Does Federal Assistance Crowd Out Private Demand for Insurance?

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Abstract

We undertake an empirical analysis of the effect of disaster aid on the demand for insurance using a unique panel dataset from Florida. We address endogeneity using instrumental variables that exploit political influence over aid amounts. In zip-codes that receive individual assistance grants, the average insurance coverage decreases by about \$17,000. When the average grant given increases by \$1,000, average insurance coverage declines by about \$6,400. This crowding out is on the intensive margin; we find no impact on take-up rates. We control for low interest government loans and find that they have no effect on insurance demand[1].

JEL Codes: D78, D81, G22, Q54

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1. Introduction

In 2011, the president of the United States issued 99 disaster declarations. This was a historical record, but in keeping with recent trends. Over the period 1950 to 2010, the average number of such declarations increased three-fold (US Government Accountability Office 2012). Federal aid is now routinely offered following a wide variety of disaster events, from floods, hurricanes, and earthquakes to terrorist attacks and, as observed recently, financial crises. This is true in the United States and in many other countries around the world.

Since early theoretical work on the Samaritan's dilemma by Buchanan (1975), economists have been interested in the potential for underinvestment in financial protection by economic agents in response to government assistance (e.g., Coate 1995; Kim and Schlesinger 2005). Several theoretical and empirical papers have explored this possibility in diverse contexts, such as long-term care insurance (Brown and Finkelstein 2008), intra-household transfers (e.g., Alger and Weibull 2010), savings and retirement (e.g., Homburg 2000; Lagerlöf 2004; Crossley and Jametti 2013), health insurance (e.g., Herring 2005), federal terrorism insurance (e.g., Brown et al. 2004), and foreign aid (e.g., Svensson 2000; Torsvik 2005).

In the context of financial protection against natural disasters, theoretical models predict, first, that if households or managers of a firm expect government relief following a disaster, they will invest less in reducing their risk ex ante. Second, post-disaster government relief may also a crowd out insurance purchases if individuals treat federal aid as a (partial) substitute for insurance and fail to insure or underinsure (Lewis and Nickerson 1989; Kaplow 1991; Kelly and Kleffner 2003).[2]

[2] Note that we are focused on financial assistance for damage property, not emergency relief in the immediate unfolding of an event for health and safety.

Surprisingly, though, tests of these theoretical predictions are mainly limited to laboratory experiments (e.g., Brunette et al. 2013) and surveys (Kunreuther et al. 1978; van Asseldonk, Meuwissen, and Huirne 2002; Botzen and van den Bergh 2012a; Petrolia, Landry, and Coble 2013; Raschky et al. 2013). Actual disaster aid often differs from the assumptions made in these papers. For instance, disaster aid does not occur with certainty (an assumption made in some, but not all papers), and the amount of aid, if any, is unclear ex ante (Botzen and van den Bergh 2012b). Disaster aid is also tied to many eligibility requirements, some related to insurance. Furthermore, whereas surveys provide important insight on how unobservable variables impact decision-making, individual responses to surveys do not always capture actual decision-making.

To test predictions regarding ex ante expectations of disaster aid, individual expectations must be observed on a large scale—something very difficult to do in practice. We can, however, observe how insurance purchases change after the receipt of aid. We thus provide an empirical test of whether US federal disaster aid to households crowds out purchase of disaster insurance in the state of Florida, using data on observed insurance purchasing behavior. Specifically, our research answers the following questions: Does the receipt of government disaster aid reduce demand for insurance? If so, how large is the reduction? The responses to these questions have important economic, social, and policy implications.

Natural disasters are one area where there is a standing federal policy regarding aid disbursements, yet a good understanding of the influence of post-disaster aid on households' financial protection decisions is lacking. We focus on flood events, as they are responsible in the United States for the greatest number of lives lost and the most damage of all natural disasters over the last century, and they account for nearly two-thirds of presidential disaster declarations (Perry 2000; Michel-Kerjan and Kunreuther 2011). As discussed in more detail below, these declarations can carry with them large amounts of off-budget spending—Congress appropriated around $50 billion after Hurricane Sandy, for example—but households are only receiving small amounts of this spending to repair or replace damaged property (e.g., Kousky and Shabman 2013). Individuals, of course, may have expectations that differ from the amounts actually allocated.

Related concepts using a variety of terms have been discussed in economics, so a brief comment on terminology is warranted. The term "crowding out" is often used to refer to situations in which government spending reduces private investment or spending (e.g., Brown and Finkelstein 2008). Similarly, in this article, we use the term "crowding out" to refer to a reduction in insurance purchases as a response to the receipt of federal disaster aid.[3] The term "charity hazard," which has also been used in the literature, refers more generally to the impact of charity donations (whether public or private) on the demand for disaster insurance (Browne and Hoyt 2000; Raschky and Weck-Hannemann 2007). We elect to use the term "crowding out" because we focus specifically on the extent to which households perceive federal aid as a substitute for insurance, such that post-disaster aid crowds out insurance purchases.

A couple of studies have examined the demand for flood insurance in the US but have not addressed the influence of disaster aid on demand (Kousky 2011; Landry and Jahan-Parvar 2011; Gallagher 2013). To our knowledge, only one empirical analysis has attempted to examine our question of how the receipt of disaster aid influences insurance purchases. Browne and Hoyt (2000) use data aggregated to a state level and estimate a fixed-effects (FE) model of the determinants of the demand for flood insurance. Surprisingly, they find a positive correlation

[3] As we explain in more detail below, residential flood insurance is available almost exclusively through the federally run National Flood Insurance Program (NFIP), which was established in 1968 as a result of a lack of availability of flood insurance in the private market; this federal program covers more than $1.2 trillion of assets today.

between flood insurance purchases and the amount of disaster aid received from the Federal Emergency Management Agency (FEMA). Though an important first empirical contribution, their analysis does not address problems of endogeneity, possible measurement error in their aid variable (their analysis included all types of aid for all disaster types), and they use a high level of aggregation.

We are able to overcome these limitations and examine the influence of disaster grants from the Individual Assistance (IA) program of FEMA provided directly to affected households for uninsured losses. We are also able to control for low-interest disaster loans from the US Small Business Administration (SBA). These two programs have long been the primary sources of direct federal aid for households that sustain damage from a disaster. We obtained individual-level data on IA payments and SBA loans (both specific to flood events), flood insurance purchases, and flood insurance claims for the state of Florida from 2000 to 2009. Florida is an ideal case for this analysis since it is the largest flood insurance market in the US, with more than 2 million policies as of December 2012, and because the state received federal disaster aid multiple times during our study period. Due to federal privacy restrictions, however, the smallest identifying geography we have for our data is the zip code. We combine our data with socioeconomic control variables from the US Census.

A potential challenge for our empirical analysis is a two-way causality problem between disaster aid and insurance: a higher penetration of flood insurance could reduce the amount of aid needed after an event.[4] In addition, there may be omitted variables, which are not captured in

[4] Indeed, RAND report prepared for the Federal Emergency Management Agency (FEMA) after Hurricane Katrina, examines a question that is the opposite of our own; that is, does insurance penetration influence amounts of aid (Dixon et al. 2006)? Their data is aggregated at the state level and they do not address endogeneity.

spatial and time fixed effects, and are likely to be correlated with both aid received and insurance purchases, such as expectations about aid, risk perceptions concerning flooding, or demographic variables for which we do not have data. A standard ordinary least squares approach will, therefore, be biased. Our preferred identification strategy is thus instrumental variables (IV). We exploit an exogenous source of variation in governmental relief that was initially highlighted by Garrett and Sobel (2003): provision of federal disaster aid is partially politically motivated. Their estimates reveal that more federal aid is spent in election years and in states that are considered more important for the outcome of the election ("swing states"). Building on this result, we use the following variables as instruments for FEMA grants: the timing of presidential and Senate elections, whether the county is a "swing" county, and the political majority of the county in the previous presidential election. The identifying assumption of our IV strategy is that the political importance of a county during election years has no effect on demand for flood insurance other than through governmental post-disaster aid payments received.

Our empirical findings reveal that FEMA IA grants have a statistically significant negative impact on average coverage per policy. If a community receives any IA grants, average insurance coverage per policy declines around $17,000. A $1,000 increase in the average IA grant decreases average insurance coverage by roughly $6,400 (for reference, the average coverage purchased on a policy in our data is $186,780). For zip codes in the top quartile of average IA grant amounts, coverage decreases by nearly $18,700. Interestingly, though, for those in the bottom quartile of average IA grant amounts, insurance coverage purchased actually *increases*, by more than $23,500. This heterogeneity may be related to how perceptions change after the receipt of aid: households receiving a large amount may believe that aid could be generous enough to substitute for insurance, but those receiving only a small amount of aid realize that insurance coverage is needed to be fully compensated for property damages after a disaster. We find that SBA disaster loans may have a very small positive effect on coverage levels and controlling for their magnitude does not materially alter our results.

We find that standard FE estimates would substantially underestimate the crowding out impacts, highlighting the need to instrument for FEMA grants. Our findings are robust to several robustness checks.

We also examine whether the number of consumers who purchase insurance changes after receipt of aid. We find that aid very slightly increases the number of residents who purchase flood insurance, but this is due to a government requirement that those who receive aid must purchase an insurance policy; when such policies are excluded, we find no impact on take-up rates. This requirement seems to be working as intended, preventing a drop in take-up rates after the receipt of disaster aid.

The remainder of the article proceeds as follows. Section II provides background on federal disaster aid and on the flood insurance market in the United States. Section III describes our data. Section IV outlines our empirical strategy. Section V discusses our findings. Section VI presents the results of several robustness checks and extensions. Section VII concludes.

2. Background on Federal Disaster Aid and Flood Insurance in the US

The current framework for federal disaster relief in the United States is provided by the Stafford Disaster Relief and Emergency Assistance Act, passed in 1988. When a disaster occurs, if managing and financing the disaster is judged to exceed the affected state's capacity, the governor may request a declaration from the president. The president may authorize public assistance—aid to local governments—and/or individual assistance to help households. The GAO (2012) has found that for declarations between 2004 and 2011, only 45% authorized individual assistance, but 94% authorized public assistance. Once authorized, the Federal Emergency Management Agency can activate these different aid programs, spending money from the Disaster Relief Fund. Congress appropriates funds into this account every year, but in exceptional disaster years, further appropriations from Congress are needed.

Over time, the number of US presidential disaster declarations has increased dramatically, from 191 declarations during the decade 1961–1970 to 597 for the decade 2001–2010. Evidence shows that many of the years with the highest number of declarations are presidential election years, and several studies confirm that aid is politically motivated to some extent (Garrett and Sobel 2003; Sylves and Búzás 2007). Among all disaster declarations, roughly two-thirds are related to flood events. Over the period January 1960 to December 2011, there were 1,955 disaster declarations, with 1,258 related to flooding.[5]

The majority of disaster relief in the United States is given as public assistance to state and local governments to pay for debris removal, emergency response, and restoration of damaged infrastructure and public buildings (Dixon et al. 2006). The focus of this paper is not on that public assistance, but rather, on grants the federal government gives to households that have experienced

[5] Data on disaster declarations is publically available on FEMA's website. Note, it is not just the number of declarations that has increased, but also the proportion of total economic losses covered by federal aid to individuals, businesses, and state and local governments. For instance, in the wake of Hurricane Diane and the associated flooding in 1955, federal relief spending covered only 6.2 percent of total damages (Moss 2010). In contrast, federal relief for the 2005 hurricane season and other disasters occurring through 2008 was roughly 70 percent of total estimated damages (Cummins, Suher, and Zanjani 2010). It was 75 percent after Hurricane Sandy in 2012—a record high. Most of these funds, however, do not go to individuals to compensate for property damage, but to local governments for reconstruction and mitigation.

a disaster. While a working paper finds evidence that nondisaster aid programs, such as food stamps and unemployment insurance, can be important safety nets after a disaster (Deryugina 2011), we restrict our analysis to direct grants that are given solely as a result of the disaster event.[6]

FEMA's IA program funds housing assistance and other needs assistance. The former covers temporary housing or home repairs, and the latter covers damage to personal property, medical or funeral expenses from a disaster, and other costs not related to housing. These are grants to individuals that do not need to be repaid and are not counted as income on tax returns, but they cannot exceed $31,900 (as of 2012; this amount is indexed to inflation). Of note, however, the average grant for the repair of a damaged home is around $4,000—significantly less than the cap (McCarthy 2010).

Receiving these grants, however, is tied to many eligibility requirements (see Kousky and Shabman [2012] for an overview). Aid is available to US citizens, noncitizen nationals, and qualified aliens. Housing assistance is available only for primary residences. Aid is supposed to be a last resort; thus a homeowner must first file an insurance claim (if she or he has insurance), and FEMA will not duplicate these benefits. Aid is intended only to make a home safe and

[6] Note that we do not examine the impact of being able to deduct disaster damage from one's taxes, and we do not examine the Federal Housing Administration's section 203(h) program, which insures mortgages made to disaster victims. The former, we believe, would have small incentive effects, and the latter is protection for lenders against the risk of default. In addition, after certain major disaster events, notably the attacks of September 11th and Hurricane Katrina, Congress has appropriated funds to stricken communities through the US Department of Housing and Urban Development's Community Development Block Grant (CDBG) Program; this, for instance, funded The Road Home Program in Louisiana. Over our time period, Florida did receive some limited CDBG funds in response to hurricane events, but much of the funds were used for rebuilding infrastructure and for assistance to local governments. We return to CDBG funding in the conclusion.

inhabitable, not to bring a home back to predisaster conditions. Certain individuals in high-risk areas are required to purchase a flood insurance policy to receive aid, a point we return to below.

As noted above, we also control for low-interest loans from the federally run SBA. Despite its name, the SBA offers loans not only to small businesses but also to homeowners. Loans are available for up to $200,000 for homeowners to repair or replace their primary residence and for up to $40,000 to repair or replace contents damaged or destroyed in the disaster. For the period we study here (2000-2009), the interest rate was not to exceed 4 percent for applicants unable to obtain credit elsewhere (as determined by the SBA). For those who could obtain credit elsewhere, the interest rate would not exceed 8 percent.[7] Loans are usually 30 years in length. SBA recipients are also required to purchase flood insurance.

Flood insurance for homeowners in the United States is generally not available from private sources but has been available through the National Flood Insurance Program (NFIP) since the program's creation in 1968. The NFIP is designed to be a partnership between the federal government and communities. Communities may voluntarily join the program by agreeing to adopt minimal floodplain management regulations; in exchange, their residents become eligible to purchase flood insurance policies through the NFIP. All communities in the state of Florida participate in the program, eliminating any sample selection issues for our analysis. The NFIP provides insurance up to a maximum limit for residential property damage, which is now set at $250,000 for building coverage and $100,000 for contents coverage. Over our study period, the menu of deductibles ranged from $500 to $5,000.

[7] For disasters after July 2010, these two ceilings were decreased to 2.5 percent and 5 percent, respectively, to reflect lower interest rates.

Premiums for the NFIP vary according to the flood zones defined by FEMA. Premiums within a zone are the same nationwide, but in high risk zones, they also vary by structural characteristics of the house, such as its elevation. By law, FEMA is allowed to raise rates only once a year; over the period we study, rate increases averaged across all zones could not exceed 10 percent. As the zones are national in scope, rates are not adjusted locally in response to extreme events and rates are not adjusted for homeowners based on their loss history (see Michel-Kerjan [2010] for a recent review of the program).

As of December 31, 2009, the end of our sample period, 5.63 million NFIP policies were in force nationwide, generating $3.22 billion in premiums (with an average annual premium per policy of $572 nationwide), and a total of $1.23 trillion of assets covered.[8] Florida, the focus of our analysis, is by far the largest flood insurance market in the United States. In 2009, the state had 2.14 million NFIP policies-in-force and $470 billion of assets covered (about 38 percent of the entire NFIP portfolio).[9]

3. Data

We compiled the data for this study from five different sources. We discuss each in turn. First, we obtained data from FEMA's IA program on grants awarded to Florida households between 2000 and 2009 for flood-related disasters. This data is for single-family residences (the focus of our analysis). The data includes the category of aid and the amount received. Addresses

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[8] As of June 2013, almost 5.54 million policies were in force representing $1.28 trillion in coverage.

[9] Texas, Louisiana, and California are the three states following in the ranking, representing, respectively, only 12 percent, 8 percent, and 5 percent of the NFIP, whether determined as the number of policies-in-force or as insured assets.

and other identifying information have been removed for privacy. The finest geographic identifier we have for each grant is the zip code. Over the period 2000–2009, slightly less than 15,300 IA grants were awarded in Florida for flood-related events. The mean IA grant was $3,667 and the median was $2,950. The smallest grant was $85 and the largest grant was $29,268.

The year-to-year variability in the IA grants provided to residents of Florida is high, as would be expected for low-probability events such as floods. The years 2004 and 2005 triggered a large number of IA grants (3,821 in 2004 and 4,143 in 2005). In 2004, four hurricanes (Charley, Frances, Ivan, and Jeanne) hit Florida and inflicted severe flood-related losses. In comparison, the years 2002, 2003, 2007, and 2009 were quieter. Within our sample, IA grants were paid in nine out of ten years, with an average of 1,517 grants per year. The three years with the highest total dollar amount of IA grants awarded in the state were 2004 ($18.2 million), 2005 ($16.5 million), and 2000 ($11.2 million).

Second, we obtained data from the US Small Business Administration on disaster loans related specifically to flood events. The data contains information on how much each borrower received. Again, addresses were removed, but we know the zip code of the recipients. Over our time period, nearly 43,600 SBA loans were given for flood-related events. The mean of all loans was $19,205 and the median was $10,000. SBA loans were granted in every year, with an average number of 3,927 per year. In 2005, the largest number of loans was granted, 24,172. The three years with the highest amount of SBA loans were 2005 ($415 million), 2006 ($229 million; probably many of these loans were delayed applications from the 2005 hurricane season), and 2004 ($38 million). The data reveals that both IA grants and SBA loans are widely used and that the number of SBA loans provided to those in need has been about twice that of IA grants. In dollar value, SBA loans are, on average, about five times larger than IA grants.

For our dependent variable (demand for flood insurance), we obtained data from the National Flood Insurance Program on both policies-in-force and claims for the entire state of Florida for 2001–2009 (we start our analysis on policies-in-forces in 2001 because we are examining lags of aid, which we have only for the year 2000 and later). We extract policies and claims for single-family residences for our analysis. Again, the smallest geographic identifier we have is the zip code, due to privacy concerns.

The NFIP policy data contains a variety of variables relating to the insurance contract, such as the coverage level, premium, and deductible. The claims dataset includes information on the claim, such as the date of the loss, the catastrophe with which it is associated, and the amount of the insurance payment. Over the period 2001–2009, the NFIP sold 10.17 million one-year, single-family residential policies in the state of Florida and collected $4.65 billion in premiums (an average of $457 per year per policy). Consistent with the IA and SBA data, the highest numbers of claims were paid in 2004 and 2005, two years in which Florida saw substantial storm surge damage from hurricanes. Each year resulted in over 16,000 individual claims; taken together, these two years triggered more than $1.3 billion in insured losses. Note, however, that many zip codes do not have any claims, even during the 2004 and 2005 years, illustrating that flooding, even when severe, can be quite local.

We construct two different dependent variables for our analysis (summary statistics on these are provided below). The first is the average coverage per policy. This variable is the ratio of total building and contents coverage limits for each policy in a zip code over the number of policies-in-force in that zip code. The second variable is the ratio of policies-in-force in each zip code over the total number of housing units. This is a measure of the take-up rate of NFIP insurance for single-family households in each zip code. While we would prefer to have the number of structures in the floodplain as the denominator, such information is not available.

Our independent variable for the cost of insurance is calculated as the sum of premiums in the zip code divided by the total coverage purchased, minus the deductibles for our sample of single-family houses.[10] We include this variable as a control; however, for two reasons we do not interpret the coefficients on this variable as, nor estimate, a price elasticity. First, we observe premiums only for policies that are actually bought and, as such, our premium variable does not truly capture the average price of insurance in the zip code. Second, the premium variable is highly correlated with risk. As stated earlier, the premium for NFIP policies is set nationally based on annual average loss calculations for each flood zone. The premium varies only by flood zone and by a limited set of characteristics of the house, such as height above base flood elevation, whether the house has a basement, and when the house was built. Given this, we cannot fully separate risk levels from premiums, and the impact of both is probably conflated in our price coefficients.

We complement our analysis with our fourth source of data, purchased from GeoLytics, which generates annual estimates at a range of geographic scales for many demographic and socioeconomic variables based on US Census data. Our empirical approach described below includes zip code and year FEs to control for both time-invariant influences at a zip code level, as well as zip code–invariant influences over time. Still, the number of housing units, the percentage of housing units that are owner occupied, and the median income in the zip code could change

[10] The cost variable used here is an average rate for $1 of coverage. NFIP premiums, however, are reduced when a higher deductible is chosen. The amount of that deduction is captured in the rate calculated here. In essence, NFIP premiums are the amount of coverage multiplied by the rate and then multiplied by a deductible factor.

over time, even if not substantially, and we include them as additional controls. Summary statistics are shown in Table 1, discussed below. Spatial variation in these variables is substantial.

Finally, data for our instruments is obtained from the Atlas of US presidential elections (http://uselectionatlas.org). Data on the outcomes of presidential and US Senate elections is collected at the level of the election district. Disaggregated data that assigns election data to zip codes is not available. To our knowledge, the Atlas of US presidential elections provides the most disaggregated form of voter registration and election data, and it is at a county level. We use this county information by assigning each zip code to its corresponding county.[11]

We use this dataset to construct our instrumental variables for IA. The instruments for IA in the zip code in the previous year (i.e., in *t-1*) are: (1) the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Presidential election and a dummy indicating that year *t-1* was a Presidential election year, (2) the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year *t-1* was a Senate election year, and (3) the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year. Our instruments, their rationale, and robustness checks are discussed further in the next section on methodology.

Table 1 presents summary statistics for the variables used in our empirical analysis across all years and zip codes. In cleaning the data, we had to drop several observations of IA grants and

[11] In cases where zip code boundaries and county boundaries overlapped, we assigned the zip code to the county in which the majority of the zip code's land area was located.

SBA loans where the zip code was entered incorrectly or was not found in our other datasets. We also dropped zip codes that were almost exclusively public or protected public lands (e.g., the Everglades).[12] Finally, we dropped 11 zip codes that had fewer than two housing units and 1 zip code in which the median income was identified as zero. Our analysis focuses on the remaining 8,425 zip codes.

As stated above, our two key dependent variables are the average amount of insurance purchased per policy in a given zip code and the take-up rate in that zip code. The former has a mean of $186,780, and the latter has a mean of 13.8 percent (although it ranges from 0 to 100 percent). As explanatory variables, we use both a binary measure of whether any IA was given to a zip code, as well as the *average* IA grant given in a zip code. We construct analogous controls for SBA loans. Across all zip codes (whether or not they received aid or a loan) and years, the mean average IA grant is $350, and the mean SBA loan is $4,833. As mentioned previously, for many zip code–years, no aid payment occurred at all and these zeros pull down the average across all zip codes. For those zip codes that had at least a positive IA or SBA payment, the average IA grant was $3,667; in zip codes that received SBA disaster loans, the average loan was $19,205.

4. Empirical Methodology

Our goal is to estimate a causal relationship between federal post-disaster aid and flood insurance coverage levels and take-up rates. We estimate a FE model first for comparison, but, as aid is likely endogenous, our preferred specification is an IV approach that exploits exogenous variation in political factors to instrument for FEMA IA grants. We thus first specify the following

[12] We also excluded some zip codes that were PO boxes only.

FE model using our balanced panel dataset with *i* indexing zip codes, *c* indexing counties in Florida, and *t* indexing years:

$$(1)\ FI_{ict} = \alpha_i + \lambda_t + \beta_1 IA_{ict-1} + \beta_2 C_{ict-1} + \beta_3 SBA_{ict-1} + \mathbf{X}_{ict}\boldsymbol{\beta_4} + u_{it}.$$

The dependent variable, *FI*, as noted above, is either the average coverage level per policy or the take-up rate of flood insurance. Zip code FEs, α_i, control for any time-invariant aspects of the zip code, such as its exposure to flood hazards. Year FEs, given by λ_t, control for shocks and changes that are common to all zip codes within a given year. *IA*, our variable for FEMA grants, is constructed in two different ways as mentioned above. In one specification, we define *IA* as a dummy variable that switches to one if zip code i received some IA in year *t* and zero otherwise. The resulting dummy variable is labeled *Positive* IA_{it}. Second, we define *IA* the average IA grant amount given per recipient in a particular zip code and year. Our SBA loan control variables are constructed analogously (that is, when we use a binary variable for IA, we also use one for SBA, for example). We also control for flood damage in the zip code using flood claims in the zip code in the previous year, noted C_{ict-1}, measured as the average flood insurance claim per policyholder. Claims should also control for the extent of damage, such that we capture responses to aid and not other impacts of the event. Our other controls, including the number of housing units, median income, and premium per policy, are given by the vector **X**. We cluster our standard errors by zip code.

As discussed, the FE model is likely biased due to reverse causality. Aid can influence insurance, but insurance penetration and coverage levels could also influence the amount of aid given. In addition, there could be correlated omitted variables, such as demographic variables (for example, highly educated households may be more likely to insure but also better able to navigate the bureaucratic red tape necessary to receive federal aid) or unobserved expectations about how much aid will be forthcoming after a disaster. Using the first lag of governmental relief only partially solves this problem as insurance purchases and underlying expectations might be correlated over time.

To address this issue, we estimate the impact of IA relief on insurance purchases using an IV model. Our identification strategy exploits an exogenous source of variation in governmental relief. A key characteristic of governmental relief in the US is that it depends on discretionary political decisions. Garrett and Sobel (2003) show that, compared to other years, federal disaster relief is, on average, $140 million higher in election years. They also show that states with greater importance for the election outcome have, on average, a higher rate of disaster declarations, everything else being equal. Hence, election years and variation in the political importance of an area can generate variation in federal aid payments. While Garrett and Sobel use states as their unit of analysis, presidents have discretion over which counties receive a declaration and it is not implausible that political influence plays some role at this scale, as well. We thus use the instruments discussed in the previous section, giving a first-stage regression in which *c* indexes the county and *t* indexes the year:

$$(2) \qquad IA_{ict} = \alpha_i + \lambda_t + \boldsymbol{Z}_{ct}\gamma \quad + C_{ict}\delta_1 + \mathbf{X}_{ict}\delta_2 + v_{ict}.$$

The FEs, α_i and λ_t, are defined in a manner similar to the corresponding variables in equation (1). Controlling for zip code FEs in the first and the second stages captures any time-invariant factors that might determine a community's political importance as well as yearly shocks. $\boldsymbol{Z}_{ct}$ denotes the vector of exogenous instruments as defined in the previous section. We also include the claims variable, C_{ict}, and the vector of other covariates, $\mathbf{X}_{ict}$. In the actual estimation, we instrument the first lag of IA, IA_{ict-1}, using the first lag of the three IVs. The lag structure of the remaining variables is also defined in accordance with equation (2).

The identifying assumption is that the demand for flood insurance should not be affected by the political majority, whether a county is a swing county, or the timing of federal elections, other than through the influence of these variables on governmental relief payments. It could be possible that counties differ in their insurance behavior according to difference in political majority, for instance because of demographic differences correlated with both political ideology and insurance demand. These differences, however, are very likely to be captured by zip code FEs. The variation of this instrument stems only from those counties that switch from a Republican to a Democratic majority or vice versa. We are not aware of any work addressing whether political preference is associated with insurance behavior. We tested many different instruments related to potential political influence and found those presented here to be the strongest.

We undertake several robustness checks and extensions. For our first robustness check, we estimate our IV models using the total amount of IA and SBA loans given to a zip code in a given year as opposed to average amounts of grants or loans. Next, we estimate our models excluding the effects of the 2004–2005 hurricane years and the 2008 financial crisis. Finally, because we have bounded observations for both dependent variables, we also estimate a Tobit model for the average coverage–dependent variable and a generalized linear model with a logit link function and the binomial family for the take-up rate–dependent variable. Our findings are robust to these specifications.

We also undertake two extensions. First, to explore possible heterogeneity in the response to aid, we examine the impact on insurance demand of being in the top or bottom quartile of aid or loans received. As a second extension, we examine the impact of two-year lags, in addition to the one-year lags in our base specifications.

5. Results

Main Results

Table 2 presents the results of Positive IA on flood insurance demand using FE and IV models. The dependent variable is either average coverage (columns 1-4) or policies-in-force per household (columns 5-8). The key independent variables, Positive IA and Positive SBA indicate whether the zip code received any IA grants or SBA loans, respectively in the previous year. Columns 1 and 2 show the FE and IV results for specifications that include only Positive IA. In column 3, we add Positive SBA as additional regressor and estimate the specification using FE, and in column 4, we use the same specification but instrument for IA. The correlation between the aid variables and flood insurance claims is low (0.06 for SBA loans and 0.26 for IA grants).

Comparing the results of the FE estimates (columns 1 and 3) with its IV counterparts (columns 2 and 4) demonstrates the importance of accounting for endogeneity. The results from the FE estimates suggest that if a zip code has received some IA, average coverage will decrease by about $1,448 the following year. However, using IV instead and instrumenting Positive IA with the political variables yields decreases of $15,500 and $17,000, respectively, depending on whether or not we control for SBA loans.

The first stage F-statistics for our IV specification are 28 and 24, respectively, and are above the critical values of the Stock-Yoko weak ID test for a 5% relative bias in the IV of 13.81. The Hansen J-test on overidentifying restrictions generates p-values of 0.22 and 0.27 and thereby does not reject the null that the instruments are uncorrelated with the error term in the second stage. The results of these tests indicate that our instruments are both relevant and valid.

Aid could not only influence the intensive but also the extensive margin, by inducing some individuals to drop their insurance policies entirely or to purchase policies when they had not done so previously. As noted earlier, however, to receive either IA or SBA aid, owners of a damaged house in a high-risk area (as defined by FEMA) must purchase or continue to hold a flood insurance policy. Because of this requirement, we hypothesized that any crowding out effect would be more likely to influence the amount of insurance purchased than the take-up rates.

Columns 5 to 8 show the results of Positive IA and Positive SBA on the number of policies in force per household. Of note, we exclude policies that were purchased as a requirement of aid (this information is available in our NFIP dataset), so that we examine only changes that were not a result of the policy requirement. The FE estimates yield a coefficient of 0.003 that is statistically significant at the 5 %-level. However, the coefficient becomes insignificant in the IV models.

Table 2 also shows the results for our other controls. Median income is negative, suggesting that, as incomes increases, households may tend to self-insure, but the coefficient is small in magnitude, and not statistically significant in all specifications. Our premium variable is negative and significant, as one would expect, but for the reasons discussed above, we do not interpret this as a price elasticity. Finally, higher average claims have a significant, positive effect on average coverage, but it is very small, close to zero effect on take-up rates. A number of factors may explain this result. The year and zip code FEs could be absorbing much of the impact of claims on insurance demand. Individuals might base their coverage levels on their mortgage or home values and may not adjust much after an event. It could also be that, in aggregated data, we cannot tease apart adjustments that are both upward and downward after a disaster event. This is worthy of further study.

We are not able to explicitly control for damaged homes in our analysis. Some might argue that, in theory, if a home is severely damaged by a flood and is not immediately repaired, the homeowner may temporarily reduce coverage commensurate with the lower value of the property.

For three reasons, we do not believe that this is the cause of the decrease in insurance purchased. First, the destruction is likely to be temporary (a few weeks or months). Second, our data is aggregated to the zip code and there may be just as many residents who undertake upgrades with their rebuilding, such that their homes are now worth more. Finally, a decline in home value is usually caused by a decline in the value of the land and is not expected to alter insurance coverage designed to repair or replace damage to the structure.

Table 3 replaces the dummy variables *positive IA* and *SBA* with *average IA* per grant and *average SBA* per loan. Again, the dependent variables are average coverage and policies in force and Table 3 contains both FE and IV specifications. Comparing the estimated coefficients for Average IA in column 1 and 2, again shows that FE clearly underestimates the effect of IA on flood insurance demand. The FE results suggest that a an additional $1,000 in average IA reduces average coverage by $220, while the IV results show that the reduction is about $6,400.

In this case the first stage F-statistics are 11.05 and lie between the critical values for a 5 % bias (13.91) and a 10 % bias (9.08). Compared to the results in Table 2, the coefficients should be interpreted more carefully and the two results should be regarded as a possible range of the effect. The FE estimates can be viewed as a lower bound and the IV estimates (including a potential bias of 10%) as the upper bound. Therefore, an increase in average IA by $ 1,000 can result in a decrease of average coverage between $226 and about $6,400. Turning to the estimates for take-up rate, neither the FE nor the IV specifications find a statistically significant effect of average IA on policies in force.

Robustness Checks

As a first robustness check, instead of using the average grant and average loan amounts as explanatory variables in our IV specifications, we use the total amount of grants or loans given to a zip code. This coefficient may identify the impact of many households receiving aid versus only a few. We find a small reduction in average coverage for increases in total grants received (this is statistically significant in some specifications and not in others). As before, total IA given does not have a statistically significant impact on take-up rates. Total SBA loans given also does not have a statistically significant impact on average coverage or take-up rates. Results are available upon request.

Second, we examine whether particular years are driving our results. Specifically, Florida experienced significant flood events in 2004 and 2005 largely due to hurricanes. In these years, Florida received higher levels of individual disaster aid than in the other years in our sample: $18 million in IA grants in 2004 and $16 million in 2005. We would thus expect that these years are somewhat driving our results. We report this check only for the IV specifications in Table 4 with average coverage and policies-in-force as the dependent variables, respectively. Note that excluding the effects of the aid provided in 2004 and 2005 entails excluding the years 2005 and 2006 from the model, since the variable is lagged by one year.

We find that the effect of Positive IA and Average IA per grant on average coverage is slightly less when we exclude 2005 and 2006 (columns 1 and 2), but similar in magnitude to our previous results. However, we do not find a statically significant effect of the two IA variables on policies in force (columns 3 and 4) once we exclude those two years.

Our final robustness check estimates different functional forms. As our average coverage variable is bounded below at zero, we estimate a Tobit model. As our take-up rate variable in Table

3 is bounded between zero and one, with many observations at zero and only a few at one, we estimate a generalized linear model with a logit link function and the binomial family estimated by quasi-maximum likelihood (Papke and Wooldridge 1996). Some have argued that using ordinary least squares may be preferable to making assumptions about the functional form inherent in these models (Angrist and Pischke 2009), and as such, this is not our preferred approach. Results from both checks are qualitatively similar and are presented in the appendix (Table A.2).

Extensions

Of interest is the question of whether the estimated effect of government aid varies with the amount given. If we find heterogeneous effects, one may be able to design more targeted strategies to limit crowding out. We examine this further, using our preferred IV strategy, by constructing two sets of dummy variables. In the first set, the variables switch to 1 if the zip code was in the top quartile (excluding zip codes that received no payments) of average IA or average SBA payments in the previous year. In the second set, the dummy variables take the value of 1 if the average IA and average SBA payments in the previous year were in the bottom quartile. To ensure that the estimated coefficients are not driven by top or bottom quartile claim numbers, we also include a dummy equal to 1 if the claims in the zip code were in the top or bottom quartile in the previous year and 0 otherwise. These results for average coverage are presented in Table 5.

We find that in zip codes receiving average IA payments in the previous year at the high end of the distribution, average coverage declined by $18,260; a slightly larger effect than we found in Table 2. In the IV specification, we find no statistically significant effect on coverage levels for zip codes in the top 25 percent of the SBA distribution. Interestingly, for the zip codes in the bottom 25 percent, the situation actually reverses. For those in the bottom quartile of average IA payments, average insurance coverage increases the following year by $23,580. This finding suggests that low amounts of aid might demonstrate to homeowners its inability to be a complete substitute for insurance and/or highlight that they were underinsured. For SBA loans, interestingly, we find a small crowding out effect of being in the bottom quartile of the distribution. Although the impact is small, we are unclear what is driving this result and it is more investigation. Again, this would be consistent with the fact that SBA loans can be increased much more substantially depending on need, so those at the low end of the distribution could simply be those who needed smaller loans.

We next run the same specifications as in Table 5, but with the take-up rate as our dependent variable. (These results are available from the authors upon request.) We find no statistically significant effect on take-up rates for being in the top or bottom 25 percent of the IA or SBA distribution.

Our final extension is to examine the impact of aid over a longer time period than just one year. To do this, we re-estimate the IV specifications with two-year lags, instead of just one-year lags. Results are shown in Table 6. The impact of two-year lags is quite similar to that of one-year lags for IA grants: a $1,000 increase in the average IA grant reduces average coverage by roughly $5,300. This finding could be due in part to a delay of several months between a disaster and the receipt of FEMA grants. We find no statistically significant impact of SBA loans for two-year lags, confirming our earlier results. When we perform the same specifications as in Table 6, but with policies-in-force as the dependent variable, the results are never statistically significant, which also confirms our earlier findings.

6. Conclusions

This article provides the first causal estimates of the effect of federal disaster aid on the demand for insurance. The possible disincentive and crowd-out effects of federal aid have been raised by commentators after major disaster events, ranging from hurricanes to financial crises, and yet empirical evidence on this topic had been lacking. To examine this issue, we use floods as a case study because floods are responsible for a large share of damages and deaths caused by natural disasters in the United States; floods are also responsible for roughly two-thirds of US presidential disaster declarations, which trigger federal relief. Florida has the largest share of flood insurance policies across the US, and all communities participate in the NFIP, eliminating any sample selection issues. We examine FEMA disaster grants and control for SBA disaster loans, the two dominant sources of aid for households that sustain damage from a presidentially declared disasters.

Overall, we find that federal post-disaster grants provided to households for uninsured losses have a statistically significant impact on insurance coverage and that the impact is on the intensive, as opposed to the extensive, margin. Requirements that tie the receipt of federal aid to insurance seem to be effective in preventing decreases in take-up rates after a disaster, and may even increase them slightly.

Using our IV results, we estimate that an increase in the average IA grant of $1,000 decreases insurance coverage by roughly $6,400. To put this in context, the median grant in our sample is only $2,950 and the average coverage level is $186,780. Thus, an increase of $1,000 would be quite a substantial increase in aid and the impact on coverage levels appears economically modest. When we focus on zip codes in the top quartile of the distribution, however, the reduction in insurance coverage is nearly three times larger. It thus seems that larger amounts of federal aid granted after a disaster have more substantial crowding out effects, as we hypothesized. Subsidized SBA loans, in contrast to grants, do not have significant impacts on insurance coverage.

Of note, we also find that coverage actually increases for those in the bottom quartile of the IA aid distribution. This may indicate that, when faced with low aid levels, which might cover only a small portion of their loss, individuals recognize the limitations on federal aid and thus find insurance necessary to be made whole after a disaster. These findings are worthy of more investigation, perhaps through surveys to uncover whether low aid levels are contrary to expectations and whether they indicate to households that aid and insurance cannot be treated as substitutes.

Our findings should be of value to ongoing discussions regarding disaster financing, a conversation that has become more critical in light of recent catastrophes that have strained federal resources, such as Hurricane Sandy.[13] Increases in disaster damages, due to both more people locating in risky areas and potential impacts of a changing climate, are likely to continue on their upward trends; this could increase the financial burden of disasters on taxpayers. Our paper suggests that the federal policy of making SBA loans the primary form of response and capping IA grants are important steps in limiting the perverse incentive effects federal disaster aid could create. It appears that the structure of Stafford Act disaster payments *to households* are having a moderate, but not substantial, impact on crowding out insurance.

[13] The findings by Strobl (2011) suggest that natural catastrophes can lead to large short-run economic disruptions at the local level in the U.S. However, these effects tend to be netted out at the state level and do not appear to have a systematic impact on annual growth rates in the U.S. His findings are in line with other recent empirical studies on the effects of natural disasters on economic growth (e.g. Noy and Cavallo 2011, Cavallo et al. 2013). See Kousky (2013) for a review.

We caution the reader, however, that there are two other areas that may have much greater crowding out effects that are worthy of complementary analysis. First, if recent aid spending is any guide, Congress is appropriating more disaster aid funds to the Department of Housing and Urban Development for post-disaster Community Development Block Grants, as opposed to FEMA (see Kousky and Shabman 2013); individuals may or may not receive large reimbursements for damage from programs implemented with these funds as local governments have enormous flexibility in how they are spent. This is another area in need of more careful analysis, particularly if homeowners begin receiving much larger grants than given under the IA program. One reason this article finds modest crowding out effects is likely due to the small size of the IA grants. Second, the vast majority of disaster aid spending goes to local governments. Future work should examine whether those dollars crowd out hazard mitigation at a local level.

Federal response will also need to address affordability issues and the distributional impacts of insurance and aid policies. Millions of Americans live below the poverty line with little financial capacity to purchase financial protection through insurance or to pay the up-front cost of flood risk reduction measures to make their residences safer. This has become a focus of Congressional action recently, when reforms that would eliminate many discounts built into the NFIP were eliminated, increasing rates for many homeowners. After outcry about the affordability of flood coverage, many of those reforms were undone in early 2014. Some observers have proposed that, rather than providing taxpayers aid money after a disaster, it would be more efficient to develop a means-based flood insurance voucher program for low-income residents currently living in flood-prone areas so they can purchase that coverage and be more financially independent should they suffer a flood (Michel-Kerjan and Kunreuther 2011). Furthermore, future work could better identify the relationship between uninsured homeowners and amounts of aid given.

TABLE 1—DESCRIPTIVE STATISTICS, FULL SAMPLE

Variable	Mean	Std. Dev.	Min.	Max.
Average coverage per policy in $1,000s	186.777	62.924	19.500	349.000
Take-up rate: Policies-in-force per household	0.138	0.176	0.000	1.000
Positive IA	0.109	0.313	0.000	1.000
Positive SBA	0.244	0.450	0.000	1.000
Average IA grant amount (in $1,000s)	0.350	1.255	0.000	16.562
Average SBA loan amount (in $1,000s)	4.833	14.029	0.000	240.000
Total claims in zip (in $100,000s)	1.711	36.297	0.000	1,984.946
Average claim per SFH (in $1,000s)	4.114	11.233	0.000	234.265
Premium per $1,000 coverage	2.527	1.387	0.908	18.825
Median income in zip ($1,000s)	41.150	14.787	8.852	171.211
Total housing units in zip	8,700.483	6,309.728	5.000	31,740.000

Notes: SFH: single-family home.
N= 8315

TABLE 2—IMPACT OF POSITIVE IA AND SBA ON FLOOD INSURANCE DEMAND

	Average coverage in $1,000s				Policies-in-force per household			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	IV	FE	IV	FE	IV	FE	IV
Positive IA $_{it-1}$	-1.448***	-15.522**	-1.449***	-17.001**	0.003**	0.021	0.003**	0.022
	(0.433)	(6.668)	(0.444)	(7.335)	(0.001)	(0.017)	(0.001)	(0.019)
Positive SBA $_{it-1}$			0.002	2.633**			0.000	-0.003
			(0.371)	(1.303)			(0.001)	(0.004)
Average claim (in $1,000s) $_{it-1}$	-0.003	0.067*	-0.003	0.072*	0.000**	-0.000	0.000**	-0.000
	(0.013)	(0.037)	(0.013)	(0.039)	(0.000)	(0.000)	(0.000)	(0.000)
Premium per $1,000 of	-12.227***	-12.145***	-12.227***	-12.109***	-0.007**	-0.007***	-0.007**	-0.007***
coverage $_{it}$	(1.292)	(1.194)	(1.292)	(1.190)	(0.003)	(0.002)	(0.003)	(0.002)
Median income (in $1,000s) $_{it}$	-0.235	-0.235*	-0.235	-0.233*	-0.000	-0.000	-0.000	-0.000
	(0.246)	(0.138)	(0.246)	(0.139)	(0.001)	(0.001)	(0.001)	(0.001)
Total Housing Units	0.003***	0.003***	0.003***	0.003***	-0.000*	-0.000***	-0.000*	-0.000***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Underid. test (F-stat)								
First-stage F-stat		28.65		24.00		28.65		24.00
Hansen J-test (P-value)		0.220		0.227		0.925		0.902
N	8315	8315	8315	8315	8315	8315	8315	8315

Notes: This table presents FE (columns 1, 3, 5, and 7) and IV (columns 2, 4, 6, and 8) estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 (columns 1-4) and policies-in-force per household (columns 5-8). Positive IA (Positive SBA) is a dummy variable that switches to one if the zip code received IA (had any SBA loan applications) in t-1 and zero otherwise. All specifications include zip code and year FEs. Panel units are zip codes. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Presidential election and a dummy indicating that year t-1 was a Presidential election year; the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year; and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year are used as instruments for Positive IA $_{it-1}$, in columns 2, 4, 6, and 8. The Stock-Yogo weak ID test critical value for a 5% (10%) maximal IV relative bias is 13.91 (9.08). Standard errors (in parentheses) are clustered at the zip code level. Constants are included but not reported. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE 3—IMPACT OF AVERAGE IA PER GRANT AND AVERAGE SBA PER LOAN ON FLOOD INSURANCE DEMAND

	Average coverage in $1,000s				**Policies-in-force per household**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	IV	FE	IV	FE	IV	FE	IV
Average IA per grant $_{it-1}$	-0.226**	-6.450**	-0.220**	-6.429**	0.001	0.006	0.001	0.006
	(0.103)	(2.601)	(0.103)	(2.594)	(0.000)	(0.006)	(0.000)	(0.006)
Average SBA per loan $_{it-1}$			-0.006	0.043*			0.000	-0.000
			(0.008)	(0.024)			(0.000)	(0.000)
Average claim (in $1,000s) $_{it-1}$	-0.005	0.141**	-0.005	0.139**	0.000**	-0.000	0.000**	-0.000
	(0.013)	(0.066)	(0.013)	(0.065)	(0.000)	(0.000)	(0.000)	(0.000)
Premium per $1,000 of	-12.232***	-12.137***	-12.229***	-12.157***	-0.007**	-0.007***	-0.007**	-0.007***
coverage $_{it}$	(1.292)	(1.200)	(1.292)	(1.202)	(0.003)	(0.002)	(0.003)	(0.002)
Median income (in $1,000s) $_{it}$	-0.235	-0.215	-0.235	-0.212	-0.000	-0.000	-0.000	-0.000
	(0.246)	(0.144)	(0.246)	(0.145)	(0.001)	(0.001)	(0.001)	(0.001)
Total housing units $_{it}$	0.003***	0.003***	0.003***	0.003***	-0.000*	-0.000***	-0.000*	-0.000***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
First-stage F-stat		11.05		11.67		11.05		11.05
Hansen J-test (P-value)		0.942		0.897		0.788		0.810
N	8315	8315	8315	8315	8315	8315	8315	8315

Notes: This table presents FE (columns 1, 3, 5, and 7) and IV (columns 2, 4, 6, and 8) estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 (columns 1-4) and policies-in-force per household (columns 5-8). Positive IA (Positive SBA) is a dummy variable that switches to one if the zip code received IA (had any SBA loan applications) in t-1 and zero otherwise. All specifications include zip code and year FEs. Panel units are zip codes. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Presidential election and a dummy indicating that year t-1 was a Presidential election year; the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year; and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year are used as instruments for Positive IA $_{it-1}$, in columns 2, 4, 6, and 8. The Stock-Yogo weak ID test critical value for a 5% (10%) maximal IV relative bias is 13.91 (9.08). Standard errors (in parentheses) are clustered at the zip code level. Constants are included but not reported. *** Significant at the 1 percent level; ** Significant at the 5 percent level; Significant at the 10 percent level.

TABLE 4—IMPACT OF IA AND SBA ON FLOOD INSURANCE DEMAND EXCLUDING 2005/2006

	Exclude 2005/2006			
	Average coverage		*Policies-in-force*	
	(1)	(2)	(3)	(4)
Positive IA $_{it-1}$	-15.896***		0.011	
	(7.055)		(0.020)	
Positive SBA $_{it-1}$	1.988		0.001	
	(1.346)		(0.004)	
Average IA per grant $_{it-1}$		-5.525**		0.002
		(2.335)		(0.006)
Average SBA per loan $_{it-1}$		0.035*		-0.000
		(0.019)		(0.000)
Average claim (in $1,000s) $_{it-1}$	0.052	0.091*	0.000	0.000
	(0.034)	(0.050)	(0.000)	(0.000)
Premium per $1,000 of	-12.194***	-12.177***	-0.008***	-0.008***
coverage $_{it}$	(1.284)	(1.287)	(0.002)	(0.002)
Median income (in $1,000s) $_{it}$	-0.198	-0.191	-0.000	-0.000
	(0.161)	(0.163)	(0.001)	(0.001)
Total housing units $_{it}$	0.002***	0.002***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
First-stage F-stat	36.15	19.89	15.30	9.51
Hansen J-test (P-value)	0.530	0.838	0.511	0.132
N	6465	6465	6465	6465

Notes: This table presents IV estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 (columns 1-4) and policies-in-force per household (columns 5-8). Columns1, 2, 5, and 6 exclude observations from the years 2005 and 2006; columns 4, 5, 7, and 8 exclude observations from the years 2008 and 2009, respectively. Average IA per grant (Average SBA per loan) is the average IA grant amount (in $1,000s) (average SBA loan amount (in $1,000s)) in t-1. All specifications include zip code and year FEs. Panel units are zip codes. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Presidential election and a dummy indicating that year t-1 was a Presidential election year; the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year; and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year are used as instruments for Positive IA $_{it-1}$, and Average IA per grant $_{it-1}$. The Stock-Yogo weak ID test critical value for a 5% (10%) maximal IV relative bias is 13.91 (9.08) Standard errors (in parentheses) are clustered at the zip code level. Constants are included but not reported. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE 5—IMPACT OF TOP AND BOTTOM QUARTILE IA AND SBA ON INSURANCE DEMAND (AVERAGE COVERAGE IN $1,000S)

Average coverage in $1,000s	(1) FE	(2) IV	(3) FE	(4) IV
Top 25 percent of average IA $_{it-1}$	-1.225**	-18.264*		
	(0.623)	(10.707)		
Top 25 percent of average SBA $_{it-1}$	-1.778***	-0.893		
	(0.468)	(0.747)		
Bottom 25 percent of IA $_{it-1}$			1.587***	23.576**
			(0.466)	(10.878)
Bottom 25 percent of SBA $_{it-1}$			-0.006	-2.974**
			(0.359)	(1.492)
Top 25 percent of claims $_{it-1}$	0.216	3.002*		
	(0.390)	(1.801)		
Bottom 25 percent of claims $_{it-1}$			0.140	-2.744*
			(0.300)	(1.486)
Premium per $1,000 of coverage $_{it}$	-11.510***	-12.213***	-11.496***	-12.006***
	(1.275)	(1.195)	(1.275)	(1.184)
Median income (in $1,000s) $_{it}$	-0.252	-0.231*	-0.255	-0.217
	(0.241)	(0.136)	(0.242)	(0.142)
Total housing units $_{it}$	0.003***	0.003***	0.003***	0.003***
	(0.001)	(0.000)	(0.001)	(0.000)
Zip FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
First-stage F-stat		9.62		21.03
Hansen J-Test (p-value)		0.274		0.742

Notes: This table presents IV estimates of the effect of Top and Bottom 25 percent of average IA $_{it-1}$ on insurance demand, measured as average coverage in $1,000. Top (Bottom) 25 percent of average IA $_{it-1}$ is a dummy variable that switches to one if the zip code was in the top (bottom) quartile of IA recipients (measured by the total value of IA grants). All specifications include zip code and year FEs. Panel units are zip codes. Instruments for Top and Bottom 25 percent of average IA $_{it-1}$, in columns 1 and 3, are: the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year. N=8,315. Standard errors (in parentheses) are clustered at the zip code level. Constants are included but not reported.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 6—IMPACT OF IA AND SBA ON INSURANCE DEMAND (AVERAGE COVERAGE IN $1,000S), TWO-YEAR LAG

	Average coverage in $1,000s		**Policies-in-force per household**	
	(1)	(2)	(3)	(4)
Positive IA $_{it-2}$	-10.425		0.008	
	(7.194)		(0.017)	
Positive SBA $_{it-2}$	0.926		-0.001	
	(1.149)		(0.003)	
Average IA per grant $_{it-2}$		-5.274*		0.000
		(2.832)		(0.006)
Average SBA per loan $_{it-2}$		0.023		0.000
		(0.023)		(0.000)
Average claim (in $1,000s) $_{it-2}$	0.009	0.067	0.000	0.000
	(0.037)	(0.062)	(0.000)	(0.000)
Premium per $1,000 of	-12.663***	-12.632***	-0.006***	-0.006***
coverage $_{it}$	(1.439)	(1.443)	(0.002)	(0.002)
Median income (in $1,000s) $_{it}$	-0.257*	-0.242	-0.000	-0.000
	(0.153)	(0.157)	(0.001)	(0.001)
Total housing units $_{it}$	0.002***	0.003***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
First-Stage F-Test	27.36	12.97	27.36	12.98
Hansen J-Test (p-value)	0.000	0.004	0.365	0.287
N	7394	7394	7394	7394

Notes: This table presents IV estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 (columns 1-4) and policies-in-force per household (columns 5-8). Average IA per grant (Average SBA per loan) is the average IA grant amount (in $1,000s) (average SBA loan amount (in $1,000s)) in t-2. All specifications include zip code and year FEs. Panel units are zip codes. The second lag of an interaction term between a dummy indicating that a county was a swing county in the last US Presidential election and a dummy indicating that year t-2 was a Presidential election year; the second lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-2 was a Senate election year; and the second lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year are used as instruments for Positive IA $_{it-2}$, and Average IA per grant $_{it-2}$. The Stock-Yogo weak ID test critical value for a 5% (10%) maximal IV relative bias is 13.91 (9.08) Standard errors (in parentheses) are clustered at the zip code level. Constants are included but not reported.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix A

Table A1 presents the descriptive statistics of the IVs.

Table A2 presents the results of the Tobit and quasi-maximum likelihood estimate robustness checks.

TABLE A1—DESCRIPTIVE STATISTICS, INSTRUMENTAL VARIABLES

Variable	Mean	Std. Dev.	Min.	Max.
Swing county Presidential $_{ct}$ X Presidential election year $_{t}$	0.350	0.477	0.000	1.000
Swing county Senate $_{ct}$ X Senate election year $_{t}$	0.048	0.215	0.000	1.000
Democratic majority $_{ct}$ X presidential election year$_{t+1}$	0.134	0.341	0.000	1.000

TABLE A2—IMPACT OF IA AND SBA ON INSURANCE DEMAND (AVERAGE COVERAGE IN $1,000S AND POLICIES-IN-FORCE PER HOUSEHOLD), TOBIT AND QUASI-MAXIMUM LIKELIHOOD ESTIMATES

	Tobit Average coverage in $1,000s		Quasi-maximum likelihood estimates Policies-in-force per household	
Average IA per grant $_{it-1}$	-0.307**		0.000**	
	(0.152)		(0.000)	
Average SBA per loan $_{it-1}$		-0.003		-0.000
		(0.013)		(0.000)
Average claim (in $1,000s) $_{it-1}$	0.006	-0.001		
	(0.017)	(0.017)		
Total claims (in $1,000s) $_{it-1}$			0.000	0.000
			(0.000)	(0.000)
Premium per $1,000 of coverage $_{it}$	-12.030***	-12.032***	-0.010***	-0.010***
	(0.307)	(0.307)	(0.003)	(0.003)
Median income (in $1,000s) $_{it}$	1.225***	1.225***	-0.000	-0.000
	(0.070)	(0.070)	(0.000)	(0.000)
Total housing units $_{it}$	0.002***	0.002***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
N	8425	8425	8425	8425

Notes: This table presents Tobit (columns 1 and 2) and quasi-maximum likelihood (Papke and Wooldridge 1996; columns 3 and 4) estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 (columns 1 and 2) or policies-in-force per household (columns 3 and 4). IA and SBA are defined as the average amount of IA per grant and the average amount of SBA per loan. All specifications include zip code and year FEs. Panel units are zip codes. Marginal effects are reported. Robust standard errors are in parentheses in columns 1 and 2. Standard errors (in parentheses) are clustered at the zip code level in columns 3 and 4. Constants are included but not reported.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

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