]

$$\max_{j\in\{0,1,\ldots J\}} \int \nu_0\Big(y - e_j(\eta_j)\cdot(p_j + c(r)) - (1 - e_j(\eta_j))\cdot r,\ j\Big)dF_j(r) \tag{11}$$

Altered location incentives. In this context, an increased willingness to take on risk comes from concavity of $\nu_0(\cdot)$ and the household's decreased marginal loss of risk when insurance becomes available—which makes enrollment potentially non-zero (see Equation 9). We characterize this behavior in terms of certainty equivalence. A sufficient condition is when the household's optimal solution to Equation 11 produces a certainty equivalent level of risk that is higher under insurance availability than it would be under no insurance availability. To explain, define the solution to Equation 11 in the following simplified notation.

$$j^*(p,\eta) = \underset{j\in\{0,1,\ldots J\}}{\arg\max} \int \nu_0\big(\tilde{y}(p_j,\eta,r),j\big)dF_j(r) \tag{12}$$

where $\tilde{y}(p_j,\eta,r)$ is residual income, as presented in Equation 11. For simplicity, we omit the j subscript from insurance availability, η. A perverse incentive arises when the household increases their willingness to take on more risk, accepting more risk than they would have otherwise. In terms of certainty equivalence, define two levels of accepted risk: accepted risk-level under the availability of NFIP insurance (r_1^*), and accepted risk-level under no availability (r_0^*).

[38]We estimate the significant effect of NFIP introduction on household purchases of insurance in Appendix E, though not explicitly accounted for in this model is the possibility that NFIP insurance enrollment alters risk exposure for households that do not purchase a policy. For example, households might perceive a different risk distribution after the community enters the NFIP because disaster aid becomes available. In the context of this model, an altered *perceived* risk distribution would enter the household's decision by integrating utilities over the distribution of risk, conditional on NFIP: $F(r|\eta)$. We suspect that this may play only a modest role due to the substantial differences in payouts. Disaster aid to households is capped at \$33,000, while NFIP insurance covers up to \$250,000 (Burby, 2001; Federal Emergency Management Agency, 2017a). Furthermore, after a household receives disaster aid, they are *required* to purchase NFIP insurance (Federal Emergency Management Agency, 2017b).

$$
\begin{aligned}
&\textit{for } \eta = 1, \\
&\qquad r_1^* \quad \textit{such that} \quad \nu_0\big(\tilde{y}(p_{j_1^*}, 1, r_1^*), j_1^*\big) = \int \nu_0\big(\tilde{y}(p_{j_1^*}, 1, r), j_1^*\big) dF_{j_1^*}(r) \\
&\textit{for } \eta = 0, \\
&\qquad r_0^* \quad \textit{such that} \quad \nu_0\big(\tilde{y}(p_{j_0^*}, 0, r_0^*), j_0^*\big) = \int \nu_0\big(\tilde{y}(p_{j_0^*}, 0, r), j_0^*\big) dF_{j_0^*}(r)
\end{aligned}
\tag{13}
$$

where j_1^* and j_0^* are defined as the optimal choices under insurance availability, $j^*(p,1)$, and no availability, $j^*(p,0)$, respectively. In the framework of this model, we describe induced risk taking from NFIP insurance as occuring when $r_1^* > r_0^*$. This is equivalent to saying their marginal loss from an increase in risk is less when insurance is available than when it is not. In notation, this implies the following condition holds.

$$
\frac{\partial}{\partial r}\left\{ \nu_0\big(\tilde{y}(p_j, \eta, r), j\big)\Big|_{\eta=1} - \nu_0\big(\tilde{y}(p_j, \eta, r), j\big)\Big|_{\eta=0} \right\} > 0, \quad \forall j \in \{0, 1, ...J\} \tag{14}
$$

To illustrate why this inequality might hold, consider these two cases—with flood insurance ($\eta = 1$) and without ($\eta = 0$)—separately. Differentiating the indirect utility function with respect to the underlying risk yields the following two components: $\frac{\partial \nu_0}{\partial r} = \frac{\partial \nu_0}{\partial \tilde{y}} \cdot \frac{\partial \tilde{y}}{\partial r}$. As the marginal utilities will only differ in each case if enrollment takes place, we will examine full enrollment, conditional on availability. Keeping r fixed and setting the residual incomes equal for each case ($y - r = y - p_j - c(r)$) produces equal marginal utility of income for each scenario. As for the second term, since $\frac{\partial \tilde{y}}{\partial r} = -1$ for no availability, there is increased risk taking so long as $\frac{\partial c}{\partial r} < 1$. For example, this holds for any r in the linear case where $c(r) = c \cdot r$, and c is between zero and one, but will not hold when the marginal co-payment is greater than one for some r.

Now consider an increase in the premium, making residual income less for the enrollment case than the non-availability case ($y - r > y - p'_j - c(r)$, for $p'_j > p_j$). This affects $\frac{\partial \nu_0}{\partial \tilde{y}}$, but not $\frac{\partial \tilde{y}}{\partial r}$. Since $v_0(\cdot)$ is concave, this puts us on a steeper part of the curve under enrollment, making $\frac{\partial \nu_0}{\partial \tilde{y}}$ larger. Now the first term of Equation 14 is more negative, thus offsetting some of the risky behavior. This suggests the following holds true.

$$\frac{\partial^2 \nu_0}{\partial r \partial p} = \frac{\partial}{\partial p}\left(\frac{\partial \nu_0}{\partial \tilde{y}} \cdot \frac{\partial \tilde{y}}{\partial r}\right) < 0 \tag{15}$$

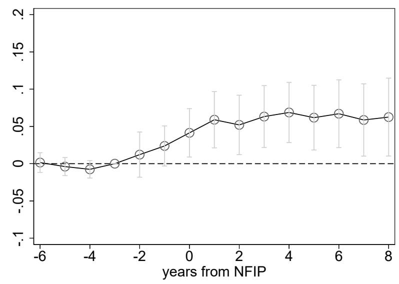
Equation 15 holds because of concavity of $\nu_0(\cdot)$, and because p enters residual income negatively. In words, this says that the marginal willingness to take on more risk is decreasing in the premium. Given that the NFIP introduced subsidized premiums—priced well below actuarially fair rates—this illustrates how NFIP insurance can exacerbate risky behavior beyond efficient levels. Whether the NFIP has increased household risk tolerance (i.e., Equation 14, or simply $r_1^* > r_0^*$) is now an empirical question.

C Naïve Specification

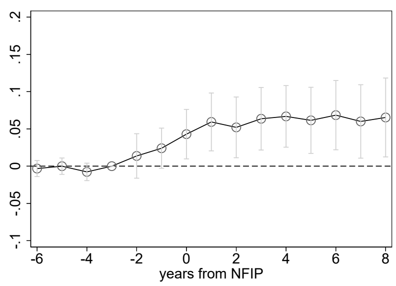
To illustrate the endogeneity issue in using actual national flood insurance enrollment, we implement a difference-in-differences strategy using a naïve version of Equation 6. Using actual NFIP enrollment allows us to directly test for reverse causality, in a *Granger*-causality sense. The naïve estimates are presented in Figure A.2. All estimates are normalized to the third leading term to illustrate the potential for an effect to take place prior to the enrollment period. For all outcomes, there is an obvious divergence in treatment prior to enrollment.

Figure A.2: Event Study Specification for (Endogenous) NFIP: Naïve Specification

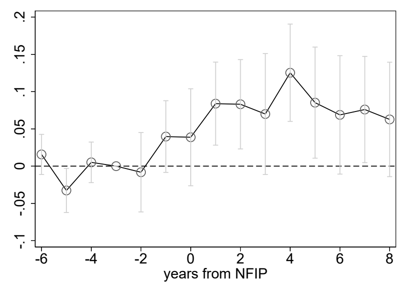
(a) Effect of NFIP on (log) Population



(b) Effect of NFIP on (log) Non-Migrants



(c) Effect of NFIP on (log) In-Migrants



Note: This figure plots the coefficients from a naïve version of Equation 6, using lagged and leading National Flood Insurance Program (NFIP) enrollment indicators directly as the explanatory variables of interest. Coefficients are estimated relative to the third leading term (rather than the first, for illustrative purposes). Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented. We use t-3 as the reference period to illustrate the pre-divergence of treatment when directly using endogenous NFIP enrollment.

D The Role of Subsidies

The introduction of the NFIP was largely associated with the availability of subsidized flood insurance premiums—insurance rates priced below actuarially fair levels. As flood insurance markets were often limited prior to the NFIP, the causal effects of the NFIP were not necessarily only through the channel of premiums. This paper examines the combined, reduced-form effects of the NFIP. We do not directly disentangle the efficiency gains offered through flood insurance availability from the efficiency losses through an inefficiently low rate structure. As estimates suggest that the total effect of the NFIP produces higher population trends in high risk areas, we can deduce that this level of migration should be socially inefficient, given the below actuarially fair rates offered through the program. How much migration in flood prone locations is socially efficient depends on the elasticity to premiums and, thus, the isolated response to discounted premiums.

An efficient flood insurance rate structure fully prices in the expected costs of flood damages.

This is the actuarially fair rate. Efficient migration outcomes, thus, occur when subsidies to premiums are zero. An alternative research design would disentangle the introduction of the NFIP from changes in subsidies. For example, given a cross-section of premium discount amounts, $discount_{cs}$, the efficiency and inefficiency effects of the NFIP may be recovered through the following specification.

$$migration_{cst} = \delta_0 \cdot postNFIP_{cst} + \delta_1 \cdot postNFIP_{cst} \times discount_{cs} + x_{cst}\beta + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \tag{16}$$

where δ_0 represents the response to unsubsidized flood insurance—when discounts are zero. This is generally an efficiency gain, as risk averse agents prefer certain outcomes, attained through flood insurance. δ_1 represents the marginal effect of discounted flood insurance. The parameters δ_0 and δ_1 are the revealed preference analogues to the marginal utilities of $A - B$ and $B - C$, respectively, in Figure 5.

Equation 16 is not directly estimated in this paper, given limited data and exogenous sources of variation in subsidies or premiums. The combined effect of NFIP is estimated in Section 6, where we interpret the causal effects of the NFIP as encompassing the subsidy effect. Importantly, when flood insurance markets exist, in absence of the NFIP, the effects estimated in Section 6 should solely represent the effects of subsidized flood insurance.

To further motivate these two margins in which NFIP may impact migration patterns, we estimate Equation 16 using a derived proxy for subsidies. We proxy for average subsidies in a county by aggregating insurance premiums and claims data, within a county. Unfortunately, these data are only available for policies acquired in 2009 or later. This further complicates identification. We construct the following proxy for the premium discount.

$$discount_{cs} = 1 - \frac{1}{|T|} \sum_{t \in T} \frac{\sum_{i \in P_{cst}} premium_{i,cst}}{\sum_{j \in C_{cst}} claim_{j,cst}}$$

The data come from two separate data sets, where $premium_{i,cst}$ represents a premium for a given policy, i, in the set of policies, P_{cst}, issued in county-state-year, cst. Similarly,

$claims_{j,cst}$ represents an insurance payout for flood damages, associated with claim, j, in the set of claims, C_{cst}. Our subsidy proxy, thus, divides cumulative payments for insurance by total insurance payouts. Any given year will exhibit beyond average surpluses and deficits, thus, the measure is averaged over the years in the sample, 2009-2020, to derive an appropriate proxy for the county.

Table A.1: Effect of Flood Insurance on Migration (Reduced Form)

Outcome: Log- Population	(1) All	(2) All	(3) Low Discounts	(4) High Discounts
Post_NFIP	0.0142	-0.0868**	-0.0242	-0.0846**
	(0.0245)	(0.0358)	(0.0437)	(0.0380)
Post_NFIP × discount	0.0508**	0.0340*		
	(0.0207)	(0.0197)		
Post_NFIP × annual floods		0.0761***	0.0411*	0.118***
		(0.0207)	(0.0223)	(0.0269)
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year-by-FHBM	Yes	Yes	Yes	Yes
Observations	53882	53882	26956	38673

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Estimates are reported in Table A.1. At face value, these estimates suggest that the majority of the NFIP response is through the subsidy channel. Interpreted strictly in a descriptive sense, the estimates demonstrate that counties with larger implied discounts on premiums produce the highest responses to NFIP. Remaining variation in NFIP suggests that the effect on population when there is no subsidy is about 1.4 percent. This is compared to the full effect of 5 percent, derived in this paper. To examine the choice of risky locations under low versus high discounted premiums, we split the sample in below and above median levels of our proxy. Doing so suggests that households have a larger propensity to live in high risk areas when under highly discounted subsidies. This is consistent with the mechanism discussed throughout the paper.

It is important to note that the differential effect of NFIP in high versus low subsidy counties may coincide with other heterogeneous responses to NFIP. Further, the premium discount is not random, but a function of various institutional elements of the program. This is a notable distinction from our primary focus on the effects of NFIP across risk levels, where the objective is to recover patterns in choices of high versus low risk areas. In contrast, proper identification of δ_1 in Equation 16 involves identifying how households directly internalize insurance rates.

E Map Assignment and Corresponding Insurance Take-Up

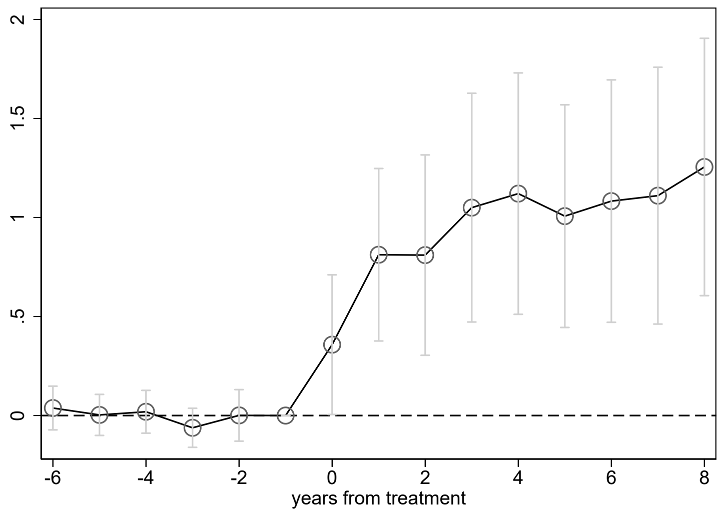
Our data on county-level policies are aggregated from transaction-level policies published by the Federal Insurance & Mitigation Administration (FIMA).[39] The data contain about 50 million policy transactions, and represent the NFIP system of record at the time the data were published. The available data date to December 2019, and contain the original effectivity date of each flood insurance policy. Because FEMA does not keep policy transaction records older than 10 years, our oldest policies are those that were in place, whether new or renewed, in 2009. What this means is that our data include policies that originally started even before 2009, because they were renewed repeatedly from the original start date, producing an incomplete count of insurance policies for any given year prior to 2009.

In Figure A.3, we present estimates from our event study specification, using our proxy for flood insurance take-up. Specifically, we examine the direct impact of map timing on the natural logarithm of cumulative flood insurance policies (plus one). The coefficient from our main specification (Equation 5) is estimated at a highly significant 0.94 (robust standard error of 0.12).[40] Most notable from this result is the large magnitude of the coefficient; insurance policies approximately doubling following the assignment of FIRMs. However, this should not be too surprising, given the high discounts offered through the NFIP.

[39] Data are available at: https://www.fema.gov/media-library/assets/documents/180376

[40] An estimate on the percent insured can be computed by subtracting the estimate on population from this estimate (i.e., to get the estimate for outcome $log(policies) - log(population)$)—about 90%. Though, given the nature of these data, this estimate should be interpreted with caution.

Figure A.3: Dynamic Coefficients on Cumulative Flood Insurance Policies



F Reduced Form Estimates with Additional Controls

Throughout this paper we provide estimates of the treatment effect of the NFIP on migration, with and without controls, to demonstrate robustness of our estimates. A primary concern in the direct estimation of the effect of a community enrolling into the NFIP is a community might (and most likely does) have a strong influence over this decision. Estimates of the impact of the NFIP would be biased, for example, if the community enrollment decision was tied to new construction and infrastructure projects, which may also affect household migration decisions. To test the sensitivity of our instrument to these potentially confounding factors, we include various controls, such as building permits—a leading indicator of future construction. We extend this exercise here to demonstrate robustness to the inclusion of different controls, as well as lags of these controls.

Table A.2 presents our main estimates with and without additional controls. In Column 1 we only include state and year fixed effects, as well as time varying FHBM-group controls. Column 2, also presented in Table 3, adds state-by-year fixed effects. In Columns 3 and 4 we add building permits for housing units and the total value of these units, respectively. These covariates might control for potential selection tied to a community's expected future growth. The inclusion of these additional variables does not significantly alter our primary estimate. In Column 4, we add income and employment controls.

For Columns 1 through 5 we maintain the same sample used in our primary estimates. Because we want to test for a lagged effect of permits on our estimates, we necessarily lose some observations. Therefore, in Column 6 we estimate our main specification on this subsample without controls for a basis of comparison. Finally, in Column 7 we include one year lags of the control variables. Overall, our estimate on map assignment does not seem particularly sensitive to the inclusion of these controls.

Table A.2: Reduced Form Estimates with Controls

Outcome: Log -Population	(1)	(2)	(3)	(4)	(5)	(6)	(7)
postFIRM-FHBM	0.0439**	0.0504***	0.0490***	0.0494***	0.0493***	0.0438**	0.0413**
	(0.0192)	(0.0173)	(0.0172)	(0.0168)	(0.0166)	(0.0182)	(0.0172)
Building Permits (Housing Units - 1,000s)			0.0131***	-0.0312***	-0.0279***		-0.00461
			(0.00186)	(0.00584)	(0.00561)		(0.00494)
Total Value of Units ($ bil)				0.354***	0.320***		0.0870**
				(0.0524)	(0.0489)		(0.0436)
Per-Capita Income ($1,000s)					0.00378***		-0.000738
					(0.000628)		(0.000477)
Unemployment (%)					0.00326***		0.00537***
					(0.000880)		(0.000641)
Building Permits -Lag							-0.0246***
							(0.00492)
Total Value -Lag							0.259***
							(0.0310)
Per-Capita Income -Lag							0.00524***
							(0.000621)
Unemployment -Lag							-0.00231***
							(0.000603)
N	64472	64472	64472	64472	64472	61389	61389
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes						
Year-by-FHBM	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE		Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

G Stock/Flow Decomposition of Migration

Throughout the main text we predominantly focus on "stock" terms to interpret the impact on the NFIP on migration choices (e.g., population levels). Using our methodology, effects on "flow" outcomes, such as in- and out-migration, may offer a different interpretation if responses are short lived. As our approach essentially compares the levels of a treated group against a control group for an entire post-treatment period, we prefer to estimate effects on total population levels, rather than changes in population. These effects will encompass any contemporaneous or short-run responses following the intervention and will, thus, represent the long-run effects of the NFIP.

The estimates in this paper present a decomposition of the effects on population into a non-migration response and an in-migration response. The sum of the two is our measure of the population level. While non-migration levels are directly a function of the decisions of individuals *not* to out-migrate, non-migration represents more of a stock of the population than the levels of out-migration would. Further, our estimates for in-migration offer a slightly different interpretation as they derive from past population movements, responses of which may be short-lived.

In this appendix, we formally decompose non-migration and out-migration into both stock and flow terms. While stock represents the accumulation of past choices, the flows will represent only contemporaneous choices. It is our opinion that our methodology will be more suited for comparing changes in static levels of outcomes over the post-treatment period, rather than contemporaneous choices. Thus, we maintain our choice of (logged) population levels as our preferred outcome of interest. Below, we write non- and out-migration as a function of past population levels and these contemporaneous decisions.

$$
\begin{aligned}
\textit{non-migration}_t =& \textit{Population}_{t-1} \cdot \textit{Prob}(\textit{Stay}_t) \\
\textit{out-migration}_t =& \textit{Population}_{t-1} \cdot \textit{Prob}(\textit{Out}_t) = \textit{Population}_{t-1} \cdot (1 - \textit{Prob}(\textit{Stay}_t))
\end{aligned}
$$

where, in each period, a fraction of the population from the previous period, $\textit{Population}_{t-1}$, will decide to stay in their county of residence, according to the probability $\textit{Prob}(\textit{Stay}_t)$, or to move out, according to the probability $\textit{Prob}(\textit{Out}_t)$. The variables $\textit{non-migration}_t$ and $\textit{out-migration}_t$ represent these two counts. As its main outcomes, this paper focuses on the natural logarithm of migration measures, due to both interpretation and underlying distribution purposes. Thus, in logs, the above equations can be written as the following.

$$Log(non\text{-}migration_t) = Log(Population_{t-1}) + Log(Prob(Stay_t))$$
$$Log(out\text{-}migration_t) = Log(Population_{t-1}) + Log(Prob(Out_t))$$

This represents the migration terms in both a stock term (population) and flow term (the contemporaneous decision to stay or move out). Though lagged population is introduced in the equations above, the long-run effect of the NFIP on population, generally, is the primary estimate of interest in the main text.[41] Table 3 (Panel A) suggests that the population for the NFIP group will be 5 percent higher than a comparable, no-NFIP control group. Adding to this the marginal effect of the NFIP on the contemporaneous probability to stay or migrate out will yield the overall effect on non- and out-migration, respectively. That is, the overall marginal effects of the NFIP on the migration outcomes can be represented as the following.

$$\frac{\Delta Log(non\text{-}migration_t)}{\Delta NFIP} = \frac{\Delta Log(Population_{t-1})}{\Delta NFIP} + \frac{\Delta Log(Prob(Stay_t))}{\Delta NFIP}$$
$$\frac{\Delta Log(out\text{-}migration_t)}{\Delta NFIP} = \frac{\Delta Log(Population_{t-1})}{\Delta NFIP} + \frac{\Delta Log(Prob(Out_t))}{\Delta NFIP}$$

The estimates of map assignment on each of the right-hand-side terms above are presented in Table A.3. Column 1 replicates the main estimates on population and Columns 2-3 estimate the effects on each flow term. As expected, the sign on out-migration probability is negative. The coefficient is also similar in magnitude to the effects on non-migration (stay) probability, as one is the log of the inverse probability of the other. The estimates in Columns 2-3 are also estimated with less precision than Column 1. This is likely due to noisier variation and/or a short-lived response.[42] Furthermore, somewhat counterintuitive is the positive overall effect on both non- *and* out-migration. That is, while an out-migration rate may be lower under the NFIP, the total effect on out-migration levels is positive due to the NFIP's dominating

[41] In potential outcomes notation, we assume that $log(Y_t(1)) - log(Y_t(0)) = log(Y_{t-1}(1)) - log(Y_{t-1}(0))$, where $Y_t(1)$ and $Y_t(0)$ are the outcomes at time t in the universe in which units are treated, or not treated, respectively.

[42] To put the approximately 0.4% estimates on out-/in-migration probability into perspective, at a growth rate of 0.4% per year, a population would take 12 years to become 4.8% larger than its initial, baseline levels.

effect on factors affecting baseline population levels (e.g., in- and non-migration).

Table A.3: Estimates for Flow versus Stock Terms

	(1) Log(Pop)	(2) Log(Prob(Stay))	(3) Log(Prob(Out))
postFIRM × FHBM	0.0481***	0.00391*	-0.00411
	(0.0163)	(0.00230)	(0.0245)
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Year × FHBM	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Declared Disaster Controls	Yes	Yes	Yes
N	64498	64437	62999

* $p < 0.1$, ** $p < .05$, *** $p < .01$

H The Role of FEMA's Community Rating System

The Community Rating System (CRS) is an additional incentive program that NFIP communities can join if they adopt floodplain management practices beyond the minimum NFIP standards. Under the CRS, flood insurance premiums are further discounted to reflect the reduction in risk. These discounts are potentially large, up to 45 percent of annual premiums depending on the assessment of a community's floodplain management. The CRS may affect migration decisions in a variety of ways. Stricter floodplain management both makes the area safer and potentially less attractive, which can have offsetting effects on migration flows. On the other hand, lower flood insurance premiums make housing more affordable and should attract migration flows.

A community's decision to enter the CRS is potentially endogenous. For example, a community that expects an increased demand for flood insurance, whether due to expected migration flows or worsening flood damages experienced by residents, would be more likely to enter the CRS. Therefore, we should be concerned about the potential selection into the program. This is similar to the issues in using NFIP directly in our empirical design, discussed in this paper.

Importantly for us, communities can only join the CRS *after* being assigned a FIRM *and* joining the NFIP. As our identification leverages the *initial* FIRM assignment as a determinant of NFIP entry, our design largely ignores any additional migration effects from the CRS. We examine this directly below.

We estimate a version of Equation 5, which includes an indicator for the subsequent enrollment into the CRS. The results are presented in Table A.4, where *postCRS* denotes CRS entry (at the county level). Columns 1-3 demonstrate a strong relationship between CRS and migration into the county. As a community may time their entry into the CRS with other time-varying characteristics of a county, we control for a set of such variables in Column 4. This includes a county's per-capita income, unemployment and jobs numbers, and a count of building permits in the county. The inclusion of these variables causes a loss in significance on the CRS estimate. This suggests that such observable characteristics may be determinants of a community's entry into the CRS. Importantly, as CRS occurs following NFIP entry, its inclusion does not affect the magnitude of our main estimates.

Table A.4: Estimates for CRS

Migration Outcome	(1)	(2)	(3)	(4)
Panel A: Log- Population				
postFIRM × FHBM	0.0501***	0.0509***	0.0509***	0.0479***
	(0.0173)	(0.0173)	(0.0173)	(0.0163)
postCRS	0.0878***	0.0890***	0.0889***	0.0334
	(0.0259)	(0.0259)	(0.0259)	(0.0269)
Panel B: Log- Non-Migrants				
postFIRM × FHBM	0.0534***	0.0542***	0.0542***	0.0509***
	(0.0177)	(0.0178)	(0.0178)	(0.0167)
postCRS	0.0964***	0.0977***	0.0977***	0.0390
	(0.0266)	(0.0265)	(0.0266)	(0.0275)
Panel C: Log- Migrants				
postFIRM × FHBM	0.0315*	0.0321*	0.0323*	0.0309*
	(0.0188)	(0.0188)	(0.0188)	(0.0181)
postCRS	-0.000535	0.000334	0.0000549	-0.0411
	(0.0261)	(0.0261)	(0.0261)	(0.0280)
CNTY FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Declared Disaster Controls			Yes	Yes
Controls				Yes
Observations	64743	64743	64743	63619

* $p < 0.1$, ** $p < .05$, *** $p < .01$

I BJS Imputation-Based Estimation

Recent literature on difference-in-differences approaches with staggered treatment may produce biased estimates when treatment effects are heterogeneous across groups and time. Studies by Sun and Abraham (2021) and Goodman-Bacon (2021) illustrate that under staggered treatment designs, treatment effect estimates reflect a weighted average of various group comparisons which may not represent the best comparisons necessary to satisfy standard parallel trend assumptions. With growing treatment dynamics, the implication is an underestimate of the true treatment effect. Fortunately, many new advances have been made to adapt the standard two-way fixed effects approach into one robust to these problems (Sant'Anna and Zhao, 2020; de Chaisemartin and D'Haultfœuille, 2020; Wooldridge, 2021; Callaway and Sant'Anna, 2021; Borusyak, Jaravel, and Spiess, 2022).

The overwhelming majority of these new advances in difference-in-differences designs with staggered treatment have been implemented in the context of binary treatment, with permanent treatment status. As the design used in this paper makes use of county level data, which is aggregated up from community level, our research design incorporates a continuous treatment metric—or multiple "doses"—which brings with it additional complications (Callaway, Goodman-Bacon, and Sant'Anna, 2021).

In this appendix, we test the robustness of our two-way fixed effects design by implementing the imputation procedure proposed by Borusyak, Jaravel, and Spiess (2022); BJS, hereafter. As this approach is only suitable for settings with binary treatment, we redefine our treatment variable to indicate the timing of the *initial* "dose". Therefore, initial dose will only indicate partial treatment for a county, and will omit information on the progression of treatment within a county. To provide additional context, the approach will be applied to both a reduced form equation—the effects of initial dose on migration—and a first stage equation—the effects of initial dose on the continuous NFIP treatment.

Our application of the BJS imputation approach proceeds as follows. Our key outcome variable is regressed on a full set year and county fixed effects, *only* on the subsample yet to be treated. The time fixed effects from this stage predict an average untreated path, and the county fixed effects adjust levels. Imputing the fitted values for treated units predict a untreated counterfactual path. The difference between the outcome and its predicted counterfactual is an estimated treatment effect for that unit, and its average is an estimate for the average treatment effect.

Estimates from the BJS imputation approach are reported in Table A.5. Bootstrapped standard errors, clustered by county, are reported in parentheses. Column 1 reports the reduced for effect of initial dose on the natural log of population. All else equal, we would expect this estimate to be smaller in magnitude than the primary estimates, as initial dose only partially encompasses the full extent of treatment. However, the estimate of 4.8% is very similar analogous estimates in Table 3. This is likely due to an the upward adjustment of the estimate from the BJS approach. This approach also produces larger standard errors, likely due to lack of meaningful variation in the new treatment indicator. Column 2 provides additional insight by estimating the effects of initial dose on the extent of NFIP assignment. The first stage estimate suggests that initial dose accounts for 66% of NFIP assignment within a county on average. Scaling Column 1 by this first stage estimate gives us an adjusted estimate for the treatment effect of NFIP. The scaled BJS estimate is about 7.2%, about 2 percentage points larger than our main results. While the upward adjustment is expected, the increase is modest in relation to our primary results.

Table A.5: BJS Imputation Estimates

	(1) Log- Population	(2) Post-NFIP
Post Initial FIRM	0.048	0.656***
	(0.032)	(0.046)
Observations	1259	1259

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < .01$

J Subsample of FHBM Communities and Communities Originally Flagged as "Flood Prone"

As our data aggregate treatment from community up to county-level, we are often left with "fractional" treatment values, which prevent us from directly conditioning on the FHBM communities and estimating a traditional event study specification. In this section, we test our estimates against subsamples of counties with at least one community who joined the emergency program, and a specification weighting by fraction of a county assigned a FHBM. As we cannot fully condition on only FHBM communities, weighting by FHBM allows a county who has 100 percent of its communities in the FHBM group to be counted in full, while placing less weight on counties who only have a fraction of communities in the FHBM group. Additionally, we test an alternative approach which identifies the impact of the NFIP using only the communities originally flagged by FEMA. As this alternative approach may produce a different first stage, below we present the scaled version of our estimates.

Table A.6, presents two-stage least square estimates on different subsamples of communities. Column 1 presents the second stage counterpart to our base specification in Table 3. In Column 2, we condition on only the counties with at least one community that has an FHBM (i.e., $FHBM_{cs} > 0$). In Column 3, we present our specification which weights observations by the FHBM fraction.

In Columns 4-6, we make use of the subgroup of communities who received an FHBM by June 1974. This is motivated by FEMA's initially targeted group of flood prone communities: "Of the 13,600 such communities so identified by December 1973, FIA had provided FIRMs or FHBMs to less than two-thirds. By June 1974, an additional 2,700 communities are identified as flood-prone" (American Institutes for Research, 2005). We are able to identify 8,205 communities that were assigned an FHBM by June 1974, consistent with the above statement. It would be preferable to make use of the full sample of 13,600 communities, however, a FEMA representative has informed us that this list is no longer available; stating that they only maintain some records for over 10 years. Column 4 replaces our interaction of FIRM timing and FHBM group indicator with the interaction between FIRM timing and FHBM groups identified by June 1974. Column 5 conditions on the sample of counties that include a community enrolled into the emergency program by June 1974. Finally, Column 6 presents our specification weighting observations by the fraction of communities within a county enrolled into the emergency program by June 1974. Overall, our estimates remain consistent with those from our primary approach.

Table A.6: 2SLS Estimates on Different FHBM Samples

Migration Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log- Population						
postNFIP	0.0530***	0.0458**	0.0333*	0.0820***	0.0465*	0.0471*
	(0.0182)	(0.0181)	(0.0179)	(0.0247)	(0.0237)	(0.0271)
Panel B: Log- Non-Migrants						
postNFIP	0.0562***	0.0486***	0.0354*	0.0909***	0.0531**	0.0536*
	(0.0187)	(0.0186)	(0.0184)	(0.0259)	(0.0248)	(0.0283)
Panel C: Log- Migrants						
postNFIP	0.0360*	0.0320	0.0248	0.0335	0.0183	0.0149
	(0.0198)	(0.0198)	(0.0196)	(0.0260)	(0.0251)	(0.0272)
N	64472	62491	62491	64472	49129	49129
Sample	All	FHBM	FHBM	All	pre-74 FHBM	pre-74 FHBM
FHBM Group	All FHBM	All FHBM	All FHBM	pre-74 FHBM	pre-74 FHBM	pre-74 FHBM
Weighted	No	No	Yes	No	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-FHBM	Yes	Yes	Yes	Yes	Yes	Yes

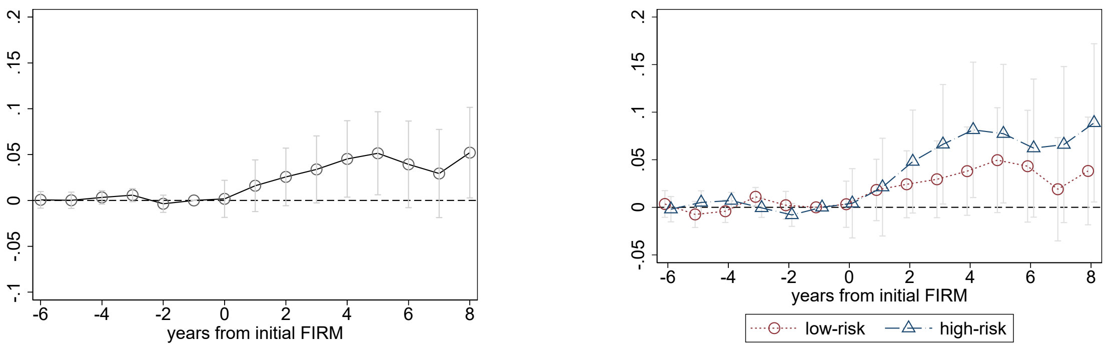
* $p < 0.1$, ** $p < .05$, *** $p < .01$

K Replicating Results using BEA Population Data

This paper uses data from the Internal Revenue Service to pin down the impacts of the NFIP on migration. Though these data are limited to tax filers, they allow us to disentangle in- versus out-migration effects. Below we recreate our main estimates using population data from the Bureau of Economic Analysis (BEA). We present the event study graphs for the impact of average effect of FIRM (Panel A), and the heterogeneous effects by risk-level (Panel B). The illustrations are similar, however, exhibiting a more gradual effect over time. Coefficients from the static regressions using our main specifications are also comparable; producing an average effect of FIRMs of 3.9 percent (5 percent in Table 3), and a heterogeneous effect of the NFIP of 3.3 percent per flood (3.6 percent from Table 4).

Figure A.4: Main Results Using BEA Population Data

(a) Average Effect of FIRMs (b) Heterogeneous Effect of FIRMs, by Flood Risk



L Leveraging FIRM Timing for All Communities

In this paper, we identify baseline effects of NFIP from the plausibly exogenous timing of initial FIRM assignments to emergency program, or FHBM, communities. This amounts to estimation of a treatment effect on the more risky, FHBM counties. In this section, we present our estimates when using *postFIRM* as our instrument for the entire sample of counties. Doing so assumes that FIRM timing was exogenous for all communities, not just emergency group communities—for whom FIRMs were an upgrade from FHBMs. Furthermore, as FHBM communities were more likely to enter the regular program—due to need and FEMA influence—we should anticipate a smaller first stage estimate when using *postFIRM* as our instrument for all counties. Therefore, it will become more important to interpret the second stage results. Our estimates of the first stage are presented in Table A.7.

Table A.7: First Stage: Effect of FIRM on NFIP Enrollment

Post-NFIP	(1)	(2)	(3)
postFIRM	0.761***	0.761***	0.761***
	(0.0134)	(0.0134)	(0.0134)
County FE	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes
Controls		Yes	Yes
Declared Disaster Controls			Yes
N	64472	64472	64472

* $p < 0.1$, ** $p < .05$, *** $p < .01$

These estimates are significantly smaller than those from our original instrument in Table 2. This suggests that our original instrument—with a first stage estimate of 0.95—map into national flood insurance enrollment much more closely than FIRM alone—with a first stage estimate of 0.76.[43] Next, we estimate our reduced form equation with *postFIRM* as our instrument. This is simply the analogue to Equation 5, and Table 3 results, but substituting *postFIRM* for *postFIRM-FHBM*. The results are in Table A.8.

Table A.8: Effect of FIRM on Migration

Migration Outcome	(1)	(2)	(3)	(4)
Panel A: Log- Population				
postFIRM	0.0370***	0.0345***	0.0333**	0.0333**
	(0.0131)	(0.0124)	(0.0135)	(0.0135)
Leading Treatment			-0.00613	-0.00615
			(0.00882)	(0.00882)
Panel B: Log- Non-Migrants				
postFIRM	0.0391***	0.0362***	0.0354**	0.0354**
	(0.0134)	(0.0126)	(0.0138)	(0.0138)
Leading Treatment			-0.00433	-0.00434
			(0.00903)	(0.00903)
Panel C: Log- Migrants				
postFIRM	0.0268*	0.0267*	0.0239	0.0240
	(0.0148)	(0.0143)	(0.0156)	(0.0156)
Leading Treatment			-0.0142	-0.0142
			(0.0119)	(0.0119)
N	64472	64472	64472	64472
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
Declared Disaster Controls				Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

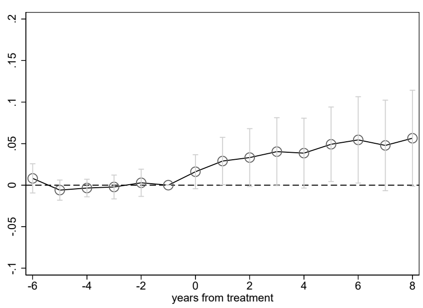
These results produce a smaller reduced form estimate than our original instrument. This is to be expected with a smaller first stage. Estimates for population indicate a 3.3 to 3.7 percent effect. Scaling by the first stage of 0.76, our results indicate an effect of about 4.3 to

[43] Coefficient estimates (and standard errors) do not change at the one-thousandth decimal point for any specification.

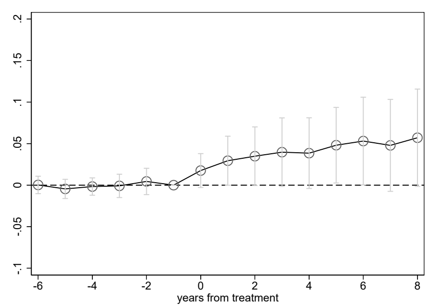
4.9 percent. These results are very similar to our primary estimates, and the same holds true for our other migration outcomes. Therefore, while we expect the timing between FHBM and FIRM assignment to be plausibly exogenous, it seems that our results do not rely heavily on this variation. The dynamic coefficients for this alternative, reduced-form specification are presented in Figure A.5. This figure shows an obviously more gradual increase in treatment effect than the FHBM-specific group effect. This is most likely due to additional urgency to enroll for communities in the emergency group.

Figure A.5: Event Study Specification for FIRM Average Treatment Effect

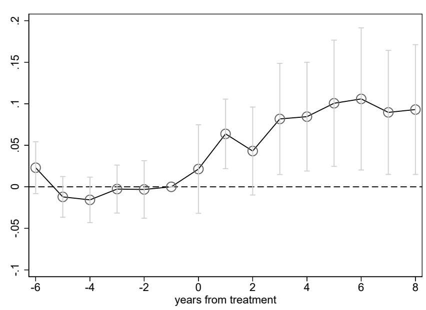
(a) Effect of NFIP on (log) Population



(b) Effect of NFIP on (log) Non-Migrants



(c) Effect of NFIP on (log) In-Migrants



M Heterogeneous Effects Across Baseline Population Levels

Table A.9 reports heterogeneous estimates of the NFIP intervention across counties with different baseline levels of population. As population is an endogenous variable, the 1990 (logged) population level of each county is used as the main interaction term. Results suggest that a county with a 10 percent higher population level in the initial year will yield roughly a 10 percent higher effect of NFIP on future population levels.

Table A.9: Heterogenous Effects of NFIP by Baseline Population

Outcome: Log- Population	(1)	(2)	(3)
postNFIP	-2.520***	-1.995***	-1.995***
	(0.353)	(0.325)	(0.325)
postNFIP × Log(baseline population)	1.144***	0.910***	0.910***
	(0.159)	(0.146)	(0.146)
County FE	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes
Year X Pop	Yes	Yes	Yes
Controls		Yes	Yes
Declared Disaster Controls			Yes
N	64408	64408	64408

* $p < 0.1$, ** $p < .05$, *** $p < .01$

N Heterogeneous Effects Across Baseline Own-to-Rent Ratio

In Table A.10, we explore the role of home ownership rates in the responsiveness to the NFIP program. Similar to population levels, home ownership is likely endogenous to flood insurance availability. Thus, to estimate this effect, we only examine heterogeneous effects across the baseline, 1990 levels of ownership; specifically, the own-to-rent ratio of a county. Ownership rates are collected from the Integrated Public Use Microdata Series (IPUMS), though the data only account for a small subset of 434 counties (across all 50 states, plus the District of Columbia). Results for this subset of counties suggest a statistically insignificant, though economically meaningful relationship; implying a county with a 10 percentage point higher own-to-rent ratio has about a 1 percent higher response to the NFIP.

Table A.10: Heterogenous Effects of NFIP by Own-to-Rent Ratio

Outcome: Log- Population	(1)	(2)	(3)
postNFIP	-0.356	-0.332	-0.339
	(0.313)	(0.282)	(0.283)
postNFIP × Own-to-Rent	0.145	0.137	0.139
	(0.128)	(0.119)	(0.119)
County FE	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes
Year X Own-to-Rent	Yes	Yes	Yes
Controls		Yes	Yes
Declared Disaster Controls			Yes
N	7922	7922	7922

* $p < 0.1$, ** $p < .05$, *** $p < .01$

O Heterogeneous Effects with Base-Year Flood Risk

A limitation of our NOAA data on flood episodes is that they restrict us to examining the heterogeneous effects of NFIP as a function of average *in-sample* floods. This may produce biased results if we anticipate NFIP to alter a community's flood risk; but characterizing a community's level of risk is important in capturing where the majority of the response is derived from. Interpreting the results from Section 7 directly, points to a stronger increase in population in the most flood-prone locations, as observed in the data.

In Table A.11, we attempt to replicate our results from Table 4, using the initial year of observation (1996) in the NOAA floods data as our metric for flood risk. The results are presented from a two-stage least squares regression, where in-sample average flood risk is specified as an endogenous regressor. Though using an unrepresentative, single year creates a noisy proxy for flood risk, our results are similar in magnitude to our main estimates.

Table A.11: Heterogenous Effects with Base-Year Floods

	Migration Outcome		
	(1)	(2)	(3)
Panel A: Log- Population			
postFIRM-FHBM	-0.00660	-0.0130	-0.0130
	(0.0478)	(0.0433)	(0.0433)
Annual Floods × postFIRM-FHBM	0.0447	0.0485	0.0486
	(0.0357)	(0.0316)	(0.0316)
Panel B: Log- Non-Migrants			
postFIRM-FHBM	-0.00933	-0.0166	-0.0166
	(0.0509)	(0.0461)	(0.0461)
Annual Floods × postFIRM-FHBM	0.0491	0.0535	0.0535
	(0.0384)	(0.0341)	(0.0341)
Panel C: Log- Migrants			
postFIRM-FHBM	0.0338	0.0346	0.0347
	(0.0464)	(0.0436)	(0.0436)
Annual Floods × postFIRM-FHBM	0.00137	0.000545	0.000608
	(0.0329)	(0.0304)	(0.0304)
N	67559	67559	67559
County FE	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes
Year X Floods	Yes	Yes	Yes
Controls		Yes	Yes
Declared Disaster Controls			Yes

* $p < 0.1$, ** $p < .05$, *** $p < .01$

P Alternative Measure of Flood Risk

In this appendix, we explore alternative measures of flood risk. Average annual flood episodes remains our preferred measure, as it is likely salient and well-understood by residents, while offering intuitive estimates. As a primary driver of flooding, historical precipitation has the advantage of being robust to any flood mitigating infrastructure projects, and should also be salient to residents. However, as flood events may occur through channels beyond localized precipitation, this is an imperfect proxy for flood risk. For example, continuous heavy rains in northern states along the Mississippi river can raise the flood risk downstream, as happened in 2019, when the Mississippi River spent 211 days above flood stage in Baton Rouge.

We estimate Equation 7, substituting flood episodes with historical precipitation levels. For our precipitation data, we use the 1981-2010 station-level climate normals published by NOAA, and aggregate to county. Climate normals are calculated by NOAA as a three-decade average of historical data. Climate normals are used as a standard against which current or forecast weather may be compared.[44] Normals also represent typical annual weather observations for an area, rather than infrequent shocks, such as a flooding event. These precipitation normals, thus, should closely align with the public's expectations. Furthermore, any detected heterogenous response of the NFIP across precipitation normals would likely arise through expectations of flood risk.

Our results are presented in Table A.12, in a manner similar to Table 5. We scale our measure of precipitation to standard deviations for interpretation. Results suggest that the NFIP contributes to an additional 3 percent increase in population size for counties one standard deviation higher in normal precipitation levels. These estimates are consistent with our main results using flood episodes.

Table A.12: Heterogeneous Effect of NFIP, by Precipitation Level

Outcome: Log- Population	(1)	(2)	(3)	(4)
postNFIP × Precipitation	0.0328*	0.0581***	0.0588***	0.0466**
	(0.0177)	(0.0210)	(0.0208)	(0.0211)
postNFIP × Income		0.0945***	0.0970***	0.0811**
		(0.0357)	(0.0352)	(0.0350)
postNFIP × Jobs			-0.0444	-0.400
			(0.0924)	(0.306)
postNFIP × Building Permits				0.294
				(0.212)
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year X Precip	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Declared Disaster Controls	Yes	Yes	Yes	Yes
N	61359	61359	61359	61359

* $p < 0.1$, ** $p < .05$, *** $p < .01$

[44] NOAA publishes more information about climate normals at https://www.ncdc.noaa.gov/data-access/land-based-station-data/land-based-datasets/climate-normals/1981-2010-normals-data

The results in this paper suggest that NFIP has a strong influence on the propensity for households to locate in relatively more flood prone areas. However, with a changing climate, whether these marginal households will be affected by increased future flooding caused by climate change depends on the relationship between current flood risk and future flood risk. To explore whether NFIP increases the likelihood of a household locating in an area prone to *future* flooding, we make use of projections made public by First Street Foundation (2020), which examine the future flood exposure of properties across the U.S.

As the First Street projections were not made public until June 2020—outside of our study period—this measure of property risk was not directly internalized by the households.[45] Thus, any positive estimated effect would be due to the correlation between the estimates of future property risk and current flood risk. Further, as these projections encompass many predictors that may be endogenous to household migration decisions, these results should be interpreted with caution. For example, as First Street specifically measures risk for specific properties in the area, local infrastructure and adaptation efforts are incorporated into the model.[46]

Table A.13 and A.14 report the estimates using First Street's estimates for projected percent of properties at risk in 2020 and 2050, respectively. The negative coefficient may derive from the fact that empirical population growth in our sample period has a strong negative correlation with percent of properties identified by First Street as at risk. This could be driven by a positive relationship between population and adaptation efforts, or even by an assumed effect of the NFIP on adaptation efforts. Because the First Street flood model is proprietary, we cannot remove any direct or indirect influence that such factors may have in their model's identification of properties at risk. Overall, the estimates in Table A.14 are imprecisely estimated, offering an inconclusive result for the role the NFIP and future flood expectations on migration.

[45] https://firststreet.org/press/2020-first-street-foundation-flood-model-launch/

[46] https://firststreet.org/research-lab/published-research/flood-model-methodology_overview/

Table A.13: Heterogeneous Effect of NFIP, by 2020 Property Risk

Outcome: Log- Population	(1)	(2)	(3)	(4)
postNFIP × % Properties at Risk (2020)	-0.261	-0.119	-0.112	-0.0650
	(0.223)	(0.224)	(0.225)	(0.211)
postNFIP × Income		0.0573*	0.0548*	0.0448
		(0.0319)	(0.0309)	(0.0305)
postNFIP × Jobs			-0.0186	-0.387
			(0.0935)	(0.309)
postNFIP × Building Permits				0.301
				(0.211)
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year X Property Risk	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Declared Disaster Controls	Yes	Yes	Yes	Yes
N	62267	62267	62267	62267

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Table A.14: Heterogeneous Effect of NFIP, by 2050 Property Risk

Outcome: Log- Population	(1)	(2)	(3)	(4)
postNFIP × % Properties at Risk (2050)	-0.241	-0.101	-0.0939	-0.0506
	(0.223)	(0.231)	(0.232)	(0.218)
postNFIP × Income		0.0573*	0.0549*	0.0449
		(0.0324)	(0.0311)	(0.0308)
postNFIP × Jobs			-0.0180	-0.385
			(0.0940)	(0.312)
postNFIP × Building Permits				0.300
				(0.212)
County FE	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes
Year X Property Risk	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Declared Disaster Controls	Yes	Yes	Yes	Yes
N	62267	62267	62267	62267

* $p < 0.1$, ** $p < .05$, *** $p < .01$