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

By Matthew Gordon*

Draft: October 11, 2022

Click for most recent version.

ABSTRACT

After a natural disaster, reconstruction aid is often targeted based on property damages. Households differ in their ability to recover, however, and these differences can be difficult to observe. This creates a tension between using aid to address the visible effects of the disaster and using it to support the most vulnerable households. Here, I develop a model of households' demand for reconstruction aid to analyze alternative approaches to targeting. The model incorporates 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 use a spatial discontinuity in the distribution of reconstruction aid to test the model's predictions. Aid effects consumption, housing investment, and remittances in ways consistent with the theory. I then use the model to estimate the benefits of counterfactual aid allocations. Conditioning aid on property damages barely outperforms allocating it to households at random. An untargeted approach that divides the aid budget between all households in the affected areas yields substantially higher welfare. The results show that spending resources to target aid based on physical damages is unlikely to be cost-effective.

* Yale School of the Environment, m.gordon@yale.edu. Thanks to Resources for the Future, Eli Fenichel, Yukiko Hashida, Karen Chen, Karen Seto, Luke Sanford, Ethan Addicott, Stephanie Weber, Simon Lang, Eugene Tan, Marc Conte, Michael Peters, Mark Rosenzweig, Stephen Newbold, Thomas Walker, Nirmal Kumar Raut, Subhrendu Pattanayak, Klaas Van't Veld, Robert Gonzalez, Nathan Cook, Mark Buntaine, Ken Gillingham, and Matthew Kotchen for their valuable feedback and support.

Should the physical property damage caused by a natural disaster determine a household's eligibility for reconstruction aid? The US and international community target a significant fraction of disaster aid based on the premise that it should[1], but the relevance of using property damages as a proxy for need is not well established in economic theory or empirically. Do households that suffer property damages from a disaster have a greater need for aid than households that suffer income losses, or households that face other types of adverse shocks?

Property damage is an attractive criteria for targeting disaster aid because it is an easily observable mark of a disaster's effects. This approach to targeting may increase inequality, however, since it tends to allocate a significant amount of aid to wealthy property owners (Howell and Elliott, 2019). Other forms of vulnerability, like the inability to smooth consumption, are harder to observe, but may be more important in determining a household's ability to recover. The question of how to target disaster aid is not well studied and has important implications for the value of information after a disaster.

This paper develops a theoretical framework to assess the effects of alternative disaster aid targeting strategies on welfare and reconstruction. I apply this framework by estimating a structural model on household survey data after the 2015 earthquake in Nepal and simulating counterfactual aid allocations. The results show that making aid conditional on property damages is not much more effective than allocating it randomly, and that collecting better data on household damages to inform aid distribution is not likely to be cost-effective. In contrast, dividing the aid budget amongst all households in the affected areas would have

[1] Author's calculations from FEMA (2022) show that in the US, 84% of FEMA aid to individuals since 2010 was targeted to those who had suffered housing damages. Internationally, OCHA (2022) shows that shelter is the second largest portion of disaster aid funding (after food aid) for the three deadliest disasters of the 21st century (the 2010 Haiti Earthquake, the 2008 Myanmar Cyclone, and the 2005 Indian Ocean Earthquake/Tsunami), though this understates the fraction of cash aid for reconstruction that is earmarked for housing, since food-aid is typically short-term and in-kind.

resulted in much larger increases in welfare and consumption.

The 2015 Nepal Earthquake was the largest earthquake 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, a multilateral donor fund committed $4.1 billion towards reconstruction, with the majority of the funds designated for rural housing reconstruction. Households qualified for a 300,000 Nepali Rupee (NPR) grant – about 110% of average annual household income – on the basis of the degree of damage to their house, with damage assessments carried out by teams of engineers. Aid disbursements took place over the 3 years following the earthquake. I analyze the aid program and simulate counterfactuals using a structural model calibrated to a panel of 6,000 rural households covering that time period. To test the model's assumptions, I estimate the effects of aid on consumption, savings, and housing reconstruction using a spatial regression discontinuity approach, and show that the estimates are similar to the model's predictions.

To compare the welfare benefits of alternative targeting approaches, I develop a measure of demand for reconstruction aid that is grounded in the structural model. Demand for aid is the amount of money a household would be willing to borrow from itself in the future in order to smooth the shock in the present. 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 whether or not they receive aid. Second, some households have no property damages, but have high need for aid in order to meet their immediate need for consumption.

Property damage is thus a misleading proxy for need. If aid had been perfectly targeted at the households sustaining the largest property damages, this would have increased total welfare by 5% 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 23%. This universal allocation results in large welfare gains because households have diminishing marginal utility 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 particularly accurate.

While these results assume the purpose of aid is to help households smooth the shock, and not to help the poor, the qualitative findings are robust to alternative ways of specifying welfare and corresponding preferences for redistribution (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 resource 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), including in the context of the Nepal earthquake (Bhusal et al., 2022). This paper builds on that work by showing that even well-intentioned targeting strategies can lead to misallocation.

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; Brown, Ravallion and van de Walle, 2018; Alatas et al., 2019; Tractman, Hendra Permana and Aryo Sahadewo, 2022; Aiken et al., 2022; Haushofer et al., 2022). The objectives of disaster aid may differ, however. Disaster aid might seek to help recipients recover from a shock, regardless of how well-off they were initially. This requires developing different criteria to analyze misallocation. Because aid in this case takes the form of a cash transfer, valuation methods that measure how much money agents are willing to trade off to receive the good are not useful. Instead, I propose an alternative measure of willingness-to-pay (WTP) that is better suited to the intentions of disaster aid – estimating how much future income a household would be willing to trade off in order to receive additional liquidity in the present. My 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.

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. Using this SWF and an equal-weighted SWF that places relatively higher weight on lower-income households, I show that irrespective of the planner's preferences for redistribution, physical damages are a poor proxy for need. Giving smaller amounts of aid, but making the program universal, outperforms damage-based targeting by either measure of welfare.

In addition to the structural model, this paper contributes evidence on the causal effects of disaster aid on consumption, income, 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.

Previous work has found persistent negative effects of disasters, especially in poor countries, often using cross-country regressions (see Kellenberg and Mobarak 2011; Dell, Jones and Olken 2014; and Botzen, Deschenes and Sanders 2019 for reviews). Using a cross-country design, McDermott, Barry and Tol (2014) find that countries with more developed financial markets tend to experience smaller GDP losses after a disaster. They suggest that households with access to borrowing and saving are better able to smooth shocks. Yang (2008) finds that the combination of official development assistance and remittances offset a large fraction of disaster damages in low-income countries, however neither study is able to observe effects of the financial flows at the household-level. In the US, studies using administrative data show transitory impacts on income for even very large disasters, though recovery is aided by reliance on formal social safety nets for insurance that not be as robust in low-income countries (Deryugina, Kawano and Levitt, 2018; Deryugina, 2017). 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.

The results from the regression discontinuity show that aid increased housing investment by 16% and food consumption by 9%, but also substituted for some types of informal insurance including remittances and migration. These effects are similar to the magnitudes predicted by the structural model, giving confidence that the model is capturing how households value aid. 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.

I show that there is significant heterogeneity in household value for aid, so in theory targeting could be important. 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 second fact explains why targeting based on property damages does not correlate well with demand for aid, and thus collecting data on damages and conditioning aid on the results is unlikely to be cost-effective. This seems to suggest that the direct 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.

The remainder of the paper is structured as follows. Section I 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 II presents a behavioral model grounded in these descriptive facts, and calibrates the model to estimate household demand for aid. Section III tests the model's predictions using a spatial regression discontinuity in the allocation of aid. Section IV then uses the calibrated model to analyze the implications of counterfactual targeting strategies on welfare and reconstruction.

I. 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[2], and the economy remains heavily dependent on migrant labor and remittances, both as means of subsistence (Raut and Tanaka, 2018; Lokshin, Bontch-Osmolovski and Glinskayai, 2007), as well as in response to environmental shocks (Maystadt, Mueller and Sebastian, 2016). Nepalis endured a decade of Civil War from 1996-2006, and another 8 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 14 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

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

housing reconstruction as the largest need area by far (Government of Nepal National Planning Commission, 2015). A multilateral donor fund raised \$4.1B for household grants to rebuild earthquake resilient houses, with the grants initially targeted at the 14 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 14 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.

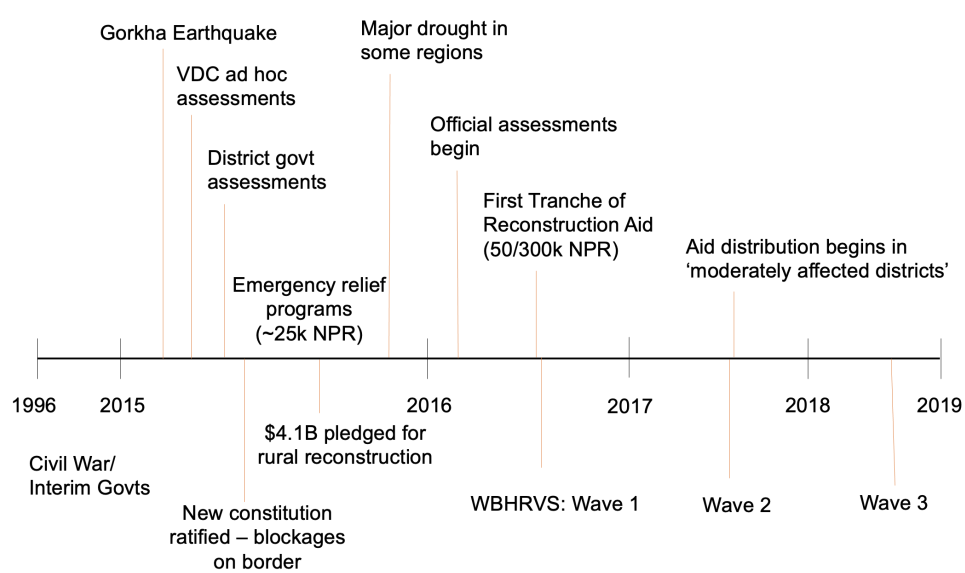

Figure 1. Timeline of Events

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, 14 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.

A. Data and Descriptive Statistics

The World Bank Household Risk and Vulnerability Survey (HRVS) is a 3-wave panel survey of 6,000 rural households from across Nepal that took place 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 recorded whether households received earthquake reconstruction aid. The locations of sample households are shown in Figure 2. I also use some geographic variables from USGS, 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.

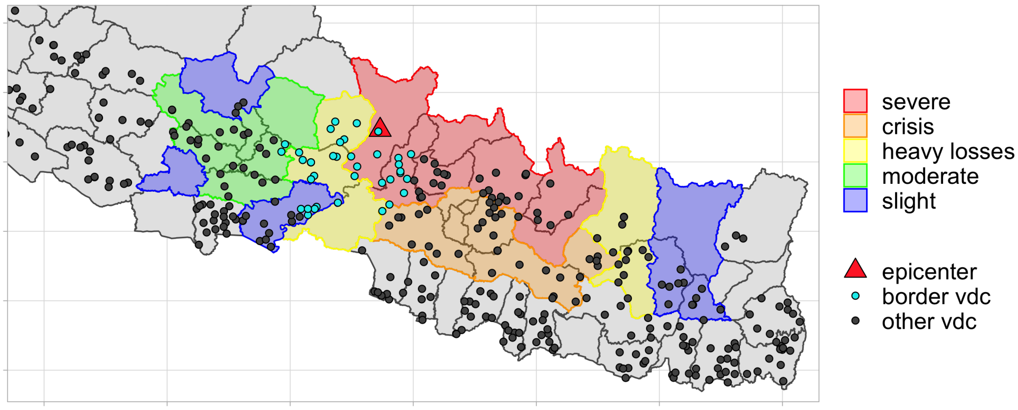

Figure 2. Map of study region

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 data. Blue circles are VDCs within 54 km of the border to the 14 most affected districts – the bandwidth selected for inference in the regression discontinuity analysis (see Section III).

Average income of the households in the sample is 270,000 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 just over $2 when including various forms of transfer income, the largest sources of which are public pensions, earthquake aid, and remittances.

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), and that mechanism appears important in this context as well, with the average household taking nearly 46,000 NPR in loans in the past year. Table 3.A shows loans to be strongly countercyclical. 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 sources of income come from migrants overseas. Thirteen percent of households received at least one installment of either emergency or reconstruction aid.

Table 1—Summary Statistics

Statistic	N	Mean	Std.Dev.	Min	Pctle.25.	Pctle.75.	Max
Consumption	17,973	187,021.36	214,090.85	10,838	106,946	213,891	10,788,308
Food Consumption	17,995	113,561.80	60,846.30	1,560	74,620	141,804	938,704
Income	17,995	267,149.16	1,821,275.55	0	49,040	252,920	220,557,220
Productive Assets	17,992	1,925,738.33	9,952,328.44	0	214,000	1,673,000	809,395,500
Home Value	17,994	1,234,676.92	2,232,907.81	0	300,000	1,200,000	70,000,000
Home Investment	17,978	21,679.01	155,016.38	0	0	0	10,500,000
Financial Assets	17,914	41,939.16	149,598.14	0	1,000	26,000	5,000,000
Loans Taken Past Year	17,992	45,881.98	217,045.17	0	0	20,000	10,000,000
Loans Made Past Year	17,991	2,239.62	28,661.61	0	0	0	1,580,000
Remittance Income	17,989	64,290.65	164,825.46	0	0	60,000	5,000,000
Household Members	17,649	4.65	1.97	1	3	6	21
Connected Migrants	17,995	0.77	1.18	0	0	1	18
Skipped Meal(%)	17,995	0.10	0.31	0	0	0	1
Earthquake Aid	17,995	9,203.66	49,113.54	0	0	0	2,182,000
Earthquake Aid(%)	17,995	0.13	0.37	0	0	0	1
NGO Aid	17,995	820.54	15,314.31	0	0	0	700,000
Public Transfers	17,995	13,113.44	62,032.99	0	0	6,000	4,344,000
Informal Transfers	17,995	489.26	21,750.34	0	0	0	2,800,000
Affected by Earthquake	17,995	0.21	0.43	0	0	0	1

Notes: Consumption includes value of all food consumption, durables, energy, utilities, rent, transportation and miscellaneous purchases. Income is the sum of wages, rent and rental income, agriculture and livestock product sales, home food production, business revenues, and any capital gains. It does not include remittances or transfers. Productive Assets are the sum of the value of land, agricultural equipment, and livestock. Survey weights are used to calculate means and standard deviations. N refers to individual household-year observations in the three year panel.

I use Townsend-style regressions to test for perfect consumption smoothing by regressing income on consumption conditional on household, and village-year fixed effects (Townsend, 1994). This tests whether idiosyncratic income shocks correlate with household consumption, suggesting an imperfect ability to consumption smooth. The results in Table 2 show statistically significant coefficients, rejecting the null hypothesis of perfect smoothing. There is important heterogeneity across several subgroups in the sample. Column 3 explores heterogeneity by caste and ethnicity. Members of the economically and politically powerful Newar group smooth consumption much 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 region. Female headed households and households with above median landholdings smooth consumption more effectively. 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 3.A. There is also a gradient with respect to elevation, with Terai (plains) households smoothing most effectively and Mountain households least. Notably, earthquake exposed households seem to smooth no worse on average, though this combines the effect of the quake with the effect of the aid.

The columns in Table 3.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. The main strategies households use to smooth consumption are remittances, loans, and migration. Each of these negatively correlate with within-household income fluctuations. However, columns (4) and (6) show that households are apparently constrained in their ability to draw on these sources of "insurance". Previous year remittances and previous year loans are negatively correlated with current year remittances and loans, conditional on the same income shock. Earthquake aid is uncorrelated with income shocks, possibly because it arrived more than a year after the event in most cases.

Table 3.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 public transfers in a given year are positively correlated with household landholdings within villages, suggesting that wealthier households are better able to take advantage of these consumption smoothing strategies.

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. The model is calibrated to households in the data using the generalized method of moments (Hansen, 1982). Section III tests the implications of the model using a spatial regression discontinuity strategy, and then Section IV considers the normative benefits of alternative targeting strategies.

Table 2—Consumption Smoothing

	log(food consumption)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(income)	0.112***	0.109***	0.114***	0.141***	0.128***	0.111***
	(0.007)	(0.009)	(0.007)	(0.013)	(0.010)	(0.007)
log(income):Dalit		0.012				
		(0.014)				
log(income):Newar		−0.049**				
		(0.017)				
log(income):Other		0.008				
		(0.009)				
log(income):Female Head			−0.013***			
			(0.002)			
log(income):Land > 50pctle				−0.019**		
				(0.007)		
log(income):Mountain					0.012	
					(0.010)	
log(income):Terai					−0.033**	
					(0.012)	
log(income):Quake Affected						0.007
						(0.015)
N	17,680	17,679	17,384	17,166	17,680	17,680
R^2	0.770	0.770	0.772	0.767	0.771	0.770

Notes: Coefficients of regression of log non transfer income on log food consumption. Households with zero or missing income or food consumption dropped. All regressions include household and village-year fixed effects. Standard errors clustered at the ward. Omitted caste category in column 2 is "Brahman/Chhetri". Omitted region category in column 5 is "Hill". Land percentile defined based on household land value during first survey wave. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table 3—Transfers

A. Within Household Variation

	Earthquake Aid	Public Transfers	NGO Transfers	Remittances	Migrants	Loans Taken
	(1)	(2)	(3)	(4)	(5)	(6)
log(income)	0.054	0.080	−0.051	−0.488***	−0.026***	−0.302
	(0.048)	(0.071)	(0.052)	(0.100)	(0.007)	(0.228)
log(remittances$_{t-1}$)				−0.263***		
				(0.023)		
log(loans$_{t-1}$)						−0.505***
						(0.023)
Fixed Effects	Household	Household	Household	Household	Household	Household
Cluster	Ward	Ward	Ward	Ward	Ward	Ward
N	17,680	17,680	17,680	11,486	17,680	11,491
R^2	0.760	0.764	0.360	0.860	0.389	0.642

B. Within Village-Year Variation

	Earthquake Aid	Public Transfers	NGO Transfers	Remittances	Migrants	Loans Taken
	(1)	(2)	(3)	(4)	(5)	(6)
land value > median	−0.003	0.488***	0.004	0.524***	0.028***	0.031
	(0.043)	(0.137)	(0.021)	(0.137)	(0.007)	(0.146)
Fixed Effects	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year	Village-Year
Cluster	Ward	Ward	Ward	Ward	Ward	Ward
N	17,166	17,166	17,166	17,161	17,166	17,163
R^2	0.696	0.152	0.637	0.149	0.135	0.195

Notes: Coefficients of regression of forms of transfer income and migration on log income or log consumption. All dependent variables in logs plus one. Households with zero or missing income or food consumption dropped. Standard errors clustered at the ward. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

II. 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 a level of 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 rate 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, however in a more stylized way it can also capture migration and remittances, if one imagines a household paying up front migration costs as a type of loan that will be repaid as remittances. Cash-on-hand 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 maximize (1) subject to (2), (3), and:

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

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

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 $B = \lambda M$.

I assume that the main effect of the earthquake is to destroy a household's housing stock, and assume income to be 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 of all, 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 cash on hand 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, allowing us to eliminate one dimension 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:

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

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 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 cash on hand. 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 points are informed by the data, however – households can borrow to smooth consumption, but not in unlimited quantities. Furthermore, when calibrated to the data, this structure will allow us to analyze the relative importance of housing investments, consumption, and savings. These tradeoffs 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 (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 is possibly more consistent with intuitions about the intentions of disaster aid, and would seem to favor a targeting strategy based on disaster exposure. The addition of more redistributive social-preferences, as in a utilitarian SWF, would favor targeting based on more direct proxies for wealth or consumption rather than housing damages. Thus, rather than take a stance on the socially optimal amount of redistribution, this specification allows me to analyze an upper-bound for the social 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:

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

This is the household's compensating variation in terms of future income.[3]

The t superscript denotes that the household's benefits will depend on the value of the state variables in period t. The household's 'willingness-to-pay' (WTP) for aid can be calculated as the discounted sum of expected payments:

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

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 set out above, however, households might be willing to pay above market interest rates (and able to repay in expectation), but unable to find a lender willing to offer them those terms. In this way, willingness-to-pay can be measured separately from ability-to-pay.

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

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

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 (II.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 $M_i - M_i^{\prime t} := \Delta M_i$. 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 aid 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.

While 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', they may be able to satisfy their reconstruction needs by drawing on savings and borrowing at the market rate R. Households with little savings, 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.

This measure of demand for aid differs from the discount rate. While the discount rate measures the amount households reduce weight on their future marginal utility of consumption, this measure of WTP incorporates differences in the marginal utility of consumption that households are able to obtain between periods, since the borrowing constraint prevents them from equating marginal utilities through time.

One potential objection to specifying social 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 our 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, we may consider this SWF to be appropriate for a policy that aims to improve consumption smoothing, rather than specifically for disaster aid.

The framework is robust to endogenizing the income process. If households could make investments to increase their incomes, this would make households close to the budget constraint have even higher values for aid, since borrowing more would allow them to increase their lifetime expected income. Thus if anything this will give lower values of aid to households near the borrowing constraint. On the other hand, there is little reason to believe that the opportunity to make profitable investments should be correlated with earthquake damages. Thus again, the results can be interpreted as an upper-bound on the value of damage-based aid targeting.

C. Model Calibration

In order to calculate household 'willingness-to-pay' I calibrate the behavioral model outlined in Section II.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 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}. Households are asked 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}. I infer values for x_{it} by noting that the total value of inter-household transfers in the model is $x_{it} - c_{it} - \iota_{it}$. I calculate these transfers as the sum of loans made, loans paid back, the value of formal savings, informal transfers, and migration expenses paid, minus new loans and loan payments, informal transfers, and remittances received. Adding food consumption and housing investment then gives the value of x_{it}, inclusive of any aid received in that period. M_i is estimated from a vector of fixed household characteristics (details in Appendix A), and the y_{it}s are then the realizations of household income in each period divided by M_i. The other state and choice variables are normalized by M_i as well. Finally, to account for life-cycle, household size, and education, 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 4, and fall within the range of normal values in the literature.

With the calibrated model in hand, I can compare the social 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 II. 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

Observed Variables:

Variable	Definition	Source
c_{it}	Consumption	Value of last week's food consumption in survey multiplied by 52.
ι_{it}	Housing Investment	Value of home repairs, improvements, and maintenence in survey.
M_i	Expected Income	Estimated from household characteristics, see Appendix A.
x_{it}	Wealth	Inferred from survey values for transfers, consumption, and investment as described in text.
h_{it}	Housing Value	Lagged estimate of resale price from survey.

Estimated Parameters:

Parameter	Estimate	Interpretation
γ	1.35	Coefficient of Risk Aversion
β	.90	Discount Factor
α	.70	Cobb-Douglass Share on Consumption
δ	.94	Depreciation Factor for Housing
R	1.11	Interest Rate (+1)
$\bar{c}$	.15	Minimum Consumption as Fraction of Expected Income
$\bar{h}$	.01	Minimum Housing as Fraction of Expected Income
λ	.05	Borrowing Constraint as Fraction of Expected Income
σ	.83	Standard Deviation of Log Income

Table 4—Estimated Parameter Values

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 or investment in productive assets.

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.

III. Empirical Evidence

Testing the above predictions is complicated due to the fact that receipt of aid is endogenous to earthquake damage, which is 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 14 most affected districts (see Figure 2). In principle, households living close to the borders of these 14 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:

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

Z_{ist} is an outcome variable for household i at time t. Aid is a variable for whether a household has received 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 single point on the border of the most affected districts in km, specified so that the slope can differ on either side of the border. X is a vector of control variables to increase precision including slope, earthquake shaking intensity, the amount of reported earthquake losses, caste, and travel time to the nearest market and health clinic.

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, though are typically between 30 and 50km. 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 log food consumption. The regression uses kernel weights 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.[4] I use heteroskedasticity robust standard errors.

Table B1 in Appendix B shows the results of the first stage regression. Households just inside the border of the most affected districts were 53% more likely to receive aid, and received an additional 30,000 NPR on average. This finding is robust to alternative choices with regard to bandwidth, kernel, a quadratic polynomial of the running variable, and a 'donut hole' specification that excludes villages within 5km of the border. Figure III illustrates the discontinuity graphi-

[4]I use triangular weights in the baseline specification and test sensitivity to other choices of kernel.

cally, by plotting the binned averages of the amount of aid received as a function of distance to the border point.

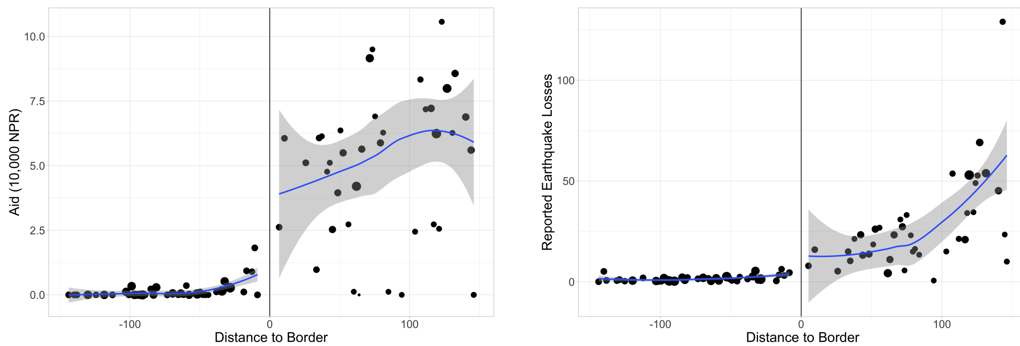

FIGURE 3. BINNED AVERAGES OF AID AND EARTHQUAKE DAMAGES AS A FUNCTION OF DISTANCE TO THE BORDER POINT.

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. Other political, institutional, and geographic factors must be a continuous function of geography at the border, 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 I. The current district borders were mostly set in the 1960s during the Panchayat Regime – a system of governance set up by the then King, 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 (Gurung, Gurung and Chidi, 2006).

Secondly, 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, however it is much more common for one or two individuals within a household to migrate and send back remittances.[5]

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 some of the control variables in X as placebos, I drop them from the right hand side. As seen in Appendix Table B2, I find no difference in age 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 do find significant differences in the fraction of high caste households, and the fraction of households claiming to have experienced the earthquake, however these results are sensitive to the bandwidth chosen, and do not appear in the plots in Appendix B. In addition, we control for caste and damages in our main specifications.

I test for travel times to the nearest bank, school, market, and healthcare clinic as a proxy for policy and public good provision, and find small differences in travel

[5] 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.

times to the nearest healthcare clinic and bank that are significant at the 10% level and go in opposite directions. These results may be spurious, but I control for both variables in the main specifications. I test for differences in the receipt of NGO aid as well and find a precise null effect. 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.

B. *Effects of Aid*

Table 5—Main Outcomes

	With Control Variables								
	food consumption	home investment	total income	productive assets	govt transfers	remittance income	loan payments	loan payments received	migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	0.09**	0.17**	0.05	0.35*	−0.26*	−0.31**	0.005	−0.01**	−0.27*
	(0.05)	(0.07)	(0.10)	(0.20)	(0.15)	(0.14)	(0.09)	(0.01)	(0.14)
N	1113	1113	942	855	942	1543	1113	556	855
Bandwidth	47.5	47.59	42.73	41.29	42.51	54.88	47.32	32.88	40.59
Wards	61	63	53	51	53	71	61	38	48
	No Controls								
	food consumption	home investment	total income	productive assets	govt transfers	remittance income	loan payments	loan payments received	migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	0.09	0.16**	0.19	0.17	−0.03	−0.31	0.05	−0.002	−0.10
	(0.08)	(0.07)	(0.12)	(0.13)	(0.09)	(0.19)	(0.14)	(0.01)	(0.07)
N	556	1201	768	855	855	768	683	556	855
Bandwidth	32.47	48.86	36.53	39.65	40.85	36.86	33.77	32.95	39.58
Wards	33	70	51	54	54	40	37	38	45
	Model Predictions: % Increase from 50k Aid								
	food consumption	home investment	total income	productive assets		borrowing/lending			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aid	0.16	0.16	0.0	0.0		−0.22			

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedastic robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Control variables include shake intensity, slope, earthquake losses, high caste, travel times to health clinic and market. Outcome variables in logs, plus one for zeros. Model predictions 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, our main specification in Table 5 shows a precisely measured effect of aid on both food consumption and home investment, and negative effects on consumption smoothing strategies including remittances, loans, migration, and government transfers, a variable that includes various small welfare programs including pensions and workfare. These results are consistent with aid partially substituting for informal insurance, but they also show that households are not completely insured, otherwise aid would have no effect on food consumption or home investment. Effects on income are null, consistent with the model's simplifying assumptions. There is an imprecisely estimated effect on productive assets, which includes farm equipment, livestock, and landholdings. Results from specifications without control variables have similar point estimates in most cases, however they are not precisely estimated. This is likely due to the curvature of the conditional expectation function close to the border discontinuity. Figure III.B shows the discontinuity in the raw data, which especially apparent for housing investment and remittances. Similar plots for additional variables appear in Appendix B.

In the third row of Table 5, I compare the findings from the regression discontinuity estimates to the model predictions by calculating counterfactual consumption, investment, and transfers responses to aid using the optimal policy and investment functions from Section II.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 50,000 NPR, equivalent to the first tranche of aid money and calculate consumption and investment again. I then calculate the percent change. The results are very close to the results from the regression discontinuity analysis.

The model apparently predicts a larger effect on consumption than I observe, but the effect on housing investment is very close. The model predictions on inter-household transfers is also close to the observed effects on remitances and public transfers. I take these simulations as suggestive evidence that the estimated policy functions are capturing something real in the heuristics households use to make decisions, and that can speak to the value households place on aid.

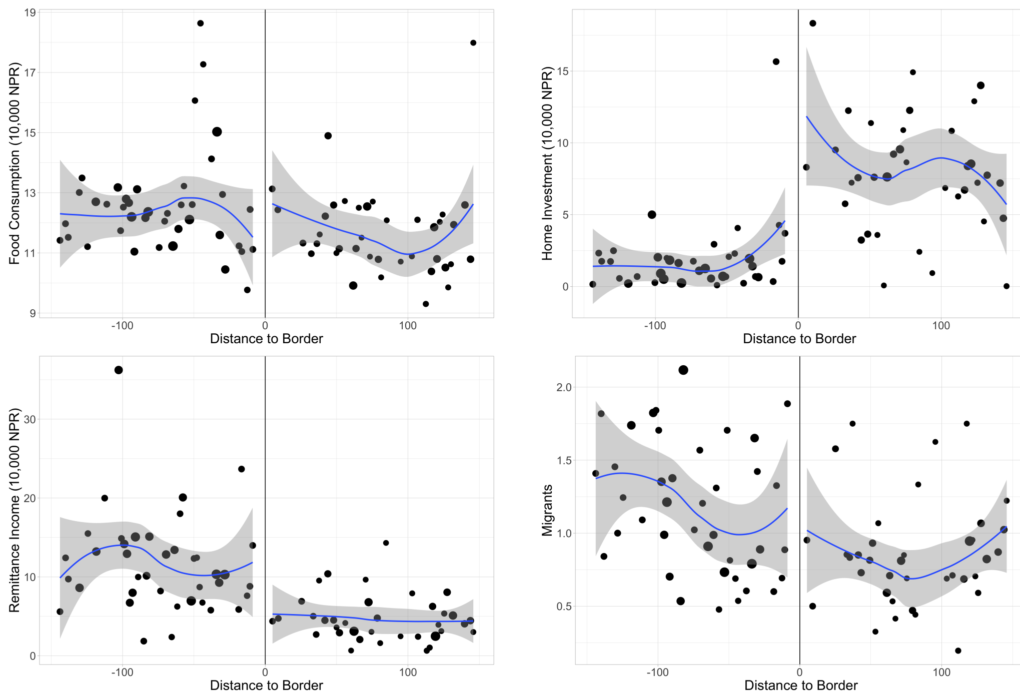

Figure 4. Binned averages of outcome variables as a function of distance to the border point.

IV. Counterfactual Reallocations

I use the model with the estimated parameters to explore counterfactual reallocations of aid, solving for each household's estimated 'willingness-to-pay' (WTP). I use the measure of WTP to assess the aggregate surplus created by different allocations, and compare these to a baseline "random allocation" scenario – as if the same amount of aid was given to a random set of households, so that the average benefits of households receiving aid is equal to the average benefits of the population. I also look at the surplus created by the actual allocation of aid, as well as the households that were targeted by NGOs. Finally, I compare the allocations using a utilitarian social welfare function that represents the preferences of a social planner that does care about redistribution.

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.

I find that the average household would give up 14% of future income in order to receive 300,000 NPR in reconstruction aid, reflecting a willingness-to-pay of 194,000 NPR, significantly less than the nominal value of the aid package. There is substantial heterogeneity, however, with a standard deviation of 156,000 NPR. The willingness to pay for aid that must be used for housing is 10% lower than unconditional aid.

The correlation between reported earthquake losses and WTP is close to zero (Pearson correlation coefficient = .01). Average WTP was also somewhat higher in the first survey wave (237,000 NPR). The decline in this measure of need could reflect both the effects of aid and natural recovery processes from the earthquake. To eliminate intertemporal comparisons which may be contaminated by these dynamics, I analyze all counterfactuals based on WTP in the first survey wave, for example analyzing the WTP of all households that eventually received aid based on their WTP in the first wave, no matter when they actually received it. I also subtract any aid received in the first wave from estimated household wealth when estimating WTP to get estimates of pre-aid WTP.

I find that the WTP of households that actually received aid was about 4% better than the random allocation. This, if anything, overstates the benefits of the actual allocation, since it is based on WTPs in the first wave, and thus doesn't take into account the delays in receiving the actual aid. I also analyze

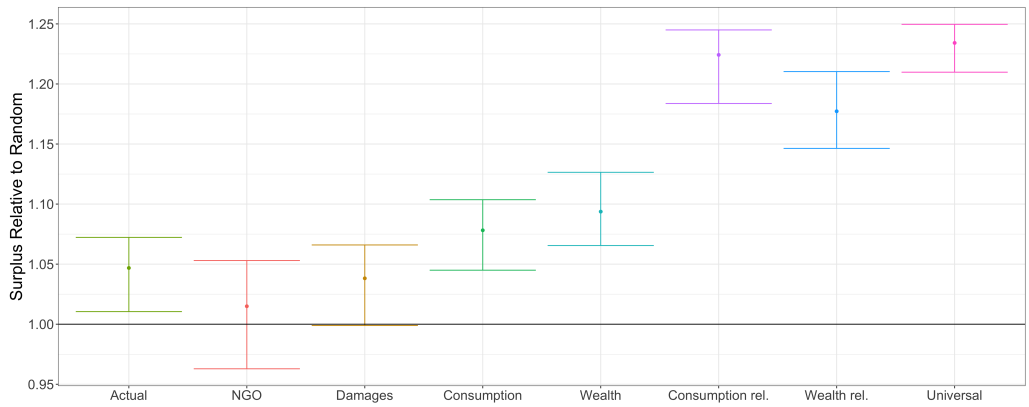

FIGURE 5. HOUSEHOLD SURPLUS FROM COUNTERFACTUAL TARGETING STRATEGIES, RELATIVE TO A RANDOM ALLOCATION (POINT ESTIMATES AND 95% CONFIDENCE INTERVALS). CONSUMPTION AND WEALTH SCENARIOS TARGET BASED ON ABSOLUTE VALUES, WHEREAS THE REL. SCENARIOS TARGET BASED ON CONSUMPTION AND WEALTH RELATIVE TO EXPECTED INCOME.

NGO allocations, finding them to be indistinguishable from random using WTP. Since WTP is increasing in expected income, I also calculate the benefits of these allocations using a utilitarian social welfare function, which should prioritize poor households. Using this social welfare function, damage based allocations are not different from random. 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 not statistically different from the random allocation.

I find that targeting absolute levels of consumption and wealth generates welfare gains of 5-10%. A utilitarian, equal-weighted social welfare function prioritizes these households by much more, however, since they have the highest marginal utility for wealth.

Much larger gains to WTP come from allocations that target based on measures of acute deprivation – especially consumption as a fraction of expected income.

Targeting based on this measure could in theory generate benefits 20-25% higher than random. 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 performs best by both aggregate WTP and utilitarian social welfare functions, reflecting the concavity of household value functions.

These results suggest that the extensive rounds of targeting and building verification did not add much value. Relief agencies would most likely have been better off 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, even if they didn't have preferences for redistribution, and solely wanted to use the aid to promote earthquake recovery. Full results comparing social welfare functions according to each scenario are in Table 6.

V. 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, as in the 2015 Nepal earthquake, and when the disaster occurs in a low-income country, where credit constraints are

Scenario	WTP	Utilitarian
Actual	1.05	0.99
	(1.01,1.07)	(0.94,1.02)
NGO	1.01	0.95
	(0.96,1.05)	(0.90,1.00)
Damage	1.04	0.95
	(1.00,1.07)	(0.90,0.98)
Consumption abs.	1.08	1.18
	(1.04,1.10)	(1.14,1.23)
Wealth abs.	1.09	1.29
	(1.07,1.13)	(1.25,1.33)
Consumption rel.	1.22	0.84
	(1.18,1.24)	(0.80,0.89)
Wealth rel.	1.17	1.16
	(1.15,1.21)	(1.12,1.20)
Universal	1.23	1.23
	(1.21,1.25)	(1.19,1.25)

Table 6—Counterfactual Targeting Scenarios: Benefits relative to Population Average for 300,000 NPR aid using different social welfare functions. Abs. refers to absolute values of consumption and wealth, rel. refers to value relative to average income. Bootstrapped 95% confidence intervals in parentheses.

more likely to bind and caste- or village-based 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.

I show evidence that households vary in their ability to smooth consumption, and that households that can borrow or receive remittances are better able to do so. Motivated by these correlations, I present and estimate a structural model of household consumption and saving linked to a framework for estimating household demand for aid.

To test the model, I estimate the effects of the reconstruction aid program on household consumption, income, investment, and transfers using a spatial regression discontinuity design, based on the fact that households in certain districts were much more likely to receive aid than households just outside those districts that received a similar level of earthquake damages. I find that receiving aid increased food consumption and housing investment, and decreased remittances and migration. When I use the structural model to simulate the effects of aid, I find that the predictions match these estimated effects reasonably 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 normative 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 for information collection.

These findings shed light on the mechanisms by which disasters and disaster aid can increase inequality, especially when targeted based on measures of disaster damages. The wealthy are twice better off in a disaster – they are slightly less likely to suffer damages, due to better housing quality, but they are also more likely to be able to smooth the shock, due to the ability to draw on loans and remittances. Aid that ignores this second factor risks wasting resources on households that don't need it.

Several factors might affect the external validity of these conclusions. First of all, an earthquake is a peculiar type of disaster, in that the main effect is usually to destroy structures, which may or may not be an important input into production. Endogenizing the income process would be more important to analyze flooding or droughts in an agricultural context, and should be considered in future research. Secondly, the aid program analyzed in this paper was for reconstruction, and was delivered more than a year after the disaster. 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 damages 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 type disaster, but more likely for recurrent disasters (Wagner, 2022).

Furthermore, as mentioned in Section I, 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 viewed as fair. One possibility for the popularity of damage-based allocations is that the criteria should be relatively transparent and easy to administer. This did not seem to be the case in Nepal, however, where disputes over the beneficiary lists led to protests and long delays in some regions (The Asia Foundation, 2016*b*). It is worth studying whether alternative allocation mechanisms, including community-based allocations or universalist approaches, could have avoided a protracted and drawn out process.

REFERENCES

Aiken, Emily, Suzanne Bellue, Dean Karlan, Chris Udry, and Joshua E. Blumenstock. 2022. "Machine learning and phone data can improve targeting of humanitarian aid." *Nature*, 603(7903): 864–870. Number: 7903 Publisher: Nature Publishing Group.

Aiyagari, S. Rao. 1994. "Uninsured Idiosyncratic Risk and Aggregate Saving." *The Quarterly Journal of Economics*, 109(3): 659–684. Publisher: Oxford University Press.

Alatas, Vivi, Abhijit Banerjee, Rema Hanna, Benjamin A. Olken, Ririn Purnamasari, and Matthew Wai-Poi. 2019. "Does Elite Capture Matter? Local Elites and Targeted Welfare Programs in Indonesia." *AEA Papers and Proceedings*, 109: 334–339.

Banerjee, Abhijit V., and Esther Duflo. 2005. "Chapter 7 Growth Theory through the Lens of Development Economics." In *Handbook of Economic Growth*. Vol. 1, 473–552. Elsevier.

Basurto, Maria Pia, Pascaline Dupas, and Jonathan Robinson. 2020. "Decentralization and efficiency of subsidy targeting: Evidence from chiefs in rural Malawi." *Journal of Public Economics*, 185: 104047.

Bhusal, Bhishma, Michael Callen, Saad Gulzar, Rohini Pande, Soledad Artiz Prillaman, and Deepak Singhania. 2022. "Does Revolution Work? Evidence from Nepal's People's War." *Working Paper*.

Botzen, W. J. Wouter, Olivier Deschenes, and Mark Sanders. 2019. "The Economic Impacts of Natural Disasters: A Review of Models and Empirical Studies." *Review of Environmental Economics and Policy*, 13(2): 167–188.

Brown, Caitlin, Martin Ravallion, and Dominique van de Walle. 2018. "A poor means test? Econometric targeting in Africa." *Journal of Development Economics*, 134: 109–124.

Calonico, Sebastian, Matias D Cattaneo, and Max H Farrell. 2020. "Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs." *The Econometrics Journal*, 23(2): 192–210.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocío Titiunik. 2017. "Rdrobust: Software for Regression-discontinuity Designs." *The Stata Journal: Promoting communications on statistics and Stata*, 17(2): 372–404.

Deaton, Angus. 1991. "Saving and Liquidity Constraints." *Econometrica*, 59(5): 1221–1248. Publisher: [Wiley, Econometric Society].

Dell, Melissa, Benjamin F. Jones, and Benjamin A. Olken. 2014. "What Do We Learn from the Weather? The New Climate-Economy Literature." *Journal of Economic Literature*, 52(3): 740–798.

Deryugina, Tatyana. 2017. "The Fiscal Cost of Hurricanes: Disaster Aid versus Social Insurance." *American Economic Journal: Economic Policy*, 9(3): 168–198. Publisher: American Economic Association.

Deryugina, Tatyana, Laura Kawano, and Steven Levitt. 2018. "The Economic Impact of Hurricane Katrina on Its Victims: Evidence from Individual Tax Returns." *American Economic Journal: Applied Economics*, 10(2): 202–233.

FEMA. 2022. "OpenFEMA Dataset: Individuals and Households Program - Valid Registrations - v1."

Francisco, Eva De. 2019. "Housing Choices and Their Implications for Consumption Heterogeneity." *International Finance Discussion Paper*, 2019(1249): 1–31.

Gablonsky, J.M., and C.T. Kelley. 2001. "A Locally-Biased form of the DIRECT Algorithm." *Journal of Global Optimization*, 21(1): 27–37.

Garrett, Thomas A., and Russell S. Sobel. 2004. "The Political Economy of FEMA Disaster Payments." In *The Encyclopedia of Public Choice.* , ed. Charles K. Rowley and Friedrich Schneider, 743–745. Boston, MA:Springer US.

Government of Nepal National Planning Commission. 2015. "Nepal Earthquake 2015 Post Disaster Needs Assessment." Kathmandu.

Gurung, Harka B., Yogendra Gurung, and Chhabi Lal Chidi. 2006. *Nepal atlas of ethnic & caste groups.* Lalitpur:National Foundation for Development of Indigenous Nationalities.

Hansen, Lars Peter. 1982. "Large Sample Properties of Generalized Method of Moments Estimators." *Econometrica*, 50(4): 1029–1054. Publisher: [Wiley, Econometric Society].

Haushofer, Johannes, Paul Niehaus, Carlos Paramo, Edward Miguel, and Michael Walker. 2022. "Targeting impact versus deprivation." 81.

Housing Recovery and Reconstruction Platform. n.d.. "Nepal, Gorkha earthquake: 18 moderately affected districts." Housing Recovery and Reconstruction Platform.

Howell, Junia, and James R Elliott. 2019. “Damages Done: The Longitudinal Impacts of Natural Hazards on Wealth Inequality in the United States.” *Social Problems*, 66(3): 448–467.

Imbens, Guido W., and Thomas Lemieux. 2008. “Regression discontinuity designs: A guide to practice.” *Journal of Econometrics*, 142(2): 615–635.

Jayachandran, Seema. 2006. “Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries.” *Journal of Political Economy*, 114(3): 538–575. Publisher: The University of Chicago Press.

Johnson, Steven G. 2022. “stevengj/nlopt.” original-date: 2013-08-27T16:59:11Z.

Jorgenson, Dale W., and Paul Schreyer. 2017. “Measuring Individual Well-Being and Social Welfare within the Framework of the System of National Accounts.” *Review of Income and Wealth*, 63(Supplement 2): S460–S477.

Judd, Kenneth L. 1998. *Numerical Methods in Economics.* Cambridge, MA, USA:MIT Press.

Kaboski, Joseph P., and Robert M. Townsend. 2011. “A Structural Evaluation of a Large-Scale Quasi-Experimental Microfinance Initiative.” *Econometrica*, 79(5): 1357–1406. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA7079.

Keele, Luke J., and Rocío Titiunik. 2015. “Geographic Boundaries as Regression Discontinuities.” *Political Analysis*, 23(1): 127–155.

Kellenberg, Derek, and A. Mushfiq Mobarak. 2011. “The Economics of Natural Disasters.” *Annual Review of Resource Economics*, 3(1): 297–312.

Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, 48(2): 281–355.

Lokshin, Michael, Mikhail Bontch-Osmolovski, and Elena Glinskayai. 2007. "Work-Related Migration and Poverty Reduction in Nepal." Social Science Research Network SSRN Scholarly Paper ID 985003, Rochester, NY.

Maystadt, Jean-François, Valerie Mueller, and Ashwini Sebastian. 2016. "Environmental Migration and Labor Markets in Nepal." *Journal of the Association of Environmental and Resource Economists*, 3(2): 417–452. Publisher: The University of Chicago Press.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics*, 142(2): 698–714.

McDermott, Thomas K.J., Frank Barry, and Richard S.J. Tol. 2014. "Disasters and development: natural disasters, credit constraints, and economic growth." *Oxford Economic Papers*, 66(3): 750–773.

Morten, Melanie. 2019. "Temporary Migration and Endogenous Risk Sharing in Village India." *Journal of Political Economy*, 127(1): 1–46. Publisher: The University of Chicago Press.

Munshi, Kaivan, and Mark Rosenzweig. 2016. "Networks and Misallocation: Insurance, Migration, and the Rural-Urban Wage Gap." *American Economic Review*, 106(1): 46–98.

Negishi, Takashi. 1960. "Welfare Economics and Existence of an Equilibrium for a Competitive Economy." *Metroeconomica*, 12(2-3): 92–97. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1467-999X.1960.tb00275.x.

Nepal Earthquake Housing Reconstruction Program Multi Donor Trust Fund. 2016. "Program Overview and Operations Manual." Nepal Earthquake Housing Reconstruction Program Multi Donor Trust Fund.

Niehaus, Paul, Antonia Atanassova, Marianne Bertrand, and Sendhil Mullainathan. 2013. "Targeting with Agents." *American Economic Journal: Economic Policy*, 5(1): 206–238.

Nordhaus, William D., and Zili Yang. 1996. "A Regional Dynamic General-Equilibrium Model of Alternative Climate-Change Strategies." *The American Economic Review*, 86(4): 741–765. Publisher: American Economic Association.

OCHA. 2022. "Financial Tracking Service."

Randell, Heather, Chengsheng Jiang, Xin-Zhong Liang, Raghu Murtugudde, and Amir Sapkota. 2021. "Food insecurity and compound environmental shocks in Nepal: Implications for a changing climate." *World Development*, 145: 105511.

Raut, Nirmal Kumar, and Ryuichi Tanaka. 2018. "Parental absence, remittances and educational investment in children left behind: Evidence from Nepal." *Review of Development Economics*, 22(4): 1642–1666. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/rode.12410.

Rust, John. 1996. "Numerical Dynamic Programming in Economics." 167.

Stanton, Elizabeth A. 2009. "Negishi Welfare Weights: The Mathematics of Global Inequality."

Strömberg, David. 2007. "Natural Disasters, Economic Development, and Humanitarian Aid." *Journal of Economic Perspectives*, 21(3): 199–222.

Tarquinio, Lisa. 2022. “The Politics of Drought Relief: Evidence from Southern India.” 73.

The Asia Foundation. 2016*a*. “Independent Impacts and Recovery Monitoring Phase 3.” 106.

The Asia Foundation. 2016*b*. “Nepal Government Distribution of Earthquake Reconstruction Cash Grants for Private Houses.”

Townsend, Robert M. 1994. “Risk and Insurance in Village India.” *Econometrica*, 62(3): 539–591.

Tractman, Carly, Yudistira Hendra Permana, and Gumilang Aryo Sahadewo. 2022. “How Much Do Our Neighbors Really Know? The Limits of Community-Based Targeting.”

Udry, Christopher. 1994. “Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria.” *The Review of Economic Studies*, 61(3): 495–526. Publisher: [Oxford University Press, Review of Economic Studies, Ltd.].

Wagle, TG Srinivasan and Swarnim, Sanjay Kathuria. 2021. “How trade spats increase poverty: the India-Nepal Episode of 2015.”

Wagner, Katherine R. H. 2022. “Adaptation and Adverse Selection in Markets for Natural Disaster Insurance.” *American Economic Journal: Economic Policy*, 14(3): 380–421.

Walker, Thomas, Yasuhiro Kawasoe, and Jui Shrestha. 2019. *Risk and Vulnerability in Nepal.* World Bank, Washington, DC.

Yang, Dean. 2008. “Coping with Disaster: The Impact of Hurricanes on International Financial Flows, 1970-2002.” *The B.E. Journal of Economic Analysis*

& Policy, 8(1). Publisher: De Gruyter Section: The B.E. Journal of Economic Analysis & Policy.

Yang, Fang. 2009. "Consumption over the life cycle: How different is housing?" *Review of Economic Dynamics*, 12(3): 423–443.

A1. *Value Function Normalization*

Define $X_{it}, H_{it}, C_{it}, I_{it}, Y_i$ as the true values of wealth, housing, consumption, 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 $log(y) = log(Y_i/M) = log(Y_i) - log(M_i) = log(Y_i) - \mu_i - \sigma^2/2 \sim N(-\sigma^2/2, \sigma^2)$.

Therefore, I want to solve for:

(A2)

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

subject to the constraints:

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

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

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 1681 points, with 40 log-spaced points for both the x and h dimensions – adding an extra set of points in each dimension at $\bar{c} - \lambda$ and $\bar{h}$ to demarcate the default region. I interpolate the value function between grid points using the multidimensional simplical scheme described in Judd (1998). I discretize the income process by exponentiating 101 Gaussian quadrature points. The large number of weights and the adjustment scheme is necessary to ensure accurate expectations near the kink in the value function. At each grid point I find the values of c and ι that maximize A1 using the DIRECT-L global optimization algorithm (Gablonsky and Kelley, 2001; Johnson, 2022). This is a semi-global optimization algorithm that helps to avoid local minima introduced by the interpolation and quadrature.

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:

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

After each maximization step, I use a 10 step modified policy iteration acceleration and McQueen-Porteus Error bound algorithms to accelerate convergence as described in Rust (1996). I then return to (A5) and repeat until the relative mean squared difference in $c^*(x, h)$ between iterations is less than 1e-2.

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 simplical interpolation scheme results in somewhat jagged policy functions, I smooth the policy functions using a tensor spline function for interpolation.

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:

$$e_{it3} = e_{it1}\log(M_i); e_{it4} = e_{it1} ihs(x_{it}); e_{it5} = e_{it1}\log(h_{it} + 1)$$

$$e_{it6} = e_{it2}\log(M_i); e_{it7} = e_{it2} ihs(x_{it}); e_{it8} = e_{it2}\log(h_{it} + 1).$$

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

We can gain two more conditions from the equations governing the evolution of the state variables:

$$e_{it9} = h_{it} - \delta(h_{it-1} + \iota_{it})$$

$$e_{it10} = x_{it} - a_{it} - R(x_{it-1} - c_{it-1} - \iota_{it-1}) - y_{it-1}.$$

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

$$e_{it11} = \log(Y_{it})^2 - \sigma^2.$$

Since the data contain variation not explicitly modelled, including life-cycle considerations and other unobserved determinants of household heterogeneity, we follow the buffer stock literature in purging these sources of variation from the estimation procedure. This requires a bit of additional consideration 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 should 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. Thus I run the following regressions:

$$
\begin{aligned}
log(c_{it}) &= \Gamma_1 W_{it} + e_{it} \\
ihs(x_{it}) &= \Gamma_2 W_{it} + e_{it} \\
h_{it} &\sim Tobit(\Gamma_3 W_{it} + e_{it}) \\
\iota_{it} &\sim Tobit(\Gamma_4 W_{it} + e_{it})
\end{aligned}
\tag{A6}
$$

Where W_{it} is a vector of exogenous 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. I then construct an adjusted dataset, where the values of consumption, income, investment, and bufferstock are the fitted values of A6 for a household with mean values of the independent variables, plus the household specific residual.

Income is treated differently, to allow for purging the same sources of heterogeneity, but also accounting for additional heterogeneity in expected income for more precise measurement of both expected income, and annual shocks. Thus the residual of the lifecycle regression is modelled as depending on an additional vector of household characteristics U_{it}, including a quadratic polynomial of land value, a cubic polynomial of household income in the first survey wave (which is excluded from the estimation), and a dummy for female headed households.

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

Then annual realized income is constructed exactly as the other variables, and expected income is constructed using the mean values of the lifecycle variables, as in the other regressions, but the household specific values of land value, wave 1 income, and household head gender.

The interpretation of household values for aid is thus slightly nuanced – it is a value for aid relative to other households with the same exogenous characteristics.

We then choose $\hat{\theta}$ to satisfy:

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

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) by generating approximately 2,000 function evaluations at random parameter combinations within a reasonable statespace.

I then fit a smoothed spline function to these values and minimize the smoothed function. I use that minima as the starting point of the GMM algorithm.

For both estimation 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. Regression Discontinuity Results and Robustness

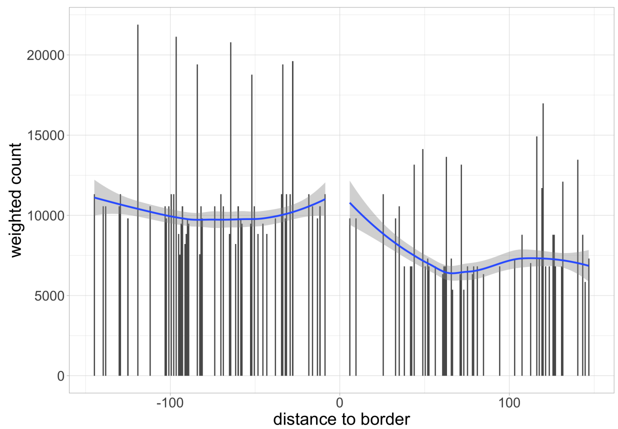

Figure B1. McCrary Test: Weighted density of households on either side of the border with smoothed means.

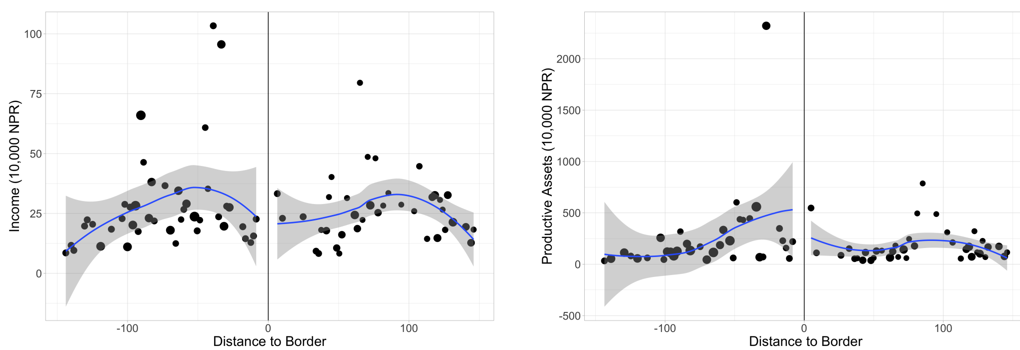

Figure B2. Binned averages of income and assets as a function of distance to the border point.

Table B1—First Stage

	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance > 0	0.53***	2.95**	0.56***	5.08***	0.56***	4.83**	0.13*	3.15***
	(0.11)	(1.41)	(0.12)	(1.58)	(0.15)	(2.11)	(0.07)	(1.07)
N	514	768	470	514	344	344	2401	2401
Bandwidth	31.75	35.3	29.16	31.74	27.07	27.07	72.18	72.18
Kernel	triangular	triangular	epanechnikov	epanechnikov	triangular	triangular	triangular	triangular
5 km Donut								
Geography Controls	X	X	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X	X	X
Wards	32	42	30	37	21	21	89	89

	Received Aid	Aid Amount	Received Aid	Aid Amount	Received Aid	Aid Amount
	(1)	(2)	(3)	(4)	(5)	(6)
Distance > 0	0.60***	3.48**	0.57***	2.76*	0.19	2.93**
	(0.12)	(1.47)	(0.12)	(1.45)	(0.13)	(1.41)
N	470	726	470	768	344	768
Bandwidth	29.27	34.82	29.75	35.44	27.29	37
Kernel	triangular	triangular	triangular	triangular	triangular	triangular
5 km Donut	X	X				
Geography Controls	X	X	X	X		
Demographic Controls	X	X				
Wards	32	42	30	37	21	21

Notes: Aid amount in tens of thousands. Local linear regressions with triangular kernel x survey weights, heteroskedastic robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Control variables include shake intensity, slope, earthquake losses, high caste, travel times to health clinic and market. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Appendix C. Determinants of Earthquake Damages

The following are the results of regressions run on the census of household building damage in 11 districts conducted after the earthquake. The dependent variable is whether the building damage was rated as a 4 or a 5, which was the official criteria for whether a household was eligible to receive aid. All regressions contain a village dummy to control for earthquake intensity.

Table B2—Placebo Tests

	elevation	quake affected	high caste	age hh	highest ed	hh members	always lived house	always lived dist	NGO transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	2.62	0.14**	−0.19**	−0.16	0.47	0.01	−0.001	−0.001	−0.03
	(12.49)	(0.06)	(0.08)	(1.60)	(0.52)	(0.11)	(0.02)	(0.02)	(0.05)
N	855	1543	1543	855	855	1543	1543	1543	983
Bandwidth	41.36	55.23	54.1	41.25	40.56	53.94	53.41	53.41	43.16
Wards	74	72	72	51	49	73	70	70	54
	chicken price	rice price	lentil price	sugar price	mutton price	time to bank	time to school	time to health	time to market
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	1.40	9.58	−29.59	0.75	22.21	−0.57*	0.02	0.18*	−0.15
	(3.55)	(17.55)	(67.80)	(1.01)	(23.31)	(0.33)	(0.02)	(0.10)	(0.10)
N	585	501	655	835	82	768	855	1068	942
Bandwidth	51.09	37.07	43.23	44.08	32.92	36.41	40.22	44.59	42.16
Wards	59	53	58	59	38	53	53	60	60

Notes: Local linear regressions with triangular kernel x survey weights, heteroskedastic robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Control variables include shake intensity, slope, and earthquake losses. Other controls excluded to put on left hand side. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table B3—Robustness Checks

	food consumption	home investment	total income	productive assets	pub transfers	remittance income	loan payments	loan payments received	n migrants
Outcomes in Levels:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	1.31	3.01*	7.53	168.52	−2.38*	−3.76	0.97	−0.02	−0.57**
	(0.82)	(1.74)	(5.79)	(141.48)	(1.43)	(2.40)	(0.97)	(0.03)	(0.28)
N	768	1543	1113	942	768	768	1237	1113	1068
Bandwidth	36.09	52.59	46.06	43.15	37.72	37.15	49.08	46.05	44.93
Wards	37	70	61	59	53	38	67	66	60
Epanechnikov Kernel:									
	food consumption	home investment	total income	productive assets	pub transfers	remittance income	loan payments	loan payments received	n migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	0.06**	0.22*	−0.10	0.45***	−0.27	−0.83	0.03	−0.02	−0.47
	(0.03)	(0.13)	(0.09)	(0.17)	(0.22)	(0.59)	(0.15)	(0.02)	(0.33)
N	1113	855	556	1068	855	855	855	855	855
Bandwidth	48.19	39.46	32.21	45.1	39.4	39.41	39.4	39.45	39.35
Wards	70	60	41	68	61	63	61	63	61
Alternate Bandwidths:									
	food consumption	home investment	total income	productive assets	pub transfers	remittance income	loan payments	loan payments received	n migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	0.06	0.12	−0.07	−0.11	−0.28	−0.19	0.20	−0.01	−0.18
	(0.08)	(0.16)	(0.21)	(0.24)	(0.31)	(0.27)	(0.25)	(0.02)	(0.19)
N	470	470	470	470	470	470	470	470	470
Bandwidth	30	30	30	30	30	30	30	30	30
Wards	12	12	12	12	12	12	12	12	12
	food consumption	home investment	total income	productive assets	pub transfers	remittance income	loan payments	loan payments received	n migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{Aid}$	0.04	0.22***	0.04	0.09	−0.01	−0.20	−0.05	−0.004	−0.04
	(0.03)	(0.09)	(0.09)	(0.11)	(0.08)	(0.13)	(0.10)	(0.01)	(0.04)
N	2612	2612	2612	2612	2612	2612	2612	2612	2612
Bandwidth	80	80	80	80	80	80	80	80	80
Wards	63	63	63	63	63	63	63	63	63

Notes: Aid amount in tens of thousands. Local linear regressions with triangular kernel x survey weights, heteroskedastic robust standard errors. Slope of running variable allowed to differ on either side of cutoff. Control variables include shake intensity, slope, earthquake losses, high caste, travel times to health clinic and market. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

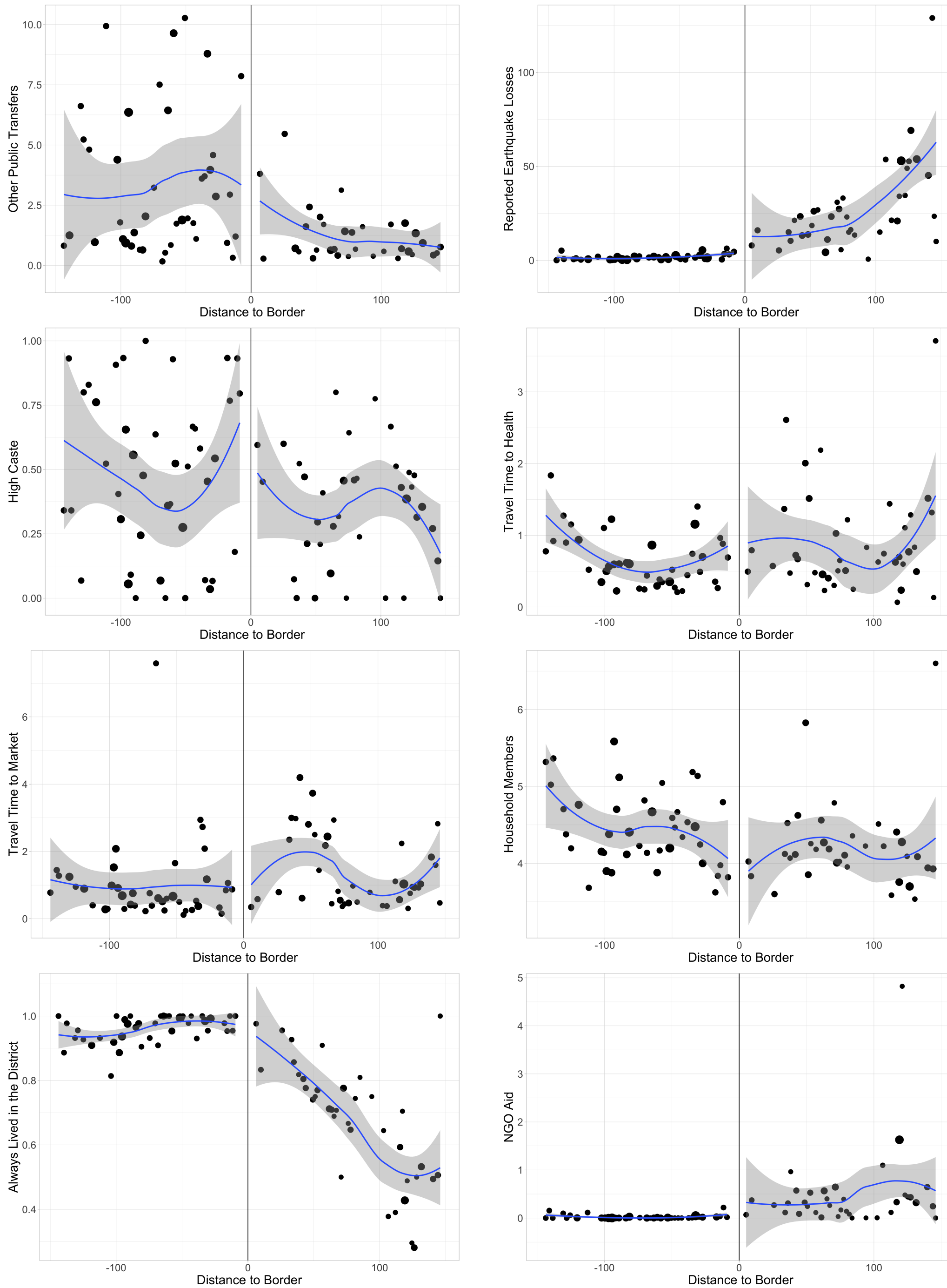

Figure B3. Binned averages of additional outcome and placebo variables as a function of distance to the border point.

Table C1—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: In columns 1 and 2, Bamboo/Timber is the excluded category. All regressions contain village fixed effects. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table C2—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. 10-20 thousand				−0.047***	
				(0.001)	
Income: Rs. 20-30 thousand				−0.085***	
				(0.002)	
Income: Rs. 30-50 thousand				−0.102***	
				(0.003)	
Income: Rs. 50 thousand +				−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: In column 4, less than 10 thousand is the excluded category. All regressions contain village fixed effects. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.

Table C3—Household Demographics

	Aid Eligible				
	(1)	(2)	(3)	(4)	(5)
Migrant HH	-0.002^{**}				
	(0.001)				
Receives Social Security		0.026^{***}			
		(0.001)			
Male Head Household			0.036^{***}		
			(0.001)		
Finished High School				-0.059^{***}	
				(0.002)	
Some Middle/High				-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: In column 5, Brahman/High Caste is the excluded category, in column 4, no education is the excluded category. All regressions contain village fixed effects. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels respectively.