Estimation of an Equilibrium Model with Externalities: Post-Disaster Neighborhood Rebuilding*

Chao Fu and Jesse Gregory
University of Wisconsin and NBER

Abstract

We study the optimal design of subsidies in an equilibrium setting, where the decisions of individual recipients impose externalities on one another. We apply the model to the case of post-Katrina rebuilding in New Orleans under the Louisiana Road Home rebuilding grant program (RH). We estimate the structural model via indirect inference, exploiting a discontinuity in the formula for determining the size of grants, which helps isolate the causal effect of neighbors' rebuilding on one's own rebuilding choices. We find that the additional rebuilding induced by RH generated positive externalities equivalent to $4,950 to each inframarginal household whose rebuilding choice was not affected by the program. Counterfactual policy experiments find that optimal subsidy policies bias grant offers against relocation, with an inverse-U-shaped relationship between the degree of bias and the severity of damages from the disaster.

*We thank Zach Flynn and Chenyan Lu for excellent research assistance. We thank Steven Durlauf, John Kennan, Chris Taber, and Jim Walker for insightful discussions. We thank the editor and two anonymous referees for their suggestions. We have also benefited from helpful comments and suggestions by seminar participants at Berkeley, the Cleveland Fed, Depaul, Duke, Iowa, Michigan, Penn, Rochester, Stanford-GSB, UCLA, UIC, UW-Milwaukee, and Wash U. All errors are ours.

1 Introduction

Individuals' choices are sometimes inevitably and endogenously inter-related due to spillover effects from one's choices onto others' payoffs. These spillovers are often not accounted for when individuals make their decisions, which may lead to inefficient equilibrium outcomes and hence leave space for policy interventions.[1] Effective policy designs require the capability of predicting and comparing the impacts of alternative counterfactual policies, which in turn relies on two essential pieces of information: 1) the nature of the spillover effects, which can be difficult to identify,[2] and 2) how decisions are made in equilibrium and how equilibrium outcomes differ across counterfactual policy environments. In this paper, we develop a unified framework to obtain both pieces of information.

To place our framework in a concrete setting, we study the rebuilding of neighborhoods affected by Hurricane Katrina under the Louisiana Road Home program (RH). RH offered rebuilding grant packages and less generous relocation grant packages to all Katrina-affected homeowners in the state with uninsured losses. The RH grant formula yielded significantly larger grant offers when an index measuring home damages fell above a particular threshold. As a result, otherwise similar households with index values just above/below this threshold faced very different financial incentives to rebuild. Using a regression discontinuity design (RDD) on the administrative micro-level data, we find robust evidence of non-linear spillover effects among neighbors' rebuilding decisions. Households just above the threshold where the incentive to rebuild jumps discontinuously were 5.0 percentage points more likely to rebuild than otherwise similar households just below that threshold. Neighbors of these households, whose financial incentives were not directly affected, were 2.4 percentage points more likely to rebuild, suggesting the existence of sizable spillover effects. Moreover, the size of spillovers varied significantly across neighborhoods, suggesting that spillover effects are likely to be non-linear.

To achieve the goal of studying the effectiveness of alternative (counterfactual) policy designs, one needs to go beyond RDD analyses and to understand the fundamental factors driving equilibrium outcomes. We develop an equilibrium model of neighbors' post-disaster rebuilding choices with amenity spillovers. Households have private preferences for consumption and for residing in their home. They also derive utility from a neighborhood amenity

[1] For instance, negative spillovers from home foreclosures are a commonly cited motivation for subsidized mortgage modifications (Cambell, Giglio, Pathak, 2010). Arguments against rent controls often cite the possibility that undermaintained properties reduce the value of nearby non-controlled properties (Autor, Palmer, and Pathak, 2012).

[2] Manski (1993) raises the reflection problem. Brock and Durlauf (2007) show that point identification of social interactions can fail even when there is no reflection problem in settings where important group-level variables are not observed by the researcher.

that depends on the fraction of neighbors who rebuild, an externality that is not internalized by individual households. In each period, households who have not yet rebuilt or sold their houses have the option to rebuild, sell or wait. Households' decisions are inter-related because of amenity spillovers. An equilibrium requires that individuals' decisions be best responses to each other. Given the RDD evidence of non-linear spillover effects, we embed in our structural model a flexible amenity spillover function. The identification of our model with such a flexible spillover function is achieved via indirect inference that fully exploits the discontinuity in the RH grant formula.

The estimated model reveals important policy implications arising from amenity spillovers. RH's full equilibrium impact on the city-wide rebuilding rate, including "feedback" effects from positive amenity spillovers, was 27% larger than the impact generated by the program's financial incentives alone (holding amenities fixed). Like many other disaster relief packages, RH provided a higher financial incentive to rebuild than to relocate. Although the conditional nature of the program created excess burden by distorting privately optimal resettlement choices, the spillover effects were strong enough such that the net average household welfare was \$2,177 higher under RH than it would have been had households been offered the same grant regardless of whether they chose to rebuild or to relocate.

Our framework is well-suited for exploring a wide range of policy interventions with various goals and/or constraints. For illustration, we examine the possibility of further improving welfare by studying a particular group of conditional grant policies, which offer a fraction $(1-\rho)$ of the RH rebuilding grant to households if they choose to relocate. We search for the optimal ρ's given different constraints. Compared to the case under the unconditional grant policy, net average household welfare would improve by \$2,638 if ρ's are restricted to be the same for all households, by \$3,613 if ρ's can differ by flooding severity, and by over \$6,000 if ρ's can be block-specific. The relationship is inversed-U-shaped between the optimal penalties against relocation (ρ) and the severity of damages from the disaster, with greater biases against relocation for areas with moderate damages.

Although our empirical application focuses on a special event, our equilibrium modeling framework can be applied/extended to other cases where individual decisions inter-relate due to spillover effects. Our findings highlight the fact that, when externalities potentially exist, accounting for equilibrium interactions among individuals and quantifying the externality is essential for the design of policies. To shed light on policy designs with relatively less restrictive modeling assumptions for identification, our paper combines the strengths of two strands of the literature on spillover effects, one relying on quasi-experiments and the other on structural models.

Consistent with our estimates, reduced-form analyses in the first strand of literature have found evidence that policies stimulating investment in housing boost the value of nearby homes not directly affected by the policies (Autor, Palmer, and Pathak 2012; Rossi-Hansberg, Sarte, and Owens 2010), and that negative spillover effects of foreclosures are larger for more proximate homes (Campbell, Giglio, and Pathak 2011; Harding, Rosenblatt, and Yao 2009). In the second strand of literature, de Paula (2009) is the closest to our work.[3] He studies inference in a continuous time model where an agent's payoff to quit an activity depends on the participation of other players. Ours is a discrete time model where neighbors' choices of the timing of rebuilding are inter-related. Our paper embeds variation from a quasi-experiment in our structural model to estimate the shape and strength of social spillovers.[4]

Although experimentally-generated variation in incentives is not always available, different and more general identification strategies than the one used in this paper are available, which typically require more structure on, for example, the selection into groups/neighborhoods.[5] For example, Brock and Durlauf (2006) and Brock and Durlauf (2007) provide methods for identifying social interactions in discrete choice models with endogenous group formation. Brock and Durlauf (2007) demonstrate partial identification of social interactions with unobserved neighborhood-level covariates. Bayer and Timmins (2007) propose an instrument for peers' behavior that is based on exogenous location characteristics and motivated by a formal location choice model to identify spillovers.

Our paper is also related to the literature studying the post-Hurricane-Katrina locations, labor market outcomes, and wellbeing of displaced New Orleans residents.[6] Most closely related to our paper, Gregory (2014) estimates a structural individual decision model of New Orleans homeowners' resettlement choices. Gregory (2014) uses the estimated model to study the trade-off of post-disaster bailouts between their short run insurance benefits and the long run efficiency losses caused by expected future bailouts distorting households' location choices (moral hazards). Instead of treating each household in isolation, our paper emphasizes the possible spillover effects from individual households' choices and the inter-related nature of households' choices in an equilibrium context. We study the optimal design of conditional subsidies that internalize spillover effects and improve household welfare in

[3]Other recent examples of equilibrium model-based approaches to studying housing and/or location choices include; Epple and Sieg (1999); Epple, Romer, and Sieg (2001); Ioannides (2003); Bayer, McMillan, and Reuben (2005); Bayer, Ferreira, and McMillan (2007); Bayer and Timmins (2007); and Ioannides and Zabel (2008).

[4]Galiani, Murphy, and Pantano (2012) use the experimentally randomized variation in neighborhood-specific financial incentives from the Moving to Opportunity (MTO) demonstration to identify the structural parameters of an individual neighborhood choice model (without social interactions).

[5]See Blume, Brock, Durlauf, and Ioannides (2010) for a comprehensive review of the literature on the identification of social interaction effects.

[6]For example, Groen and Polivka, 2010; Zissimopolous and Karoly, 2010; Vigdor, 2007 and 2008; Paxson and Rouse, 2008; and Elliott and Pais, 2006.

equilibrium.

The rest of the paper is organized as follows: Section 2 provides additional policy background. Section 3 describes our dataset and RDD results. Section 4 describes the structural equilibrium model. Section 5 explains our estimation. Section 6 presents the estimation results. Section 7 presents our counterfactual experiments, and Section 8 concludes. Additional details are provided in the appendix.

2 Background Information

Hurricane Katrina struck the U.S. Gulf Coast on August 29, 2005. The storm and subsequent flooding left two thirds of the city's housing stock uninhabitable without extensive repairs, the costs of which significantly exceeded insurance payouts for many pre-Katrina homeowners in New Orleans. Among the nearly 460,000 displaced residents, many spent a considerable amount of time away from the city or never returned. Congress approved supplemental relief block grants to the Katrina-affected states. Possible uses of these grants were hotly debated, with proposals ranging from mandated buyouts to universally subsidized reconstruction. The state of the Louisiana used its federal allocation to create the Louisiana Road Home program, which provided cash grants for rebuilding or relocating to pre-Katrina Louisiana homeowners with uninsured damages.[7]

A participating household could accept its RH grant as a rebuilding grant or as a relocation grant. Subject to an upper limit of \$150,000, both grant types provided compensation equal to the "value of home damages" minus the value of any insurance payouts already received. The RH grant formula yielded significantly larger grant offers when an index measuring home damages fell above a particular threshold. There were several important differences between rebuilding and relocation grants. While both provided the same cash payout,[8] relocation grant recipients were required to turn their properties over to a state

[7]Other policies targeted to the Gulf Coast in the aftermath of Hurricane Katrina included Federal Emergency Management Agency (FEMA) small assistance grants in the hurricane's immediate aftermath and Gulf Opportunity Zone subsidies to firms for capital reinvestments and the hiring and retention of displaced workers. The program other than RH that most directly impacted homeowners' ability to rebuild was the Small Business Administration (SBA) Disaster Loan program, which provided loans to homeowners with uninsured damages who met certain credit standards. The SBA Disaster Loan program is a standing program that, despite being federally subsidized, has non-trivial credit standards, and the program rejected a large majority of applicants from the Gulf Coast in the aftermath of Katrina (Eaton and Nixon, 2005). For that reason, we allow for the possibility of credit constraints in our equilibrium model.

[8]The cash grants for relocating and for rebuilding were the same except for one particular circumstance. All RH grants were initially capped at the pre-Katrina value of a household's home. For households classified as "low or moderate income," this cap was waved for rebuilding grants (in response to the argument that the provision had disparate impacts by race, because identical homes had different market values in predominantly black versus white neighborhoods) but not for relocation grants.

land trust. For households with partial home damages, this stipulation introduced a sizable opportunity cost to relocating. On the other hand, rebuilding grant recipients were only required to sign covenant agreements to use their grants for rebuilding and to not sell their homes for at least three years. We provide additional details in Section 4.2 on the incentive effects of these program rules and differences in these incentives on either side of the grant formula discontinuity.

Grant recipients often experienced lengthy delays between initiating their grant applications and receiving a grant. RH was announced in February, 2006, but the median grant payment date occurred after Katrina's second anniversary in 2007, which is captured in our model. Despite the program's slow rollout, RH had disbursed nearly ten billion dollars to Louisiana homeowners by Katrina's fifth anniversary.

3 Data, Policy Details, and RDD Analyses

3.1 Data

The main data for our analysis are the administrative property records of the Orleans Parish Assessor's Office (Assessor's property data) and the administrative program records of the Louisiana Road Home grant program (RH data). The Assessor's property data provide information on the timing of home repairs and home sales for the full universe of New Orleans properties. For each property, the data provide annual appraised land and structure values for 2004-2010, which we use to infer the timing of home repairs, and the date and transaction price of all post-Katrina home sales.

The RH data provide detailed information on the grant amount offered to each applicant household and whether the applicant chose a rebuilding grant (which required the household to rebuild and not to sell for at least three years), a relocation grant (which required the household to turn its property over to a state land trust with no additional compensation for any as-is value of the property), or chose not to participate. The data also include all of the inputs to the RH grant offer formula; including a repair cost appraisal and a replacement cost appraisal for each home, and the total value of private insurance payments paid to each household. Together with the RH grant formula, such information enables us to compute both types of RH grants for each household regardless of its actual choice.

We merge the RH data and the Assessor's property data at the property level by street address. We also obtain measures of the depth of flooding on each Census block from a FEMA-provided data set created from satellite images, and the demographic composition of each Census block from the 2000 Census. Because our focus is on spillover effects from homeowners' rebuilding choices, we exclude homes that were renter-occupied when Katrina occurred and Census blocks that contained fewer than five owner-occupied homes. The resulting dataset contains 60,175 households living in 4,795 blocks.

Solving our model requires a measure of the wages available to each household in and away from New Orleans $\{w_{it}^1, w_{it}^0\}_t$. We impute these variables with a two step procedure that combines data from the Displaced New Orleans Residents Survey (DNORS)[9] on the distribution of earnings and occupations in New Orleans during the year prior to Katrina and data from the American Community Survey (ACS) on occupation-specific trends in prevailing wages across labor markets from 2005-2010. The first step uses nearest Mahalanobis distance matching to assign each household a "donor" DNORS record. The second step imputes Post-Katrina wage offers by adjusting the household head's and spouse's pre-Katrina annual earnings by an occupation-MSA-specific wage index estimated with ACS data (see details in online Appendix II). The imputed wage measures capture the fact that the incentive to return to New Orleans varied across households of different occupations (e.g. construction wages increased and personal service wages fell post-Katrina). Because of the extent of imputation in these variables, we do not exploit variation in labor market incentives for identification.

Lastly, we use data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax to obtain information on neighborhood-level credit conditions. These data cannot be merged at the household level to our other data sources. Instead, we compute the average Equifax Risk Score (TM) within 1/4 mile of each block's centroid, and assign each household a simulated credit score $risk_i \sim N(\overline{risk}_{buf(i)}, 85)$, where $\overline{risk}_{buf(i)}$ is the average risk-score calculated for household i's block and 85 is the within-block standard deviation of risk scores.[10]

3.1.1 Summary Statistics

Table 1 presents descriptive statistics for our sample of homeowning households (Column 1) and the subsample of households whose houses were damaged and left unlivable by Katrina (Column 2). Forty-six percent of households lived in areas that received less than 2 feet of flooding, while over 20% of households were from areas that received over 5 feet of flooding.

[9]Fielded by RAND in 2009 and 2010, the Displaced New Orleans Residents Survey located and interviewed a population-representative 1% sample of the population who had been living in New Orleans just prior to Hurricane Katrina.

[10]It would be ideal to allow credit scores to vary systematically by household characteristics within a block. Our cruder approach to modeling credit availability is driven by a data limitation, namely that we observe a "spatial moving average" of credit scores but not microdata. Given the high degree of both racial and economic segregation in New Orleans, however, we do not expect that conditioning credit score draws on additional observables within neighborhoods would change our results in a meaningful way.

Not surprisingly, households with damaged houses were disproportionately more likely to have lived in areas that were heavily flooded. Households with damaged houses were more likely to be black, without college education, and with lower credit scores. For an average damaged house, insurance covered only 52% of repair costs. Over 60% of households with damages participated in the RH program and a vast majority of them chose the rebuilding grant as opposed to the relocation grant. Thirteen percent of these households rebuilt their houses within 1 year of Katrina; by the fifth anniversary of Katrina, this fraction rose to 54%. Table A1 in the appendix shows that the same correlation between damages and demographics holds at the block level.

3.2 The RH Grant Discontinuity and Post-Katrina Rebuilding

Subject to an upper limit of $150,000, RH provided grant compensation to households equal to the "value of their home damages" minus the value of any insurance payouts already received. Home damages were valued at the cost of component-by-component repairs in cases where the estimated repair cost was 51% or less of the home's estimated full replacement cost, and at the full replacement cost otherwise, i.e.,

$$\text{RH Grant} = \begin{cases} \min\Big(\quad [\mathit{RepairCost}] \quad - [\text{Insurance Payout}] \ ; \ \$150\text{k}\Big) & \text{if } \dfrac{[\mathit{RepairCost}]}{[\text{Replacement Cost}]} < 51\% \\ \min\Big([\text{Replacement Cost}] - [\text{Insurance Payout}] \ ; \ \$150\text{k}\Big) & \text{if } \underbrace{\dfrac{[\mathit{RepairCost}]}{[\text{Replacement Cost}]}}_{\text{Damage Fraction}} \geq 51\% \end{cases} .$$

Assuming households could not perfectly control their appraised damage fractions, variation in grant offers very close to the 51% damage threshold can be thought of as approximately random and thus orthogonal to the sorts of unmeasured neighborhood-level variables that can confound the identification of social spillovers in purely observational settings. This policy cutoff approximates an experiment in which the private incentives of some households were experimentally manipulated without directly changing the incentives of their neighbors. Assuming households responded to private incentives, spillover effects are identified by differences between the rebuilding patterns of neighbors of households with just above versus just below 51% damage.

Figure 1 shows that the policy discontinuity did in fact discontinuously affect households' private incentives to rebuild and in turn their private rebuilding choices. The left panel of Figure 1 plots the average opportunity cost of declining a RH rebuilding grant within damage-fraction bins.[11]

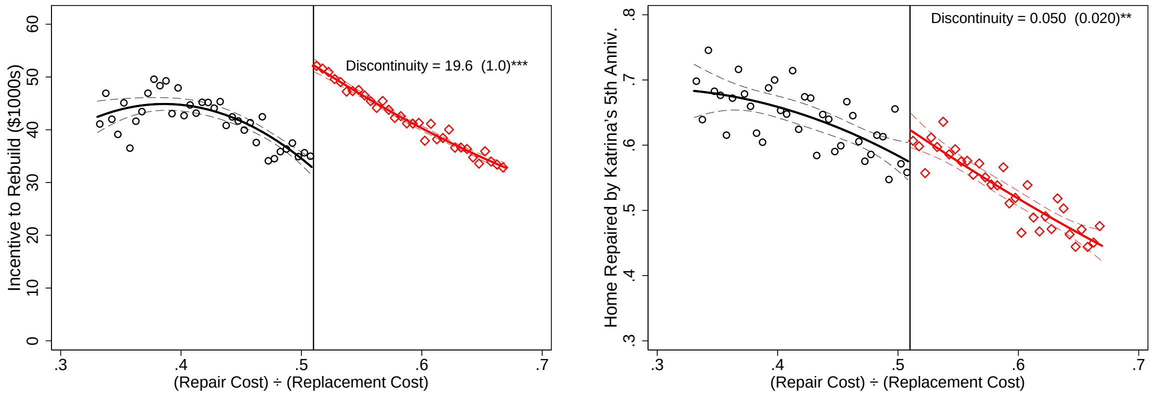
[11]For each household, the opportunity cost is defined as the smaller of the as-is value of the household's

Table 1: Descriptive Statistics, Households

Variable	All HHs	HHs with initially damaged homes
Demographic composition:		
Percent black (Census block)	57	65
Percent college educated (Census tract)	51	49
Pre-Katrina block flood exposure:		
< 2 feet	46	23
2 - 3 feet	12	16
3 - 4 feet	11	16
4 - 5 feet	10	15
5 - 6 feet	6	9
> 6 feet	15	21
Equifax risk score (spatial moving average):		
<600	20	21
600-625	17	18
625-650	17	18
650-675	14	14
675-700	12	9
700-725	10	10
>725	11	9
Home damage and insurance:		
Damage fraction (repair cost ÷ replacement cost)	.39 (sd=.32)	.58 (sd=.21)
Insurance fraction (insurance ÷ replacement cost)	.23 (sd=.21)	.30 (sd=.22)
Importance of Road Home grant formula discontinuity:		
Damage fraction within 2 pct. pts. of RD threshold	4.4	6.6
Road Home participation:		
Nonparticipant	49	36
Rebuilding grant (option 1)	44	55
Relocation grant (option 2 or 3)	6	9
Home repaired by the pre-Katrina owner by year:		
Immediately after Katrina	33	0
1 year after Katrina	42	13
2 years after Katrina	47	21
3 years after Katrina	52	29
4 years after Katrina	65	47
5 years after Katrina	70	54
Observations:	60,175	40,291

Note: This table reports summary statistics at the household level for the dataset analyzed in this paper. The sample includes all homes that were owner occupied in 2005, and located in Census blocks that contained at least five owner occupied homes in 2005.

Figure 1: Households' Financial Incentives and Rebuilding Choices by Appraised Home Damage Fraction



(a) (b)

Note: The left panel of this figure shows the average opportunity cost of relocating instead of rebuilding within narrow home-damage-fraction bins. The opportunity cost of relocating instead of rebuilding was the smaller of a household's RH rebuilding grant offer (which the household passed up if it sold its home privately) and its home's as-is value (which the household had to turn over to the state if it accepted a RH relocation grant). The right panel shows the average rebuilding rate 5 years after Katrina within narrow home-damage-fraction bins.

$$Cost_i = \overline{cost} \; + \; \mathbf{\Delta}^{(cost)} \times 1_{R_i > 0} \; + \; h(R_i; a^{(cost)}) \; + \; e_i, \tag{1}$$

where R_i is Household i's damage fraction minus .51, and $h(.)$ is a continuous function. Throughout the paper, the function $h(.)$ takes the form,

$$h(R_i; a) = a_1 R_i \; + \; a_2 R_i^2 \; + \; a_3 R_i \times 1_{R_i > 0} \; + \; a_4 R_i^2 \times 1_{R_i > 0} \tag{2}$$

That is, we use a second-order polynomial that allows for different patterns when the running variable is above the RH threshold.

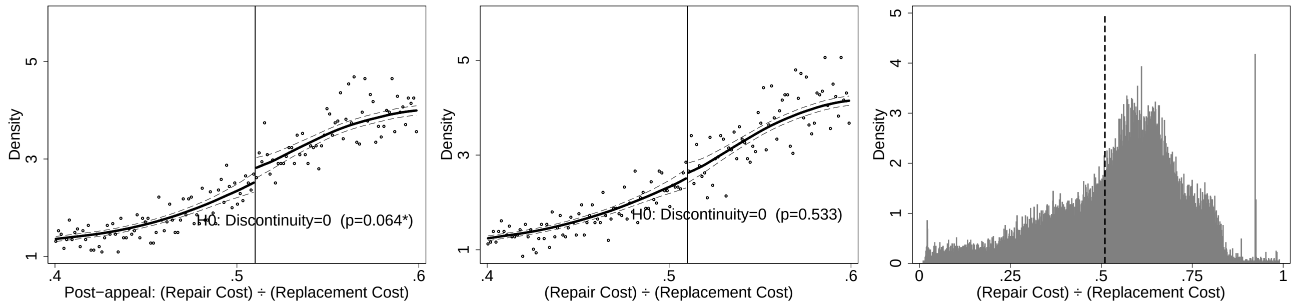
The right panel of Figure 1 plots the rebuilding rate as of Katrina's fifth anniversary within damage-fraction bins,

$$\underbrace{Y_i}_{\text{Repair Dummy}} = \overline{y} \; + \Delta^{(y)} \times 1_{R_i > 0} \; + \; h(R_i; a^{(y)}) \; + \; e_i. \tag{3}$$

On average, the opportunity cost of relocating increased by \$19.6k at the 51% damage

damaged property (the opportunity cost of choosing a RH relocation instead of rebuilding grant) and the size of the household's RH grant offer (the opportunity cost of selling privately). Among households with damaged homes, 6.6% have a damage fraction within two percentage points of the 51% discontinuity, and 45% of blocks contain at least one such household.

Figure 2: Distribution of Appraised Home Damage Fractions



(a) (b) (c)

Note: Panel (a) of this figure plots the density of RH-appraised home damage fractions (repair cost ÷ replacement cost) close to the 51% RH grant threshold once all appeals of initial appraisals had been adjudicated. Panel (b) plots the density of *initial* RH-appraised home damage fractions close to the 51% grant-offer threshold. Panel (c) shows the full distribution of RH-appraised damage fractions.

threshold, and the probability of rebuilding increases by 5.0 percentage points.[12]

3.2.1 Validity Tests

This quasi-experiment is only credible if households were unable to perfectly control the value of their "damage fraction" running variable relative to the 51% damage threshold. Panels (a) and (b) of Figure 2 compute McCrary tests for continuity in the density of damage fractions at 51% based on two different definitions of the damage fraction variable. The damage fraction in panel (a) is based on households' *final* damage appraisals, incorporating the adjudicated decisions on all household appeals of initial damage appraisals, and exhibits a somewhat larger density just above 51% than just below 51% (p=.064). The damage fraction in panel (b) is based on households' *initial* damage appraisals. A McCrary test applied to these "first-appraisal" damage fractions fails to reject continuity at the 51% threshold (p=0.533). We therefore treat the first-appraisal damage fraction as the running variable in all substantive analyses. Panel (c) confirms that a non-trivial portion of the overall damage-fraction distribution falls near the 51% threshold.

Table 2 assesses the balance of pre-determined covariates above and below the 51% threshold. Columns (1) and (2) report each variable's mean among households with just below and just above 51% damage. Column (3) reports the p-value of the null that the two are equal.[13] These tests fail to reject the null of balance for any covariates; including the fraction of same-block neighbors with undamaged homes, block-level demographics and

[12]Appendix Table A2 shows that these results are robust to alternative specifications, including local linear regression using an optimal bandwidth (Calononico, Catteneo, and Titiunik 2014).

[13]We restrict the sample to households with a damage fraction between 0.33 and 0.67, and for each variable

the depth of flooding. The table also compares the probability of each covariate exceeding its unconditional 10th, 25th, 50th, 75th, and 90th percentiles above/below the 51% damage threshold, and again fails to reject balance in each case. Similarly, the predicted probability that a household rebuilds within 5 years of Katrina from a probit regression with all of these block characteristics included as explanatory variables (a propensity score) exhibits no jump at the 51% damage threshold. Finally, and crucially, we find no evidence that same-block neighbors damage fractions are functions of one another, which would invalidate the RD design as a framework for studying spillovers. Specifically, we fail to reject the null that the fraction of same-block neighbors with damage above 51% is the same for households whose own damage fraction is just above 51% and households whose own damage fraction is just below 51%.

Remark 1 *One concern ex ante was clustering of damage severity at the block level, for example, inspectors might have sometimes assigned identical damage estimates within blocks, causing groups of neighbors to be simultaneously affected by the discontinuous change in incentives at the grant formula discontinuity. Our balance estimates find no evidence of such a phenomenon.*

3.2.2 Data Evidence

Given the validity of the grant formula RDD, we exploit the quasi-experiment to examine the impact of RH financial incentives on households' private rebuilding choices and spillover effects from one's rebuilding onto neighbors' choices. One natural question is how two elasticities can be identified by studying the impact of one policy shock. Crucially for our analysis, the nature of the policy's "treatment" varies across households. Households with a running variable far from the 51% threshold (i.e., far from being shocked with a jump in the private financial incentive to rebuild) are not *directly* affected by the quasi-experiment, so the response by those types of households when directly-affected neighbors change their rebuilding choices identifies the spillover elasticity. The choices of directly-affected households with a running variable close to the 51% threshold identify the elasticity of rebuilding choices with respect to private financial incentives.

Z_i we estimate a flexible regression of the form,

$$Z_i = \overline{z} \ + \ \boldsymbol{\Delta}^{(z)} \times 1_{R_i > 0} \ + \ h(R_i; a^{(z)}) \ + \ e_i$$

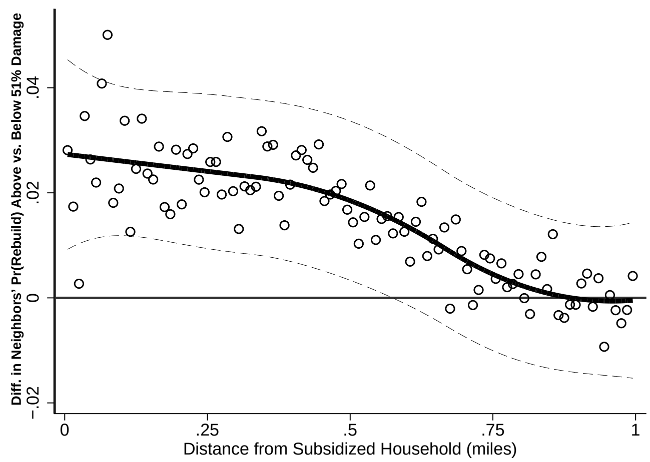
Columns (1) and (2) of Table 2 report the left limit ($\overline{z}$) and right limit ($\overline{z}+\boldsymbol{\Delta}^{(z)}$) of each variable's conditional expectation as the damage fraction goes to .51. Column (3) reports the p-value associated with null that $\boldsymbol{\Delta}^{(z)} = 0$.

Table 2: Balance of Predetermined Covariates Above and Below the 51% Home Damage

	limit as (repair cost) ÷ (replacement cost) ↗ 51% (1)	limit as (repair cost) ÷ (replacement cost) ↘ 51% (2)	p-value of difference between (1) and (2) (3)
Fraction of homes undamaged (Census block):	0.048 (0.004)	0.046 (0.004)	0.698
Fraction black (Census block):	0.713 (0.011)	0.717 (0.01)	0.768
Fraction college (Census block group)			
Fraction college	0.474 (0.005)	0.480 (0.005)	0.342
Fraction college < 10th city-wide pctile	0.088 (0.009)	0.098 (0.008)	0.373
Fraction college < 25th city-wide pctile	0.215 (0.012)	0.213 (0.011)	0.910
Fraction college < 50th city-wide pctile	0.491 (0.015)	0.484 (0.013)	0.729
Fraction college < 75th city-wide pctile	0.845 (0.013)	0.816 (0.012)	0.094
Fraction college < 90th city-wide pctile	0.943 (0.009)	0.946 (0.008)	0.778
Poverty rate (Census tract):			
Poverty rate	0.198 (0.003)	0.200 (0.003)	0.774
Poverty < 10th city-wide pctile	0.052 (0.009)	0.054 (0.008)	0.875
Poverty < 25th city-wide pctile	0.194 (0.013)	0.194 (0.011)	0.979
Poverty < 50th city-wide pctile	0.522 (0.015)	0.523 (0.014)	0.974
Poverty < 75th city-wide pctile	0.788 (0.012)	0.790 (0.011)	0.916
Poverty < 90th city-wide pctile	0.924 (0.009)	0.909 (0.008)	0.192
Equifax risk score (neighborhood s.m.a.):			
Average risk score	636.7 (1.4)	638.4 (1.4)	0.425
Average risk score < 10th city-wide pctile	0.103 (0.009)	0.119 (0.008)	0.177
Average risk score < 25th city-wide pctile	0.260 (0.013)	0.260 (0.012)	0.992
Average risk score < 50th city-wide pctile	0.567 (0.015)	0.535 (0.013)	0.116
Average risk score < 75th city-wide pctile	0.830 (0.013)	0.831 (0.011)	0.929
Average risk score < 90th city-wide pctile	0.958 (0.009)	0.949 (0.008)	0.462
Flooding (Census tract):			
Flood depth	3.14 (0.06)	3.17 (0.05)	0.753
Flooding < 2 feet	0.293 (0.012)	0.288 (0.011)	0.772
Flooding 2-4 feet	0.409 (0.014)	0.411 (0.013)	0.910
Flooding 4-6 feet	0.222 (0.012)	0.229 (0.011)	0.676
Flooding > 6 feet	0.077 (0.010)	0.072 (0.009)	0.729
Propensity score: pr(rebuild by t=5 | Z_j)	0.576 (0.003)	0.580 (0.003)	0.449
Same-block neighbors' circumstances:			
Avg. neighbors' damage fraction	0.535 (0.004)	0.528 (0.003)	0.180
Frac. of neighbors with >51% damage	0.624 (0.008)	0.616 (0.007)	0.451

Note: Columns (1) and (2) report the average values of background variables among households with appraised home damage fractions (repair cost ÷ replacement cost) just above 51% versus just below 51%, the threshold at which RH grant offers increased discontinuously. Column (3) reports the p-value associated with the null that the two are equal.

Figure 3: Difference Above vs. Below 51% Home Damage in the Rebuilding Rate of Close-by Neighbors



Note: This figure shows the difference between the rebuilding rates of neighbors of households with just above versus just below 51% home damage (repair cost ÷ replacement cost) by distance from the home. Specifically the figure plots the estimated values of $\Delta^{(d)}$ from Equation (4) for $d = 0, ..., 1$.

We first measure the spatial scope of spillovers by estimating regressions of the form

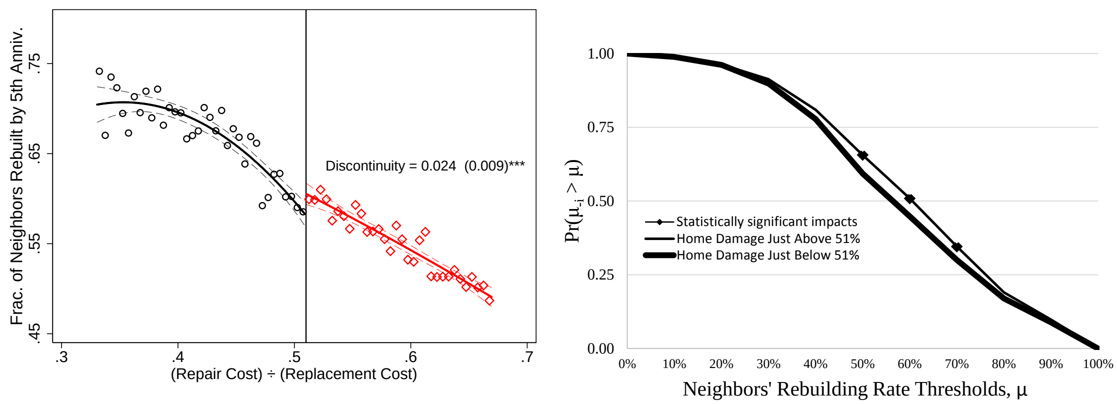
$$\mu_i^{(d)} = \mu \ + \ \mathbf{\Delta}^{(d)} \times 1_{R_i > 0} \ + \ h(R_i; a^{(d)}) \ + \ e_i \tag{4}$$

where $\mu_i^{(d)}$ is the repair rate of homes located between d and $d+.01$ miles from household i, and $\mathbf{\Delta}^{(d)}$ captures the difference between the rebuilding rate d miles from households with just above 51% damage and just below 51% damage. Figure 3 plots the estimated values of $\mathbf{\Delta}^{(d)}$ for $d = 0$ to 1 miles. While the rebuilding rate of the directly subsidized households increased by 5.0 percentage points at the 51% damage threshold, the rebuilding rate of immediate neighbors increased by about 2.5 percentage points. That spillover effect was roughly constant with distance up to 1/3 of a mile from directly subsidized households before decaying to zero beyond that.[14] In New Orleans, the standard Census geographic unit that best corresponds to this spatial extent of spillovers is the Census block, which leads us to treat a Census block as an economy in our model.[15]

[14]The impacts reported in Figure 3 are all relative to a baseline rebuilding rate of about 60%. There was a 59.5% average rebuilding rate among the immediate neighbors of households with $R_i \in (.48, .51)$. There was a 60.5% average rebuilding rate among neighbors about one mile from households with $R_i \in (.48, .51)$. These "baseline" rebuilding rates increased monotonically with distance from the household directly affected by the RDD experiment.

[15]Our point estimates suggest that household i 's rebuilding had smaller spillovers onto households living in *different* Census blocks than household i's block, given the same distance. With sufficient data, we could verify whether or not spillovers are literally zero for homes in different Census blocks. However, the sample of homes very close to block boundaries is too small for us to reject either zero spillovers across block boundaries or identical spillovers across block boundaries.

Figure 4: Difference Above vs. Below 51% Home Damage in the Distribution of Same-Block-Neighbor Rebuilding Rates



(a) Neighbors' Rebuilding Rate by Home Damage Fraction

(b) Neighbors' Rebuilding Rate CDF Above/Below Grant Threshold

Note: The left panel of this figure plots the average rebuilding rate of households' same-Census-block neighbors within narrow home-damage-fraction (repair cost ÷ replacement cost) bins. The right panel shows the CDF of same-block-neighbor rebuilding rates for households with just above and just below 51% home damage. See the discussion of Equation (6) in the text for details about the estimation procedure.

We next present estimates of the spillover effects of a larger private grant offer on the average rebuilding rate and on the *distribution* of rebuilding rates of neighbors in the same Census block.

$$\mu_{j(i),-i} = \overline{\mu} + \overline{\boldsymbol{\Delta}} \times 1_{R_i > 0} + h(R_i; a^{(\mu)}) + e_i \quad (5)$$

$$\begin{aligned} 1(\mu_{j(i),-i} > .1) &= \mathbf{S^{(10)}} + \boldsymbol{\Delta}^{\mathbf{(10)}} \times 1_{R_i > 0} + h(R_i; a^{(10)}) + e_i \\ 1(\mu_{j(i),-i} > .2) &= \mathbf{S^{(20)}} + \boldsymbol{\Delta}^{\mathbf{(20)}} \times 1_{R_i > 0} + h(R_i; a^{(20)}) + e_i \\ &\vdots \\ 1(\mu_{j(i),-i} > .9) &= \mathbf{S^{(90)}} + \boldsymbol{\Delta}^{\mathbf{(90)}} \times 1_{R_i > 0} + h(R_i; a^{(90)}) + e_i \end{aligned} \quad (6)$$

where $j(i)$ denotes household i's census block, and $\mu_{j(i),-i}$ denotes the rebuilding rate of i's same block neighbors (excluding i). Because the running variable R_i is normalized to be zero at a damage fraction of 51%, the parameter $\overline{\mu}$ recovers the rebuilding rate of the neighbors of those with just below 51% damage, and $S^{(10)}$, $S^{(20)}$,..., $S^{(90)}$ recover the probability that the neighbors of those with damage just below 51% rebuild above rates of 10%, 20%,...,90%. Similarly, the parameter $\overline{\Delta}$ recovers the difference above versus below 51% damage in neighbors' rebuilding rate, and $\Delta^{(10)}$, $\Delta^{(20)}$,..., $\Delta^{(90)}$ recover differences above versus below 51% damage in the probability that the neighbors rebuild at rates above 10%, 20%,...,90%.

Figure 4 summarizes these results. The top panel plots the rebuilding rate of households' same-block neighbors within narrow damage fraction bins, which jumps by 2.4 percentage at the 51% damage grant threshold. The bottom panel plots the neighbors' rebuilding rate "survival" functions for households with just below 51% damage (constructed from the estimates of $S^{(10)}$,..., $S^{(90)}$) and just above 51% damage ($S^{(10)} + \Delta^{(10)}$,..., $S^{(90)} + \Delta^{(90)}$). The relatively steep slope over a wide range of rebuilding rates implies that the grant discontinuity quasi-experiment occurred on blocks with a wide range of "baseline" rebuilding rates. A comparison of the plots for households with above and below 51% damage reveals that spillover effects operated primarily by pushing some blocks that would have experienced rebuilding below the rates of 50%, 60%, and 70% to above these rates. This pattern suggests that an exogenous shock to rebuilding has a large effect on amenity values in areas with baseline rebuilding rates near this range and a relatively small effect on amenity values in areas with very low baseline rebuilding rates. Appendix Table A2 shows robustness to alternative specifications.

3.3 From RDD to a Model

To achieve the main goals of this paper, we need to go beyond RDD and build a model. The first goal is to evaluate the welfare impact of the RH program, which requires the ability to infer quantitatively households' preferences from their observed choices. In particular, to evaluate RH's choice-based subsidy structure, we need to compare the gains from the amenity spillover relative to the losses for marginal households whose choices were distorted by the subsidies. The second and more important goal is to provide information for future policy designs, which involves comparing equilibrium impacts of various counterfactual policies. Prediction of these impacts requires a solid understanding of households' choices and the interaction among households, which, in turn, requires knowledge of the fundamental factors underlying the observed outcomes. In addition, counterfactual policy analyses, being out-of-sample predictions, involve extrapolations that call for a structural model.

RDD analyses provide two clear messages that are instrumental for us to make some of the key modeling choices. The first message is that spillovers significantly exist in the data, which suggests that a model ignoring spillovers might have misleading policy implications. Therefore, although it involves more modeling, an equilibrium approach, rather than a single agent decision framework, is necessary. The second message is that spillovers are likely to be highly nonlinear. Based on these findings, we build an equilibrium model of neighbors' choices in the presence of amenity spillovers, and allow for a very flexible specification of the spillover function.

4 Model

Displaced households (homeowners) make dynamic decisions about moving back to (and rebuilding) their pre-Katrina homes. In every period, a household that has not moved back or sold its property can choose to 1) move back and rebuild, or 2) sell the property, or 3) wait until the next period. Each household's decision potentially influences the block's attractiveness, a spillover effect that is not internalized by individual households. The model incorporates the following factors that influence a household's net payoff from rebuilding: (i) the cost of home repairs relative to other non-repair options, (ii) household's labor market opportunities in and out of New Orleans, (iii) the strength of the household's idiosyncratic attachment to the neighborhood, (iv) the exogenous state of the neighborhood (e.g., flood damages, infrastructure repairs and unobserved amenities), and (v) the influence of neighbors' rebuilding choices on the attractiveness of the neighborhood.

4.1 Primitives

There are J communities/blocks; and each block is the setting of an equilibrium.[16] There are I households living in different communities. Let $j(i)$ be the block where household i owns its home, and I_j be the set of households living in j. Hurricane Katrina occurs at time $t = 0$. Each household lives forever but has the option to rebuild each period only from 1 to $T = 5$, where each period is one year. Households differ in their housing-related costs, labor market opportunities, levels of attachment to their community and accesses to credit. All information is public among neighbors but is only partially observed by the researcher.[17]

4.1.1 Monetary Incentives

Housing-Related Costs Several housing-related costs and prices influence the financial consequences of each of the three options: 1) i's remaining mortgage balance when Katrina occurred ($M_i \geq 0$); 2) the cost/value of the pre-Katrina physical structure of i's house (p_i^s) (superscript s for structure); 3) the cost of repairing/restoring the house from it's damaged state ($k_i \leq p_i^s$); 4) the (endogenous) market value of the property (the damaged house and

[16] We choose to focus on the equilibrium within each block in order to achieve a more detailed understanding of interactions and spillovers among neighbors. An alternative modeling framework would treat a larger unit, e.g., the whole region, as one economy. Relative to our framework, the second framework may provide a broader view, but most likely at the cost of abstracting from some of the micro features we consider in order to remain tractable.

[17] Given that households in our model are neighbors, we have assumed a complete information structure. The main predictions from our model would still hold if one assumes incomplete information among neighbors. Regardless of the model's information structure, however, it is reasonable to allow for and hence important to account for factors that are common knowledge to the households but are unobservable to the researcher.

the land) if sold privately p_i, 5) the value of insurance payments received ($ins_i \leq k_i$); and 6) the additional incentives created by RH.

If household i has yet to rebuild entering period t, the household may return and reside on the block in period t by paying a one-time repair cost k_i at the beginning of period t, i.e., within a year.[18] Households who rebuild are reimbursed for uninsured damages by a RH (option 1) grant $G_{1i} = \min(\$150{,}000, k_i - ins_i)$. Reflecting RH's slow rollout, grants are dispersed at the start of $t = 3$ if repairs occurred earlier and are dispersed at the time repairs occur otherwise.

For each period that it resides away from its pre-Katrina block, a household rents accommodation comparable to its pre-Katrina home at a cost of $rent_i = \delta \times p_i^s$, where δ is the user cost of housing. The household can sell its property either through RH (option 2) for a price $G_{2,i}$ or privately for a price p_i. The private sales price, as we specify later, depends on the replacement cost of the structure (p_i^s), its damage (k_i), neighborhood characteristics, and *the neighborhood's rebuilding rate* μ_j.

Labor Market Opportunities Household i faces different wages in New Orleans $\{w_{it}^1\}_t$, and outside of New Orleans $\{w_{it}^0\}_t$. The two vectors of wages differ across households, which is another source of variation that may lead to different choices across households.

4.1.2 Household Preferences

A household derives utility from consumption (c), neighborhood amenities, and an idiosyncratic taste for a place. The values of the last two components are normalized to zero for the outside option. The (relative) value of amenities in community j consists of an exogenous part a_j and an endogenous part that depends on the fraction (μ_{jt}) of neighbors who have rebuilt.[19] Households differ in their attachment to their community (η_i), which stands for their private non-pecuniary incentives to return home, assumed to follow an i.i.d. $N\left(0, \sigma_\eta^2\right)$.

[18]For simplicity, we assume that rebuilding occurs during the same period (year) that the rebuilding cost is paid. This assumption should be realistic in the vast majority of cases, as 92% of residential construction starts are completed within one year, and the median time to completion is under six months (Census Survey of Construction, 2005-2010).

[19]Presumably spillover effects can operate via channels that are more general than the rebuilding rate or the fraction of agents who take relevant actions. For feasibility reasons, the literature has typically abstracted away from more general spillover effects. For example, in Bayer and Timmins (2005), the *fraction* of neighbors taking the relevant action enters individuals utility linearly. We make a weaker assumption by allowing household utility to be a much more flexible function of the rebuilding rate.

Household i's per-period utility $v(\cdot)$, suppressing its dependence on (c, a, η), is given by,

$$v_{it}(\mu_{j(i),t}; d_{it}) = \begin{cases} \ln(c_{it}) & \text{if } d_{it} < 1 \\ \ln(c_{it}) + a_{j(i)} + g(\mu_{j(i),t}) + \eta_i & \text{if } d_{it} = 1, \end{cases} \tag{7}$$

where $d_{it} = 1$ if household i has chosen to rebuild by period t, $d_{it} = -1$ if i has sold its house by time t, and $d_{it} = 0$ if neither is true. $\mu_{j(i),t} \in [0, 1]$ is the fraction of neighbors who have rebuilt by time t, and $g(\mu)$ is a non-decreasing function governing the amenity spillovers.[20]

Notice that d_{it} represents one's status at time t; one's action at time t is reflected by a change in d_{it} relative to d_{it-1}. We assume that both selling and rebuilding are absorbing states and hence the only feasible changes in d_{it} over time are $0 \to 1$ or $0 \to -1$. Therefore, $d_{it} > d_{it-1}$ is equivalent to rebuilding in period t; and $d_{it} < d_{it-1}$ is equivalent to selling in period t.[21]

4.1.3 Intertemporal Budget Constraint/Financing Constraints

Letting $1(\cdot)$ be the indicator function, the household intertemporal budget constraint is given by,

$$\begin{aligned}
c_{it} \;=\; & \left.1\Big(d_{it}=1\Big)\times w_i^1 \;+\; 1\Big(d_{it}<1\Big)\times w_i^0 \right\} \text{labor earnings} \\
& \left.-\; 1\Big(d_{it}<1\Big)\times \text{rent}_i - 1\Big(d_{it}>-1\Big)\times \mathit{mortgage}_{it} \right\} \text{flow housing costs} \\
& \left.\begin{aligned} &-\; 1\Big(d_{it}>d_{i,t-1}\Big)\times k_i \\ &+\; 1\Big(d_{i3}=1 \;\text{ and }\; t=3\Big)\times G_{1i} \\ &+\; 1\Big(d_{it}>d_{it-1} \;\text{ and }\; t>3\Big)\times G_{1i} \end{aligned}\right\} \text{repair costs/reimbursements} \\
& \left.+\; 1(d_{it}<d_{it-1})\times \max\Big(G_{2i},\; p_i\Big) \right\} \text{home sale proceeds} \\
& \left.+\; A_{it} - A_{it+1}\Big/R_t \right\} \text{change in asset holding.}
\end{aligned}$$

The first line gives one's labor income. The second line is the flow housing cost, which equals the rent cost if one lives outside of the city plus the mortgage payment if the household still owns its home. The next line is the one-time repair cost one incurs if one rebuilds in this

[20] A non-decreasing spillover function rules out the possibility of particularly strong "congestion" effects, which we view as reasonable in our framework as the number of residents is not allowed to exceed the pre-disaster equilibrium level.

[21] Fewer than 4% of households repaired and then sold their home later during the sample period.

period $(d_{it} > d_{it-1})$. The next two lines summarize the grant one gets for rebuilding, reflecting the fact that the RH grants were typically paid out more than two years after Katrina. The second last line represents the event of a household selling its property $(d_{it} < d_{it-1})$ to RH or privately, whichever gives a higher price. Finally, the household can also change its asset holding at interest rate R_t, with the restriction that,

$$A_{it} \geq \begin{cases} 0 & \text{if } risk_i < \rho^* \\ -\infty & \text{if } risk_i \geq \rho^* \end{cases},$$

where $risk_i$ is household i's Equifax Risk Score (TM). Only households with risk scores above ρ^* may borrow to finance home repairs.[22]

Property Sales Price The price at which a household can sell its home privately p_i is endogenous and affected by the equilibrium neighborhood rebuilding status, such that

$$\ln(p_i) = P\left(p_i^s, k_i, z_{j(i)}, \mu_{j(i),T}\right) + \epsilon_i.$$

The function $P(\cdot)$ captures physical and amenity values of the house. The physical value depends on the house's pre-Katrina physical structure cost (p_i^s) and its damage status captured by k_i. The amenity value depends on both exogenous observable block characteristics vector $z_{j(i)}$ and the *endogenous* block rebuilding rate $\left(\mu_{j(i),T}\right)$. We use the final rebuilding rate $\mu_{j(i),T}$ as a determinant of the price to capture the idea that house buyers are forward looking and care about the future amenity in the neighborhood. The last term ϵ_i is idiosyncratic and known to the household, which may be correlated with other unobservables such as block amenities and individual tastes.

4.2 Household Problem

Given the fraction of households who have rebuilt by the end of $t-1$ and the endogenous law of motion for future rebuilding rates $(\Gamma_{jt}(\mu))$, the discounted value of remaining lifetime

[22] The assumption that households with risk scores above ρ^* have *unlimited* credit access is less restrictive than it might seem. In our framework, the rebuilding decision is the only major investment choice that households face, so the meaningful assumption is that "unconstrained" households have sufficient credit to finance home repairs/rebuilding.

utility for households who have already rebuilt by period t is,

$$V_{it}^{1}\left(\mu_{j(i),t-1}\right) = \sum_{t' \geq t} \beta^{t'-t}\ v_{it'}\Big(\mu_{j(i),t'};1\Big), \tag{8}$$
$$s.t.\ \mu_{t'} = \Gamma_{jt'}(\mu_{t'-1}) \text{ for all } t' \geq t,$$

where the superscript on $V^{d}(\cdot)$ denotes the status $d \in \{1, 0, -1\}$ and we have suppressed the dependence of V on other state variables $(k_i, p_i^s, ins_i, risk_i, M_i, \{w_{it}^1, w_{it}^0\}_t, A_{it})$.
For households who have sold their houses by the beginning of t, this value is

$$V_{it}^{-1}\left(\mu_{j(i),t-1}\right) = \sum_{t' \geq t} \beta^{t'-t}\ v_{it'}\Big(\mu_{j(i),t'};-1\Big). \tag{9}$$

At $t \in \{1, 2, .., T\}$, households that have not rebuilt or sold their houses choose to rebuild, sell or wait, such that

$$V_{it}^{0}\left(\mu_{j(i),t-1}\right) = \max\left\{\begin{array}{l} v_{it}\Big(\mu_{j(i),t};0\Big) + \beta V_{it+1}^{0}\left(\mu_{j(i),t}\right), \\ V_{it}^{-1}\left(\mu_{j(i),t-1}\right), \\ V_{it}^{1}\left(\mu_{j(i),t-1}\right) \end{array}\right\} \tag{10}$$
$$s.t.\ \mu_t = \Gamma_{jt}\left(\mu_{t-1}\right)$$

Beyond T, rebuilding is not an option, so that $\Gamma_{jt}\left(\mu_T\right) = \mu_T$ for all $t > T$ and

$$V_{i,T+1}^{0}\left(\mu_{j(i),T}\right) = \max\left\{V_{it}^{-1}\left(\mu_{j(i),T}\right), \sum_{t' \geq T} \beta^{t'-T}\ v_{it'}\Big(\mu_{j(i),T};0\Big)\right\}.$$

Remark 2 *Notice that the fraction of neighbors who rebuild μ_j affects both the utility associated with rebuilding and the price at which a home can be sold privately. As such, depending on the relative magnitudes of the two effects and on their interactions with household private incentives, it is possible that an increase in μ_j could increase the incentive to rebuild for some households and reduce that incentive for others.*[23]

[23] In our model houses that are sold are not counted as contributing to the rebuilding rate. A main reason is lack of credible data on the timing of rebuilding for sold houses. Households on the left-hand side of the discontinuity have a stronger incentive to sell privately than through RH. To the extent that homes sold privately were rebuilt more quickly than those sold to RH and later auctioned, this would presumably bias down our estimates of spillover effects.

4.3 Equilibrium

Definition 1 *Given $\mu_{j,0}$ and $\mu_t = \mu_T$ for all $t > T$, an equilibrium in community j consists of (i) a set of optimal household decision rules $\{\{d_{it}^*(\cdot)\}_{t=1}^T\}_{i \in I_j}$, (ii) a sequence of period-specific rebuilding rates $\{\mu_{j,t}\}_{t=1}^T$, and (iii) laws of motion $\{\Gamma_{jt}(\cdot)\}_{t=1}^T$ such that,*
(a) Given $\left\{\mu_{j,t}\right\}_{t=1}^T$, $\left\{\left\{d_{it}^*(\cdot)\right\}_{t=1}^T\right\}_{i \in I_j}$ comprise optimal decisions.
(b) The laws of motion $\{\Gamma_{jt}(\cdot)\}_{t=1}^T$ are consistent with individual choices such that,

$$\mu_{j,t} = \Gamma_{jt}\left(\mu_{jt-1}\right) = \mu_{jt-1} + \frac{\sum_{i \in I_j} I(d_{i,t}^* > d_{i,t-1}^*)}{I} \text{ for } 1 \leq t \leq T.$$

With social spillover effects, multiple equilibria may exist (from the researcher's point of view), all of which can be computed given the structure of our model. One commonly assumed equilibrium selection rule for empirical applications is that agents agree on the equilibrium that maximizes their joint welfare, e.g., Jia (2008). We use this equilibrium selection rule because we deem it reasonable in the context of a game among neighbors. As a robustness check, we have re-estimated our model selecting the equilibrium that minimizes joint welfare. Our counterfactual experiment results remain robust, as shown in Online Appendix Table A7.

Remark 3 *We have assumed away contemporaneous shocks for the following reasons. First, choice reversals are rare in the data,[24] suggesting that contemporaneous shocks are weak relative to other forces such as households' permanent heterogeneity embedded in our model. Second, introducing uncertainty in our model would add great complications. Given the small number of households in each block, we have realistically modeled each household as a big player. As a result, household-level shocks would induce aggregate uncertainty: if one household changes its decision due to an unforeseen shock, the rebuilding rate and hence the equilibrium also changes. Solving for the equilibrium in a model like ours with the addition of aggregate uncertainty is beyond this paper.*[25]

[24]Over 96% of households classified as rebuilding in our OPAO data stayed in New Orleans at least until the end of our sample period. Similarly, from 2011 to 2014 fewer than 2% of households changed their home address away from their pre-Katrina block after having returned. These numbers are calculated for the population who in 2004 lived in a New Orleans and had a home mortgage, the best available proxy for home ownership, using quarterly residence-location data from the NYFRB Equifax Consumer Credit Panel.

[25]For recent advances in dealing with large state space problems in dynamic discrete choice settings, see, for example, Arcidiacono et al. (2016).

4.3.1 Tipping

Viewing a problem from an equilibrium perspective not only involves a different modeling framework than an individual decision model, it also bears important policy implications. We discuss one of these implications, the possibility of "tipping" in the presence of multiple equilibria. Even though agents agree on the equilibrium that maximizes their joint welfare *given the set of possible equilibria,* there can still be room for policy interventions because policies can affect the equilibrium set. For example, a policy change may introduce a new equilibrium with a higher rebuilding rate that would not have been self-consistent otherwise, i.e., a "tipping" phenomenon, as illustrated in online Appendix I.

In many cases, it is both convenient and perhaps reasonable for researchers to approximate an outcome variable as a smooth function of explanatory variables. With "tipping" being a potential event, this approach may no longer be appropriate, because when "tipping" happens, there will necessarily be a "jump" in the equilibrium outcomes. Modifying the smooth function by adding certain discontinuity points may help if one knows the locations (e.g., combinations of community characteristics and policies) and the magnitude of these jumps, however, such information is usually not available when performing ex ante policy evaluations. Our framework lends itself toward obtaining such knowledge by explicitly modeling and solving for the equilibrium.

4.4 Further Empirical Specifications

Going from RDD to a model requires assumptions, some of which have been discussed above. The following describes our parametric specifications of two other important components of the model.

4.4.1 Exogenous Neighborhood Amenities

The exogenous component of block-specific amenity values are not directly observable to the researcher, and are modeled as

$$a_{j(i)} = z'_{j(i),t}\gamma + b_{j(i)},$$

where $z'_{j(i),t}\gamma$ captures heterogeneity in amenity values across blocks based on pre-determined block observable characteristics (z), including flood exposure, pre-Katrina demographic composition, and a linear time trend to capture city-wide improvements in infrastructure.[26] $b_j \sim N\left(0, \sigma_b^2\right)$ captures heterogeneity in block amenity values unobservable to the researcher.

[26] We have also estimated a more flexible model, where time trends are allowed to vary by block-level flooding severity. The fit and policy implications of this more flexible model, as presented in the online appendix, are similar to those of the original (more parsimonious) model.

4.4.2 Amenity Spillovers

The amenity spillover function, which characterizes the impact of the block rebuilding rate on the block's amenity value, is given by

$$g(\mu) = S \times \Lambda(\mu; \lambda),$$

where the parameter S measures the total change in amenity utility associated with a block transitioning from a 0% rebuilding rate to a 100% rebuilding rate. $\Lambda : [0,1] \to [0,1]$ is the Beta cumulative distribution function, with parameters $\lambda = [\lambda_1, \lambda_2]'$. The Beta CDF is a parsimonious but flexible function that allows for a wide range of spillover patterns, illustrated in online appendix Figure A5. The parameter λ_2 governs the function's *shape*, and λ_1 governs the *location* of the strongest marginal spillovers.

5 Model Estimation

5.1 Parameters Estimated outside of the Model

To reduce computational burden, we estimate the home price offer function outside of the model, with the following form

$$\ln(p_i) = P_1\left(p_i^s, k_i, z_{j(i)}\right) + P_2(\mu_{-i,j(i),T}) + \epsilon_i,$$

where $P_1()$ is a flexible function with polynomials and interactions and $P_2()$ is a linear spline. OLS estimates of this equation are likely to be biased for several reasons. First, $\mu_{-i,j(i),T}$ is likely to be correlated with the residual ϵ_i, because unobserved block amenities $b_{j(i)}$ that directly affect offered home prices should also affect neighbors' rebuilding choices. Second, offered prices are only observed for households who choose to sell, which will cause selection bias if idiosyncratic household attachment η_i is correlated with unobserved house traits ϵ_i.

We use fixed effects $\chi_{\tau(i)}$ for Census tracts, a larger unit of geography nesting Census blocks, to control for unobserved block amenities, where $\tau(i)$ denotes the Census tract household i belonged to. This approach controls for unobservable factors affecting house prices that are common within a tract. We account for selection using the Heckman (1979) two-step procedure. We use the RH grant formula discontinuity as the excluded instrument in a first stage probit predicting the probability of a home sale,[27] and include the associated

[27]A non-parametric selection-correction using a polynomial in the estimated "propensity score" ($\widehat{sale_i}$) as a control function yields nearly identical results.

inverse Mills ratio as a regressor in the second stage estimating equation, such that

$$\ln(p_i) = P_1\left(p_i^s, k_i, z_{j(i)}\right) + P_2(\mu_{j(i),T}) + \rho\lambda(\Phi^{-1}(\widehat{sale}_i)) + \chi_{\tau(i)} + e_i. \tag{11}$$

5.2 Parameters Estimated within the Model

The vector of structural parameters (Θ) to be estimated within the model consists of the parameters governing: 1) the dispersion of household attachment (σ_η), 2) the exogenous block-specific amenity values (γ, σ_b), 3) the nature of amenity spillovers (S, λ), and 4) the credit score threshold for borrowing (ρ^*). The estimation is via indirect inference, which consists of two steps. Step 1 computes from the data a set of "auxiliary models" that summarize the patterns in the data to be targeted for the structural estimation. Step 2 repeatedly simulates data with the structural model, computes corresponding auxiliary models using the simulated data, and searches for model parameters that match the simulated auxiliary models with those in Step 1.

5.2.1 Auxiliary Models

The auxiliary models that we target include:

1. RDD estimates of the private rebuilding elasticity: $\overline{y}$ and $\mathbf{\Delta}^{(y)}$ from equation (3) characterizing the left and right limits of the private rebuilding rate at the 51% damage grant threshold.

2. RDD estimates of spillovers from private rebuilding choices onto neighbors' rebuilding choices: $\overline{\mu}$ and $\overline{\Delta}$ from Equation (5) characterizing the left and right limits of a household's neighbors' rebuilding rate at the 51% damage grant threshold, and $\mu^{(p)}$ and $\Delta^{(p)}$ from equations (6) for $p = 10, 20, ..., 90$ characterizing the left and right limits of the likelihood that a household's neighbors' rebuilding rate exceeds each threshold at the 51% damage grant threshold.

3. Descriptive regressions of year t private rebuilding indicators on block flood exposure and average block credit scores for t=1,...,5.

5.2.2 Estimation Algorithm

Our estimation algorithm involves an outer loop searching over the space of structural parameters, and an inner loop computing model-generated auxiliary models.

The Inner Loop With simulated data, computing auxiliary models follows the same procedure as described above. We focus on describing the solution to the model. Given Θ, for each community j observed in the data, simulate N copies of communities j_n that share the same observable characteristics but differ in unobservables, at both the individual and the community level. The unobservables are drawn from the distributions governed by (σ_η, σ_b). For each simulated community, solve for the equilibrium as follows, where we suppressing the block subscript j.

1. For each block, locate all possible "self-consistent" period T block rebuilding rates: for each $n_T = 1, ..., I$, compute the offer price for each household $p_i = P\left(p_i^s, k_i, z_{j(i)}, \mu_{j,T} = n_T/I\right)$, count the number of simulated households $n_T^*(n_T; \Theta)$ who prefer to rebuild when $\mu_{j,T}^* = n_T/I$, which is self consistent if $n_T^*(n_T; \Theta) = n_T$.
2. Select the self-consistent $\mu_{j,T}$ that maximizes total block welfare $W_{T-1} = \sum_i V_{i,T-1}$. Store the associated offer price for each household.
3. Taking equilibrium home prices as given, locate all possible "self-consistent" period $T-1$ block rebuilding rates: for each $n_{T-1} = 1, ..., I$, count the number of simulated block households $n_{T-1}^*(n_T; \Theta)$ who prefer to rebuild when $\mu_{j,T-1}^* = n_T/I$, which is self consistent if $n_{T-1}^*(n_{T-1}; \Theta) = n_{T-1}$.
4. Select the self-consistent $\mu_{j,T-1}$ that maximizes total block welfare $W_{T-1} = \sum_i V_{i,T-1}$.
5. Repeat steps 3 and 4 for $t = T-2, T-3, ..., 1$.

The Outer Loop Let $\overline{\beta}$ denote our chosen set of auxiliary model parameters computed from data. Let $\widehat{\beta}(\Theta)$ denote the corresponding auxiliary model parameters obtained from simulating datasets from the model (parameterized by a particular vector Θ) and computing the same estimators. The structural parameter estimator is the solution

$$\widehat{\Theta} = \text{argmin}_\Theta \; [\widehat{\beta}(\Theta) - \overline{\beta}]' W [\widehat{\beta}(\Theta) - \overline{\beta}],$$

where W is a weighting matrix. We obtain standard errors for $\widehat{\beta}(\Theta)$ by numerically computing $\partial\widehat{\Theta}/\partial\overline{\beta}$ and applying the delta method to the variance-covariance matrix of $\overline{\beta}$. We augment the indirect inference strategy with an importance sampling technique suggested by Sauer and Taber (2012) that ensures a smooth objective function.

5.3 Identification

Although all of the structural parameters are identified jointly, we provide a sketch of identification here by describing which auxiliary models are most informative about certain structural parameters. More details can be found in the appendix.

5.3.1 σ_η, σ_b, and $g(\mu)$

The logic follows three steps for identifying parameters governing the unobserved heterogeneity in households' payoffs (η, b, and $g(\mu)$). First, identifying the dispersion of $\eta + b + g(\mu)$. The elasticity of rebuilding with respect to private financial incentives is governed primarily by the dispersion of unobserved heterogeneity in preferences for rebuilding across all households. A household's own rebuilding decision follows a threshold rule based on whether or not $\eta + b + g(\mu)$ exceeds a particular value.[28] All else equal, rebuilding choices will be less price-elastic if unobserved heterogeneity is more disperse, because any given change in the utility threshold for rebuilding caused by a change in financial incentives sweeps over a smaller fraction of unobserved heterogeneity. The dispersion of $\eta+b+g(\mu)$ is thus identified mainly from the size of RDD parameter Δ^y, the difference between the rebuilding rates of households with damage levels just above versus below the RH grant formula discontinuity, relative to the change in incentives Δ^{cost} across the grant threshold.[29]

Second, identifying the distribution of idiosyncratic heterogeneity, characterized by σ_η, separately from the distribution of block-level heterogeneity, characterized by σ_b and $g(\mu)$. The relative variance of idiosyncratic and block-level heterogeneity governs the dispersion of rebuilding rates *across* blocks. If the variance of $\big(b+g(\mu)\big)$ is small, unobserved heterogeneity in payoffs will be mostly idiosyncratic to households within blocks, and blocks with similar observable fundamentals will experience similar rebuilding rates (i.e. μ_j will have a relatively small variance conditional on observables). If the variance of $\big(b+g(\mu)\big)$ is large, there will be large differences *between* blocks with similar observable fundamentals in the average payoff to rebuilding, and μ_j will have a larger variance conditional on observables. The variance of η (i.e. σ_η) is thus separately identified from the variance of $\big(b+g(\mu)\big)$ mainly by the auxiliary model parameters $S^{(10)}$, $S^{(20)}$, ..., $S^{(90)}$ measuring the CDF of block rebuilding rates among

[28]For example, a household would prefer to rebuild in period 5 versus not rebuilding if,

$$b_{j(i)} + \eta_i + \left(\frac{1-\beta}{\beta^5-\beta^9}\right) g(\mu_{j(i),5}) > \left(\frac{1-\beta}{\beta^5-\beta^9}\right) \max_{\{c_{it}\}} \left(\sum_{t=1}^{8} \ln c_{it} \Big| \text{does not rebuild} \right)$$
$$- \left[\left(\frac{1-\beta}{\beta^5-\beta^9}\right) \max_{\{c_{it}\}} \left(\sum_{t=1}^{8} \ln c_{it} \Big| \text{rebuild at } t=5 \right) + Z'_{j(i)t}\gamma \right]$$

[29]In principle, variation in other financial incentives like private insurance settlements, the market values of households' properties, and the prevailing wages in post-Katrina New Orleans in household members' pre-Katrina occupations could aid in identification. We rely, instead, on the RD variation in financial incentives for identification, because we expect that differences across households in these other financial incentives to be correlated with households' idiosyncratic attachment to home and/or the unobserved amenities in households' neighborhoods. On the other hand, while households with damages on either side of the RH grant formula threshold faced significantly different incentives to rebuild, they faced similar distributions of η and b.

household in a particular pre-determined circumstance.

Third, identifying the spillover function $g(\mu)$ separately from the distribution of exogenous block-level amenities b. The spillover function $g(\mu)$ governs the effect of one household rebuilding on its neighbors' incentive to rebuild, and hence the extent to which private incentives will generate spillover effects. A private incentive for particular households to rebuild will have larger spillover effects on the choices of neighbors when $g(\mu)$ is steeper. These spillover effects will only occur on blocks where the damage levels are within particular ranges (not necessarily connected) if $g(.)$ is sufficiently nonlinear, while spillovers will occur similarly across all blocks if $g(.)$ is approximately linear. The identification challenge is that unobserved group-level variables such as b also cause neighbors to behave similarly, so inferring spillover effects in this way is invalid if households' financial incentives are correlated with b. We solve this identification challenge by exploiting the variation in financial incentives generated by the RH grant discontinuity, variation which is as-good-as random and thus orthogonal to b. [30]

In particular, the amplitude and shape of a general non-decreasing smooth amenity spillover function $g(\mu)$ are identified by spillovers from the discontinuously higher RH grant offers made to households with damages just above versus just below the RH grant formula threshold ($\overline{\Delta}$, $\Delta^{(10)}$,..., $\Delta^{(90)}$), compared to the direct effect of those higher grant offers on private rebuilding choices (Δ^y). Under our parameterization of the amenity spillover function, $g(\mu; S, \lambda_1, \lambda_2)$, the amplitude of $g(\mu)$ is governed by the parameter S, and the shape of $g(\mu)$ is governed by the parameters λ_1 and λ_2. The average spillover measure $\overline{\Delta}$ is therefore particularly informative about the value of S, and the pattern of spillovers onto the probabilities that neighbors' rebuilding rates exceed the different thresholds ($\Delta^{(10)}$, ..., $\Delta^{(90)}$) is particularly informative about the values of the shape parameters λ_1 and λ_2.

Given functional form assumptions, our parameterized model is technically identifiable via moments describing the correlation between neighbors' choices that do not have a causal interpretation without the RDD. However, if identified entirely off functional forms, the model is at higher risk of attributing correlation between neighbors' choices caused by a common effect (b_j) to spillovers or vice versa. The RH formula discontinuity provides exogenous variation that shifts individual households' incentives but is uncorrelated with neighborhood-level unobservables (b_j), which serves as a more reliable way to disentangle the role of neighborhood-level unobservables from causal spillover effects ($g(\mu_j)$) within the range of variation in the data. The model identified as such gives us more confidence in its policy implications.

[30] See Online Appendix V for a more formal presentaion of this argument.

5.3.2 Other Parameters

The credit risk cutoff parameter ρ^* determines the fraction of households in each neighborhood who are borrowing constrained. Because most RH grants were paid more than two years after Hurricane Katrina, the prevalence of borrowing constraints strongly influences the predicted timing of rebuilding. For households who were borrowing constrained, the self-financing of home repairs prior to RH grants being disbursed would have entailed a significant reduction to lifetime consumption utility, forcing them to delay rebuilding. For households who were able to borrow, consumption would be smooth across periods regardless of the timing of repairs. The parameter ρ^* is thus mainly identified by the extent to which the repair rate hazard increased after the disbursement of RH grants across neighborhoods with different mean Equifax risk scores.

Finally, the parameters γ describing exogenous differences in amenity values across flood categories are identified by differences in rebuilding rates across flood categories beyond what would be predicted by households' private incentives.

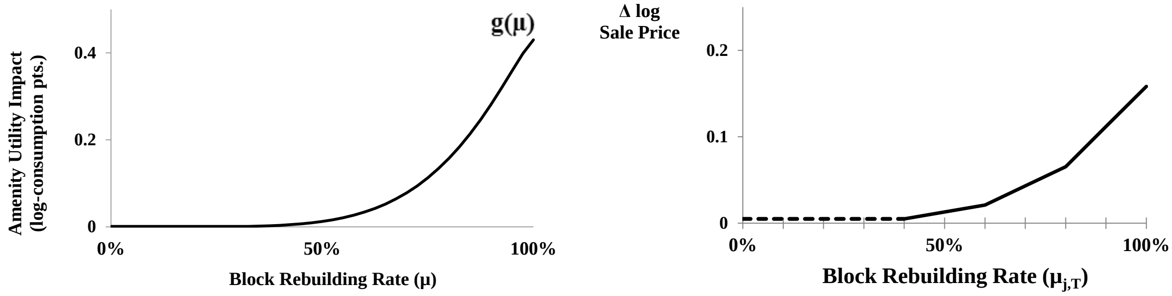
6 Results

6.1 Parameter Estimates

The top panel of Table 3 presents the estimated parameters governing the spillover function. To illustrate the spillover effects, we plot the estimated spillover function $g(\mu)$ in the left panel of Figure 5. Fully rebuilding a block increases its flow amenity valuation by the equivalent of a 43 log-point increase in annual non-housing consumption. However, the incremental impact of additional rebuilding depends critically on the block's initial rebuilding rate. The marginal impact of rebuilding on a block's amenity value is close to zero in areas with rebuilding rates below 50%; yet it is substantial in areas with rebuilding rates above 50%. The right panel of Figure 5 plots the estimated effect of a block's rebuilding rate on house offer prices, i.e., the spline in μ_{-i} from estimating equation (11). A home's price increases by nearly 20% if all of the homes on its block are rebuilt ($\mu_{-i} = 1$ relative to $\mu_{-i} = 0$). The marginal impact of rebuilding on home prices is the highest in areas with the highest rebuilding rate.

The next panels of Table 3 characterize the exogenous components of block amenities. An estimated set of year-specific utility intercepts increases monotonically with time, presumably reflecting city-wide infrastructure repairs. Coefficient estimates on flood exposure do not exhibit a clear pattern, suggesting that a block's amenity value is not strongly correlated with its flooding risk *per se*. We find significant heterogeneity in the unobserved

Figure 5: Spillover Effects of Rebuilding on Flow Amenity Utility and Offered Home Prices



(a) (b)

Note: The left panel of this figure plots the estimated shape of the equilibrium model's amenity spillover function $g(\mu)$. The right panel plots the estimated impact of same-block neighbors' rebuilding on home price offers (specifically, the neighbors' rebuilding rate spline from Equation (11)).

component of blocks' flow amenity values (σ_b=0.39). In comparison, the cross-block standard deviation of equilibrium amenity levels net of unobservables $\left(z_j'\gamma + g(\mu_{j,t})\right)$ is 0.41 at $t = 1$, and 0.46 at $t = 5$. The standard deviation of households' idiosyncratic attachment (σ_η) is 0.60 log-consumption points, which is 12.9% of the average log consumption level across all households.

Finally, our estimated Equifax credit score threshold (ρ^*) for securing a rebuilding loan is 678.5. This threshold is considerably higher than the commonly-cited "rule of thumb"cutoff of 620 for securing a standard mortgage (Keys, Mukherjee, Seru, and Vig 2008), and thus is consistent with the fact that the federally-subsidized SBA Disaster Loan program rejected a large majority of applicants from the Gulf Coast in the aftermath of Katrina (Eaton and Nixon, 2005).

6.2 Model Fit

Figure 6 illustrates the model's fit to the RD parameters that we targeted as auxiliary models. The model closely replicates the difference between the rebuilding rate of households with just below 51% damage and just above 51% damage ($\Delta^{(y)}$ from equation (3)), the difference in the mean and distribution of same-block-neighbor rebuilding rates between these two groups ($\overline{\Delta}$ from equation (5) and $\Delta^{(10)}, ..., \Delta^{(90)}$ from equation(6)).

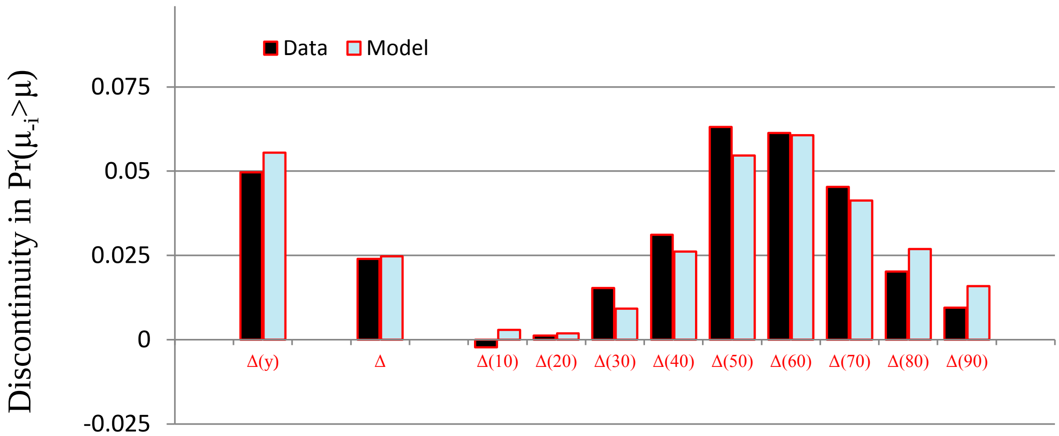
In the appendix, Figures (A2) illustrate the model's fit to rebuilding trends, for the full sample, by block flood exposure and by average neighborhood credit scores. Overall, the model fits the data well. It captures many of the major differences in rebuilding trends, which are not directly targeted during the estimation, although the fit for some flooding

Table 3: Structural Parameter Estimates

Parameter:	Estimate
Spillover function: S x BetaCDF(μ; λ_1,λ_2)	
S: Spillover magnitude	0.43 [0.008]
λ_1: Location of spillover threshold	0.82 [0.003]
λ_2: Steepness of spillover nonlinearity	6.99 [0.401]
Year-specific intercepts	
Year 1	-1.39 [0.048]
Year 2	-0.98 [0.027]
Year 3	-0.65 [0.031]
Year 4	-0.31 [0.030]
Year 5+	0.08 [0.018]
Observable heterogeneity in flow location payoffs: Z'γ	
Flood exposure:	
< 2 feet	-0.22 [0.007]
2-3 feet (reference)	---
3-4 feet	0.07 [0.046]
4-5 feet	-0.14 [0.013]
5-6 feet	-0.18 [0.059]
> 6 feet	-0.08 [0.058]
Unobserved heterogeneity in flow location payoffs:	
σ_η: Variance of idiosyncratic attachment to pre-Katrina block	0.60 [0.035]
σ_b: Variance of unobserved block effect	0.39 [0.028]
Credit Access:	
Cuttoff Credit Score for Rebuilding Loans (ρ*)	678.5 [8.70]
Observations - household-periods	300,875
Observations - households	60,175

Note: This table reports estimates of the equilibrium rebuilding model's structural parameters. Estimation is by indirect inference. Standard errors are computed by applying the delta method to the (clustered at the Census block level) standard errors of the underlying target auxiliary model parameters.

Figure 6: Goodness of Fit: Actual and Model-Predicted R.D. "Spillover" Coefficients



categories is not as good. Figure A3 shows that the model fits well to the distribution of block rebuilding rates five years after Katrina. Table A3 shows the model's fit to the rebuilding rates of subgroups within finer subgroups, none of which are targeted moments.

7 Counterfactual Policy Simulations

We use the estimated equilibrium model to perform three sets of counterfactual policy analyses that shed light on the optimal structure of disaster relief grants. The first quantifies spillover effects by decomposing the full impacts of RH into its impact via private financial incentives alone and that via amenity spillovers. The second illustrates how policy implications can change once equilibrium interactions are accounted for, by contrasting welfare effects of RH with those under unconditional grants. In the appendix we also present results of a policy that eliminates the discontinuity in the RH formula so that grant becomes a continuous function of damages. Finally, we illustrate how our modeling framework can be used for optimal policy designs.

7.1 RH's Direct Effects and Feedback Effects

To measure the importance of the "feedback" effects from amenity spillovers, we compare the full equilibrium impact of RH with the impact generated by the program's financial incentives alone (holding amenities fixed). To begin, we simulate equilibrium rebuilding choices without RH grants. We then simulate the impact of introducing RH's financial incentives while holding amenity values fixed, which boils down to an individual decision model without spillovers. Finally, we simulate our equilibrium model with endogenous amenity values, which measures the full equilibrium impact of RH.

Table 4 reports the impacts of RH on rebuilding rates, without and with amenity spillovers, as of Katrina's 5th anniversary. RH's private incentives increased the rebuilding rate by 6.3 percentage points, from a rate of 61.7% without grants. RH's full equilibrium impact was 1.7 percentage points larger, implying an average multiplier effect of 1.27 (8.0/6.3). The direct RH effect from private financial incentives and the equilibrium multiplier were both larger in areas that suffered from moderate flooding (between 2 and 5 feet), relative to areas with the least and the most severe flooding. Comparing areas with different rebuilding rates without RH, we find that the impact of RH decreases with an area's no-grant rebuilding rate. Yet, consistent with the spillover function estimates, the equilibrium multiplier increases with an area's no-grant rebuilding rate.

Table 4: RH's Partial-Equilibrium and Equilibrium Effects on Rebuilding

	(1)	(2)	(3)	(4)
		Rebuilding Rate Impacts		
Subgroup	No grants Rebuilding Rate	Partial Equilibrium Road Home	Equilibrium Road Home	Spillover Multiplier
All	61.7	+6.3	+8.0	1.27
Flood depth:				
< 2 feet	76.2	+4.0	+4.5	1.13
2-3 feet	59.7	+10.5	+14.1	1.34
3-4 feet	59.5	+7.9	+11.2	1.42
4-5 feet	46.2	+9.4	+12.6	1.34
5-6 feet	35.6	+7.6	+9.3	1.22
>6 feet	42.4	+6.3	+8.0	1.27
Rebuilding Rate w/o RH:				
90-100%	99.3	+0.1	+0.2	2.00
80-90%	85.1	+3.5	+5.3	1.51
70-80%	75.6	+5.5	+8.8	1.60
60-70%	66.0	+7.4	+11.0	1.49
50-60%	55.1	+8.0	+11.2	1.40
40-50%	45.4	+9.0	+11.8	1.31
30-40%	36.7	+9.7	+11.7	1.21
20-30%	26.2	+10.3	+11.9	1.16
10-20%	16.6	+13.4	+14.7	1.10
0-10%	4.7	+14.7	+14.9	1.01

Source: Authors' calculations using the estimated equilibrium model.

7.1.1 Potential Multiple Equilibria and Policy Implications

As mentioned earlier, even though households agree on the equilibrium to be selected *given the set of possible equilibria,* policy interventions can affect the equilibrium *set.* As a result, impacts of a given policy may vary substantially across blocks, depending on the nature of each block's equilibrium set without the policy and the degrees to which these sets vary with the policy. Our framework is well-suited to study such implications, because it allows us to calculate the full set of equilibria under any given policy.

We start with relating block characteristics to the nature of their equilibrium sets. We divide blocks into two groups based on equilibrium uniqueness. Group 1 accounts for 86% of blocks, all of which have a unique equilibrium with and without RH. Group 2 consists of all the other blocks, i.e., those with multiple equilibria in at least one case. As shown in Panel A of Table 5, relative to Group 1 blocks, Group 2 blocks are more likely to have been exposed to more severe flooding, to have higher fractions of households that are black, with lower than college education and/or lower credit scores. In other words, multiple equilibria are more likely to exist in more disadvantaged blocks.

Next, we contrast RH impacts between the two groups, as shown in Panels B and C of Table 5. Consistent with Group 2 being more disadvantaged, the average rebuilding rate would have been lower in Group 2 (58%) than in Group 1 (62%) without RH; and RH's financial incentive alone increased the rebuilding rate more for Group 2 (8.7%) than for Group 1 (5.9%). Consistent with "tipping", RH generated a much larger multiplier effect for Group 2 in the presence of multiple equilibria. As a result, RH's full equilibrium impact was much higher for Group 2 (16.6% on rebuilding rate and $8,600 on welfare) than for Group 1 (6.3% on rebuilding rate and $630 on welfare). Thanks to multiple equilibria, RH was more effective where help was needed the most, i.e., more disadvantaged blocks.

7.2 Welfare Effects of RH versus Unconditional Grants

The RH grant program discouraged households from relocating by requiring relocating households to give their properties to a state land trust. Compared to unconditional transfers, conditional transfers entail an efficiency loss or "excess burden" for households whose choices are distorted by the conditions associated with the transfer. However, when considered in an equilibrium framework with spillovers, the conditional nature of the RH transfer may improve total welfare if the value of the positive externality generated by RH-induced rebuilding exceeds the private losses from the program's distortion.

To study the welfare consequences of RH's conditional structure, we compare the RH equilibrium outcomes with those under an unconditional grant program that pays a grant computed with the RH rebuilding grant formula to all households regardless of their choices. We compute the equivalent variation (EV_i^{RH}) necessary to make a household's equilibrium welfare under the unconditional grant policy equal to that under RH, which measures households' utility difference under the two policies in dollars.[31] To account for the difference in subsidies granted in two programs, we compute household-level net welfare impacts $\left(dW_i^{RH}\right)$ by subtracting the change in program costs, i.e.,

$$dW_i^{RH} = EV_i^{RH} - (Grant_{i,RH} - Grant_{i,Uncond}). \tag{12}$$

Table 6 summarizes the results. Column (1) reports that 9.1% of households were "marginal" in the sense that their rebuilding choices under RH differed from what they would have made under the unconditional grant policy. This fraction was the smallest in the least flooded areas and highest in moderately flooded areas. The next three columns show the net welfare impacts of RH relative to the unconditional grant $\left(dW_i^{RH}\right)$ for inframarginal, marginal and all households, respectively. For an average inframarginal household, welfare improved by \$4,950 as the result of the conditional grant structure that induced positive spillovers from the rebuilding of marginal households. An average marginal household, however, was worse off by an equivalent of \$24,360. Overall, RH increased average household welfare by \$2,177 (\$131M in total), relative to the unconditional grant policy. Except for areas with 5-6 feet of flooding, where a \$475 loss occurred, welfare improved across areas with different flooding exposures. The improvement was particularly significant in moderately flooded areas, which is consistent with our previous finding that RH's impacts on rebuilding rates were larger in these areas.

Remark 4 *Our calculation of dW_i^{RH} does not take into account the value of properties turned over to the state by RH relocation grant recipients. Before Katrina, the total value of the land on which these properties sat (appraised by the parish Assessor's Office for property tax purposes) was \$54M. Assigning this value to the properties in the welfare calculation would increase the calculated per capita "government savings" from RH compared to the unconditional grant program by \$908 (=\$54M / 60,175). While we do not have reliable data on the post-Katrina value of these properties, we expect the value to be substantially lower*

[31]Specifically, we find the dollar amount that when paid as a constant per-period flow from $t = 1, ..., T$ under the unconditional grant policy provides household i the same discounted lifetime utility i receives in equilibrium under RH. Letting ev_i denote the per-period payment, our equivalent variation measure EV_i is the present discounted value of this stream,

$$EV_i = \left(\frac{ev_i}{r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)$$

Table 5: Neighborhood Traits, Rebuilding Rate Impacts, and Welfare Impacts by Number of Equilibria

A. Neighborhood Charactoristics	Group 1: Unique	Group 2: Multiple
Pre-Katrina block flood exposure:		
< 2 feet	51	16
2 - 3 feet	10	24
3 - 4 feet	9	23
4 - 5 feet	9	14
5 - 6 feet	7	3
> 6 feet	14	19
Demographic composition:		
Percent black (Census block)	55	67
Percent college educated (Census tract)	52	47
Equifax risk score (spatial moving average):		
<600	18	22
600-625	16	21
625-650	17	17
650-675	14	15
675-700	13	6
700-725	10	8
>725	12	10
Percent of replicated blocks	84.0	16.0
B. RH Rebuilding Impacts		
No grants Rebuilding Rate	62.1	58.0
Partial Eqm. RH Impact	+5.9	+8.7
Equilibrium RH Impact	+6.3	+16.6
Multiplier	1.07	1.92
C. RH Welfare Impacts		
Equilibrium RH Impact (per capita)	$627	$8,602

than $54M, because the properties are located disproportionately in areas of the city that received heavy flooding and in the neighborhoods that were slowest to rebuild.

7.3 The Optimal Generosity of Relocation Grants

We have shown that RH improved average household welfare by $2,177 relative to an unconditional grant policy. In the following, we examine the potential for further improvements by

Table 6: Decomposing the Welfare Effects of RH's Rebuilding Stipulations

Group	% Marginal	Inframarginal Households ($)	Marginal Households ($)	**Total ($)**
All	9.1	4,950	-24,360	**2,177**
< 2 feet	4.8	1,954	-35,050	**140**
2-3 feet	15.7	12,890	-19,170	**7,726**
3-4 feet	13.4	10,010	-18,350	**6,133**
4-5 feet	14.8	7,384	-21,300	**2,988**
5-6 feet	11.0	2,894	-26,570	**-475**
> 6 feet	9.7	4,453	-23,240	**1,656**

Note: This table reports the impact of the Louisiana Road Home program on average household welfare relative to an unconditional grant policy. RH required households who accepted "relocation" grants to turn their properties over to a state land trust, while the unconditional grant policy pays RH "rebuilding" grants to all households regardless of their rebuilding choices. RH offering smaller net grant packages to households who do not rebuild affects welfare through three channels: (1) changes to equilibrium property values, (2) changes to the non-pecuniary utility households derive from their equilibrium location choices (measured as equivalent variations), and (3) reductions to the size of net grant packages (for inframarginal non-rebuilding households). Because item (3) is an equal-sized benefit to the government, the total change to social welfare is the sum of (1) and (2).

exploring a particular form of conditional subsidy policies: we take as given the RH rebuilding grant formula and make the relocation grant a fraction $(1-\rho)$ of that of the rebuilding grant, but without the requirement that relocating households turn their property over to the state. We consider different constraints on how flexibly the policy maker may vary ρ; and for a given constraint, we search for the vectors of ρ's that maximize the equilibrium average net welfare. The constraints we consider require, respectively, that a uniform ρ be applied to all households, to all households within each subgroup defined by 1) block-level demographics, 2) the fraction of damaged houses in a block, 3) block-level flood exposure, and 4) the interaction of 2) and 3).[32] In addition, we also consider a case where ρ's are allowed to be block-specific, which although hard to implement serves as an upper bound on the welfare impacts a policy might achieve.

Remark 5 *We conduct a particular set of counterfactual policies for illustration. Given a different policy space defined by specific constraints, one could use our model to search for policies that satisfy given optimality criteria.*

[32]Criterion 1) defines 28 groups by the cross product of $I\left(P(black)>0.5\right)$, $I\left(P(\text{college-educated})>0.5\right)$ and average credit score category. Criterion 2) defines 10 groups with the fractions <10%, 10-20%,... 90-100%. Criterion 3) defines 6 groups.

Table 7 summarizes the impacts of these constrained optimal subsidy policies on household welfare, government savings, and net welfare, i.e., the counterparts of EV_i^{RH}, $(Grant_{i,RH} - Grant_{i,Uncond})$ and dW_i^{RH} as defined in (12). Relative to the welfare level under the unconditional grant policy, savings in grant funds dominates the household welfare changes under the uniform

Table 7: The Welfare Consequences of Alternative Policies

Policy	(1) Per capita Govt. Savings	(2) Per capita Δ HH Welfare	(3) Per capita Δ Tot. Welfare	(4) Aggregate Δ Tot. Welfare
Unconditional grants **[reference policy]**	\$0	\$0	\$0	\$0
Category-specific welfare-maximizing ρ*:				
City is one category (uniform policy)	\$9,593	-\$6,945	\$2,648	+\$159M
Categories based on block demographics	\$9,555	-\$6,618	\$2,936	+\$177M
Categories based on t=0 damage-%	\$9,111	-\$6,022	\$3,090	+\$186M
Categories based on flood depth	\$8,342	-\$4,731	\$3,611	+\$217M
Categories based on t=0 damage-%, and flood depth interactions	\$7,047	-\$2,980	\$4,066	+\$244M
Perfect block-level targeting	\$3,951	\$2,048	\$6,000	+\$361M

Note: This table summarizes the results of counterfactual experiments comparing average household welfare under policies that offer smaller grants to households who do not rebuild to average welfare under a policy that pays RH rebuilding grants unconditionally. Specifically, we consider policies that offer a fraction $(1-\rho)$ of the RH rebuilding grant to households if they choose to relocate, where ρ is chosen optimally subject to various constraints. The constraints we consider include; (1) that ρ be uniform city-wide, (2) that ρ be uniform within flood depth categories, (3) that ρ be uniform within baseline-block-rebuilding-rate categories, and (4) that ρ may be household-specific.

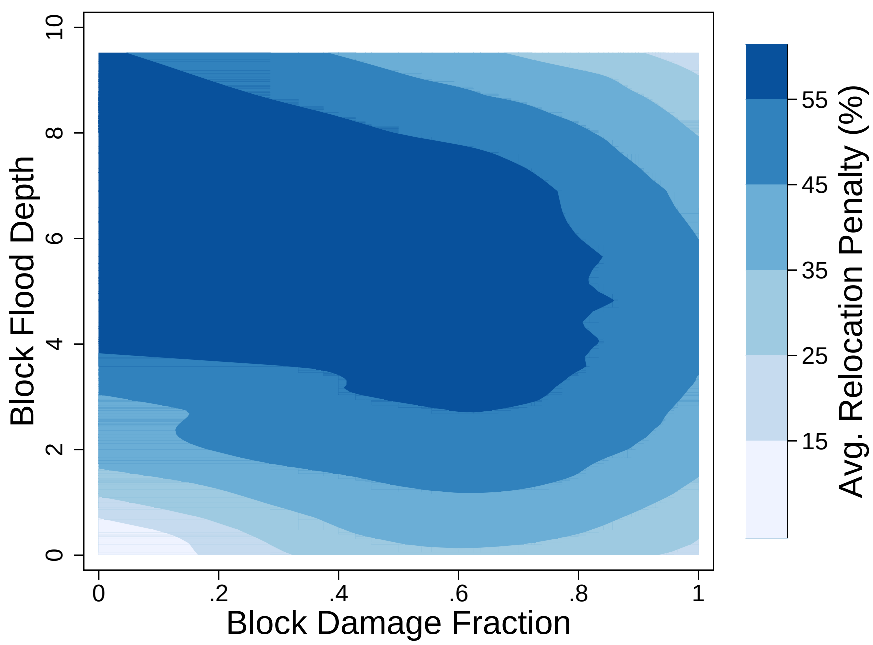
conditional subsidy policy, increasing the net average household welfare by \$2,648. Targeting by block demographics and by the fraction of blocks' damaged homes both lead to minor improvement over the uniform subsidy. While targeting by flood exposure yields smaller reduction in government expenditures, the approach yields substantially smaller private welfare losses for households, and generates larger net welfare improvements of \$3,613 per household over the unconditional policy.

The most interesting result comes from block-specific subsidies, which not only save government grant money, but also improve household welfare, leading to a net welfare increase of \$6,000 per household. It is worth noting that in the absence of externalities, a distorting conditional subsidy policy could not improve household welfare relative to an unconditional subsidy policy. In the presence of externalities, not only can carefully-designed conditional subsidies improve household welfare, but also at lower government costs, leading to a "win-

win" scenario.

To illustrate the nature of the optimal policies, Figure 7 shows the optimal block-specific penalty ρ by block-level flood exposure and the block-level home-damage rate. The relationship is inversed-U-shaped. The optimal policy heavily penalizes ($\rho > 50\%$) relocation in neighborhoods with moderate damage rates and/or flood exposure, as those neighborhoods tend to have many households close to the margin of rebuilding and are ones where additional rebuilding generates substantial positive externalities for inframarginal households. In contrast, relatively few households are close to being marginal in neighborhoods that received very light or very severe damages, so it is optimal to provide grants with few strings attached.

Figure 7: Summary of Optimal Block-Specific Relocation Penalties



Note: This figure summarizes our estimated rules for optimally targeting relocation penalties at the block level. Considering a class of policies that offer the Road Home rebuilding grant to all households who rebuild and a fraction $(1 - \rho)$ of that grant to households who relocate, we calculated the optimal block-specific penalty ρ^* that maximize welfare on each block. We then regress the block-specific ρ^* values on block flood depth, the block damage rate, block percent black, block percent college-educated, and block average block Equifax risk scores. This figure's contours summarize the predicted values from this regression evaluated at the city-wide average values of the race, education, and credit variables.

8 Conclusion

Many housing policies are predicated on the idea that housing investments generate positive externalities. The optimal design of these policies requires an understanding of the nature of these externalities, the decision making processes of individual households, and the way those decisions intertwine in equilibrium. Toward that end, we have developed a framework that combines the strength of quasi-experimental research designs and the strength of structural equilibrium modeling. We have applied this framework to the case of post-Katrina reconstruction. The quasi-experimental variation in private financial incentives that we exploit for identification admits a causal interpretation of both the direct effects of RH financial incentives on rebuilding and of the spillover effects of those incentives onto neighbors' rebuilding choices. Our equilibrium model replicates these internally-consistent causal relationships and other patterns not directly targeted for identification.

We have found that rebuilding caused economically important amenity spillovers: the distorting RH program led to higher welfare compared to an unconditional subsidy policy. We have illustrated how our framework can inform the design of optimal subsidy policies, which further improve household welfare while saving government costs, compared to unconditional subsidies. Such a "win-win" situation would be an implausible prediction if researchers were to treat household decisions in isolation.

Although our empirical application focuses on a special event and a particular source of identifying variation, our equilibrium modeling framework for studying private investment choices in equilibrium can be applied/extended to other cases where individual decisions are inter-related due to spillover effects. Our strategy for identifying such models using quasi-experimental variation is also promising for studies that aim at shedding light on policy designs with relatively less restrictive modeling assumptions for identification.

Several extensions to our framework are worth pursuing. The first one is to embed our model into a more general equilibrium framework that considers equilibrium interactions within an entire city or region. A more general framework would also model equilibrium in the labor market, allowing for the possibility of downward sloping local labor demand (e.g., Albouy 2009, Roback 1982) conditional on population and the possibility that changes to the size of the local population shifts the local labor demand curve. Identifying these more general equilibrium models, however, would require data from multiple markets. A related but different extension would examine another dimension of policy impact heterogeneity, one that differs across different types of block geographies (Jacobs, 1961).

Another extension is to incorporate other spatially-biased policies and consider them simultaneously with the rebuilding grant policy. The existence of other distortive policies may lead to situations where the number of households living in certain areas is inefficiently high prior to a disaster occurring. This could happen due to, for example, moral hazard resulting from the precedent of generous post-disaster bailouts (Gregory 2014) or the federal income tax code's relatively favorable treatment of less productive places (Albouy 2009, Colas and Hutchinson 2015). Although existing estimates of these distortive effects are relatively small compared to the direct incentives for locating in particular cities and neighborhoods following some disasters, optimal rebuilding grant policies may differ depending on whether or not these other existing distortions are accounted for.

References

[1] Albouy, David. 2009. "The Unequal Geographic Burden of Federal Taxation." Journal of Political Economy 117(4):635-667.

[2] Anenberg, Elliot and Edward Kung. forthcoming. "Estimates of the Size and Source of Price Declines Due to Nearby Foreclosures." *American Economic Review.*

[3] Autor, David, Christopher Palmer, and Parag Pathak. forthcoming. "Externalities from Rent Control? Evidence from Cambridge MA." *Journal of Political Economy.*

[4] Bayer, P., F. Ferreira, and R. McMillan. 2007. "A Unified Framework for Measuring Preferences for Schools and Neighborhoods," *Journal of Political Economy*, 115(4)588-638.

[5] Bayer, P., R. McMillan, and K. Reuben. 2005. "Resedential Segregation in General Equilibrium," NBER Working Paper 11095.

[6] Bayer, Patrick and Christopher Timmins. 2007. "Estimating Equilibrium Models of Sorting Across Locations." *Economic Journal*, 117(518): 353-374.

[7] Bayer, Patrick and Christopher Timmins. 2005. "On the equilibrium properties of locational sorting models." *Journal of Urban Economics*, 57(3): 462-477.

[8] Bayer, Patrick and Christopher Timmins. 2007. "Estimating Equilibrium Models of Sorting Across Locations." *Economic Journal*, 117(518): 353-374.

[9] Bajari, Patrick, C. Lanier Benkard and Jonathan Levin. 2007. "Estimating Dynamic Models of Imperfect Competition." *Econometrica*, 75(5): 1331–1370.

[10] Bajari, Patrick, Han Hong, John Krainer and Denis Nekipelov. 2010. "Estimating Static Models of Strategic Interactions." *Journal of Business and Economic Statistics*, 28(4): 469-482.

[11] Berry, Steven, James Levinsohn and Ariel Pakes. 1995. "Automobile Prices in Market Equilibrium." *Econometrica*, 63(4): 841-890.

[12] Bjorn, P., and Q. Vuong. 1984. "Simultaneous Equations Models for Dummy Endogenous Variables: A Game Theoretic Formulation With Application to Labor Force Participation." SSWP 537, California Institute of Technology.

[13] Blume, Lawrence, William Brock, Steven Durlauf, and Yannis Ioannides. 2010. "Identification of Social Interactions, "in Handbook of Social Economics, J. Benhabib, A. Bisin, and M. Jackson, eds., Amsterdam: North Holland.

[14] Bresnahan, Timothy and Peter Reiss. 1990. "Entry in Monopoly Markets." *The Review of Economic Studies*, 57: 531-553.

[15] Brock, William and Steven Durlauf. 2001. "Discrete Choice with Social Interactions," *Review of Economic Studies*, 68: 235-260.

[16] Brock, William and Steven Durlauf. 2006. Multinomial Choice with Social Interactions,Ein *The Economy as an Evolving Complex System*, L. Blume and S. Durlauf, eds., New York: Oxford University Press.

[17] Brock, William and Steven Durlauf. 2007. "Identification of Binary Choice Models with Social Interactions," *Journal of Econometrics*, 140(1), 52-75.

[18] Calononico, Sebastian, Matias Catteneo, and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression Discontinuity Designs." *Econometrica*, 82(6): 2295-2326.

[19] Campbell, John Y., Stefano Giglio, and Parag Pathak. 2011. "Forced Sales and House Prices." *American Economic Review.*

[20] Ciliberto, Federico and Elie Tamer. 2009. "Market Structure and Multiple Equilibria in Airline Markets." *Econometrica*, 77(6): 1791-1828.

[21] Das, Mitali, Whitney Newey, and Francis Vella. 2003. "Nonparametric Estimation of Sample Selection Models." *Review of Economic Studies*, 70(1): 33-58.

[22] de Paula, Aureo. 2009. "Inference in a Synchronization Game with Social Interactions," *Jornal of Econometrics*, 148(1):56-71.

[23] de Paula, Aureo. 2013. "Econometric Analysis of Games with Multiple Equilibria." *Annual Review of Economics*, 5: 107-131.

[24] Einav, L. 2010. "Not All Rivals Look Alike: Estimating an Equilibrium Model of the Release Date Timing Game." *Economic Inquiry*, 48(2): 369–390.

[25] Elliott, J. R. and J. Pais. 2006. "Race, class, and Hurricane Katrina: Social differences in human responses to disaster." Social Science Research 35(2):295-321.

[26] Epple, D., T. Romer, and H. Sieg. 2001. "Interjurisdictional Sorting and Majority Rule: An Empirical Analysis," *Econometrica*, 69: 1437E465.

[27] Epple, D. and H. Sieg. 1999. "Estimating Equilibrium Models of Local Jurisdictions," *Journal of Political Economy*, 107: 645E81.

[28] Gregory, J. 2012. "The Impact of Rebuilding Grants and Wage Subsidies on the Resettlement Choices of Hurricane Katrina Victims," *Working Paper*

[29] Groen, J. A. and A. E. Polivka. 2009. "Going Home after Hurricane Katrina: Determinants of Return Migration and Changes in Affected Areas." Demography 47(4):821-844.

[30] Guerrieri, Veronica, Daniel Hartley, and Erik Hurst. 2013. "Endogenous Gentrification and Housing Price Dynamics." *Journal of Public Economics*, 100:45-60.

[31] Harding, John P. , Eric Rosenblatt, and Vincent W. Yao. 2009. "The Contagion Effect of Foreclosed Properties." *Journal of Urban Economics,* 66: 164-178.

[32] Ioannides, Y. and J. Zabel. 2008. "Interactions, Neighborhood Selection, and Housing Demand," *Journal of Urban Economics*, 63: 229-252.

[33] Ioannides, Yannis M. 2003. "Interactive Property Valuations." *Journal of Urban Economics*, 53: 145-170.

[34] Jacobs, Jane. 1961. "The death and life of great American cities." *Random House.*

[35] Jia, Panle. 2008. "What Happens When Wal-Mart Comes to Town: An Empirical Analysis of the Discount Retailing Industry." *Econometrica,* 76: 1263–1316.

[36] Manski, C. 1993. "Identification of Endogenous Social Effects: The Reflection Problem," *The Review of Economic Studies*, 60: 531-542

[37] McCarthy, K. D.J. Peterson, N. Sastry, and M. Pollard. 2006. "The Repopulation of New Orleans After Hurricane Katrina." Technical report. RAND Corp.

[38] Keys, Benjamin, Tanmoy Mukherjee, Amit Seru, and Vikrant Vig. 2008. "Securitization and Screening: Evidence from Subprime Mortgage Backed Securities." *Quarterly Journal of Economics*, 125(1).

[39] Paxson, C. and C. Rouse. 2008. "Returning to New Orleans after Hurricane Katrina." *American Economic Review*, 98(2):38-42.

[40] Roback, J. 1982. "Wages, Rents, and the Quality of Life." The Journal of Political Economy 90(6):1257-1278.

[41] Rossi-Hansberg Sarte Owens. 2010. "Housing Externalities." *Journal of Political Economy*, 118(3):485-535.

[42] Sauer, R and C. Taber. 2017. "Importance sampling in indirect inference." *NBER Working Paper No. 23669.*

[43] Tamer, Elie. 2003. "Incomplete Simultaneous Discrete Response Model with Multiple Equilibria." *The Review of Economic Studies*, 70(1): 147-165.

[44] Todd, P. and K. Wolpin. 2008. "Ex Ante Evaluation of Social Programs," *Annals of Economics and Statistics*, 91/92: 263-291

[45] U.S. Census Bureau's Survey of Construction (2005, 2006, 2007, 2008, 2009, 2010).

[46] Vigdor, J. 2008. "The Economic Aftermath of Hurricane Katrina." The Journal of Economic Perspectives 22:135-154.

[47] Vigdor, J. L. 2007. "The Katrina Effect: Was There a Bright Side to the Evacuation of Greater New Orleans?" The B.E. Journal of Economic Analysis & Policy 7(1).

[48] Zissimopoulos, J. and L. Karoly. 2010. "Employment and Self-Employment in the Wake of Hurricane Katrina." Demography 47(2):345-356.

Appendix I. Illustration of "Tipping"

Figure A1 illustrates the phenomenon of "tipping." The top panel of Figure A1 plots a hypothetical private demand schedule for rebuilding evaluated at the amenity level associated with a 0% rebuilding rate and the actual marginal benefit curve.[33] Self-consistent rebuilding rates are the zeros of the latter curve. Tipping

[33] The private demand curve is downward sloping by definition as it is simply a highest-to-lowest ordering of individual households' net benefits to rebuilding. The actual marginal benefit curve incorporates each additional household's positive contribution to block amenities and can thus be downward or upward sloping.

is shown in the bottom panel, where a subsidy causes additional higher rebuilding rates to become self-consistent.

Table A1: Descriptive Statistics, Census Blocks

Variable	All blocks Mean (S.D.)	Blocks with any initially damaged homes Mean (S.D.)
Demographic composition:		
Number of households	12.6 (7.7)	12.9 (7.9)
Percent black (Census block)	61.7 (48.6)	69.0 (46.3)
Percent college educated (Census tract)	45.1 (49.8)	40.9 (49.2)
Pre-Katrina block flood exposure:		
< 2 feet	46.4 (49.9)	29.2 (45.5)
2 - 3 feet	12.8 (33.5)	17.0 (37.5)
3 - 4 feet	10.3 (30.5)	13.7 (34.4)
4 - 5 feet	9.9 (29.8)	13.1 (33.7)
5 - 6 feet	6.8 (25.2)	9.0 (28.7)
> 6 feet	13.7 (34.4)	18.1 (38.5)
Equifax risk score (spatial moving average):		
<600	23.8 (42.6)	25.8 (43.8)
600-625	18.0 (38.4)	19.6 (39.7)
625-650	16.4 (37.0)	16.1 (36.8)
650-675	12.4 (32.9)	11.7 (32.2)
675-700	10.9 (31.2)	8.7 (28.2)
700-725	9.0 (28.6)	8.6 (28.1)
>725	9.5 (29.4)	9.4 (29.2)
Home damage and insurance:		
Damage fraction (repair cost ÷ replacement cost)	0.38 (0.29)	0.50 (0.22)
Insurance fraction (insurance ÷ replacement cost)	0.22 (0.15)	0.26 (0.14)
Importance of Road Home grant formula discontinuity:		
Any HHs with damage fraction within 2 pct. pts. of RD threshold	28.2 (45.0)	37.3 (48.4)
# of HHs with damage fraction within 2 pct. pts. of RD threshold	0.56 (1.28)	0.74 (1.43)
Road Home participation:		
Nonparticipant	50.3 (30.6)	40.0 (26.2)
Rebuilding grant (option 1)	43.4 (27.1)	51.7 (23.8)
Relocation grant (option 2 or 3)	6.3 (11.1)	8.2 (12.1)
Home repaired by the pre-Katrina owner by year:		
Immediately after Katrina	34.3 (43.4)	13.0 (25.4)
1 year after Katrina	44.9 (45.2)	27.1 (37.5)
2 years after Katrina	50.3 (43.8)	34.2 (38.5)
3 years after Katrina	55.3 (41.7)	40.8 (38.0)
4 years after Katrina	66.4 (32.9)	55.6 (30.8)
5 years after Katrina	70.9 (29.4)	61.5 (27.9)
Observations:	4,795	3,622

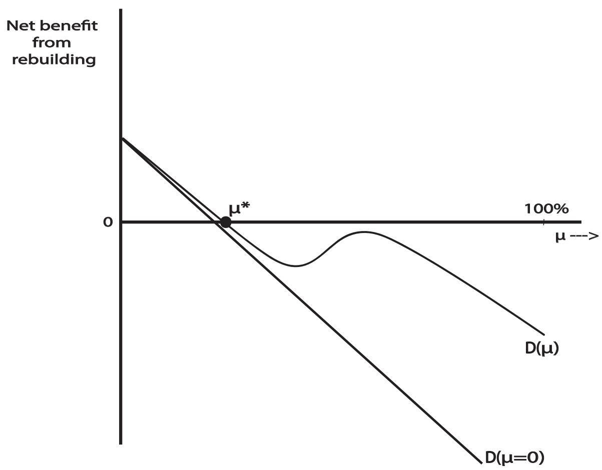
Note: This table reports summary statistics at the Census block level for the dataset analyzed in this paper. The sample used to compute the first column includes all Census blocks that contained at least five owner occupied homes in 2005. The second column excludes blocks with no initially damaged homes. The reported statistics are for the homes on those blocks that were owner occupied in 2005.

Table A2: Robustness of RDD Estimates to Alternative Specifications and Bandwidths

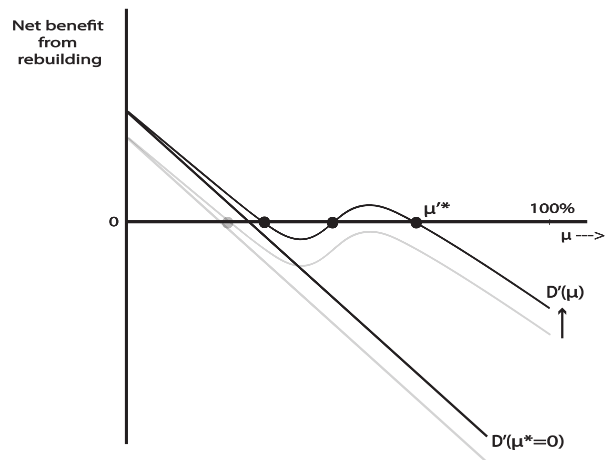
		(1)	(2)	(2)
		Dependent Variable		
Control function	Bandwidth	Opportunity cost to not rebuilding ($1,000s)	Home repaired by 5th Anniversary	Neighbors' rebuilding rate
2nd order polynomials	$R_i \in [.33,.67]$	19.627***	0.050**	0.024***
		(1.027)	(0.020)	(0.009)
Local linear regression	Rectangular kernel, bw=0.1	18.076***	0.048***	0.018**
		(0.884)	(0.018)	(0.008)
Local linear regression	Rectangular kernel, bw=0.06	17.954***	0.037*	0.024**
		(1.105)	(0.023)	(0.010)
Local linear regression	Optimal triangular kernel	19.825***	0.045***	0.021**
		(1.013)	(0.017)	(0.010)

Note: This table shows RDD estimates corresponding to those presented in Figures 1 and with alternative control function specifications and bandwidths.

Figure A1: Sketch of Equilibrium and the Possibility of "Tipping"



(a)



(b)

Note: This figure provides a stylized illustration of equilibria in our equilibrium model. Both panels plot hypothetical private demand schedules for rebuilding evaluated at the amenity level associated with a 0% rebuilding rate as well as actual marginal benefit curves. The private demand curve is downward sloping by definition as it is simply a highest-to-lowest ordering of individual households' net benefits to rebuilding. The actual marginal benefit curve incorporates each additional household's positive contribution to block amenities and can thus be downward or upward sloping. Self-consistent rebuilding rates are the zeros of the latter curve. The bottom panel illustrates how "tipping" can occur if a subsidy causes additional higher rebuilding rates to become self-consistent.

Online Appendix: Not For Publication

Appendix II. Data Imputations

To solve our model numerically, we must impute values for several of the model's exogenous variables we do not observe in our estimation dataset, which covers the full universe of homeowning households in New Orleans when Katrina occurred. This appendix describes our imputation procedures.

II.1 Wages

We impute a New Orleans annual household earnings offer (i.e. the wage offer w_i^1) and an "outside option" annual household earnings offer (i.e. the wage offer w_i^0) for each household using geocoded microdata on households' pre-Katrina labor earnings from the Displaced New Orleans Residents Survey (DNORS)[34] and information about occupation-specific differences in prevailing wages across labor markets and across time from the 2005-2010 American Community Survey. The procedure involves two steps. In the first step we match each household in our dataset to a "donor" DNORS record using nearest Mahalanobis distance matching on a set of variables that are available for all households,[35] and impute to each record the labor market variables (household head and spouse's occupations and pre-Katrina annual earnings) of its DNORS donor record. We then compute w_i^1 and w_i^0 using the expressions,

$$w_i^0 = w_{i,t<0}^{head} \left(\frac{\exp\left(\theta^0_{occ(i,head),t>0}\right)}{\exp\left(\theta^1_{occ(i,head),t<0}\right)} \right) + w_{i,t<0}^{spouse} \left(\frac{\exp\left(\theta^0_{occ(i,spouse),t>0}\right)}{\exp\left(\theta^1_{occ(i,spouse),t<0}\right)} \right)$$

$$w_i^1 = w_{i,t<0}^{head} \left(\frac{\exp\left(\theta^1_{occ(i,head),t>0}\right)}{\exp\left(\theta^1_{occ(i,head),t<0}\right)} \right) + w_{i,t<0}^{spouse} \left(\frac{\exp\left(\theta^1_{occ(i,spouse),t>0}\right)}{\exp\left(\theta^1_{occ(i,spouse),t<0}\right)} \right)$$

where $w_{i,t<0}^{head}$ is the household head's pre-Katrina annual earnings, $w_{i,t<0}^{spouse}$ is his or her spouse's pre-Katrina annual earnings (zero if the household head is single), and the terms $\theta^m_{occ,\tau}$ are log-wage indices estimated with data from the 2005-2010 ACS specific to labor markets $m \in \{0, 1\}$ (with $m = 0$ refering to the "outside" option, defined as the pooled group of all metro areas in the Census-defined South region – the typical destination of households displaced from New Orleans – and $m = 1$ referring to New Orleans) and time

[34]Fielded by RAND in 2009 and 2010, the Displaced New Orleans Residents Survey located and interviewed a population-representative 1% sample of the population who had been living in New Orleans just prior to Hurricane Katrina.

[35]To allow us to match based on the OPAO property record variables that we observe for all households, we first merged the DNORS data with respondents' OPAO property records. We then performed the matching procedure, matching on the following variables; appraised pre-Katrina home values, pre-Katrina neighborhood demographic variables, block-level flood exposure, the extent of Katrina-related home damages measured by the decline in appraised home values from 2005 (prior to Katrina) to 2006, indicators for whether and when post-Katrina home repairs occurred, and indicators for whether and when a home was sold after Katrina.

periods τ (with $\tau < 0$ referring to pre-Katrina wages and $\tau > 0$ referring to post-Katrina wages).[36]

II.2 Non-Housing Assets

We impute an initial asset holding ($A_{it=0}$) for each household using asset data from Displaced New Orleans Residents Survey and the 2005 Panel Study of Income Dynamics.[37] First, using data from the PSID, we estimate a flexible statistical model of the *distribution* of non-housing assets conditional on a household's observable characteristics. We use a logistic regression to estimate the probability that a household has zero liquid assets conditional the household's observable traits,[38] and we estimate a sequence of 99 quantile regressions (one for each quantile 1 to 99) to recover the distribution of assets conditional on the asset holding being positive. Then, using this estimated asset model, we draw 500 simulated asset holdings for each DNORS household from the *conditional* distribution of assets given the household's observable characteristics. Lastly, we match each household in our analysis dataset to a "donor" DNORS record using nearest Mahalanobis distance matching on a set of variables that are available for all households,[39] and impute to each record a random draw from the DNORS donor record's simulated asset distribution.

II.3 Home Damages for Non-Road Home Households

Lastly, we impute home replacement cost estimates and home repair cost estimates for households who did not apply to RH (and thus did not undergo RH damage appraisals). We first impute estimated replacement

[36]The composition adjusted log-wage indices $\theta^m_{occ,\tau}$ are the estimated 2-digit occupation by time period (either pre-Katrina or post-Katrina) by labor market (New Orleans or the pooled "other metro South") fixed effects from the regression,

$$\ln(earn_{i,\tau}) = X'_{i,\tau}a + \theta^m_{occ(i,\tau),\tau} + e_{i,\tau}$$

where $earn_{i,\tau}$ is a worker's annual labor earnings, measured in the 2005-2010 ACS, and X is a vector of flexibly interacted demographic and human capital variables.

[37]Liquid assets are defined to be the sum of a household's non-IRA stock holdings, bond holdings, and holdings in checking accounts, savings accounts, money market accounts, and CDs.

[38]The explanatory variables include; indicators for solo-female headed household, solo-male headed household, the more educated household head being a high school dropout, the more educated household head having attended college but not received a bachelor's degree, the more educated household head having a bachelor's degree, a household head being black, the household residing in an urban area, the household residing in the south, an interaction of southern and urban, indicators for each of the four highest housing value quintiles, the age of the male head if present and the female head's age otherwise, and the square of the age of the male head if present and the square of the female head's age otherwise. When linking these estimates back to DNORS households, all DNORS households are classified as Southern and urban. The other inputs depend on the household's survey responses.

[39]To allow us to match based on the OPAO property record variables that we observe for all households, we first merged the DNORS data with respondents' OPAO property records. We then performed the matching procedure, matching on the following variables; appraised pre-Katrina home values, pre-Katrina neighborhood demographic variables, block-level flood exposure, the extent of Katrina-related home damages measured by the decline in appraised home values from 2005 (prior to Katrina) to 2006, indicators for whether and when post-Katrina home repairs occurred, and indicators for whether and when a home was sold after Katrina.

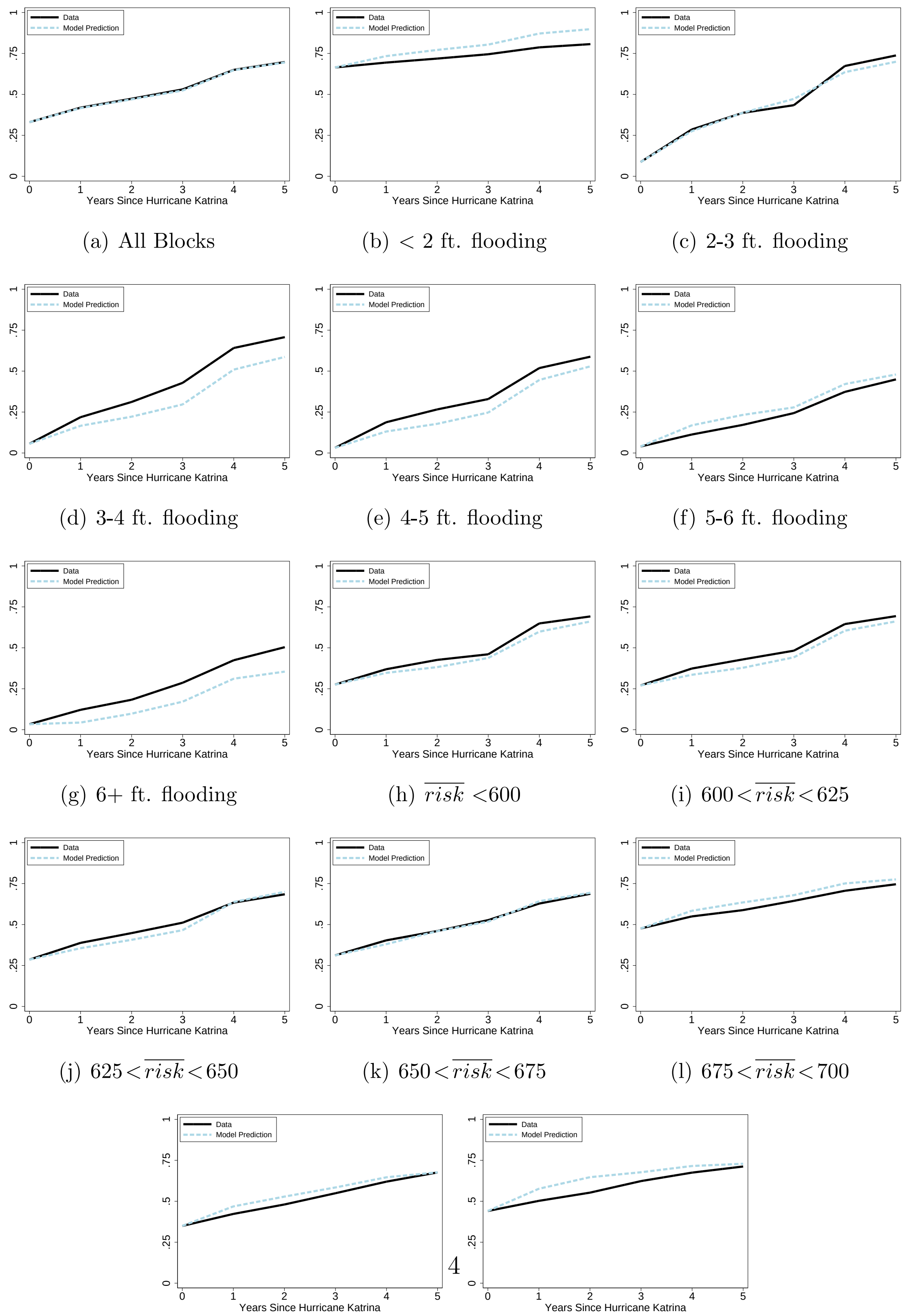
costs using the predicted values from a regression estimated among RH applicants of the log RH replacement cost estimate on log pre-Katrina appraised home value, pre-Katrina neighborhood demographic traits, and flood exposure. We then impute a damage fraction using the predicted estimate from nonlinear least squares estimates ($r^2 \approx .9$) of the statistical model:

$$\text{DamageFraction}_i = \frac{\exp(\widetilde{X}_i' a)}{1 + \exp\left(\widetilde{X}_i' a\right)}$$

where $\widetilde{X}_i$ includes a polynomial in flood exposure, a polynomial in the percentage drop in the OPAO appraised value, and interactions of the two. Note that this imputation model is a smooth function of continuously distributed exogenous variables, and thus imputed records for nonapplicants do not contribute to any observed "jumps" in outcomes at the 51% grant formula threshold.

Appendix III. Additional Model Specifications and Simulations

Figure A2: Goodness of Fit: Trends in Fraction of Homes Livable by Neighborhood Characteristics



(a) All Blocks

(b) < 2 ft. flooding

(c) 2-3 ft. flooding

(d) 3-4 ft. flooding

(e) 4-5 ft. flooding

(f) 5-6 ft. flooding

(g) 6+ ft. flooding

(h) $\overline{risk}$ <600

(i) 600 < $\overline{risk}$ < 625

(j) 625 < $\overline{risk}$ < 650

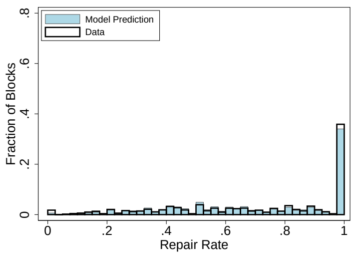
(k) 650 < $\overline{risk}$ < 675

(l) 675 < $\overline{risk}$ < 700

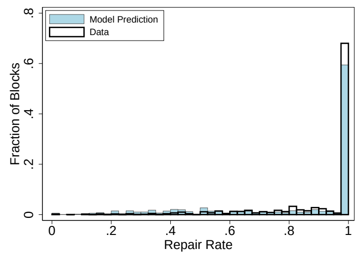
(m) 700 < $\overline{risk}$ < 725

(n) $\overline{risk}$ >725

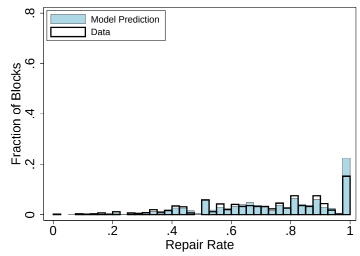
Figure A3: Goodness of Fit: Histogram of 5th-Anniv. Block Repair Rates by Neighborhood Characteristics



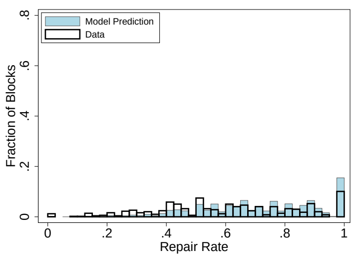
(a) All Blocks



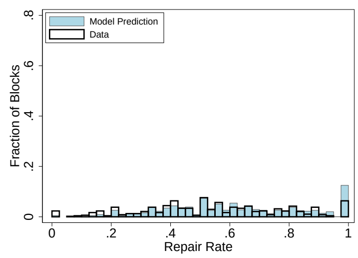
(b) < 2 ft. flooding



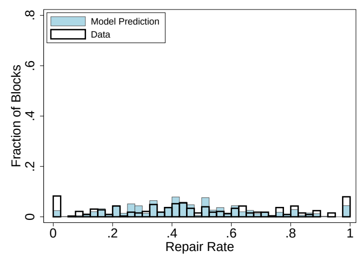
(c) 2-3 ft. flooding



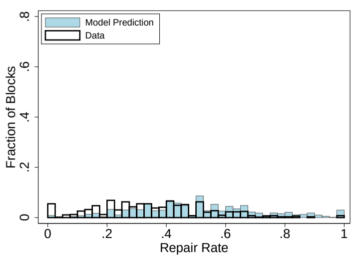
(d) 3-4 ft. flooding



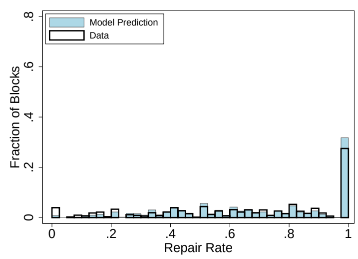
(e) 4-5 ft. flooding



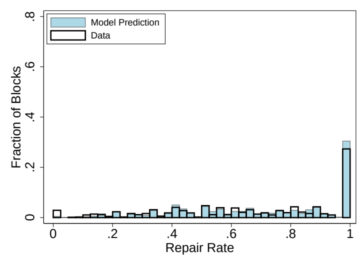
(f) 5-6 ft. flooding



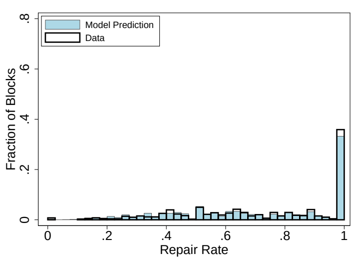
(g) 6+ ft. flooding



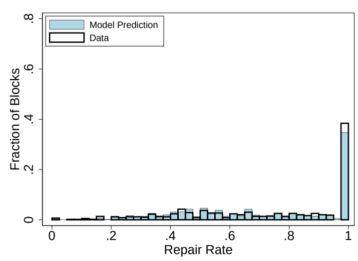
(h) $\overline{risk}$ <600



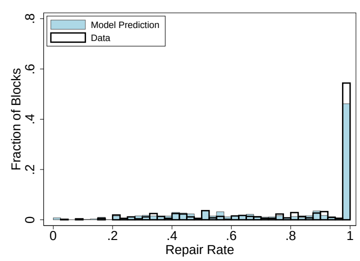
(i) $600 < \overline{risk} < 625$



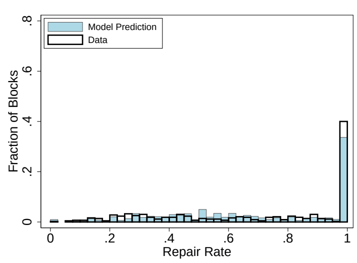
(j) $625 < \overline{risk} < 650$



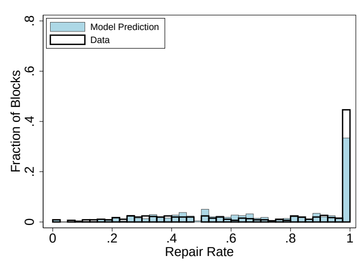
(k) $650 < \overline{risk} < 675$



(l) $675 < \overline{risk} < 700$



(m) $700 < \overline{risk} < 725$



(n) $\overline{risk}$ >725

Table A3: Goodness of Fit to Non-Targeted Moments:
5th-Anniv. Rebuilding Rate by Subgroups

Subgroup	Data	Model
Home damages: < median	84.7	83.5
	(0.21)	
Home damages: > median	54.2	55.9
	(0.29)	
Insurance payout: < median	78.8	78.9
	(0.24)	
Insurance payout: > median	60.2	60.5
	(0.28)	
Tract poverty: < median	65.8	67.8
	(0.27)	
Tract poverty: > median	73.2	71.7
	(0.26)	
Tract majority noncollege	66.0	68.4
	(0.27)	
Tract majority college	72.8	71.0
	(0.25)	
Tract majority nonblack	79.4	75.6
	(0.26)	
Tract majority black	63.0	65.9
	(0.25)	
Not low/moderate income household	70.2	70.0
	(0.24)	
Low/moderate income household	68.3	69.4
	(0.31)	
Uninsured damages	54.8	57.9
	(0.4)	
No uninsured damages	74.4	73.7
	(0.21)	

Source: Authors' calculations using the estimated equilibrium model.

Appendix IV. Additional Model Specifications and Simulations

IV.1 Model with More Flexible Specification of Amenities

To assess the model's robustness, we re-estimated the model allowing amenity utility to follow separate linear time trends within each of the six flood-depth categories. Figure A4 shows the fit of this version of the model to rebuilding time trends, which is slightly improved relative to the more parsimonious specification. the model. Table A4 compares the impact of RH on rebuilding rates in this model relative to the baseline model. The alternative specification predicts an equilibrium impact of 8.4 percentage points, compared to an impact of 8.0 percentage points in the baseline model. Table A5 compares the welfare implications of RH versus an unconditional grant policy as assessed by the two model specifications. The two models yield similar predictions about the fraction of households that are marginal (9.1% and 8.7% in the baseline and more-flexible models) and similar predictions about the per-household welfare impact of RH's distortionary structure (+\$2,177 and +\$2,754 in the baseline and more-flexible models).

Table A4: Rebuilding Rate Impacts Implied by the Baseline Model and Model that Allows Neighborhood Amenities to Follow Different Time Trends by Flood Category

	(1)	(2)	(3)
		Rebuilding Rate Impacts	
Universe	No grants Rebuilding Rate	Baseline Model	Beseline + amenity time trends by flooding
All households	61.7	+8.0	+8.4

Note: This table compares the predicted impact of RH on equilibrium rebuilding rates in the baseline model and a re-estimated version of the model that allows for households' amenity valuations to follow separate linear time trends within each of the six flood categories.

IV.2 The Impact of Removing the RH Grant Formula Discontinuity

To assess whether the discontinuity in RH's grant formula itself was an important factor in determining the program's impact, we simulated rebuilding choices under a version of RH where all grants are based on *damage* estimates (as opposed to *replacement cost* estimates). This is equivalent to calculating all grants based on the first of the two grant formulas on page 9. Table A6 compares the rebuilding rate impacts of these two versions of RH. Overall the impacts are similar, with slightly smaller impacts occurring under the "smooth" RH policy. This is because the "smooth" formula offers somewhat smaller grants for households with >51% home damage.

Table A5: Welfare Impacts in the Baseline Model and the Model that Allows Neighborhood Amenities to Follow Different Time Trends by Flood Category

	(1)	(2)	(3)	(4)
	% Marginal		Welfare Impacts ($ per capita)	
Universe	Baseline Model	Beseline + amenity time trends by flooding	Baseline Model	Beseline + amenity time trends by flooding
All households	9.1	8.7	2,177	2,754

Note: This table compares the predicted impact of RH on household welfare in the baseline model and a re-estimated version of the model that allows for households' amenity valuations to follow separate linear time trends within each of the six flood categories.

Table A6: Rebuilding Rate Impacts of a Road Home Program that Uses a "Smooth" Formula, Paying all Households Based on Damages Estimates

	(1)	(2)	(3)
		Equilibrium Rebuilding Impacts	
Subgroup	No grants Rebuilding Rate	Actual Road Home	"Smooth" Road Home
All	61.7	+8.0	+7.0
Flood depth:			
< 2 feet	76.2	+4.5	+3.9
2-4 feet	59.6	12.7	11.1
>4 feet	42.3	9.8	8.4
Rebuilding Rate w/o RH:			
80-100%	88.8	4.0	2.5
60-80%	70.3	10.0	7.9
40-60%	50.4	11.5	10.6
20-40%	33.0	11.8	10.6
0-20%	6.2	14.9	14.4

Note: This table compares the predicted impacts of RH and a RH style with a "smooth" grant formula using the ewstimated model.

IV.3 Model with More Flexible Specification of Amenities

With social spillover effects, multiple equilibria may exist (from the researcher's point of view), all of which can be computed given the structure of our model. One commonly assumed equilibrium selection rule for empirical applications is that agents agree on the equilibrium that maximizes their joint welfare, e.g., Jia (2008). We use this equilibrium selection rule because we deem it reasonable in the context of a game among neighbors. As a robustness check, we have re-estimated our model selecting the equilibrium that minimizes joint welfare. Our counterfactual experiment results remain robust, as shown in Table A7.

Table A7: RH's Equilibrium Effects on Rebuilding by Equilibrium-Selection Rule

Subgroup	(1) Baseline Model	(2) Alternative Eqm.-Selection Rule
All	+8.0	+7.6
Flood depth:		
< 2 feet	+4.5	+4.4
2-3 feet	+14.1	+13.1
3-4 feet	+11.2	+10.5
4-5 feet	+12.6	+11.4
5-6 feet	+9.3	+8.8
>6 feet	+8.0	+7.6
Rebuilding Rate w/o RH:		
90-100%	+0.2	+0.2
80-90%	+5.3	+5.0
70-80%	+8.8	+7.7
60-70%	+11.0	+9.9
50-60%	+11.2	+10.3
40-50%	+11.8	+11.4
30-40%	+11.7	+11.5
20-30%	+11.9	+11.1
10-20%	+14.7	+13.4
0-10%	+14.9	+12.7

Note: This table compares the simulated equilibrium impacts of the Road Home grant program on rebuilding rates using the baseline model (column 1), which assumes that the total-welfare-maximizing equilibrium is selected on blocks with multiple self-consistent equilibria, to the simulated impact of RH using a (re-estimated) version of the model that assumes the total-welfare-*minimizing* equilibrium occurs in such cases (column 2). Source: Authors' calculations using the estimated equilibrium models.

Appendix V. Identification

We show identification of a simplified, one-period version of our model. Given our model assumption that neighborhood and household unobservables are permanent, having multiple-period data will only help identification.

V.1 Simplified Model

Households face a discrete choice of whether to rebuild and receive u_{i1} or relocate and receive u_{i0}:

$$u_{i1} = \ln c_1(z_i) + g(\mu_{j(i)}) + x_j'\beta + b_{j(i)} + \epsilon_{ij} \tag{13}$$

$$u_{i0} = \ln c_0(z_i), \tag{14}$$

where z_i is household characteristics or household-level incentive shifters. In our context, the exogenous incentive shifter z is an indicator that a household's rebuilding cost assessment falls above the policy formula discontinuity. $j\,(i)$ is the neighborhood that i belongs to, x is neighborhood observable characteristics, such as flood exposure. $\mu_{j(i)}$ is rebuilding rate in the neighborhood. b is unobservable neighborhood characteristics, ϵ_{ij} is household's idiosyncratic taste for moving back

Household i will move back $d_i = 1$ if $u_{i1} > u_{i0}$, therefore, the following holds:

$$\Pr(d_i = 1|x, z, \mu) = F_{b+\epsilon}\Big(\underbrace{\ln c_1(z_i) - \ln c_0(z_i)}_{\Delta c(z_i)} + g(\mu) + x_j'\beta\Big).$$

This implies the expected rebuilding rate in the neighborhood is determined by x and the distribution of z in neighborhood $j\,(i)$. In our context, given that z is an indicator function, the average of z would serve as a sufficient summary statistic, which we denote by $Z_{j(i)}$. Therefore, the expected rebuilding rate is given by $\mu\left(x_{j(i)}, Z_{j(i)}\right)$.

V.2 Identification

Assumptions:

1. ϵ_{ij} is independent of (z, b, x).
2. z_i is independent of $b_{j(i)}$ for all i, and thus $Z_{j(i)}$ is independent of $b_{j(i)}$.
3. x is independent of b.

Claim: Given Assumptions 1 to 3, marginal rate of substitution between neighbors' rebuilding $\mu(x_{j(i)}, Z_{j(i)})$ and private consumption c; MRS = $\dfrac{\Delta u}{\Delta\mu(x_{j(i)}, Z_{j(i)})} \Big/ \dfrac{\Delta\mu(x_{j(i)}, Z_{j(i)})}{\Delta c}$ is identified.

Proof. The attractiveness of a block varies with x, which generates variation in expected rebuilding rates $\mu(x_{j(i)}, Z_{j(i)})$. We can trace out the spillover function $g(\mu)$ by performing the following calculation over a range of values of the exogenous vector x that yield different predicted rebuilding rates $\mu(x_{j(i)}, Z_{j(i)})$ and exploiting experimental variation in z_i and $Z_{j(i)}$ (the discontinuity)

$$Pr(d_i = 1|X, Z) = F_{b+\epsilon}\Big(\underbrace{\ln c_1(z_i) - \ln c_0(z_i)}_{\Delta c(z_i)} + g(\mu(x_{j(i)}, Z_{j(i)})) + x_j'\beta\Big)$$

$$\begin{aligned}\delta_1(x, z, \overline{z}) = \frac{\Delta Pr(d_i = 1|X, Z)}{\Delta z_i} &\approx F'_{b+\epsilon}\Big(\underbrace{\ln c_1(z_i) - \ln c_0(z_i)}_{\Delta c(z_i)} + g(\mu(x_{j(i)}, Z_{j(i)})) + x_j'\beta\Big) \\ &\times \Big(\frac{\Delta c(z_i)}{\Delta z_i} + \underbrace{\frac{\Delta E(b_{j(i)}|z_i)}{\Delta z_i} + \frac{\Delta E(\epsilon_i|z_i)}{\Delta z_i}}_{\text{assumed}=0}\Big)\end{aligned} \tag{15}$$

$$\begin{aligned}\delta_2(x, z, \overline{z}) = \frac{\Delta Pr(d_i = 1|X, Z)}{\Delta Z_{j(i)}} &\approx F'_{b+\epsilon}\Big(\underbrace{\ln c_1(z_i) - \ln c_0(z_i)}_{\Delta c(z_i)} + g(\mu(x_{j(i)}, Z_{j(i)})) + x_j'\beta\Big) \\ \times \Big(\frac{\Delta g(\mu(x_{j(i)}, Z_{j(i)}))}{\Delta\mu} &\times \underbrace{\frac{\Delta\mu(x_{j(i)}, Z_{j(i)})}{\Delta Z_{j(i)}}}_{\delta_3(x,z,\overline{z})} + \underbrace{\frac{\Delta E(b_{j(i)}|\overline{z})}{\Delta\overline{z}} + \frac{\Delta E(\epsilon_i|\overline{z})}{\Delta\overline{z}}}_{\text{assumed}=0}\Big)\end{aligned} \tag{16}$$

Note that many variables in the vector X vary continuously, letting us nonparametrically identify $E\big(g(\mu(x_{j(i)}, Z_{j(i)}))\big)$.[40] Each of these three δ's are nonparametrically identified by flexibly measuring how these conditional probabilities depend on the right hand side variables (x, z, Z). The marginal rate of substitution is thus as well, given by,

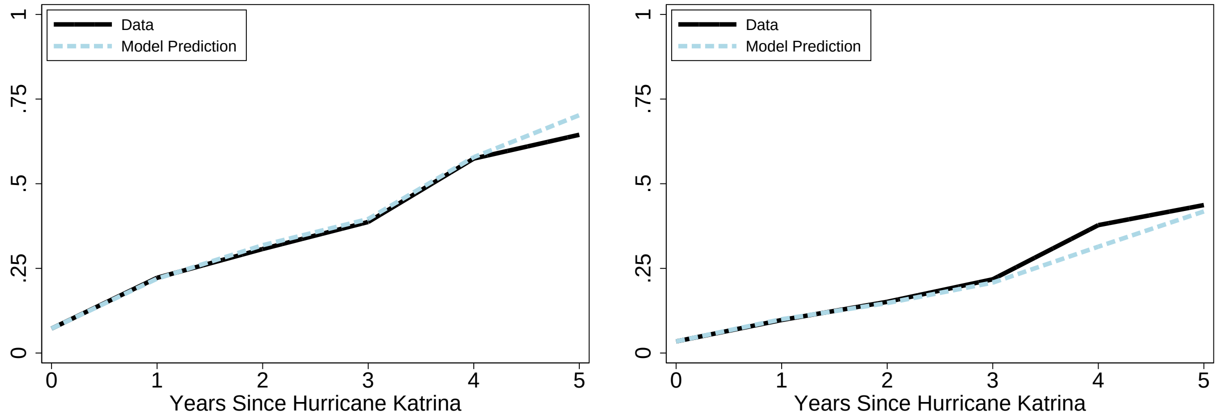
$$MRS(x, z, Z) = \frac{\delta_2(x, z, Z)/\delta_3(x, z, Z)}{\delta_1(x, z, Z)}.$$

■

Note that this expression is only consistent for MRS $= \frac{\Delta u}{\Delta\mu(x_{j(i)}, Z_{j(i)})} \Big/ \frac{\Delta\mu(x_{j(i)}, Z_{j(i)})}{\Delta c}$ under the assumption that the incentive shifters z_i and $Z_{j(i)}$ are uncorrelated with ϵ_i and $b_{j(i)}$, as illustrated with the above equations (15) and (16) with the terms labeled "assumed=0," which motivated us to exploit quasi-experimental variation to the incentives of households and neighboring households that, as shown in the body, appears consistent with these assumptions.

[40] We use Δ's instead of partial derivatives because the discontinuity we exploit as an instrument can generate large jumps in rebuilding incentives in some cases.

Figure A4: Goodness of Fit: Trends in Fraction of Homes Livable Predicted by the Model Allowing for Flood-Category-Specific Amenity Time Trends

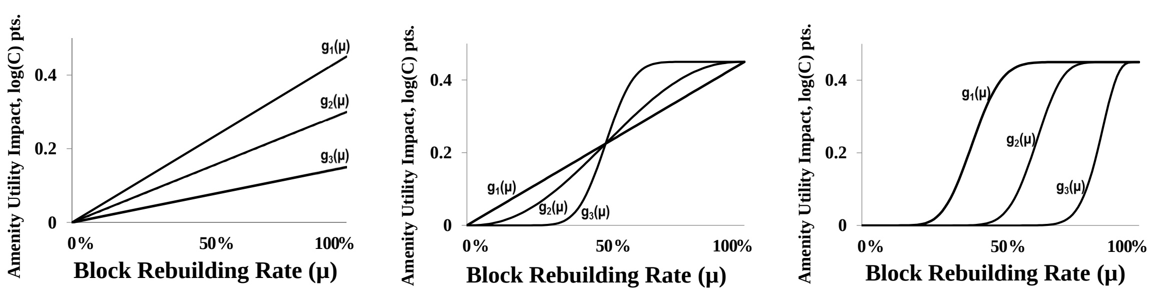


(a) 2-3 ft. flooding

(b) > 4 ft. flooding

Source: Authors' calculations using a re-estimated version of the model that allows for households' amenity valuations to follow separate linear time trends within each of the six flood categories.

Figure A5: Parameterization of the Amenity Spillover Function



(a) Amplitude: $S^{(3)} > S^{(2)} > S^{(1)}$

(b) Shape: $\lambda_2^{(3)} > \lambda_2^{(2)} > \lambda_2^{(1)}$

(c) Location $\lambda_1^{(3)} > \lambda_1^{(2)} > \lambda_1^{(1)}$