Moving to Flood Plains:
The Unintended Consequences of the National Flood Insurance Program on Population Flows

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September 10, 2018

Abstract

Despite the large costs of covering flood losses, little is known about whether flood insurance availability affects the decision to live and stay in more flood-prone areas. In this paper, we present evidence that suggests households in flood-prone areas would have otherwise moved to less risky areas, absent flood insurance availability. We identify the effect of flood insurance availability on population flows by exploiting the within- and across-county variation in the various programs the federal government implemented to encourage flood-prone areas to join the National Flood Insurance Program (NFIP). Results suggest that flood insurance availability caused population to increase by 4 to 5 percent in high flood-risk counties. Furthermore, we find that NFIP causes a 4.4 percent increase in population per one standard deviation increase in risk. Our findings highlight the potential for flood insurance availability to contribute to flood damages by altering incentives to reside in risky areas.

JEL Codes: Q58, H12, H84
Keywords: Environmental Policy, Crisis Management, Disaster Aid

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Any errors are our own.

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 also 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 locating 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, 2017). Moreover, the economic effects of these disasters persist years afterward. 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) documents that direct disaster aid provided to affected counties amounts to $155-$160 per capita, in addition to a further $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 insurance has induced people to move to or stay in flood-prone areas. This moral hazard response would increase the burden on taxpayers, adding to the program's already existing inefficiencies (Kahn and Smith, 2017). While identifying moral hazard is important, it is difficult to identify causal effects because joining the NFIP is voluntary and can be driven by several factors. The resulting potential for reverse causality means that a naïve, direct examination of the impact of NFIP adoption on population growth may produce biased estimates. For example, increased migration into a county could alter its flood risk or increase its relative demand for coverage, making it more likely to enroll into the program.

To mitigate these concerns, we instead test for moral hazard from the NFIP by exploiting programs that the Federal Emergency Management Agency (FEMA) implemented to pressure risky communities to join the NFIP. FEMA is mandated to identify and map out areas with elevated flood risks. It then imposes sanctions on these areas if they do not join the NFIP within a year of their flood map being published. Since FEMA identifies these risky areas in an objective manner—based on risk-level—we see this variation as independent of endogenous inter-temporal factors that can also determine NFIP enrollment. We examine how population flows change in more risky counties relative to less risky counties, exploiting the plausibly exogenous publication timing of flood maps developed for flood-prone areas. Our identification of the causal effect of NFIP enrollment on population in risky areas assumes that absent the pressure from FEMA to join the NFIP, these counties would have experienced population changes similar to low-risk counties. We present evidence in favor of this assumption by demonstrating that more risky and less risky counties follow similar trajectories in migration patterns prior to the publication of flood maps, and only diverge following map publication.

Results indicate that flood insurance availability affected the decision to live and stay in more flood-prone areas. We estimate that population in counties whose communities are pressured to join the NFIP increases by 4 to 5 percent. This effect is primarily driven by existing residents choosing to remain in these high-risk areas when they receive federal flood protection, where the counterfactual response would have been to migrate out. By comparison, we find little evidence of increases in migration to flood-prone 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. These estimates are robust to various specifications—including adding time-varying controls for county-level income and employment characteristics, and county-specific time trends—and to accounting for water-related natural disaster events which might induce take-up.

We provide further evidence of moral hazard by estimating the heterogeneous effects of NFIP by underlying flood propensity. To do so, we use cross-sectional information on historical flood risk to examine the treatment intensities of NFIP across different levels of empirical risks. Whereas our original estimates measure a treatment effect on the primarily risky counties that FEMA directly targeted, our heterogenous estimates fully incorporate empirical risk levels into these treatment effects. Using this approach, we estimate that NFIP produces an additional 4.4 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; though we do find some evidence of increased migration into these areas.

This study contributes to the literature in the following ways. To our knowledge, we provide the first causal evidence of the effect of flood insurance availability on population growth in flood-prone areas. In doing so, we complement existing work on the take-up and effects of flood insurance, which document a positive relationship between flood risk and flood insurance demand in coastal counties (Landry and Jahan-Parvar, 2011; Kriesel and Landry, 2004). Gallagher (2014), and Kousky and Shabman (2014) find that flood insurance take-up increases in the year following hurricanes and large flooding events, even though the underlying flood risk in the area did not change. 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.

More broadly, because our results suggest that people take on more risk, we also contribute to the larger body of research on the moral hazard responses to insurance availability. Such responses have been found to occur with health insurance (e.g., Einav, Finkelstein, Ryan, Schrimpf, and Cullen, 2013; Spenkuch, 2012; Keane and Stavrunova, 2016), life insurance (Cawley and Philipson, 1999), and automobile insurance (Weisburd, 2015; Dionne, Michaud, and Dahchour, 2013).

Finding evidence of a moral hazard from 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 the moral hazard produced 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, 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 moral hazard produced by the NFIP led to costs that were 6.6% higher than they would have been absent the program. As for Harris County (Houston), we estimate that the NFIP was responsible for a meaningful 15% 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 (Barreca et al., 2016). We show that NFIP 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 moral hazard-associated costs of insuring populations against flood risks, suggest significant inefficiencies in the rate structure of the program. Therefore, going forward, we propose that policy aim to better incorporate heterogeneous flood risks into insurance premiums.[1]

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 for moral hazard in national flood insurance. In Section 4, we present our empirical strategy to estimate the magnitude of moral-hazard from flood insurance. In Section 5, we discuss the data used in this paper. In Section 6, we present our primary results. In Section 7, we

[1]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, our findings suggest that flood insurance rates are not high enough in risky areas to offset the negative externalities associated with moral hazard.

extend our primary estimates to allow heterogeneous effects by risk level. Finally, in Section 8, we conclude with a brief discussion of our findings.

2 Background

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). Beginning in 1973, various federal programs were implemented to systematically identify and price underlying flood risks. The Flood Insurance Administration in the Department of Housing and Urban Development created the earliest flood maps, which were called Flood Hazard Boundary Maps (FHBMs). While these maps are not as detailed as present-day flood maps, they indicate which communities were more flood-prone before flood insurance was available. Most of these flood-prone communities were identified between 1973 and 1978.[2]

In 1979, the responsibility of creating flood maps was transferred to the Federal Emergency Management Agency (FEMA). FEMA creates Flood Insurance Rate Maps (FIRMs) rather than FHBMs. Relative to FHBMs, FIRMs depict more levels of flood risk within mapped communities (Morrissey, 2006). The level of detail on FIRMs allows premiums to vary by the riskiness of the zone in which the property is located, and identifies which properties are required to carry flood insurance. FEMA is also mandated to evaluate the need to create new or update existing flood maps at least once every five years. Thus, while roughly half the communities in our sample were mapped or re-mapped during the first ten years of FEMA, some communities received their first FIRMs after 1989. The last big push to create and update flood maps relevant to our study period was 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.[3]

[2]While a few (less than 3 percent) FHBMs were created before 1978, our understanding is that these were only created for emergency or temporary enrollments into the NFIP following a major disaster. Similarly, some maps were created before 1973 (Morrissey, 2006).

[3]Some FIRMs were created before 1979, but these are also just hand-drafted emergency maps made after a major flood (Morrissey, 2006).

The flood risks depicted on maps are based on the outcome of flood hazard studies. These studies use geophysical and environmental data, land and aerial surveys, and interviews with the local population. Importantly, the flood maps we use in our paper are based 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 (Pralle, 2017; TMAC, 2015). In fact, as late as 2015, a federal advisory committee recommended that "FEMA should use population growth as an indicator of areas with increased potential flood risk."[4] Because we use data prior to 2011, communities that have higher expected future flood risk, whether due to anticipated construction or population growth, should not be more likely to receive flood maps.

Several flood disaster-related policies make flood map publication a strong driver of future NFIP enrollment. The Flood Disaster Protection Act of 1973 requires that communities join the NFIP within one year of being identified by FEMA as flood-prone, or be sanctioned. These sanctions make the affected communities ineligible for most forms of disaster assistance, and prevent property owners in those communities from acquiring flood insurance. Communities that do not have flood hazard areas identified by FEMA can still voluntarily join the NFIP, but staying out of the program does not open them to the same sanctions (Michel-Kerjan, 2010). Property owners in special flood hazard areas (or 100-year floodplains) who have federally-backed mortgages are mandated to obtain flood insurance, conditional on their communities joining the NFIP. However, because this policy was not strictly enforced through the 1980s, take-up of flood insurance within NFIP communities was low.

In the 1990s, two policy changes occurred that made map publication and updates a stronger predictor of future NFIP enrollment. In 1994, the Riegel Community Development Regulatory Improvement Act was passed. This new law penalizes mortgage lenders that do not verify whether borrowers who are required to carry flood insurance actually have one. In addition, FEMA conducted Cover America, which was an extensive information campaign

[4]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. Our analysis uses only data up to 2011, before FEMA would have incorporated factors that affect future flooding, such as population growth and construction, in measuring flood risk.

from 1994 to 2000. This resulted in greater awareness about the flood risks and sanctions from not joining the NFIP (Chivers and Flores, 2002). Figure 1 illustrates how take-up increased from the 1990s onward. Today, the NFIP covers about \$1.2 trillion worth of property (Michel-Kerjan and Kunreuther, 2011). The coverage limits are \$250,000 for residential buildings and \$500,000 for non-residential buildings (Burby, 2001).

3 Theoretical Framework

We begin with a stylized model of residency choice to illustrate the manner in which moral hazard arises in the context of nationally subsidized flood insurance. The general framework is similar to those discussed in prior work on decisions involving 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 incurs the full cost of flood risk.

As this is a household-specific decision, we omit individual-level subscripts for convenience. We follow similar notation as Einav, Finkelstein, Ryan, Schrimpf, and Cullen (2013), though our model differs significantly. Consider a possible choice of county, j, from a set of all possible counties $0, 1, ...J$, where $j = 0$ indicates an 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 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 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. 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.[5]

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

Sorting decision. In the first period, the household optimally chooses a county of residence, while taking information of insurance availability, η, into account. This is an optimal sorting problem. For a given vector of flood risks ($r_j \in \mathbf{r}$, $\forall j \in \{0, 1, ...J\}$), the household

[5] Note that, 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.

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.

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

Moral hazard. In this context, moral hazard comes from the household's increased demand for high-risk areas when 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.

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

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—now treating this as a national-level treatment. Moral hazard comes from the household's willingness to take on more risk when insurance is available than they would 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 moral hazard occurring when $r_1^* > r_0^*$. Therefore, under this condition, the household is willing to take on more risk when insurance is available than they would otherwise. 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 under moral hazard.

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

To illustrate why this inequality might hold, consider these two cases—with insurance availability ($\eta = 1$) 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 moral hazard 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 moral hazard. 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. This demonstrates a basis for which moral hazard can be governed, and thus, existence of moral hazard (i.e., Equation 7, or simply $r_1^* > r_0^*$) is indicative of premiums (or marginal co-payments) being too low. The existence of moral hazard—and the efficient pricing of premiums—is now an empirical question. In Section 4, we lay out our framework for empirically testing for the presence of moral hazard from the NFIP.

4 Empirical Strategy

This paper examines the moral hazard created by the NFIP. We take a *revealed* preference approach to testing Equation 7, and describe moral hazard as occuring when households participate in more risky behavior—for example, moving to and staying in risky areas—when insured against the potential costs of this behavior. As FEMA primarily targets high-risk areas to join the NFIP, finding evidence of moral hazard hinges on correctly identifying the treatment effect of NFIP (η in Section 3) on the treated areas. However, an approach that directly uses NFIP adoption to construct the treatment variable may produce biased results due to potential reverse causality. That is, as we are primarily interested in the causal effect of NFIP enrollment on population growth, any underlying causal effects of population growth on NFIP enrollment would bias our estimates.

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 4, this group of targeted communities strongly correlates with empirical flood risk. 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 program. In our sample, 99% of counties with at least one FHBM ultimately received a FIRM, providing evidence that this targeted group remained consistent.

Communities were given one year to join the NFIP after receiving a FIRM, before being sanctioned; thus, we exploit this intervention as a plausibly exogenous incentive that induced many of the communities to enroll into the NFIP. This strategy should be robust to anticipated changes in population, as the timing of initial FIRM assignment was independent of these expectations (as described in Section 2). We demonstrate that our proposed instrument strongly predicts NFIP adoption by estimating the following first stage equation:

$$postNFIP_{cst} = \alpha \cdot \textit{postFIRM-FHBM}_{cst} + x_{cst}\tilde{\beta} + \tilde{\lambda}_{st} + \tilde{\gamma}_{cs} + \tilde{\varepsilon}_{cst} \tag{9}$$

where $postNFIP_{cst}$ indicates actual enrollment into NFIP for county, c, in state, s, at year, t. This variable represents the proportion of a county which has been enrolled, and is thus between zero and one. *postFIRM-FHBM* describes the proportion of a county assigned an FHBM (before the start of our data) that has also been assigned a FIRM at some time t or earlier. 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 employment, income, and natural disasters. We include a set of state-by-year and county fixed effects, λ_{st} and γ_{cs}, respectively.

In the reduced form, we implement a difference-in-differences approach and compare high-risk areas—proxied by FHBM counties—to low-risk areas, before and after initial FIRM assignment. As FHBM communities do not encompass the entire population of NFIP enrollees, our approach is an instrumental variable design. Our identification requires that changes in population over time in the FHBM counties would track closely with non-FHBM counties, absent FIRM assignment, *and* that FEMA interventions affect our primary outcomes only through NFIP enrollment. Our primary estimating equation for the intent-to-treat (ITT) of NFIP on population flows is the following:[6]

$$migration_{cst} = \delta \cdot postFIRM\text{-}FHBM_{cst} + x_{cst}\beta + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \tag{10}$$

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 NFIP on the population outcomes. Causal interpretations for NFIP enrollment should be made on the scaled coefficient, δ/α.

Because we use FHBM assignment to proxy for risky counties, we interpret a positive effect on population ($\delta > 0$) from areas being pressured to join the NFIP as evidence of moral hazard. That is, if we observe that population in risky counties increases after the publication of their communities' flood maps, our interpretation is that impending flood insurance availability caused individuals to believe that they could be compensated for future potential flood losses by the government, and therefore became more likely to reside in flood-prone areas.

In Figure 4, we document a relationship between treatment assignment and empirical risk. Because of this, our primary strategy should, in itself, provide sufficient evidence of moral hazard. However, we present further evidence of moral hazard by exploiting additional variation in the treatment group's empirical risk level. In Section 7, we allow for heterogeneous effects within treatment by implementing a triple-differences (DDD) estimator. To do this, we extend Equation 10 by allowing the treatment effect of NFIP to vary based on a measure of actual risk: county-level historical flood propensity. Formally, we estimate the following equation:

[6] Our specification defines our "treatment" group based on FHBM assignments in the 1970s. These group assignments map closely to FIRM assignments made decades later. Specifically, 99% of counties with at least one FHBM have at least one FIRM ultimately assigned to them.

$$
\begin{aligned}
migration_{cst} = & \delta_0 \cdot postFIRM\text{-}FHBM_{cst} \\
& + \delta_1 \cdot postFIRM\text{-}FHBM_{cst} \times flood\ risk_{cs} \\
& + x_{cst}\beta + \mu_t \cdot flood\ risk_{cs} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst}
\end{aligned} \tag{11}
$$

where *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, $\mu_t \cdot$ *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 ITT effect of NFIP when *flood risk* is equal to zero.

In this specification, δ_1 is our coefficient of interest. It measures the additional ITT effect of NFIP coming from one additional flood episode per year. In this sense, δ_1 estimates the extent to which NFIP produces moral hazard, and is analogous to the cross-derivative in Equation 11, but in a *revealed* preference context. An estimate of $\delta_1 > 0$ implies the NFIP produces a larger increase in population in historically risky areas, suggesting that households internalize this reduction in risk through coverage of potential losses. We define a county's flood risk according to the average annual flood episodes experienced in that area over the time-span of our data, as reported by NOAA.[7]

5 Data

5.1 Population

To implement our empirical strategy, we require data on population and migration over a long time period. We obtain this from county-to-county migration files published by the Internal Revenue Service for years 1990 through 2011, which they construct from individual tax returns received each year.[8] 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

[7]Due to data constraints, we use in-sample floods instead of pre-NFIP (or pre-flood map) floods. We discuss and test for potential endogeneity in Online Appendix A.2.

[8]Data are available beyond 2011, but methodological changes make it inappropriate to link data post-2011.

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.[9] From these files, we construct county-level population, which is the sum of non-migrants (residents that did not change counties) and inflow migrants (new residents who moved from another county). Importantly, although our data do not cover the first two decades of the NFIP, they do cover the entire period when flood map publication became a stronger predictor of NFIP adoption (see Figure 1).

These 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 does not affect our main results, it prevents us from conducting additional analyses, such as examining where in-migrants are coming from, or where out-migrants are moving to. Second, there were methodological changes in 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.

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 pressured by FEMA to join. We obtain this information from the Community Status Book published by FEMA.[10] 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

[9] 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.

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

next pressured 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.[11] An issue which sometimes arises when aggregating up to county is that communities and counties 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 may attenuate our point estimates for effects at the county-level. Aggregating from communities up to counties also implies that we will have fractional treatments at the county-level. This means that our treatment indicator, *FHBM*, 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 2 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 of entirely FHBM communities. However, this does not imply that most of the US is risky, since typically only a small fraction of land area covered by a FHBM is identified to be a Special Flood Hazard Area. According to the Federal Emergency Management Agency (1983), only 4% of the total U.S. land area is within the 100-year floodplain (Maantay and Maroko, 2009; Robinson, 2004). Nevertheless, a county's position in the distribution is indicative of its assessed flood risk relative to other counties.

Because our data are aggregated to the county level, we use the fraction of communities with an FHBM as a measure of flood risk. That is to say, the larger the fraction of a county with an FHBM, the higher the risk of flood. For example, 65% of communities in Salt Lake City county have a FHBM. This places the county in the bottom quartile of counties by fraction with FHBM, which corresponds with its low empirical flood risk. By comparison,

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

all communities in Orleans Parish (New Orleans), a very flood-prone county, have a FHBM. The relationship between FHBM and empirical flood risk can be seen in Figure 4. This plots our county FHBM measure against a cross-section of mean-annual floods, as reported by the National Oceanic and Atmospheric Administration (NOAA). We show that there is a positive and statistically significant relationship between these two measures of flood risk.

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. The data contain a list of counties for which the state governor requested and was granted a federal disaster declaration. Natural disasters include fire- and water-related events such as hurricanes. A federal disaster declaration allows counties to receive disaster assistance. Over 75% of flood-related requests are approved, and the request for a federal disaster declaration requires documentation of damage assessments. The lack of information on declined requests for disaster declarations is a potential limitation of using these data. However, these data are still considered the most comprehensive source of data on large flooding events.[12] We also use annual flood episodes reported by National Oceanic and Atomospheric Administration (NOAA) for our analysis in Section 7. Finally, we use county-level income and employment data from the Bureau of Economic Analysis.[13]

6 Results

6.1 First Stage Estimates

We begin our analysis by testing the relevance of our instrument for NFIP enrollment. As FHBM communities were strongly incentivized to join the NFIP following FEMA's FIRM assignments, we anticipate a near one-to-one relationship. It is important to note however, that communities not directly targeted can enroll into the program as well. Due to the

[12] See Gallagher, 2014 for a discussion of other data on flooding events.

[13] The BEA also publishes intercensal estimates of county-level population. However, the nature of the data does not allow us to account for dynamic effects of the NFIP in the way that the annual IRS data does.

potential for reverse causality, we are not identifying off of this particular source of variation.

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 the inclusion of additional controls, where estimates do not change at the thousandths decimal place after controlling for demographic variables (Column 2), or declared disaster controls (Column 3). 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 NFIP will be slightly larger.

The dynamics of the first stage can be seen in Figure 5, where we regress NFIP on a series of lagged and leading terms of our instrument. The final lagged term represents the effect through the rest of the data, so that the estimates are relative to the pre-intervention periods. These estimates illustrate the strong incentives that FEMA imposed on affected communities, inducing the majority of take-up in the first year following the intervention.

6.2 Graphical Evidence

Before presenting our main results, we present evidence to support the parallel trajectories assumption of our difference-in-differences approach. 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 equation:

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

where $\bar{L}$ is the number of lags and $\underline{L}$ is the number of leads. This equation estimates the dynamic effects at each point of take-up for FHBM counties, *newFIRM-FHBM*, which indicates the fraction of communities in a county enrolling at a given point in time. These coefficients are estimated relative to the pre- period, as the final lag, *post-FIRM*, estimates the average effect through the end of the data. The leading terms serve as placebos, as we should not expect to see responses to future period treatments, and thus formally test our parallel trends assumption.

To illustrate the endogeneity problem in using actual NFIP take-up, we implement a difference-in-differences strategy using a naïve version of Equation 12.[14] Using actual NFIP take-up allows us to directly test for reverse causality in a *Granger*-causality sense.[15] The naïve estimates are presented in Figure 6. For all outcomes, there is an obvious divergence in treatment prior to NFIP enrollment. For effects on population (Panel a) and non-migration (Panel b), these estimates suggest migration changes up to three years prior to enrollment may have contributed to future NFIP take-up.

With sufficient evidence that invalidates the naïve approach, we directly test for reverse *Granger*-causality in our primary design, instrumenting for NFIP with FEMA interventions. Estimates from Equation 12 are presented in Figure 7. 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 biasing factors that partially determine NFIP adoption. 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, static estimates of the effect of NFIP on *in-migration* are

[14]In the naïve approach, we use actual NFIP take-up rather than map publication to determine treatment status and timing:

$$migration_{cst} = \sum_{l=-\underline{L}}^{\bar{L}-1} \tilde{\delta}_l \cdot newNFIP_{cst-l} + \tilde{\delta}_{\bar{L}} \cdot postNFIP_{cst-\bar{L}} + x_{cst}\tilde{\beta} + \tilde{\lambda}_{st} + \tilde{\gamma}_{cs} + \tilde{\varepsilon}_{cst}$$

[15]This approach only allows us to test for *Granger* reverse causality, but does not allow us to test for reverse causality within the same period. For example, from Figure 6, at the time of enrollment, we cannot disentangle the causal effect of NFIP enrollment on migration from the effect of migration on NFIP enrollment.

not statistically significant.

6.3 The Causal Effect of NFIP on Population Flows

Table 3 presents the reduced form estimates from Equation 10, our primary estimating equation. 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 add county, time-varying controls, including per-capita income and unemployment rates. Including these controls does not significantly change our estimate. In Column 3, 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 NFIP. Consistent with the evidence we showed in Figure 7, we see no effect prior to treatment for all three outcomes.

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

Finally, in Column 5 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. Although using this specification causes our point estimates to drop for all outcomes, the main estimates are still statistically significant. They are also not statistically different from the estimates we obtain using our primary 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 NFIP—would have been to move out. After scaling, this amounts to a 5.6% effect of NFIP on non-migrants. Although we estimate a 3% effect of NFIP on in-migration, these coefficients are estimated with less precision than our results for population and non-migrants.

To further verify the robustness of our main results, we perform a series of placebo treatments. For each permutation, we randomly *re-assign* treatment groups (*FHBM*) to each county.[16] Next, we randomly assign each county a time path for *FIRM* assignments.[17] In each permutation, we construct our placebo "post-treat" variable and estimate Equation 10 on each outcome. This procedure provides insight as to whether our estimates are significant just by chance. 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 8. 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. Panels A and B illustrate that our point estimates are in the far right tails of the distributions. In fact, from 1,000 permutations, no placebo estimate is greater than our true point estimates. The results for our in-migration outcome produce a one-sided p-value of 0.015. 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.

7 Effects of NFIP by Risk Severity

In this section, we exploit further variation in historical flood risk within treated groups in order to *directly* measure the heterogeneous effects of NFIP on high-risk areas. That is, although FEMA assignment of FHBM is largely driven by the level of flood risk a given community has, there is still significant variation within this level of risk which is not fully accounted for in our main approach. Therefore, we implement a DDD approach in order to

[16] To avoid restrictions on the distribution of FHBM between zero and one, we simply re-assign values for each county directly from the sample of counties. This is similar to bootstrap methods.

[17] That is, 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.

estimate the additional impact of NFIP on areas which experienced *historically* more floods than other areas that are also treated.

In Figure 9, we split our data into below- and above-median[18] annual flood episodes and present estimates for Equation 12 for each subsample. It is clear that the majority of the effect of NFIP comes from the high-risk treated counties in our sample. Similar to our difference-in-differences estimates, we see very little to no divergence between groups for all outcomes. In addition, the divergence in the post-period between high- and low-flood risk areas does not seem to be immediate, in contrast to the average treatment effects in Figure 7.

The DDD estimates from Equation 11 are presented in Table 4. Column 1 presents our base specification, fully controlling for all pair-wise interactions. 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% on population. With an annual flood risk standard deviation of 1.45, these estimates suggest that NFIP has an additional impact of about 4.4 percent for a one standard deviation increase in flood risk. This result fully characterizes the types of incentives which NFIP produces, illustrating that 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 3-3.7% on the number of non-migrants. Though we lack statistical power, our estimates still imply a meaningful 1.2-1.6% effect on the number of in-migrants per additional flood.

Assuming flood damages are proportional to population size, our DDD estimates suggest that the moral hazard produced by the NFIP 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

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

risk (see Figure 3) in our sample, our estimates suggest that the NFIP contributed to a 6.6% increase in costs attributed to Hurricane Katrina. As for Harris County (Houston), Texas, which ranks outside the 90th percentile in historical flood risk, we estimate that the NFIP was responsible for a 15% increase in damages from Hurricane Harvey.

Furthermore, these estimates imply that the true level of risk is not being efficiently incorporated into insurance rates. As these estimates do not directly disentangle the utility from reduced risk from the disutility from insurance costs, our results imply that premiums must be too low in these areas. Therefore, if the intention is to provide the right incentives to adapt to the future risks of climate change, policymakers must account for the moral hazard produced by the NFIP.

8 Discussion and Conclusion

This paper presents evidence of costly unintended consequences produced by the U.S. National Flood Insurance Program (NFIP). Our findings show that population increases in flood-prone areas as a direct response to community enrollment into the NFIP. This program provides highly subsidized flood insurance, securing households against expensive damages from future floods. Thus, our findings suggest that the benefits households receive in the form of a reduction in potential risks far exceed insurance rates, thereby altering location incentives.

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 midst 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. In the case of national flood insurance, our results indicate that the true level of risk is not being properly incorporated into efficient insurance rates. These inefficient rates produce costly externalities, for example, in the form of increased damages from natural disasters. Our estimates of the moral hazard 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 15 percent increase in damages from Hurricane Harvey.

Adaptation may be a necessary component of 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 subsidized flood insurance. If policy is intent on providing the right incentives to encourage adaptation to future risks of climate change, it must consider the moral hazard produced by the NFIP. With growing concerns of the financial sustainability of the National Flood Insurance Program, this may mean restructuring the program sooner rather than later.

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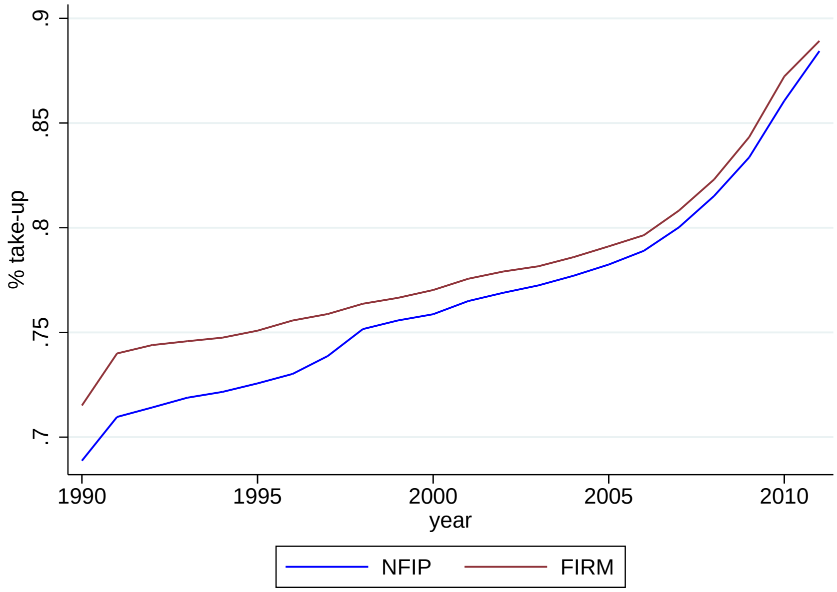
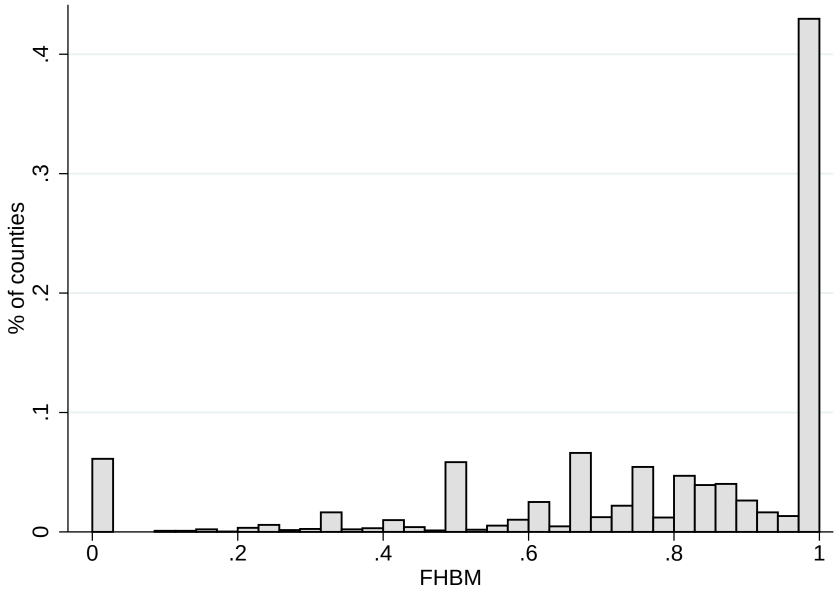
Figures

Figure 1: NFIP Enrollment and FIRM Assignment



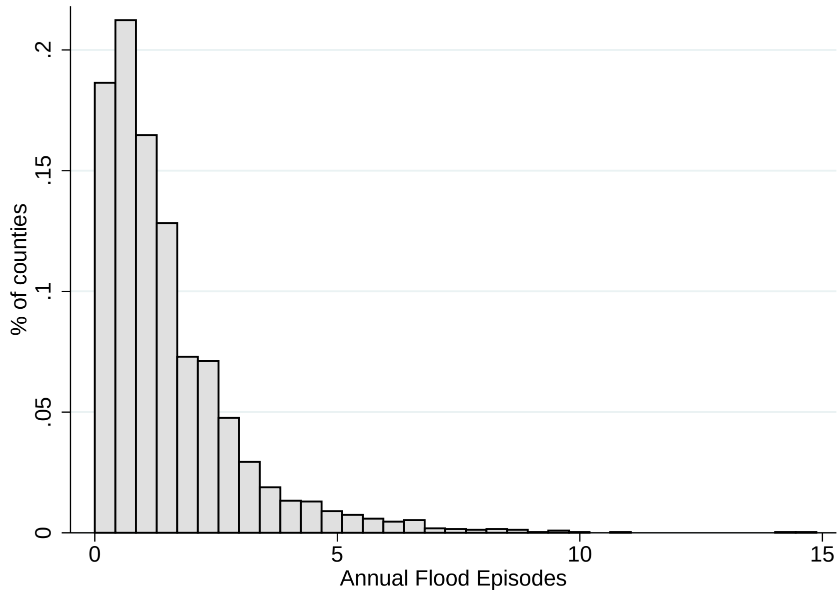
Note: This figure plots the fraction of counties enrolled in the National Flood Insurance Program (NFIP) and assigned a flood insurance rate map (FIRM) over time.

Figure 2: Distribution of FHBM Counties



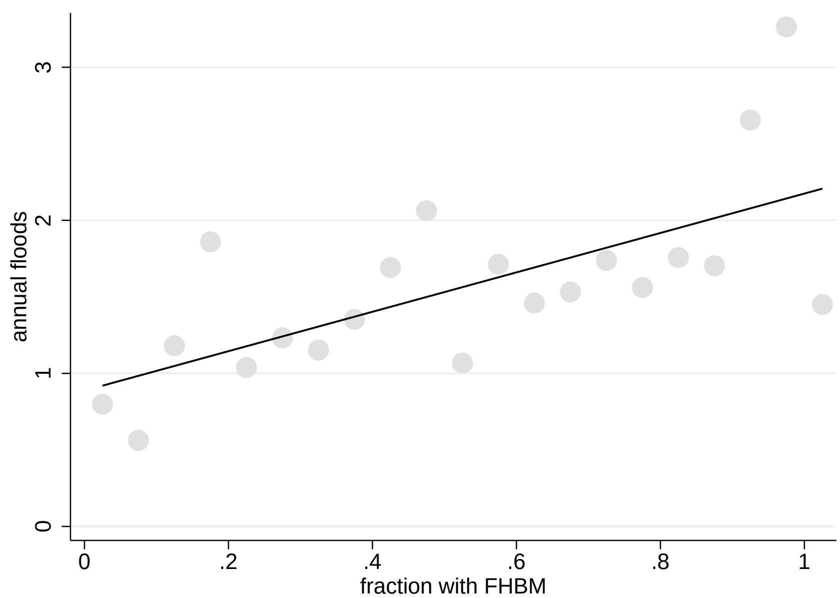
Note: This figure plots the distribution of fractions of a county (communities within a county) which FEMA has identified as flood-prone by publishing a Flood Hazard Boundary MAP (FHBM) for them. The vast majority of these assignments occurred in the 1970s.

Figure 3: Distribution of Flood Risk Across Counties



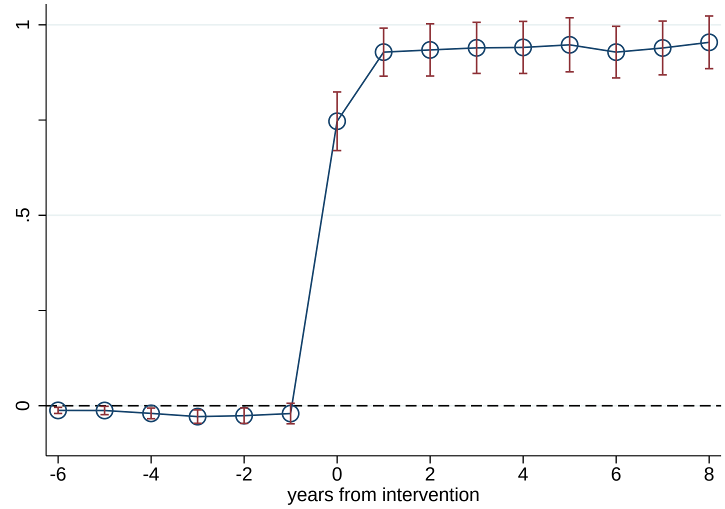
Note: This figure plots the distribution of county flood risk, defined as historical annual flood episodes (reported by the National Oceanic and Atmospheric Administration).

Figure 4: 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.

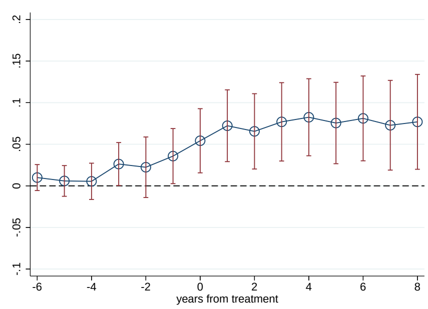
Figure 5: First Stage: The Effect of FEMA Intervention on NFIP Enrollment



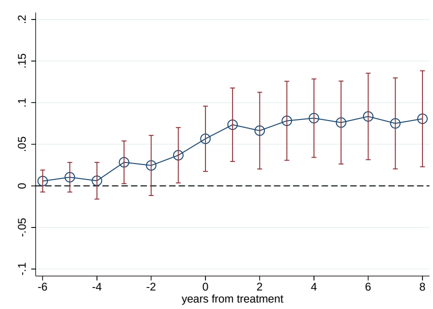
Note: This figure presents they dynamic coefficients from our first stage regression of the effect of post-FIRM FHBM intervention by FEMA—which granted affected areas a 1 year grace period before sanctions were imposed—on actual NFIP enrollment. 95 percent confidence interval bars are presented. Standard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

Figure 6: Event Study Specification of Impact of (Endogenous) NFIP: Naïve Specification

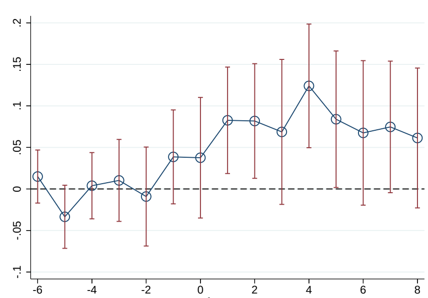
(a) Effect of NFIP on (log) Population



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



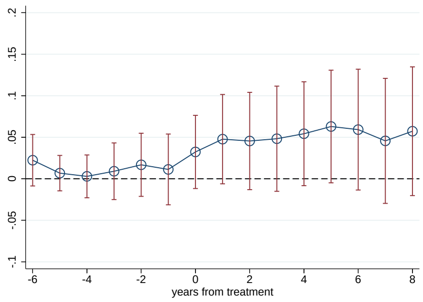
(c) Effect of NFIP on (log) Migrants



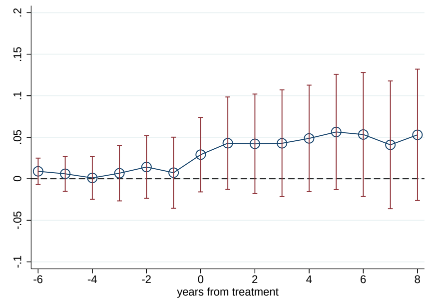
Note: This figure plots the coefficients from a naïve version of Equation 12, using lagged and leading National Flood Insurance Program (NFIP) enrollment indicators directly as the explanatory variables of interest. The final lagged term includes the entire post period, thus, all coefficients are defined relative to the pre-NFIP period. Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented. These results simply serve the purpose of illustrating the pre-divergence of treatment when directly using endogenous NFIP enrollment.

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

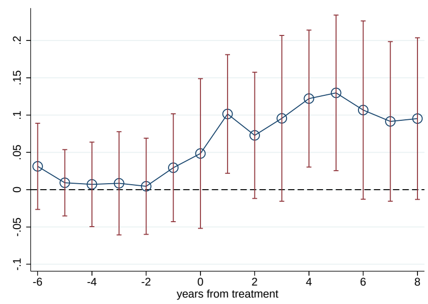
(a) Effect of NFIP on (log) Population



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



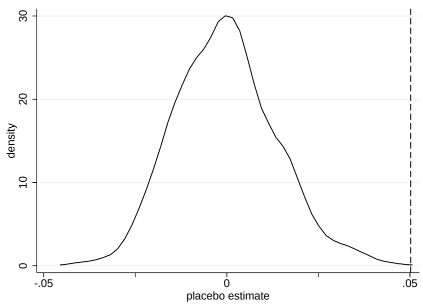
(c) Effect of NFIP on (log) Migrants



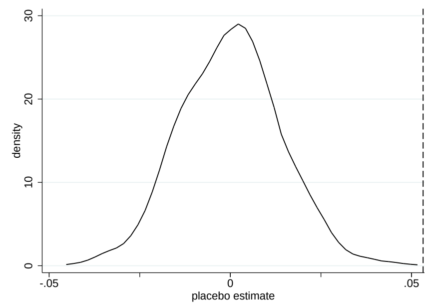
Note: This figure plots the reduced-form coefficients from Equation 12, using lagged and leading Flood Insurance Rate Map terms in our instrument. The final lagged term includes the entire post period, thus, all coefficients are defined relative to the pre-FIRM period. Estimates are plotted for log-population (Panel a), log-non-migrants (Panel b), and log-migrants (Panel c) outcomes. 95% confidence intervals are presented.

Figure 8: Placebo Estimates

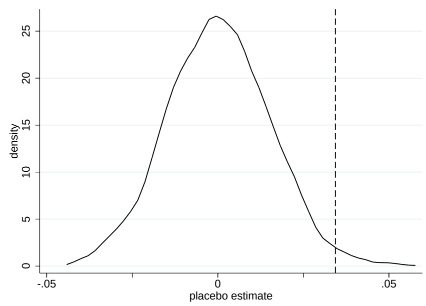
(a) Effect of NFIP on (log) Population



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



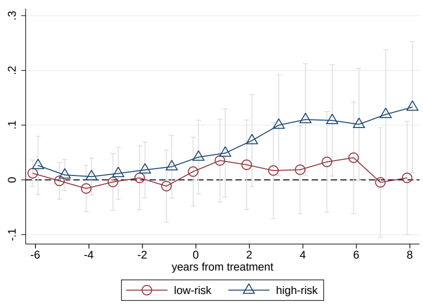
(c) Effect of NFIP on (log) Migrants



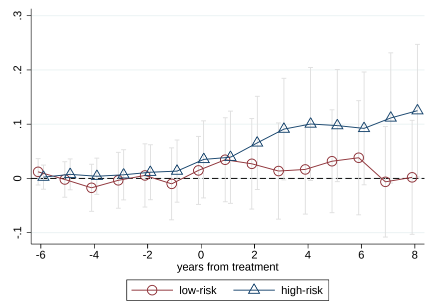
Note: Above are the resulting distributions of 1,000 placebo estimates of Equation 10 for each migration outcome. The dashed lines mark the corresponding point estimates from Table 3. The implied p-values are 0.000, 0.000, and 0.015 for Panels a, b, and c, respectively.

Figure 9: Heterogeneous Effects of NFIP, by Flood Risk

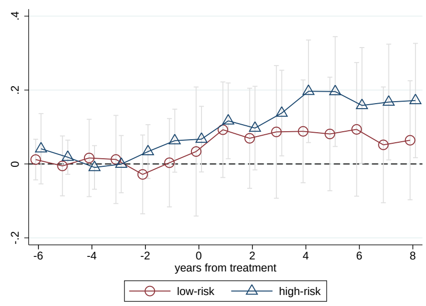
(a) Effect of NFIP on (log) Population



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



(c) Effect of NFIP on (log) Migrants



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. The final lagged term includes the entire post periods, thus, all coefficients are defined relative to the pre-FIRM period. 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	0.926
	(0.224)
FHBM	0.780
	(0.280)
Total Exemptions	71194.5
	(221581.6)
Non-Migrant Tax Exemptions	67151.2
	(212434.4)
Migrant Tax Exemptions	4042.0
	(10438.7)
Annual Flood Episodes	1.50
	(1.45)
Water-Related Decleared Disasters	0.143
	(0.450)
Per-Capita Income	26932.0
	(10731.3)
Unemployment Rate	6.484
	(3.290)

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: NFIP enrollment on FIRM and FHBM assignment

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

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

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. In Column 2, we control for county-level demographic variables, including per-capita income, unemployment rate, and job counts. Column 3 includes county-level water-related declared natural disasters. The estimates are not sensitive to the inclusion of these controls. Standard errors in parentheses are clustered on county. Note that 99% of counties with an FHBM ultimately receive a FIRM.

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

Migration Outcome	(1)	(2)	(3)	(4)	(5)
Panel A: Log- Population					
postFIRM-FHBM	0.0502***	0.0480***	0.0496***	0.0497***	0.0267*
	(0.0173)	(0.0163)	(0.0183)	(0.0183)	(0.0140)
Leading Treatment			0.00747	0.00744	0.00302
			(0.0131)	(0.0131)	(0.00998)
Panel B: Log- Non-Migrants					
postFIRM-FHBM	0.0532***	0.0507***	0.0530***	0.0530***	0.0303**
	(0.0177)	(0.0166)	(0.0188)	(0.0188)	(0.0141)
Leading Treatment			0.0105	0.0105	0.00644
			(0.0134)	(0.0134)	(0.0101)
Panel C: Log- Migrants					
postFIRM-FHBM	0.0344*	0.0343*	0.0313	0.0315	0.0107
	(0.0188)	(0.0182)	(0.0204)	(0.0204)	(0.0226)
Leading Treatment			-0.0138	-0.0138	-0.0183
			(0.0177)	(0.0177)	(0.0178)
N	64472	64472	64472	64472	64472
County FE	Yes	Yes	Yes	Yes	Yes
State X Year FE	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
Declared Disaster Controls				Yes	Yes
County Time Trend					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). County-level demographic controls in Columns 2-5 include 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 3-5 as a falsification, and county-specific time trends are included in Column 5. Standard errors in parentheses are clustered on county. Note that 99% of counties with an FHBM ultimately receive a FIRM.

Table 4: Heterogeneous Effects of NFIP on Migration, by Flood Risk

	Migration Outcome		
	(1)	(2)	(3)
Panel A: Log- Population			
postFIRM-FHBM	0.00834	0.00158	0.00161
	(0.0256)	(0.0248)	(0.0248)
Annual Floods × postFIRM-FHBM	0.0272^{*}	0.0331^{**}	0.0331^{**}
	(0.0151)	(0.0141)	(0.0141)
Panel B: Log- Non-Migrants			
postFIRM-FHBM	0.00639	-0.00111	-0.00108
	(0.0269)	(0.0261)	(0.0261)
Annual Floods × postFIRM-FHBM	0.0307^{*}	0.0369^{**}	0.0369^{**}
	(0.0163)	(0.0154)	(0.0154)
Panel C: Log- Migrants			
postFIRM-FHBM	0.0103	0.00963	0.00971
	(0.0271)	(0.0264)	(0.0264)
Annual Floods × postFIRM-FHBM	0.0123	0.0158	0.0159
	(0.0156)	(0.0149)	(0.0149)
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

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

Note: Above are OLS estimates of our triple-differences specification in 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 NFIP (in reduced-form) from one additional flood per year. Standard errors in parentheses are clustered on county.

A Online Appendices

A.1 Alternative Instrumental Variable

FEMA's assignment of FHBMs in the 1970s map closely into the communities that ultimately receive a FIRM. Our data aggregate community up to the county-level, and our sample suggests that nearly 99 percent of counties with an FHBM ultimately receive a FIRM; though there is still some variation between FHBM and FIRM proportions within county. In this paper, we proxy treatment with FHBM assignment, as these counties were identified as risky by FEMA, and thus were more likely to ultimately enroll into the NFIP. We exploit the timing of FIRM assignment and use the interaction of *FHBM* and *post-FIRM* as our instrument for NFIP enrollment. Not separately controlling for *post-FIRM* produces estimates as deviations from baseline, rather than as deviations from post-FIRM assignment. As our *postFIRM-FHBM* instruments for endogenous NFIP enrollment ($treat \times post$), we argue that this deviation from baseline fixed effects is the variation we want to isolate.[19]

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 $postFIRM \times FHBM$). Note that this will change not only our reduced form equation, but the first stage as well. Therefore, of primary interest is the scaled two-stage least squares estimates. Our estimates of this alternative first stage are presented in Table 5.

Table 5: 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

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

[19]Note that *postFIRM-FHBM* and *postFIRM* are extremely collinear, and split our estimates when including both in our primary estimation.

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 NFIP enrollment much more closely than FIRM alone—an estimate of 0.76. 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 6.

Table 6: 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 nearly identical to our primary estimates, and the same holds true for our other migration outcomes. Therefore, exploiting *FHBM* adds little to our overall analysis, but only produces a first stage closer to one.

A.2 Exogeneity of County Flood Risk

As our NOAA annual floods data begin in 1996, we are not able to use pre-NFIP flood risk for our analysis in Section 7. Using average flood risk over these periods with high enrollment into flood insurance may produce biased estimates in our DDD specification if NFIP systematically alters a county's flood risk. For example, if NFIP enrollment causes a community to invest in flood mitigation infrastructure, we would suspect our heterogeneous treatment effects of NFIP by flood risk to be downward biased. That is, our measure of risk would reveal the communities which invested the least in flood mitigation infrastructure, presumably contributing a lower amount to population.

In this section we provide evidence that our DDD estimates are consistent by examining the relationship between NFIP enrollment and flood occurrences. Note that we do not anticipate these estimates to be *causal*, as it is well-documented in the literature that major floods increase flood insurance take-up (Gallagher, 2014; Kousky and Shabman, 2014). In this scenario, our estimates of the effect of NFIP enrollment on flood risk will be downward biased (away from zero), as floods will *Granger* cause enrollment, followed by reductions in floods. In a difference-in-differences framework, this is represented by a positive effect in the pre-NFIP periods. Thus, rejecting a negative effect of NFIP on floods provides evidence that our estimates are consistent.

We estimate the following equation:

$$floods_{cst} = \delta \cdot \textit{post-NFIP}_{cst} + \lambda_{st} + \gamma_{cs} + \varepsilon_{cst} \tag{13}$$

where $floods_{cst}$ is the annual flood episodes, reported by NOAA, for a given county-state-year, cst. *post-NFIP*$_{cst}$ reports the fraction of a county enrolled into NFIP, and λ_{st} and γ_{cs} are state-by-year and county-state fixed effects, respectively. The estimate for Equation 13 is reported in Table 7. Standard errors are clustered by county.

Table 7: Effect of NFIP on County Floods

	floods
Post-NFIP	-0.150
	(0.0960)
County FE	Yes
State X Year FE	Yes
N	51712

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

These estimates suggest a reduction of 0.15 in future floods from the NFIP, however, this relationship is not statistically significant. Given that this estimate is plausibly biased away from zero, this provides strong evidence that using historical floods from this time period should not bias our estimates. To further test the consistency of our estimates in Section 7, we now difference out these estimated effects of NFIP on floods, and use residuals in our DDD estimates. These residuals are, by construction, orthogonal to NFIP treatment. In the same manner as our primary estimates, we average the residual floods by county and use this cross-sectional representation of flood risk to estimate the differential effects of NFIP across counties. We present the two stage least squares estimates (using average county flood residuals as our instrument for average floods) in Table 8. These estimates are marginally larger in magnitude, suggesting the correctly anticipated direction of bias, though not signficantly different from those in Table 4.

Table 8: Heterogeneous Effects of NFIP on Migration Using Residualized Floods (2SLS)

	Migration Outcome		
	(1)	(2)	(3)
Panel A: Log- Population			
postFIRM-FHBM	0.00707	0.000585	0.000620
	(0.0256)	(0.0247)	(0.0247)
Annual Floods × postFIRM-FHBM	0.0282*	0.0338**	0.0339**
	(0.0151)	(0.0141)	(0.0141)
Panel B: Log- Non-Migrants			
postFIRM-FHBM	0.00508	-0.00214	-0.00212
	(0.0269)	(0.0261)	(0.0261)
Annual Floods × postFIRM-FHBM	0.0317*	0.0377**	0.0377**
	(0.0164)	(0.0154)	(0.0154)
Panel C: Log- Migrants			
postFIRM-FHBM	0.00921	0.00886	0.00894
	(0.0271)	(0.0264)	(0.0264)
Annual Floods × postFIRM-FHBM	0.0131	0.0164	0.0165
	(0.0156)	(0.0149)	(0.0149)
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

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

These estimates are marginally larger in magnitude than those in Table 4, suggesting the correctly anticipated direction of bias. However, these differences are not statistically significant. Therefore, we are confident that the use of in-sample floods to evaluate a county's average risk level should not significantly bias our estimates.