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Does Federal Disaster Assistance Crowd Out Private Demand for Insurance?

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We present the first causal estimates of the effect of federal disaster relief on insurance demand using a unique panel dataset of insurance contracts and disaster aid disbursements. We address endogeneity using instrumental variables that exploit political influence over aid amounts. We find that a $1 increase in average aid grants decreases average insurance coverage by about $6, with variation depending on aid amount. This crowding out effect is on the intensive, as opposed to the extensive margin; we find no impact on take-up rates. Government loans, as opposed to grants, have no effect on insurance demand on either margin and might thus be a better policy tool.

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I. 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).

Surprisingly, though, tests of those 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, 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. Furthermore, while surveys provide important insight on how unobservable variables impact decision-making, individual responses to surveys do not always capture actual decision-making.

We provide the first empirical evidence of a crowding out effect associated with US federal disaster aid to households, using data on observed insurance purchasing behavior rather than experimental or survey data. If lower risk protection is indeed a problem associated with federal disaster aid, one readily observable outcome would be changes in insurance purchases. Natural disasters are one area where there is a standing federal policy regarding aid, yet a good understanding of the influence of post-disaster aid on households' financial protection decisions is lacking. We focus on flood events, since 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).

The previous theoretical work on this topic is centered on how ex ante expectations influence decisions to insure. To test those predictions, expectations about post-disaster aid 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. As such, our research answers the following questions: Does the receipt of government disaster aid reduce demand for insurance? If so, how large is the reduction? Finally, do all aid programs have the same impact? The responses to these questions have important economic, social, and policy implications.

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. For example, in the health care context, Brown and Finkelstein (2008) analyze the effect of Medicaid on the demand for private long-term care insurance. Similarly, in this paper, we use the term “crowding out” to refer to a reduction in insurance purchases as a response to the receipt of federal disaster aid.[2] Of note, we focus on federal reimbursement for property damage, not on emergency response. As such, we do not analyze the extent to which federal emergency response crowds out private relief efforts (e.g., donations to the American Red Cross). 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 future 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

[2] 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 (which we show are critical), possible measurement error in their aid variable (their analysis included all types of aid given to individuals, businesses, and the public sector for any type of emergency and reconstruction effort and for all disaster types), and they use a high level of aggregation.

We are able to overcome these limitations and estimate a causal effect of federal disaster aid on flood insurance purchases. Specifically, we examine the influence of disaster grants from the Individual Assistance (IA) program of FEMA provided directly to affected households for uninsured losses, as well as the influence of low-interest disaster loans from the US Small Business Administration (SBA). Those programs have long been the two primary sources of direct federal aid for households that sustain damage from a disaster. We have 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 over a 10-year period, 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. Our final panel dataset contains annual observations of all our variables for zip codes in Florida between 2000 and 2009.

A potential challenge for our empirical analysis is a two-way causality problem between disaster aid and insurance: a higher penetration of flood insurance will reduce the amount of aid needed after an event.[3] In addition, omitted variables are likely to be correlated with both aid received and insurance purchases, such as expectations about aid 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 relief 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 federal aid: 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 should have no effect on demand for flood insurance other than through governmental post-disaster aid payments received.

An important question is whether the two federal disaster aid programs we analyze have a similar effect on insurance demand. Since the IA aid from FEMA is a grant that the recipient does not have to repay, theory would predict that its availability would reduce the quantity of insurance demanded (Ehrlich and Becker 1972). It is unclear, however, whether the influence will be on the amount of insurance purchased or the decision to purchase insurance at all; we thus test for both effects. SBA disaster loans must be repaid over time, although at a lower rate than the homeowner

[3] Indeed, the one other empirical analysis of the relationship between aid and insurance, a 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 the endogeneity issue.

would obtain in the private market (if she or he could obtain a private loan at all). On the one hand, this access to liquidity could be seen as a substitute for insurance and thus reduce insurance demand. On the other hand, since the borrower will repay the entire loan with interest, purchasing insurance may be preferred (depending on the cost of insurance and the interest rate on the SBA loan). The overall impact on insurance purchases is thus uncertain.

Our empirical findings reveal that FEMA IA grants have a statistically significant negative impact on average coverage per policy. A $1,000 increase in the average IA grant decreases average insurance coverage by between $5,322 and $6,350. For zip codes in the top quartile of average IA grant amounts, coverage decreases by nearly $18,700; a much more economically significant impact. Interestingly, though, for those in the bottom quartile of average IA grant amounts, insurance coverage purchased actually increases, by more than $20,500. 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. In contrast, we find that SBA disaster loans have no systematic effect on coverage levels. We find that standard FE estimates would substantially underestimate the crowding out impacts, highlighting the need to instrument for disaster aid. 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 paper 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.

II. 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. 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.[4]

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 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 aid that is solely given as a result of the disaster event.[5]

[4] 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.

[5] 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

We focus first on FEMA’s IA program. Such aid is grouped into housing assistance and other needs assistance. The former covers temporary housing or home repairs, while 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 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.

The other primary form of federal aid for homeowners who sustain damage in a disaster is a low-interest loan 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

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.

SBA). For those who could obtain credit elsewhere, the interest rate would not exceed 8 percent.[6] 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.

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. Since 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).

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

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.[7] 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).[8]

III. 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 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, just fewer 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

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

[8] 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.

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 from the US Small Business Administration data on SBA 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 at the US Department of Homeland Security 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 since 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.[9] 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 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

[9] 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 the deductible factor.

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.[10]

We use this dataset to construct our instrumental variables, two for IA and two for SBA. The instruments for the average IA grant 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 (which we define as counties in which the difference in votes for the Democrats and Republicans was no more than 5 percent in the last US Senate election) and a dummy indicating that year *t-1* was a Senate election year[11] and (2) the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election[12] and a dummy indicating that year *t* was a presidential election year. The instruments for average SBA loan amount in *t-1* are: (1) the first lag of an interaction term between a dummy indicating that a county had a strong majority (such that the winning party had at least 70 percent[13] of the votes) in the last US Senate election and a dummy indicating that year *t-1* was a Senate election year and (2) the first lag of an interaction term between a dummy indicating that that a county had a strong majority (again, meaning that the winning party had 70 percent or more of the votes) in the most recent presidential election and a dummy indicating that year *t-1* was a presidential election year.

[10] 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.

[11] During our sample period, US Senate elections in Florida took place in 2000, 2004, and 2006.

[12] During our sample period, US presidential elections took place in 2000, 2004, and 2008.

[13] In our sample, this applies to about 37 percent of our observations.

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 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).[14] 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 the *average* IA grant or SBA loan given in a zip code. 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. 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.

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

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

Take-up rate: Policies-in-force per household	0.138	0.176	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.

IV. 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 almost surely endogenous, our preferred specification is an IV approach that exploits exogenous variation in political factors to instrument for governmental aid payments. We thus first specify the following FE model using our balanced panel dataset with *i* indexing zip codes, *c* indexing counties in Florida, and *t* indexing years:

$$(1)\quad FI_{ict} = \alpha_i + \lambda_t + \beta_1 IA_{ict-1} + \beta_2 C_{ict-1} + \mathbf{X}_{ict}\boldsymbol{\beta}_{\mathbf{3}} + u_{it}.$$

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. We specify *IA* as the average IA grant amount given per beneficiary in a particular zip code and year. We construct an analogous variable for SBA loans. We 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 just stated, we know, however, that the FE model is probably 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 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 and SBA 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. 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, $\mathbf{X}_{ict}$.

We use the same specification of equation (2) for both measures of governmental relief, *IA* and *SBA*, and we include their respective average variables. 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). We apply the same approach for SBA_{ict-1}.

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 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, since 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.

V. Results

A. Main Results

Table 2 presents the results of both the FE and IV models for average insurance coverage as our dependent variable. The endogenous variables of interest are the two different measures of aid, average IA grant amount and average SBA loan amount in the zip code in the previous year. Columns 1 and 2 show results for specifications that include only the IA variables, and columns 3 and 4 show results for only the SBA variables. We estimate the effects of IA and SBA separately, but it is also possible that the coefficient of one individual aid variable captures the effects of the other, excluded aid variable. Although the correlation between average IA and SBA is relatively low (0.17), we confirm the robustness of our results by including both IA and SBA variables in the same specification, as shown in columns 5–8. In column 6 we instrument only for IA, in column 7 we instrument only for SBA, and in column 8 we instrument for both. The correlation between the aid variables and claims is also low (0.06 for SBA loans and 0.26 for IA grants).

The IV results are more than an order of magnitude greater than the FE results, demonstrating the importance of accounting for endogeneity. The IV results indicate that a $1,000 higher average IA per grant leads to a statistically significant decrease in average coverage of between $5,322 and $6,350[15]. IV coefficients on the IA variables are not much changed depending

[15] The median IA grant in our sample is $2,950 and the average coverage level is $186,780.

on whether we include the SBA as a covariate. The Kleinbergen-Paap F statistics and the first-stage F-statistics for all IV specifications indicate that the political variables we used are strong instruments. The Hansen J-test on overidentifying restrictions yields p-values of 0.32 and 0.81 and 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. The results for the SBA variables, in contrast, are never significantly different from zero for any of the IV results. The results of the Kleinbergen-Paap test, the first-stage F-statistics, and the Hansen J-test show that the political IVs are strong and valid instruments for lagged average SBA in specifications 4 and 8. If we include IA as additional regressor, the Hansen J-test is 0.04.

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 IV 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, claims are not statistically significant. 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.

TABLE 2—IMPACT OF IA AND SBA ON INSURANCE DEMAND (AVERAGE COVERAGE IN $1,000S)

Average coverage in $1,000s	(1) FE	(2) IV	(3) FE	(4) IV	(5) FE	(6) IV	(7) IV	(8) IV
Average IA per grant $_{it-1}$	-0.279**	-6.350*			-0.275**	-6.240*	-0.124	-5.322*
	(0.115)	(3.221)			(0.115)	(3.149)	(0.301)	(2.906)
Average SBA per loan $_{it-1}$			-0.006	0.307	-0.004	0.042	-0.148	0.161
			(0.009)	(0.296)	(0.009)	(0.028)	(0.268)	(0.260)
Average claim (in $1,000s) $_{it-1}$	-0.003	0.138	-0.09	-0.017	-0.003	0.135	-0.003	0.110
	(0.013)	(0.080)	(0.013)	(0.016)	(0.013)	(0.078)	(0.012)	(0.072)
Premium per $1,000 of coverage $_{it}$	-11.502***	-12.138***	-11.501***	-11.598***	-11.501***	-12.159***	-11.457***	-12.228***
	(1.275)	(1.200)	(1.276)	(1.116)	(1.275)	(1.201)	(1.102)	(1.206)
Median income (in $1,000s) $_{it}$	-0.256	-0.215	-0.257	-0.233*	-0.256	-0.212	-0.268**	-0.206
	(0.242)	(0.144)	(0.241)	(0.139)	(0.242)	(0.145)	(0.133)	(0.145)
Total housing units $_{it}$	0.003***	0.003***	0.003***	0.003***	0.003***	0.003***	0.003***	0.003***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(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*
Instrumented	-	IA	-	SBA	-	IA	SBA	IA/SBA
Underid. test (F-stat)		25.94		22.71		28.26	18.52	12.37
First-stage F-stat		13.60		11.46		14.80	9.44	11.42/6.90
Hansen J-test (P-value)		0.687		0.812		0.578	0.043	0.316
N	8,425	8,315	8,425	8,425	8,425	8,315	8,425	8,315

Notes: This table presents FE (columns 1, 3, and 5) and IV (columns 2, 4, 6, 7, and 8) estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000. IA and SBA are defined as the average amount of IA per grant and the average amount of SBA per loan. All specifications include zip code and year FEs. Panel units are zip codes. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year are used as instruments for Average IA per grant$_{it-1}$, in columns 2 and 6. The first lag of an interaction term between a dummy indicating that a county had a strong majority in the last Senate election and a dummy indicating that year t-1 was a Senate election year and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year are used as instruments for Average SBA per loan$_{it-1}$, in columns 4 and 7. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year are used as instruments for Average IA per grant_{it-1} and Average SBA per loan_{it-1}, in column 8. 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.

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.

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. Still, in Table 3, we replace the dependent variable with the number of policies-in-force per household in each zip code and year. 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 rest of the structure of the table is similar to that of Table 2: we first present the FE and IV results for specifications that include IA and SBA separately (columns 1–4), followed by FE and IV estimates for specifications that include IA and SBA at the same time and alternately instrument for either IA, SBA, or both aid variables at the same time (columns 5–8). The aid variables are again defined as average values per grant or loan.

Table 3 shows that, for all IV results, neither form of aid has a statistically significant impact on take-up rates. The results of the Kleinbergen-Paap test, the first-stage F-test, and the Hansen J-test again suggest that our instruments are both relevant and valid for all IV specifications. Our results on the impact of individual assistance on take-up rates stand in contrast to those of Browne and Hoyt (2000), who find an increase in the statewide take-up rate of flood insurance after governmental relief was paid. One explanation for this difference could be that Browne and Hoyt do not account for new contracts that were purchased as a requirement of receiving aid. We thus run the same specifications as in Table 3, except that this time we *include* the policies that were purchased because of this requirement. With these policies included, the coefficients are essentially unchanged, but the small positive effect of IA grants becomes statistically significant. The policy requirements to purchase insurance in order to obtain aid appears to generate a small number of new insurance policies.

TABLE 3—IMPACT OF IA AND SBA ON INSURANCE DEMAND (POLICIES-IN-FORCE PER HOUSEHOLD, EXCLUDING NEW POLICIES REQUIRED FOR GRANTEES AND LOAN BENEFICIARIES)

Policies-in-force per household	(1) FE	(2) IV	(3) FE	(4) IV	(5) FE	(6) IV	(7) IV	(8) IV
Average IA per grant $_{it-1}$	0.001**	0.008			0.001**	0.008	0.000	0.008
	(0.000)	(0.006)			(0.000)	(0.006)	(0.001)	(0.006)
Average SBA per loan $_{it-1}$			0.000	0.000	0.000	-0.000	0.000	-0.000
			(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Total claims (in $1000s) $_{it-1}$	0.000	-0.000	0.000	0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Premium per $1,000 of coverage $_{it}$	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Median income (in $1000s) $_{it}$	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total housing units $_{it}$	-0.000	-0.000***	-0.000	-0.000***	-0.000	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(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*
Instrumented	-	IA	-	SBA	-	IA	SBA	IA/SBA
Underid. test (F-stat)		28.94		23.43		30.50	21.77	12.27
First-stage F-stat		14.76		11.82		16.11	10.99	12.51/7.19
Hansen J-test (P-value)		0.499		0.682		0.552	0.685	0.885
N	8,425	8,315	8,425	8,425	8,425	8,315	8,425	8,315

Notes: This table presents FE (columns 1, 3, and 5) and IV (columns 2, 4, 6, 7, and 8) estimates of the effect of IA and SBA on insurance demand, measured as policies-in-force per household. The dependent variable is policies-in-force per household minus policies purchased under the mandatory purchasing requirement. 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. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last 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 Average IA per grant$_{it-1}$, in columns 2 and 6. The first lag of an interaction term between a dummy indicating that a county had a strong majority in the last 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 that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year are used as instruments for Average SBA per $loan_{it-1}$, in columns 4 and 7. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year are used as instruments for Average IA per $grant_{it-1}$ and Average SBA per $loan_{it-1}$, in column 8. 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.

B. 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 loan 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-in-force as the dependent variable.[16] 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.

Perhaps not surprisingly, we find somewhat lower coefficients on the average IA variable, and our results are only marginally significant. This suggests that the crowding out effect from federal disaster aid may vary depending on the level of aid provided. The small amounts of aid

[16] We also run the same specifications with take-up rates as the dependent variable and find no statistically significant impact, save for a small positive and marginally significant impact of US Small Business Administration loans in the specification in which we instrument for both variables.

associated with milder disaster years might have a smaller effect on insurance demand. The larger amounts from years with multiple or bigger disasters, however, could have a stronger crowding out effect. In the next section, we explore this possibility of crowding out occurring only when the amount of disaster aid is large; we discuss the policy implications in the conclusion.

We also examine the robustness of our results to excluding the 2008 financial crisis, although some of this should be captured in our year FEs. We run the same IV specifications shown in Table 4, but this time we exclude the year 2009. Results are very similar to those shown in Tables 2 and 3, so we do not report them in table form. (These results are available from the authors upon request.) Specifically, when we exclude 2009, we find that a $1,000 increase in the average IA grant decreases average coverage by roughly between $5,100 and $5,300.[17] Similar to our main results, we find no significant impact of SBA loans on average coverage. The impact on take-up rates is similar to our findings with the full sample, except that in a couple specifications, an increase in the average SBA loan amount leads to a very small decrease in take-up rates at a 5 percent or 10 percent significance level (coefficients of -0.001).

[17] We extend this test by excluding each year in our sample individually, although we are not concerned about confounding effects in the other years. We obtain results that are roughly the same as those shown in Table 2 for all of these tests except the exclusion of 2001 and 2005. Excluding these years, however, eliminates one-third of the observations for which one of our instrumental variables (dummy swing county Senate $_{ct-1}$ X Senate election year $_{t-1}$) is one since, within our short panel, US Senate elections were held in Florida only in 2000, 2004, and 2006. In this case, the first-stage F-test is around 6 and the second-stage coefficient of IA is no longer significant at conventional levels. We, therefore, believe that we cannot infer much from the results that exclude these years since our instruments are no longer relevant.

Our final robustness check estimates different functional forms. Since our average coverage variable is bounded below at zero, we estimate a Tobit model. Since 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.3).

TABLE 4—IMPACT OF IA AND SBA ON INSURANCE DEMAND (AVERAGE COVERAGE IN $1,000S), EXCLUDING 2005 AND 2006

Average coverage in $1,000s	(1)	(2)	(3)	(4)	(5)
Average IA per grant $_{it-1}$	-4.653*		-4.529*	-1.134	-0.406
	(2.831)		(2.272)	(0.796)	(2.534)
Average SBA per loan $_{it-1}$		0.068	0.028	-0.756*	0.175
		(0.268)	(0.023)	(0.392)	(0.410)
Average claim (in $1,000s) $_{it-1}$	0.077	-0.006	0.074	0.010	-0.004
	(0.059)	(0.022)	(0.056)	(0.022)	(0.054)
Premium per $1,000 of coverage $_{it}$	-12.175***	-12.097***	-12.195***	-11.686***	-12.376***
	(1.424)	(1.364)	(1.427)	(1.336)	(1.444)
Median income (in $1,000s) $_{it}$	-0.197	-0.229	-0.195	-0.284**	-0.201
	(0.309)	(0.293)	(0.309)	(0.287)	(0.304)
Total housing units $_{it}$	0.002***	0.002***	0.002***	0.003***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Instrumented	IA	SBA	IA	SBA	IA/SBA
Underid. test (F-stat)	50.49	14.65	48.04	15.99	11.40
First-stage F-stat	27.46	7.32	25.79	8.28	25.01/5.52
Hansen J-test (P-value)	0.699	0.065	0.661	0.030	0.027
N	6,465	6,551	6,465	6,551	6,465

Notes: This table presents IV estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000 with the years 2005 and 2006 excluded. IA and SBA are defined as the average amount per grant and per loan in dollars. All specifications include zip code and year FEs. Panel units are zip codes. Instruments for Average IA per grant$_{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. Instruments for Average SBA per loan$_{it-1}$, in columns 2 and 4, are: the first lag of an interaction term between a dummy indicating that a county had a strong majority in the last 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 that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. Instruments for Average IA per grant$_{it-1}$ and Average SBA per loan$_{it-1,}$ in column 5, are: the first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. 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.

C. 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. Our robustness check excluding the years 2005 and 2006 yields a lower coefficient for the average IA per grant variable (around -4.6, rather than -6.3), suggesting that the size of the aid individuals receive may matter. 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 $18,780; a much larger effect than we found in Table 2. 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 $20,550. 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, we find no statistically significant effect of being in the bottom quartile of the distribution. 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.

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) IV	(2) IV	(3) IV	(4) IV
Top 25 percent of average IA $_{it-1}$	-18.782*			
	(10.739)			
Top 25 percent of average SBA $_{it-1}$		78.092		
		(109.589)		
Bottom 25 percent of IA $_{it-1}$			20.551**	
			(9.100)	
Bottom 25 percent of SBA $_{it-1}$				-9.173
				(8.136)
Top 25 percent of claims $_{it-1}$	3.085*	-0.159		
	(1.825)	(1.153)		
Bottom 25 percent of claims $_{it-1}$			-2.436*	0.684
			(1.321)	(0.388)
Premium per $1,000 of coverage $_{it}$	-12.210***	-11.191***	-12.046***	-11.430***
	(1.293)	(1.432)	(1.187)	(1.287)
Median income (in $1,000s) $_{it}$	-0.233*	-0.394	-0.217	-0.264**
	(0.250)	(0.359)	(0.259)	(0.246)
Total housing units $_{it}$	0.003***	0.004**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Instrumented	IA	SBA	IA	SBA
Underid. test (F-stat)	27.10	0.90	68.64	30.88
First-stage F-stat	9.84	0.45	27.28	15.31
Hansen J-test (P-value)	0.279	0.812	0.664	0.633
N	8,315	8,425	8,315	8,425

Notes: This table presents IV estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000, with the years 2005 and 2006 excluded. IA and SBA are defined as the average amount per grant and per loan in dollars. All specifications include zip code and year FEs. Panel units are zip codes. Instruments for Average IA per $grant_{it-1}$, 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. Instruments for Average SBA per $loan_{it-1}$, in columns 2 and 4, are: the first lag of an interaction term between a dummy indicating that a county had a strong majority in the last 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 that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. Instruments for Average IA per $grant_{it-1}$ and Average SBA per $loan_{it-1,}$ in column 5, are: the first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. 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	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
Average IA per grant $_{it-2}$	-6.503**		-6.390**	-0.485*	-5.584*
	(3.247)		(3.213)	(0.248)	(3.126)
Average SBA per loan $_{it-2}$		0.032	0.031	0.008	0.085
		(0.244)	(0.025)	(0.196)	(0.233)
Average claim (in $1,000s) $_{it-2}$	0.091	-0.039**	0.088	-0.029	0.072
	(0.073)	(0.017)	(0.072)	(0.018)	(0.070)
Premium per $1,000 of coverage $_{it}$	-12.577***	-11.868***	-12.604***	-11.844***	-12.670***
	(1.442)	(1.407)	(1.444)	(1.410)	(1.443)
Median income (in $1,000s) $_{it}$	-0.242	-0.270	-0.241	-0.270	-0.239
	(0.249)	(0.237)	(0.249)	(0.238)	(0.247)
Total housing units $_{it}$	0.003***	0.002***	0.003***	0.002***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Zip FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Instrumented	IA	SBA	IA	SBA	IA/SBA
Underid. test (F-stat)	31.67	29.34	33.53	23.17	14.38
First-stage F-stat	16.66	14.83	17.64	11.96	12.90/8.27
Hansen J-test (P-value)	0.165	0.824	0.144	0.679	0.059
N	7,394	7,492	7,394	7,492	7,394

Notes: This table presents IV estimates of the effect of IA and SBA on insurance demand, measured as average coverage in $1,000, with the years 2005 and 2006 excluded. IA and SBA are defined as the average amount per grant and per loan in dollars. All specifications include zip code and year FEs. Panel units are zip codes. Instruments for Average IA per grant$_{it-1}$, in columns 1 and 3, are: the first lag of an interaction term between a dummy indicating that a county was a swing county in the last US Senate election and a dummy indicating that year t-1 was a Senate election year and the first lag of an interaction term between a dummy indicating that the county had a Democratic majority in the last presidential election and a dummy indicating that year t was a presidential election year. Instruments for Average SBA per loan$_{it-1}$, in columns 2 and 4, are: the first lag of an interaction term between a dummy indicating that a county had a strong majority in the last Senate election and a dummy indicating that year t-1 was a Senate election year and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. Instruments for Average IA per grant$_{it-1}$ and Average SBA per loan$_{it-1}$, in column 5, are: the first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 was a presidential election year. 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.

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 between $5,585 and $6,500. 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.

VI. Conclusions

This paper 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 aid to households hit by hurricanes to aid given to banks during the financial crisis, and yet empirical evidence on this topic had been lacking. To examine this issue, we use floods as a case study since 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 are able to examine both FEMA disaster grants and 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 has 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 between $5,300 and $6,300. To put this in context, the median grant in our sample is only $2,950 and the average coverage level is $186,780. 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 grant after a disaster have more substantial crowding out effects, as we hypothesized. Subsidized SBA loans, in contrast to grants, have no statistically significant impact 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. Given that nearly half of the US population lives in coastal counties, this is a critical economic issue[18]. Furthermore, a changing climate, including anticipated levels of sea level rise, as well as more people living in high-risk areas of the coast will only amplify these concerns; this will increase the financial burden those at risk who are not properly protected impose on taxpayers (Strobl 2011). To minimize households' under-protection, this paper suggests that federal post-disaster aid to repair damages should be limited to loans or very small grant amounts. Currently, 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.

Federal response will also need to address affordability issues and the distributional impacts of capping aid, issues we have not examined here. 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. 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). Concerns about the affordability of insurance have become amplified following the passage of reform legislation in July 2012 that will increase rates for some policyholders in the NFIP.

[18] 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.

While this paper provides a first empirical analysis of the disincentive to purchase flood insurance created by federal post-disaster aid, more research on this topic is clearly warranted. This analysis was limited by the inability to obtain parcel-level geographic information. Should federal agencies be willing to share that information, it could help identify the incentive effects more cleanly than data aggregated to higher spatial scales. It would also allow for the identification of heterogeneous responses to aid across different types of households. One related and still unanswered question is whether aid to local and state governments to rebuild public infrastructure creates a similar crowd-out effect or disincentive to invest in hazard mitigation. Finally, more empirical evidence on the incentive effects of federal aid for other disaster types, coupled with survey evidence on ex ante household expectations, would allow for a more comprehensive understanding of this issue. In light of our findings, we consider the likely consequences of a major federal disaster reform an important area for future research, in the US and abroad.

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Appendix A: First-Stage Regression

Table A1 shows the first-stage results of the IV regressions for Tables 2 and 3. For IA, we regress the first lag of average IA per grant on the first lag of an interaction term between a dummy indicating that a county was a swing county (which we define as counties for which the difference in votes for the Democrat and Republican was 5 percent or smaller in the last US Senate election) and a dummy indicating that year *t-1* was a Senate election year, as well as 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. For SBA, we regress the first lag of average SBA per loan on the first lag of an interaction term between a dummy indicating that a county had a strong majority (meaning that the winning party had 70 percent or more of the votes) in the last US Senate election and a dummy indicating that year *t-1* was a Senate election year, as well as the first lag of an interaction term between a dummy indicating that that county had a strong majority (again, meaning that the winning party had 70 percent or more of the votes) in the last presidential election and a dummy indicating that year *t-1* was a presidential election year.

All regressions also account for average claims in the zip code in the previous year (in $1,000s), as well as the premium per $1,000 coverage, the percentage of homes in the zip code that are owner occupied, the median income in the zip code (in $1000s), the total number of housing units, and year and zip code FEs. Column 1 in Table A1 contains the first-stage estimates for column 2 in Tables 2 and 3. Column 2 contains the first-stage estimates for column 4 in Tables 2 and 3. Column 3 contains the first-stage estimates for column 6 in Tables 2 and 3. Column 4 contains the first-stage estimates for column 7 in Tables 2 and 3. Columns 5 and 6 contain the first-stage estimates for column 8 in Tables 2 and 3.

The first-stage estimates reveal the following pattern. Being a swing county in the year prior to a Senate election is significant at the 1 percent level but, contrary to our expectations, yields a negative sign. The coefficients on the interaction terms for zip codes with a large majority are more consistent with the political science literature in that those areas that are "safe" for one party are not given extra IA grants. The interaction term of being in a county with a Democratic majority in the year prior to a presidential election has a positive and highly statistically significant effect on average IA payments. These results indicate that, in the year preceding a presidential election, the incumbent government might use federal relief as a form of vote-buying in counties with an opposition majority.

Given that we already control for year FEs, this result is not driven by the fact that the following year has a presidential election, in which the likelihood of presidential disaster declarations is generally higher (Garrett and Sobel 2003). The F-statistics for joint significance of all instruments in explaining the respective relief variable are above 20 for the IA and SBA dummy (not reported here), the Total SBA (column 1 in Table A1), and the Average IA (column 3). In these cases, we can be confident that their power is adequate to produce IV estimates with no or little bias. In the case of Total IA, the F-statistic is around 8.5, which is slightly below the suggested threshold of 10 (Staiger and Stock 1997). Therefore, the results using this definition of IA should be evaluated accordingly. In the case of average SBA per loan, the first-stage F-test suggests that the instruments are rather weak and the estimated coefficient might still be biased.

Table A2 presents the descriptive statistics of the IVs.

Table A3 presents the results of the Tobit and quasi-maximum likelihood estimate robustness checks.

TABLE A1—FIRST-STAGE ESTIMATES FOR IV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	IA	SBA	IA	SBA	IA	SBA	IA	SBA
Swing county Senate $_{ct-1}$ X	-0.278***		-0.309***	-0.680	-0.310***		-0.345***	-0.674
Senate election year $_{t-1}$	(0.069)		(0.069)	(0.786)	(0.071)		(0.071)	(0.788)
Democratic majority $_{ct-1}$ X	0.104***		0.120***	-2.069**	0.092**		0.109***	-2.068**
presidential election year $_{t-1}$	(0.038)		(0.039)	(0.837)	(0.039)		(0.039)	(0.839)
Strong majority Senate $_{ct-1}$ X		-2.888***	-0.129**	-2.022***		-2.912***	-0.147**	-2.042***
Senate election year $_{t-1}$		(0.604)	(0.063)	(0.513)		(0.599)	(0.065)	(0.503)
Strong majority Presidential $_{ct-1}$ X		2.081**				2.066**		
presidential election year $_{t-1}$		(0.812)				(0.812)		
N	8,315	8,425	8,315	8,315	8,315	8,425	8,315	8,315

Notes: Coefficients are reported. Robust standard errors are in parentheses. First-stage estimates. The respective dependent variable is regressed on an interaction term between a dummy indicating that a county was a swing county in the last 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 is a presidential election year are used as instruments for Average IA per grant$_{it-1}$, in columns 1, 3, 5, and 7. The first lag of an interaction term between a dummy indicating that a county had a strong majority in the last 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 that county had a strong majority in the last presidential election and a dummy indicating that year t-1 is a presidential election year, in columns 2, 4, 6, and 8. The first lag of an interaction term between a dummy indicating that a county was a swing county in the last Senate election and a dummy indicating that year t-1 was a Senate election year, 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 is a presidential election year, and the first lag of an interaction term between a dummy indicating that that county had a strong majority in the last presidential election and a dummy indicating that year t-1 is a presidential election year. All specifications include the following additional control variables: Premium per $1,000 coverage, Median income ($1,000s), Total housing units, and zip code and year FEs. Columns 1–4 also include Average claim (in $1,000s) $_{it-1}$, while columns 5–8 also include Total claims (in $1,000s) $_{it-1}$, columns 3 and 7 further include average SBA per loan in zip i and year t, while columns 4 and 8 further include average IA per grant in zip i and year t. Standard errors (in parentheses) are clustered at the zip code level. Full results are available upon request.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 level.

TABLE A2—DESCRIPTIVE STATISTICS, INSTRUMENTAL VARIABLES

Variable	Mean	Std. Dev.	Min.	Max.
Swing county Senate $_{ct}$ X Senate election year $_{t}$	0.048	0.215	0.000	1.000
Democratic majority $_{ct}$ X presidential election year	0.134	0.341	0.000	1.000
Strong majority Senate $_{ct}$ X Senate election year $_{t}$	0.099	0.299	0.000	1.000
Strong majority presidential $_{ct}$ X presidential election year $_{t}$	0.110	0.312	0.000	1.000

TABLE A3—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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