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

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Abstract

Despite the large costs of covering flood losses, little is known about whether national flood insurance affects households' decision to sort into more flood-prone locations. In this paper, we test whether subsidized flood insurance alters residency choices by exploiting the within- and across-county variation in various programs the federal government implemented to encourage flood-prone areas to join the National Flood Insurance Program (NFIP). We leverage the Federal Emergency Management Agency's identification of communities with elevated flood risk and the lengthy, plausibly exogenous process of mapping these 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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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 happen to be 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 have encouraged households to increasingly move to and stay in coastal counties, which are likely to be flood-prone (Kahn, 2005; Boustan, Kahn, and Rhode, 2012). Because most of US economic activity is concentrated on its ocean and Great Lakes coasts (Rappaport and Sachs, 2003), there may also be 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 weather-related natural disasters are increasing over time, from $8.9 billion during the early 1980s, to $45.1 billion in more recent years (Bouwer et al., 2017). 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 an additional $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 more risk. In this paper, we examine whether providing nationally subsidized flood insurance 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). We derive an analytical expression demonstrating how migration to flood-prone locations is exacerbated under subsidized insurance, where premiums are priced well below actuarially fair rates. As a result, this paper empirically tests whether households take on more risk by locating in high-risk areas when subsidized flood insurance is made available.

While the existence of these perverse effects of nationally subsidized flood insurance is an important policy question, isolating the causal channel requires that the access to coverage was independent of confounding factors additionally impacting migration behavior in these communities. It is difficult to identify causal effects because a community's decision to join the NFIP is voluntary and can be driven by several, endogenous factors. The resulting potential for selection means that a naïve, direct estimation of the impact of national flood insurance adoption on population growth may produce biased estimates. For example, our estimates of the impact of the NFIP would be biased if a community's enrollment decision was tied to unobserved, new construction and infrastructure projects, which may also affect migration decisions.

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 and incentivized these communities to enter the regular program, granting access to federally subsidized flood insurance.[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 exploit this roll-out of initial FIRMs for FHBM communities 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 plausibly exogenous publica-

[1]Because of the potential for households to endogenously purchase insurance, throughout, we examine enrollment at the community-level—a necessary requirement in order to receive federally subsidized flood insurance. We discuss the mapping and enrollment processes in Section 2 and examine insurance take-up in Online Appendix B.

tion timing of the upgrade to flood insurance rate maps developed for the flood-prone areas, which allowed them entrance into the regular flood insurance program. Our identification of the causal effect of national flood insurance enrollment on population assumes that, absent the pressure from FEMA to join the NFIP, these communities would have experienced population changes similar to non-FHBM communities. We discuss the institutional details behind the roll-out of upgraded flood maps, and present evidence in favor of our identifying assumption by demonstrating that these two types of communities follow similar patterns in migration prior to the publication of upgraded flood maps, and only diverge following map publication.

Results indicate that households are sufficiently mobile such that national flood insurance directly affects their decision to live in these flood-prone areas. We estimate that population in counties whose communities are impelled to join the NFIP increases by 4 to 5 percent. This effect is primarily driven by existing residents choosing to remain in these locations when they receive federal flood protection, where the counterfactual response would have been to migrate out. 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 federal flood insurance, and thus more responsive to its availability.

As our estimates of the impact of subsidized flood insurance availability on migration only estimate an average effect on the "more likely to be treated" group of communities—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 incorporate empirical risk levels to determine whether the NFIP has increased households' willingness to take on *more* risk. We estimate 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. Although we focus on the residency effects of *initial* flood maps, our paper is also related to Gibson, Mullins, and Hill (2019), who exploit updates to existing flood maps to examine the role of beliefs about flood risks in housing prices in New York City, and to Ben-Shahar and Logue (2016), who document perverse effects of subsidies to homeowners insurance for properties in Florida that face greater threat of severe weather. In complementing these studies, our findings provide credible, comprehensive evidence on questions posed by older studies (Government Accountability Office, 1982; Cordes and Yezer, 1998). Relatedly, Gregory (2014) estimates a dynamic discrete choice model to show that the Louisiana Road Home grant program increased the rebuilding rate in New Orleans after Katrina.

We also complement existing work 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 insure, 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 when insured at a subsidized rate against those risks, 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.

Detecting the presence of perverse incentives from subsidized flood insurance availability has important policy implications. At present, the NFIP covers over $1 trillion worth of property (Michel-Kerjan, 2010). Total damages have increased together with incurred losses over the years, raising concerns over the financial viability of providing subsidized flood insurance. Assuming flood damages are proportional to population size, our estimates suggest that increased migration to flood-prone areas, induced by the NFIP, has been responsible for a significant share of the costs associated with recent floods. To illustrate, consider recent flooding in New Orleans Parish, Louisiana, which ranks in the 75th percentile in historical flood risk nationally, and Harris County (Houston), Texas, which is in the 90th percentile. The NFIP spent over $16 billion in insurance payouts as a result of Hurricane Katrina, much of which went to New Orleans, a city partially below sea level (Michel-Kerjan and Kousky, 2010). Our heterogenous estimates based on historical flood-propensity suggest that the NFIP-induced migration led to costs that were 6.6 percent higher than they would have been absent the program. As for Harris County (Houston), we calculate that the NFIP was responsible for a meaningful 14 percent increase in damages from Hurricane Harvey.

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 (e.g., Barreca, Clay, Deschenes, Greenstone, and Shapiro, 2016). We show that national flood 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, which illustrate the large external costs of insuring populations against flood risks, suggest that subsidizing premiums produces an inefficient flow of migration to flood-prone locations, additionally hindering climate change adaptation.[2]

The remainder of the paper proceeds as follows: Section 2 discusses the relevant background of the National Flood Insurance Pogram. In Section 3, we lay out the theoretical framework

[2]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 enhancing impact. Therefore, this paper focuses on the social marginal cost of national flood insurance, in the form of induced population growth in flood-prone locations. We discuss this in more detail in Section 3.

for the mechanism in which national flood insurance may lead to increased risk taking. In Section 4, we present our empirical strategy to test for perverse incentives from flood insurance. In Section 5, we discuss the data used in this paper. In Section 6, we present estimates of the impact of the flood insurance program on migration. In Section 7, we present our primary results, testing for relative risk taking by estimating heterogeneous effects of the flood insurance program, by risk level. 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 NFIP and early flood mapping efforts. We then describe the process by which these early flood maps were upgraded to modern flood maps, and how this upgrading process provides plausibly exogenous variation in access to subsidized flood insurance.

2.1 Objectives of the National Flood Insurance Program

The National Flood Insurance Program (NFIP) was created through the National Flood Insurance Act of 1968. Before the NFIP, private insurers were largely unable 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. 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).[3] With the help of private engineering firms, the NFIP was able to finish identifying these communities in 1978. The NFIP issued Flood Hazard Boundary Maps (FHBMs) as a tentative measure to identify 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, 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, these communities were enrolled in the emergency phase of the NFIP. 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 phasewhich grants full access to expanded coveragecommunities had to wait for the FIA to complete a comprehensive flood insurance study and upgrade their flood maps to Flood Insurance Rate Maps (FIRMs).[4]

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. About half of the flood-prone FHBM communities identified during the 1970s were given their initial FIRMs early on, while many communities received their initial FIRMs after 1989, delaying their full participation in the NFIP regular phase.[5] FEMA was consistent in targeting this group over time, with 99% of counties that have at least one FHBM ultimately upgraded to FIRMs. By 2011, less than 3 percent of FHBM communities remained in the emergency phase (Federal Emergency Management Agency, 2011).

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

[4]This responsibility was transferred to the newly created Federal Emergency Management Agency (FEMA) in 1979.

[5]The data for our analysis begins in 1990. Also, some FIRMs were created before 1979, but these are also just hand-drafted emergency maps made after a major flood (Morrissey, 2006).

The timing of this initial FIRM upgrade to existing FHBM communities motivates our empirical strategy. In particular, communities that were upgraded to FIRMs were incentivized to enroll in the regular phase of the NFIP, or face sanctions. The main sanction was being cut off from federal disaster aid.[6] 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 priority (Browne et al., 2019). Discussed in more detail in Appendix G, the 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 flood maps for FHBM communities was a time and labor-intensive process due to the comprehensive flood insurance studies required. Figure 2 plots the distribution of communities by the year of their FHBM against the mean number of years before they were upgraded to FIRM. Figure 2 illustrates a natural backlog, where communities that got their initial FHBM at the peak of FHBM assignments waited the longest for their FIRM upgrade.

Compared to the old flood maps, FIRMs were detailed enough to divide communities into zones according to the level of flood risk.[7] This allowed premiums to vary by the risk-level

[6] 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 flood insurance. In 1994, the Riegel Community Development Regulatory Improvement Act, penalizes mortgage lenders that do not verify whether borrowers required to carry flood insurance actually do so, was passed. In addition, FEMA conducted Cover America, which was an extensive information campaign from 1994 to 2000.

[7] 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).

of the zone in which the property is located, and identifies which properties are required to carry flood insurance (Morrissey, 2006), as flood insurance is mandatory for 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). Panel B of Figure 1 illustrates how community enrollment in NFIP increased from the 1990s onward. In Appendix B, we also show that the number of 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 flood insurance.

2.4 The NFIP Today

The final push to create and update flood maps relevant to our study period began in 1997, when FEMA started the Flood Map Modernization Initiative. The goal of this program was to transition from paper maps to digital flood maps, and to create new digital flood maps where necessary. The National Flood Insurance Reform Act of 1994 required that FEMA assess the need to revise and update flood maps once every five years.

National flood insurance remains highly subsidized due to a combination of grandfathering and outdated flood maps (CBO, 2017; Kunreuther et al., 2017; Lee and Wessel, 2017). 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). In 2016, the average premium was $520 per year (CBO, 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 to over $2,000 (American Institutes for Research, 2005; Lee and Wessel, 2017). The extreme under-pricing of rates has led to wide conjecture about the potential perverse incentives being produced by the program.

Several laws have 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 reflect changed conditions and gradually increasing insurance premiums to full actuarial risk rates, especially for high-risk properties. Gibson, Mullins, and Hill (2019) 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 removed the insurance premium increases in favor of 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 before 2012 in this paper.

3 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 nationally subsidized flood insurance. 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 comes from exogenous flood damages). 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 flood insurance—the household then decides whether to enroll. If the household does not enroll in flood insurance, or if flood 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 national flood 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.[8] 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 flood insurance program ($\eta_j = 1$), we assume the household enrolls into coverage if the following inequality holds.

$$\int \nu_1(y - p_j - c(r))dF_j(r) > \int \nu_1(y - r)dF_j(r) \tag{1}$$

Under the case in which national flood insurance is not available ($\eta_j = 0$), enrollment is exogenously determined. Note that differences in demand for insurance come directly from the uncertainty of flood-risk. Define the enrollment choice, conditional on availability, as the following.[9]

$$e_j(\eta) = \begin{cases} \arg\max_{e \in \{0,1\}} \; e \cdot \int \nu_1(y - p_j - c(r))dF_j(r) & \\ \qquad\qquad\qquad + (1-e) \cdot \int \nu_1(y - r)dF_j(r), & \textit{if } \eta = 1 \\ 0, & \textit{if } \eta = 0 \end{cases} \tag{2}$$

Sorting decision. In the first period, the household optimally chooses a county of resi-

[8]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 flood insurance program, potentially biasing our estimates. Moreover, FEMA informed us that data from most of our study period is no longer available.

[9]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.

dence, 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,

$$
\begin{aligned}
&\max_{j,x}\ u(x, j), \qquad\qquad j \in \{0, 1, ...J\} \\
&\textit{subject to} \\
&p_x \cdot x + e_j(\eta_j) \cdot (p_j + c(r_j)) + (1 - e_j(\eta_j)) \cdot r_j = y
\end{aligned}
\tag{3}
$$

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 following modified optimization problem.[10]

$$
\max_{j \in \{0,1,...J\}} \int \nu_0\Big(y - e_j(\eta_j) \cdot (p_j + c(r)) - (1 - e_j(\eta_j)) \cdot r,\ j\Big) dF_j(r) \tag{4}
$$

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

[10] We estimate the significant effect of NFIP introduction on household purchases of insurance in Online Appendix B, though not explicitly accounted for in this model is the possibility that national flood 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 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).

insurance becomes available—which makes enrollment potentially non-zero (see Equation 2). We characterize this behavior in terms of certainty equivalence. A sufficient condition is when the household's optimal solution to Equation 4 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 4 in the following simplified notation.

$$j^*(p,\eta) = \underset{j\in\{0,1,\ldots J\}}{\arg\max} \int \nu_0\big(\tilde{y}(p_j,\eta,r),j\big)dF_j(r) \tag{5}$$

where $\tilde{y}(p_j,\eta,r)$ is residual income, as presented in Equation 4. 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 national flood insurance (r_1^*), and accepted risk-level under no availability (r_0^*).

$$\begin{aligned}
&\textit{for } \eta = 1, \\
&\qquad r_1^* \quad \textit{such that} \quad \nu_0\big(\tilde{y}(p_{j_1^*},1,r_1^*),j_1^*\big) = \int \nu_0\big(\tilde{y}(p_{j_1^*},1,r),j_1^*\big)dF_{j_1^*}(r) \\
&\textit{for } \eta = 0, \\
&\qquad r_0^* \quad \textit{such that} \quad \nu_0\big(\tilde{y}(p_{j_0^*},0,r_0^*),j_0^*\big) = \int \nu_0\big(\tilde{y}(p_{j_0^*},0,r),j_0^*\big)dF_{j_0^*}(r)
\end{aligned} \tag{6}$$

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 national flood 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.

$$\frac{\partial}{\partial r}\left\{\nu_0\big(\tilde{y}(p_j,\eta,r),j\big)\Big|_{\eta=1} - \nu_0\big(\tilde{y}(p_j,\eta,r),j\big)\Big|_{\eta=0}\right\} > 0, \quad \forall j \in \{0,1,\ldots J\} \tag{7}$$

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 7 is more negative, thus offsetting some of the risky behavior. This suggests the following holds true.

$$\frac{\partial^2 \nu_0}{\partial r \partial p} = \frac{\partial}{\partial p}\left(\frac{\partial \nu_0}{\partial \tilde{y}} \cdot \frac{\partial \tilde{y}}{\partial r}\right) < 0 \tag{8}$$

Equation 8 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 subsidized insurance can exacerbate risky behavior beyond efficient levels. Whether the NFIP has increased household risk tolerance (i.e., Equation 7, or simply $r_1^* > r_0^*$) is now an empirical question.

3.1 Illustration of Perverse Incentive

Equation 7 illustrates an unintended result of flood insurance, whereby a household, not subject to the full costs of flood risks, is willing to take on more risk when provided with flood insurance. Figure 3 depicts this inequality for two locations with two discrete levels of expected flood risk, $\bar{r} < \bar{r}'$, coming from two symmetric distributions—represented here by the bounds defined by the diagonal, dashed lines. In a setting in which there is no market for flood insurance, the household maximizes expected utility over the distribution of flood risks. The household's derived 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 an "efficient" market for flood insurance, where rates are set to the expected cost from flood risks. At this actuarially fair rate that the household pays with certainty, the marginal cost of insurance equals the marginal expected damages incurred by the household. Because of risk-aversion and diminishing returns, the household receives higher marginal utility of flood insurance in the greater risk zones, however, these increases in costs are fully priced into premiums.

Now consider the current setting under the NFIP, where flood insurance is offered at subsidized rates. Suppose premiums are set for each location at a fraction, $\theta \in (0, 1)$, of expected flood damages. Under the low-risk location, because residual income from subsidized insurance is greater than in the actuarially fair case ($y - \theta\bar{r} > y - \bar{r}$), the household receives a higher marginal utility of flood insurance under subsidized rates. In the insurance premium dimension, the result is an increase in utility *along* the curve. In terms of expected risk, the level of utility the household receives in the low-risk ($\bar{r}$) and high-risk ($\bar{r}'$) locations are represented as points C and C', respectively. The welfare returns from no insurance market to a subsidized market (i.e., $C - A$ and $C' - A'$) are even larger in the high flood-risk location.

In practice, when no market for flood insurance is available, a household might start as relatively indifferent between two locations at two opposite extremes of flood risk. That is, the high-risk location may offer similar levels of expected utility at point A' as the low-risk area at point A; for example, if the high risk area has a higher utility curve due to additional amenities, such as beach front property. The introduction of subsidized flood insurance eliminates this indifference, and the household favors the riskier property.

In this paper, we avoid characterizing this behavior as a "moral hazard," which normally depicts a risky, hidden action on the part of the policy holder, preventing the insurer from fully pricing the behavior into premiums. In our setting, national flood insurance is intentionally priced below actuarially fair levels in order to induce take-up by households. The resulting incentives that they produce are perverse—in the sense that they produce unintended results—and are certainly inefficient—as rates are below marginal damages—but actions are fully observed by the insurer through household location choices. Further, given the limited, natural occurring identifying variation from the program, this paper directly estimates the impact of the flood insurance program on migration patterns across different risk levels. That is, to the extent to which the counterfactual of NFIP is no market for flood insurance (which was often the case), we focus our attention on the magnitudes $C - A$ versus $C' - A'$, in terms of location choice. This paper does not explicitly decompose this effect into an efficiency improving (i.e., $B - A$) and efficiency diminishing (i.e., $C - B$) effect; however, as the NFIP prices premiums below actuarially fair levels, a strict monotonicity assumption implies an efficient level of consumption (resulting in utility at point B) exists between these two magnitudes (points A and C).

4 Empirical Strategy

This paper examines the perverse incentives created by the NFIP. We take a *revealed* preference approach to testing Equation 7. This amounts to estimation of a heterogeneous effect of nationally subsidized flood insurance on migration, by risk level. This is because, under these perverse incentives, the marginal loss from an increase in risk is smaller when flood insurance is available than when it is not. As described in Section 3, this reduction in potential risks creates higher marginal utility of flood insurance for residents of risky locations, than those of low-risk locations. Results indicating an increased *revealed* preference for communities with subsidized insurance is not sufficient in testing if households have changed their willingness to take on more risk, since the counterfactual is no national flood insurance (rather than low risk); however, in order to estimate the heterogeneous response of interest, we first focus our attention on the baseline impact of the program itself.

A community's decision to enter the flood insurance program is most likely endogenous and correlated with other factors that might drive population. To overcome this problem, we exploit FEMA's direct targeting of risky areas following the Flood Disaster Protection Act of 1973. We use the Flood Hazard Boundary Map (FHBM) assignments in the 1970s to isolate variation in risky areas with a higher propensity to enter the program. As shown in Figure 6, this group of targeted communities strongly correlates with empirical flood risk. 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 Flood Insurance Rate Maps (FIRMs), which describe the rate structure communities would face if they enroll 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 the introduction of FEMA and the subsequent rollout of FIRMs for FHBM communities, communities were given one year to join the NFIP before being sanctioned; thus, we exploit this intervention 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 (as described in Section 2). Furthermore, we expect that—conditional on FHBM assignment—the timing of the map upgrade to FIRMs is plausibly exogenous in our setting. The relationship in timing of map assignments is illustrated in Section 2. We demonstrate that our proposed instrument strongly predicts national flood insurance entrance by estimating the following first stage equation.

$$postNFIP_{cst} = \alpha \cdot postFIRM_{cst} \times FHBM_{cs} + x_{cst}\tilde{\beta} + \mu_t \times FHBM_{cs} + \tilde{\lambda}_{st} + \tilde{\gamma}_{cs} + \tilde{\varepsilon}_{cst} \tag{9}$$

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 in 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.[11] α estimates the relationship between FEMA targeting 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 would come through their impact on migration decisions, as the characteristics of a community are often determined by the population residing there. We allow FHBM communities 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 insurance rate map update (i.e., FIRM) 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 requires that changes in population over time in the FHBM counties would track closely with non-FHBM counties, absent FIRM assignment, *and* that these FEMA interventions affect our primary outcomes only through national flood insurance enrollment (i.e., the exclusion restriction). 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 conditional 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. These results are presented in Appendix G.

[11] Conditioning directly on FHBM communities is not feasible in our setting as we aggregate to county-level, producing fractions rather than binary indicators. See Figure 4 for the distribution of this variable. In Online Appendix E, we condition on only counties with at least one FHBM, and additionally weight observations by fraction of a county with a FHBM. In Online Appendix G, we examine an alternative to this approach which uses FIRM timing on the entire sample.

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 may drive population growth, such as new construction and infrastructure projects.[12] 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 E, we produce estimates that identify strictly off of the initial set of communities orginally flagged by FEMA as flood-prone.[13] Doing so produces similar results. Our primary estimating equation for the intent-to-treat (ITT) effect of the NFIP on population flows is the following:

$$migration_{cst} = \delta \cdot postFIRM_{cst} \times FHBM_{cs} + x_{cst}\beta + \mu_t \times FHBM_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \tag{10}$$

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.

We interpret FHBM assignment as indicating the relatively riskier counties with a higher propensity to be treated. Because we use FHBM assignment to proxy for the counties that are ultimately impelled to enter the regular program, a positive effect on population ($\delta > 0$) would suggest that the subsidized flood insurance program increased the incentive to reside in these more flood-prone locations. However, as the less risky, non-FHBM communities,

[12] 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 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.

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

had a lower propensity to enter the regular program, our specification above does not directly estimate the *relative* impact of subsidized flood insurance *across* risk levels. That is, Equation 10 only estimates a treatment (intent) effect on the treated, and does not speak to the impact on less risky, less likely to be treated groups, who may have had a similar response. To state this a different way, an overall positive effect of the NFIP on migration does not alone demonstrate an increased willingness to take on risk; and a homogeneous response across different risk levels would even argue against increased risk-taking by households. Testing Equation 7 proceeds by testing for a heterogeneous response.

To provide evidence of NFIP-induced risk taking, we exploit *within* FHBM variation in empirical risk level to directly test the expression in Equation 7. In Section 7, we estimate a heterogeneous treatment effect of nationally subsidized flood insurance. Formally, we estimate the following equation.

$$\begin{aligned} migration_{cst} =& \delta_0 \cdot postNFIP_{cst} \\ &+ \delta_1 \cdot postNFIP_{cst} \times \textit{flood risk}_{cs} \\ &+ x_{cst}\beta + \mu_t \cdot \textit{flood risk}_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \end{aligned} \tag{11}$$

where $\textit{flood risk}_{cs}$ is our measure of average annual flood episodes for a county-state, cs. We control for time-varying confounders specific to flood-prone areas, $\mu_t \cdot \textit{flood risk}_{cs}$. 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. It measures the additional effect of the NFIP coming from one additional flood episode per year. Whereas δ in Equation 10 gets at the treatment effect of the NFIP, δ_1, in Equation 11, estimates whether subsidized flood insurance increases the likelihood that households reside in high-risk versus low-risk locations. This specification is, therefore, directly analogous to the cross-derivative in Equation 7 (depicted in Figure 3). An estimate of $\delta_1 > 0$ implies that the NFIP produces a larger increase in population in historically risky areas, suggesting that households internalize this reduction in expected risk from the coverage of potential losses. We define a county's flood risk according to the average annual flood episodes experienced in that area over the timespan of our data, as reported by NOAA.[14]

Estimation of Equation 11 is performed by two-stage least squares, using the map assignments from Equation 9 and their interaction with flood risk as instruments for the baseline and heterogeneous effects of the NFIP, respectively. In Appendix I, we discuss the potential for δ_1 to be identified off of endogenous NFIP enrollment. This approach assumes that selection into the program is constant across flood risk levels and absorbed into the baseline effects (δ_0). We examine the potential for this by comparing the pre-NFIP trends between high and low risk areas in an event study setting.

5 Data

5.1 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.[15] 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 their 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. As is standard, we construct all of our population outcomes using the number of exemptions to proxy for the number of people.[16] 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 national flood insurance adoption (see

[14]Due to data constraints, we use in-sample floods rather than pre-NFIP (or pre-flood map) floods. We demonstrate robustness in Appendix H, which present similar results when using flood counts in the initial period of observation.

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

[16]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.

Figure 1, Panel B).

The IRS data have a few potential limitations, which we account for in this paper. First, for confidentiality the IRS does not report totals based on fewer than 10 tax returns. While this should not affect our 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. Finally, these data will not reflect moves by those individuals not required to file an income tax return. In Appendix F, we show that we obtain similar results when using BEA population data.

5.2 National Flood Insurance Program

Constructing our instrument 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.[17] The data contain information on the publication dates of community-level Flood Hazard Boundary Maps (FHBM) and Flood Insurance Rate Maps (FIRM), and the dates that communities adopted the NFIP or were sanctioned by FEMA for not joining the NFIP. The inclusion of 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 pushed 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.[18] An issue which sometimes arises when aggregating up to county is that communities and coun-

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

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

ties can overlap. 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. As this will most likely show up as classical measurement error in an explanatory variable, it should only attenuate our point estimates for effects at the county-level.

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 4 presents the distribution of counties by the fraction of their communities with a FHBM. More than 70 percent of counties have at least one community under FHBM, and 45 percent of counties are comprised entirely of FHBM communities. However, only a small fraction of land in an FHBM community is typically a Special Flood Hazard Area. According to FEMA Agency (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. Figure 6 shows that there is a positive and statistically significant relationship between emergency program status and historical flood risk, as measured by a cross-section of mean-annual floods recorded by NOAA.

5.3 Other Data

We also 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.[19] 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

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

for a federal disaster declaration requires documentation of damage assessments. The lack of information on declined requests for disaster declarations is a potential limitation of using these data.[20] We also use data on historical flood episodes published by FEMA for our analysis in Section 7. FEMA generated the data by processing multiple datasets from NOAA, FEMA Individual Assistance (IA) and the NFIP.[21]

County-level characteristics and demographics data are additionally collected 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 presented in Table 1.

6 Baseline Effects of NFIP Enrollment

6.1 First Stage Estimates

We begin our analysis by testing the relevance of our instrument for national flood insurance enrollment. It is important to note that, 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 incentivized to join the NFIP following map upgrades, we anticipate a near one-to-one relationship.

Estimates for Equation 9 can be seen in Table 2. Standard errors are clustered by county. 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 (Col-

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

[21] See FEMA.gov/data-visualization. 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.

umn 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 (i.e., those from Equations 10 and 11) 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 7, 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.

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 policy 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 Online Appendix B, 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 10, with several lagged and leading terms for FIRM assignment. Specifically, we estimate the following event study specification.

$$\begin{aligned} migration_{cst} = & \sum_{l=-\underline{L}}^{\bar{L}-1} \delta_l \cdot newFIRM_{cst-l} \times FHBM_{cs} + \delta_{\bar{L}} \cdot postFIRM_{cst-\bar{L}} \times FHBM_{cs} \\ & + x_{cst}\beta + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \end{aligned} \tag{12}$$

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, and therefore tests for parallel trends in the pre-FIRM period.

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 12. Using actual NFIP enrollment allows us to directly test for reverse causality, in a *Granger*-causality sense. The naïve estimates are presented in Figure 8. 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.

With sufficient evidence that invalidates the naïve approach, we directly test for *Granger*-causality in our primary design, instrumenting for the NFIP with FEMA interventions. Estimates from Equation 12 are presented in Figure 9. It is immediately clear that for population (Panel A) and non-migration (Panel B) outcomes, exploiting FEMA map publication timing allows us to circumvent the same confounding factors that partially determine national flood insurance adoption.

Furthermore, we do not see any 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. On the other hand, Panel C shows that our in-migration outcome still exhibits positive, though insignificant, divergence in the year prior to treatment. These inflow data make up a much smaller proportion of total population levels than our non-migrant outcome, and thus may be more susceptible to noise. For these reasons, we will primarily focus on our *population* and *non-migration* outcomes. Furthermore, in Section 6.3, we will show that in most specifications, average effects of the NFIP on *in-migration* are not statistically significant, whereas estimates on *non-migration* and *population* are.[22]

6.3 The Effect of the NFIP on Population Flows

Table 3 presents the reduced form estimates from Equation 10. 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 5%, or about a 5.25% when scaling by the first stage. 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 county-level, time-varying controls, including building permits, per-capita income, and unemployment rates. Any potential for endogenous selection into FIRM assignment—correlated with these observables—should be apparent from this specification; however, including these controls does not significantly change our estimate. In Online Appendix C, we extend this sensitivity analysis to additionally include lags of these controls.

We might be concerned that, rather than responding to flood insurance availability, households are instead responding directly to previous major disasters which triggered a response from these communities in the form of 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 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 9, 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.

[22]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.

Though not statistically different from the estimates in our base specification (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 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 should 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.

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 63%.[23] This is because in-migration encompasses a much lower baseline level of variation in population than that of non-migration. In contrast, an estimate of 5.4% is needed on non-migration in order to fully account for the changes in population, which is on par with our estimates.

Our estimates illustrate a large impact of subsidized flood insurance on the long-run population of a community—estimated to increase by a magnitude of 5 percent. We confirm these estimates using an alternative dataset—population counts from the BEA—in Online Appendix F. 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%. This suggests year-to-year migration on par with our long-run impact of subsidized flood insurance. Further, in Online Appendix B 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.

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 risk in order to estimate the added impact subsidized flood insurance has on areas which experienced historically more floods than other areas that are also treated. In contrast to the baseline impact of the NFIP estimated in the previous section, this approach directly tests the expression in Equation 7, 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 10, we split our data into below- and above-median number of annual flood episodes (as reported by NOAA) and plot the reduced form coefficients of Equation 12 for each sub-

[23] This estimate is calculated by regressing our outcome, $log(population)$, on our two sources of migration: $log(in-migrants)$ and $log(non-migrants)$—which tells us that 8% of the variation comes from in-migrants, relative to the 92% from non-migrants. This allows us to compute the coefficient that would arise if NFIP were only affecting population through in-migration, as 0.05/0.08.

sample.[24] As with Figure 9, we see little evidence that either the low or high historical risk-level, treated counties diverge from the control before treatment. Furthermore, the fact that low-risk, treated counties track the high-risk, treated counties before treatment supports the validity of our approach. Figure 10 shows that high-risk, treated counties exhibit a greater divergence after treatment, indicating that the majority of the effect of national flood insurance comes from the high-risk counties in our sample. This result is consistent with the condition in Equation 7. In Appendix I, we show results when defining treatment by (endogenous) NFIP enrollment.

Our main estimates from Equation 11, estimated by two-stage least squares, are presented in Table 4. Column 1 presents our base specification. We include demographic controls in Column 2 and add declared 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.[25] This result fully characterizes the types of incentives which 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 characteris-

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

[25] 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.

tics; 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 roughly tests how much other observable attributes could 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.

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 5) 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 speak to the impact of underpriced insurance. However, we suspect that flood insurance was largely unavailable in many of these communities prior to the NFIP and are, therefore, unable to directly disentangle the efficiency impact of reduced risk from the additional distortionary behavior produced from inefficient pricing.[26] Our results, therefore, can only suggest that households are sufficiently mobile to respond to such incentives and that subsidized premiums have (unintentionally) exacerbated the risky behavior we uncover. However, 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.

[26] A discussion of this is presented in Section 3.

8 Discussion and Conclusion

This paper presents evidence of costly, unintended consequences produced by the U.S. National Flood Insurance Program (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: the NFIP causes larger population increases in riskier areas. Thus, our findings suggest that the private benefit households receive in the form of a reduction in potential risks produces adverse behavior, 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.

Subsidizing flood losses provides consistent incentives to rebuild and reside in areas with high risk. Our results show that households are mobile, producing costly behavior from national flood insurance which is only exacerbated by its subsidized nature. Our estimates of the external costs produced by the NFIP suggest that it 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 subsidized flood insurance. With growing concerns over the financial sustainability of the flood insurance program, this may mean restructuring the program sooner rather than later.

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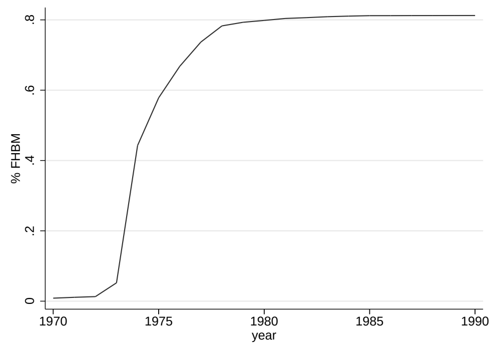
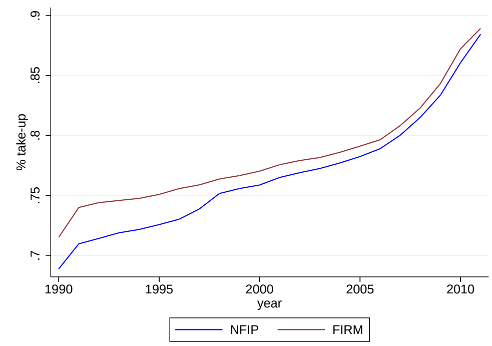
Figures

Figure 1: FHBM Roll-Out; NFIP Enrollment and FIRM Assignment

(a) FHBM Roll-Out

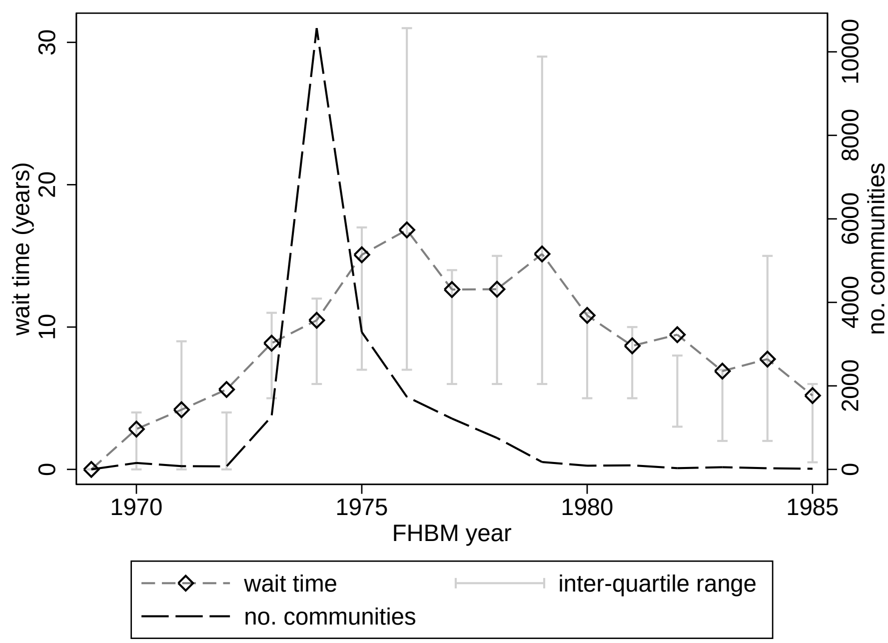


(b) NFIP Enrollment and FIRM Assignment



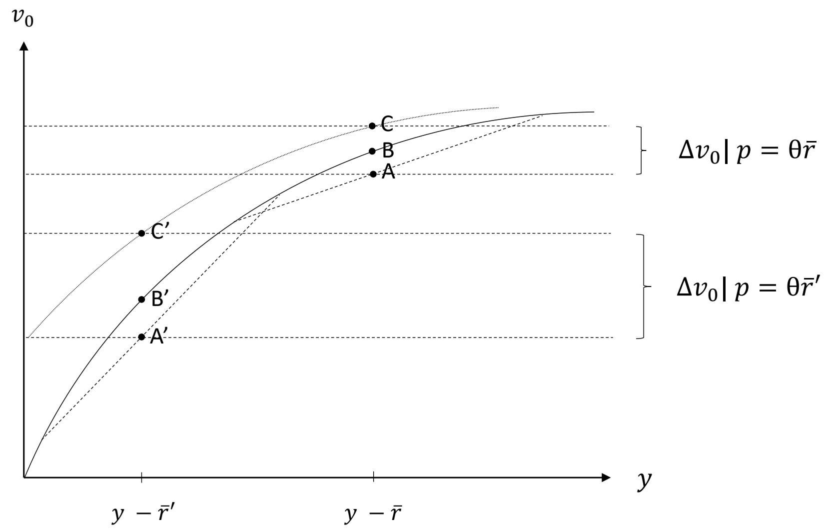
Note: Panel A plots the roll-out of flood hazard boundary maps (FHBMs), which covers the periods taking place prior to the start of our data. Panel B plots the fraction of counties enrolled in the National Flood Insurance Program (NFIP) and assigned a flood insurance rate map (FIRM) for the timespan of our data.

Figure 2: FIRM Wait Time and Distribution of Pending FIRMS by FHBM Year



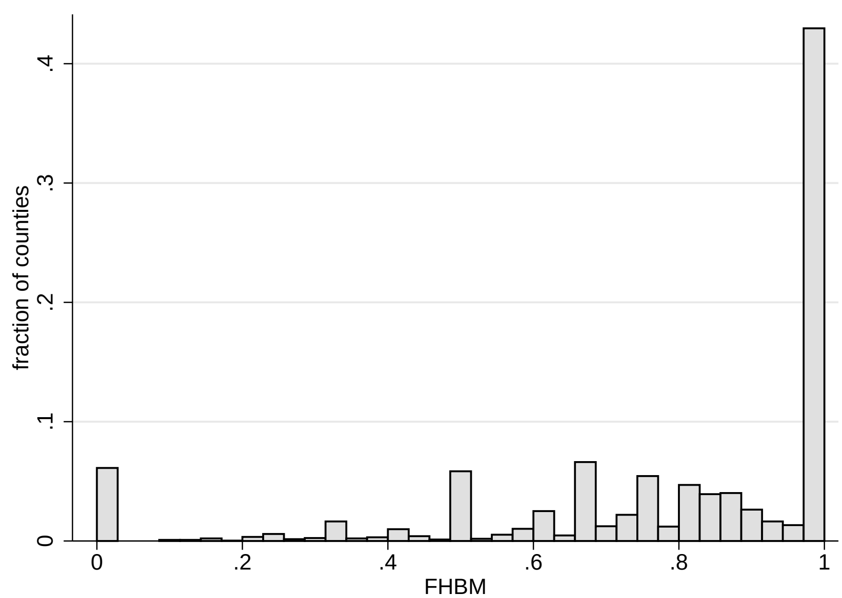
Note: This figure plots the number of years that a FHBM community waited to get their FIRM upgrade, as well as the distribution of communities assigned a FHBM by year of assignment. Vertical bars indicate the inter-quartile range of wait time.

Figure 3: Marginal Utilities from Flood Insurance



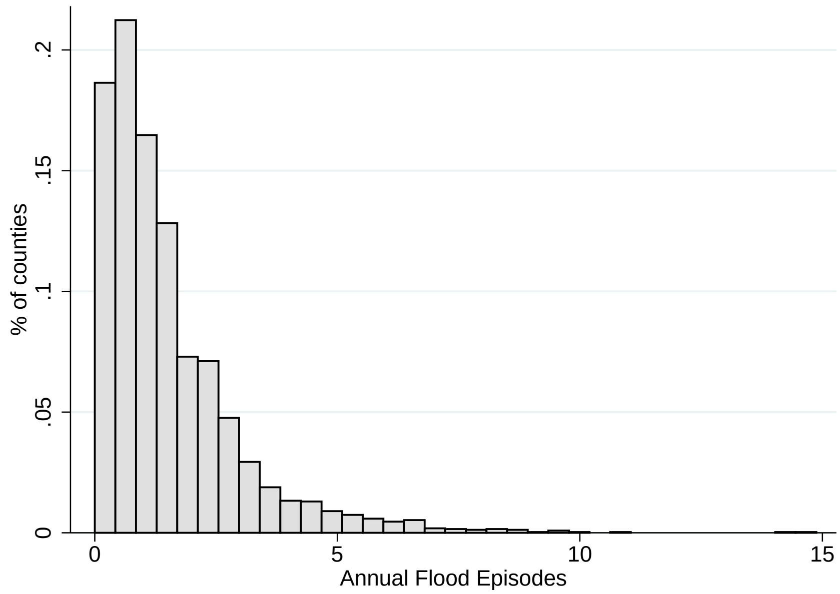
Note: A utility function for household location choice is illustrated, depicting the marginal effect of flood insurance under two discrete levels of expected risk, $\bar{r} < \bar{r}'$, coming from two different distributions of flood risk. The figure depicts a representative household's level of (expected) 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 $\theta \in (0, 1)$.

Figure 4: Distribution of FHBM Counties



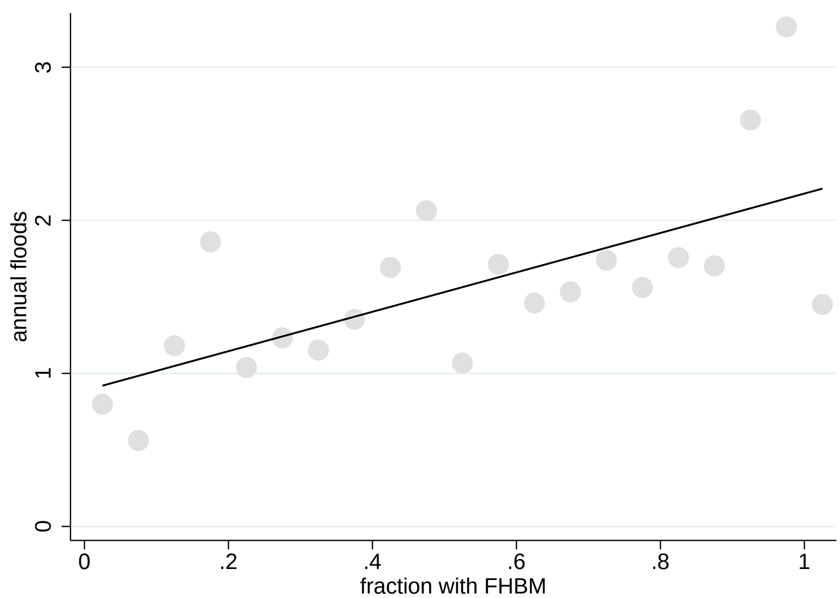
Note: This figure plots the distribution of fractions of a county (communities within a county) which FEMA has identified as flood-prone by publishing a Flood Hazard Boundary MAP (FHBM) for them. The vast majority of these assignments occurred in the 1970s.

Figure 5: Distribution of Flood Risk Across Counties



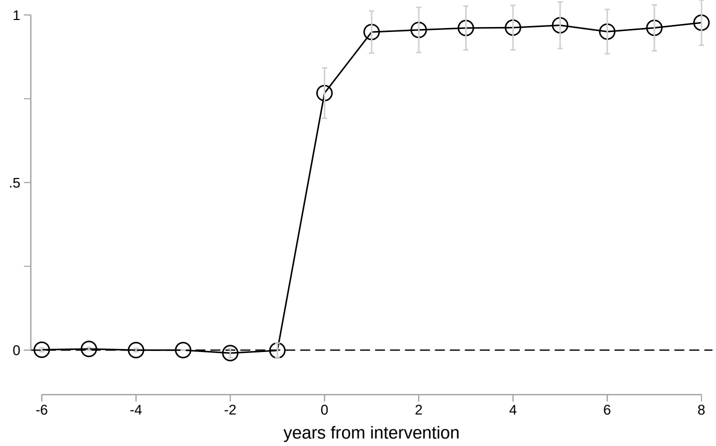
Note: This figure plots the distribution of county flood risk, defined as historical annual flood episodes (reported by the National Oceanic and Atmospheric Administration).

Figure 6: Relationship Between FHBM and Flood Risk



Note: This figure presents a bin scatter plot of the fraction of a county with a Flood Hazard Boundary Map (FHBM) (on the horizontal axis) and historical annual flood episodes (reported by the National Oceanic and Atmospheric Administration), with a bin size of 0.05 FHBM. The fitted line has a slope of 0.73 with a t-statistic of 8.47, clustered by county.

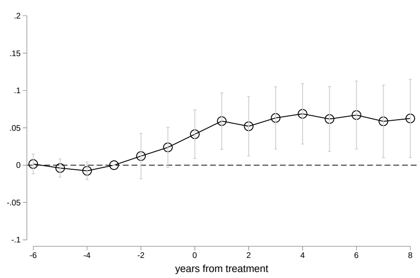
Figure 7: First Stage: The Effect of FEMA Intervention on NFIP Enrollment



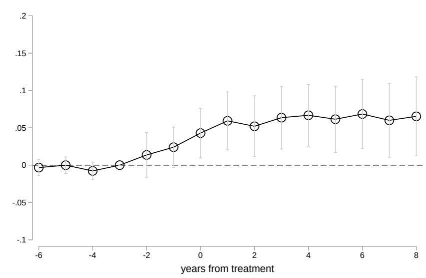
Note: This figure presents the dynamic coefficients from our first stage regression of the effect of post-FIRM FHBM intervention by FEMA—which granted affected areas a 1 year grace period before sanctions were imposed—on actual NFIP enrollment. 95 percent confidence interval bars are presented. Standard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

Figure 8: Event Study Specification for (Endogenous) NFIP: Naïve Specification

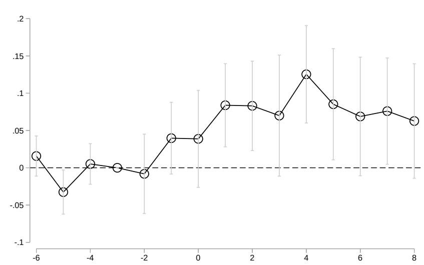
(a) Effect of NFIP on (log) Population



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



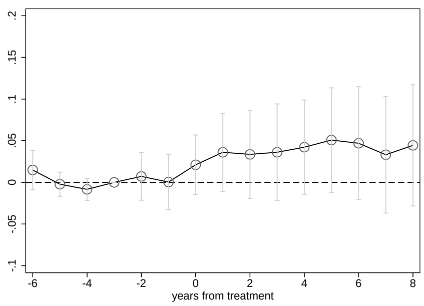
(c) Effect of NFIP on (log) Migrants



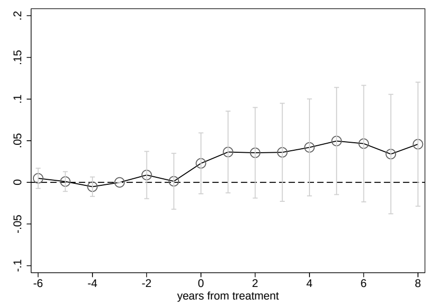
Note: This figure plots the coefficients from a naïve version of Equation 12, using lagged and leading National Flood Insurance Program (NFIP) enrollment indicators directly as the explanatory variables of interest. Coefficients are estimated relative to the third leading term (rather than the first, 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.

Figure 9: Event Study Specification for Intent to Treat of NFIP

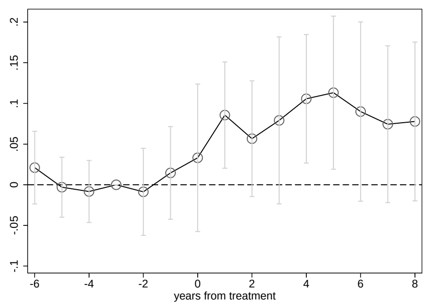
(a) Effect of NFIP on (log) Population



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



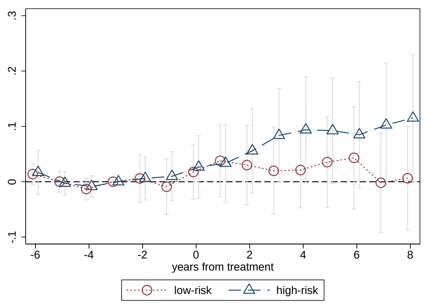
(c) Effect of NFIP on (log) Migrants



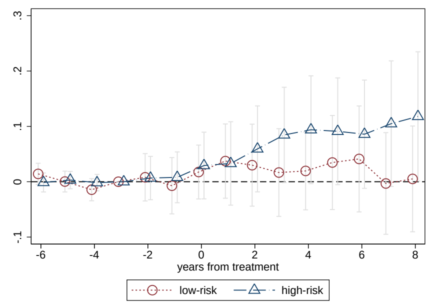
Note: This figure plots the reduced-form coefficients from Equation 12, using lagged and leading Flood Insurance Rate Map terms in our instrument. Coefficients are estimated relative to the third leading term. Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented. For consistency, we maintain the use of t-3 as the reference period.

Figure 10: Heterogeneous Effect of NFIP, by Flood Risk

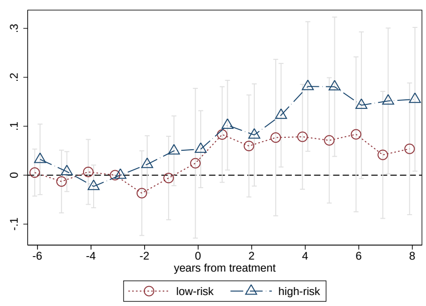
(a) Effect of NFIP on (log) Population



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



(c) Effect of NFIP on (log) Migrants



Note: This figure plots the reduced-form coefficients from Equation 12, 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. Coefficients are estimated relative to the third leading term. 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. 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. We use the number of tax exemptions as a proxy for population, where non-migrants are defined as returns filed in the same county in back-to-back years, and migrants refer to filings in a different county from one year to the next.

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

Post-NFIP	(1)	(2)	(3)	(4)
postFIRM $\times$ 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 Enrollment (NFIP) enrollment on Flood Hazard Boundary Map (FHBM) and Flood Insurance Rate Map (FIRM) assignment. Column 2, includes time-specific constants on our FHBM measure. In Column 3, we control for county-level characteristics including building permits, total value permitted housing units, per-capita income, unemployment rate, and job counts. Column 4 includes county-level water-related declared natural disasters. The estimates are not sensitive to the inclusion of these controls (up to the thousandths decimal place for Columns 2 through 4). 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)
$\text{newFIRM}_{t-2} \times \text{FHBM}$							0.0420**
							(0.0200)
Panel C: Log- Migrants							
postFIRM × 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)
$\text{newFIRM}_{t-2} \times \text{FHBM}$							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 10 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 our heterogeneous effects from Equation 11 with outcomes log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c). Coefficients of interest are on the additional impact of the NFIP from one additional flood per year. 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 × Annual Floods	0.0393**	0.0354**	0.0406**	0.0465***
	(0.0169)	(0.0166)	(0.0168)	(0.0162)
postNFIP × Income		0.0544*	0.0584**	0.0444
		(0.0295)	(0.0293)	(0.0279)
postNFIP × Jobs			-0.0617	-0.519*
			(0.0933)	(0.308)
postNFIP × 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 another 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.

A Appendix: 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,[27] 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***

[27] (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)

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."

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).[28]

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.

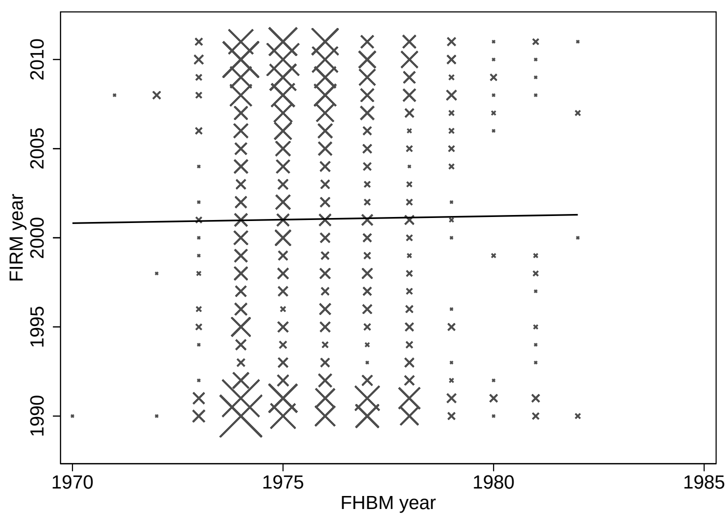
Because FHBMs are essentially preliminary flood maps that FEMA intends to upgrade to FIRMs, it is possible that this observed decreasing wait time is related to the timing in which communities "enter" and "exit" the "queue." Figure 2 plots the distribution of communities by the year of their FHBM and 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

[28]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.

is unsurprising given the constraints on the number of studies FEMA was able to conduct per year (Comptroller General of the United States, 1976).[29] 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 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.

[29]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."

B Appendix: Map Assignment and Corresponding Insurance Take-Up

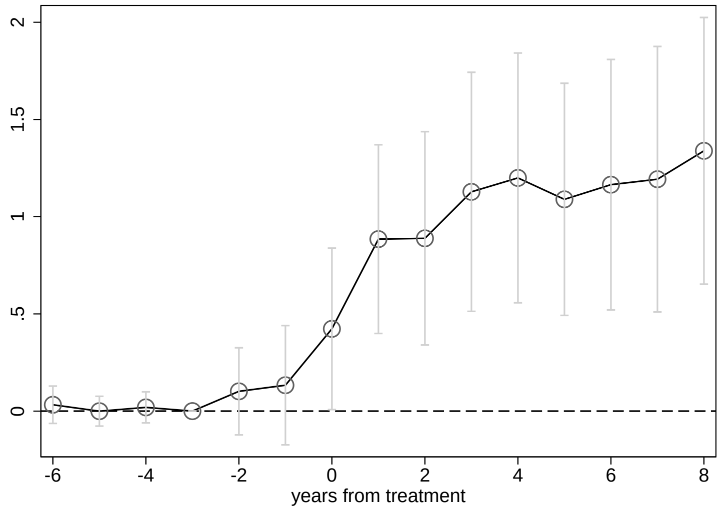
Our data on county-level policies are aggregated from transaction-level policies published by the Federal Insurance & Mitigation Administration (FIMA).[30] The data contain about 50 million policy transactions, and represent the NFIP system of record at the time the data were published. The available data date to December 2019, and contain the original effectivity date of each flood insurance policy. Because FIMA 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.2, 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 10) is estimated at a highly significant 0.94 (robust standard error of 0.12).[31] 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.

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

[31] 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.2: Dynamic Coefficients on Cumulative Flood Insurance Policies



C Appendix: 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.1 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.1: 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

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

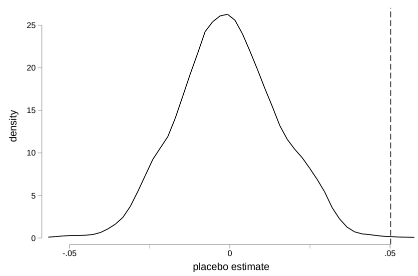
D Appendix: Placebo Tests

To further verify the robustness of our main results, we perform a series of placebo treatments—similar to the approach proposed by Bertrand, Duflo, and Mullainathan (2004) who show the inconsistency of standard errors in difference-in-differences estimates. For each permutation, we randomly *re-assign* treatment from the set of all counties. In each permutation, we jointly draw, with replacement, a set of placebo *FHBM* and *postFIRM* variables for each county.[32] In each permutation, we construct our placebo (reduced form) treatment variable and estimate Equation 10 on each outcome. This procedure produces a check on our county-level clustered standard errors. We perform 1,000 permutations using the same specification as Column 1 of Table 3—with only county and state-by-year fixed effects. The resulting kernel densities of the placebo estimates are presented in Figure A.3. In each of the panels, the vertical dashed lines correspond to the respective Column 1 estimates in Table 3. We use this distribution of placebo estimates to compute one-tailed p-values, in a manner consistent with synthetic control methods (Abadie, Diamond, and Hainmueller, 2010). Panels A and B illustrate that our point estimates are in the far right tails of the distributions. In fact, from 1,000 permutations, only one placebo estimate is greater than our true point estimates for both population and non-migrants. The results for our in-migration outcome produce a one-sided p-value of 0.035. These results are consistent with our county-level clustered standard errors and confirm that it is highly unlikely that the size of our estimates are the result of chance.

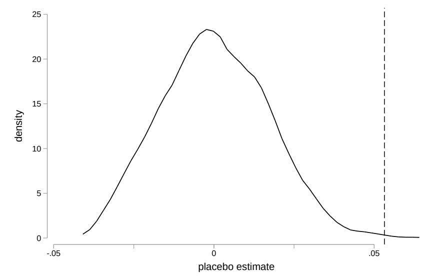
[32]Because each community in a county is treated at different times, we observe unique paths of *post-FIRM* for each county. Therefore, we randomly take an empirical path for this variable from the sample of counties, and re-assign it as a placebo path.

Figure A.3: Placebo Estimates

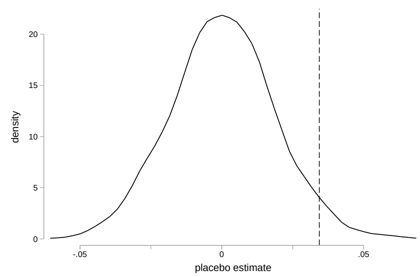
(a) Effect of NFIP on (log) Population



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



(c) Effect of NFIP on (log) Migrants



Note: Above are the resulting distributions of 1,000 placebo estimates of Equation 10 for each migration outcome. The dashed lines mark the corresponding point estimates from Table 3. The implied (one-tailed) p-values are 0.001, 0.001, and 0.035 for Panels a, b, and c, respectively.

E Appendix: 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—since we aggregate to county due to the nature of our outcomes—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.2, 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.2: 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

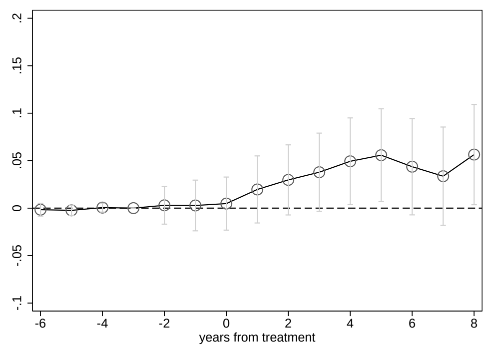
* $p < 0.1$, ** $p < .05$, *** $p < .01$

F Appendix: Replicating Results using BEA Population Data

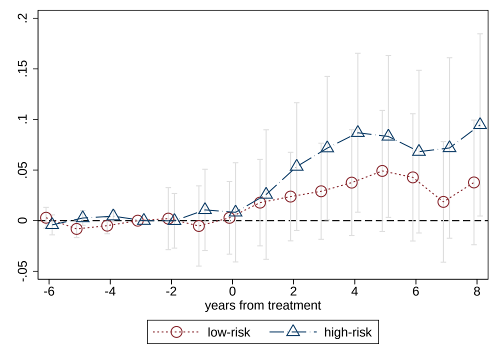
This paper uses data from the Internal Revenue Service to pin down the impacts of the NFIP on migration. Though these data are limited to tax filers, they allow us to disentangle in- versus out-migration effects. Below we recreate our main estimates using population data from the Bureau of Economic Analysis (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



G Appendix: 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, rather than *postFIRM* for the "more likely to be treated," *FHBM*, group (i.e., the interaction $postFIRM \times FHBM$). Doing so assumes that FIRM timing was exogenous for all communities, not just emergency group communities—for whom FIRMs were an update 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.3.

Table A.3: 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.[33] Next, we estimate our reduced form equation with *postFIRM* as our instrument. This is simply the analogue to Equation 10, and Table 3 results, but substituting *postFIRM* for *postFIRM-FHBM*. The results are in Table A.4.

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

Table A.4: Effect of FIRM on Migration

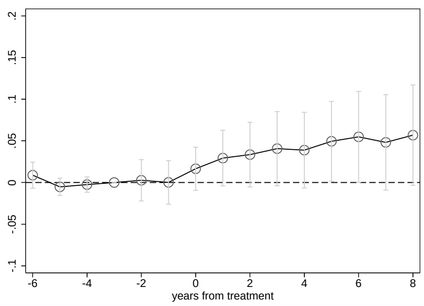
Migration Outcome	(1)	(2)	(3)	(4)
Panel A: Log- Population				
postFIRM	0.0370***	0.0345***	0.0333**	0.0333**
	(0.0131)	(0.0124)	(0.0135)	(0.0135)
Leading Treatment			-0.00613	-0.00615
			(0.00882)	(0.00882)
Panel B: Log- Non-Migrants				
postFIRM	0.0391***	0.0362***	0.0354**	0.0354**
	(0.0134)	(0.0126)	(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

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

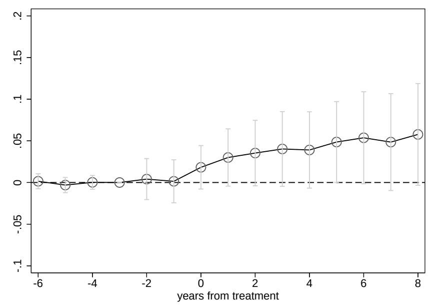
These results produce a smaller reduced form estimate than our original instrument. This is to be expected with a smaller first stage. Estimates for population indicate a 3.3 to 3.7 percent effect. Scaling by the first stage of 0.76, our results indicate an effect of about 4.3 to 4.9 percent. These results are very similar to our primary estimates, and the same holds true for our other migration outcomes. Therefore, though we expect the timing between emergency program entrance 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

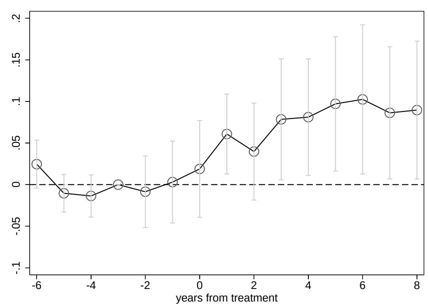
(a) Effect of NFIP on (log) Population



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



(c) Effect of NFIP on (log) Migrants



H Appendix: 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.5, 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.5: 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

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

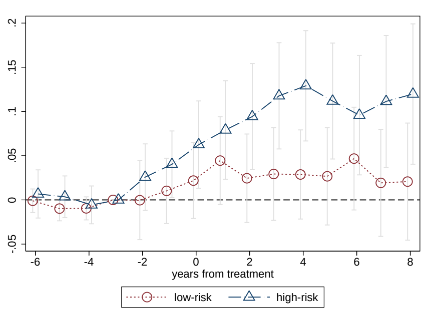
I Appendix: Heterogeneous Effects Using NFIP Directly

Section 6 illustrates the issues that arise when estimating directly off of potentially endogenous community enrollment into the flood insurance program. These problems occur when estimating the baseline effects of flood insurance availability on migration; however, if this bias is constant across communities with different risk levels, a heterogeneous effect may still be identified. That is, if high risk and low risk communities select into the program in similar ways over time, but households respond directly to the insurance availability in these communities in different ways, we can directly pin down these differences. In the context of a triple-differences approach, identification of a heterogeneous effect across high- and low-risk communities does not require that trends between NFIP and non-NFIP groups follow similar trends absent NFIP, just that the extent to which they may diverge from each other is due to differences in risk.

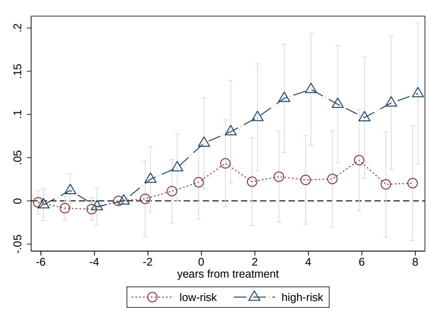
In Figure A.6, we examine the potential for identification of a heterogeneous effect of, potentially endogenous, NFIP entry. We plot the dynamic coefficients of our naïve specification for two separate samples: below and above median floods. As in Figure 8, divergence in migration is apparent prior to treatment; however, the two groups do not seem to diverge from each other significantly until the communities enroll into the program. Furthermore, after accounting for the pre-NFIP differences between the two groups, estimates are on par with those from our instrumental variable regressions.

Figure A.6: Heterogeneous Effects of NFIP, by Flood Risk

(a) Effect of NFIP on (log) Population



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



(c) Effect of NFIP on (log) Migrants