Targeting Disaster Aid: A Structural Evaluation of a Large Earthquake Reconstruction Program

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ABSTRACT

This paper studies the question of how to target aid after a natural disaster. Disaster aid programs often use property damage as a criterion for eligibility. A household's ability to insure against shocks is harder to observe, but it may be more important in determining how the disaster affects welfare. I develop a model of household demand for reconstruction aid, incorporating the exposure to a shock and the ability to borrow for consumption smoothing, and I calibrate the model using household survey data following the 2015 earthquake in Nepal. Conditioning aid on household property damage does not significantly improve welfare relative to allocating aid at random. An untargeted approach that divides the aid budget equally between all households in the affected areas yields larger welfare gains. Spending resources to assess physical damages for targeting purposes is thus unlikely to be cost effective.

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I. Introduction

Cash transfers to households after a natural disaster could be an effective way to mitigate the negative impacts of the disaster and promote reconstruction, especially when households lack insurance or other means of smoothing the shock. A common approach to targeting aid in post-disaster reconstruction programs is to make eligibility conditional on disaster-related property damage. The relevance of using property damage as a proxy for need is not well established in economic theory or empirically, however. In fact, this approach to targeting may increase inequality, since it tends to allocate a significant amount of aid to wealthy property owners (Howell and Elliott, 2019). Other forms of vulnerability, including barriers to credit and insurance markets, are harder to observe, but may be more important in determining a household's post-disaster welfare and ability to recover.

Yet property damage may be an attractive criterion for targeting because it is an easily visible mark of a disaster's effects. In the US, FEMA spends on average more than $1 billion annually on aid to homeowners who suffer housing damages in a disaster - 84% of all aid to individuals and households.[1] Internationally, shelter makes up the second largest portion of disaster aid funding, after food aid.[2] In disasters in low-income countries, funds earmarked for housing reconstruction are often the only sources of cash aid for households.

This paper studies the question of how to target aid after a disaster, using the 2015 Nepal earthquake as a case study. The 2015 earthquake was the

[1]Author's calculations from FEMA (2022). Average from 2010-2020.

[2]Author's calculations from OCHA (2022) using the three deadliest disasters of the 21st century: the 2010 Haiti Earthquake, the 2008 Myanmar Cyclone, and the 2005 Indian Ocean Earthquake/Tsunami.

largest in Nepal since 1934, and by some estimates left 12% of the country homeless (The Asia Foundation, 2016*b*). In the months after the earthquake, households could qualify for a large cash grant based on the degree of damage to their house.

I develop a theoretical framework to assess the effects of alternative disaster aid targeting strategies on welfare and reconstruction, and I apply this framework by calibrating a structural model to a panel of rural households covering the reconstruction period. The model assumes that households can draw upon savings, loans, and other forms of informal insurance to smooth shocks, but they face a borrowing constraint. I build evidence for these assumptions by exploiting a spatial discontinuity in aid delivery. Regression discontinuity estimates show that aid increases consumption and housing investment, but decreases remittances – a common form of informal insurance. These results are consistent with the model's predictions.

To compare the welfare benefits of alternative targeting approaches, I construct a measure of demand for reconstruction aid that is grounded in the structural model. This is measured as the amount of income a household would be willing to forgo in the future in exchange for receiving aid in the present. I show that this measure of demand corresponds with the preferences of a social planner that is concerned with helping households smooth consumption, but not redistributing wealth. Demand for aid goes down when households have alternative means of insurance, and increases when liquidity constraints bind. This measure of household value for aid does not correlate well with property damages for two reasons. First, some households that experienced damages have 'insurance' – access to loans and remittances that allow them to recover regardless of whether they receive aid. Second, some households have no property damage, but have high value for aid in order to meet their immediate need for consumption.

Property damage is thus a misleading proxy for need. The results of simulated counterfactual aid allocations show that making aid conditional on property damage is not much more effective than allocating it randomly. If aid had been perfectly targeted at the households sustaining the largest property damages, this would have increased total welfare by 4% relative to an allocation where recipient households are chosen at random. In contrast, an untargeted approach that divided the total amount of aid evenly among all households in the affected areas would have increased total household surplus by 12%. This universal allocation results in larger welfare gains because households have diminishing willingness-to-pay for aid. Thus spreading a smaller amount of aid across more households generates more aggregate welfare than targeting larger amounts of aid at fewer households – especially when the targeting is based on poor proxies for welfare. This implies that collecting better data on household damages to inform aid distribution may not be cost effective.

While these results assume the purpose of aid is for consumption smoothing, the qualitative findings are robust to an equal-weighted utilitarian social welfare function that prioritizes giving aid to the poorest households. I also explore 'reduced form' social welfare functions, that assume the planner wants to either increase food consumption or housing reconstruction. An untargeted approach to aid would also increase food consumption by more than damage-based targeting, although damage-based targeting results in more housing reconstruction.

This paper contributes to the growing literature on targeting aid and measuring misallocation by developing a framework for studying the targeting of disaster aid. The existence of nepotism and politics in the distribution of disaster aid has been well documented (Basurto, Dupas and Robinson, 2020; Tarquinio, 2022; Mahadevan and Shenoy, 2023), including in the context of the Nepal earthquake (Bhusal et al., 2022; Pathak and Schündeln, 2022). I build on that work by showing that even well-intentioned targeting strategies can fail to meaningfully enhance welfare.

I develop a way to measure the misallocation of aid, taking into account different possible objectives of disaster aid. In the context of anti-poverty programs, targeting strategies are often evaluated by how accurately they direct aid to the lowest-consumption households, or the households with the highest marginal value for cash (Niehaus et al., 2013; Hallegatte et al., 2016; Hanna and Karlan, 2017; Hanna and Olken, 2018; Alatas et al., 2019; Aiken et al., 2022; Haushofer et al., 2022; Banerjee et al., 2023). Disaster aid, on the other hand, might seek to help recipients recover from a shock, regardless of how well-off they were initially. My measure of demand for aid can be interpreted as a household's willingness-to-pay to smooth a shock. This measure has the additional feature of distinguishing a household's willingness-to-pay from their ability-to-pay, which is relevant when households face credit market failures.

Previous work using structural models to study the welfare implications of disaster aid has mainly focused on programs in the US after Hurricane Katrina (Gregory, 2017; Fu and Gregory, 2019). Their focus is on program design to minimize moral hazard and maximize rebuilding externalities is likely to be more important in an urban context in a wealthy country, whereas Banerjee et al. (2023) argues that targeting, the focus of this paper, is the first order concern in the design of social insurance in low-income countries.

In addition to the welfare analysis, this paper contributes evidence on the causal effects of disaster aid on consumption, income, and investment in a low-income country using a spatial regression discontinuity approach. The research design relies on the fact that households in certain districts were prioritized for aid, and that households close to the borders of those districts experienced similar levels of earthquake damages, but differed in their likelihood of receiving aid.

The results show that receipt of aid has large but imprecisely estimated effects on household food consumption and housing reconstruction. Aid also substituted for some types of informal insurance including remittances and migration. These effects are consistent with a model of partially insured households, and similar to the magnitudes predicted by the structural model.

I also show evidence that there is considerable heterogeneity in households' ability to smooth consumption. The forms of insurance used in this context – mainly loans, remittances and migration – are not equally available to all households. In particular, wealthy households are less likely to experience damages from the disaster, but conditional on damages, they are also better able to recover by drawing on sources of informal insurance. This could explain why some studies find a correlation between disaster exposure and inequality (Howell and Elliott, 2019).

Heterogeneity in the ability to smooth shocks also explains why the effects of disasters vary across contexts. Previous work has found that the negative effects of disasters on GDP is larger in poorer countries (see Kellenberg and Mobarak 2011; Dell, Jones and Olken 2014; Botzen, Deschenes and Sanders 2019; and Kousky 2019 for reviews). In the US, studies using administrative data often show transitory impacts on income for even very large disasters, though recovery is aided by reliance on insurance and formal social safety nets that may not be as robust in low-income countries (Deryugina, 2017; Deryugina, Kawano and Levitt, 2018; Gallagher and Hartley, 2017; Billings, Gallagher and Ricketts, 2022; Gallagher, Hartley and Rohlin, 2023). Closest to this paper, Tarquinio (2022) finds that drought declarations in India increase household consumption, but the effect is smaller when the declarations are poorly targeted.

I show that there is significant heterogeneity in household value for aid, so in theory targeting could be important. These values are not correlated with disaster property damage, however. This seems to suggest that the effects of the earthquake on housing were just a small portion of total need relative to existing market failures and other sources of household vulnerability. If these vulnerabilities are not easily observable, giving a smaller amount of aid to a larger number of people might be a better strategy than attempting to target.

The remainder of the paper is structured as follows. Section II provides context on the earthquake and the aid program, and shows descriptive evidence of the means by which households smooth consumption, as well as heterogeneity in ability to draw upon these forms of informal insurance. Section III presents a behavioral model grounded in these descriptive facts, and calibrates the model to estimate household demand for aid. Section IV tests the model's predictions using a spatial regression discontinuity in the allocation of aid. Section V then uses the calibrated model to analyze the implications of counterfactual targeting strategies on welfare and reconstruction.

II. Context and Data

With a 2015 per capita GDP of $3,330 PPP, Nepal ranks as one of the poorest countries in the world outside of Africa. Two-thirds of households are employed in agriculture or livestock rearing,[3] and the economy remains heavily dependent on migrant labor and remittances as means of subsistence (Raut and Tanaka, 2018; Lokshin, Bontch-Osmolovski and Glinskayai, 2007), and as a means of insuring against environmental shocks (Maystadt, Mueller and Sebastian, 2016).

The Himalayan portion of the country is a subduction zone that experiences frequent earthquakes. In April 2015 the Gorkha earthquake, named after the district where it occurred, measured a 7.8 on the Richter scale - the largest earthquake in Nepal since 1934. The earthquake also triggered landslides throughout the region flattening entire villages. The final death toll was nearly 9,000 people, and an estimated 12% of the country had their homes destroyed (The Asia Foundation, 2016*b*).

In the fourteen most severely affected districts, several small emergency cash grants were distributed within six months of the earthquake. Households that had their homes completely destroyed typically received 25,000 NPR (about $250) to buy emergency supplies and procure shelter before winter set in. Village Development Committee (VDC) leaders compiled initial lists of households eligible for benefits, with some discretion in their ability to do so (The Asia Foundation, 2016*b*; Pathak and Schündeln, 2022).

The earthquake took place in a context of poor state capacity. Nepalis endured a decade of Civil War from 1996-2006, and another eight years of

[3]Data from World Bank. In 2015 exchange rates averaged 100 NPR to 1 USD from January-April

transitional governments in the lead-up to the earthquake. Shortly after the earthquake, Nepali delegates ratified a constitution to form the basis of a new federal government. Controversies over representation lead to protests and a blockade of the border with India, resulting in fuel shortages. This, along with a drought in 2016, greatly increased food insecurity (Randell et al., 2021; Wagle, 2021).

An initial needs assessment conducted after the earthquake identified rural housing reconstruction as the largest need area by far (Government of Nepal National Planning Commission, 2015). A multilateral donor fund pledged $4.1B for household grants to rebuild earthquake resilient houses, with the grants initially targeted at the fourteen most affected districts (Nepal Earthquake Housing Reconstruction Program Multi Donor Trust Fund, 2016).

Households qualified for reconstruction grants if their house required complete rebuilding, as certified by teams of engineers sent to the fourteen districts by the central government. This resulted in new beneficiary lists that were, in many cases, significantly less generous than the initial VDC lists, leading to more protests and unrest in some districts. Although some households had begun repairs, The Asia Foundation (2016*a*) found that 75% of displaced households were still living in temporary shelters 18 months after the earthquake, with many more moving back into their partially reconstructed and potentially unsafe homes.

For households that qualified, the reconstruction grants were delivered in three tranches, with engineers certifying progress on rebuilding before each disbursement. The first funds were delivered in early 2016, a full year after the earthquake. By July 2018, 60% of eligible households in the most-affected districts had received at least two installments, compared to only

15% of eligible households outside those districts (Housing Recovery and Reconstruction Platform, 2018). These reconstruction grants are the focus of the targeting analysis in this paper, as they were much larger than the emergency aid disbursements discussed above.

Within villages, earthquake damages negatively correlated with income, assets, and education (see Appendix C). This seemed mainly to reflect wealthier households using better building materials, as cement and reinforced concrete buildings fared better on average. The aid response did not favor the poor, however. Bhusal et al. (2022) find that elite caste members received more aid, even after controlling for assessed damages. This bias was partially reversed in VDCs with mayors from non-elite castes, however.

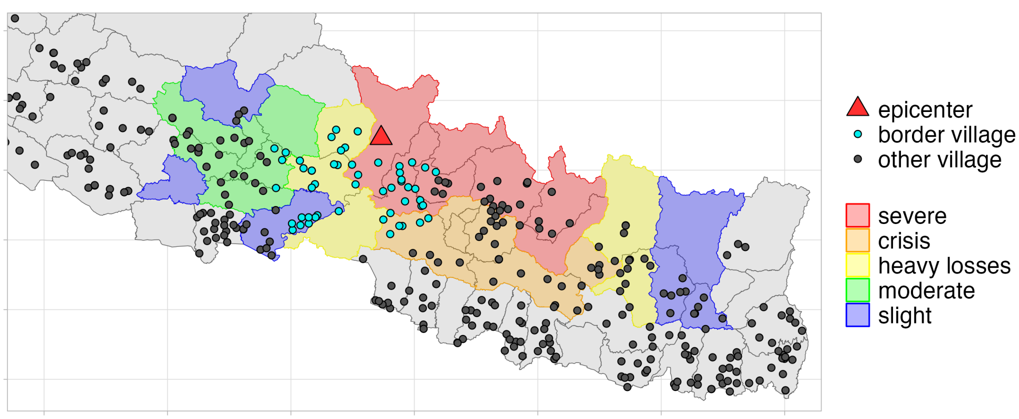

FIGURE 1. MAP OF STUDY REGION

Notes: District colors denote earthquake damage designations from Housing Recovery and Reconstruction Platform (2018). 'Severe' and 'Crisis' districts were prioritized for aid. Black circles represent VDCs present in the survey. Blue circles are VDCs within 59 km of the eastern border of the 'Severe' and 'Crisis' districts. This is the bandwidth selected for inference in the first stage of the regression discontinuity analysis (see Section IV).

A. *Data and Descriptive Statistics*

The World Bank Household Risk and Vulnerability Survey (WBHRVS) is a 3-wave representative panel survey of 6,000 rural households from across Nepal that covers 2016-18, with one panel corresponding to each year in that time period (Walker, Kawasoe and Shrestha, 2019). The survey collected data on household asset, livelihood, and consumption variables, as well as shocks experienced and coping strategies. It also records whether households received earthquake reconstruction aid. The locations of sample households are shown in Figure 1. I use data from the US Geological Survey to measure peak ground acceleration during the earthquake, slope and elevation. Finally, to look at the correlates of earthquake damages in Appendix C, I use the post-earthquake building census conducted by the National Reconstruction Authority.

Average income of the households in the sample is 275,000 NPR, as shown in Appendix Table B1. This does not include remittances and transfer income, which, given an average household size of 4.66, comes out to approximately $1.60 per person per day. This goes up to nearly $2 when including remittance income.

Long-term migration is an important livelihood strategy. Nearly half of the households in the sample are economically connected to a migrant, with many connected to more than one. More than half of the migrants mentioned by the households in the sample were in another country – with India, Southeast Asia, and the Middle East as the main destinations. 70% of the migrants left more than a year before the survey.

Previous work has shown the importance of state contingent loans as a mechanism for risk-sharing among rural households (Udry, 1994). Loans are important in this context as well, with the average household taking 28,000 NPR in short term loans in the past year. The most common source of loans is from friends and relatives (approximately 40%) with money lenders and savings groups accounting for most of the rest. Formal banking services account for a very small fraction of loans. The survey finds substantial food insecurity, with 10% of households reporting that they needed to skip a meal in the last 30 days, although this varied significantly over time (from 24% in the first wave immediately following the earthquake, down to 1% two years later).

Of the twenty-one percent of households that report experiencing losses from the earthquake, 97% of those report asset losses, compared to 22% reporting income losses – consistent with findings in the post-disaster needs assessment prioritizing housing as the main focus of reconstruction. In addition to the nature of the earthquake as a shock that primarily destroys fixed capital, another possible explanation for this distribution of losses is that many households' primary source of income comes from migrants overseas which was unaffected by the earthquake. Thirteen percent of households received at least one installment of either emergency or reconstruction aid.

B. Consumption Smoothing

Targeting could increase the value of aid as a form social-insurance if it is directed towards households that are least able to smooth consumption (Chetty, 2006). To test for heterogeneity in household ability to smooth consumption, I use Townsend-style regressions, that regress income on consumption conditional on household, and village-year fixed effects (Townsend, 1994). This tests whether idiosyncratic income shocks correlate with household consumption. The results in Table 1 show statistically significant coefficients on income, rejecting the null hypothesis of perfect smoothing. For a 10% reduction in income, households reduce food consumption by 1.2% on average.

There is important heterogeneity across several subgroups in the sample, however. The second column explores heterogeneity by caste and ethnicity. Members of the economically and politically powerful Newar caste smooth consumption more effectively than other groups as indicated by the negative coefficient on the interaction term. This could be consistent with Bhusal et al. (2022) findings on preferential access to aid amongst the politically dominant caste. The last three columns show heterogeneity by sex of household head, value of landholdings, and earthquake exposure. Female headed households and households with above median landholdings smooth consumption more effectively.

Landownership is a proxy for wealth, and the results suggest that wealthier households are better insured. The negative coefficient on female-headed households might be somewhat surprising in the light of often presumed increased vulnerability amongst these households, however one potential explanation is that 62% percent of these households receive remittances, compared to 27% of male-headed households, and remittances tend to be strongly counter cyclical, as seen in Table 2.A. Notably, earthquake exposed households seem to smooth no worse on average, though this combines the effect of the earthquake with the effect of the aid. Appendix Tables B2 and B3 show robustness to caste-year fixed effects, and using rainfall as an instrument for income.

To test which types of transfers are pro- or counter-cyclical with respect

TABLE 1—CONSUMPTION SMOOTHING

	log(food consumption)				
	(1)	(2)	(3)	(4)	(5)
log(income)	0.116***	0.114***	0.114***	0.126***	0.113***
	(0.008)	(0.010)	(0.008)	(0.008)	(0.007)
log(income):Dalit		0.011			
		(0.017)			
log(income):Newar		-0.049*			
		(0.020)			
log(income):Other		0.002			
		(0.010)			
log(income):Female Head			-0.012***		
			(0.002)		
log(income):Land > Median				-0.021*	
				(0.008)	
log(income):Quake Affected					0.013
					(0.015)
Num.Obs.	16763	16762	16763	16763	16763
R2	0.763	0.763	0.764	0.763	0.763
SE Cluster	Strata	Strata	Strata	Strata	Strata
Household FE	X	X	X	X	X
Village-Year FE	X	X	X	X	X

Notes: All regressions include household and village-year fixed effects. Standard errors clustered at the survey strata. Observations with zero or missing income, or less than 3 observations dropped. Land percentile defined based on household land value during first survey wave. Omitted category for caste is "Brahmin/Chhetri". + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

to household income fluctuations, I regress log income on various types of transfers, conditional on a household fixed effect. The columns in Table 2.A show that remittances, migration, and loans negatively correlate with within-household income fluctuations, suggesting that they are used as forms of informal insurance. Cash savings are pro-cyclical, suggesting that households save in a good year and draw on savings in bad years. Earthquake aid is positively correlated with income shocks, possibly because it arrived more than a year after the event in most cases. Government transfers are also pro-cyclical, highlighting the absence of formal social insurance that responds to shocks.

To test how various forms of transfers correlate with wealth, I regress a dummy for above median landholdings on various forms of transfer income, conditional on a village-year fixed effect that captures aggregate shocks to the village. Table 2.B shows that cash savings, remittances, migration, and loans in a given year are positively correlated with household landholdings within villages, suggesting that these sources of support are more easily available to wealthier households.

Taken together these correlations indicate that households vary in their ability to smooth income shocks, possibly due to differential access to savings, loans, remittances and migration opportunities. This implies that aid might have greater benefits if targeted towards households that lack these forms of informal insurance. In the next section, I present a model of household behavior and a normative framework that formalizes these intuitions.

III. Model of Willingness-to-pay for Reconstruction Aid

A. Household Behavior

In order to conceptualize how household value for reconstruction aid may vary, I draw on a stylized model of household behavior that captures key sources of heterogeneity. These include varying levels of damage from the earthquake, abilities to self-insure, and earnings potential. Ability to self-insure is particularly important for a large covariate shock in a developing country where informal networks form an important risk sharing mechanism (Munshi and Rosenzweig, 2016; Morten, 2019). I capture these sources of heterogeneity in a dynamic model of incomplete insurance (Deaton, 1991; Aiyagari, 1994; Banerjee and Duflo, 2005; Kaboski and Townsend, 2011)

Table 2—Transfers

A. Within Household Variation

	Earthquake Aid (1)	NGO Transfers (2)	Govt Transfers (3)	Informal Transfers (4)	Cash Savings (5)	Remittances (6)	Migrants (7)	Loans Taken (8)
log(income)	2.358+	-0.221	0.001***	0.261	7.776**	-8.578***	-0.051**	-3.495+
	(1.171)	(0.206)	(0.000)	(0.191)	(1.888)	(1.658)	(0.012)	(1.753)
Num.Obs.	16763	16763	16763	16760	16685	16763	16763	16748
R2	0.493	0.333	0.651	0.342	0.496	0.604	0.374	0.354
SE Cluster	Strata	Strata	Strata	Strata	Strata	Strata	Strata	Strata
Fixed Effects	Household	Household	Household	Household	Household	Household	Household	Household

B. Within Village-Year Variation

	Earthquake Aid (1)	NGO Transfers (2)	Govt Transfers (3)	Informal Transfers (4)	Cash Savings (5)	Remittances (6)	Migrants (7)	Loans Taken (8)
Land > Median	-0.006	-0.102	0.001	-0.173	34.395***	21.192*	0.041*	3.963+
	(0.348)	(0.179)	(0.000)	(0.726)	(3.023)	(7.149)	(0.015)	(1.920)
Num.Obs.	16763	16763	16763	16760	16685	16763	16763	16748
R2	0.488	0.362	0.121	0.078	0.141	0.140	0.141	0.109
SE Cluster	Strata	Strata	Strata	Strata	Strata	Strata	Strata	Strata
Fixed Effects	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year

Notes: Panel A shows income regressed on various forms of transfer income and a migration dummy conditional on a household fixed effect. Panel B shows the results of a regression of forms of transfer income and a migration on dummy on an indicator for land value greater than median in the first survey wave conditional on a village-year fixed effect. All dependent variables except migration in thousands of NPR. Households with zero or missing income or less than 3 observations dropped. Standard errors clustered at the survey strata. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

that incorporates the option to invest in a durable housing stock. Once calibrated to the data, the model allows me to analyze the welfare implications of counterfactual aid targeting strategies.

In the model, households indexed by i choose food consumption c, and housing investment ι in period t, to maximize an infinite discounted stream of utility deriving from food consumption and a housing stock, h:

$$U_i = E[\sum_{t=0}^{\infty} \beta^t u(c_{it}, h_{it}, \iota_{it})] \tag{1}$$

where β is the discount factor. For $u(c, h, \iota)$ I use a standard isoelastic functional form: $u(c, h, \iota) = \frac{(c^{\alpha}(h+\iota)^{(1-\alpha)})^{(1-\gamma)}}{1-\gamma}$, that allows for estimation of the importance of housing relative to food consumption, as well as risk aversion between periods (Yang, 2009; Francisco, 2019). h_{it} measures the economic value of the housing stock in common units wth food consumption. The housing stock evolves according to:

$$h_{it+1} = \delta(h_{it} + \iota_{it}) \tag{2}$$

with depreciation factor δ. In each period t, households receive a stochastic income shock, Y_{it} drawn from $log(Y_i) \sim N(\mu_i, \sigma^2)$. This allows households to differ in their expected earnings, but they are assumed to have a common variance of log income. Given the income process, household expected income is $\exp(\mu_i + \sigma^2/2) := M_i$.

In order to smooth consumption, households can borrow and save at interest rate R. Liquidity at the beginning of each period is denoted as x_{it}, which evolves according to:

$$x_{it+1} = R(x_{it} - c_{it} - \iota_{it}) + Y_{it}. \tag{3}$$

Finally, I assume that housing markets are illiquid, and households are subject to a borrowing constraint, B, so households choose c and ι to maximize (1) subject to (2), (3), and:

$$x_{it} - c_{it} - \iota_{it} \geq B \tag{4}$$

$$\iota_{it} \geq 0. \tag{5}$$

The latter constraint ensures that households cannot liquidate their housing stock to fund consumption, which is a realistic feature of rural housing markets in many low-income countries. Because B may be negative, if x_{it} is sufficiently close to B, and $R > 1$, a household could exceed the borrowing constraint, with $x_{it+1} < B$ even if c_{it} and ι_{it} are zero. To avoid this, I follow Kaboski and Townsend (2011) in allowing households to default, which I observe in the data. In the case of default, households receive a minimum consumption and housing bundle:

$$(6) \qquad c_{it} = \underline{c}; \iota_{it} = 0; \text{ and } h_{it} = \max[h_{it}, \underline{h}]; x_{it+1} = RB + Y_{it}$$

These minimum consumption and housing bundles, as well as the borrowing limit B are assumed to be some fraction of steady state consumption in an unconstrained model: $\underline{c} = \bar{c}\alpha M_i$, $\underline{h} = \bar{h}(1-\alpha)\frac{\delta}{1-\delta}M_i$, and $B = \lambda M_i$, where $\bar{c}, \bar{h}$, and λ are parameters to be estimated.

The effects of the earthquake can show up either as destroyed housing stock, or as a temporary income shock. I take the permanent component of income, μ_i, as exogenous. While endogenizing the income process in this model would be possible with a few additional parameters (see e.g. Kaboski and Townsend 2011), this more parsimonious and tractable specification can be justified for two reasons. First, it is consistent with the findings of the post-disaster needs assessment and the survey data discussed above, that emphasized the effects of the earthquake on housing rather than income. Furthermore, I test whether earthquake losses are associated with lower income in Appendix Table C1, and I find a null effect across a range of specifications. Secondly, since the survey data only covers the period following the earthquake, expected income should be thought of as post-earthquake expected income – which is what would be recorded in a post-disaster needs assessment.

I model aid (a_{it}) by assuming it is additive to liquidity such that $x_{it+1} =$

$R(x_{it} + a_{it} - c_{it} - \iota_{it}) + Y_{it}$. While the aid was supposed to be used for building earthquake resilient housing, this specification allows for a certain degree of fungibility, and for reasonable parameter values, households with very poor housing stock will use the extra liquidity for investment.

Finally, all state and control variables are normalized by expected income, M_i, which reduces the dimensionality of the state space. If $V(.)$ is the value function associated with the maximization of (1), define $\nu(.) = M_i^{\gamma-1}V(.)$. This allows us to express x, h, c, and ι as fractions of expected income, which is done in everything that follows without changing notation. To avoid additional confusion, I redefine y_i as the log-normal random variable Y_i divided by its expected value: M_i. Note that $log(y_i) = log(\frac{Y_i}{M_i}) \sim N(-\frac{\sigma^2}{2}, \sigma^2)$. These transformations are fully elaborated in Appendix A.

Combining (1) - (6), the households problem can be written recursively, with the Bellman equation:

(7)

$$\begin{aligned} M_i^{\gamma-1}V(x_{it}, h_{it}|M_i) = \nu(x_{it}, h_{it}) = \\ \max\Big\{ \max_{c_{it},\iota_{it}} u(c_{it}, h_{it}, \iota_{it}) + \beta E\big[\nu\big(R(x_{it} - c_{it} - \iota_{it}) + y_i, \delta(h_{it} + \iota_{it})\big)\big], \\ \nu_d(h_{it})\Big\} \end{aligned}$$

subject to the normalized versions of (4) and (5), where

$$\nu_d(h_{it}) = u(\bar{c}\alpha, \max[h_{it}, \bar{h}\alpha\frac{\delta}{1-\delta}], 0) + \beta E\Big[\nu\big(R\lambda + y_{it}, \delta\max[h_{it}, \bar{h}\alpha\frac{\delta}{1-\delta}]\big)\Big]$$

represents the value function in a state of default. The first term within the curly brackets in (7) is the value of households maximizing the value of current period consumption and flows from their housing stock, plus the discounted continuation value of solving the same problem in the next period, with the new values of their stocks of housing and liquidity. The second term is the value of defaulting and consuming the minimum consumption and housing bundle.

The model introduces a lot of structure to household decision making. Two other assumptions are worth discussing in more detail. First of all, the model is partial equilibrium, with exogenous income and interest rates. While a general equilibrium model would certainly be of interest, given the cost in terms of complexity, a partial equilibrium approach can be justified given that I hold the total amount of aid constant across all counterfactuals. Even if inflows of aid have substantial macro-economic consequences, it is less likely that different allocations of the same amount of aid would have dramatically different implications for credit or input markets.

Secondly, this framework does not allow for externalities resulting from household decisions. In their structural analysis of disaster aid in the aftermath of Hurricane Katrina, Fu and Gregory (2019) find the existence of rebuilding externalities. Agglomeration economies are less important in rural Nepal than in New Orleans, possibly reducing the importance of these externalities. On the other hand, to make their model tractable, Fu and Gregory (2019)'s model features deterministic income, whereas in rural Nepal income is stochastic and highly variable, justifying my shift in emphasis.

Despite these limitations, the key features of the model are informed by the data and context. Furthermore, when calibrated to the data, this structure permits an analysis of the relative importance of housing investments, consumption, and savings. The way households make tradeoffs between these choices will allow me to coherently define a notion of a household's demand for aid and analyze the welfare of counterfactual targeting strategies.

B. Demand for Aid

Judging the merits of different allocations of aid inherently requires making interpersonal comparisons between households. Furthermore, assuming the planner has a fixed budget means that giving aid to one household requires taking it away from another – limiting the scope for static Pareto improvements.

Much of the targeting literature that has focused on poverty alleviation has used a utilitarian social welfare function to assess benefits (Hallegatte et al., 2016; Aiken et al., 2022; Haushofer et al., 2022). Given a concave value function, this functional form favors redistribution to the poorest households, which is usually not the explicit criteria of disaster reconstruction programs. If it was, aid programs would not condition aid on housing damages, unless housing damages were a good proxy for chronic poverty. I will show evidence that this is not the case in this context.

On the other hand, analyses of the allocation of non-market goods frequently seek to maximize willingness-to-pay – a metric that makes little conceptual sense when considering cash aid, since the nominal value of the aid is the same to all households. Furthermore, maximizing willingness-to-pay for disaster aid also seems at odds with intuitions about the purpose of such programs.

Instead, in order to compare various allocations, I adopt a social welfare function (SWF) consistent with the idea that the purpose of disaster aid is to help households smooth consumption and maintain their quality-of-life in the aftermath of a large shock. Intuitively, this type of objective would be more consistent with a targeting strategy based on disaster exposure.

Formally, I measure what percentage of future income a household would be willing to give up in exchange for the amount of aid disbursed by the reconstruction program. This may be thought of as a measure of dynamic and unconstrained willingness-to-pay. I infer this value by solving for a tax on future income, τ_i^t, such that a household is indifferent between their current liquidity and income, and receiving the aid, but paying a tax on future income:

$$V(x_{it}, h_{it}|M_i) = V(x_{it} + a_{it}, h_{it}|(1-\tau_i^t)M_i). \tag{8}$$

This is the household's compensating variation in terms of future income.[4] The t superscript denotes that the household's benefits will depend on the value of the state variables in period t. The household's 'willingness-to-pay' (WTP) for aid can be calculated as the discounted sum of expected payments:

$$WTP_{it} = \sum_{t=0}^{\infty} \beta^t(\tau_i^t M_i) = \frac{\tau_i^t M_i}{1-\beta}. \tag{9}$$

Thus, rather than take a stance on the socially optimal amount of redistribution, this SWF allows me to analyze an upper-bound for the value of targeting based on damages. I also compare the results to the equal-weighted utilitarian SWF.

[4]Given the value function normalization described above, the percentage of expected income a household would be willing to give up can be expressed as:

$$\tau_i^t = 1 - [\frac{\nu(x_{it}, h_{it})}{\nu(x_{it} + a_{it}, h_{it})}]^{\frac{1}{1-\gamma}}.$$

Measuring demand for aid in this manner is like asking a household how much they would be willing to borrow from their future self in order to smooth the present shock. It is only useful in the context of a borrowing constraint, since if markets were complete, households could borrow at the interest rate. In the model, however, households might be willing to pay above market interest rates (and be able to repay in expectation), but they are unable to find a lender willing to offer those terms. In this way, willingness-to-pay can be measured separately from ability-to-pay.

This form of SWF satisfies some desirable theoretical properties, including an idealized form of inter-temporal Kaldor-Hicks efficiency. If households were allowed to make transfers through time, households that don't receive aid could be compensated with the future income of those that do to create a Pareto improvement. Further, in the idealized scenario where the social planner has a fixed budget but can borrow without constraint, and repayment was guaranteed, the transfers could take place up front.

In addition, this measure of demand is connected to previous work on social welfare functions and non-market valuation. It is well known that maximizing a social welfare function weighted by the inverse marginal utility of income – so called Negishi weights – is equivalent to maximizing willingness-to-pay (Negishi, 1960). Negishi weights are often used for non-market valuation to separate distributional questions from questions of efficiency by replicating the market allocation that would occur under complete markets and freezing the distribution of income (Nordhaus and Yang, 1996). In the case of cash aid, however, since cash is fungible with income up to the interest rate, a Negishi weighted social welfare function will not differentiate between allocations.

For comparison, a social welfare function that maximizes the sum of equation (9) for all households allocates aid to the households that have the highest value of $\tau_i^t M_i$, or ΔM_i, since it represents a discrete change in expected household income. Defining $\frac{\Delta V}{\Delta A_i}$ and $\frac{\Delta V}{\Delta M_i}$ as the discrete analogs of the derivative of the value function with respect to aid and average income, some algebraic manipulation from equation (8) shows that:

$$\frac{\frac{\Delta V}{\Delta A_i}}{\frac{\Delta V}{\Delta M_i}} = \frac{\Delta M_i}{\Delta A_i}. \tag{10}$$

Since the amount of the aid package is the same across all households, allocating aid to households with the highest ΔM_i is equivalent to allocating aid to households with the highest $\frac{\frac{\Delta V}{\Delta A_i}}{\frac{\Delta V}{\Delta M_i}}$. This is exactly the result that would occur from maximizing a social welfare function weighted by $(\frac{\Delta V}{\Delta M_i})^{-1}$. So instead of weighting by the inverse marginal utility of income, this results in weighting by the inverse marginal utility of *expected* income. Therefore this social welfare function can be seen as freezing the distribution of expected lifetime resources. In other words, the social planner is interested in helping households smooth temporary shocks, but not in redistributing long-run wealth.

Although specifying welfare in this way is subject to the usual ethical critiques of Negishi weights (see Stanton 2009), my intent is to show that even under a specification of social welfare that sets aside equity concerns, conditioning aid on disaster exposure is suboptimal. This is due to the fact that this form of targeting does not take into account household ability to smooth consumption through borrowing.

While wealthy households with damages may have a higher 'ability-to-pay' due to their higher expected income, they may be able to satisfy their reconstruction needs by drawing on savings and borrowing at the market rate R. Households with little savings or informal insurance, however, might be willing to pay significantly more than R to meet current needs, but are unable to do so due to incomplete credit markets.

One potential objection to specifying welfare in this manner is that households may have high WTP for reasons that have nothing to do with the earthquake. A household may be liquidity constrained due to a sequence of bad harvests, or unrelated macroeconomic factors, for example. On the other hand, this can be seen as a positive feature of this SWF, since what is determined to be a 'disaster' may be quite arbitrary, and determined by political economy considerations (Garrett and Sobel, 2004), or whether there is a competing news cycle (Strömberg, 2007). It's hard to imagine that these arbitrary considerations matter much to the potential recipients of the aid. To be precise, however, this measure of demand is appropriate for analyzing a policy that aims to improve consumption smoothing, and would be appropriate if we hope to use disaster aid as a form of social-insurance (Chetty, 2006). Thus one way of interpreting the results is as a measure of what fraction of the shocks faced by households in the survey are related to earthquake induced property damages.

Furthermore, while my measure assumes household expected income is invariant to aid, if households could make investments to increase their incomes, this could change the optimal targeting strategy, making it more desirable to target households based on investment opportunities. On the other hand, there is little reason to believe that the opportunity to make profitable investments should be correlated with earthquake damages, especially in the rural agricultural sector in Nepal. If true, this suggests that my results will overestimate the relative value of damage-based aid targeting in this setting, since they do not account for these investment opportunities.

Lastly, it is also important to note that, since the data is collected starting from one year after the earthquake, these results are best interpreted as relevant for long-term reconstruction aid, rather than emergency aid. In fact, long-term reconstruction was the primary goal of the major aid program that I analyze. Smaller amounts of aid an in-kind goods were distributed in the immediate aftermath of the earthquake, but most of the total aid budget was disbursed one to three years after the earthquake, which is not uncommon for major disasters.

C. Model Calibration

In order to estimate individual households' WTP for aid, I calibrate the behavioral model outlined in Section III.A to the households in the data using the method of simulated moments (MSM) with the optimal weighting matrix (Hansen, 1982).

In addition to the state variables and parameters described in the previous section, I follow Kaboski and Townsend (2011) by introducing multiplicative measurement error in household expected income. I draw a vector of log normally distributed measurement error shocks with mean zero and log variance of σ_m. Since all other state and choice are normalized by expected income, this introduces measurement error in those variables as well. While the data come from a high quality survey, measurement error is an important concern, and could affect parameter estimates, since fluctuations in income that do not correspond to changes in consumption will make households appear to be more insured than they are in reality. Furthermore, since my estimates of household expected incomes will be noisy, the variance of household income draws will be greater than in reality.

After introducing measurement error, the model has three state variables: x_{it}, h_{it}, M_i, two continuous choice variables: c_{it}, ι_{it}, plus the binary decision to default: d_{it}, and ten parameters: $\theta = [\beta, \gamma, \alpha,\ R, \delta, \bar{c}, \bar{h},\ \lambda, \sigma, \sigma_m]$. The three state variables summarize the sources of household heterogeneity. The choice variables are used to define moment conditions that allow me to calibrate the parameter vector by finding the set of parameters that best matches observed choices to model predictions.

The WBHRVS data has information on food consumption that I take to be c_{it}, as well as household repair, maintenance, home improvements, and additions, which I take to be: ι_{it}. The survey also asks households what they would have to pay to purchase a home like this today. Since this value should include the value of any additions or repairs undertaken during the survey year, I used the lagged value as an estimate of h_{it}. This does a good job of capturing earthquake damages. Households that report experiencing earthquake damages have an average housing value 46% lower than households that don't report damages.

The y_{it}s are the realizations of household income in each period. M_i is estimated from a vector of fixed household characteristics. Since expected income is important for normalizing the other state variables, as well as for scaling household value for aid, it is crucial that it is unbiased with respect to damages. Therefore I only estimate expected income using household characteristics that are exogenous to earthquake damages and aid (details in Appendix A). The M_is are then multiplied by the vector of simulated measurement error shocks.

Beginning of period liquidity, x_{it}, includes lagged cash savings minus debts owed, capital gains, income, aid received, as well as new loans taken and informal transfers, including remittances. Since the model does not allow for investments in income, I restrict attention to short-term consumption loans (term $<$ three years, taken within the past three years), and calculate estimated annuities based on each loans' term and interest rate. I follow Kaboski and Townsend (2011), I define default, d_{it}, as an outstanding loan balance more than three months after the term of the loan has expired.

Finally, to account for unmodelled variation from the business cycle, and household age structure, size, and education levels, I purge these sources of variation from the model, again following the approach in Kaboski and Townsend (2011). Full details are in Appendix A.

This leaves us with the parameter vector, which is estimated by MSM using the optimal weight matrix. I describe the procedure for doing so in detail in Appendix A. The resultant parameter values are shown in Table 3, and generally fall within the range of normal values in the literature where previous estimates exist. One estimate of note is the coefficient of risk aversion, which is on the higher side of previous estimates and higher than Kaboski and Townsend (2011)'s estimate of 1.16, though perhaps not exceptional given the context of poor households in the aftermath of a major earthquake.

Using the calibrated model, I can compare the aggregate welfare from various targeting strategies by calculating each household's WTP for aid as described in the previous section. As discussed, these counterfactuals require accepting the structure of the model in Section III. To ensure that the model

Variable	Definition	Source
c_{it}	Consumption	Value of last week's food consumption in survey multiplied by 52.
ι_{it}	Housing Investment	Spending on home repairs, improvements, maintenance, furniture, and large appliances over the last 12 months.
d_{it}	Default	1 if a household has unpaid debts more than 3 months past the term of the loan, otherwise 0.
Y_{it}	Realized Income	Income from wages, agricultural sales, food home production, land, housing, and equipment rentals, and pensions and other government programs.
M_i	Expected Income	Estimated from household characteristics, see Appendix A.
x_{it}	Liquidity	Lagged cash savings minus debts owed, capital gains, income, aid received, as well as new loans taken and informal transfers and remittances
h_{it}	Housing Value	Lagged estimate of resale price from survey. Age of housing stock also used to estimate depreciation.
Parameter	**Estimate (SE)**	**Interpretation**
γ	4.444 (0.24)	Coefficient of Risk Aversion
β	0.976 (0.008)	Discount Factor
α	0.736 (0.007)	Cobb-Douglass Share on Consumption
δ	0.983 (0.005)	Depreciation Factor for Housing
R	1.001 (0.000)	Interest Rate (+1)
$\bar{c}$	0.219 (0.030)	Minimum Consumption as Fraction of Expected Income
$\bar{h}$	0.067 (0.058)	Minimum Housing as Fraction of Expected Income
λ	0.600 (0.006)	Borrowing Constraint as Fraction of Expected Income
σ	0.792 (0.020)	Standard Deviation of Log Income
σ_m	0.559 (0.007)	Standard Deviation of Log Measurement Error

Table 3—Model Variables and Calibrated Parameters

is capturing something real in the way households make decisions and value liquidity, I run regressions on household's self-reported coping strategies on estimated WTP as a fraction of household income. The results are in Table 4. Column 1 regresses the maximum interest rate on an outstanding household loan on WTP. Households that are paying higher interest rates have higher WTP, supporting the interpretation of WTP as demand for liquidity. Column 2 shows that households that reported spending down their savings in the past year have higher WTP, and column 3 reports that households that cut down on food consumption in the past year have higher WTP. These all support the interpretation of WTP as demand for liquidity.

To gain further confidence in the model, we can test the model's predictions regarding household responses to aid. For each household, I can compute the model's predicted consumption, investment, and savings decisions. I can also make a counterfactual set of predictions associated with an increase in household liquidity associated with the size of the aid package. In the following section, I use a quasi-experimental feature of the aid distribution strategy to estimate the reduced form causal effects of the aid on household decision variables, and I compare these estimated effects to the model's predictions.

IV. Regression Discontinuity Estimates of the Causal Effect of Aid

Receipt of aid is endogenous to observed and unobserved earthquake damages, which are likely to be correlated with outcomes of interest, including consumption, income, savings, and housing investment. In addition, the earthquake occurred in a distinctive region of the country – disproportion-

Table 4—Willingness to Pay for Aid and Household Coping Strategies

	(1)	(2)	(3)
(Intercept)	13.85***	14.68***	14.89***
	(0.37)	(0.31)	(0.27)
max interest rate	0.08***		
	(0.02)		
spent savings		0.97*	
		(0.58)	
cut food consumption			2.79*
			(1.64)
Num.Obs.	2086	2086	2086
R2	0.009	0.001	0.001

Notes: Regressions of household coping strategies on WTP for aid as a percent of future income (in percentage points). Sample is second wave of WBHRVS, households in earthquake affected districts, which is the sample used for the counterfactual simulations in Section V. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

ately affecting the mountainous districts surrounding Kathmandu. To estimate the causal effects of aid, I rely on an administrative feature of the reconstruction aid program – that aid was prioritized for the fourteen most affected districts (see Figure 1). In principle, households living close to the borders of these fourteen districts, but on opposite sides of the boundary are expected to have similar levels of earthquake damages, as well as similar geographic, demographic, and economic characteristics. However they differed significantly in their probabilities of receiving aid. A report in 2018 found that in the most affected districts (red and orange in Figure 1), 60% of eligible households had received at least 2 tranches of aid, whereas in the moderately affected districts (yellow, green, and blue in Figure 1), only 15% of eligible households had received the same (Housing Recovery and Reconstruction Platform, 2018).

These features suggest the use of a spatial regression discontinuity (RD) approach to compare outcomes between households very close to the borders of the designated districts. I therefore estimate a regression of the form:

$$Z_{it} = \beta_1 \widehat{\text{Aid}}_{it} + \beta_2 d_i + \beta_3 d_i \mathbf{1}_{\{d_i > 0\}} + \beta_4 X_i + e_{it}. \tag{11}$$

Z_{it} is an outcome variable for household i at time t. Aid_{it} is a variable for whether a household has received at least their first tranche of aid by time period t, and it is instrumented with a dummy variable for whether a household is on the 'right' side of the border ($\mathbf{1}_{\{d_i>0\}}$), making this a fuzzy RD specification.

The running variable d is distance to the eastern border of the 14 most affected districts. The specification allows the slope of the running variable to differ on either side of the border. Cattaneo, Idrobo and Titiunik (2020) also suggest using distance to a specific point on the border for two dimensional regression discontinuity applications rather than distance to the entire border, so I also show an alternative specification that uses distance to the point on the border that is closest to a village. Appendix B shows robustness to an alternative border point.

X is a vector of control variables in some specifications to increase precision including self-reported earthquake damages, slope, distance to the epicenter of the earthquake, age and education of the household head, caste, number of household members, and the travel time to the nearest market and health clinic. In most specifications I drop villages from districts that were unaffected by the earthquake, but I also test robustness to this decision in Appendix B.

Since the RD analysis restricts the regression to a subset of 'border' households, one of the crucial parameters for the RD analysis is determining the bandwidth within which a household is included in the regression. A wider bandwidth typically allows for more precision, at the cost of biased estimates if there is curvature in the slope of the running variable near the border point. I use the methods from Calonico et al. (2017) and Calonico, Cattaneo and Farrell (2020) to calculate the optimal bandwidths for both point-estimation and inference, which vary for each dependent variable, typically falling between 25 and 60 km in the main specifications. I test robustness to alternate bandwidths in Appendix B. The blue dots in Figure 1 indicate villages that fall within the bandwidth for inference for the first stage regression – 59 km. All regressions use kernel times survey weights. The kernel weights give more weight to households closer to the border, allowing me to control for distance to the border non-parametrically.[5] I use heteroskedasticity-robust standard errors in all specifications, but I follow Kolesár and Rothe (2018) by not clustering standard errors at the village, which can cause problems when the running variable only takes on a discrete number of values, as is the case in this setting.

Figure 2 shows the identification strategy graphically. The left panel shows the binned averages of the probability of receiving aid as a function of distance to the border point. There is a clear jump in the probability of receiving aid at the border. The right panel shows self-reported earthquake damages plotted in the same way – there is no comparable discontinuity. Table B5 in the appendix shows the results of the first stage regression – households just inside the border of the most affected districts were 24% more likely to receive aid, and received nearly 40,000 NPR more aid on average in the primary specification. Alternative specifications give mostly

[5] I use triangular weights in the baseline specification and test sensitivity to the uniform and epanechnikov kernels in Appendix B.

similar results, including different choices of bandwidth, kernel, distance to alternate border points, inclusion of less affected districts, inclusion of control variables, and a 'donut hole' specification that excludes villages within 5 km of the border.

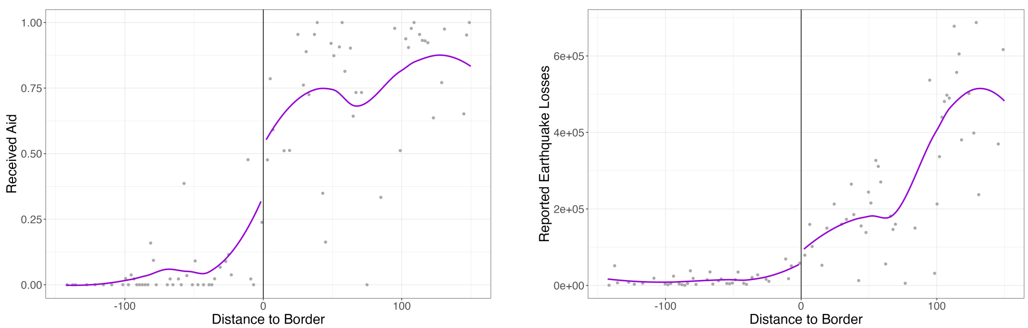

FIGURE 2. BINNED AVERAGES OF FRACTION RECEIVING AID AND EARTHQUAKE DAMAGES AS A FUNCTION OF DISTANCE TO THE BORDER WITH LOESS SMOOTHING.

A. Identification Assumptions

For β_1 to accurately identify the causal effects of aid, it must be the case that the receipt of aid is the only thing that changes discontinuously at the border. Other political, institutional, and geographic factors must be a continuous function of geography, and in particular there cannot be sorting across the border – a potent concern in spatial RD settings as discussed by Keele and Titiunik (2015).

These assumptions hold in this setting for two reasons. First of all, prequake political institutions in Nepal were weak as discussed in Section II. The district borders were mostly set in the 1960s during the Panchayat Regime – a system of governance in which district representatives were elected at local levels to serve on a partyless National Assembly that was largely powerless to do anything other than rubber-stamp the King's agenda. For these reasons, there shouldn't be large differences in policy across district boundaries. Furthermore, district borders do not demarcate boundaries between ethnic groups, giving us no reason to believe that informal institutions change discretely at the border either (Gurung, Gurung and Chidi, 2006).

Regarding sorting, land and housing markets in rural Nepal are not very liquid. While rural to urban and international migration are important livelihood strategies, I do not observe much rural-to-rural migration in the data. If entire households migrated away from regions that did not receive aid, this could also be problematic. It is much more common for one or two individuals within a household to migrate and send back remittances, however.[6]

As suggested by Imbens and Lemieux (2008) and Lee and Lemieux (2010), we can gain confidence in the underlying RD assumptions by conducting placebo tests on household demographic, political, and geographic variables. To do so I run a version of equation (11), replacing Z with a placebo variable that should not change with the receipt of aid.

As seen in Appendix Figures B4, most household demographic variables appear smooth at the border, although some are noisy. Table B6 formally tests for discontinuities, and finds no difference in age, the number of household members, the probability that a household member has completed 5 or 10 years of schooling, or the fraction of households that have always lived in the same house or same district on either side of the border, supporting

[6]A McCrary test for discontinuities in the density of households on either side of the border is not informative in this setting because the sample frame for the survey is based on the 2010 census, but sampling occurred after the earthquake in 2015 (McCrary, 2008). I plot the density of households as a function of distance to the border anyway in Appendix B, and do not observe any obvious discontinuities.

the idea that household demographics are comparable. I also find no differences in the receipt of NGO aid or non-earthquake government transfers, supporting the hypothesis of no policy changes at the border.

I find a borderline significant difference (significant at the 10% level) in the fraction of high-caste households and self-reported earthquake damages, and a significant difference at the 5% level on the difference in the highest level of education attained by a household member.

I also test for travel times to the nearest bank, school, market, and health-care clinic in Table B7 as a proxy for policy and public good provision. I find a negative and significant effect on travel time to the nearest bank, and no differences for the others. Finally, I test for differences in prices of common food items to see if aid had macroeconomic spillover effects not captured by my model, and I find no significant effects.

The few observed differences could also be simply due to random noise. Examining the plots in Figure B4 does not show large discontinuities at the border for household education, caste, earthquake damages, or travel time to the nearest bank, and furthermore, these estimated differences are not significantly different from zero at other choices of bandwidth – both narrower and wider bandwidths (see Table B6 and B7). In fact at some bandwidths, the sign flips for the fraction of high caste households and the travel time to the nearest bank. To the extent that there are real differences, I include these variables as controls in some specifications and find that it does not substantially change the main results.

B. Effects of Aid

Turning attention to outcomes of interest, the main specification in the first row of Table 5 shows large effects of aid on both food consumption and home investment, though the effect on housing investment is imprecisely estimated. There is also a large negative effect on remittances, significant at the 10% level, and more modest positive effects on cash savings, new loans, and investments that are not significantly different from zero. The effect on income is positive and significant, and the effect on migration is negative and non-significant in the main specification. Using distance to the border point gives qualitatively similar results, though the effects on food consumption and remittances are smaller, and the effect of housing investment is more precisely estimated.

Figure 3 shows the discontinuities for these variables in the data, which are clearly apparent for housing investment and remittances in particular. Similar plots for additional variables, including the placebos, appear in Appendix B. The effects on income are less clear in the graphs, and potentially driven by high variability near the border. In Appendix B, I show robustness to alternative choices of kernel, bandwidth, control variables, border points, and logged dependent variables. The estimates are noisy, and both magnitudes and precision vary. Most specifications show similar qualitative results, however - a large drop in remittances, a large but noisy increase in housing investment, and a smaller but still substantial effect on food consumption. Effects on income are usually positive but noisy. Effects on other variables are smaller and not significantly different from zero.

In the third row of Table 5, I compare the regression discontinuity estimates to the model predictions by calculating counterfactual consumption,

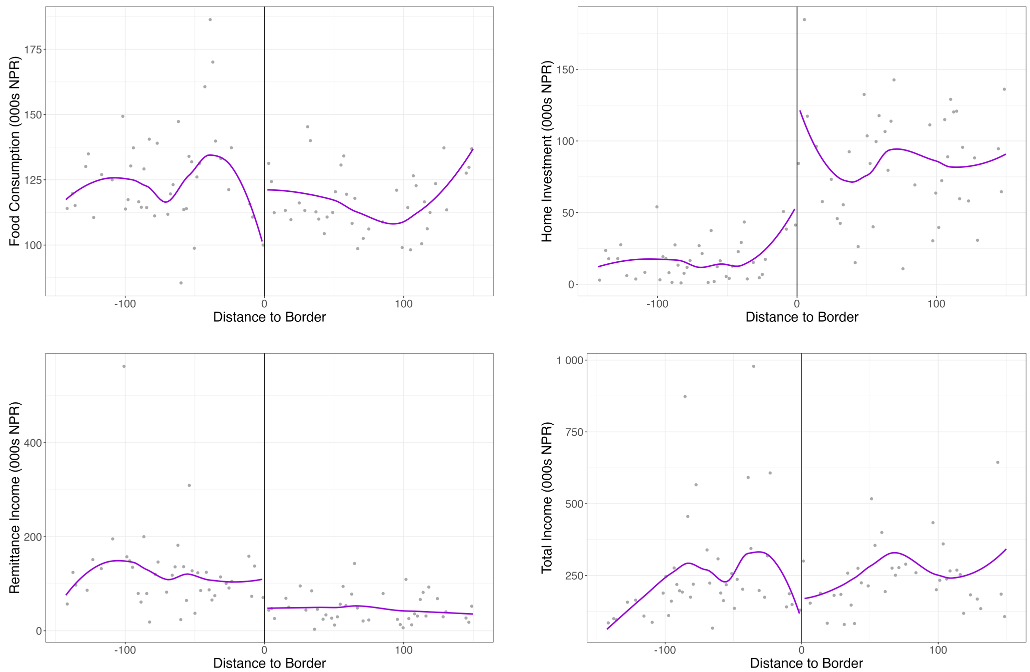

Figure 3. Binned averages of outcome variables as a function of distance to the border with Loess smoothing. Positive values indicate villages inside the districts that were prioritized for aid.

investment, and borrowing responses to aid using the optimal policy and investment functions from Section III.C. I restrict attention to the households that actually received aid, since the model predictions will differ for different subsets of households. I subtract any aid received from each household's liquidity and recalculate predicted consumption, investment, and borrowing. Then I add back 300,000 NPR, and calculate consumption, investment, and borrowing again.

The predicted effects are comparable to the results from the regression discontinuity analysis. The model predicts smaller effects on consumption and housing investment than I observe, though both are within the 95%

confidence intervals. The larger observed effect on housing investment could be a result of the conditionality of the aid program, which in theory required households to spend the aid on rebuilding their housing. The model also predicts large standard deviations in the effects of aid on consumption and investment, which could be one reason why the effects from the regression discontinuity are imprecisely estimated. The model's predicted effect on borrowing is about halfway between the estimated effects on remittances in the two different specifications. I take these simulations as evidence that the estimated policy functions are capturing something real in the way households make decisions, and that can speak to the value households place on aid.

Table 5—Main Outcomes

	Regression Discontinuity Estimates: Distance to Border as Running Variable							
	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	97,976**	335,780	−231,146*	54,686	98,978	79,467	647,554**	−0.65
	(43,376)	(211,132)	(122,572)	(120,387)	(105,825)	(97,689)	(314,229)	(0.43)
N	784	999	784	940	1091	608	694	828
Bandwidth	29.64	33.2	28.63	31.8	34.77	24.77	25.4	29.82
Wards	63	37	35	59	57	44	61	39
	Regression Discontinuity Estimates: Distance to Single Border Point as Running Variable							
	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	61,353*	307,328**	−94,973	13,687	83,105	−12,940	359,918*	−0.33
	(31,908)	(150,588)	(81,690)	(69,881)	(68,825)	(51,760)	(184,813)	(0.26)
N	479	1261	1224	1636	873	1047	871	1880
Bandwidth	31.29	46.11	43.72	52.25	39.09	41.48	38.05	57.2
Wards	39	63	57	76	56	56	56	76
	Model Predictions: Response to 300k Aid							
	food consumption	home investment	borrowing				total income	
	(1)	(2)	(3)				(7)	
Aid	39,540	79,840	-180,621				0.0	
	(32,539)	(75,309)	(76,691)				0.0	

Notes: Top panel shows regression discontinuity estimates using local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors, and optimal bandwidths. Slope of running variable allowed to differ on either side of cutoff. Middle panel is the same, except using distance to the point on the border that is closest to a village as the running variable. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively. Bottom panel shows model predicted effects on choice variables for households that actually received aid. Population estimated standard deviations in parentheses.

V. Counterfactual Reallocations

I use the calibrated model to explore counterfactual allocations of aid by solving for each household's estimated WTP for aid. I use the measure of WTP to assess the aggregate surplus created by different allocations, and compare these to a baseline "random allocation" scenario. The random allocation simulates choosing the set of recipient households at random, so that the benefits are equal to the population average WTP. I also analyze the surplus created by the actual allocation of aid.

As discussed above, this measure of household WTP is increasing in income. Thus, an allocation could result in low welfare gains if it prioritizes of low-income households. Therefore, I also compare targeting strategies using a utilitarian social welfare function. This criterion prioritizes poor households with a high marginal utility for liquidity. Furthermore, I show the aggregate effects of each allocation on food consumption and housing reconstruction. Full results for all welfare measures and targeting schemes are reported in Table 6.

I analyze all counterfactuals based on WTP in the second survey wave, which is when a significant number of households received the first tranche of reconstruction aid. This will somewhat overstate the value of the actual allocation of aid, since some households didn't get the aid until later, and those that did received aid in several tranches over multiple years. My approach analyzes the value of receiving all the aid at once. I subtract any aid received from household liquidity to get estimates of pre-aid WTP. Since I use lagged measures of the housing stock, that state variable reflects the state of the housing stock in the first survey wave, right after the earthquake, and is thus uncontaminated by the effects of aid. I also restrict my analysis to the earthquake affected districts (including moderately and slightly affected districts) to set a reasonable restriction on the eligible population. 58% of households in these districts reported experiencing earthquake damages, and an estimated 38% of households received aid.

In all counterfactuals, except for the universal allocation, I hold the fraction of households receiving aid constant. In the universal allocation, I hold the budget constant, by assuming that the amount of aid to each household is .38 x 300,000 = 114,000 NPR. I use survey weights in the analysis that follows to make the estimates representative of the population in these districts. 95% confidence intervals are bootstrapped using 1,000 replicates, and reflect sampling error, but not specification error from the model or parameter estimates.

I find that the average household would give up 15.1% of future income in order to receive 300,000 NPR in reconstruction aid, reflecting a WTP of 1.6 million NPR in present value – more than 5 times the nominal value of the aid[7]. There is substantial heterogeneity, however, with a standard deviation of nearly 1.4 million NPR. If I restrict aid such that household must invest all of it in their housing stock, the WTP for this type of conditional aid is on average 34% lower than for unconditional aid. If a planner was able to perfectly identify the households with the highest WTP for aid, this optimal allocation of aid would improve welfare by 77% relative to a random allocation.

I find that the WTP of households that actually received aid was 5% better than the random allocation, although this improvement is not statistically different from zero at a 95% confidence threshold. This, if anything,

[7]Present value calculation uses the estimated discount factor of 0.976.

Table 6—Counterfactual Targeting Scenarios

	Targeting Scenario						
	Actual	Damages	Consumption	Housing	Liquidity	Universal	Optimal
Welfare Measure	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WTP	1.05	1.04	1.06	1.21	1.36	1.12	1.77
	(1.00, 1.09)	(0.99, 1.09)	(1.00, 1.09)	(1.15,1.24)	(1.30, 1.40)	(1.09, 1.16)	(1.69, 1.80)
Utilitarian	0.18	0.20	1.32	0.49	2.57	1.12	2.64
	(-0.04, 0.37)	(-0.05, 0.31)	(0.31, 2.31)	(-0.02, 1.01)	(2.43, 2.73)	(0.20, 2.16)	(2.55, 2.83)
Consumption Increase	0.97	0.97	1.04	0.90	1.39	1.05	1.78
	(0.93, 1.02)	(0.93, 1.02)	(0.99, 1.08)	(0.85, 0.93)	(1.34, 1.43)	(1.02, 1.08)	(1.73, 1.83)
Housing Increase	1.25	1.23	0.96	1.84	0.61	0.92	2.28
	(1.15, 1.28)	(1.13, 1.26)	(0.90, 1.03)	(1.71, 1.88)	(0.54, 0.67)	(0.90, 0.94)	(2.10, 2.33)

Notes: Benefits relative to Population Average for 300,000 NPR aid allocated to 38% of the population according to various targeting strategies – except universal scenario which uses 0.38*300,000 NPR allocated to the entire population. Bootstrapped 95% confidence intervals in parentheses using survey weights. Utilitarian refers to an 'equal-weighted' utilitarian social welfare function. Consumption, housing, and liquidity allocations based on households with the lowest values of those variables. Damages based on households with the highest self-reported earthquake damages.

overstates the benefits of the actual allocation, since it is based on WTP in the second wave, and thus doesn't take into account the delays in receiving the aid. As a best case scenario for damage based targeting, I take survey self-reported damages as truth and analyze an allocation of aid if policy-makers were able to perfectly target the households with the worst physical damages. I find that the welfare from this approach is similar to the true allocation – a non significant 4% improvement over the random allocation. Neither the actual nor the damage based allocation does well by the pro-poor utilitarian social welfare function either. However, if the goal of the planner is simply to increase housing investment, then these allocations do better than random. Thus a planner would need to believe there are large externalities or other unmeasured benefits from housing reconstruction to justify a damage-based allocation.

Columns 3-5 show the benefits of targeting the lowest consumption, housing stock, and liquid wealth households respectively. Households may have poor housing stock due to earthquake damages or for reasons that pre-date the disaster. Targeting households with the lowest housing stocks and lowest liquidity performs significantly better than the random allocation – 21% and 36% gains in welfare respectively. Of courses these estimates assume that a planner could perfectly identify these households. Using imperfect proxies for liquidity or housing would reduce the gains. Targeting low consumption and low liquidity households does the best by the utilitarian social welfare function, as expected.

Finally, I analyze a universal aid allocation – dividing the aid budget amongst all the households in the sample. This could be an alternative to targeting, if good proxies for need are difficult to collect. This approach does better than the random allocation or the damage based allocation by either the WTP or the utilitarian welfare measure, and this is without considering the savings associated with targeting and administration of the allocation. These results reflect the concavity of household value functions over liquidity, which results in diminishing WTP and diminishing marginal utility for aid. Giving all households a little bit of aid moves them up the steep section of their value functions, and this effect can outweigh the gains to targeting larger amounts of aid, depending on the allocation. Intuitively, both the universal and the random allocation 'waste' some aid by giving it to households that don't need it. But the universal allocation makes sure that all households can cover some basic needs, while a random allocation may miss some very needy households, while giving larger amounts to less needy households.

These results suggest that the extensive rounds of targeting and building verification did not add much value by most measures. Relief agencies would have generated more welfare if they had instead spent the resources used during the targeting and verification process on increasing the aid budget, and then allocating aid randomly, or dividing the budget into smaller amounts and distributing it universally.

VI. Conclusion

Natural disasters may present households with shocks that existing institutional arrangements are incapable of smoothing. This is especially likely when the disaster is outside of recent lived experience, and when the disaster occurs in a low-income country, where credit constraints are more likely to bind and informal risk sharing networks can be overwhelmed by a large covariate shock. In these circumstances, aid can facilitate reconstruction and consumption smoothing by providing liquidity to ease these constraints.

Targeting aid could be important, if households differ in their value for aid, and good proxies for these values are available to those in charge of distribution. This paper concludes that, while there is substantial variation in household values for aid, disaster-related property damages are a poor proxy for those values. In the context of the 2015 Nepal earthquake, a universal approach to aid distribution would have increased welfare by more, regardless of preferences for redistribution. This strategy has the additional benefit of not requiring any budget or time for needs assessment.

These conclusions arise from a calibrated structural model of household consumption, investment, and saving linked to a framework for estimating household demand for aid. My measure of demand, defined as the amount of future income a household would give up in order to access additional liquidity in the present, is consistent with the goals of a social planner that aims to use aid to help households smooth the shock. All else equal, earthquake damages increase demand for aid. Also important, however, is whether a household can draw on forms of informal insurance, including remittances and loans.

These findings shed light on the mechanisms by which disasters and disaster aid can increase inequality, especially when targeted based on measures of disaster damages. The wealthy are twice better off in a disaster – they are slightly less likely to suffer damages due to their better housing quality, but they are also more likely to be able to smooth the shock, due to their ability to draw on loans and remittances.

Several factors might affect the external validity of these conclusions. An earthquake is a particular type of disaster, in that the main effect is usually to destroy structures, which may be a less important input into production in a rural agricultural setting. Endogenizing the income process would be more important to analyze flooding or droughts, or targeting aid in an urban setting, and should be considered in future research. Second, the aid program analyzed here was delivered more than a year after the disaster. Although this time frame may be typical for reconstruction aid, the implications for targeting emergency relief in the immediate aftermath of a disaster could differ, especially when aid is in the form of in-kind goods rather than cash. Finally, analyzing the optimal amount of targeting is likely to depend upon the distribution of wealth and disaster damages in a given context, and the existence of other social insurance programs. The framework presented in this paper could be adapted to analyze how changes in both the average wealth and the distribution of wealth affect the optimal amount of targeting, and this may be a promising avenue for future research.

There are other potential downsides to allocating aid based on damages that are beyond the scope of this paper, but could be examined in future work. Aid that is conditional on property damage might create perverse incentives if property owners fail to internalize the full risks of building in disaster-prone areas, or under-invest in hazard mitigation more generally. This is probably unlikely for a once-a-century earthquake, but more plausible for recurrent disasters (Kousky, Michel-Kerjan and Raschky, 2018; Wagner, 2022).

Furthermore, as mentioned in Section II, disputes over beneficiary lists led to protests and significant delays in some areas. Given the importance of speed in aid delivery, more research should address the political economy concerns of targeting, and what types of allocations are easiest to administer. One possible explanation for the popularity of damage-based aid is that the eligibility criteria should be relatively transparent and easy to administer. This did not seem to be the case in Nepal, however (The Asia Foundation, 2016*b*). Given my results, it is crucial to study whether alternative allocation mechanisms, including community-based or universal approaches, would have been perceived as more fair by those that lived through the disaster.

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ONLINE APPENDIX:

Appendix A. Structural Model and Calibration

A1. Value Function Normalization

Define $X_{it}, H_{it}, C_{it}, I_{it}, Y_{it}$ as the true values of wealth, housing, consumption, housing investment, and income respectively, with $x_{it}, h_{it}, c_{it}, \iota_{it}, y_{it}$ as the same variables normalized by $E(Y_{it}) = M_i = exp(\mu_i + \sigma^2/2)$. From equation (1) we have:

$$\text{(A1)} \qquad U_i = E[\sum_{t=0}^{\infty} \beta^t \frac{\left(C_{it}^{\alpha}(H_{it} + I_{it})^{(1-\alpha)}\right)^{(1-\gamma)}}{1-\gamma}]$$

$$= E[\sum_{t=0}^{\infty} \beta^t \frac{\left((Mc_{it})^{\alpha}(M(h_{it} + \iota_{it}))^{(1-\alpha)}\right)^{(1-\gamma)}}{1-\gamma}]$$

$$= M^{1-\gamma} E[\sum_{t=0}^{\infty} \beta^t \frac{\left(c_{it}^{\alpha}(h_{it} + \iota_{it})^{(1-\alpha)}\right)^{(1-\gamma)}}{1-\gamma}].$$

Thus if ν is the value function associated with maximizing $E[\sum_{t=0}^{\infty} \beta^t u(c_{it}, h_{it}, \iota_{it})]$, $V_i = M_i^{1-\gamma}\nu$ is the value function associated with maximizing equation (1). The normalization carries through to all the state variables without issue, observing that $log(y_{it}) = log(Y_{it}/M_i) = log(Y_{it}) - log(M_i) = log(Y_{it}) - \mu_i - \sigma^2/2 \sim N(-\sigma^2/2, \sigma^2)$.

Therefore, I solve for:

(A2)

$$M_i^{\gamma-1}V(x_{it},h_{it}|M_i) = \nu(x_{it},h_{it}) =$$
$$\max\Big\{\max_{\iota,c} u(c_{it},h_{it},\iota_{it}) + \beta E[\nu(R(x_{it}-c_{it}-\iota_{it})+y,\delta(h_{it}+\iota_{it}))],$$
$$\nu_d(h_{it})\Big\}$$

subject to the constraints:

$$x_{it}-c_{it}-\iota_{it} \geq \lambda \tag{A3}$$

$$\iota_{it} \geq 0. \tag{A4}$$

and the corresponding optimal consumption and investment functions.

A2. Finding the Policy Functions

It is possible to solve for the value function and optimal policy functions, $c^*(x,h,M)$, $\iota^*(x,h,M)$, and $d^*(x,h,M)$, given a set of parameters using Value Function Iteration (VFI). I start with the guess that the value function is equal to zero everywhere. I store the value function in a grid of 1600 points, with 40 equally-spaced points in both the x and h dimensions. I interpolate the value function between grid points using the multidimensional simplical scheme described in Judd (1998). I discretize the income process by exponentiating 30 Gaussian quadrature points. At each grid point I find the values of c and ι that maximize A1. Since the default decision introduces a kink in the value function, which can lead to local minima in the objective function resulting from the approximation and interpolation scheme, I use a semi-global optimization algorithm at each grid point(Gablonsky and

Kelley, 2001; Johnson, 2022).

The value of the objective function at the maximum is then stored as ν_2. Thus, for each subsequent iteration n+1 I solve for:

$$\nu_{n+1} = \max u(c_t, h_t, \iota_t) + \beta E[\nu_n(x_{t+1}, h_{t+1})]. \tag{A5}$$

This is repeated until the relative mean squared difference in $c^*(x,h)$ between iterations is less than .0025.

A3. Parameter Calibration

I calibrate the parameter vector by minimizing the MSM objective function using a set of 15 moment conditions derived from the model described below. To do so I use a nested fixed-point algorithm: the outer loop iterates over parameters, while the inner loop estimates the optimal policy functions at each set of parameters.

Since approximation errors in the inner loop can generate local minima in the objective function, I first search for an approximate global minima of equation (A6) using the Direct-L semi-global optimization algorithm (Powell, 1998). I use that minima to estimate the optimal weighting matrix, W (Hansen, 1982). I also use it as the starting point of a local optimization algorithm in a second iteration to estimate the parameter vector ($\hat{\theta}$) to satisfy:

$$\begin{aligned}\hat{\theta} = \arg\min_{\theta} \Big(\sum_{i=1}^{N}\sum_{t=2}^{3}\sqrt{w_i}g_{it}(\theta, x_{it}, h_{it}, M_i, \iota_{it}, c_{it}, d_{it}, y_{it})\Big)^T \widehat{W} \\ \Big(\sum_{i=1}^{N}\sum_{t=2}^{3}\sqrt{w_i}g_{it}(\theta, x_{it}, h_{it}, M_i, \iota_{it}, c_{it}, d_{it}, y_{it})\Big).\end{aligned} \tag{A6}$$

Where $g_{it} = \{e_{it1}, ..., e_{it15}\}^T$ and w_i are the survey weights, and $\widehat{W}$ is the inverse of the estimated covariance matrix from the first stage optimization. The first survey wave is dropped because lagged cash savings and lagged housing value are needed to construct the state variables.

Moment Conditions

The optimal policy functions for consumption, investment, and default define the first three moment conditions:

$$\begin{aligned} e_{it1} &= c^*(x_{it}, h_{it}, M_i) - c_{it} \\ e_{it2} &= \iota^*(x_{it}, h_{it}, M_i) - \iota_{it} \\ e_{it3} &= d^*(x_{it}, h_{it}, M_i) - d_{it}. \end{aligned} \tag{A7}$$

Following Kaboski and Townsend (2011), I gain six additional moment conditions by interacting e_1 and e_2 with transformations of each of the state variables. Intuitively, this helps ensure that the model's predictions are not biased in expectation for any values of the state variables:

$$\begin{aligned} e_{it4} &= e_{it1} \log(M_i) \\ e_{it5} &= e_{it1} \text{ ihs}(x_{it}) \\ e_{it6} &= e_{it1} \log(h_{it} + 1) \\ e_{it7} &= e_{it2} \log(M_i) \\ e_{it8} &= e_{it2} \text{ ihs}(x_{it}) \\ e_{it9} &= e_{it2} \log(h_{it} + 1). \end{aligned}$$

The inverse hyperbolic sine function is used for x_{it} to handle negative values.

Moment conditions 10-12 help to pin down interest rates by using data on household capital income and loan payments made and received.

$$e_{it10} = (R-1)\mathit{Cash\ Savings}_{it} - \mathit{Capital\ Gains}_{it}$$
$$e_{it11} = \sum_{T=1}^{3} \frac{R^T(R-1)}{R^T-1}\mathit{Loans\ Taken}_{it} - \mathit{Loan\ Payments\ Made}_{it}$$
$$e_{it12} = \sum_{T=1}^{3} \frac{R^T(R-1)}{R^T-1}\mathit{Loans\ Made}_{it} - \mathit{Loan\ Payments\ Received}_{it}$$

T is the term of the loan. I use any outstanding consumption (non-investment) loans in period t with a 3 year term or less.

The next moment condition helps to pin down depreciation rates based on a hedonic approach using the age of the home. Consider the regression with household and year fixed effects:

$$\text{(A8)}\qquad \log(\mathit{Housing\ Value}_{it}) = a_1 \mathit{Age\ of\ Home}_{it} + a_2 \mathit{Housing\ Investment}_{it} + \eta_i + \phi_t + e_{it}.$$

The coefficient $a_1 = \delta - 1$ in the model. We can use the Frisch-Waugh-Lovell theorem to derive an appropriate moment condition based on this regression:

$$e_{it13} = \widetilde{\mathit{Age\ of\ Home}}_{it}\Big(\widetilde{\mathit{Housing\ Value}}_{it} - (\delta-1)\widetilde{\mathit{Age\ of\ Home}}_{it}\Big).$$

where $\widetilde{\mathit{Housing\ Value}}_{it}$ and $\widetilde{\mathit{Age\ of\ Home}}_{it}$ are the residuals of a regression of those variables on housing investment and the household and survey year fixed effects. Table B4 shows estimates of equation A8, as well as robustness of the estimates to other specifications.

The final two moment conditions helps identify the variance of the income process and measurement error. If s_{it} is a measurement error shock drawn i.i.d from $log(s_{it}) \sim N(-\frac{\sigma_m^2}{2}, \sigma_m)$, then $\log(s_i y_i) \sim N(-\frac{\sigma^2}{2} - \frac{\sigma_m^2}{2}, \sigma^2 + \sigma_m^2)$, and $E[\log(s_i y_i)^2] - (\frac{\sigma^2}{2} + \frac{\sigma_m^2}{2})^2 = \sigma^2 + \sigma_m^2$. Therefore I use the following moment conditions:

$$e_{it14} = \log(s_{it}y_{it}) + \frac{\sigma^2}{2} + \frac{\sigma_m^2}{2}$$
$$e_{it15} = \log(s_{it}y_{it})^2 - \left(\frac{\sigma^2}{2} + \frac{\sigma_m^2}{2}\right)^2 - \sigma^2 - \sigma_m^2.$$

All moment conditions are normalized by dividing by the average value of the relevant variable in the data (e.g. average consumption, investment, default, etc...). I estimate these moments using 11,115 household-year observations from the second and third waves – I also drop approximately 5% of households that did not answer large portions of the survey, making the construction of key variables impossible. For households that answered 'Don't Know' for certain rare types of income, especially capital gains, I impute a zero for that category of income.

Purging Lifecycle Variation and Household Heterogeneity

Since the data contain variation not explicitly modelled, including lifecycle considerations and other unobserved determinants of household heterogeneity, I follow the buffer stock literature in purging these sources of variation from the calibration procedure (Kaboski and Townsend, 2011).

This procedure is necessary to ensure that household values for aid are not biased by life cycle considerations, household size, or other systematic differences between households. Consumption and savings patterns may differ between older and younger families, for example, even if they had the same values for aid. Purging variation associated with these differences requires careful consideration, however, since our counterfactuals address targeting, the value of which depends on household heterogeneity.

To be precise, our model says that households are only heterogeneous in their history of shocks to income and housing stock – including the earthquake – and their expected income, and I seek to understand how this heterogeneity correlates with different targeting programs.

If a targeting program correlates with any of the exogenous characteristics purged from the data, then removing that source of variation will remove any value created (or destroyed) by systematically targeting those households. Thus purging regional variation, for example, is undesirable, since earthquake damages vary across space, and a targeting program might want to take that into account.

On the other hand, the age structure of the household, household size (including migrant members), and education can be plausibly seen as exogenous to the earthquake damages, as well as the targeting strategies under consideration, yet may account for systematic differences in behavior. Survey fixed effects are included to capture macroeconomic fluctuations - including price changes and exchange rates.

Purging these sources of variation results in a nuanced interpretation of household values for aid. My estimates reflect household value for aid conditional on the set of exogenous household characteristics. This is consistent with the idea that aid is to be used for smoothing welfare through the disaster, and not redistributing between different types of households.

Thus I run the following regressions:

$$\begin{aligned} log(C_{it}+1) &= \Gamma_1 W_{it} + \epsilon^1_{it} \\ log(H_{it}+1) &= \Gamma_2 W_{it} + \epsilon^2_{it} \\ d_{it} &= \Gamma_3 W_{it} + \epsilon^3_{it} \\ X_{it} &= \Gamma_4 W_{it} + \epsilon^4_{it} \end{aligned} \tag{A9}$$

Where W_{it} is a vector of household characteristics containing quadratic polynomials of age of the head of household, education, and the number of members (including migrants), as well as the number of children and elderly members, and survey wave fixed effects. I add one to consumption and housing to maintain households with a zero value for those variables, since these households could be particularly valuable from a targeting perspective. The R squared values of these regressions are .30, .07, .09, and .01 respectively.

I then construct an adjusted dataset, where the values of consumption, investment, housing wealth, savings, and credit are the fitted values of A9 for a household with mean values of the independent variables, plus the household specific residual. For example, adjusted household consumption is constructed as:

$$\tilde{C}_{it} = \exp(\widehat{\Gamma}_1 \bar{W}_{it} + \widehat{\epsilon}^1_{it}). \tag{A10}$$

Income is treated similarly, but I also allow for additional household heterogeneity in order to more accurately estimate expected income. I include a vector of additional characteristics, U_{it}, which includes the value of household landholdings, the gender of the head of household, the number and destination of previous migrants, migrant earnings, ethnicity and village fixed effects. The R squared of the income regression with these additional characteristics is .24. I then construct expected income and income shocks as follows:

$$log(Y_{it} + 1) = \Gamma_5 W_{it} + \omega U_{it} + \epsilon^5_{it} \tag{A11}$$

$$\tilde{Y}_{it} = \exp(\widehat{\Gamma}_5 \bar{W}_{it} + \widehat{\omega} U_{it} + \widehat{\epsilon}^5_{it}) \tag{A12}$$

$$M_{it} = \exp(\widehat{\Gamma}_5 \bar{W}_{it} + \widehat{\omega} U_{it}) \widehat{\xi}_t \tag{A13}$$

Where $\widehat{\xi}_t = \sum_i w_i \exp(\widehat{\epsilon}^5_{it})$ is the weighted smear factor used to retransform the expected income values back to the scale of the original variable (Duan, 1983). The adjusted values of consumption, housing, default, liquidity, and income are all also multiplied by a common factor to ensure that their means in the adjusted data are the same as in the original data. Lastly, housing investment is adjusted by multiplying housing investment in the data by the ratio of adjusted income to unadjusted income. The adjusted data is then used to both calibrate the parameters and conduct counterfactuals.

All code is written in R and is available from:
https://github.com/mdgordo/nepal_earthquake/.

Appendix B. Additional Figures and Tables

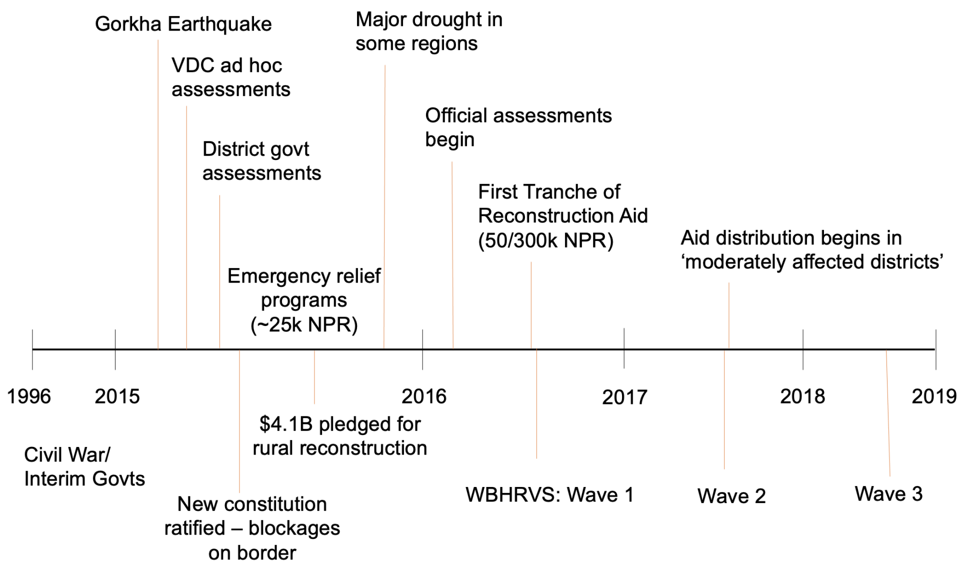

Figure B1. Timeline of Events

Table B1—Summary Statistics

Statistic	N	Mean	Std.Dev.	Min	Max
Consumption	17,804	187,612	211,443	10,838	10,788,308
Food Consumption	17,804	114,106	60,856	1,560	938,704
Income	17,804	275,255	1,830,606	196	220,557,220
Productive Assets	17,801	1,949,996	10,004,033	0	809,395,500
Investments	17,802	39,070	284,081	0	15,000,000
Jewelery	17,763	73,082	174,515	0	12,500,000
School Costs	17,795	14,940	43,153	0	4,477,000
Cash Savings	17,723	50,521	161,026	0	5,015,000
Home Value	17,803	1,228,868	2,232,471	0	70,000,000
Home Investment	17,787	23,078	154,726	0	10,500,000
Age of House	17,804	15.30	13.62	0	122
Household Members	17,500	4.66	1.98	1	21
Connected Migrants	17,804	0.77	1.19	0	18
Migrants Past Year	17,804	0.23	0.63	0	11
Overseas Migrants	17,804	0.40	0.71	0	8
Female Headed(%)	17,500	0.21	0.41	0	1
Remittance Income	17,804	62,988	164,272	0	5,000,000
Loans Taken Past Year	17,789	28,725	123,356	0	7,000,000
Loans Made Past Year	17,804	2,196	26,262	0	1,500,000
Default(%)	17,778	0.19	0.40	0	1
Skipped Meal(%)	17,804	0.10	0.31	0	1
Earthquake Aid	17,804	18,445	74,799	0	2,182,000
Earthquake Aid(%)	17,804	0.13	0.37	0	1
NGO Aid	17,804	830	15,395	0	700,000
Public Transfers	17,758	4,483	10,876	0	320,500
Informal Transfers	17,804	2,571	22,791	0	2,807,540
Earthquake Losses (%)	17,804	0.21	0.43	0	1
Earthquake Losses	17,804	47,428	200,354	0	4,000,000

Notes: Consumption includes value of all food consumption, durables, energy, utilities, rent, transportation and miscellaneous purchases. Income is the sum of wages, rental income, agriculture and livestock sales, home food production, business revenues, pension and other public welfare. It does not include remittances or transfers. Productive Assets are the sum of the value of land, agricultural equipment, and livestock. Investments include land purchased, business investments, livestock purchases or farm equipment purchases. Cash savings includes cash on hand and bank savings plus insurance and savings group assets. Loans reflect short term credit with a duration of three years or less. Survey weights are used to calculate means and standard deviations. N refers to non-missing household-year observations in the three year panel.

Table B2—Consumption Smoothing: Caste Year Fixed Effects

	log(food consumption)				
	(1)	(2)	(3)	(4)	(5)
log(income)	0.120***	0.130***	0.119***	0.132***	0.118***
	(0.013)	(0.018)	(0.014)	(0.012)	(0.013)
log(income):Dalit		-0.018			
		(0.024)			
log(income):Newar		-0.063			
		(0.037)			
log(income):Other		-0.011			
		(0.018)			
log(income):Female Head			-0.013***		
			(0.002)		
log(income):Land > Median				-0.024***	
				(0.004)	
log(income):Quake Affected					0.010
					(0.022)
Num.Obs.	16763	16762	16763	16763	16763
R2	0.693	0.693	0.695	0.693	0.693
SE Cluster	Strata	Strata	Strata	Strata	Strata
Household FE	X	X	X	X	X
Caste-Year FE	X	X	X	X	X

Notes: All regressions include household and caste-year fixed effects. Standard errors clustered at the survey strata. Observations with zero or missing income, or less than 3 observations dropped. Land percentile defined based on household land value during first survey wave. Omitted category for caste is "Brahmin/Chhetri". + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B3—Consumption Smoothing: Rainfall Instruments

	log(food consumption)				
	(1)	(2)	(3)	(4)	(5)
fit log(income)	0.150***	0.099***	0.164***	0.164***	0.163***
	(0.019)	(0.024)	(0.017)	(0.025)	(0.021)
fit log(income):Dalit		0.094***			
		(0.025)			
fit log(income):Newar		0.017			
		(0.068)			
fit log(income):Other		0.077***			
		(0.022)			
fit log(income):Female Head			-0.010***		
			(0.002)		
fit log(income):Land > Median				-0.024	
				(0.036)	
fit log(income):Quake Affected					-0.228***
					(0.042)
Num.Obs.	16763	16762	16763	16763	16763
R2	0.672	0.669	0.671	0.672	0.656
SE Cluster	HH	HH	HH	HH	HH
Household FE	X	X	X	X	X

Notes: All regressions include household fixed effects, and instrument for income using total rainfall in each of the preceding 12 months. Rainfall data comes from Funk et al. (2015). Standard errors clustered at the household. Observations with zero or missing income, or less than 3 observations dropped. Land percentile defined based on household land value during first survey wave. Omitted category for caste is "Brahmin/Chhetri". + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B4—Housing Depreciation: Hedonic Regressions

	log(house value) (1)	log (house value + 1) (2)	house value (3)	log(house value) (4)	log (house value + 1) (5)	house value (6)	log(house value) (7)	log (house value + 1) (8)	house value (9)
Age of House	-0.021**	-0.022**	-0.018***	-0.017**	-0.017*	-0.018***	-0.015*	-0.016*	-0.015**
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Home Investment				0.000	0.000	0.000	0.000	0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Num.Obs.	16612	16763	16763	16596	16747	16747	16596	16747	16747
R2	0.700	0.532		0.701	0.532		0.716	0.544	
Estimator	OLS	OLS	Poisson	OLS	OLS	Poisson	OLS	OLS	Poisson
Clustered SE	strata	strata	strata	strata	strata	strata	strata	strata	strata
Household FEs	X	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X			
Linear Time Trend							X	X	X
District*Year FEs							X	X	X

Notes: Coefficients of OLS and Poisson regression analogs of the housing depreciation moment condition described in Appendix A. Columns differ in the fixed effects included and whether the dependent variable is log house value (with zeros dropped) or log house value + 1 in the OLS specifications, or house value for the poisson specifications. All regressions have standard errors clustered at the survey strata. Households with less than 3 observations dropped. Column 5 is the basis of the moment condition estimated using MSM. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B1. Regression Discontinuity Plots and Tables

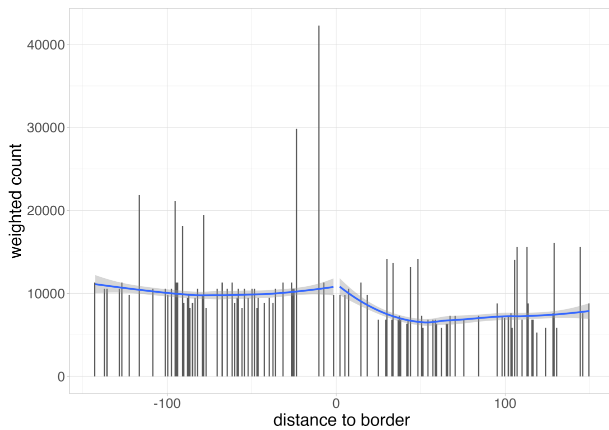

Figure B2. McCrary Test: Weighted density of households on either side of the border with smoothed means.

Table B5—First Stage

	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Distance > 0	0.24***	39,688***	0.18***	38,285***	0.09	36,211***	0.27***	38,764***	0.53***	37,422***	0.44***	42,112**	0.22**	39,354***
	(0.08)	(8,506)	(0.07)	(10,161)	(0.07)	(11,158)	(0.09)	(9,439)	(0.11)	(9,628)	(0.17)	(17,699)	(0.10)	(11,684)
N	1093	2404	1357	1137	1093	917	784	1357	476	1225	476	476	1357	1357
Bandwidth	34.66	62.07	39.41	36.34	34.26	30.26	28.6	40.56	20.54	37.14	20	20	40	40
Kernel	triangular	triangular	triangular	triangular	triangular	triangular	epanechnikov	epanechnikov	uniform	uniform	triangular	triangular	triangular	triangular
Controls			Damages	Damages	Damages/Geo/Demo	Damages/Geo/Demo								
Include unaffected Dists.														
5 km Donut														
Border	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern
Wards	54	81	58	44	46	35	37	63	32	49	11	11	32	32

	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Distance > 0	0.29***	40,455***	0.26***	57,092***	0.23***	39,750***	0.41***	57,931***	1.18***	37,706***	−0.01	41,413***	0.24***	16,635***
	(0.07)	(9,661)	(0.09)	(15,289)	(0.08)	(8,495)	(0.11)	(19,365)	(0.23)	(11,583)	(0.26)	(14,275)	(0.06)	(4,724)
N	2315	2315	1009	1009	1093	2449	1047	829	262	695	304	873	1580	3026
Bandwidth	60	60	35.06	34.39	35.43	62.13	41.14	37.67	13.36	37.23	21.46	39.32	10.55	23.65
Kernel	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular
Controls														
Include unaffected Dists.					X	X							X	X
5 km Donut			X	X										
Border	Eastern	Eastern	Eastern	Eastern	Eastern	Eastern	Point 1	Point 1	Point 2	Point 2	Point 3	Point 3	Whole	Whole
Wards	55	55	56	54	57	87	56	63	16	35	26	44	65	112

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Full set of control variables in columns 5 and 6 includes self-reported earthquake, slope, distance to the epicenter, age and education of household head, a dummy for high caste, number of household members, and the travel time to nearest market and health clinic. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

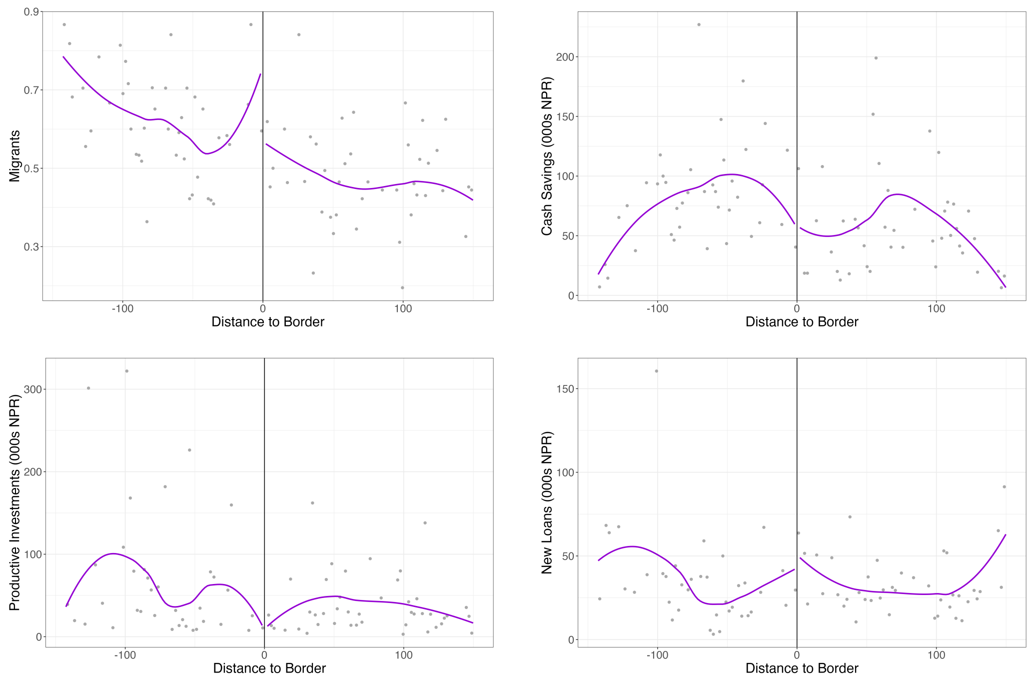

FIGURE B3. BINNED AVERAGES OF INCOME AND SAVINGS AS A FUNCTION OF DISTANCE TO THE BORDER WITH LOESS SMOOTHING. POSITIVE VALUES INDICATE VILLAGES INSIDE THE DISTRICTS THAT WERE PRIORITIZED FOR AID.

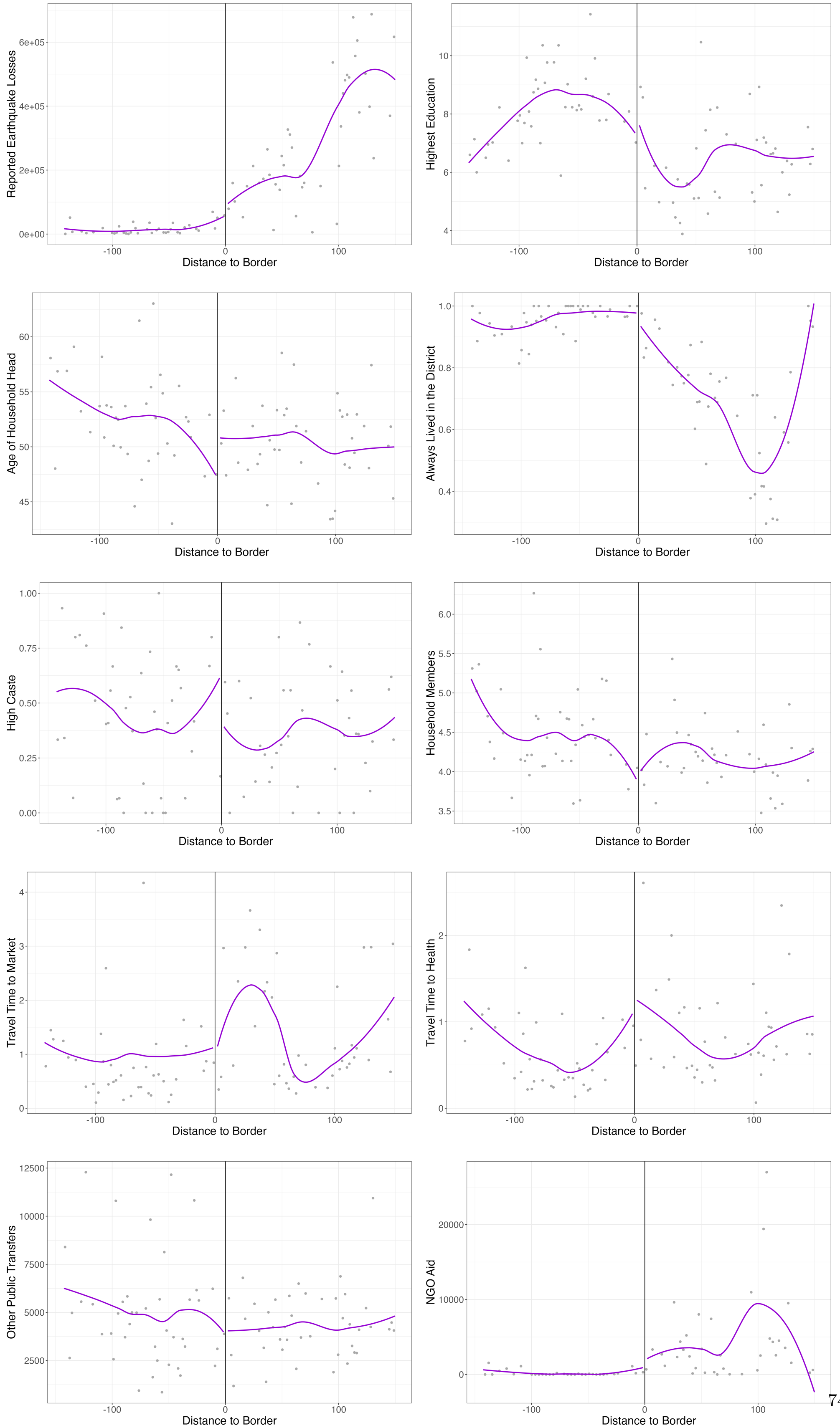

Figure B4. Binned averages of placebo variables as a function of distance to the border with Loess smoothing. Positive values indicate villages inside the districts that were prioritized for aid.

Table B6—Placebo Tests: Damages and Demographics

	quake damages	high caste	age of head	highest educ.	HH members	class 5	class 10	always lived house	always lived dist	NGO transfers	non quake aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance > 0	130,733*	−0.74*	15.91	4.45**	0.25	−0.45	−0.17	−0.22	−0.22	2,632	1,530
	(73,801)	(0.43)	(11.52)	(2.13)	(0.72)	(0.29)	(0.34)	(0.16)	(0.16)	(2,903)	(5,928)
N	1312	1093	962	476	476	476	1093	1093	1093	784	784
Bandwidth	38.97	34.88	33.03	22.35	22.54	19.13	34.25	34.24	34.24	29.63	26.95
Wards	56	58	57	32	35	32	45	65	65	33	39

	quake damages	high caste	age of head	highest educ.	HH members	class 5	class 10	always lived house	always lived dist	NGO transfers	non quake aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance > 0	108,690	0.82*	3.97	4.86	0.25	−0.43	−0.39	−0.21	−0.21	3,597	987
	(88,261)	(0.48)	(13.85)	(3.53)	(1.27)	(0.42)	(0.41)	(0.14)	(0.14)	(2,312)	(7,858)
N	476	476	476	476	476	476	476	476	476	476	476
Bandwidth	20	20	20	20	20	20	20	20	20	20	20
Wards	11	11	11	11	11	11	11	11	11	11	11

	quake damages	high caste	age of head	highest educ.	HH members	class 5	class 10	always lived house	always lived dist	NGO transfers	non quake aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance > 0	125,989	−0.41	13.54	5.44	0.66	−0.40	−0.27	−0.26	−0.26	2,682	2,497
	(105,112)	(0.47)	(17.46)	(4.27)	(1.46)	(0.48)	(0.44)	(0.18)	(0.18)	(2,799)	(9,380)
N	872	872	872	872	872	872	872	872	872	872	872
Bandwidth	30	30	30	30	30	30	30	30	30	30	30
Wards	21	21	21	21	21	21	21	21	21	21	21

	quake damages	high caste	age of head	highest educ.	HH members	class 5	class 10	always lived house	always lived dist	NGO transfers	non quake aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance > 0	131,127	−0.87	18.02	4.10	0.35	−0.19	−0.07	−0.21	−0.21	4,457	3,874
	(104,030)	(0.60)	(17.42)	(3.88)	(1.39)	(0.44)	(0.42)	(0.19)	(0.19)	(3,679)	(9,124)
N	1357	1357	1357	1357	1357	1357	1357	1357	1357	1357	1357
Bandwidth	40	40	40	40	40	40	40	40	40	40	40
Wards	32	32	32	32	32	32	32	32	32	32	32

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors. Slope of running variable allowed to differ on either side of cutoff. No control variables. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table B7—Placebo Tests: Prices and Public Goods

	chicken price	rice price	lentil price	sugar price	mutton price	time to school	time to health	time to market	time to bank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance > 0	33.57	74.34	108.78	5.69	27.79	0.03	−4.60*	−0.55	−1.71***
	(67.04)	(64.28)	(69.47)	(11.90)	(125.76)	(0.10)	(2.47)	(0.43)	(0.52)
N	588	573	590	791	232	435	390	784	476
Bandwidth	40.67	31.6	32.86	33.87	39.44	17.19	14.73	27.17	19.07
Wards	49	62	56	45	58	31	26	35	32
	chicken price	rice price	lentil price	sugar price	mutton price	time to school	time to health	time to market	time to bank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance > 0	11.27	30.82	60.28	−6.27	264.13	0.06	−0.09	−0.43	−1.39**
	(86.69)	(22.44)	(74.38)	(13.16)	(269.62)	(0.13)	(0.36)	(0.40)	(0.61)
N	203	275	293	360	79	476	476	476	476
Bandwidth	20	20	20	20	20	20	20	20	20
Wards	11	11	11	11	10	11	11	11	11
	chicken price	rice price	lentil price	sugar price	mutton price	time to school	time to health	time to market	time to bank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance > 0	33.36	68.25	111.55	0.24	120.26	0.13	0.15	−0.66	−0.54
	(87.88)	(70.51)	(105.65)	(14.10)	(114.82)	(0.16)	(0.43)	(0.54)	(0.45)
N	366	532	551	665	137	872	872	872	872
Bandwidth	30	30	30	30	30	30	30	30	30
Wards	20	21	21	21	20	21	21	21	21
	chicken price	rice price	lentil price	sugar price	mutton price	time to school	time to health	time to market	time to bank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance > 0	35.44	77.01	75.40	9.25	25.89	0.26	0.41	−0.89	1.19
	(75.27)	(107.56)	(64.78)	(13.51)	(123.49)	(0.17)	(0.53)	(0.64)	(0.85)
N	588	878	894	1051	232	1357	1357	1357	1357
Bandwidth	40	40	40	40	40	40	40	40	40
Wards	31	32	32	32	31	32	32	32	32

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors. Slope of running variable allowed to differ on either side of cutoff. No control variables. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table B8—Robustness Checks

Control for Damages:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	115,580*	343,695	−378,221**	68,853	95,849	191,326.10	938,548**	−1.00*
	(59,642)	(269,320)	(190,597)	(151,756)	(126,900)	(152,641)	(472,218)	(0.56)
N	961	1086	1572	940	1266	1135	783	1485
Bandwidth	33.09	35.13	44.23	33.02	37.75	36.56	27.29	43.26
Wards	63	37	58	63	63	69	63	63

Control for Damages, Demographics and Geography:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	174,219	529,314	−651,684	−7,283	236,679	579,984	935,608	0.09
	(144,848)	(389,624)	(399,522)	(356,595)	(340,564)	(525,855)	(638,777)	(0.45)
N	784	1874	1572	1114	1091	1223	1656	476
Bandwidth	26.77	51.2	45.75	36.41	35.55	37.1	48.04	22.65
Wards	37	58	61	50	58	63	76	31

Logged Dependent variable:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	0.57	9.74**	−3.87	0.81	5.39	2.15	0.94	−0.45
	(0.49)	(4.22)	(3.25)	(2.56)	(4.14)	(2.82)	(0.67)	(0.30)
N	1005	1478	608	721	783	917	476	828
Bandwidth	33.54	43.07	23.92	26.13	29.21	31.38	21.67	29.82
Wards	63	63	32	37	37	63	34	39

Logged Dependent variable with Damage Controls:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	0.53	10.50**	−15.86**	0.75	7.39	2.68	1.96	−0.69*
	(0.61)	(5.08)	(7.27)	(3.49)	(5.51)	(3.56)	(1.22)	(0.39)
N	1267	1565	1572	1071	1310	1093	1135	1485
Bandwidth	38	44.61	44.03	34.91	38.34	35.11	36.7	43.26
Wards	65	65	65	63	58	65	63	63

Logged Dependent variable with Damage/Demographic/Geography Controls:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	0.80	16.11	−0.27	−2.04	8.06	7.70	1.18	0.07
	(1.26)	(11.83)	(5.12)	(4.65)	(5.83)	(9.03)	(1.77)	(0.31)
N	916	1435	476	1766	2310	1135	694	476
Bandwidth	31.62	42.56	22.99	49.48	60.02	36.31	25.42	22.65
Wards	47	58	32	73	79	65	35	31

Uniform Kernel:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	59,573**	374,673	−68,833	19,477	62,999	13,273	252,460*	−1.02*
	(23,585)	(248,670)	(74,432)	(74,186)	(62,694)	(44,567)	(139,919)	(0.53)
N	476	1086	435	419	434	435	476	1268
Bandwidth	20.73	35.46	15	15.54	16.18	16.23	19.22	37.81
Wards	46	43	22	22	22	23	37	65

Epanechnikov Kernel:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	96,746**	335,966	−64,195	14,204	102,604	−4,632	844,485**	−0.15
	(41,471)	(208,516)	(72,513)	(89,902)	(101,480)	(47,231)	(409,637)	(0.24)
N	784	1565	476	591	783	476	783	476
Bandwidth	27.63	45.46	20.13	23.81	26.82	21.54	28.79	22.47
Wards	67	47	27	31	43	35	70	32

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Full set of controls includes self-reported earthquake damages, slope, distance to the epicenter of the earthquake, age and education of the household head, caste, number of household members, and the travel time to the nearest market and health clinic. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table B9—Robustness Checks

Alternate Bandwidths:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	77,625	201,593	−65,387	56,138	82,485	10,048	357,602	−0.12
	(52,097)	(220,266)	(128,223)	(157,404)	(116,181)	(83,663)	(353,182)	(0.41)
N	476	473	476	460	475	476	476	476
Bandwidth	20	20	20	20	20	20	20	20
Wards	11	11	11	11	11	11	11	11

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	97,952	295,222	−246,834	43,639	99,963	149,912	787,718	−0.66
	(68,656)	(248,346)	(172,692)	(164,633)	(141,417)	(114,811)	(512,295)	(0.60)
N	871	866	872	853	871	872	870	872
Bandwidth	30	30	30	30	30	30	30	30
Wards	21	21	21	21	21	21	21	21

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	86,316	359,003	−312,637*	40,883	83,005	113,277	740,333	−0.83
	(63,234)	(248,573)	(188,495)	(176,551)	(137,018)	(188,491)	(616,462)	(0.64)
N	1356	1350	1357	1334	1355	1355	1353	1357
Bandwidth	40	40	40	40	40	40	40	40
Wards	32	32	32	32	32	32	32	32

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	79,592*	303,835	−251,762**	17,178	44,567	20,643	428,989	−0.65
	(43,293)	(184,784)	(125,504)	(121,986)	(97,216)	(141,836)	(403,362)	(0.41)
N	1835	1829	1836	1808	1834	1834	1832	1836
Bandwidth	50	50	50	50	50	50	50	50
Wards	44	44	44	44	44	44	44	44

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	50,569	255,325	−198,368**	−26,232	26,677	−38,431	32,737	−0.49
	(31,253)	(157,292)	(100,473)	(96,438)	(77,189)	(95,035)	(299,730)	(0.31)
N	2314	2308	2315	2278	2310	2312	2310	2315
Bandwidth	60	60	60	60	60	60	60	60
Wards	55	55	55	55	55	55	55	55

Including Unaffected Districts:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	96,481**	334,950	−252,773*	53,928	35,153	146,875	578,033**	−0.76
	(43,293)	(210,442)	(131,927)	(117,922)	(69,046)	(136,063)	(284,025)	(0.47)
N	916	955	917	898	1965	784	607	1093
Bandwidth	31.2	33.11	30.57	31.55	53.85	29.62	24.82	34.92
Wards	72	37	37	64	85	58	58	45

5 km Donut Hole:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	12,163	257,127*	−56,430	−149,515	35,264	−31,504	39,332	−0.41**
	(16,896)	(149,933)	(76,789)	(130,668)	(99,300)	(42,393)	(88,850)	(0.19)
N	392	522	392	593	787	392	351	306
Bandwidth	21.15	24.9	22.64	25.41	29.98	18.45	15.93	14.79
Wards	32	28	30	37	48	35	29	28

Distance to Alternative Border Point:

	food consumption	home investment	remittance income	cash savings	new loans taken	investments	total income	migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{Aid}$	46,157	152,401	−176,484	26,424	72,451	98,369	−7,191,592	1.91
	(50,083)	(147,376)	(190,219)	(103,407)	(74,835)	(90,055)	(49,130,223)	(9.83)
N	349	778	349	981	871	871	304	304
Bandwidth	26.71	36.95	27.05	43.7	39.26	39.77	22.75	23.94
Wards	40	41	37	67	53	74	32	39

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedasticity-robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Alternative Border Point is the point where the border intersects the 28th parallel. Donut Hole specification drops villages within 5 km of either side of the border. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Appendix C. Determinants of Earthquake Damages and Effect of Earthquake on Income

Table C1—Effects of Earthquake Damages on Income

A. All Survey Waves

	log(income)				
	(1)	(2)	(3)	(4)	(5)
(Intercept)	11.752***	11.522***	11.717***	11.466***	14.834***
	(0.009)	(0.053)	(0.012)	(0.374)	(1.265)
Earthquake Losses (000s NPR)	0.000	0.000	0.001***	0.020**	0.020**
	(0.000)	(0.000)	(0.000)	(0.008)	(0.007)
log(elevation)					-0.306**
					(0.108)
log(distance to epicenter)					-0.224
					(0.146)
Num.Obs.	16763	16763	16763	16763	16763
R2	0.000	0.043	-0.013	-4.503	-4.469
Fixed Effects		Village + Wave		District + Wave	District + Wave
IV			Shake Intensity	Shake Intensity	Shake Intensity

B. First Survey Wave

	log(income)				
	(1)	(2)	(3)	(4)	(5)
(Intercept)	11.587***	11.566***	11.574***	11.771***	15.154***
	(0.015)	(0.085)	(0.019)	(0.497)	(1.521)
Earthquake Losses (000s NPR)	0.000	0.000	0.000	0.015	0.014+
	(0.000)	(0.000)	(0.000)	(0.009)	(0.007)
log(elevation)					-0.267*
					(0.124)
log(distance to epicenter)					-0.237
					(0.181)
Num.Obs.	5549	5549	5549	5549	5549
R2	0.000	0.032	-0.002	-3.094	-2.350
Fixed Effects		Village		District	District
IV			Shake Intensity	Shake Intensity	Shake Intensity

Notes: Panel A tests the effect of earthquake losses on log income using all three survey waves. Since households that experienced earthquake losses also were more likely to receive aid, this could include the effects of aid. Panel B tests the effect of earthquake losses on log income in the first survey wave before reconstruction aid disbursements began and finds fairly precise null effects, although the final two columns are positive and borderline significant. Thus if anything this would bias estimates of WTP higher for households with higher earthquake damages. In both panels, column 1 is estimated using OLS. Column 2 adds village dummies and survey wave fixed effects in Panel A. Since higher losses could be associated with bigger houses and wealthier households, columns 3-5 instrument for earthquake losses using earthquake intensity (peak ground acceleration from USGS). Columns 4 and 5 add district dummies to control for regional differences in incomes, as well as wave fixed effects in panel A. Column 5 adds additional controls for elevation and distance to the epicenter. Earthquake losses self reported in thousands of NPR. Households with zero or missing income or less than 3 observations dropped. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C2—Building Materials

	Aid Eligible			
	(1)	(2)	(3)	(4)
Foundation:Cement/Stone/Brick	−0.084***			
	(0.003)			
Foundation:Mud mortar-Stone/Brick	0.162***			
	(0.002)			
Foundation:Other	0.041***			
	(0.007)			
Reinforced Concrete	−0.412***			
	(0.003)			
Roof:Bamboo/Timber-Light		0.018***		
		(0.001)		
Roof:Reinforced Concrete		−0.477***		
		(0.002)		
Total Square Feet			−0.0001***	
			(0.00000)	
Death or Injury occurred				0.056***
				(0.003)
N	747,137	747,137	747,137	747,137
R^2	0.355	0.346	0.305	0.299

Notes: Estimated from post earthquake building census in the 11 most affected districts. Dependent variable is whether households were deemed eligible for aid, which indicates that engineers scored the damage to their home as a 4 or 5 on a 1-5 scale. Excluded category in column 1 is Bamboo/Timber foundation. Excluded category in column 2 is Heavy Bamboo/Timber roof. All regressions include village dummies. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table C3—Household Assets and Income

	Aid Eligible				
	(1)	(2)	(3)	(4)	(5)
Owned TV	−0.064***				
	(0.001)				
Owned Mobile Phone		−0.040***			
		(0.001)			
Owned Land			0.074***		
			(0.002)		
Income: Rs. 10-20k NPR				−0.047***	
				(0.001)	
Income: Rs. 20-30k NPR				−0.085***	
				(0.002)	
Income: Rs. 30-50k NPR				−0.102***	
				(0.003)	
Income: Rs. 50k+ NPR				−0.118***	
				(0.005)	
Has Bank Account					−0.052***
					(0.001)
N	747,137	747,137	747,137	747,137	747,137
R^2	0.302	0.300	0.300	0.303	0.301

Notes: Estimated from post earthquake building census in the 11 most affected districts. Dependent variable is whether households were deemed eligible for aid, which indicates that engineers scored the damage to their home as a 4 or 5 on a 1-5 scale. All regressions include village dummies. Excluded category in column 4 is income under 10 thousand. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table C4—Household Demographics

	Aid Eligible				
	(1)	(2)	(3)	(4)	(5)
Migrant Connected	−0.002**				
	(0.001)				
Receives Social Security		0.026***			
		(0.001)			
Male Headed Household			0.036***		
			(0.001)		
Finished High School				−0.059***	
				(0.002)	
Some Middle/High School				−0.027***	
				(0.001)	
Newar					−0.054***
					(0.002)
Other Caste					0.006***
					(0.001)
N	747,137	747,137	747,137	747,137	747,137
R^2	0.299	0.299	0.300	0.300	0.300

Notes: Estimated from post earthquake building census in the 11 most affected districts. Dependent variable is whether households were deemed eligible for aid, which indicates that engineers scored the damage to their home as a 4 or 5 on a 1-5 scale. Excluded category in column 4 is less than high school. Excluded category in column 5 is high caste (Brahmin/Chhetri). All regressions include village dummies. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.