Structural Estimation of an Equilibrium Model with Externalities: Program Evaluation of Post-Katrina Rebuilding Grants*

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Abstract

We develop and estimate an equilibrium model of New Orleans homeowners' post-Hurricane Katrina rebuilding choices in which neighborhood amenity values depend endogenously on households' rebuilding choices. Using administrative program participation data from the Louisiana Road Home rebuilding grant program and exploiting a discontinuity in the Road Home program's grant formula, we first compute reduced form estimates of the impact of neighbors' rebuilding choices on the probability that a household rebuilds. Treating these reduced form estimates as target auxiliary models, we estimate the equilibrium model's structural parameters by indirect inference. We find that post-Katrina rebuilding generated quantitatively important spillover effects. The requirement that homeowners must rebuild their pre-Katrina homes in order to receive the Road Home program's most generous grant package increased the rebuilding rate by about three percentage points relative to a policy that provided these grants unconditionally. This additional rebuilding by marginal households generated amenity improvements with an annual flow value to inframarginal households equivalent to 20% of the cost of grants to marginal households.

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1 Introduction

In recent decades, government disaster relief packages in the United States have regularly included grant payments to individual property owners as compensation for uninsured losses. Often times these grant payments are made with the condition that the grant be used to rebuild in a particular location. The precedent of regularly bailing out disaster victims represents a *de facto* social insurance policy with the usual tradeoff between short-run welfare improvements, generated by the transfers addressing failures in insurance and lending markets, and efficiency losses generated by policy-induced moral hazard in households' location choices (Gregory (2013)). But at first glance the policy of conditioning grants on particular rebuilding locations seems unambiguously misguided. Constraining the set of possible resettlement locations available to a grant recipient must (weakly) reduce the recipient's welfare all else equal.

In standard models the strongest plausible rationale for requiring disaster relief grants to be used to rebuild in a specific location is the possibility that rebuilding generates positive spillovers. Rebuilding a home instead of leaving it blighted presumably makes the surrounding area a more attractive place to live, and these sorts of externalities are often not internalized by individual households. As a consequence, it is plausible that rebuilding rates might be inefficiently low in certain areas without location-tied subsidies. The wisdom of attaching rebuilding location requirements to grant payments thus depends on the value of amenity improvements caused by any additional rebuilding relative to the excess burden associated with distorting privately optimal resettlement choices.

In this paper, we develop and estimate an equilibrium model of neighbors' post-disaster rebuilding choices that embeds the possibility that the density of rebuilding on a block contributes to the block's amenity valuation. We study the case of home reconstruction in New Orleans after Hurricane Katrina and in particular the equilibrium effects of the Louisiana Road Home program, which offered rebuilding grants and somewhat less generous relocation grant packages to all affected homeowners with uninsured losses in the state. We use the model to quantify the rebuilding grant program's full equilibrium impact, including "feedback" effects from positive amenity spillovers. More importantly, we use the model to study the consequences for welfare and for equilibrium rebuilding outcomes of counterfactual policies that place more or less stringent requirements on households' resettlement locations to receive various grant packages.

In the model we consider, households have private preferences for consumption and for residing in their pre-Katrina home and derive utility from a neighborhood amenity that depends on the fraction of neighbors who rebuild. In each period, households who have not yet rebuilt decide whether or not to do so. Households' rebuilding decisions are inter-related because of amenity spillovers, the shape and scale of which are embedded in the structure of the model. An equilibrium requires that households' decisions are best responses.

Although the nature of spillovers is key to the impacts of many policy interventions, identifying spillover effects is a fundamentally difficult task (Manski, 1993). It is often difficult, and in some settings impossible, to determine whether an observed correlation between peers' choices occurs because of social spillovers or because peers are influenced by common factors that are unobservable to the researcher. We exploit a discontinuity in the Road Home program's grant calculation formula that breaks this identification problem. Whether a household falls slightly above or below this discontinuity strongly influences the household's incentive to rebuild but has no direct effect on the household's neighbors' incentives to rebuild. Spillover effects are thus identified by comparing the rebuilding rates of the neighbors of otherwise similar households induced by this program quirk to make different rebuilding choices. Our empirical analysis begins by computing reduced form treatment effect estimates based on this research design. We then treat these reduced form estimates as the "targets" for the estimation of the equilibrium model, which embeds this program artifact.

We study a unique data set constructed from two primary sources. The first source is the administrative records of the Louisana Road Home program that include the components of the program's grant size determination formula, the program participation decisions of all pre-Katrina New Orleans homeowners, and information about households the pre-Katrina circumstances, storm-related home damage, and private insurance. We link these records to the Orleans Parish Assessor's Office administrative property database, which provides records of post-Katrina home sales and home repairs for the universe of homes that were owner occupied before Katrina. We also incorporate data on block-level flood exposure based on satellite observations from the Federal Emergency Management Agency (FEMA), information on the distribution of occupations, wages, insurance payouts, and other measures from the Displaced New Orleans Residents Survey, and information on neighborhood demographic composition from the 2000 decennial Census.

We estimate the equilibrium model via indirect inference. This approach involves repeatedly simulating data with the structural model, computing auxiliary models in both the simulated data and the true data, and searching for the model parameters that most closely match the auxiliary model estimates in the simulated data and the true data. Like many other discrete choice models, our estimation is complicated by the fact that our objective function can be discontinuous in parameter values. To facilitate the estimation, we augment our procedure with an importance sampling technique that smooths the objective function (Sauer and Taber (2012)).

We find evidence of large and economically important spillover effects. Our RD estimates find that on average, exogenously inducing one neighbor to return and rebuild who otherwise would not have causes more than one *additional* household to return and rebuild. Consistent with related work (Gregory, 2013), policy simulations performed with our estimated structural model find that the Road Home program's financial incentives significantly increased rebuilding in post-Katrina New Orleans, though, strikingly, the Road Home program's full equilibrium impact is nearly twice as large as that suggested by partial equilibrium policy experiments that measure the impact of the Road Home program's private incentives alone. Finally, we find that the additional rebuilding induced by the Road Home option 1's rebuilding requirement generated amenity improvements with a value to inframarginal households (those with undamaged homes or who would have rebuilt anyways) that exceeds previous estimates of the efficiency costs associated with guaranteed post-disaster bailouts.

This paper relates to a growing literature on spillover effects in the context of housing. Rossi-Hansberg, Sarte, and Owens (2010) study spillover effects from targeted urban revitalization subsidies, finding evidence of significant but highly localized positive spillovers. Autor, Palmer, and Pathak (2012) study the removal of rent controls in Cambridge, Massachusetts and find significant positive impacts on the prices of never-controlled properties, presumably caused by spillover effects from increased investment in previously rent controlled properties. Campbell, Giglio, and Pathak (2011) and Harding, Rosenblatt, and Yao (2009) study contagion effects from "forced" home sales and foreclosures, finding modest spillover effects that diminish rapidly with distance from the distressed property. Others have attempted to quantify gentrification by measuring the housing price effects of proximity to other high priced housing (Guerrieri, Hartley, Hurst, 2013; Ioannides, 2003), finding that closer proximity to a high-priced neighborhood leads to higher housing prices. We contribute to the literature on externalities in housing markets by studying a disruption and policy response that are large enough to identify non-linearities in housing spillovers, and by estimating an equilibrium model of housing investments that allows for counterfactual policy experiments and welfare analysis.

This paper is also related to the narrower literature studying the post-Hurricane Katrina locations, labor market outcomes, and wellbeing of displaced New Orleans residents (Groen and Polivka, 2010; Zissimopolous and Karoly, 2010; Vigdor, 2007 and 2008; Paxson and Rouse, 2008; and Elliott and Pais, 2006). The present paper relates most closely to Gregory (2013), which estimates a partial equilibrium structural model of New Orleans homeowners' resettlement choices and uses the model to study the extent to which post-disaster bailouts improve welfare by relaxing financing constraints and the extent of efficiency loss caused by expected future bailouts distorting households' location choices. This paper studies a larger dataset of administrative program records, which allows a more transparent identification strategy (regression discontinuity). Also, this paper studies a model that embeds equilibrium amenity spillovers, allowing us to quantify that important component of the welfare effects of this class of policies.

Like many other coordination games, our model admits multiple equilibria. There have been many studies on the identification and estimation of these games without imposing equilibrium selection rules, e.g., Bresnahan and Reiss (1990), Tamer (2003) and Ciliberto and Tamer (2009).[1] A different approach, the one that we follow, is to introduce an equilibrium selection mechanism that specifies which equilibrium is picked as part of the econometric model. For example, Bjorn and Vuong (1984) select an equilibrium at random. Jia (2008) assumes that the data is generated from an extremal equilibrium, one that is most profitable for one player. Given that the game we study is one that is among neighbors, we think it is reasonable to assume that the Pareto dominant equilibrium is selected in cases where multiple equilibria exist.

The rest of the paper is organized as follows: Section 2 provides additional policy background. Section 3 describes the structural equilibrium model. Section 4 describes estimation. Section 5 describes our dataset. Section 6 describes our reduced reduced form estimates and estimates of the model's structural parameters. Section 7 describes the results of counterfactual experiments, and section 8 concludes.

2 Hurricane Katrina and the Road Home Program

Hurricane Katrina struck the U.S. Gulf Coast on August 29, 2005. In the days following the storm's initial impact, the levees that protect New Orleans gave way in several places, allowing flood waters to cover roughly 80% of the city (McCarthy et al., 2006). 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 most 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.

In the months following Hurricane Katrina, Congress approved supplemental relief block grants (Community Development Block Grants) to the Katrina-affected states.[2] Possible uses of these grants were hotly debated, with proposals ranging from mandated buyouts that would have effectively closed some neighborhoods to universally subsidized reconstruction. The state of the Louisiana decided to use its federal allocation to create the Louisiana Road Home program, a program designed to assist pre-Katrina Louisiana homeowners by

[1] See de Paula (2013) for a comprehensive survey.

[2] Congress has regularly provided large Community Development Block Grants to local and state governments to assist with disaster recovery. Localities typically have considerable discretion over the use of these grants.

providing cash grants for rebuilding or relocating that did not need to be repaid.[3] The Road Home program was announced in February, 2006. Long delays occurred at multiple stages of the application process, and most homeowners experienced lengthy delays between initiating their grant application and receiving a grant. The median grant payment date occurred after Katrina's second anniversary. Despite the program's slow rollout, the program disbursed nearly ten billion dollars to Louisiana homeowners by Katrina's fifth anniversary.

The Road Home program offered three main participation options. Option 1 offered grant compensation to households who chose to rebuild. This option paid a cash grant equal to the estimated value of property damages minus the value of any private insurance payouts up to a maximum of \$150,000.[4] The program estimated the value of property damages by computing both an estimated replacement cost ($\widehat{\$\text{Replace}}$) and and estimated repair cost ($\widehat{\$\text{Repair}}$) and applying the decision rule,[5]

$$\text{Road Home Damage Estimate} = \begin{cases} \widehat{\$\text{Repair}} & \text{if} \quad \widehat{\$\text{Repair}} < 51\% \times \widehat{\$\text{Replace}} \\ \widehat{\$\text{Replace}} & \text{if} \quad \widehat{\$\text{Repair}} \geq 51\% \times \widehat{\$\text{Replace}} \end{cases} \tag{1}$$

That is, property damages were valued at the cost of replacing the home when the estimated repair cost was more than 51% of the estimated replacement cost, and were valued at the estimated cost of repairing the home otherwise. Homeowners who accepted an option 1 grant were required to repair and reside in the pre-Katrina home for at least three years and to purchase any required flood insurance.

Road Home options 2 and 3 offered grant compensation to households choosing not to rebuild. Recipients of option 2 and option 3 grants were required to transfer their homes to a state land trust in return for grant compensation. The option 2 grant paid the same cash award as the option 1 rebuilding grant (estimated value of property damages minus the value of any private insurance payouts) and required the recipient to purchase another home in Louisiana. Option 3 imposed no location or home-purchase requirement but paid a grant that was 40% smaller.

The Road Home program generated a strong incentive to rebuild for many households.

[3] Specifically, the Road Home program was funded through a U.S. Department of Housing and Urban Development Community Development Block Grant and was administered by the Louisiana Office of Community Development.

[4] When the Road Home program was first announced, its grant determination formula placed a cap on grant payments equal to the smaller of \$150,000 and the pre-Katrina market value of the house. Citing that program provision, a group of plaintiffs sued HUD and the State of Louisiana alleging that this formula had a disparate negative impact on black households, because pre-Katrina property values were lower in black neighborhoods holding housing quality constant. In response to this suit, the Road Home program waved the cap based on pre-Katrina market values for low- and moderate-income homeowners.

[5] The Road Home program calculated "replacement cost" estimates as the home's floor area times \$130. The program calculated "repair cost" estimates using a program formula that attached a dollar value to each item or room type found to be damaged during an in-person home inspection.

A household who accepted a Road Home relocation grant was compensated for the value of uninsured property damages but received no additional compensation for the as-is value of their property. A household who sold its damaged home privately received the as-is market value of the property (or the repaired value of the home after paying out of pocket for repairs) but no additional compensation for the value of uninsured damages. Both of these options for selling one's home thus entailed an opportunity cost, because a household who accepted a Road Home rebuilding grant maintained ownership of their property *and* received grant compensation for the value of uninsured damages.

Our empirical analysis exploits the fact that otherwise similar households faced significantly different incentives to rebuild as a consequence of the Road Home program's discontinuous grant calculation formula. Figure 1 illustrates this idea by plotting the as-is home value and the Road Home grant offer by home damage level for a hypothetical household. The horizontal axis plots the home's "damage fraction" (the ratio of repair cost to replacement cost). When damages are minor, selling privately tends to entail the smaller opportunity cost, because the value of the foregone grant compensation is small. When damages are more severe, the opportunity cost of selling privately is larger, and the opportunity cost of accepting a relocation grant is smaller. Because of the discontinuity in the Road Home grant formula, the opportunity cost of not rebuilding jumps significantly at the 51% damage threshold.

3 Model

We consider a model in which displaced households (homeowners) make dynamic decisions about moving back to (and rebuilding) their pre-Katrina home.[6] 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 to 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 (the extent of flood damages, infrastructure repairs, etc.), and (v) the influence of neighbors' rebuilding choices on the attractiveness of the neighborhood.

[6]Moving back and rebuilding are defined as one indivisible action.

3.1 Primitives

There are I_j households ($i = 1, ..., I_j$) in a community/block j, and each community is a closed economy.[7] Let $j(i)$ be the block household i belongs to. Time t starts from $t = 0$ when Hurricane Katrina occurs. Each household lives forever but has the option to rebuild each period only from 1 to T. 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.

3.1.1 Housing-Related Costs

Several housing-related costs and prices influence the financial consequences of rebuilding relative to staying away; 1) i's remaining mortgage balance when Katrina occurred ($M_i \geq 0$); 2) the replacement value of i's home (the physical structure) based on its size and quality (p_i^s) ; 3) the cost of repairing/restoring the home from it's damaged state ($k_i \leq p_i^s$); 4) the market value of the house if sold privately p_i, 5) the value of insurance payments received ($ins_i \leq k_i$); and 6) the additional incentives created by the Road Home program.

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. Households who rebuild are reimbursed for uninsured damages by a Road Home (option 1) grant $G_{1i} = \min(\$150,000, k_i - ins_i)$. Reflecting the Road Home program's slow rollout, grants are dispersed at $t = 2$ 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 pre-Katrina home either through the Road Home program (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 value of the structure (p_i^s), its damage (k_i), neighborhood characteristics *and the rebuilding status of the neighborhood* μ_j.

3.1.2 Labor Market Opportunities

Households differ in their human capital levels (h_i) and occupations (o_i). Let $r^1(o_i)$ and $r^0(o_i)$ represent occupation-specific rental rates of human capital in New Orleans and away from New Orleans. A household with occupation o_i and human capital levels h_i faces the following wages,

$$w_i^l = r^l(o_i)h_i, \text{ for } l = 0, 1.$$

[7] It will be interesting to embed our model into a general equilibrium framework that treats the whole region as one economy. We leave this extension for future work.

3.1.3 Attachment to Community

Households differ in the strength of the attachment to their community (η_i), which stands for their private non-pecuniary incentives to return home.

3.2 Household Problem

A household derives utility from consumption (c), neighborhood amenities, and 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_j) of neighbors who rebuild. Assuming that rebuilding is an absorbing state, household i's per-period utility payoffs are characterized by,

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

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.[8]

We model rebuilding as an absorbing state. The value of discounted (at rate β) remaining lifetime utility for households who have already rebuilt at the begining of period t is,

$$V_{it}^{1}\left(\mu_{j(i),t-1}\right) = \sum_{t' \geq t} \beta^{t'-t}\ v_{it'}\left(\mu_{j(i),t'}; 1\right), \tag{2}$$
$$s.t.\ \mu_{t'} = \Gamma_{jt'}(\mu_{t'-1})\text{for all } t' \geq t.$$

where $\Gamma_{jt}(\mu)$ is the endogenous law of motion for μ.

Selling one's house is also an absorbing state. The value of discounted remaining lifetime utility for households who have sold their houses by the beginning of period t is,

$$V_{it}^{-1}\left(\mu_{j(i),t-1}\right) = \sum_{t' \geq t} \beta^{t'-t}\ v_{it'}\left(\mu_{j(i),t'}; -1\right). \tag{3}$$

At each period $t \in \{1, 2, .., T\}$, households that have not rebuilt or sold their houses make their decisions after observing the fraction $\mu_{j(i),t-1}$ of neighbors who had already

[8] A non-decreasing spillover function rules out crowding effect, which is reasonable in our framework as the number of residents will not exceed the pre-disaster equilibrium level.

moved back by period $t-1$. The value function for such a household is

$$V_{it}^{0}\left(\mu_{j(i),t-1}\right)=\max\left\{\begin{array}{l} v_{it}\Big(\mu_{j(i),t};0\Big)+\beta V_{it+1}^{0}\left(\mu_{j(i),t}\right), \\ V_{it}^{-1}\left(\mu_{j(i),t-1}\right), \\ V_{it}^{1}\left(\mu_{j(i),t-1}\right) \end{array}\right\} \tag{4}$$

$$s.t.\ \mu_t=\Gamma_{jt}\left(\mu_{t-1}\right)$$

Finally households who have not rebuilt by time T are assumed to derive outside-option utility from then on, so $\Gamma_{jt}\left(\mu_T\right)=\mu_T$ for all $t>T$, and

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

where the last equality follows trivially as a permanently staying-away household derives no value from a house that is unsold.

Remark 1 *Notice that the only feasible changes in d_{it} over time are $0\rightarrow 1$ or $0\rightarrow -1$. As such, $d_{it}>d_{it-1}$ is equivalent to rebuilding in period t; and $d_{it}<d_{it-1}$ is equivalent to selling in period t.*

3.2.1 Intertemporal Budget Constraint/Financing Constraints

The household intertemporal budget constraint is,

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

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.

Finally, each household is characterized by an Equifax credit risk score $risk_i$. Households with risk scores above a threshold ρ^* may borrow to finance home repairs, and households with risk scores below ρ^* are ineligible for rebuilding loans.

$$A_t \geq \begin{cases} 0 & \text{if } risk_i < \rho^* \\ -\infty & \text{if } risk_i \geq \rho^* \end{cases} \tag{5}$$

3.3 Equilibrium

Definition 1 *Given the terminal value functions $\{V_{i,T+1}(\cdot)\}_{i\in I_j}$, 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 $\left\{\mu_{j,t}\right\}_{t=1}^T$ and $\{\Gamma_{jt}(\cdot)\}_{t=1}^T$, $\left\{\left\{d^*_{it}(\cdot)\right\}_{t=1}^T\right\}_{i\in I_j}$ comprise optimal decisions.*

(b) *The laws of motion $\{\Gamma_{jt}(\cdot)\}_{t=1}^T$ are consistent with individual choices such that,*

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

(c) *Equilibrium rebuilding rates $\{\mu_t\}_{t=1}^T$ follow that law of motion, such that,*

$$\mu_{j,t} = \Gamma_{jt}(\mu_{j,t-1}) \text{ for all } t.$$

Because of the presence of social spillover effects, multiple equilibria may exist (from the researcher's point of view) on any given block. One commonly assumed equilibrium selection rule for empirical applications of equilibrium models is that agents agree on the equilibrium that maximizes their joint welfare. We deem this equilibrium selection rule to be a reasonable one in the context of a game among neighbors, and apply this selection rule in our empirical analyses.

4 Empirical Implementation and Estimation

4.1 Further Empirical Specifications

In the following, we introduce further specifications of the model used in our empirical analysis.

4.1.1 Information

All information is public in the model, but not all model components are observable to the researcher. At the household level, a household's occupation (o_i), human capital (h_i) and level of attachment to the community (η_i) are unobservable to the researcher. We model the distribution of (o_i, h_i) as correlated with observable household characteristics x_i, and distributed as $H(o_i, h_i|x_i)$. Idiosyncratic home attachment η_i is assumed to be drawn from i.i.d. $N(0, \sigma_\eta^2)$. Denote the block-specific distribution of household observable characteristics with $Q_j(x)$.[9]

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 characteristics (z) that are observable to the researcher, including flood exposure, pre-Katrina demographic composition, and a linear time trend to capture city-wide improvements in infrastructure. $b_j \backsim N(0, \sigma_b^2)$ is a random effect that captures heterogeneity in block amenity values that are not observable to the researcher.

4.1.2 Amenity Spillovers

The amenity spillover function is given by

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

where 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 $\lambda = [\lambda_1, \lambda_2]'$. λ_1 measures the location of a steeper spillover "threshold" region, and λ_2 measures the steepness of that threshold.[10]

[9] For estimation, we stochastically impute a set of exogenous variables to each household using a procedure that exploits the much more detailed information available to us for the approximately 1% of households who completed Displaced New Orleans Residents Survey interviews. Details are in the appendix.

[10] The Beta CDF is a convenient choice, because it has support over the unit interval and nests many shapes. For instance $[\lambda_1, \lambda_2] = [1, 2]$ leads to a spillover function $g(\mu) = S \times \mu$. By setting λ_2 sufficiently

4.1.3 House Sales Prices

We model the offered price of a house as given by

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

where, p_i^s is the structure's replacement value, $k_i \leq p_i^s$ is the cost of the repairs needed to fully restore the structure, $z_{j(i)}$ is a vector of exogenous observable block characteristics, $\mu_{j(i),T}$ is the *endogenous* block rebuilding rate at time T, and e_i is a residual capturing the market's valuation of unobserved characteristics of i's the property. $\mu_{j(i),T}$ enters the pricing function as house buyers are forward looking and care about the future amenity in the neighborhood.[11]

4.1.4 Rebuilding Load Credit Risk Score Threshold

Our data from Equifax contain spatial moving averages of credit risk scores within 1/4-mile radius buffers, but do not contain individual level risk scores. The within-buffer standard deviation of risk scores is 85, so we model individual risk scores as draws from

$$risk_i \sim N(\overline{risk}_{buf(i)}, 85)$$

where $\overline{risk}_{buf(i)}$ is the average risk-score within the 1/4-mile buffer centered around household i's block centroid.

4.2 Indirect Inference Estimation

The vector of structural parameters (θ) to be estimated consists of the dispersion of household attachment (σ_η), the parameters governing exogenous block-specific amenity values (γ, σ_b), the parameters governing the nature of amenity spillovers (S, λ), and the parameters governing the borrowing interest rate function (ρ).

The estimation is via indirect inference. The approach involves two stages. The first step is to compute from the data a set of "auxiliary models" that summarize the patterns in the data to targeted for the structural estimation. The second step involves repeatedly simulating data with the structural model, computing corresponding auxiliary models using the simulated data, and searching for the model parameters that most cause the auxiliary

high, this parameterization can generate a spillover function that is arbitrarily close to 0 for $\mu < \lambda_1$ and arbitrarily close to S for $\mu > \lambda_1$. Less extreme non-linear functions occur at intermediate values of S.

[11] A more flexible specification will allow the price to be period-specific and to depend on all future rebuilding rates (e.g., price at time t depends on $\left\{\mu_{j(i),t'}\right\}_{t'=t}^{T}$). However, this may make the incentive to move back non-monotone over time, leading to great complications in solving the model. Given that our focus is not on the housing market, we leave this flexible specification for future work.

model estimates computed from the simulated data and from the true data to match as closely as possible. Indirect inference is well-suited for estimating models like ours that are straightforward to simulate (under any particular parameterization) but for which it is difficult to evaluate a likelihood function or a set of model-implied moments directly.

4.2.1 Stage One: Choice of Auxiliary Models

The auxiliary models the we target include:

1. Regression discontinuity design (RDD) estimates of the causal effect of financial incentives on a household's choice to rebuild, and RDD estimates of the causal effect of one household rebuilding on the probability that its neighbors rebuild.

2. OLS estimates summarizing the raw partial correlations between same-block neighbors' rebuilding choices.

3. Fixed effects (FE) estimates summarizing the partial correlation between same-block neighbors rebuilding choices after controlling for the common effect of unobserved tract amenities.

4. Aggregate rebuilding rates on the 1st, 2nd, 3rd, 4th, and 5th anniversaries of Katrina.

Details regarding the specification of these models follow now.

1. RDD Models: Recall that the Road Home program estimated a replacement cost (\$103 $\times$ sq. ft.) and an item by item repair cost for each home. Road Home grants compensated households based on the (larger) replacement cost if the repair cost estimate was more than 51% of the replacement cost. Compensation was for the (smaller) repair cost if the repair cost estimate was less than 51% of the replacement cost. Toward an RDD estimation, we define the running variable:

$$\text{running}_i = \frac{(\text{Road Home repair cost estimate})_i}{(\text{Road Home replacement cost estimate})_i} \tag{6}$$

We then estimate the three equations:

$$\frac{\text{FinIncentive}_i}{\text{ReplacementCost}_i} = h(\text{running}_i; \alpha_r) + \alpha_d \times 1(\text{running}_i > .51) + Z'_{j(i),-i}\alpha_z + e_i \quad (7)$$

$$Y_{i,5} = h(\text{running}_i; \alpha_r) + \alpha_d \times 1(\text{running}_i > .51) + Z'_{j(i),-i}\alpha_z + e_i \quad (8)$$

$$\mu_{j(i),5,-i} = h(\text{running}_i; \alpha_r) + \alpha_d \times 1(\text{running}_i > .51) + Z'_{j(i),-i}\alpha_z + e_i \quad (9)$$

where $h(.)$ is a quadratic function of the running variable that allows the first and second derivatives to change at running_i=.51, $1(.)$ is the indicator function, and $Z_{j(i),-i}$ is a vector of covariates describing the circumstances of i's same block neighbors.[12] The outcome variables in these three equations are; household i's financial incentive to rebuild, an indicator that i rebuilt by Katrina's fifth anniversary $Y_{i,5}$, and the fraction of i's same block neighbors who had rebuilt by Katrina's 5th anniversary $\mu_{j(i),-i}$. Together, equations (4) and (5) identify the causal effect of financial incentives on a household's choice to rebuild. Equation (7) identifies the impact of an exogenous change in one neighbor's rebuilding choice on the rebuilding choice of it's neighbors.

We also "target" a set of linear probability models that measure raw associations (after conditioning on observable factors) between neighbors' choices and one's own choices and a set of regressions designed to provide additional evidence on the average causal effects of neighbors' rebuilding choices on a household's own probability of rebuilding, and in particular nonlinearities in these spillover effects. Including estimates of both "contaminated" partial correlations and causal behavioral spillovers as targets allows us to separately identify the importance of unobserved block amenities (measured by σ_b^2) and the causal effect of neighbors' rebuilding choices on local amenity values – the structural object of primary interest – (captured by $g(\mu)$). Separate identification of these structural parameters is achieved, because the OLS estimates of partial correlations between one's neighbors' choices and one's own choice depend on both the magnitude of amenity spillovers and on the common influence of unobserved amenities, and the average causal effects of neighbors' choices on one's own choices depend only on the magnitude of amenity spillovers.

The linear probability models that we target take the form,

$$Y_{it} = g(\mu_{j(i),-i}; \alpha_\mu) + X'_i\alpha_x + Z'_{j(i)}\alpha_z + e_i \qquad \text{for } t = 1, ..., T \quad (10)$$

[12]We estimate these regression discontinuity models using the subsample of households for whom the Road Home appraised damage share running variable fell between 0.33 and 0.67.

where Y_{it} is an indicator that household i has rebuilt by a particular date, X_i is a vector of fixed or predetermined household variables, $Z_{j(i)}$ is a vector of pre-determined characteristics of the block j where i's home is located, and $g(.)$ is a piecewise linear spline in $\mu_{j(i),-i}$, the fraction i's neighbors with outcome $Y = 1$.

The parameters α_x, α_z, and α_μ estimated by OLS cannot be interpreted as causal effects, because unobserved block $j(i)$ amenities influence both one's own choice (through ϵ_i) and one's neighbors' choices ($\mu_{j(i),-i}$). As a second approach to recovering causal effects, augmenting our regression discontinuity estimates, we estimate set of linear probability models that use Census tract fixed effects to account for Census block neighbors being influenced by common unobserved amenities. Specifically, we target models of the form,

$$Y_{it} = g(\mu_{j(i),-i}; \alpha_\mu) + X_i'\alpha_x + Z_{j(i)}'\alpha_z + u_{\tau(i)} + e_i \qquad \text{for } t = 1, ..., T \tag{11}$$

where the added term $u_{\tau(i)}$ is a fixed effect for the Census tract in which Census i's home is located. On average, New Orleans Census tracts contain about 56 Census blocks, so under the assumption that unobserved local amenities have a roughly constant affect within Census tracts on rebuilding probabilities, the coefficients describing the influence of neighbors' rebuilding choices capture causal behavioral spillovers.
Lastly, estimation requires estimation of the offered home prices policy function,

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

OLS estimates of this equation will generate biased estimates of slope coefficients for two reasons. First, $\mu_{j(i),T}$ is likely to be correlated with the error residual e_i, because unobserved block amenities $b_{j(i)}$ affect both offered home prices and neighbors' rebuilding choices. We address this concern by controlling for Census tract fixed effects. Second, offered prices are only observed for households who choose to sell. In the likely event that the idiosyncratic propensity to rebuild η_i is correlated with unobserved house traits e_i, the estimated impact of the block rebuilding rate on offered prices will be biased if block rebuilding choices impact the probability that a home is sold. We account for this potential selection problem using the Heckman two-step procedure. With a first stage probit we estimate the probability that a household sells its home privately, treating an indicator that the household's "damage share" running variable falls above the 51% grant formula threshold as excluded instrument for selection. We then include the inverse Mills ratio associated with the predicted home sale probability as a regressor in the second stage estimating equation. Our estimating equation takes the form.

$$\ln(p_i) = P\left(p_i^s, k_i, z_{j(i)}, \mu_{j(i),T}\right) + \rho\lambda(\Phi^{-1}(\widehat{sale}_i)) + \widetilde{e}_i$$

4.2.2 Stage Two: Estimation Algorithm

Our estimation algorithm involves an outer loop searching over the space of structural parameters, and an inner loop that computes auxiliary models using simulated data from the structural model.

The Inner Loop With simulated data, computing auxiliary models is straightforward and follows the same procedure as described in Stage One. We focus on describing the solution to the model, given a set of parameter values Θ.

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 $(\sigma_\eta, \gamma, \sigma_b)$. 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 by repeatedly (for each $n_T = 1, ..., I$), guessing that n_T/I is the rebuilding rate, computing the implied offered price for each household $p_i = P\left(p_i^s, k_i, z_{j(i)}, \mu_{j,T} = n_T/I\right)$, counting the number of simulated block households $n_T^*(n_T; \Theta)$ who prefer to rebuild when $\mu_{j,T}^* = n_T/I$, and deeming $\mu_{j,T}^* = n_T/I$ 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 offered price for each household.

3. Taking equilibrium home prices as given, locate all possible "self-consistent" period $T-1$ block rebuilding rates by repeatedly (for each $n_{T-1} = 1, ..., I$), guessing that n_{T-1}/I is the rebuilding rate, counting the number of simulated block households $n_{T-1}^*(n_T; \Theta)$ who prefer to rebuild when $\mu_{j,T-1}^* = n_T/I$, and deeming $\mu_{j,T-1}^* = n_{T-1}/I$ 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 S datasets from the model (parameterized by a particular vector θ) and computing the same estimators. The structural parameter estimator is then the solution

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

where W is a weighting matrix. Standard errors may be obtained by numerically computing $\partial\widehat{\theta}/\partial\overline{\beta}$ and applying the delta method to VCE($\overline{\beta}$).

We augment the indirect inference strategy with an importance sampling technique suggested by Sauer and Taber (2012) that ensures a smooth objective function even though the procedure is simulation-based and the model outcomes are discrete.

5 Data and Descriptive Analysis

Our empirical analysis relies on five data sources; two main administrative data sources and three auxiliary data sources. (1) Administrative records from the Road Home program provide a record of which households applied to the program, the damage appraisals used by the program to determine the size of each household's grant offer, and a record of which households acceted the grants that they were offered. (2) We use records from the Orleans Parish Assessor's Office (OPAO) property database to construct measures of the timing of home repairs and home sales for the full universe of New Orleans homes that were owner-occupied just prior to Hurricane Katrina. The database provides an appraised land value and an appraised improvement value (the value of structures) for each property for calendar-years 2004-2009 and provides the date and transaction price for all home sales over that period. We construct a set of indicators for whether repairs had yet occurred on each of the first four anniversaries of Katrina based on the sequence of appraised improvement values. We merge these program records by street address to the Road Home program records. (3) A FEMA-provided data set constructed based on satellite images contributes information on each home's flood exposure. (4) Census tract-level, block-group-level, and block-level aggregate variables created from the 2000 Decennial Census of Population and Housing contribute information on neighborhood pre-Katrina poverty rates, residential stability, and racial composition. (5) The field work and data collection effort from the Displaced New Orleans Residents Survey (DNORS) provide a probability sample from the population of households who owned homes in New Orleans before Katrina and provide information from each sampled household about: demographic variables, pre-Katrina and post-Katrina labor market outcomes, measures of Katrina-related home damage and level of insurance coverage, and information on post-Katrina migration (RAND, 2010).

To facilitate estimation of the structural model, we impute a set of more detailed background information to each household's record based on the dataset studied by Gregory (2013). For non-Road Home applicants, we also impute their Road Home damage appraisal had they applied. The details of these imputations are in the appendix.

Table 1 presents descriptive statistics for the homeowning households on blocks included in our main estimation sample. More than half of our sample lived on a majority black block prior to Katrina. Less than a third of homeowners received no flooding, and nearly one third received more than four feet of flooding. The average private insurance payout to households with severely damaged homes averaged roughly half of the estimated cost of repairs. More than half of households with damaged homes received a Road Home option 1 rebuilding grant and a similar fraction had completed repairs by Katrina's fifth anniversary.

Figure 2 illustrates geographic variation in storm damage and in the timing of repairs within the city. Panels (a) and (b) depict average 2005 home values and the year-2000 fraction of residents who were black at the block-level. A comparison of these panels to panel (c), which depicts block-level flood exposure, confirms the widely documented fact that the the neighborhoods that escaped flooding – the "Uptown" neighborhood in Southwest New Orleans and several "West Bank" neighborhoods (those South of the Mississippi River) for example – were relatively affluent and almost entirely non-black. However, the areas receiving the heaviest flooding included both affluent neighborhoods and poorer neighborhoods. Panels (d), (e), and (f) depict the distribution of initial storm damage and block-level repair rates on the second and fifth anniversaries of the storm. As expected, initial damage is highly correlated with flood exposure. On Katrina's second anniversary, just prior to the payment of most Road Home grants, a significant disparity had emerged in the repair rate in black versus nonblack neighborhoods (and, similarly, affluent versus poorer neighborhoods). By Katrina's 5th anniversary, repair rates were no longer highly correlated with neighborhoods' pre-Katrina demographics.

Figure 3 shows the empirical distribution of Road Home Type 1 damage appraisals (based on homes' floor area), Type 2 damage appraisals (based on a checklist of items/rooms needing repairs), and the ratio of the Type 2 and Type 1 appraisals, which we refer to as *damage share* appraisals. The median Type 1 appraisal is about \$250,000, and the median Type 2 appraisal is just above \$110,000. Both distributions exhibit a large variance. Figure 4 illustrates the empirical density of the ratio of Type 2 appraisal to the Type 1 appraisal, the quantity that we treat as the running variable in our regression discontinuity analysis and refer to as the "Road Home damage share appraisal." Importantly for the credibility of our regression discontinuity estimation strategy (McCrary, 2008), a substantial density of damage share appraisals fall near the location of the Road Home grant formula discontinuity at 51% and we cannot reject the null hypothesis that the density of the running variable is continuous across that threshold.[13]

[13] Appendix Figure A.1 presents the McCrary test applied to a version of the running variable that incorporates the results of homeowner appeals and a set of adjustments made after the initial disbursement of grants to correct mistakes made by a program contractor. Not surprisingly, a small discontinuity in the density of that variable is present at the 51% Road Home grant formula threshold in this adjusted variable. This occurs for two reasons. First, the incentive to appeal an initial appraisal was larger if the initial appraisal fell below the 51% threshold. Second, Road Home compensated households who were initially awarded too small grants as the result of contractor mistakes but did not try to recover money from households awarded

Finally, as is standard in the RD literature, we further examine the validity of the RD design by verifying that the average value of important pre-determined covariates is smooth across the "critical value" of the running variable. Figure 5 presents the results of this exercise. We do not find a significant difference above and below the critical value in the average racial composition of blocks, log of pre-Katrina home values, log of non-Road Home insurance payouts, or (importantly for the validity of our RD spillovers analysis) the fraction of neighbors with a running variable greater than 51%.

6 Estimation Results

We next turn to our estimation results. We first present the reduced form evidence that we treat as the set of target auxiliary models for our indirect inference estimation, and then we present estimates of the equilibrium model's structural parameters.

6.1 Auxialary Model Results

6.1.1 Regression Discontinuity Analysis

We first present the results of our regression discontinuity analysis. For this analysis, we restrict attention to households whose Road Home damage share appraisal, the running variable in our analysis, fell between 0.33 and 0.67 and who were not fully insured.[14]

Figure 6 plots the financial incentive to rebuild – that is, the opportunity cost of selling the home in the most lucrative manner available instead of rebuilding it under Road Home option 1 – as a function of the Road Home damage share appraisal. This average financial incentive to rebuild jumps by 20.4% (S.E. of 0.9%) of the home's replacement cost at the 51% grant formula threshold. Figure 7 plots the fraction of households who rebuilt by Katrina's fifth anniversary as a function of the Road Home damage share appraisal. The rebuilding rate jumps by 4.9 percentage points (S.E. of 2.0 percentage points) at the 51% grant formula threshold. Treating these estimates as the "first stage" and the "reduced form" of a fuzzy regression discontinuity design, these estimates suggest that each 10 percentage point increase in a rebuilding subsidy as a fraction of a home's replacement cost increases the probability that a household rebuilds by 2.4 percentage points (see Table 2 for details of this calculation).

Figure 8 plots the rebuilding rate of same-block neighbors as a function of a household's Road Home damage share appraisal. We find that the rebuilding rate of same-block

too large grants. As a robustness check, we compute R.D. estimates of the impact of both pre-adjustment and post-adjustment financial incentives on rebuilding choices, in both cases using the pre-appeal running variable as the forcing variable, and do not find substantively different results.

[14] Appendix figure A.2 replicates our main RD estimates using different bandwidths and different order polynomials. Our main results are not sensitive to the choice of bandwidth or polynomial order.

neighbors jumps by 2.3 percentage points (S.E. of 0.9 percentage points) at the 51% grant formula threshold. Under the assumption that the size of one household's grant offer only influences neighbors' rebuilding choices through the grant's impact on the household's rebuilding choice, Figures 7 and 8 form the "first stage" and the "reduced form" of fuzzy RD estimates of spillovers from one household's choice onto the choices of neighbors. These estimates imply that exogenously changing one household's rebuilding choice from "no" to "yes" increases the equilibrium rebuilding rate among same-block neighbors by 46.6 percentage points (see Table 3 for details of this calculation), a remarkably strong spillover effect.

Because the first stage of this fuzzy RD spillover calculation is somewhat noisy (t=2.45), one fears that the point estimate is biased upward. To address this concern, we compute a weak-instrument robust 95% confidence interval for this estimate by computing Anderson-Rubin tests for a wide range of point null-hypotheses and noting the values that cannot be rejected at the 5% confidence level. This robust confidence interval is bounded below by 12.0 percentage points. Since the average block contains 17 owner occupied homes, even this low end estimate implies that exogenously flipping one household's rebuilding choice to "yes" on average causes more than one additional neighbor to rebuild.

6.1.2 House Price offers and Nonlinearities in Behavioral Spillovers

While estimates of average amenity spillovers provide some guidance for policy, precise policy prescriptions also depend strongly on the "shape" of amenity spillovers – that is, the extent to which the marginal spillover from one additional household rebuilding is different on blocks with very little rebuilding and blocks with more rebuilding. To investigate nonlinearities in spillover effects, we estimate fixed effects linear probability models that allow rebuilding choices to depend on splines in the rebuilding rate of neighbors and hedonic housing transaction price regressions that allow transaction prices to depend on splines in the rebuilding rate of neighbors.

Figure 9 highlights these estimates.[15] Panel (a) plots the estimated splines relating the rebuilding rate among neighbors to the probability of rebuilding by Katrina's fifth anniversary. The OLS estimate finds that changing the rebuilding rate of neighbors from 0% to 100% is a associated with a 45 percentage point increase in a probability that a household rebuilds. Controlling for Census tract fixed effects reduces this esimate to 20 percentage points, consistent with our expectation that the OLS specification recovers an upwardly biased estimate of the causal relationship between neighbors' choices due to unobserved amenities simultaneously influencing a household's rebuilding choice and the choices of neighbors. Both specifications find that the marginal effect of an additional neighbor

[15] See appendix Table A.1 presents our full set of linear rebuilding probability model estimates.

rebuilding on a household's own likelihood of rebuilding is largest on blocks with at least 1/3 of homes rebuilt.

Panel (b) plots the estimated spline from a fixed effects hedonic housing price regression relating composition-constant home transaction prices to the rebuilding rate of neighbors. Because few private sales occurred on blocks with very low rebuilding rates (Road Home relocation grant offers were typically larger than the market value of properties in very heavily-damaged areas) we treat blocks with rebuilding rates below 40% as a single pooled category. Even after controlling for individual household circumstances and Census tract fixed effects, higher rebuilding rates among same-block neighbors were associated with higher home transaction prices. Increasing the rebuilding rate from 40% (the upper limit of the reference category) to 100% increases transaction prices by 16%, and the *marginal* impact from an additional neighbor's rebuilding increases as more neighbors rebuild.

6.2 Structural Model Parameter Estimates

Table 5 presents estimates of the model's structural parameters. The upper panel presents the estimated effects of block-level flood exposure on the flow payoff to living in the pre-Katrina home. We do not find large direct effects of flood exposure or pre-Katrina neighborhood demographic characteristics on the flow payoff to living in the pre-Katrina home. The middle panel presents the estimated dispersion of individual and block heterogeneity. These parameters are central to pinning down the elasticity of location choices with respect to private financial incentives, which we will study more directly with our counterfactual policy simulations. The bottom panel presents estimates of the parameters governing the shape and strength of the structural amenity spillover function. Figure 10 plots this spillover function. The difference in flow payoff between residing on a fully rebuilt block instead of completely devastated block is equivalent to utility benefit of increasing consumption by 44 log consumption points, and the marginal impact of one additional neighbor rebuilding is largest on blocks that are more than 50% rebuilt.

7 Counterfactual Policy Simulations

We now turn to a set of counterfactual policy simulations performed with the estimated equilibrium model. We first assess the impact of the Louisiana Road home program on rebuilding rates and on welfare, paying particular attention the role of amenity spillovers in "multiplying" the program's direct (partial equilibrium) effect that occurs by altering households' private financial incentives.[16] Next, we compare the welfare effects of the Road

[16]These simulations hold credit conditions constant when policies change (i.e. when the Road Program is removed). To the extent that some of the Road Home program's impact occurred by relaxing credit

Home program to the welfare effects of a counterfactual policy that makes Road Home option 1 grants available to all households regardless of where they resettle. Finally, we evaluate policies that designed to discourage rebuilding in disaster-vulnerable areas.[17]

7.1 Equilibrium Impact of the Louisiana Road Home Program

Our first set of policy experiments quantify the Road Home program's impact on the equilibrium rate of rebuilding in post-Katrina New Orleans. We also examine the relative importance of private incentives and amenity spillovers in generating this impact by decomposing the program's impact into a component that occurred as a result of the policy changing individuals' private incentives (holding amenity levels fixed at the level that would have occurred in the absence of Road Home, even as rebuilding rates change) and a component attributable to a "multiplier" from amenity spillovers. We also compute the consumption value of the amenity improvements generated by the additional rebuilding that the Road Home program caused. Specifically, we compare equilibrium choices under three scenarios:

1. No grant are available ("no grants").
2. Road Home grants are available, but amenities are held fixed at the level that occurs under the "no grants" scenario ("partial equilibrium Road Home")
3. Road Home grants are available, and amenities adjust in equilibrium ("equilibrium Road Home")

Table 6 summarizes the results of these simulation experiments. Column 1 reports the fraction of households with uninsured losses who repair their homes by Katrina's fifth anniversary under the "no grants" scenario. Column 2 reports the rebuilding rate impact of the "partial equilibrium Road Home" scenario relative to "no grants" scenario. Column 3 reports the rebuilding rate impact of the "equilibrium Road Home" scenario relative to "no grants" scenario. The first row of the table reports these results for all households with uninsured losses, and the lower rows report these results separately by flood exposure categories. Our simulation experiments find that the Road Home program's private financial incentives increased the aggregate rebuilding rate by 3.2 percentage points (from a base of 60%), while the Road Home program's full equilibrium impact on the rebuilding rate was 5.2 percentage points. These results suggest that amenity spillovers generated a rebuilding

constraints (Gregory, 2013), the simulation results presented here will provide lower bounds on the program's actual impacts.

[17]For each policy considered, we compute 50 simulations of each block. Each simulation assigns each of the block's households their actual exogenous variables, draws the shared random amenity component b and household heterogeneity terms η_i from their estimated distributions, and computes the equilibrium outcome.

multiplier of about 1.6. We do not find significant heterogeneity in these impacts across flood exposure categories.

Next we study the mechanisms that account for this rebuilding multiplier. In particular, we examine whether the Road Home program's rebuilding multiplier typically occurred by spillovers shifting the location of a block's unique self-consistent rebuilding rate or because spillovers caused blocks to "tip" to higher self-consistent rebuilding rates. Figure 11 illustrates this potential tipping phenomenon in our framework. The top panel plots the private demand for rebuilding evaluated at the amenity level associated with a 0% rebuilding rate, which is downward sloping by definition as it is simply a highest-to-lowest ordering of individual households' net benefits to rebuilding, and the actual marginal benefit curve, which 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 this curve. We refer to as "tipping" the phenomenon illustrated in the bottom panel, namely the introduction of a subsidy causing additional higher rebuilding rates to become self-consistent.

Table 7 reports these simulation results. We find that the Road Home program caused a relatively small number of blocks to tip, but block rebuilding rates were dramatically affected on the blocks that did tip. On 5.5 percent of blocks, a unique equilibrium existed in the absence of the Road Home program, while additional higher equilibria occurred under the Road Home program. The multiplier effect of amenity spillovers was concentrated heavily on these blocks, where the average equilibrium impact of the Road Home program on the equilibrium rebuilding rate was 24.3 percentage points.

Table 8 quantifies the value of positive amenity spillovers generated by increased rebuilding under the "equilibrium Road Home" scenario compared to the "no grants" scenario. For each block j, we compute the consumption equivalent of the Road Home program's amenity spillover for each inframarginal household i (i.e. for each household who rebuilt even under the "no grants" scenario) as the solution $\widetilde{B}_{i,j,\text{Spill}}$ to the equality,

$$\ln\left(C_{i,j,\text{NoGrants}}+\widetilde{B}_{i,j,\text{Spill}}\right)-\ln\left(C_{i,j,\text{NoGrants}}\right)=g\left(\mu^*_{i,j,\text{RH}}\right)-g\left(\mu^*_{i,j,\text{NoGrants}}\right) \qquad (12)$$

where $C_{i,j,\text{RH}}$ is the period 5 consumption of household i on block j without a policy intervention, μ^*_{NoGrants} is the equilibrium block rebuilding rate in the "no grants" scenario, and μ^*_{RH} is the "equilibrium Road Home" block rebuilding rate. Solving this for $\widetilde{B}_{i,j,\text{Spill}}$ yields,

$$\widetilde{B}_{i,j,s,\text{Spill}} = C_{i,j,s,\text{NoGrants}} \times \left(\exp\left[g\Big(\mu^*_{i,j,s,\text{RH}}\Big) - g\Big(\mu^*_{i,j,s,\text{NoGrants}}\Big) \right] - 1 \right) \tag{13}$$

Using this expression and the results of our counterfactual simulation experiments, we compute the aggregate external value of amenity spillovers as the sum of these amenity spillovers among inframarginal households averaged across simulations,

$$\widetilde{B}^{\text{Total}}_{\text{Spill}} = \frac{1}{S} \sum_{s=1}^{S} \left(\sum_{j=1}^{J} \sum_{i \in I_j} Y_{i,j,s,t=5} \times \widetilde{B}_{i,j,s,\text{Spill}} \right) \tag{14}$$

We find that the improvement to flow amenity values caused by Road Home-induced rebuilding, measured in the fifth year after Katrina, was about \$40M per year, about 1.5% of the \$2.6B in Road Home option 1 rebuilding grants paid to the New Orleans homeowners in our sample. Extrapolating this flow value even several years yields a benefit estimate that is greater than preivious estimates of the long-run efficiency loss from expected future bailouts to New Orleans distorting households' location choices. Gregory (2013) estimates that efficiency cost to be less than four percent of the policy's expected flow cost.

7.2 Welfare Consequences of the "Option 1" Rebuilding Requirement

Next, we study the consequences of the Road Home's requirement that a household must rebuild its home the same location in order to receive the more generous option 1 grant package. Specifically, we compare choices under the Road Home program choices under an alternative policy that provides option 1 grants to all households with uninsured losses, regardless of the resettlement location. We identify the fraction of belonging to each of two groups; (1) marginal rebuilders: households who choose to rebuild under the Road Home policy but who would have chosen to relocate if the policy did not include a rebuilding requirement, and (2) attached/inframarginal households: households with undamaged homes or who preferred to rebuild even if option 1 grants are paid unconditionally. Then, as a back of the envelope assessment of the welfare consequences of Road Home's rebuilding requirement, we compare the total cost of option 1 grants paid to marginal rebuilders to the value to inframarginal rebuilders of the amenity improvements caused by additional rebuilding caused by Road Home's rebuilding requirement.

Table 9 presents the results of this exercise. We find that 71% of all households fall in the "attached" category, choosing reside on their pre-Katrina block even when option 1 grants are provided unconditionally, and 3.4% of all households fall in the "marginal" category, choosing to rebuild only if a rebuilding requirement is attached to the option 1 grant. A somewhat larger fraction of households are marginal in more heavily flooded neighborhoods. We find that grant payments to marginal households totaled $198 million, and that these rebuilding by these households generated amenity spillovers to inframarginal households with a flow value of $44 million per year, 22% of the policy's one-time cost. Thus, assuming these amenity improvements are persistent and using a social discount rate less than 22% annually, the present value of amenity improvements exceeds the cost of the grants paid to marginal households. We reach similar conclusions when this exercise is repeated separately by flood category, with the value of flow amenity improvements for inframarginal households ranging from 17% to 31% of the cost of the option 1 grants paid to marginal households.

7.3 Post-Disaster Buyout Programs

In some circumstances, an optimal policy might be to discourage rebuilding in areas with small populations and for which mitigation against future disasters or other types of public goods provision is costly. In fact, although never enacted, many suggested immediately after Hurricane Katrina that for especially vulnerable New Orleans neighborhoods, the government should provide incentives to relocate and not to rebuild. We study two policies that are designed for such a purpose. In the first (second) policy, all homeowners are given the option to sell their home to the state for 100% (125%) of its pre-Katrina value. No grants for rebuilding are provided under either of these policies.

Columns 2 and 3 of Table 10 present the impact of these policies on the average (across blocks) fraction of homes in a livable state on Katrina's fifth anniversary. Columns 5 and 6 present the impact of these policies on the fraction of blocks on which no households rebuild. We find that the 100% buyout reduces the proportion of homes repaired on the average block by about seven percentage points, with the impact concentrated in the more heavily flooded areas of the city. The policy that provides buyouts at 125% of the pre-Katrina home value generates a nearly nine percentage point reduction in average block-level repair rates. Both policies generate modest increases in the fraction of blocks with zero rebuilding – for instance, they increase the fraction of all blocks with no rebuilding from 6.3% to 9.0% and 9.9% respectively – but even in the most heavily flooded areas fall short of discouraging all rebuilding.

8 Conclusion

Many housing policies are predicated on the idea that housing investments can generate positive externalities. This paper studies amenity spillovers from an extreme type of housing investment, residential reconstruction in New Orleans after Hurricane Katrina. We develop an equilibrium model of households' rebuilding decisions, allowing for the possibility that rebuilding choices cause amenity spillovers and, hence, are inter-related. We have estimated the structural model via indirect inference.

We find that rebuilding caused economically important amenity spillovers: the Louisiana Road Home rebuilding grant program's full equilibrium impact – including "feedback" effects from positive amenity spillovers – was almost twice the impact generated by the program's financial incentives alone (holding amenities fixed). We also find that spillover effects are highly nonlinear, which can admit a tipping phenomenon where small changes in policy can generate large changes in rebuilding rates and in welfare. Finally, our estimates suggest that at plausible social discount rates, the present value of amenity improvements caused by the Road Home program's requirement that households rebuild in order to receive an option 1 grant exceeds the cost of rebuilding grants paid to "marginal" households who would have preferred to relocate.

Future research, extensions.

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Appendix

A1. Road Home Grant Formula

$$\text{RH\_1}_i = \begin{cases} \min\left[\$150\text{k} \quad ; \quad \text{P}_i^{\text{Pre-K}} \quad ; \quad \text{T2}_i + 1\left[\text{T2}_i \geq .51\cdot\text{T1}_i\right] \times \left[\text{T1}_i - \text{T2}_i\right] - \text{Ins}_i\right] & \text{if LMI}_i = 0 \\ \min\left[\$150\text{k} \quad ; \quad \quad ; \quad \text{T2}_i + 1\left[\text{T2}_i \geq .51\cdot\text{T1}_i\right] \times \left[\text{T1}_i - \text{T2}_i\right] - \text{Ins}_i\right] & \text{if LMI}_i = 1 \end{cases}$$

$$\text{RH\_2}_i = \begin{cases} \min\left[\$150\text{k} \quad ; \quad \text{P}_i^{\text{Pre-K}} \quad ; \quad \text{T2}_i + 1\left[\text{T2}_i \geq .51\cdot\text{T1}_i\right] \times \left[\text{T1}_i - \text{T2}_i\right] - \text{Ins}_i\right] & \text{if LMI}_i = 0 \\ \min\left[\$150\text{k} \quad ; \quad \text{P}_i^{\text{Pre-K}} \quad ; \quad \text{T2}_i + 1\left[\text{T2}_i \geq .51\cdot\text{T1}_i\right] \times \left[\text{T1}_i - \text{T2}_i\right] - \text{Ins}_i\right] & \text{if LMI}_i = 0 \end{cases}$$

$$\text{P}_i^{\text{Post-K}} \approx \text{HPI} \times \text{P}_i^{\text{Pre-K}} - \text{T2}_i$$

$$\text{FITR} = \left(\underbrace{\begin{array}{l} \quad \text{As-is Structure Value} \\ + \text{ Insurance Payment} \\ + \text{ Option 1 Grant} \end{array}}_{\text{Wealth after Opt. 1}} \right) - \max \left(\underbrace{\begin{array}{l} \quad 0 \\ + \text{ Insurance Payment} \\ + \text{ Option 2 Grant} \end{array}}_{\text{Wealth after Opt. 2}} ; \underbrace{\begin{array}{l} \quad \text{As-is Structure Value} \\ + \text{ Insurance Payment} \\ + \text{ 0} \end{array}}_{\text{Wealth after Priv. Sale}} \right)$$

$$= \min\left[(\text{As-is Structure Value}) + (\text{Option 1 Grant}) - (\text{Option 2 Grant}) \quad ; \quad (\text{Option 1 Grant})\right]$$

B. Data Imputations

Our data include individual-level demographic and labor market data only for the roughly 1% of New Orleans homeowners sampled by the Displaced New Orleans Residents Survey (DNORS). For use in model simulations, we impute these variables for non-DNORS respondents using a matching-based imputation procedure that treats the subpopulation covered by DNORS as the pool of "donor" records. Using nearest Mahalanobis distance matching on the set of variables that are present for all of the records in our dataset (appraised pre-Katrina home values, pre-Katrina neighborhood demographics information, block-level flood exposure, Katrina-related home damages, and the timing of post-Katrina home repairs and sales), we match a donor record to to each "target" record requiring imputation. We then assign the donor record's values to any missing variables on each target record.

We then impute post-Katrina New Orleans wage offers and post-Katrina "outside option" wage offers to each worker using a regression of workers' pre-Katrina annual earnings on a set of human capital variables, the contemporaneous composition-adjusted local wage in the worker's occupation (measured in the American Community Survey), and a worker fixed effect. We impute post-Katrina wage offers by evaluating the estimated regression equation with period-specific and market-specific composition-adjusted occupation wages. When generating these predictions, we define the "outside option" to be the set of other Southern metropolitan areas.

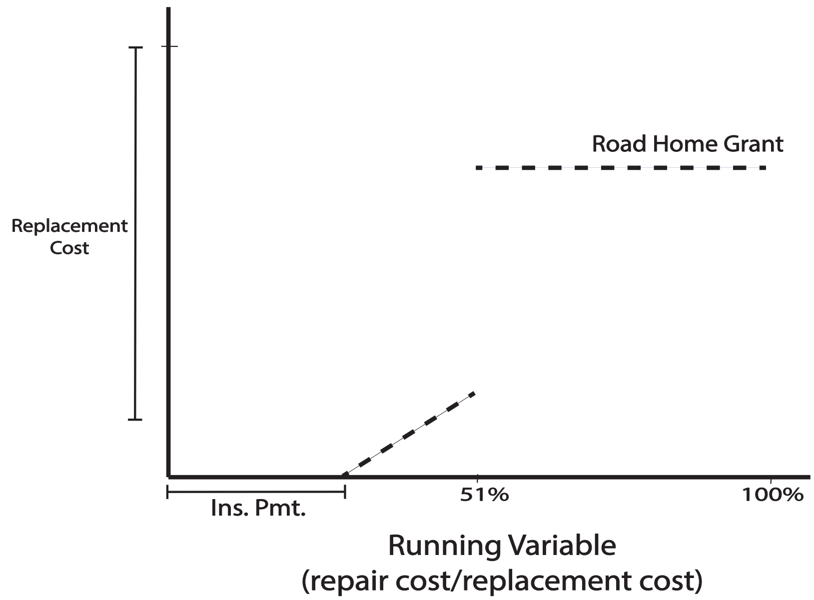
Finally we impute home replacement cost and home repair cost estimates for households who did not apply to the Road Home program (and thus did not undergo Road Home damage appraisals). We first impute estimated replacement costs using the predicted values from a regression estimated among Road Home applicants of the log Road Home 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:

$$\Big(\widehat{\%\ \text{damage}_i}\Big) = \Big(1 + \exp\Big(- (\widetilde{X}_i' a)\Big)\Big)^{-1}$$

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 exogenous variables, and thus in expectation imputed records for nonapplicants do not contribute to the observed "jumps" in any outcomes at the 51% grant formula threshold.

Figure 1: The Opportunity Cost of Not Rebuilding

a. Road Home grant offer by home damage state



b. Offered private sale price and Road Home grant offers by damage state

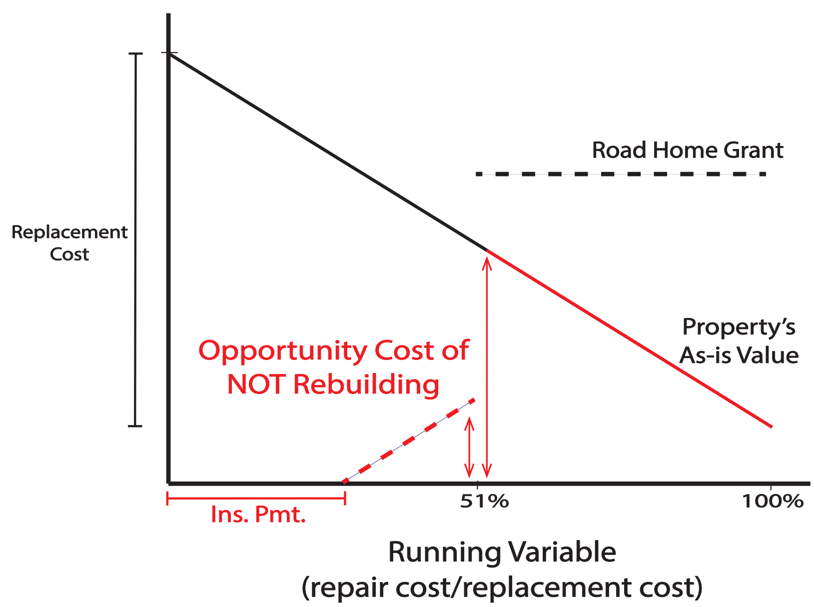
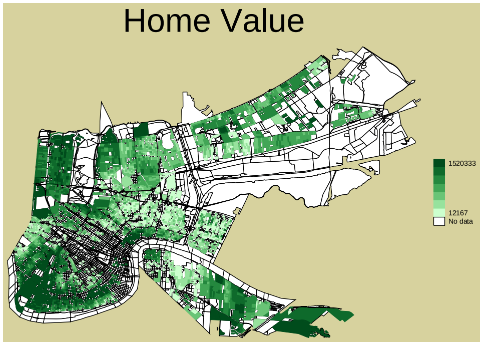
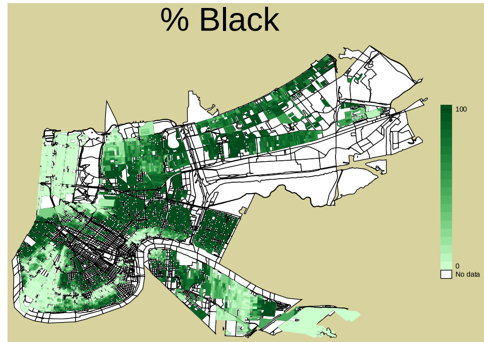


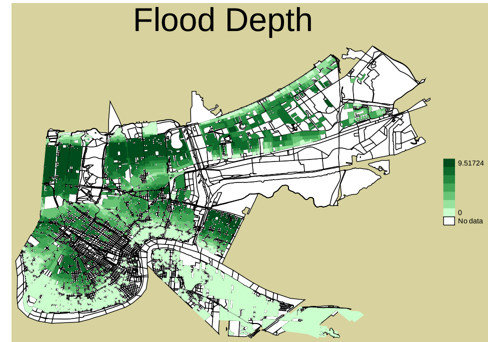
Figure 2: Pre-Katrina Neighborhood Characteristics, Storm Damage, and Rebuilding Patterns



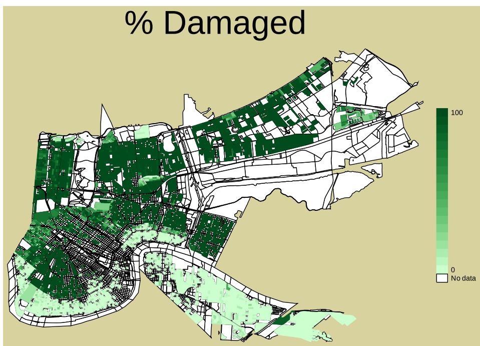
(a) Pre-Katrina home value



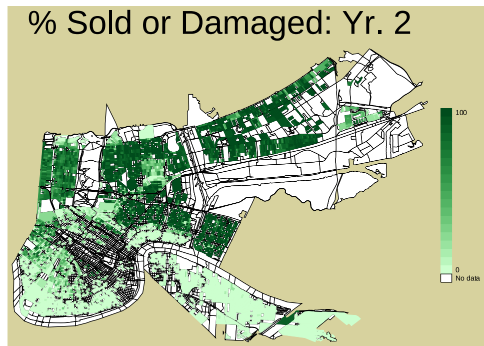
(b) Block % black (2000 Census)



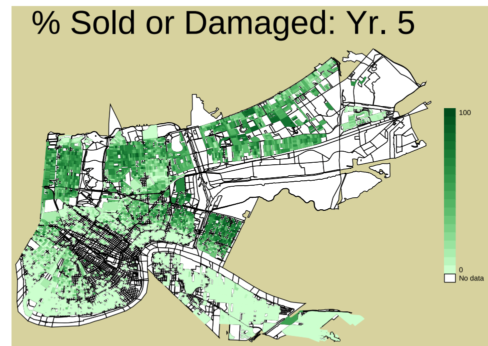
(c) Flood depth at crest



(d) % of homes initially unlivable



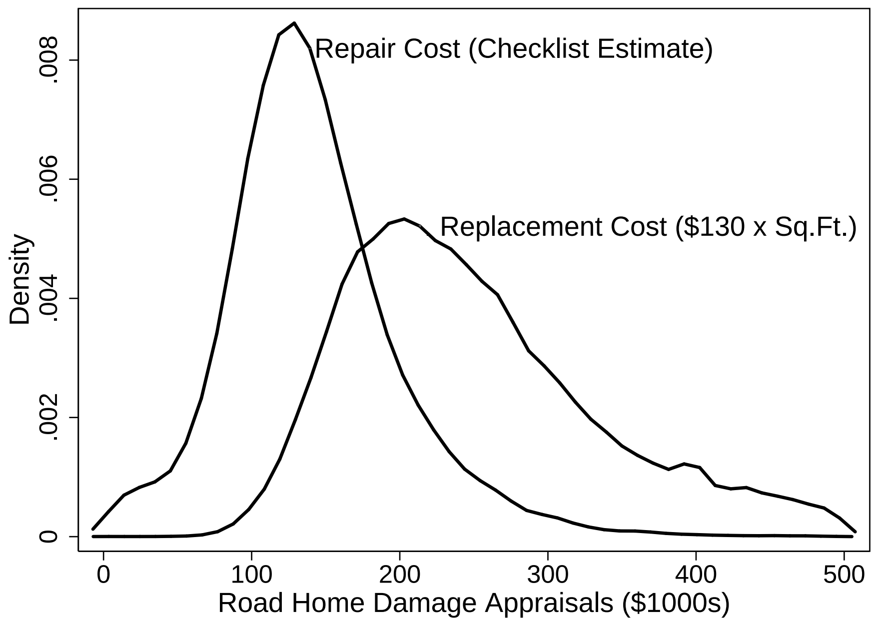
(e) % of homes unlivable or sold – 2nd anniversary



(f) % of homes unlivable or sold – 5th anniversary

Figure 3: Road Home Repair Cost Estimates

a. Type 1 and Type 2 Road Home damage estimates



b. Distribution of the running variable

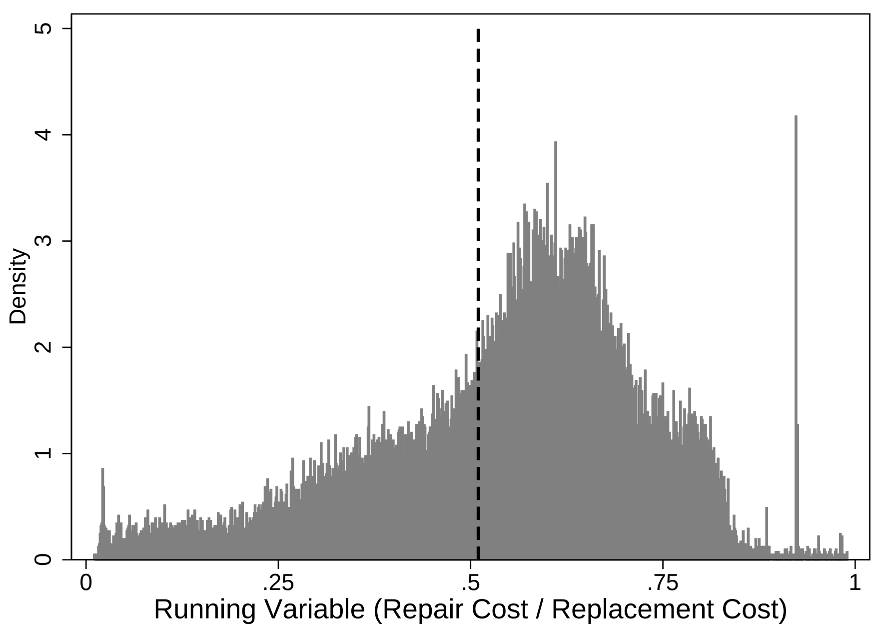


Figure 4: McCrary Density Test For Manipulation of the Running Variable

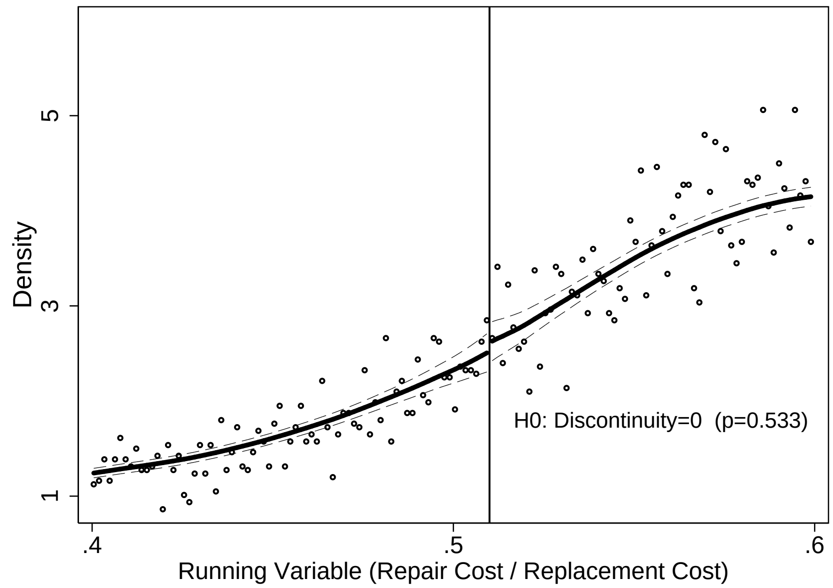


Figure 5: Validity of the RDD: Covariate Balance Tests

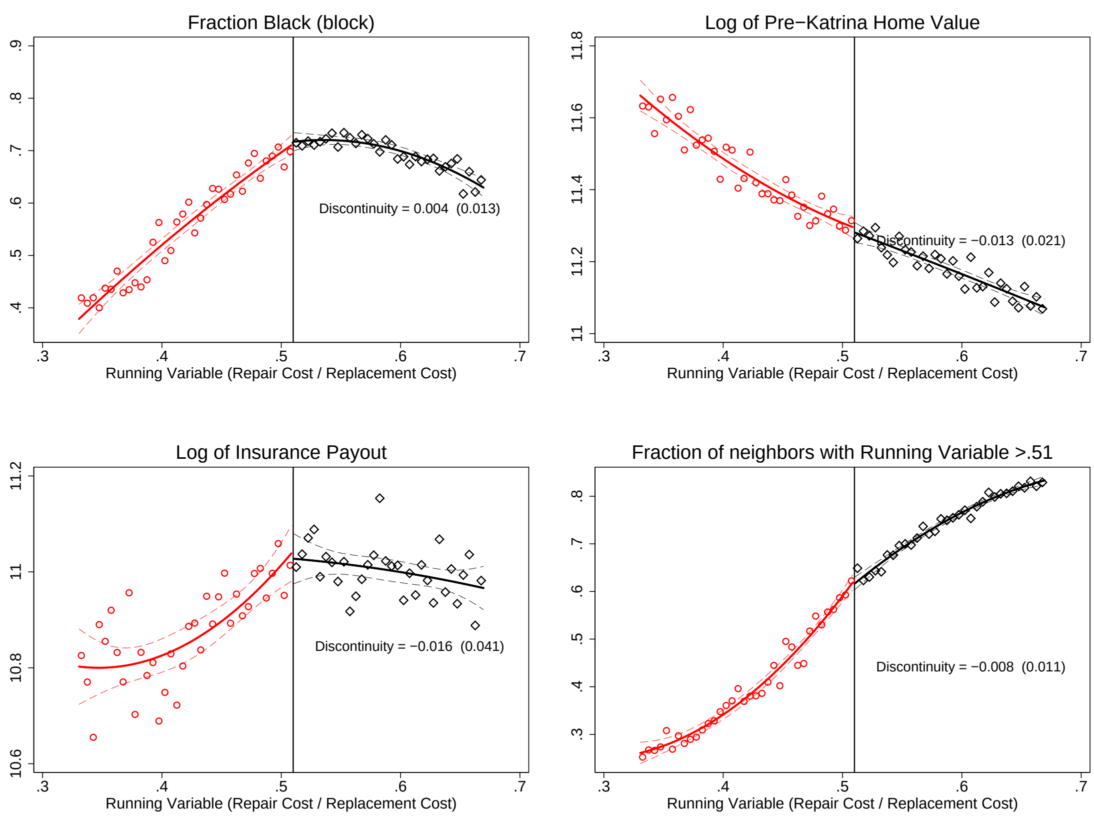


Figure 6: Opportunity Cost of Not Rebuilding by Home Damage State

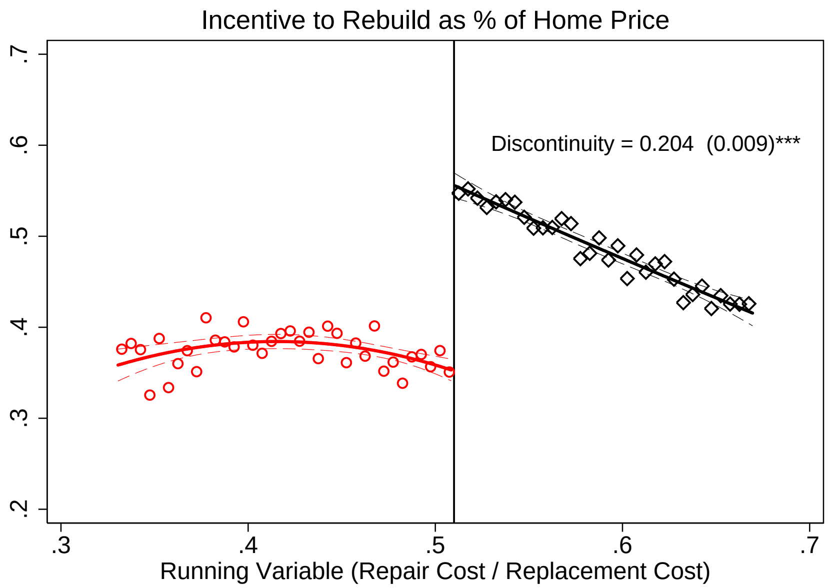


Figure 7: Rebuilding Rates by Home Damage State

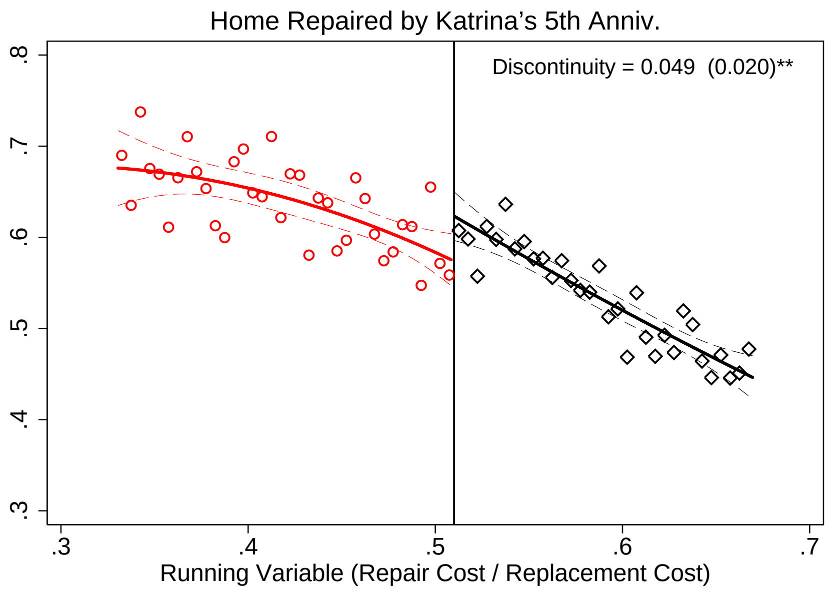


Figure 8: The Rebuilding Rate of Same Block Neightbors by Home Damage State

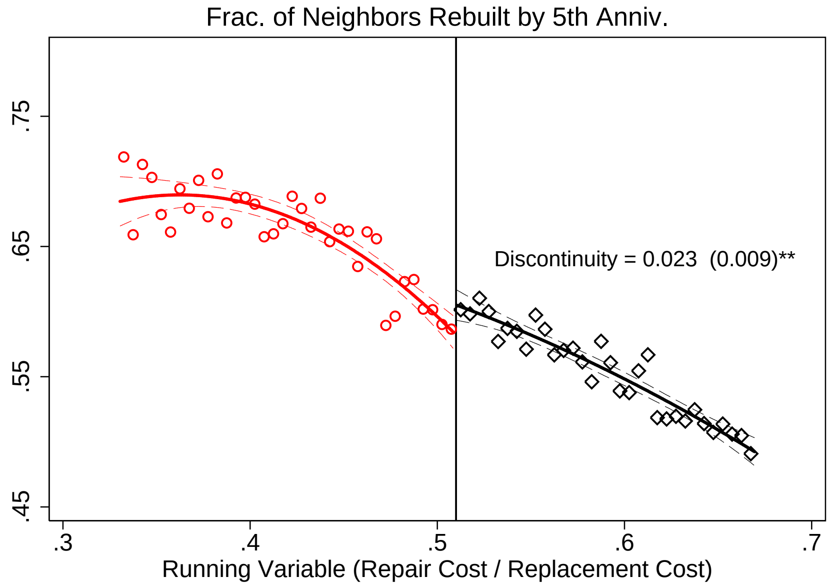
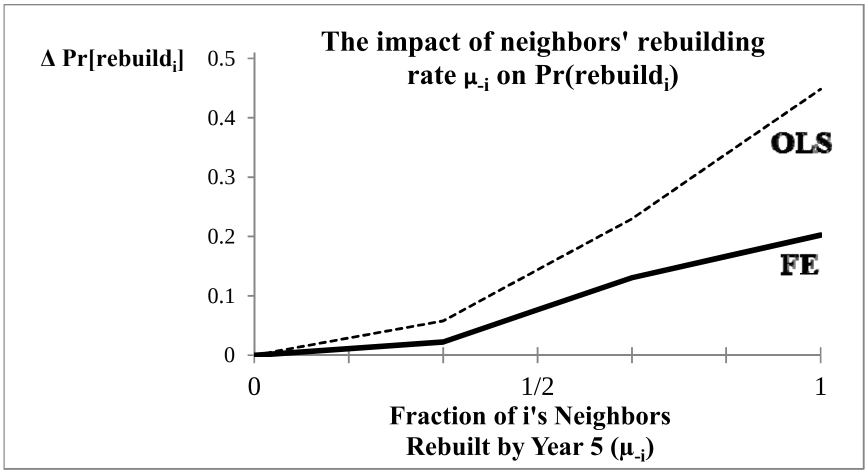


Figure 9: The Effects of Neighbors' Rebuilding Choices on Prices and Behavior

a. Spillover spline $h(\mu)$ from fixed effects linear probability model:

$$rebuild_{i,T} = \underbrace{h(\mu_{j(i)-i})}_{\text{spillover effect}} + \underbrace{z_i'\gamma + u_{\tau(i)}}_{\text{composition adj.}} + e_i$$



b. Spillover spline $h(\mu)$ from fixed effects hedonic home price regression:

$$log(SalesPrice_i) = \underbrace{h(\mu_{j(i)-i})}_{\text{spillover effect}} + \underbrace{z_i'\gamma + u_{\tau(i)}}_{\text{composition adj.}} + \underbrace{\rho\lambda(\Phi^{-1}(\widehat{sale_i}))}_{\text{selection correction}} + e_i$$

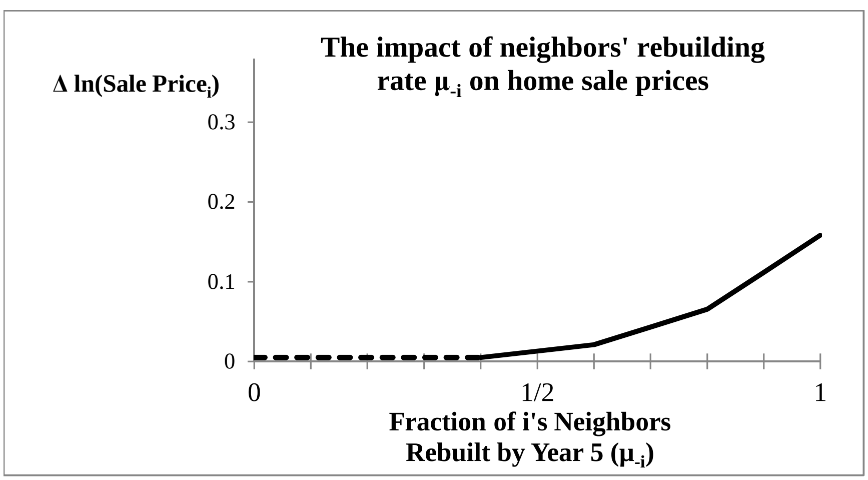


Figure 10: Estimated Amenity Spillover Function

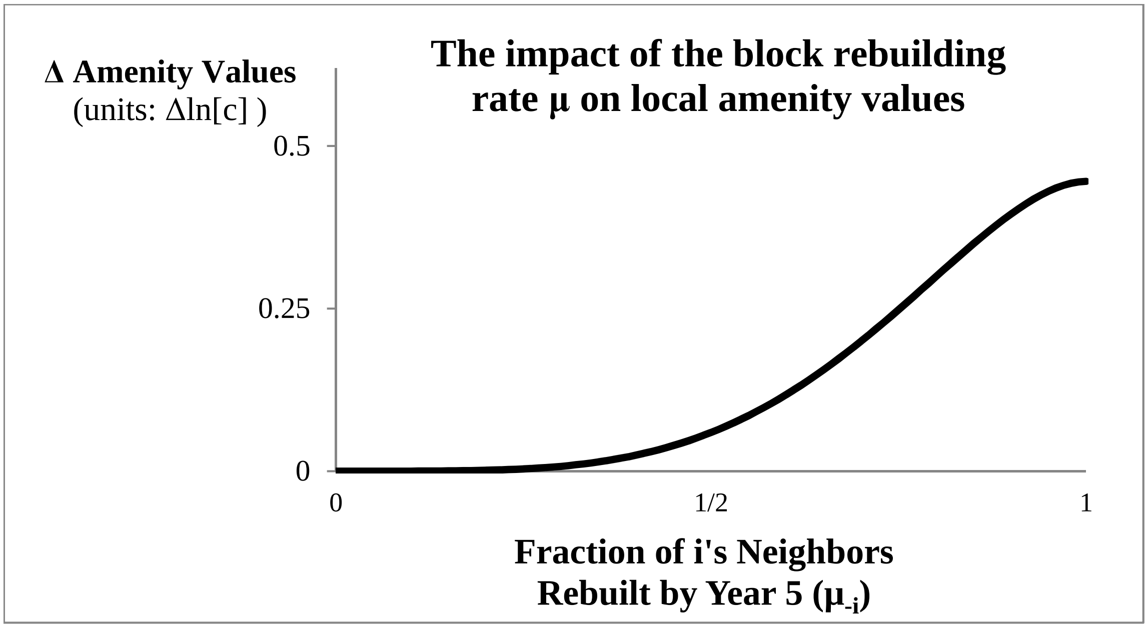


Figure 11. Illustration of Subsidy-Induced “Tipping”

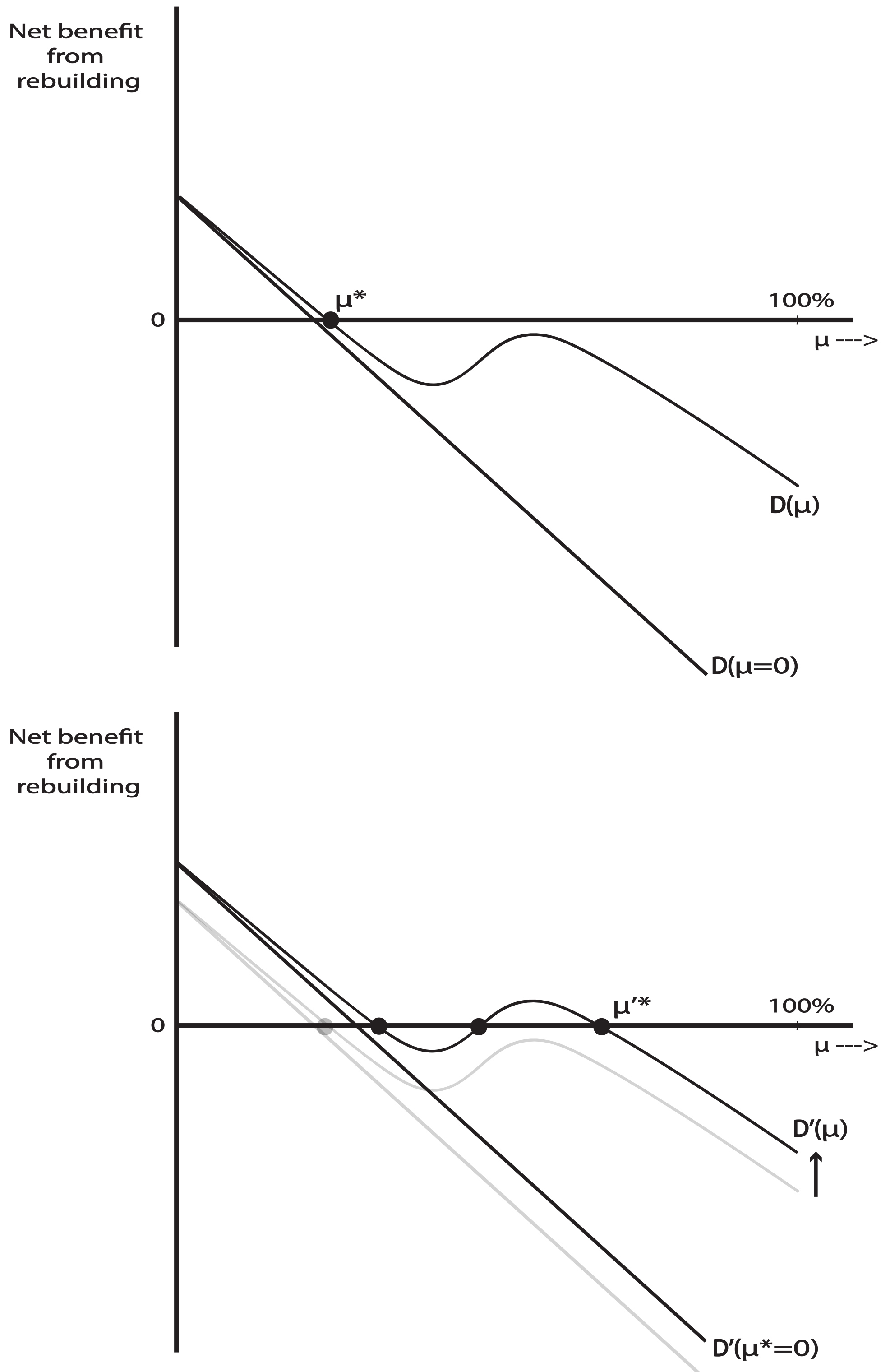


Table 1. Descriptive Statistics

Variable	Mean	S.D.	N
All households:			
Neighborhood demographics:			
Percent black (Census block)	57		60,175
Percent college educated (Census tract)	51		60,175
Pre-Katrina block flood exposure:			
< 2 feet	46		60,175
2 - 3 feet	12		60,175
3 - 4 feet	11		60,175
4 - 5 feet	10		60,175
5 - 6 feet	6		60,175
> 6 feet	15		60,175
Percent with severely damaged homes	67		60,175
Households with severely damaged homes:			
Damages and available resources:			
Repair cost (as percentage of home's replacement cost)	58	21	40,291
Private insurance (as percentage of home's replacement cost)	30	22	40,291
Equifax risk score (spatial moving average):			
<600	21		40,291
600-625	18		40,291
625-650	18		40,291
650-675	14		40,291
675-700	9		40,291
700-725	10		40,291
>725	9		40,291
Road Home participation:			
Nonparticipant	36		40,291
Rebuilding grant (option 1)	55		40,291
Relocation grant (option 2 or 3)	9		40,291
Home repaired by the pre-Katrina owner by year:			
1 year after Katrina	13		40,291
2 years after Katrina	21		40,291
3 years after Katrina	29		40,291
4 years after Katrina	47		40,291
5 years after Katrina	54		40,291

Source: Merged Orleans Parish Assessors Office property records and Lousiana Road Home administrative program microdata linked to block/tract/neighorhood background data from FEMA, the Displaced New Orleans Residents Survey, and the 2000 Decennial Census.

Table 2. The Impact of Financial Incentives on Rebuilding Choices

	(1)	(2)	(3)
	[First Stage]	[Reduced Form]	[2sls]
Dependent Variable:	Opportunity cost to not rebuilding (as frac. of home val.)	Home repaired by 5th Anniversary	Home repaired by 5th Anniversary
Opportunity cost to not rebuilding (as a fraction of home's value)			**0.244****
			(0.102)
Running variable > 51%	0.203***	0.049**	
	(0.010)	(0.020)	
Observations:	21,569	21,569	21,569
Controls:			
Quadratic in running variable	yes	yes	yes
Discontinuity x (Quadratic in running variable)	yes	yes	yes
Leave-one-out block average opportunity cost to not rebuilding	yes	yes	yes

Notes: This table reports estimates of the effects of financial incentives to rebuild on the probability of rebuilding. The sample includes households who were not fully insured and with a running variable (the Road Home repair cost estimate divided by the Road Home replacement cost estimate) between .33 and .67. The opportunity cost of not rebuilding is the smaller of the home's as-is value (foregone by households who accept a Road Home grant) and the cost of needed repairs not covered by insurance payments (foregone by households who sell privately). Source: Authors' calculations using Orleans Parish Assessor's Office administrative property data linked with administrative application/participation data from the Louisisana Road Home program.

Table 3. The Impact of one Household's Rebuilding Choice on the Choices of Neighbors

	(1)	(2)	(3)
	[First Stage]	[Reduced Form]	[2sls]
Dependent Variable:	Household i's home repaired by 5th anniv.	Household i's home repaired by 5th anniv.	Household i's home repaired by 5th anniv.
Household i rebuilds by Katrina's 5th Anniv.			**0.466**** **(0.232)** **[0.12 , 2.19]**[a]
Household i's running variable > 51%	0.049** (0.020)	0.023** (0.009)	
Observations:	21,569	21,569	21,569
Controls:			
Quadratic in running variable	yes	yes	yes
Discontinuity x (Quadratic in running variable)	yes	yes	yes
Leave-one-out block average opportunity cost to not rebuilding	yes	yes	yes

Notes: This table reports estimates of the effect of one households' choice to rebuild on the average rebuilding choice of same-block neighbors. The sample consists of all households in the "first-stage" equations who were not fully insured and with a running variable (the Road Home repair cost estimate divided by the Road Home replacement cost estimate) between .33 and .67. For each of these included observations, the outcome "neighbors' rebuilding rate" variable is computed using all same-block neighbors (leaving out the first-stage household). Source: Authors' calculations using Orleans Parish Assessor's Office administrative property data linked with administrative application/participation data from the Louisiana Road Home program.

[a]Weak-instrument robust 95% confidence interval (computed by inverting Anderson-Rubin test statistic).

Table 4. Model Parameter Estimates

Parameter:	Estimate
Observable heterogeneity in flow location payoffs: Z'γ	
Flood exposure:	
< 2 feet	0.029 (0.023)
2-3 feet (reference)	--
3-4 feet	-0.041 (0.04)
4-5 feet	0.009 (0.047)
5-6 feet	-0.104 (0.07)
> 6 feet	0.083 (0.02)
Demographic composition:	
Tract is majority college educated (reference)	0.092 (0.065)
Block is majority black	-0.038 (0.069)
1(t=1) x Tract majority noncollege	-0.207 (0.078)
1(t=1) x Block majority black	0.059 (0.088)
1(t=2) x Tract majority noncollege	-0.008 (0.104)
1(t=2) x Block majority black	0.047 (0.087)
1(t=3) x Tract majority noncollege	-0.016 (0.066)
1(t=3) x Block majority black	0.055 (0.019)
1(t=4) x Tract majority noncollege	-0.017 (0.091)
1(t=4) x Block majority black	0.036 (0.051)
δ_0: Intercept	-1.042 (0.043)
δ_1: Time trend	0.306 (0.03)
Unobserved heterogeneity in flow location payoffs:	
σ_η: Variance of idiosyncratic attachment to pre-Katrina block	0.739 (0.019)
σ_b: Variance of unobserved block effect	0.524 (0.006)
Spillover function: S x BetaCDF(μ; λ_1,λ_2)	
S: Spillover magnituge (flow payoff diff. b/w a 0% and 100% rebuilt block)	0.446 (0.018)
λ_1: Location of spillover threshold	0.702 (0.073)
λ_2: Steepness of spillover threshold	6.348 (2.606)
Observations - household-periods	334,615
Observations - households	66,923

Notes: This table reports indirect inference estimates of the equilibrium rebuilding model's structural parameters.
Source: Authors' calculations using Orleans Parish Assessor's Office administrative property data linked with administrative application/participation data from the Louisisana Road Home program.

Table 5. Decomposition of the Road Home Program's Impact

Group	(1) **No Grants**	(2) **Road Home:** ***private incentives only*** , (block amenities held constant)	(3) **Road Home:** **(**full equilibrium impact)
All Households	60.0	*+3.2*	*+5.2*
Flood exposure			
< 2 feet	63.1	*+3.2*	*+5.0*
2-3 feet	60.7	*+3.7*	*+6.1*
3-4 feet	58.4	*+3.5*	*+5.7*
4-5 feet	57.6	*+3.4*	*+5.3*
5-6 feet	53.4	*+3.2*	*+5.4*
>6 feet	62.0	*+2.6*	*+4.4*

Source: Authors' calculations computed using the estimated equilibrium model.

Table 6. Prevalence and Consequences of Tipping

A. Proportion of blocks with multiple simultaneous-move rebuilding-rate equilibria with and without the Road Home program

No grant program	Road Home program: Unique equilibrium	Road Home program: Multiple equilibria	Total
Unique equilibrium	84.9	***5.5 (tipping)***	90.4
Multiple equilibria	5.6	4.0	9.6
Total	90.5	9.5	100.0

B. Decomposing Road Home's impact: shifting a particular equilibrium and tipping

	(1)	(2)	(3)
	Block-level rebuilding rates (baseline) and program impacts		
Nature of block-level impact	**No Grants**	**Road Home:** ***private incentives only*** , (block amenities held constant)	**Road Home:** **(**full equilibrium impact)
Blocks that tip	50.1	*+6.5*	*+24.3*
Blocks that do not tip	74.3	*+1.8*	*+2.3*

Source: Authors' calculations computed using the estimated equilibrium model.

Table 7. The Consumption Equivalent of Positive Rebuilding Spillovers

Subgroup:	(1) Flow spillover benefit to inframarginal households	(2) Total value of Road Home option 1 grants	(3) Spillover as a % of spending
All sample household	$39.9M	$2,622.0M	1.5%
Flood exposure:			
< 2 feet	$7.9M	$422.1M	1.9%
2-3 feet	$6.4M	$419.0M	1.5%
3-4 feet	$7.0M	$454.6M	1.5%
4-5 feet	$6.0M	$423.7M	1.4%
5-6 feet	$3.6M	$241.0M	1.5%
> 6 feet	$8.9M	$661.7M	1.4%

Source: Authors' calculations computed using the estimated equilibrium model.

Table 8. Rebuilding Choices with Actual Road Home and with Unconditional Road Home

	(1)	(2)	(3)
	Fraction who rebuild under:		
Subgroup:	Both policies (attached household)	Actual R.H. only (marginal households)	Neither policy (detached households)
All sample household	71%	3.4%	25.5%
Flood exposure:			
< 2 feet	85.7	1.7	12.6
2-3 feet	63.6	4.0	32.4
3-4 feet	60.7	4.1	35.2
4-5 feet	59.0	4.3	36.7
5-6 feet	55.0	4.5	40.5
> 6 feet	60.4	6.1	33.5

	(4)	(5)
	Cost of option 1 grant payments to marginal households	Amenity improvements for "attached" households from marginal rebuilding
All sample household	$197.7M	$43.7M
Flood exposure:		
< 2 feet	$28M	$8.7M
2-3 feet	$26.9M	$5.8M
3-4 feet	$31.8M	$6.3M
4-5 feet	$29.7M	$5.9M
5-6 feet	$19.6M	$3.5M
> 6 feet	$61.7M	$13.4M

Source: Authors' calculations computed using the estimated equilibrium model.

Table 9. The Effect of Relocation Subsidies

	(1)	(2)	(3)	(4)	(5)	(6)
	Block-level rebuilding rates			Fraction of blocks with *no rebuilding*		
Ceterogy	No Grants	Impact of 100% buyouts	Impact of 125% buyouts	No Grants	Impact of 100% buyouts	Impact of 125% buyouts
All sample blocks	73.0	*-6.7*	*-8.8*	6.3	*+2.7*	*+3.6*
Flood exposure:						
< 2 feet	87.0	*-2.5*	*-3.8*	2.9	*+1.1*	*+1.6*
2-3 feet	62.6	*-9.6*	*-12.5*	8.9	*+3.5*	*+4.6*
3-4 feet	60.4	*-10.3*	*-13.5*	9.4	*+4.1*	*+5.6*
4-5 feet	55.0	*-10.4*	*-13.3*	12.3	*+5.9*	*+7.6*
5-6 feet	55.0	*-10.4*	*-13.3*	12.3	*+5.9*	*+7.6*
> 6 feet	62.3	*-10.3*	*-12.7*	8.8	*+3.5*	*+4.7*

Source: Authors' calculations computed using the estimated equilibrium model.

A Appendix

Figure A.1: McCrary Test Applied to Post-Appeal/Adjustment Running Variable

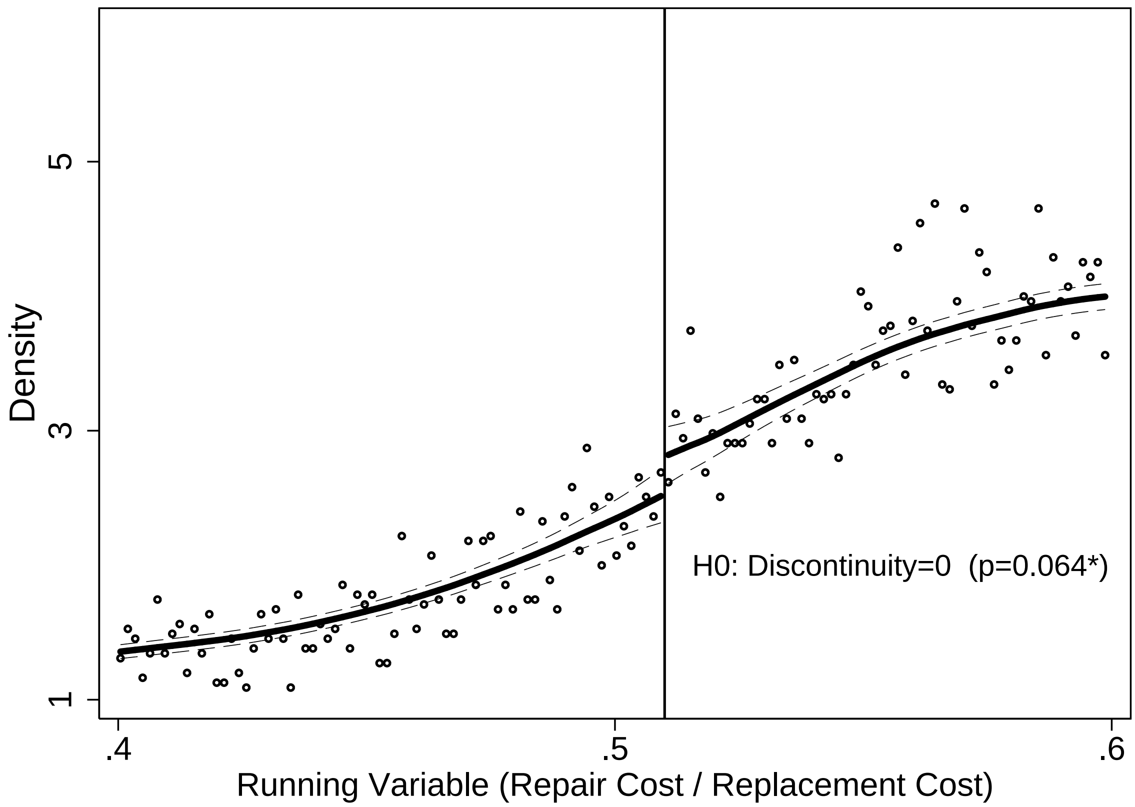


Figure A.2: Robustness of RD Estimates to Choice of Bandwidth and Polynomial Order

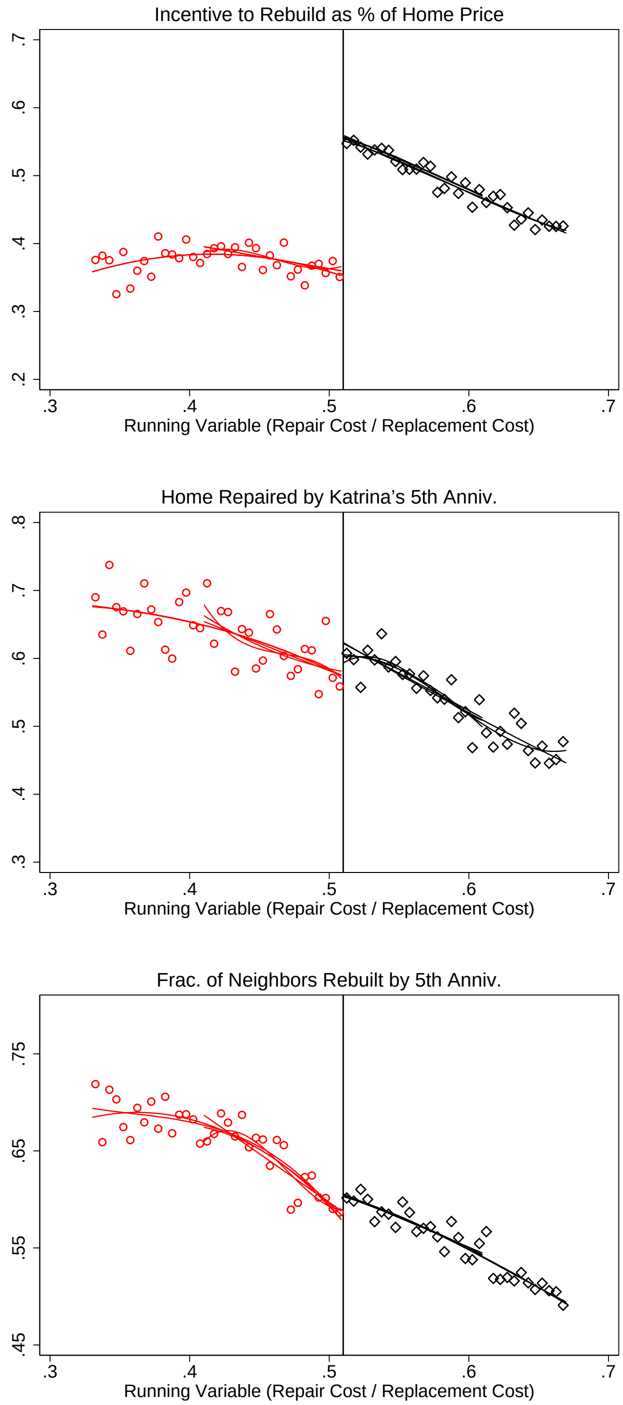


Table A1. Linear Probability Rebuilding Models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Rebuilt by Katrina's 1st Anniversary		Rebuilt by Katrina's 2nd Anniversary		Rebuilt by Katrina's 3rd Anniversary		Rebuilt by Katrina's 4th Anniversary		Rebuilt by Katrina's 5th Anniversary	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Spillovers:										
Indicator that zero neighbors have rebuilt	**-0.038*** **	**-0.047*** **	**-0.063*** **	**-0.067*** **	**-0.070*** **	**-0.075*** **	**-0.175*** **	**-0.160*** **	**-0.249*** **	**-0.220*** **
	(0.006)	**(0.006)**	**(0.006)**	**(0.006)**	**(0.006)**	**(0.006)**	**(0.011)**	**(0.012)**	**(0.017)**	**(0.018)**
Fraction of neighbors rebuilt	**-0.089*** **	**-0.080** **	**0.226*** **	**0.249*** **	**0.514*** **	**0.418*** **	**0.417*** **	**0.225*** **	**0.174*** **	**0.067**
	(0.033)	**(0.034)**	**(0.036)**	**(0.039)**	**(0.033)**	**(0.038)**	**(0.042)**	**(0.046)**	**(0.056)**	**(0.059)**
max(0, [Fraction of neighbors rebuilt]-1/3)	**1.495*** **	**0.509*** **	**0.990*** **	**0.504*** **	**0.476*** **	**0.219*** **	**0.216*** **	**0.211*** **	**0.341*** **	**0.258*** **
	(0.069)	**(0.069)**	**(0.069)**	**(0.073)**	**(0.062)**	**(0.07)**	**(0.063)**	**(0.067)**	**(0.074)**	**(0.078)**
max(0, [Fraction of neighbors rebuilt]-2/3)	**-0.153***	**0.008**	**-0.300*** **	**-0.338*** **	**-0.248*** **	**-0.400*** **	**0.085***	**-0.277*** **	**0.140*** **	**-0.115** **
	(0.082)	**(0.072)**	**(0.07)**	**(0.073)**	**(0.06)**	**(0.067)**	**(0.049)**	**(0.055)**	**(0.047)**	**(0.052)**
Flood exposure:										
< 2 feet flooding	-0.061***	-0.007*	-0.066***	-0.022***	-0.058***	-0.029***	-0.037***	0.012	-0.029***	0.011
	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)	(0.008)	(0.009)
2 - 3 feet flooding [reference]	-	-	-	-	-	-	-	-	-	-
3 - 4 feet flooding	-0.011***	0.001	-0.012**	0.001	-0.012*	0.004	-0.024***	-0.009	-0.033***	-0.024**
	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)	(0.008)	(0.01)
4 - 5 feet flooding	-0.004	-0.005	-0.008	-0.012*	-0.013**	-0.022***	-0.035***	-0.046***	-0.047***	-0.051***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.009)	(0.01)	(0.009)	(0.011)
5 - 6 feet flooding	-0.002	0	0.001	-0.01	-0.01	-0.024***	-0.035***	-0.052***	-0.062***	-0.070***
	(0.005)	(0.004)	(0.006)	(0.007)	(0.008)	(0.009)	(0.01)	(0.012)	(0.01)	(0.013)
> 6 feet flooding	-0.029***	-0.001	-0.036***	-0.022***	-0.043***	-0.034***	-0.087***	-0.087***	-0.123***	-0.117***
	(0.004)	(0.004)	(0.006)	(0.008)	(0.007)	(0.01)	(0.01)	(0.014)	(0.01)	(0.015)
Neighborhood characteristics:										
Block is majority black	-0.023***	-0.006	-0.037***	0.001	-0.031***	0.006	-0.019**	0.023**	-0.007	0.037***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.01)
Equifax risk score (S.M.A.) <600	-0.001		-0.021**		-0.003		0.032***		0.044***	
	(0.006)		(0.008)		(0.009)		(0.012)		(0.012)	
Equifax risk score (S.M.A.) 600-625	-0.014**		-0.030***		-0.005		0.024**		0.039***	
	(0.006)		(0.008)		(0.009)		(0.011)		(0.012)	
Equifax risk score (S.M.A.) 625-650	-0.006		-0.016*		0.002		0.033***		0.055***	
	(0.006)		(0.008)		(0.009)		(0.011)		(0.011)	
Equifax risk score (S.M.A.) 650-675	-0.010*		-0.015*		0		0.020*		0.041***	
	(0.006)		(0.008)		(0.009)		(0.011)		(0.011)	
Equifax risk score (S.M.A.) 675-700	0		-0.009		0.011		0.028***		0.041***	
	(0.006)		(0.008)		(0.009)		(0.011)		(0.011)	
Equifax risk score (S.M.A.) 700-725	-0.003		-0.013*		-0.006		-0.001		0.009	
	(0.005)		(0.008)		(0.009)		(0.01)		(0.01)	
Equifax risk score (S.M.A.) >725 [reference]	-		-		-		-		-	
Propert damage and available resources:										
Running variable spline (0,.3)	0.035	0.023	-0.053	-0.032	-0.076	-0.031	-0.123*	-0.064	-0.202***	-0.159**
	(0.041)	(0.034)	(0.053)	(0.051)	(0.057)	(0.057)	(0.065)	(0.066)	(0.063)	(0.064)
Running variable spline (.3,.4)	-0.422***	-0.389***	-0.436***	-0.379***	-0.656***	-0.626***	-0.802***	-0.806***	-0.847***	-0.869***
	(0.084)	(0.072)	(0.112)	(0.108)	(0.126)	(0.125)	(0.142)	(0.142)	(0.139)	(0.14)
Running variable spline (.4,.5)	0.121*	0.137**	0.045	0.141	0.095	0.209*	0.051	0.251*	-0.076	0.139
	(0.065)	(0.058)	(0.094)	(0.091)	(0.113)	(0.112)	(0.136)	(0.137)	(0.136)	(0.138)
Running variable spline (.5,.6)	-0.158***	-0.076*	-0.309***	-0.264***	-0.446***	-0.438***	-0.362***	-0.323***	-0.390***	-0.392***
	(0.046)	(0.042)	(0.069)	(0.067)	(0.087)	(0.086)	(0.114)	(0.114)	(0.116)	(0.116)
Running variable spline (.6,.7)	-0.067*	-0.045	-0.134**	-0.132**	-0.188**	-0.126*	-0.521***	-0.536***	-0.518***	-0.475***
	(0.036)	(0.034)	(0.058)	(0.06)	(0.074)	(0.075)	(0.108)	(0.113)	(0.113)	(0.117)
Running variable spline (.7,.8)	0.170***	-0.141***	0.285***	-0.003	0.449***	0.082	0.440***	0.259*	0.207	0.085
	(0.038)	(0.043)	(0.063)	(0.069)	(0.087)	(0.094)	(0.127)	(0.141)	(0.135)	(0.15)
Running variable spline (.8,1)	-0.057***	0.073***	-0.109***	0.01	-0.108**	0.033	0.201***	0.286***	0.322***	0.379***
	(0.018)	(0.019)	(0.031)	(0.032)	(0.044)	(0.046)	(0.066)	(0.07)	(0.07)	(0.074)
Insurance payout (as % of replacement cost)	0.006	0.013***	0.018**	0.029***	0.042***	0.053***	0.089***	0.111***	0.117***	0.131***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.009)	(0.009)	(0.011)	(0.012)	(0.011)	(0.012)
Constant	0.113***		0.212***		0.226***		0.358***		0.506***	
	(0.011)		(0.015)		(0.017)		(0.021)		(0.023)	
Observations	40,291	40,291	40,291	40,291	40,291	40,291	40,291	40,291	40,291	40,291
Census tract fixed effects:	no	yes	no	yes	no	yes	no	yes	no	yes

Source: Merged Orleans Parish Assessors Office property microdata and Lousiana Road Home administrative program microdata linked to block/tract/neighorhood background data from FEMA, the Displaced New Orleans Residents Survey, and the 2000 Decennial Census.

Table A2. RD Estimates: The "Effect" of *Post-Appeal* Financial Incentives on Rebuilding Choices

	(1)	(2)	(2)
	[First Stage]	[Reduced Form]	[2sls]
Dependent Variable:	Opportunity cost to not rebuilding (as frac. of home val.)	Home repaired by 5th Anniversary	Home repaired by 5th Anniversary
Opportunity cost to not rebuilding (as a fraction of home's value)			**0.288****
			(0.120)
Appraised damage share > 51%	0.171***	0.049**	
	(0.010)	(0.020)	
Observations:	21,569	21,569	21,569
Controls:			
Quadratic in appraised damage share	yes	yes	yes
Discontinuity x (Quadratic in appraised damage share)	yes	yes	yes
Leave-one-out block average opportunity cost to not rebuilding	yes	yes	yes

Notes: This table reports estimates of the effects of financial incentives to rebuild on the probability of rebuilding, computed using variables that reflect Road Home appeals and adjustments. The sample includes households who were not fully insured and with a running variable (the Road Home repair cost estimate divided by the Road Home replacement cost estimate) between .33 and .67. The opportunity cost of not rebuilding is the smaller of the home's as-is value (foregone by households who accept a Road Home grant) and the cost of needed repairs not covered by insurance payments (foregone by households who sell privately). Source: Authors' calculations using Orleans Parish Assessor's Office administrative property data linked with administrative application/participation data from the Louisisana Road Home program.