Targeting Disaster Aid: Visibility and Vulnerability after the 2015 Nepal Earthquake

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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 both the exposure to a shock and the ability to borrow for consumption smoothing. I calibrate the model using household survey data following the 2015 earthquake in Nepal, and I use a spatial discontinuity in the distribution of reconstruction aid to test the model's assumptions. Aid increases consumption and housing investment, but decreases remittances, consistent with a model of incomplete insurance. I use the calibrated model to estimate the benefits of counterfactual aid allocations. Conditioning aid on household property damage barely outperforms allocating aid at random. The property damage criterion excludes many liquidity-constrained households that have high demand for aid, and it includes wealthy, well-insured households that have low demand. An untargeted approach that divides the aid budget equally between all households in the affected areas yields substantially 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

Giving cash aid 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 many disasters, funds earmarked for shelter are the only source of cash aid directly 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 largest in that region since 1934, and by some estimates left 12% of the country homeless (The Asia Foundation, 2016*b*). In the months after the earthquake, households

[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.

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 assumption of incomplete insurance. In addition, the magnitudes of the observed effects are similar to 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 only concerned with helping households smooth consumption, and 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 6% 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 52%. This universal allocation results in large 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 not accurate. This implies that collecting better data on household damages to inform aid distribution is not 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 (Jorgenson and Schreyer, 2017). An untargeted approach to aid would also increase consumption by more than damage-based targeting, although the latter approach 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). I build on that work by showing that even well-intentioned targeting strategies can lead to misallocation.

I develop a way to measure that misallocation, taking into account the objectives of disaster aid. In the context of anti-poverty programs, targeting strategies can be 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. As articulated in Banerjee et al. (2023), sometimes a household's willingness to pay for \$1 can be more than \$1.

Furthermore, I connect this measure of demand for aid to previous work on social welfare functions (SWFs) and non-market valuation. It is well known that maximizing a social welfare function weighted by the inverse marginal utility of income is equivalent to maximizing willingness-to-pay (Negishi, 1960; Nordhaus and Yang, 1996). I show that the allocation that results from the modified measure of WTP is equivalent to a social welfare function weighted by the inverse marginal utility of *expected* income. As such, it can be thought of as holding long-run inequality constant. In other words, the social planner is interested in helping households smooth temporary shocks, but not in redistributing long-run wealth.

In addition to the structural model, this paper contributes evidence on the causal effects of disaster aid on consumption, income, and investment at the household level 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 my 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). Some studies using cross-country data find that countries with more developed financial markets tend to experience smaller GDP losses after a disaster McDermott, Barry and Tol (2014). In the US, studies using administrative data 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, Kawano and Levitt, 2018; Deryugina, 2017; Billings, Gallagher and Ricketts, 2022). 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, both as means of subsistence (Raut and Tanaka,

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

2018; Lokshin, Bontch-Osmolovski and Glinskayai, 2007), and as a means of insuring against environmental shocks (Maystadt, Mueller and Sebastian, 2016). Nepalis endured a decade of Civil War from 1996-2006, and another eight years of transitional governments in the lead-up to the earthquake.

The Himalayan portion of the country is a subduction zone that experiences frequent earthquakes. In April 2015 the Gorkha earthquake, named after the province where it occurred, measured a 7.8 on the Richter scale - the largest quake in this region since 1934. The earthquake triggered landslides throughout the region – entire villages were flattened, killing nearly 9,000 people immediately, and leaving an estimated 12% of the country homeless (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*).

Shortly after the earthquake, Nepalis held elections for delegates to a constitutional convention. Controversies over representation lead to protests and a blockade of the border with India, resulting in fuel shortages. This, along with a drought the following year, 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 raised $4.1B

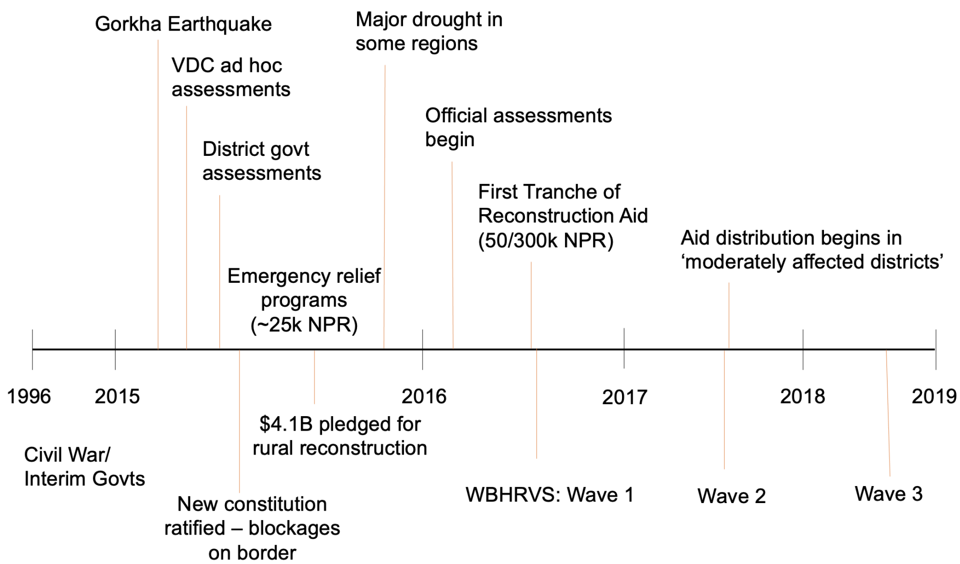

Figure 1. Timeline of Events

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 June 2016, fourteen months 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, n.d.).

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.

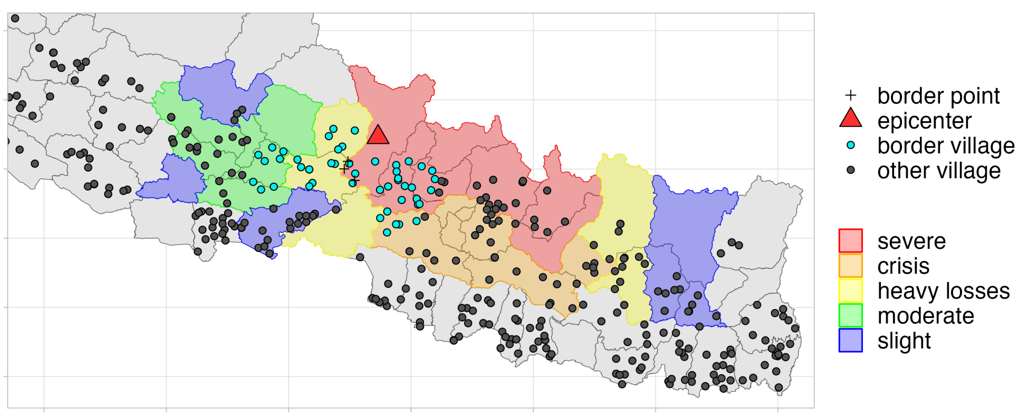

Figure 2. Map of study region

Notes: District colors denote earthquake damage designations from Housing Recovery and Reconstruction Platform (n.d.). 'Severe' and 'Crisis' districts were prioritized for aid. Black circles represent VDCs present in the survey. Blue circles are VDCs within 75 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). Villages in slight and none districts were dropped in the main specifications. Crosses mark the border points used in the regression discontinuity analysis.

A. Data and Descriptive Statistics

The World Bank Household Risk and Vulnerability Survey (HRVS) 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 2. I also use variables from the US Geological Survey, including 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, not including 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 earlier.

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 46,000 NPR in 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 – another possible explanation for this breakdown of losses is that many households' primary source of income comes from migrants overseas. 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, which 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 1.1% on average.

There is important heterogeneity across several subgroups in the sample, however. Column 3 explores heterogeneity by caste and ethnicity. Members of the economically and politically powerful Newar group smooth consumption more effectively than other groups as indicated by the negative coefficient on the interaction term. This is consistent with Bhusal et al. (2022) findings on preferential access to aid amongst the dominant caste. Columns 4-6 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.

Table 1—Consumption Smoothing

	log(food consumption)				
	(1)	(2)	(3)	(4)	(5)
log(income)	0.114*** (0.008)	0.114*** (0.009)	0.113*** (0.008)	0.126*** (0.008)	0.112*** (0.007)
log(income):Dalit		0.010 (0.018)			
log(income):Newar		−0.039* (0.021)			
log(income):Other		0.0005 (0.010)			
log(income):Female Head			−0.012*** (0.002)		
log(income):Land > Median				−0.023** (0.008)	
log(income):Quake Affected					0.012 (0.015)
N	16,699	16,698	16,699	16,699	16,699
R^2	0.763	0.763	0.765	0.763	0.763

Notes: All regressions include household and village-year fixed effects. Standard errors clustered at the ward. 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". ***, **, and * denote significance at the 1, 5, and 10 percent level respectively.

Table 2—Transfers

A. Within Household Variation

	Earthquake Aid	NGO Transfers	Informal Transfers	Cash Savings	Remittances	Migrants	Loans Taken
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(income)	2.285*	−0.197	0.112	4.830***	−9.555***	−0.051***	0.707
	(1.130)	(0.189)	(0.160)	(1.479)	(2.507)	(0.012)	(3.221)
lag remittances					−0.445***		
					(0.045)		
lag loans							−0.813***
							(0.089)
Fixed Effects	Household	Household	Household	Household	Household	Household	Household
Cluster	Ward	Ward	Ward	Ward	Ward	Ward	Ward
N	16,699	16,699	16,696	16,621	11,142	16,699	11,146
R^2	0.493	0.352	0.335	0.492	0.809	0.375	0.783

B. Within Village-Year Variation

	Quake Aid	Work Aid Wages	Govt Transfers	NGO Aid	Remittances	Migrants	Loans Taken
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Land Value > Median	0.022	−0.042	0.616	−0.102	21.209**	0.041**	18.984**
	(0.368)	(0.030)	(0.463)	(0.179)	(7.286)	(0.015)	(7.512)
Fixed Effects	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year
Cluster	Ward	Ward	Ward	Ward	Ward	Ward	Ward
N	16,699	16,699	16,699	16,699	16,694	16,699	16,696
R^2	0.489	0.161	0.122	0.362	0.140	0.142	0.098

Notes: Coefficients of regression of forms of transfer income and migration on dummy for land value in first survey wave greater than median. 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 ward. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Landowners being able to smooth effectively is consistent with the mechanism proposed in Jayachandran (2006), in which wage laborers bear the brunt of agricultural productivity shocks. 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.

The columns in Table 2.A show log income regressed on various forms of transfers, conditional on a household fixed effect. This specification identifies which types of transfers are pro- or counter-cyclical with respect to household income fluctuations. Remittances and migration 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.

Columns (4) and (6) show that previous year remittances and previous year loans are negatively correlated with current year remittances and loans, conditional on the same income shock. This is suggestive evidence of borrowing constraints. Earthquake aid is positively correlated with income shocks, possibly because it arrived more than a year after the event in most cases.

Table 2.B regresses a dummy for above average landholdings on various forms of transfer income, conditional on a village-year fixed effect that captures aggregate shocks to the village. This specification tests how various forms of transfers correlate with a proxy for wealth within a village. Remmittances, migration, and

loans in a given year are positively correlated with household landholdings within villages.

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

III. Framework

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 the key variables at play. These include varying levels of damage from the earthquake and varying abilities to self-insure. This second factor 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 features in the style of a buffer-stock consumption smoothing model (Deaton, 1991; Aiyagari, 1994; Banerjee and Duflo, 2005; Kaboski and Townsend, 2011) that incorporates investment in a durable housing stock. Once calibrated to the data, the model allows me to compare the surplus generated by counterfactual aid targeting strategies.

In the model, households (indexed by i) choose food consumption c, and housing investment ι, 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 relative importance of housing, as well as risk aversion between periods (Yang, 2009; Francisco, 2019). 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 $log(Y_i) \sim N(\mu_i, \sigma^2)$.

In order to smooth consumption, households can borrow from and lend to other households in the economy at interest rate R. This captures loans as a source of consumption smoothing as discussed in the previous section. 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. 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) and impose minimum consumption and housing thresholds, $\underline{c}$ and $\underline{h}$. This allows households to default, in which case the state and control variables are restricted to:

$$c_{it} = \underline{c}; h_{it} = \max[h_{it}, \underline{h}]; x_{it+1} = B; \text{ and } h_{it+1} = \delta \max[h_{it}, \underline{h}] \tag{6}$$

Minimum consumption, minimum housing, and the borrowing limit B are multiples of household expected income, which, given the income process, is $\exp(\mu + \sigma^2/2) := M$. So $\underline{c} = \bar{c}M$, $\underline{h} = \bar{h}M$, and $B = \lambda M$.

The effects of the earthquake can show up either as a 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. Secondly, since the survey data only covers the period following the earthquake, income can be thought of as a post-disaster observable – in principle, it 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 $\nu(.) = M^{\gamma-1}V(.)$. 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 mean. 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:

$$
\begin{aligned}
&M_i^{\gamma-1}V(x_{it}, h_{it}|M_i) = \nu(x_{it}, h_{it}) = \\
&\qquad \max\Big\{\max_{\iota,c} u(c_{it}, h_{it}, \iota_{it}) + \beta E[\nu(R(x_{it} - c_{it} - \iota_{it}) + y_i, \delta(h_{it} + \iota_{it}))], \\
&\qquad \nu_d(h_{it})\Big\}
\end{aligned} \tag{7}
$$

subject to (4) and (5), and $c \geq \underline{c}; h_{it} + \iota \geq \underline{h}$, where

$$\nu_d(h_{it}) = u(\bar{c}, \max[h_{it}, \bar{h}], 0) + \beta\nu(\lambda, \delta\max[h_{it}, \bar{h}])$$

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 preceding equations introduce a lot of structure to household decision making. The key features are informed by the data, however – households can borrow to smooth consumption and rebuild housing, but not in unlimited quantities. 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.

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, targeting aid based on housing damages rather than means-testing using other measures of deprivation would make little sense.

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. This type of objective would seem to favor a targeting strategy based on disaster exposure relative to a SWF with more redistributive preferences. For example, the equal-weighted utilitarian SWF employed by the anti-poverty targeting literature would favor targeting based on more direct proxies for wealth or consumption. 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.

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 wealth 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

[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:

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

aid can be calculated as the discounted sum of expected payments:

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

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, it has parallels to the Negishi-weighted social welfare function (Negishi, 1960), which would choose a vector of aid allocations $a_1, ..., a_N$ to maximize:

$$W = \sum_{i=1}^{N} (\frac{dV_i}{dY_i})^{-1} V(x_i, h_i | M_i).$$

Negishi weights are frequently used 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, equation (III.B) reduces to $W = NR$ for any allocation.

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:

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

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.

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 incomes, 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 willingness-to-pay 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. It is also important to note that, since the data is collected a year after the earthquake, these results are best interpreted as relevant for long-term reconstruction aid, rather than emergency aid. In fact this was the goal of the major aid program, and most of the aid was disbursed one to three years after the earthquake.

Finally, 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. 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. This suggests that my results will possibly overestimate the relative value of damage-based aid targeting in this setting.

C. Model Calibration

In order to calculate household 'willingness-to-pay' I calibrate the behavioral model outlined in Section III.A to the households in the data. The model has three state variables (x_{it}, h_{it}, M_i), two choice variables (c_{it}, ι_{it}), and 9 parameters $(\theta = \{\beta, \gamma, \alpha, R, \delta, \bar{c}, \bar{h}, \lambda, \sigma\})$. The state variables summarize all sources of household heterogeneity in the model. The choice variables are used 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 received aid (and thus experienced severe damages) have an average housing value of just under 4 times average income in the first survey wave, whereas households that didn't receive aid have an average housing value of 8 times average income.

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 both for normalizing the other state variables, and 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). Furthermore, I regress earthquake damages on income in Appendix C and find a precise zero effect. This holds after instrumenting for earthquake damages with earthquake intensity, and adding district fixed effects. I also estimate expected remittances using a similar approach to income, and I include the non-cyclical component of remittances to both expected and realized income.

I infer values for household wealth, x_{it}, by noting that the total value of household end of period liquidity in the model is $x_{it}+a_{it}-c_{it}-\iota_{it}$. To calculate savings, I use data on household cash, bank savings, and jewelry, as well as formal insurance and savings group assets, loans and informal transfers made, investments in productive assets and human capital (education costs). I calculate debts as the sum of loans taken plus informal transfers. The cyclical component of remittances is added to savings and debts when negative. To incorporate the cost of debt service, end of period liquidity is then Savings $-(R-1)$Debt. Subtracting aid and adding consumption and investment gives beginning of the period liquidity.

All state and choice variables are normalized by M_i. Finally, to account for unmodelled variation from the business cycle, and household age structure, size, and education levels, I follow Kaboski and Townsend (2011) in purging these sources of variation from the model. Full details are in Appendix A.

This leaves us with the parameter vector, which is estimated by minimizing the sum of squared errors of a vector of moment conditions. I describe the procedure for doing so in Appendix A. My approach largely follows Kaboski and Townsend (2011). The resultant parameter values are shown in Table 3, and generally fall within the range of normal values in the literature. One notable exception is the coefficient of risk aversion, which is quite high, though perhaps not exceptional

Model Calibration

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, maintenence, furniture, and large appliances in survey.
Y_{it}	Realized Income	Income from all sources in the survey, plus non-cyclical component of remittances, see Appendix A.
M_i	Expected Income	Estimated from household characteristics, see Appendix A.
x_{it}	Wealth	Inferred from survey values for savings, consumption, and investment as described in text. Household savings and credit also used separately to identify interest rates.
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	8.58 (0.23)	Coefficient of Risk Aversion
β	0.94 (0.13)	Discount Factor
α	0.64 (0.03)	Cobb-Douglass Share on Consumption
δ	0.92 (0.002)	Depreciation Factor for Housing
R	1.05 (0.002)	Interest Rate (+1)
$\bar{c}$	0.13 (2.6)	Minimum Consumption as Fraction of Expected Income
$\bar{h}$	0.31 (2.6)	Minimum Housing as Fraction of Expected Income
λ	0.16 (0.29)	Borrowing Constraint as Fraction of Expected Income
σ	0.58 (0.02)	Standard Deviation of Log Income

Table 3—Model Variables and Calibrated Parameters

given the context of poor households in the aftermath of a major earthquake.

With the calibrated model in hand, I can compare the aggregate value of various targeting strategies by calculating each household's value for aid as described in the previous section. As discussed, these counterfactuals require accepting the structure of the model in Section III. We need not do so blindly, however. The model generates testable predictions about household responses to aid. In particular, our model predicts that households partially smooth, so aid should have some effect on housing investment and consumption, but also some negative effects on smoothing strategies including migration and remittances. Although aid was contingent on housing, our model predicts it is fungible, and households will use it for both housing and consumption as needed. Finally, we should see limited effects of aid on income.

Validating these predictions can help build confidence in the underlying assumptions. I use a regression discontinuity estimation strategy in the following section to do so.

IV. Empirical Evidence

Testing the above predictions is complicated due to the fact that receipt of aid is endogenous to both observed and unobserved earthquake damages, which are likely to be correlated with outcomes of interest, including consumption, income, savings, and housing quality. In addition, the earthquake occurred in a distinctive region of the country – disproportionately affecting the mountainous districts surrounding Kathmandu. To infer 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 2). 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 2), 60% of eligible households had received at least 2 tranches of aid, whereas in the moderately affected districts (yellow, green, and blue in Figure 2), only 15% of eligible households had received the same (Housing Recovery and Reconstruction Platform, n.d.).

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:

$$Z_{it} = \beta_1 \widehat{\text{Aid}}_{it} + \beta_2 d_i + \beta_3 d_i \mathbf{1}_{\{d_i > 0\}} + \beta_4 X_i + e_{it}. \tag{10}$$

Z_{it} is an outcome variable for household i at time t. Aid is a variable for whether a household has received at least their first tranche of aid, and it is instrumented with a dummy variable for whether a household is on the 'right' side of the border, making this a fuzzy RD specification.

The running variable d is distance to a point on the border of the most affected districts in km, specified so that the slope can differ on either side of the border[5]. X is a vector of control variables in some specifications to increase precision. I use the Belloni, Chernozhukov and Hansen (2014) double-lasso procedure to select control variables for each specification from a list including time period dummies, shake intensity, slope, elevation, earthquake damages, distance to the

[5] Cattaneo, Idrobo and Titiunik (2020) recommend using distance to a specific point on the border for two dimensional regression discontinuity applications rather than distance to the entire border. I choose the place where the border intersects the 28th north parallel, as it is in an area with a high density of surveyed villages (see Figure 2). In Appendix B I show the results of several other choices including distance to the eastern border, distance to the entire border, and distance to two alternative points - the points on the border closest to a village on either side.

epicenter, household size, age, education, caste, time living in the district, travel time to the nearest bank, health clinic, school, and market. 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 30 and 50 km. I test robustness to alternate bandwidths in Appendix B. The blue dots in Figure 2 indicate villages that fall within the bandwidth for inference for the first stage regression. 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.[6] 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.

Table B1 in Appendix B shows the results of the first stage regression. Households just inside the border of the most affected districts were 42% more likely to receive aid, and received an additional 33,000 NPR on average in the primary specification. Alternative specifications give similar results, including different choices of bandwidth, kernel, distance to alternate border points (or distance to

[6]I use triangular weights in the baseline specification and test sensitivity to the epanechnikov kernel in Appendix B.

the entire border), inclusion of less affected districts, and a 'donut hole' specification that excludes villages within 5km of the border. Figure 3 illustrates the discontinuity graphically, by plotting the binned averages of the probability of receiving aid as a function of distance to the border point.

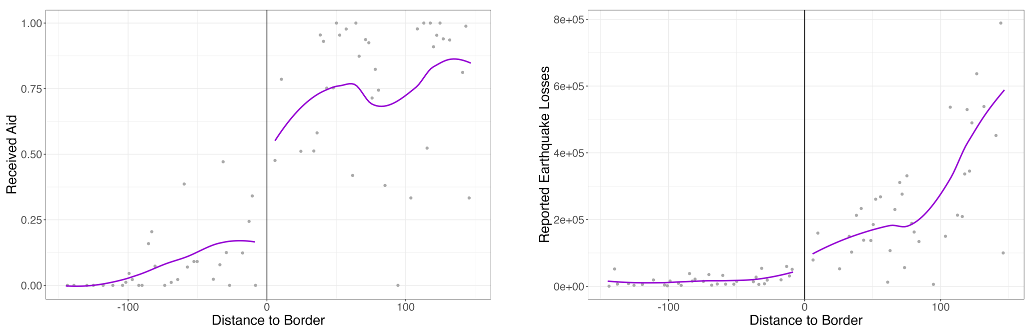

Figure 3. Binned averages of fraction receiving aid and earthquake damages as a function of distance to the border point with Loess smoothing.

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, pre-quake 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.[7]

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 (10), replacing Z with a placebo variable that should not change with the receipt of aid. Since I use the control variables in X as placebos, I drop them from the right hand side of the regression.

As seen in Appendix Table B2, I find no difference in age, caste, or education of household heads, the number of household members, or the fraction of households that have always lived in the same house or same district on either side of the border, supporting the idea that household demographics are comparable. I find a borderline significant difference in the probability that a household head has completed at least five years of schooling. I also test for travel times to the nearest bank, school, market, and healthcare clinic as a proxy for policy and public good provision. I find a negative and significant effect on travel time to the nearest

[7]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.

bank, and no differences for the others. Since a bank account was a prerequisite for receiving aid, it is possible that households that received aid were more aware of the locations of nearby bank branches. I also test for differences in the receipt of NGO aid and other government aid programs and find no differences. 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 be simply due to chance, as the p-values are not adjusted for multiple hypothesis testing. 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*

Table 4—Main Outcomes

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	52,833*	205,225	−176,813**	121,283	312,691	430,845	−0.71**
	(29,010)	(223,787)	(83,968)	(339,171)	(259,849)	(285,584)	(0.34)
N	782	473	824	333	627	433	565
Bandwidth	38.05	29.49	38.91	25.95	34.41	28.39	32.3
Wards	43	31	58	31	50	51	42

Model Predictions: Response to 300k Aid

	food consumption	home investment	borrowing			total income	
	(1)	(2)	(3)			(6)	
Aid	25,180	82,457	-192,363			0.0	
	(17,160)	(62,976)	(66,790)			0.0	

Notes: 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. Excluded households in unaffected or 'slightly' affected districts. Model predictions table gives the mean and standard deviations of the predicted effect for households that actually received aid. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Turning attention to outcomes of interest, the main specification in the first row of Table 4 shows large, but imprecisely measured effects of aid on both food consumption and home investment (food consumption shows a significant increase at the 10% level). The effects on consumption smoothing strategies, remittances and migration, are large, negative, and precisely estimated. Estimated effects on savings and income are positive but imprecise. Figure 4 shows the discontinuities for these variables in the raw data, which are especially visually apparent for housing investment and remittances. Similar plots for additional variables, including the placebos, appear in Appendix B.

These results are consistent with aid partially substituting for informal insurance, but they also suggest that households are not completely insured, otherwise aid would have no effect on food consumption or home investment. In Appendix B, I show robustness to alternative choices of kernal, bandwidth, control variables, border points, and logged dependent variables. The estimates are noisy, and both magnitudes and significance levels vary. Most specifications show similar qualitative results, however - a large drop in remittances and migration, a large but noisy increase in housing investment, and a smaller but still substantial effect on food consumption.

In the third row of Table 4, I compare the findings from the regression discontinuity estimates to the model predictions by calculating counterfactual consumption, investment, and savings responses to aid using the optimal policy and investment functions from Section III.C. Restricting attention to the same sample of border households from the RD analysis that actually received aid, I subtract aid from each household's x_{it} and recalculate predicted consumption and investment. Then I add back 300,000 NPR, and calculate consumption and investment again. The predicted effects are reasonably close to the results from the regression discontinuity analysis.

The model apparently predicts smaller effects on consumption and housing investment than I observe, but the effect on savings is very close to the observed effects on remittances. 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. I take these simulations as suggestive 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.

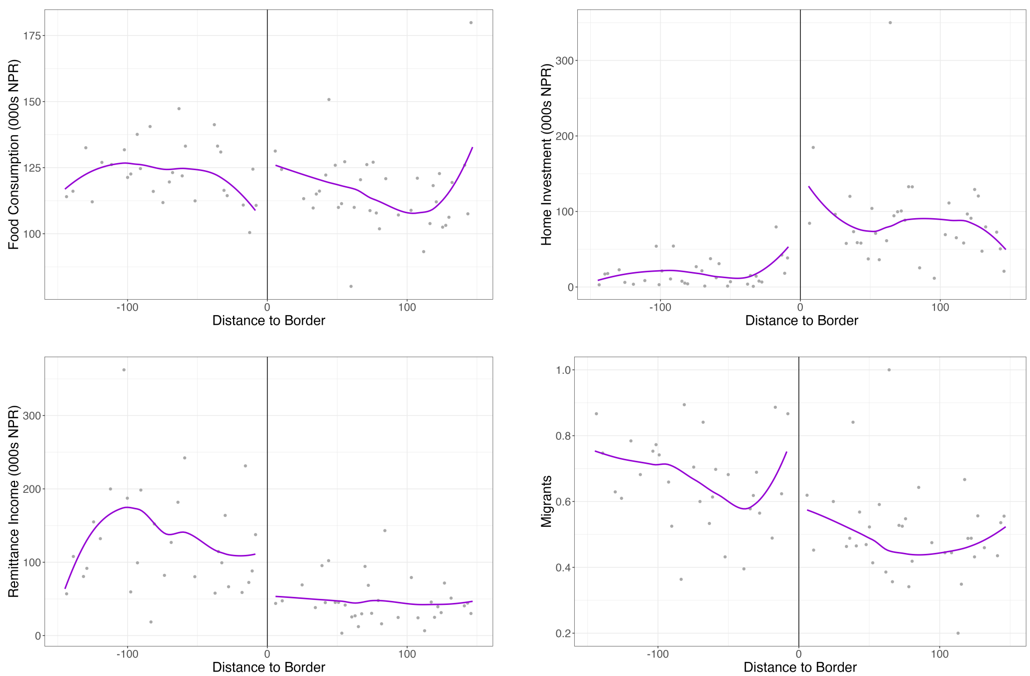

FIGURE 4. BINNED AVERAGES OF OUTCOME VARIABLES AS A FUNCTION OF DISTANCE TO THE BORDER POINT WITH LOESS SMOOTHING.

V. Counterfactual Reallocations

I use the calibrated model to explore counterfactual allocations of aid by solving for each household's estimated 'willingness-to-pay' (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. This scenario chooses 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 well as the households that received aid from NGOs.

As discussed above, this measure of WTP is increasing in income. Thus, an allocation could result in low welfare gains if it prioritizes of low-income households. Therefore, as a robustness check, I also compare targeting strategies using a utilitarian social welfare function. This criterion prioritizes poor households with a high marginal utility for liquidity.

An estimated 38% of households received aid, while about 58% of households reported earthquake damages. In all counterfactuals except for the universal allocation, I hold the fraction of households receiving aid constant, and I continue to use survey weights in the analysis that follows to make the estimates representative of the population in these districts. Error bars are bootstrapped non-parametrically using 1,000 replicates, and reflect sampling error, but not specification error from the model or parameter estimates. To eliminate intertemporal comparisons which may be contaminated by the effects of aid, I analyze all counterfactuals based on WTP in the first survey wave. I also subtract any aid received in the first wave from household liquidity to get estimates of pre-aid WTP.

I find that the average household would give up 16.5% of future income in order to receive 300,000 NPR in reconstruction aid, reflecting a willingness-to-pay of 417,000 NPR. There is substantial heterogeneity, however, with a standard deviation of nearly 300,000 NPR. The willingness-to-pay for aid that must be used for housing is on average 29% lower than for unconditional aid. The optimal allocation of aid would theoretically improve upon the random allocation by 53%.

I find that the WTP of households that actually received aid was about 6% better than the random allocation. This, if anything, overstates the benefits of

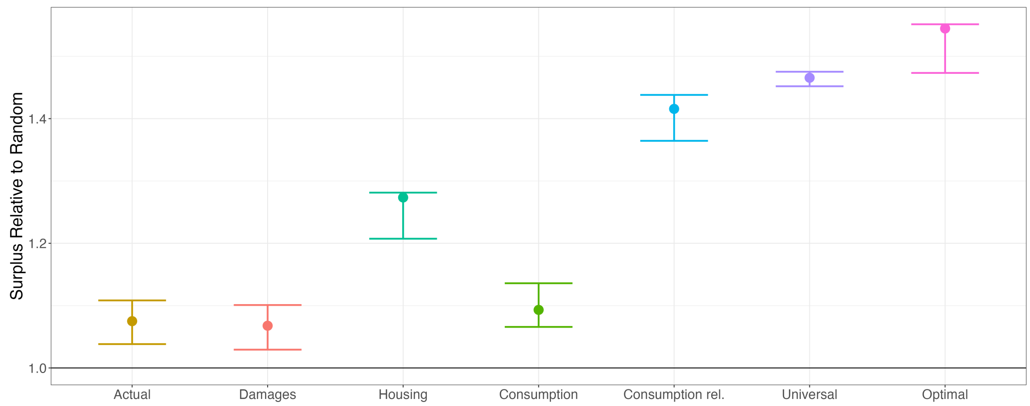

Figure 5. Household surplus from counterfactual targeting strategies, relative to a random allocation (point estimates and 95% confidence intervals). Consumption and housing scenarios target based on absolute values, whereas consumption rel. targets based on consumption relative to expected income.

the actual allocation, since it is based on WTP in the first 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 reported damages as truth and analyze an allocation of aid if policymakers were able to perfectly target the households with the worst damages. I find that the welfare from this approach is similar to the true allocation – a 6% improvement over the random allocation. These targeting strategies perform somewhat better when analyzed according to the utilitarian social welfare function, though not as good as some of the alternatives.

I find that targeting absolute levels of consumption and housing generates welfare gains of 8% and 25% respectively. The utilitarian criterion prioritizes these households by much more, as expected, since they have the highest marginal utility for wealth.

Larger gains to WTP come from allocations that target based on measures of acute deprivation, measured by consumption as a fraction of expected income[8].

[8]This is distinct from measures of subsistence as defined in Bryan, Chowdhury and Mobarak (2014)

Targeting based on this measure could in theory generate benefits 41% higher than the random allocation. These households have high income, and high current consumption needs, thus they are more willing to sacrifice future income for present liquidity. These measures are likely difficult to target in practice, however. Most research on proxy-means-testing focuses on targeting long term deprivation, and Tractman, Hendra Permana and Aryo Sahadewo (2022) finds that community based targeting generally does not prioritize dynamic measures of need.

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. Somewhat surprisingly, this allocation generates nearly as much welfare as the optimal allocation. It also performs best by the utilitarian criterion. 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 dominates any gains to targeting larger amounts of aid. Full results comparing targeting approaches according to each criteria are in Table 5.

These results suggest that the extensive rounds of targeting and building verification did not add much value. Relief agencies would have created more value by spending the resources used during the targeting and verification process on increasing the aid budget, and then allocating aid randomly, or dividing it into smaller amounts between more households. This finding is robust to preferences

or Jensen and Miller (2008). These papers define subsistence as food making up a high share of total expenditure, while I measure acute deprivation as low consumption relative to expected income. The definitions coincide when a household with high expected income has low liquidity, and thus is forced to spend its entire budget on basic needs. They diverge in that some households could be considered subsistence, but if they have low expected incomes, they could be in a state of permanent, rather than acute deprivation. These households could have high consumption relative to income (not acute deprivation), but still spend the majority of their budget on essentials (subsistence).

for redistribution, as the universal approach dominates by both the WTP and utilitarian criteria.

	Actual	Damage	Housing	Consumption	Consumption Rel.	Universal	Optimal
Wtp	1.06	1.06	1.25	1.08	1.41	1.52	1.53
	(1.03,1.10)	(1.02,1.09)	(1.19,1.26)	(1.05,1.12)	(1.36,1.43)	(1.51,1.53)	(1.46,1.54)
Utilitarian	1.96	1.94	2.23	2.15	1.61	2.27	2.12
	(1.52,2.12)	(1.50,2.08)	(1.82,2.51)	(1.72,2.67)	(1.08,2.10)	(1.78,2.76)	(1.81,2.29)

Table 5—Counterfactual Targeting Scenarios: Benefits relative to Population Average for 300,000 NPR aid using different social welfare functions (except universal scenario which uses .38*300,000 NPR. Bootstrapped 95% confidence intervals in parentheses.

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 household values for aid do indeed differ, 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 increase welfare by much more than alternative targeting strategies, regardless of preferences for redistribution.

To arrive at these conclusions, I calibrate a structural model of household consumption 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. Much more important, however, is whether a household can draw on forms of informal insurance, including remittances and loans.

To test the assumptions of the model, I estimate the effects of the reconstruction aid program on household consumption, income, investment, and transfers using a spatial regression discontinuity design. I find that receiving aid increased food consumption and housing investment, and decreased remittances and migration. This is consistent with a model of partially insured households using remittances and migration to smooth shocks. When I use the structural model to simulate the effects of aid, I find that the predictions of the model match these estimated effects well.

Finally, I use the model to explore several different allocation rules, finding that targeting based on damages, no matter how accurate, is unlikely to meaningfully increase the value of aid. By a variety of criteria, the benefits of dividing the aid budget amongst all households vastly outperforms damage based targeting. This strategy has the additional benefit of not requiring any budget or time for needs assessment.

These findings also 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. 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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Appendix A. Structural Model and Calibration

A1. Value Function Normalization

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

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

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

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

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 $log(y_i) = log(Y_i/M) = log(Y_i) - log(M_i) = log(Y_i) - \mu_i - \sigma^2/2 \sim N(-\sigma^2/2, \sigma^2)$.

Therefore, I want to 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:

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

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

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 $c^*(x, h, M)$, and $\iota^*(x, h, M)$ 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 2500 points, with 50 equally-spaced points for 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 13 Gaussian quadrature points. At each grid point I find the values of c and ι that maximize A1 using a semi-global optimization algorithm to avoid local minima introduced by the interpolation and quadrature around the kink(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 i 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 sum of squared errors of a set of moment conditions derived from the model. To do so I use a nested fixed-point algorithm.

The optimal consumption and investment functions define the first two moment conditions:

$$e_{it1} = c^*(x_{it}, h_{it}, M_i) - c_{it}$$

$$e_{it2} = \iota^*(x_{it}, h_{it}, M_i) - \iota_{it}.$$

Since the value function kink often results in somewhat jagged policy functions, I smooth the policy functions by taking the average of the four closest grid points.

Following Kaboski and Townsend (2011), I gain six additional moment conditions by interacting e_1 and e_2 with functions of each of the state variables so that the model's predictions are not biased for any values of the state variables:

$$\begin{aligned} e_{it3} &= e_{it1} \log(M_i) \\ e_{it4} &= e_{it1} \text{ihs}(x_{it}) \\ e_{it5} &= e_{it1} \log(h_{it} + 1) \\ e_{it6} &= e_{it2} \log(M_i) \\ e_{it7} &= e_{it2} \text{ihs}(x_{it}) \\ e_{it8} &= e_{it2} \log(h_{it} + 1). \end{aligned}$$

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

I pin down interest rates by using data on household capital income and interest payments.

$$e_{it9} = (R-1)\,\mathit{Credit} - \mathit{Interest\ Paid}$$

$$e_{it10} = (R-1)Savings - Capital\ Income$$

Depreciation rates are measured using an approach from Malpezzi, Ozanne and Thibodeau (1987) based on the age of the housing stock.

$$e_{it11} = \frac{1}{1-\delta} - Age\ of\ Home$$

The final moment condition helps identify the variance of the income process and is:

$$e_{it12} = \log(Y_{it})^2 - \sigma^2.$$

Since the data contain variation not explicitly modelled, including life-cycle 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 differ between older and younger families, for example, and these differences would bias my estimates of how each of these sets of households value aid. Purging variation associated with these differences requires careful consideration, however, since our counterfactuals address targeting, which is highly dependent 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 the business cycle - including price changes and exchange rate fluctuations.

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:

$$\begin{aligned} log(c_{it}) &= \Gamma_1 W_{it} + e_{it} \\ log(h_{it}+1) &= \Gamma_2 W_{it} + e_{it} \\ Savings_{it} &\sim \text{Tobit}(\Gamma_3 W_{it} + e_{it}) \\ Credit_{it} &\sim \text{Tobit}(\Gamma_4 W_{it} + e_{it}) \\ \iota_{it} &\sim \text{Tobit}(\Gamma_5 W_{it} + e_{it}) \end{aligned} \tag{A6}$$

Where W_{it} is a vector of household characteristics including quadratic polynomials of age of the head, education, and the number of members (including migrants), as well as the number of children and elderly members, and survey wave fixed effects. I then construct an adjusted dataset, where the values of consumption, investment, housing wealth, savings, and credit are the fitted values of A6 for a household with mean values of the independent variables, plus the household specific residual.

Income and remittances are treated differently, to allow for purging the same sources of heterogeneity, but also accounting for additional household specific variation in order to estimate expected income and remittances, and annual shocks more precisely. Thus the residual of the life cycle regression is modelled as depending on an additional vector of household characteristics U_{it}, including a quadratic polynomial of land value, a dummy for female headed households, village and caste/religion fixed effects, and a detailed migration history (from before the earthquake) that includes data on migrant destination, gender, age, education, and earnings.

Then annual realized income (and remittances) is constructed exactly as the other variables. Expected income and expected remittances are constructed using the mean values of the life cycle variables ($\bar{W}_{it}$), as in the other regressions, but the household specific values of land value, gender of the household head, village, caste, religion, and migration history. This procedure for income is summarized in the following equations.

$$\begin{aligned}
\log Y_{it} &= \Gamma_5 W_{it} + \nu_{it} \\
\hat{\nu}_{it} &= \omega U_{it} + \epsilon_{it} \\
\tilde{Y}_{it} &= \exp(\hat{\Gamma}_5 \bar{W}_{it} + \hat{\nu}_{it}) \\
E(\tilde{Y}_i) &= \exp(\hat{\Gamma}_5 \bar{W}_{it} + \hat{\omega} U_{it})
\end{aligned} \tag{A7}$$

Remittances are similar, except modeled using a Tobit estimator. Finally, household average income (M_i) is calculated as $E(\tilde{Y}_i) + E(Remit\tilde{t}ances_i)$. Expected remittances are also added to realized income. Realized remittances above $E(\tilde{Remittances_i})$ are treated as credit, realized remittances below $E(\tilde{Remittances_i})$ are treated as savings. Household liquidity (x_{it}) is then calculated as $Savings + (R-1)Credit$.

Using the adjusted data, I then estimate the parameter vector by choosing $\hat{\theta}$ to satisfy:

$$\hat{\theta} = \arg\min_{\theta} \sum_{i=1}^{N} \sum_{t=2}^{3} (\sqrt{w_i} g_{it}(\theta, x_{it}, h_{it}, M_i, \iota_{it}, c_{it}, y_{it}))^T \sum_{i=1}^{N} \sum_{t=2}^{3} (\sqrt{w_i} g_{it}(\theta, x_{it}, h_{it}, M_i, \iota_{it}, c_{it}, y_{it})). \tag{A8}$$

Where $g_{it} = \{e_{it1}, ..., e_{it11}\}^T$ and w_i are the survey weights.

Since approximation errors can generate local minima in the objective function, I first search for an approximate global minima of equation (A8) using the Direct-L algorithm (Powell, 1998). I use that minima as the starting point of the GMM algorithm.

For both calibration and counterfactuals, the sample is restricted to households in earthquake affected districts (the colored districts in Figure 2).

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

Appendix B. Additional Figures and Tables

Table B1—Summary Statistics

Statistic	N	Mean	Std.Dev.	Min	Max
Consumption	17,713	187,444	211,564	10,838	10,788,308
Food Consumption	17,735	114,080	60,853	1,560	938,704
Income	17,735	274,691	1,833,728	196	220,557,220
Productive Assets	17,732	1,953,425	10,022,727	0	809,395,500
Home Value	17,734	1,223,625	2,222,368	0	70,000,000
Home Investment	17,718	22,931	154,749	0	10,500,000
Liquid Savings	17,651	55,732	170,411	0	5,015,000
Total Savings	17,615	255,220	728,513	0	45,434,000
Livestock Value	17,733	56,412	70,668	0	1,515,000
Loans Taken Past Year	17,732	46,204	218,239	0	10,000,000
Loans Made Past Year	17,735	2,196	26,310	0	1,500,000
Remittance Income	17,729	63,058	164,472	0	5,000,000
Household Members	17,435	4.66	1.97	1	21
Connected Migrants	17,735	0.77	1.19	0	18
Skipped Meal(%)	17,735	0.10	0.31	0	1
NGO Aid	17,735	832	15,425	0	700,000
Public Transfers	17,735	4,483	10,879	0	320,500
Informal Transfers	17,735	480	21,860	0	2,800,000
Earthquake Aid	17,735	9,351	49,450	0	2,182,000
Earthquake Aid(%)	17,735	0.13	0.37	0	1
Earthquake Losses	17,735	47,300	200,329	0	4,000,000

Notes: Household consumption includes value of all food consumption, durables, energy, utilities, rent, transportation and miscellaneous purchases. Household income is the sum of wages, rental income, agriculture and livestock sales, home food production, business revenues, capital gains, 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. Liquid savings includes cash and bank savings, insurance and savings group assets, plus loans and informal transfers made. Total savings includes liquid savings, plus investments in productive assets, jewelery, and education costs. Survey weights are used to calculate means and standard deviations. N refers to individual household-year observations in the three year panel.

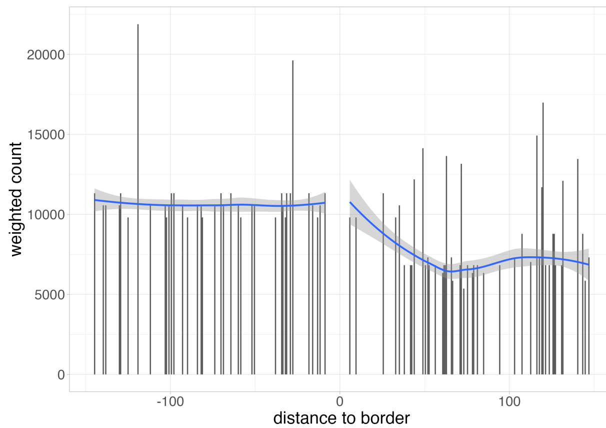

Figure B1. McCrary Test: Weighted density of households on either side of the border with smoothed means.

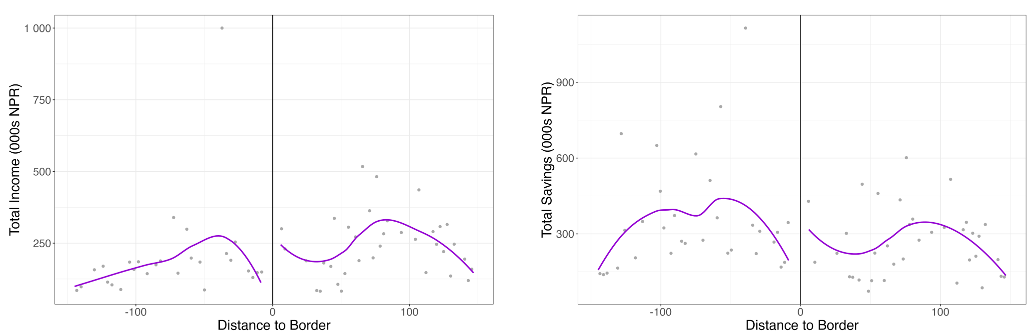

Figure B2. Binned averages of income and savings as a function of distance to the border point with Loess smoothing.

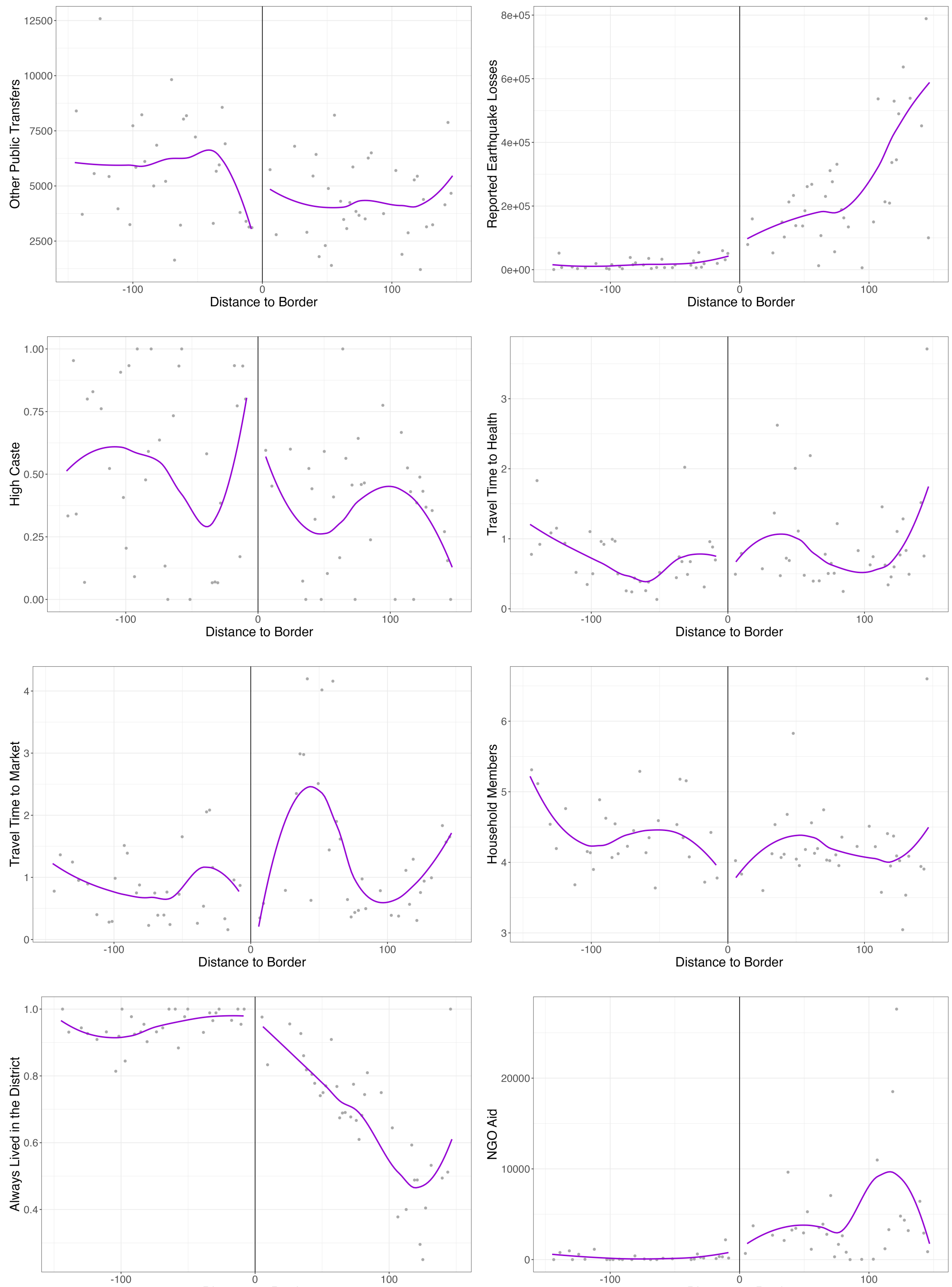

Figure B3. Binned averages of placebo variables as a function of distance to the border point with Loess smoothing.

Table B2—First Stage

	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)
Distance > 0	0.42***	32,892.07**	0.44***	32,380.65*	0.41	31,178.91	0.38***	37,398.34***	0.42***	32,892.07**
	(0.10)	(16,615.19)	(0.10)	(17,869.20)	(0.32)	(49,427.21)	(0.09)	(12,450.94)	(0.10)	(16,615.19)
N	993	565	912	348	478	478	2194	2194	993	565
Bandwidth	47.68	32.69	43.16	27.37	30	30	80	80	47.68	32.69
Kernel	triangular	triangular	epanechnikov	epanechnikov	triangular	triangular	triangular	triangular	triangular	triangular
Exclude Less affected Dists.	X	X	X	X	X	X	X	X	X	X
5 km Donut									X	X
Border	Point	Point	Point	Point	Point	Point	Point	Point	Point	Point
Wards	50	31	48	26	12	12	53	53	50	31

	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)
Distance > 0	0.41***	32,692.87**	0.24***	41,515.79***	0.40***	29,423.88***	0.78***	98,024.00***	1.41***	37,436.83***
	(0.10)	(16,572.61)	(0.08)	(11,387.34)	(0.14)	(9,600.90)	(0.17)	(32,085.64)	(0.23)	(10,568.07)
N	1120	565	1031	861	377	853	214	170	262	1337
Bandwidth	45.52	32.53	34.67	31.25	5.51	10.71	22.27	21.48	15.55	56.82
Kernel	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular	triangular
Exclude Less affected Dists.			X	X			X	X	X	X
5 km Donut										
Border	Point	Point	Eastern	Eastern	14 Dists	14 Dists	Alt Point 1	Alt Point 1	Alt Point 2	Alt Point 2
Wards	57	37	42	33	25	37	27	27	24	54

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 B3—Placebo Tests

	high caste	age hh	highest ed	hh members	class 5	class 10	always lived house	always lived dist	slope	NGO transfers	non quake aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Distance > 0	2.05	6.13	−118.03	−0.37	−0.43*	5.02	103.26	103.26	10.69	−13,821.87	7,124.28
	(36.62)	(9.76)	(1,192.37)	(1.16)	(0.25)	(69.96)	(15,089.51)	(15,089.51)	(58.41)	(15,706.73)	(4,545.50)
N	303	565	303	478	738	303	303	303	303	303	825
Bandwidth	21.75	32.83	22.16	30.12	36.22	20.64	21.12	21.12	23.61	18.03	40.73
Wards	27	39	24	34	48	28	24	24	26	18	58

	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	−3.86	53.55	96.39	−8.31	2,213.69	−2.42	−0.29	54.61	−1.22***
	(67.07)	(41.30)	(64.42)	(8.53)	(12,073.72)	(2.22)	(0.22)	(452.63)	(0.46)
N	439	542	530	795	53	303	912	303	912
Bandwidth	44.7	41.06	38.79	44.98	22.82	18.03	43.59	22.42	42.68
Wards	48	50	51	58	23	28	50	26	50

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 B4—Robustness Checks

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
With Optimal Controls:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	35,000	68,277	$-101{,}813^{***}$	$-169{,}583$	298,755	$-110{,}877^{*}$	-0.57^{***}
	(34,029)	(148,125)	(30,217)	(112,356)	(264,140)	(66,652)	(0.21)
N	565	473	303	803	710	214	738
Bandwidth	32.12	29.59	19.56	40.68	37.94	15.14	37.75
Wards	33	31	24	55	41	24	40
Logged Dependent Variables:							
	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	0.46^{*}	0.74	-10.61^{**}	-2.98	23.32	1.65^{**}	-0.49^{**}
	(0.24)	(5.00)	(5.22)	(5.24)	(87.30)	(0.69)	(0.24)
N	738	345	477	333	286	825	565
Bandwidth	36.96	26.24	29.1	25.88	20.1	39.76	32.3
Wards	43	34	36	32	26	50	42
5km Donut Hole Specification:							
	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	$52{,}833^{*}$	205,225	$-176{,}813^{**}$	121,283	312,691	430,845	-0.71^{**}
	(29,010)	(223,787)	(83,968)	(339,171)	(259,849)	(285,584)	(0.34)
N	782	473	824	333	627	433	565
Bandwidth	38.05	29.49	38.91	25.95	34.41	28.39	32.3
Wards	43	31	58	31	50	51	42
Epanechnikov Kernel:							
	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	50,697	650,221	769,474	$-24{,}916$	$-20{,}488$	$-2{,}694{,}249$	-0.69^{**}
	(32,366)	(2,435,929)	(2,389,364)	(95,245)	(352,283)	(7,121,856)	(0.31)
N	650	300	303	717	210	303	565
Bandwidth	33.62	23.72	23.86	35.47	13.28	24.73	32.3
Wards	40	28	28	49	28	46	51
Alternate Bandwidths:							
	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	48,972	194,387	$-221{,}024$	$-2{,}167$	376,170	523,119	-0.80
	(112,203)	(699,188)	(406,724)	(420,572)	(937,096)	(899,149)	(1.10)
N	478	473	477	461	458	478	478
Bandwidth	30	30	30	30	30	30	30
Wards	12	12	12	12	12	12	12
	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	46,235	207,706	$-115{,}784$	31,442	213,892	136,422	-0.32
	(31,637)	(167,082)	(97,944)	(109,801)	(356,615)	(314,624)	(0.27)
N	2194	2186	2191	2151	2136	2194	2194
Bandwidth	80	80	80	80	80	80	80
Wards	53	53	53	53	53	53	53

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. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table B5—Robustness Checks

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	Distance to Eastern Border as Running Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	83,556*	284,049	−313,512*	6,190	427,477	407,921*	−0.76
	(43,905)	(179,880)	(160,011)	(110,483)	(300,460)	(217,292)	(0.47)
N	906	1586	1286	674	713	648	1161
Bandwidth	32.41	51.5	43.12	25.96	28.4	25.46	37.85
Wards	46	42	48	29	53	56	42

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	Including ‘Slight’ and ‘None’ Districts:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	61,161	189,463	−218,344*	−9,982	259,532	−3,760,180	2.81
	(70,028)	(196,304)	(132,476)	(158,403)	(340,966)	(13,412,210)	(19.41)
N	348	473	477	461	331	303	303
Bandwidth	26.09	30.33	29.52	29.4	27.86	25.36	22.84
Wards	40	37	50	40	61	57	37

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	Distance to Entire Border - Including ‘Slight’ and ‘None’ Districts:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	76,818***	93,278*	−52,593	−41,805	109,014	−53,072	0.10
	(21,424)	(56,499)	(50,132)	(91,423)	(115,864)	(147,111)	(0.18)
N	1155	1455	1329	2173	1265	1329	1284
Bandwidth	16.67	23.56	23.26	30.82	20.24	22.78	19.04
Wards	62	65	68	73	62	65	66

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	Distance to Alternate Point 1 as Running Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	63,776*	309,449**	−91,569	13,124	−120,549	358,515*	−0.35
	(33,343)	(154,385)	(73,886)	(72,872)	(337,002)	(195,264)	(0.32)
N	562	1238	1718	1566	1816	1025	1245
Bandwidth	34.91	46.16	56.09	51.24	57.72	40.84	45.98
Wards	45	64	77	79	87	64	64

	food consumption	home investment	remittance income	liq savings	tot savings	total income	migration bin
	Distance to Alternate Point 2 as Running Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{Aid}$	68,934**	201,686	−318,976**	2,378	98,556	276,609*	−0.85**
	(32,693)	(251,482)	(153,201)	(103,566)	(178,747)	(156,344)	(0.36)
N	1035	1369	1163	630	417	479	1253
Bandwidth	43.77	54.02	50.3	34.92	28.1	29.03	52.19
Wards	71	70	71	52	75	80	75

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. ***, **, 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 on Income and Labor Supply

	log(income)			days worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Earthquake Losses	0.00004	0.0003	0.029	-0.065^{***}	-0.560^{***}	0.132
	(0.0001)	(0.0002)	(0.037)	(0.017)	(0.053)	(1.912)
IV with Shake Intensity		X	X		X	X
District Dummies			X			X
N	5,547	5,547	5,547	5,547	5,547	5,547

Notes: Estimated using first wave of household survey. Earthquake losses self reported in thousands of NPR. Households with zero or missing income dropped. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

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. Households were deemed Aid Eligible if 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. Households were deemed Aid Eligible if 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. Households were deemed Aid Eligible if 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.