

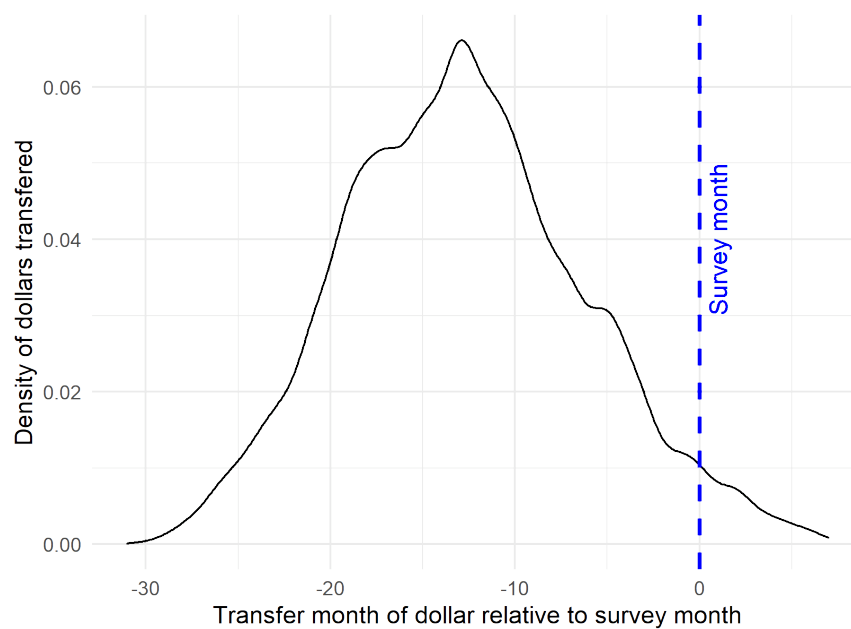
Targeting Impact Versus Deprivation

Online Appendix

By Johannes Haushofer, Paul Niehaus, Carlos Paramo, Edward Miguel, and Michael Walker

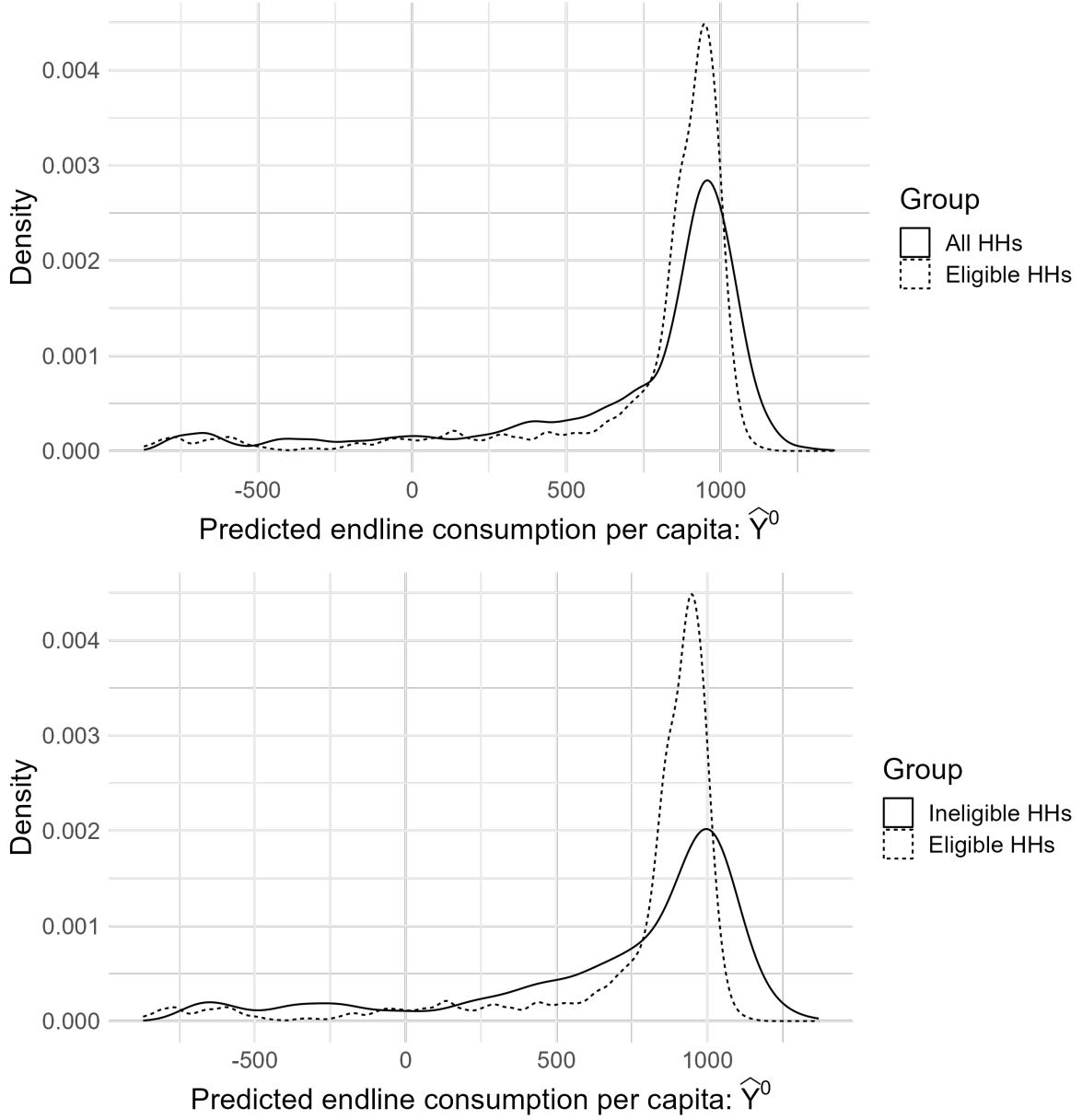
A Appendix exhibits

Figure A.1: Transfer date relative to endline survey time



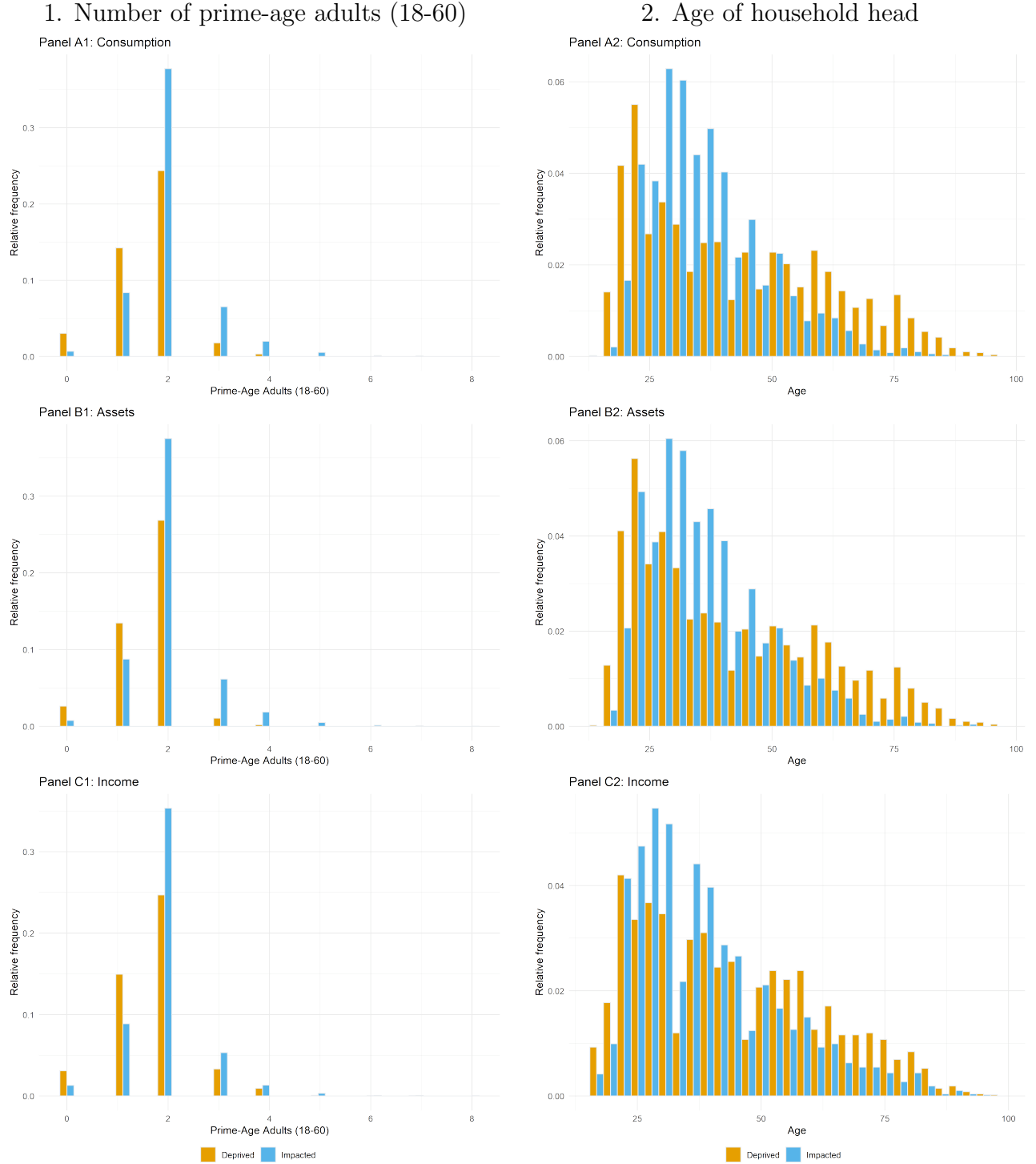
Notes: This figure plots the density of dollars transferred relative to the month when a household completed the endline survey (conducted by the research team) for treated households matched to treatment timing data from the NGO *GiveDirectly*.

Figure A.2: Kernel densities of predicted endline consumption



Notes: The top figure compares the kernel density of endline consumption predictions using GRF when models are trained with all household data against the case when models are trained with eligible households data, the latter being the one used for our main results. The bottom figure compares the density of endline consumption predictions using GRF when models are trained with ineligible household data against the case when models are trained with eligible households data. We present the distributions of model predictions trained on the relevant subsets of the data in order to have conservative estimates of these distributions (rather than training a single model with all the available data). Monetary values are in USD PPP (2016).

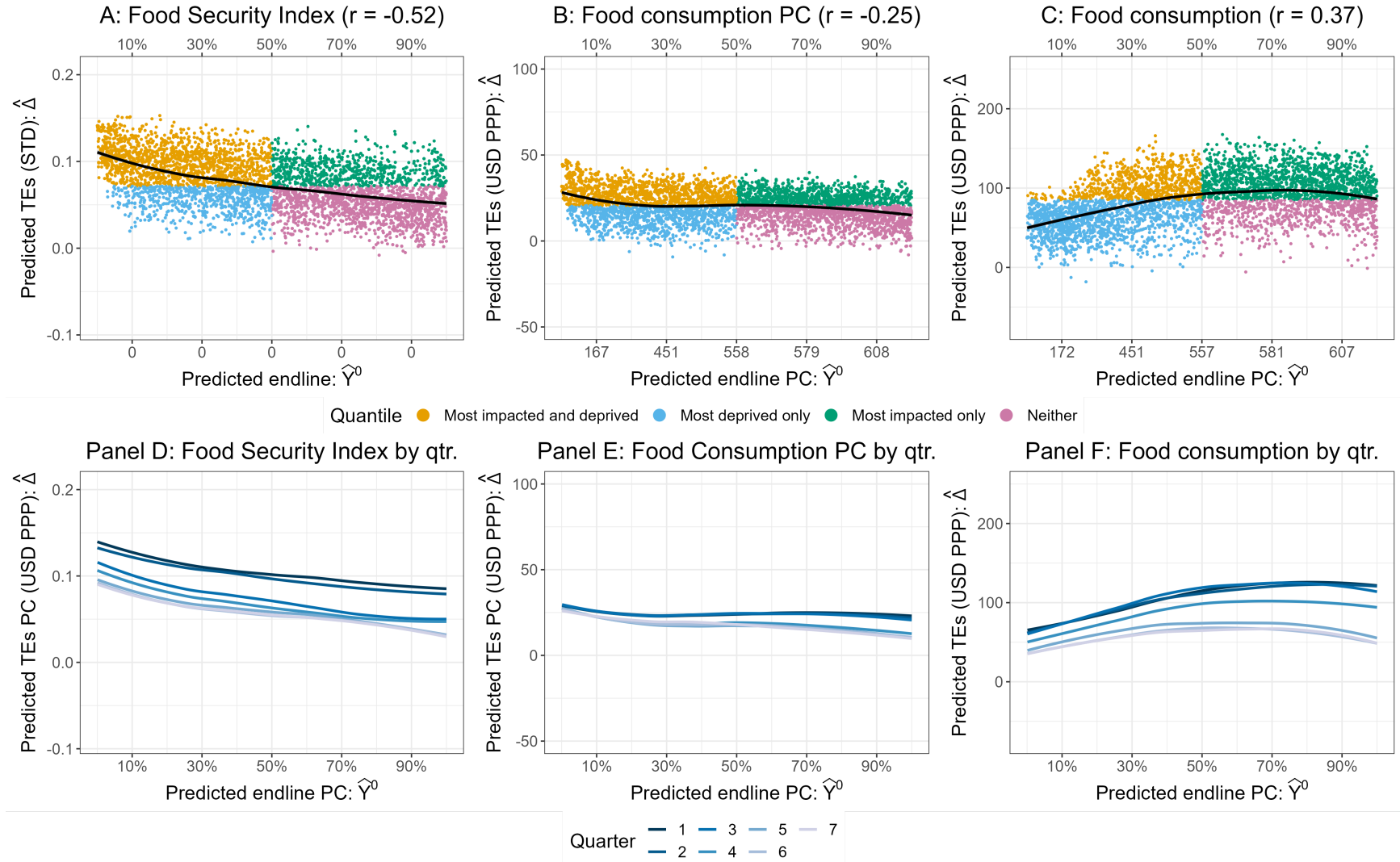
Figure A.3: Demographic characteristics in the most impacted and deprived households



Notes: This figure plots the baseline distribution of two key demographic characteristics – the number of prime-age adults in a household (Column 1), and the age of the household head (Column 2), by classification into the most deprived and most impacted groups from the median model in terms of the difference in average treatment effects between the most deprived and most impacted groups for each outcome (as used in Figure 1).

Figure A.4: Predicted treatment effects ($\hat{\Delta}_h$) and untreated per capita values ($\hat{y}_h^0$) for food security

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Notes: This figure demonstrates that the pre-specified food security index appears to behave more similarly to a per-capita measure than a household measure. It follows the format of Figure 1: panels a, b and c plot scatterplots of predicted treatment effects for an outcome (y-axis) against the predicted endline values (x-axis) for that same outcome with a local regression line. Panels d, e and f plot the regression lines for each quarter after treatment. Both predicted endline and predicted treatment effects are estimated from generalized random forest models with the same set of covariates. Predicted endline values are from models trained on time-demeaned data; a constant was added to the reported statistics so that the overall predicted mean matches the observed sample mean. Panel A is an index of food security measures. Monetary values are in USD PPP (2016) for panels B and C.

Figure A.5: Cross-outcome relationships in predicted treatment effects with observable assets



Notes: This figure reproduces Figure 4, but replaces our main (total) assets measure by an observable assets measure. Monetary values are in USD PPP (2016).

Table A.1: Overlap of socially optimal households to target with most deprived and most impacted

	(1)	(2)	(3)
CARA: α	CE	Most deprived	Most impacted
<i>Panel A: Consumption</i>			
0.0000	\$50	0.26	1.00
0.0005	\$49	0.43	0.79
0.0010	\$49	0.54	0.68
0.0075	\$41	0.91	0.35
0.0150	\$33	0.96	0.29
<i>Panel B: Assets</i>			
0.0000	\$50	0.26	1.00
0.0005	\$49	0.49	0.74
0.0010	\$49	0.63	0.61
0.0075	\$41	0.92	0.34
0.0150	\$33	0.96	0.30
<i>Panel C: Income</i>			
0.0000	\$50	0.45	1.00
0.0005	\$49	0.51	0.94
0.0010	\$49	0.54	0.89
0.0075	\$41	0.74	0.70
0.0150	\$33	0.81	0.63

Notes: This table reproduces Table 1, panel A, but also for additional outcomes (Assets and Income). Column 1 denotes the certainty equivalent (CE) of a 50-50 lottery over \$0 or \$100 under the specified CARA α parameter value. Column 2 (3) reports the share of households belonging to D (I) that are also “socially optimal” for a planner to treat. Socially optimal households are those in the top 50% of households ranked by potential gains from treatment using a CARA utility function for the risk aversion parameter (α) given in the row label. Reported shares are the mean of 150 5-fold GRF iterations; median ratios are similar (not shown).

Table A.2: Overlap of socially optimal households with most deprived and most impacted using CRRA utility

	(1)	(2)
CRRA: ρ	Most deprived	Most impacted
<i>Panel A: Consumption</i>		
0.0000	0.26	1.00
0.5000	0.36	0.87
1.0000	0.43	0.78
2.0000	0.53	0.68
4.0000	0.64	0.59
<i>Panel B: Income</i>		
0.0000	0.45	1.00
0.5000	0.51	0.93
1.0000	0.54	0.90
2.0000	0.59	0.85
4.0000	0.65	0.79

Notes: This table reproduces the social welfare analysis of Table 1, but using constant relative risk aversion (CRRA) utility. Since CRRA is not defined for negative values, we shift the distribution of $Y(0)$ by a constant such that the minimum value is 200 USD PPP (2016) per capita, which is approximately 1/4 of the extreme poverty line. Column 1 (2) reports the share of households belonging to I (D) that are also “socially optimal” for a planner to treat. Socially optimal households are those in the top 50% of households ranked by potential gains from treatment using a CRRA utility function for the risk aversion parameter (ρ) given in the row label. Reported shares are the mean of 150 5-fold GRF iterations; median ratios are similar (not shown).

Table A.3: Overlap of socially optimal households to target with most deprived and most impacted for observable assets

	(1)	(2)	(3)
CARA: α	CE	Most deprived	Most impacted
<i>Panel A: All assets</i>			
0.0000	\$50	0.26	1.00
0.0005	\$49	0.49	0.74
0.0010	\$49	0.63	0.61
0.0075	\$41	0.92	0.34
0.0150	\$33	0.96	0.30
<i>Panel B: Observable assets</i>			
0.0000	\$50	0.34	1.00
0.0005	\$49	0.50	0.81
0.0010	\$49	0.59	0.72
0.0075	\$41	0.89	0.44
0.0150	\$33	0.94	0.39

Notes: This table reproduces Table 1 for all assets (referred to as *assets* elsewhere in the paper, Panel A) and for observable assets (those that an enumerator would have been able to observe during the survey, Panel B)

Table A.4: Robustness check: only observable assets

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: All Assets</i>										
Predicted	232	85	128	343	-258	189	178	195	207	-29
Actual	213	53	96	336	-283	182	154	167	188	-34
					[-313,-253]					[-123,58]
<i>Panel B: Observable assets</i>										
Predicted	165	45	92	232	-187	128	120	137	142	-22
Actual	150	25	67	224	-198	121	111	120	123	-12
					[-222,-173]					[-82,75]

Note: This table reproduces Table 2 for all assets (referred as *assets* elsewhere in the paper, Panel A) and observable assets (those that an enumerator would have been able to observe during the survey, Panel B). We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. For additional details refer to Table 2. Monetary values are in USD PPP (2016).

Table A.5: Differences in characteristics and treatment effects by group classification using consumption

	(1) All	(2) D	(3) D^C	(4) $D - D^C$	(5) SO	(6) SO^C	(7) $SO - SO^C$	(8) I	(9) I^C	(10) $I - I^C$	(11) $D - I$
<i>Panel A: Baseline demographics, Mean (SD)</i>											
Household size	4.40 (2.21)	2.99 (1.57)	5.82 (1.80)	-2.83 (0.05)	4.73 (2.71)	4.06 (1.45)	0.67 (0.07)	6.04 (1.63)	2.74 (1.27)	3.30 (0.05)	-3.05 (0.04)
Number of prime-age adults in household	1.88 (0.77)	1.59 (0.71)	2.11 (0.72)	-0.52 (0.03)	1.88 (0.93)	1.89 (0.60)	-0.01 (0.03)	2.07 (0.78)	1.64 (0.67)	0.44 (0.03)	-0.50 (0.02)
Number of children in household	2.46 (1.88)	1.34 (1.37)	3.59 (1.64)	-2.26 (0.05)	2.85 (2.23)	2.06 (1.34)	0.79 (0.06)	3.84 (1.48)	1.07 (1.04)	2.76 (0.04)	-2.50 (0.03)
Education level of household head	6.48 (3.72)	5.52 (3.93)	7.45 (3.23)	-1.92 (0.12)	5.50 (3.86)	7.49 (3.28)	-1.99 (0.12)	6.98 (3.27)	5.97 (4.07)	1.01 (0.12)	-1.47 (0.10)
Age of household head	42.37 (15.47)	45.32 (18.10)	39.39 (11.52)	5.93 (0.50)	47.54 (16.18)	37.00 (12.63)	10.54 (0.47)	40.42 (12.05)	44.34 (18.10)	-3.92 (0.52)	4.94 (0.45)
Female household head	0.26 (0.44)	0.43 (0.50)	0.09 (0.28)	0.34 (0.01)	0.38 (0.49)	0.13 (0.34)	0.25 (0.01)	0.20 (0.40)	0.32 (0.46)	-0.11 (0.01)	0.23 (0.01)
Widow	0.18 (0.39)	0.36 (0.48)	0.01 (0.08)	0.35 (0.01)	0.32 (0.47)	0.05 (0.22)	0.26 (0.01)	0.12 (0.33)	0.25 (0.43)	-0.13 (0.01)	0.24 (0.01)
Household owns any livestock	0.27 (0.44)	0.18 (0.38)	0.35 (0.48)	-0.18 (0.01)	0.33 (0.47)	0.20 (0.40)	0.13 (0.01)	0.37 (0.48)	0.16 (0.36)	0.22 (0.01)	-0.20 (0.01)
Household owns land	0.84 (0.36)	0.84 (0.37)	0.85 (0.36)	-0.01 (0.01)	0.92 (0.28)	0.77 (0.42)	0.15 (0.01)	0.88 (0.32)	0.80 (0.40)	0.08 (0.01)	-0.04 (0.01)
Respondent self-employed	0.27 (0.44)	0.20 (0.40)	0.34 (0.47)	-0.14 (0.01)	0.23 (0.42)	0.31 (0.46)	-0.08 (0.01)	0.29 (0.45)	0.25 (0.43)	0.04 (0.01)	-0.09 (0.01)
Respondent employed	0.33 (0.47)	0.32 (0.46)	0.35 (0.48)	-0.03 (0.02)	0.36 (0.48)	0.30 (0.46)	0.07 (0.01)	0.37 (0.48)	0.29 (0.45)	0.08 (0.02)	-0.06 (0.01)
Number of meals eaten yesterday	2.29 (0.68)	2.22 (0.69)	2.37 (0.66)	-0.16 (0.02)	2.11 (0.67)	2.48 (0.64)	-0.37 (0.02)	2.23 (0.66)	2.36 (0.69)	-0.13 (0.02)	-0.01 (0.02)
Received any loan	0.54 (0.50)	0.46 (0.50)	0.61 (0.49)	-0.15 (0.02)	0.53 (0.50)	0.54 (0.50)	-0.01 (0.02)	0.62 (0.48)	0.45 (0.50)	0.17 (0.02)	-0.16 (0.01)
Any loan denied	0.24 (0.43)	0.22 (0.41)	0.26 (0.44)	-0.05 (0.01)	0.24 (0.43)	0.24 (0.43)	-0.00 (0.01)	0.27 (0.44)	0.22 (0.41)	0.05 (0.01)	-0.05 (0.01)
<i>Panel B: Endline Treatment Effects (SEs)</i>											
Respondent hours worked last week	1.05 (0.98)	1.90 (1.32)	-0.30 (1.34)	2.20 (1.83)	-0.37 (1.29)	2.50 (1.45)	-2.87 (1.91)	-0.24 (1.28)	2.23 (1.38)	-2.47 (1.78)	2.14 (1.56)
Indicator for household self-employed	0.04 (0.02)	0.04 (0.02)	0.03 (0.02)	0.01 (0.03)	0.02 (0.02)	0.05 (0.02)	-0.02 (0.03)	0.06 (0.02)	0.01 (0.02)	0.05 (0.03)	-0.02 (0.03)
Indicator for household employed	-0.03 (0.02)	-0.04 (0.02)	-0.02 (0.02)	-0.01 (0.03)	-0.02 (0.02)	-0.04 (0.02)	0.02 (0.03)	-0.05 (0.02)	-0.01 (0.02)	-0.05 (0.03)	0.01 (0.03)
Interhousehold transfers received	9.85 (7.13)	-4.82 (9.51)	24.22 (9.81)	-29.04 (13.13)	20.23 (10.10)	-0.75 (9.22)	20.98 (13.14)	28.77 (10.44)	-9.45 (8.94)	38.22 (13.29)	-33.59 (11.71)
Interhousehold transfers sent	7.96 (2.75)	5.39 (3.53)	9.51 (4.16)	-4.12 (5.48)	10.72 (3.75)	5.27 (4.10)	5.44 (5.62)	9.38 (4.11)	6.39 (3.89)	2.99 (5.81)	-3.99 (4.72)
Total value of loans taken in last 12 months	3.48 (4.03)	-5.70 (4.81)	10.84 (6.74)	-16.54 (8.45)	5.96 (5.01)	1.13 (6.61)	4.83 (8.57)	8.03 (6.12)	-1.56 (5.54)	9.59 (8.39)	-13.73 (6.94)
Total value of loans given in last 12 months	3.43 (0.79)	4.04 (0.99)	2.51 (1.29)	1.53 (1.65)	2.66 (0.91)	4.25 (1.31)	-1.59 (1.62)	2.70 (1.13)	4.14 (1.18)	-1.44 (1.68)	1.34 (1.39)
Number of household members	0.18 (0.07)	0.13 (0.09)	0.11 (0.09)	0.02 (0.12)	0.24 (0.12)	0.12 (0.08)	0.11 (0.14)	0.13 (0.09)	0.18 (0.08)	-0.05 (0.11)	0.00 (0.11)

Notes: This table presents differences in baseline characteristics (Panel A) and treatment effects estimated via OLS (Panel B) based on households' classification into most deprived (D), socially optimal (SO), or most impacted (I) groups and their complements (denoted by ^{C}) across our 150 models. Specifically, households are assigned to a group (e.g. the most deprived) if they are classified in that group in more than 50% of the 150 models. In Panel A, we report group means and standard deviations (columns 1, 2, 3, 5, 6, 8 and 9) and test for differences between groups using OLS, group indicators and their interaction (columns 4, 7, 10 and 11). In Panel B, column 1 reports the coefficient (and standard error) from a regression on treatment status and a constant, with standard errors are clustered at the village level. Columns 2–4 present group ATEs from a regression on treatment status, deprivation status, and the interaction between the two; columns 5–7 do the same by socially optimal status, and columns 8–10 to most impacted status. Column 11 presents the difference in treatment effects for the most deprived and most impacted; standard errors for this difference are calculated by bootstrapping the regressions in columns 2-4 and 8-10 999 times.

Table A.6: Differences in characteristics and treatment effects by group classification using assets

	(1) All	(2) D	(3) D^C	(4) $D - D^C$	(5) SO	(6) SO^C	(7) $SO - SO^C$	(8) I	(9) I^C	(10) $I - I^C$	(11) $D - I$
<i>Panel A: Baseline demographics, Mean (SD)</i>											
Household size	4.40 (2.21)	2.88 (1.40)	5.91 (1.77)	-3.04 (0.05)	4.00 (2.53)	4.77 (1.76)	-0.78 (0.07)	5.72 (1.93)	3.06 (1.58)	2.65 (0.06)	-2.82 (0.04)
Number of prime-age adults in household	1.88 (0.77)	1.60 (0.66)	2.10 (0.77)	-0.50 (0.02)	1.79 (0.81)	1.95 (0.72)	-0.15 (0.03)	2.05 (0.77)	1.68 (0.70)	0.37 (0.03)	-0.44 (0.02)
Number of children in household	2.46 (1.88)	1.25 (1.22)	3.66 (1.64)	-2.41 (0.05)	2.24 (2.13)	2.67 (1.59)	-0.43 (0.06)	3.55 (1.72)	1.35 (1.30)	2.20 (0.05)	-2.29 (0.04)
Education level of household head	6.48 (3.72)	5.77 (4.00)	7.18 (3.28)	-1.41 (0.12)	5.57 (3.83)	7.33 (3.40)	-1.75 (0.12)	6.89 (3.32)	6.06 (4.04)	0.83 (0.11)	-1.11 (0.10)
Age of household head	42.37 (15.47)	44.03 (17.98)	40.73 (12.30)	3.30 (0.50)	45.78 (16.56)	39.20 (13.65)	6.58 (0.52)	40.32 (12.19)	44.43 (17.96)	-4.11 (0.51)	3.68 (0.45)
Female household head	0.26 (0.44)	0.37 (0.48)	0.14 (0.35)	0.23 (0.01)	0.37 (0.48)	0.16 (0.36)	0.21 (0.01)	0.20 (0.40)	0.32 (0.47)	-0.12 (0.01)	0.17 (0.01)
Widow	0.18 (0.39)	0.31 (0.46)	0.06 (0.24)	0.25 (0.01)	0.30 (0.46)	0.08 (0.27)	0.22 (0.01)	0.12 (0.32)	0.26 (0.44)	-0.14 (0.01)	0.19 (0.01)
Household owns any livestock	0.27 (0.44)	0.07 (0.26)	0.46 (0.50)	-0.39 (0.01)	0.16 (0.37)	0.37 (0.48)	-0.21 (0.01)	0.41 (0.49)	0.12 (0.32)	0.29 (0.01)	-0.34 (0.01)
Household owns land	0.84 (0.36)	0.82 (0.38)	0.86 (0.35)	-0.04 (0.01)	0.87 (0.34)	0.82 (0.39)	0.05 (0.01)	0.86 (0.35)	0.83 (0.38)	0.03 (0.01)	-0.03 (0.01)
Respondent self-employed	0.27 (0.44)	0.22 (0.41)	0.32 (0.47)	-0.10 (0.01)	0.32 (0.47)	0.22 (0.41)	0.10 (0.01)	0.37 (0.48)	0.16 (0.37)	0.21 (0.01)	-0.15 (0.01)
Respondent employed	0.33 (0.47)	0.31 (0.46)	0.35 (0.48)	-0.03 (0.02)	0.45 (0.50)	0.22 (0.41)	0.23 (0.01)	0.42 (0.49)	0.24 (0.43)	0.18 (0.02)	-0.10 (0.01)
Number of meals eaten yesterday	2.29 (0.68)	2.23 (0.69)	2.35 (0.66)	-0.12 (0.02)	1.99 (0.62)	2.58 (0.60)	-0.59 (0.02)	2.14 (0.63)	2.45 (0.69)	-0.30 (0.02)	0.09 (0.02)
Received any loan	0.54 (0.50)	0.46 (0.50)	0.61 (0.49)	-0.15 (0.02)	0.51 (0.50)	0.56 (0.50)	-0.04 (0.01)	0.62 (0.49)	0.45 (0.50)	0.17 (0.02)	-0.16 (0.01)
Any loan denied	0.24 (0.43)	0.22 (0.42)	0.26 (0.44)	-0.04 (0.01)	0.25 (0.43)	0.23 (0.42)	0.01 (0.01)	0.26 (0.44)	0.22 (0.41)	0.05 (0.01)	-0.04 (0.01)
<i>Panel B: Endline Treatment Effects (SEs)</i>											
Respondent hours worked last week	1.05 (0.98)	1.32 (1.34)	0.71 (1.34)	0.62 (1.84)	1.74 (1.24)	0.37 (1.41)	1.37 (1.81)	1.17 (1.24)	0.88 (1.38)	0.29 (1.73)	0.16 (1.61)
Indicator for household self-employed	0.04 (0.02)	0.03 (0.02)	0.04 (0.02)	-0.00 (0.03)	0.04 (0.02)	0.04 (0.02)	-0.00 (0.03)	0.05 (0.02)	0.02 (0.02)	0.03 (0.03)	-0.02 (0.03)
Indicator for household employed	-0.03 (0.02)	-0.02 (0.02)	-0.03 (0.02)	0.01 (0.03)	-0.02 (0.02)	-0.03 (0.02)	0.01 (0.03)	-0.03 (0.02)	-0.02 (0.02)	-0.01 (0.03)	0.01 (0.03)
Interhousehold transfers received	9.85 (7.13)	-12.80 (8.69)	32.31 (10.33)	-45.12 (12.73)	15.41 (9.64)	4.60 (9.64)	10.81 (12.99)	25.35 (9.53)	-5.84 (10.05)	31.19 (13.44)	-38.16 (11.49)
Interhousehold transfers sent	7.96 (2.75)	7.98 (3.79)	7.84 (4.01)	0.14 (5.57)	9.78 (3.84)	6.19 (4.07)	3.59 (5.68)	7.41 (3.99)	8.46 (3.74)	-1.05 (5.42)	0.57 (4.89)
Total value of loans taken in last 12 months	3.48 (4.03)	1.02 (4.74)	5.70 (6.68)	-4.68 (8.28)	1.21 (5.07)	5.52 (6.47)	-4.31 (8.40)	7.53 (6.05)	-0.81 (5.49)	8.33 (8.23)	-6.51 (7.11)
Total value of loans given in last 12 months	3.43 (0.79)	3.74 (1.05)	3.09 (1.26)	0.65 (1.69)	4.24 (0.91)	2.65 (1.27)	1.59 (1.56)	3.02 (1.24)	3.81 (1.12)	-0.79 (1.74)	0.72 (1.46)
Number of household members	0.18 (0.07)	0.16 (0.08)	0.18 (0.09)	-0.02 (0.11)	0.23 (0.11)	0.14 (0.08)	0.09 (0.14)	0.19 (0.09)	0.15 (0.09)	0.04 (0.13)	-0.03 (0.12)

Notes: This table presents differences in baseline characteristics (Panel A) and treatment effects estimated via OLS (Panel B) based on households' classification into most deprived (D), socially optimal (SO), or most impacted (I) groups and their complements (denoted by ^{C}) across our 150 models. Specifically, households are assigned to a group (e.g. the most deprived) if they are classified in that group in more than 50% of the 150 models. In Panel A, we report group means and standard deviations (columns 1, 2, 3, 5, 6, 8 and 9) and test for differences between groups using OLS, group indicators and their interaction (columns 4, 7, 10 and 11). In Panel B, column 1 reports the coefficient (and standard error) from a regression on treatment status and a constant, with standard errors are clustered at the village level. Columns 2–4 present group ATEs from a regression on treatment status, deprivation status, and the interaction between the two; columns 5–7 do the same by socially optimal status, and columns 8–10 to most impacted status. Column 11 presents the difference in treatment effects for the most deprived and most impacted; standard errors for this difference are calculated by bootstrapping the regressions in columns 2-4 and 8-10 999 times.

Table A.7: Differences in characteristics and treatment effects group classification using income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	All	D	D^C	$D - D^C$	SO	SO^C	$SO - SO^C$	I	I^C	$I - I^C$	$D - I$
<i>Panel A: Baseline demographics, Mean (SD)</i>											
Household size	4.40 (2.21)	3.56 (2.08)	5.23 (2.01)	-1.67 (0.07)	4.52 (2.61)	4.27 (1.69)	0.25 (0.07)	5.01 (2.43)	3.80 (1.76)	1.21 (0.07)	-1.44 (0.05)
Number of prime-age adults in household	1.88 (0.77)	1.66 (0.81)	2.06 (0.68)	-0.40 (0.03)	1.93 (0.81)	1.85 (0.72)	0.08 (0.02)	1.98 (0.78)	1.78 (0.73)	0.20 (0.02)	-0.30 (0.02)
Number of children in household	2.46 (1.88)	1.82 (1.74)	3.10 (1.79)	-1.29 (0.06)	2.63 (2.18)	2.29 (1.50)	0.34 (0.06)	3.01 (2.07)	1.93 (1.50)	1.07 (0.06)	-1.18 (0.04)
Education level of household head	6.48 (3.72)	5.32 (3.83)	7.63 (3.22)	-2.31 (0.12)	5.92 (3.84)	7.06 (3.51)	-1.14 (0.12)	6.58 (3.60)	6.37 (3.84)	0.21 (0.13)	-1.26 (0.09)
Age of household head	42.37 (15.47)	47.20 (17.61)	37.58 (11.11)	9.62 (0.46)	45.11 (16.24)	39.51 (14.07)	5.60 (0.54)	41.61 (13.95)	43.12 (16.82)	-1.51 (0.51)	5.57 (0.36)
Female household head	0.26 (0.44)	0.45 (0.50)	0.07 (0.25)	0.38 (0.01)	0.30 (0.46)	0.21 (0.41)	0.09 (0.01)	0.22 (0.41)	0.30 (0.46)	-0.08 (0.01)	0.23 (0.01)
Widow	0.18 (0.39)	0.37 (0.48)	0.00 (0.00)	0.37 (0.01)	0.24 (0.43)	0.13 (0.33)	0.12 (0.01)	0.15 (0.36)	0.22 (0.41)	-0.07 (0.01)	0.22 (0.01)
Household owns any livestock	0.27 (0.44)	0.20 (0.40)	0.33 (0.47)	-0.12 (0.01)	0.32 (0.47)	0.21 (0.41)	0.11 (0.01)	0.35 (0.48)	0.18 (0.39)	0.17 (0.01)	-0.15 (0.01)
Household owns land	0.84 (0.36)	0.85 (0.35)	0.83 (0.38)	0.03 (0.01)	0.93 (0.25)	0.75 (0.43)	0.19 (0.01)	0.93 (0.25)	0.75 (0.43)	0.18 (0.01)	-0.08 (0.01)
Respondent self-employed	0.27 (0.44)	0.12 (0.33)	0.42 (0.49)	-0.29 (0.01)	0.16 (0.37)	0.38 (0.49)	-0.22 (0.01)	0.19 (0.40)	0.34 (0.47)	-0.15 (0.01)	-0.07 (0.01)
Respondent employed	0.33 (0.47)	0.31 (0.46)	0.35 (0.48)	-0.04 (0.01)	0.57 (0.49)	0.08 (0.28)	0.49 (0.01)	0.60 (0.49)	0.07 (0.25)	0.54 (0.01)	-0.29 (0.01)
Number of meals eaten yesterday	2.29 (0.68)	2.14 (0.70)	2.44 (0.62)	-0.30 (0.02)	2.26 (0.68)	2.33 (0.68)	-0.07 (0.02)	2.30 (0.66)	2.29 (0.69)	0.02 (0.02)	-0.16 (0.02)
Received any loan	0.54 (0.50)	0.46 (0.50)	0.61 (0.49)	-0.16 (0.02)	0.54 (0.50)	0.53 (0.50)	0.01 (0.02)	0.58 (0.49)	0.49 (0.50)	0.09 (0.02)	-0.12 (0.01)
Any loan denied	0.24 (0.43)	0.21 (0.41)	0.27 (0.45)	-0.06 (0.01)	0.27 (0.44)	0.22 (0.41)	0.05 (0.01)	0.28 (0.45)	0.21 (0.40)	0.07 (0.01)	-0.07 (0.01)
<i>Panel B: Endline Treatment Effects (SEs)</i>											
Respondent hours worked last week	1.05 (0.98)	1.47 (1.27)	0.22 (1.37)	1.25 (1.81)	0.52 (1.23)	1.62 (1.41)	-1.10 (1.78)	-0.03 (1.25)	1.98 (1.41)	-2.01 (1.79)	1.50 (1.38)
Indicator for household self-employed	0.04 (0.02)	0.03 (0.02)	0.04 (0.02)	-0.02 (0.03)	0.04 (0.02)	0.03 (0.02)	0.01 (0.03)	0.06 (0.02)	0.01 (0.02)	0.05 (0.03)	-0.04 (0.03)
Indicator for household employed	-0.03 (0.02)	-0.04 (0.02)	-0.02 (0.02)	-0.02 (0.03)	-0.03 (0.02)	-0.03 (0.02)	0.00 (0.03)	-0.04 (0.02)	-0.02 (0.02)	-0.02 (0.03)	0.00 (0.02)
Interhousehold transfers received	9.85 (7.13)	9.45 (9.92)	10.19 (9.93)	-0.73 (13.81)	15.27 (8.94)	4.76 (10.57)	10.51 (13.43)	13.09 (8.78)	7.02 (10.42)	6.07 (13.04)	-3.64 (10.40)
Interhousehold transfers sent	7.96 (2.75)	6.26 (3.65)	8.82 (4.38)	-2.55 (5.91)	7.57 (3.50)	8.60 (4.36)	-1.03 (5.68)	5.16 (3.74)	10.66 (4.27)	-5.50 (5.85)	1.11 (3.54)
Total value of loans taken in last 12 months	3.48 (4.03)	1.92 (4.08)	3.48 (7.07)	-1.56 (8.24)	8.20 (4.94)	-0.57 (6.42)	8.77 (8.28)	8.32 (5.25)	-1.04 (6.09)	9.36 (8.10)	-6.40 (5.51)
Total value of loans given in last 12 months	3.43 (0.79)	3.14 (0.85)	3.43 (1.37)	-0.29 (1.64)	3.88 (1.00)	3.08 (1.23)	0.81 (1.60)	2.83 (1.17)	3.99 (1.09)	-1.16 (1.60)	0.30 (1.31)
Number of household members	0.18 (0.07)	0.21 (0.10)	0.10 (0.10)	0.10 (0.14)	0.19 (0.12)	0.18 (0.09)	0.01 (0.15)	0.16 (0.11)	0.17 (0.09)	-0.01 (0.14)	0.04 (0.10)

Notes: This table presents differences in baseline characteristics (Panel A) and treatment effects estimated via OLS (Panel B) based on households' classification into most deprived (D), socially optimal (SO), or most impacted (I) groups and their complements (denoted by ^{C}) across our 150 models. Specifically, households are assigned to a group (e.g. the most deprived) if they are classified in that group in more than 50% of the 150 models. In Panel A, we report group means and standard deviations (columns 1, 2, 3, 5, 6, 8 and 9) and test for differences between groups using OLS, group indicators and their interaction (columns 4, 7, 10 and 11). In Panel B, column 1 reports the coefficient (and standard error) from a regression on treatment status and a constant, with standard errors are clustered at the village level. Columns 2–4 present group ATEs from a regression on treatment status, deprivation status, and the interaction between the two; columns 5–7 do the same by socially optimal status, and columns 8–10 to most impacted status. Column 11 presents the difference in treatment effects for the most deprived and most impacted; standard errors for this difference are calculated by bootstrapping the regressions in columns 2-4 and 8-10 999 times.

Table A.8: Welfare inputs for food security: predicted per capita untreated outcomes (y_h^0) and ATEs (Δ_h) by group

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: Food security index</i>										
Predicted	0.06	-0.09	-0.06	-0.03	-0.06	0.07	0.08	0.09	0.09	-0.01
Actual	0.04	-0.17	-0.06	-0.09	-0.08	0.08	0.14	0.12	0.12	0.02
					[-0.12,-0.05]					[-0.04,0.06]
<i>Panel B: Food consumption</i>										
Predicted	470	358	462	555	-198	84	73	105	108	-35
Actual	467	346	445	552	-206	100	73	131	131	-58
					[-241,-170]					[-147,32]
<i>Panel C: Food consumption per-capita</i>										
Predicted	470	357	406	431	-74	21	23	28	28	-5
Actual	467	345	386	421	-76	28	25	37	32	-7
					[-95,-56]					[-35,20]

Notes: This table reproduces Table 2 for food security-related outcomes. The food security index is an index of questions about the food consumption of adults and children over the past 7 days (see Appendix B for details). We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. The rest of the details follow Table 2. Monetary values are in USD PPP (2016) for panels B and C.

Table A.9: Ratio of consumption and assets relative to income for a fixed sample

	All	Deprived (D)	Socially Optimal (<i>SO</i>)	Impacted (I)
Consumption Δ_h	310	247	439	405
Assets Δ_h	182	163	197	206
Income Δ_h	85	63	138	114
Consumption / Income	3.65	3.95	3.18	3.55
Assets / Income	2.14	2.61	1.43	1.81

SO defined for consumption and $\alpha = 0.001$. Deprived and Impacted groups are defined for consumption in order to keep groups fixed across outcomes. Monetary values are in USD PPP (2016).

Table A.10: Re-targeting dynamics: Share of deprived households targeted in round 2 by whether they were targeted in round 1

	Central scenario $\alpha = 0.001$			Extreme curvature $\alpha = 0.015$		
	Targeted R1	Targeted R2	Population share	Targeted R1	Targeted R2	Population share
Deprived R1	0.54	0.53	0.50	0.96	0.78	0.50
Deprived & Targeted in R1	1.00	0.88	0.27	1.00	0.77	0.48
Deprived & Not Targeted in R1	0.00	0.11	0.23	0.00	0.97	0.02
Not deprived R1	0.46	0.47	0.50	0.04	0.22	0.50
Not Deprived & Targeted in R1	1.00	0.87	0.23	1.00	0.03	0.02
Not Deprived & Not Targeted in R1	0.00	0.14	0.27	0.00	0.23	0.48

Deprivation (D) and targeting (SO) are defined based on consumption. For additional details see Table 1, panel A and panel C (Dynamic). R1 (R2) denotes round 1 (2) of targeting. For $\alpha = 0.001$, only 12% of households shift status between rounds.

Table A.11: Welfare analysis: sensitivity of results to the threshold of the null hypothesis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Most deprived						Most impacted				
			p-val	p-val	p-val	p-val		p-val	p-val	p-val	p-val
	CE	Share	$D > 0.85$	$D > 0.90$	$D > 0.95$	$D = 1.00$	Share	$I > 0.85$	$I > 0.90$	$I > 0.95$	$I = 1.00$
<i>Panel A: Consumption, CARA</i>											
$\alpha = 0.0000$	\$50	0.26	0.00	0.00	0.00	0.00	1.00	1.00	1.00	1.00	1.00
$\alpha = 0.0005$	\$49	0.43	0.00	0.00	0.00	0.00	0.79	0.19	0.04	0.00	0.00
$\alpha = 0.0010$	\$49	0.54	0.00	0.00	0.00	0.00	0.68	0.01	0.00	0.00	0.00
$\alpha = 0.0075$	\$41	0.91	0.85	0.38	0.01	0.00	0.35	0.00	0.00	0.00	0.00
$\alpha = 0.0150$	\$33	0.96	0.98	0.93	0.31	0.00	0.29	0.00	0.00	0.00	0.00

Notes: This table replicates panel a from table 1, with additional null hypotheses for the overlap between I, D, and S.O. For the test $I = 1.00$ with $\alpha = 0$, the p-value is set to 1 since the bootstrap distribution is degenerate by definition.

B Appendix: Data & variable construction

B.1 Baseline predictors

The baseline survey collected data on a number of household characteristics that might predict endline outcomes and treatment effects. From among these, we first selected a subset that have been documented as appearing in other proxy means tests used to target social protection programs in comparable low-income countries. Specifically, we retained all variables in the intersection of household-level variables in the PMTs studied by [Kidd and Wylde \(2011\)](#), [Alatas et al. \(2012\)](#), and [Niehaus et al. \(2013\)](#), for a total of 31 potential predictors. Among these we retained those that exhibit non-trivial amounts of variation in our data, yielding 24 potential predictors. Table [B.1](#) summarizes this selection procedure.

In our pre-analysis plan we planned to further narrow our feature selection by keeping predictors that increased the adjusted R -squared of a regression predicting baseline outcomes. This procedure is not well-defined, however (since whether or not a variable increases the adjusted R -squared depends on what other variables are included). It also creates further complications for inference, since it uses the data once to select predictors before then using them again to form predictors. Our preferred approach is therefore to use a list of 16 covariates selected by hand, prior to any analysis, based on our knowledge of the local context. These covariates are

1. Household size
2. Respondent a widow
3. Respondent female
4. Household has children
5. Household has school-aged children
6. Household has children 3 or under
7. Household has children 6 or under
8. Household has an elderly (65+) member
9. Household owns any livestock
10. Household owns any land
11. Household owns more than 0.25 acres of land
12. Household owns TV or radio
13. Number of meals eaten yesterday
14. Number of meals with protein yesterday
15. Respondent self-employed
16. Respondent employed

As a robustness check we also examine results using a data-selected subset of 24 candidate predictors. Specifically, we run LASSO regressions using these 24 variables to predict our four main outcomes, and retain all variables selected by LASSO in any of these four models.

This yields a list of 15 variables that is in fact quite similar to our hand-selected list (see Table B.1). Re-estimating our core specifications using this alternative list of predictors, we obtain results that are generally quite similar to our main results (Tables D.1 and D.2).

B.2 Outcomes

The endline survey contained detailed modules on economic activities such as household expenditures and crop production, asset ownership, psychological well-being, health and nutrition, and female respondents surveyed by a female enumerator were also administered a module on female empowerment and gender-based violence. We construct four aggregate outcomes using this data: consumption expenditure, income, assets, and a food security index.

Consumption expenditure is defined as the total annualized household expenditure. Income is total annualized household income, given by the sum of agricultural profits, profits from self-employment, and wage earnings. Assets is equal to the total value of household's assets, excluding land and houses. Each of these variables is winsorized at the 99th percentile and expressed in USD PPP terms.

The food security index is a weighted average of standardized food security covariates constructed according to [Anderson \(2008\)](#). The index is calculated using the following variables:

1. Number of days adults skipped or cut the amount of meals in the past 7 days
2. Number of days children skipped or cut the amount of meals in the past 7 days
3. Number of days adults went the entire day without meals in the past 7 days
4. Number of days children went the entire day without meals in the past 7 days
5. Number of days adults went to bed hungry in the last 7 days
6. Number of days children went to bed hungry in the past 7 days
7. Number of meals eaten yesterday that included meat, fish, or eggs

Table B.1: Selecting Proxy Means Test (PMT) Variables

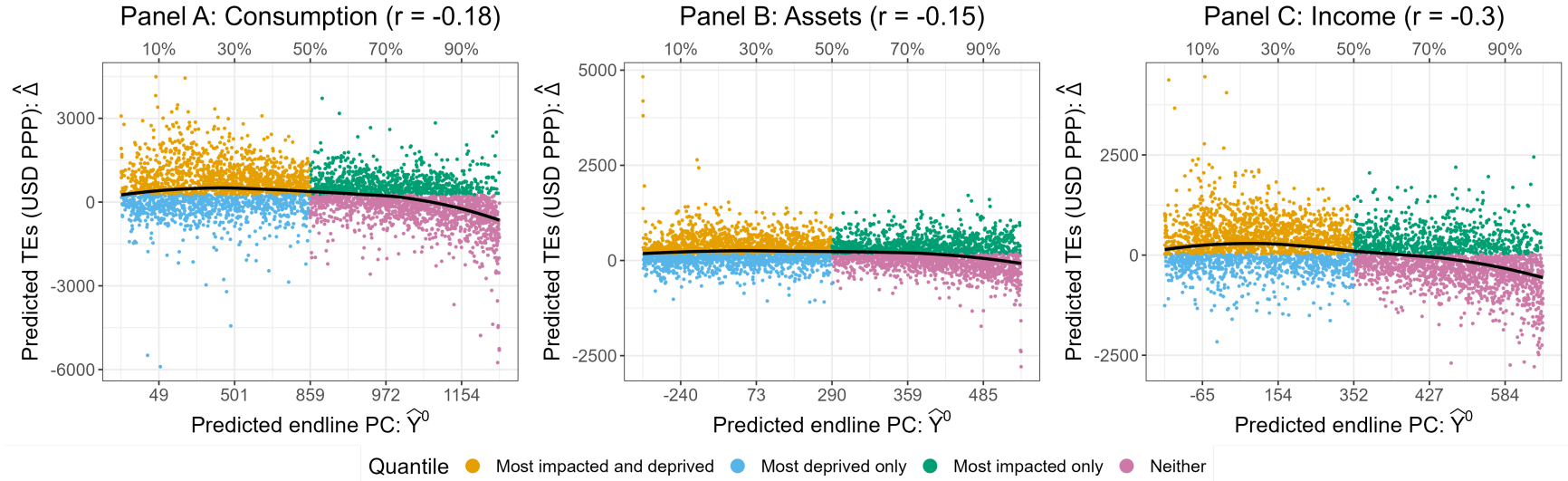
(1)	(2)	(3)	(4)	(5)	(6)
Measure	Source	Collected in GE Baseline (1/0)	Variation in GE baseline data (1/0)	Selected (preferred) PMT list	Selected via LASSO (robustness check)
<i>Panel A: Human Capital</i>					
Education of Household Head	KW, A	1	1		Indicator for above median edu for HH head
Highest level of education in household	KW, A	1	1		Indicator for above median edu for highest in HH
Female literacy	KW	0	–		
Number of children in school	KW, A	1	1		Number of children in school
<i>Panel B: Demographic Characteristics</i>					
Household Size	KW, A	1	1	Household size	Household size
Number of Children	KW, A	1	1	Indicator for children; Indicator for child under 3; Indicator for child under 6; Indicator for school-age children	Indicator for has children
Gender/marital status of head (e.g. widow)	KW, A	1	1	Respondent is female; Respondent is widow	Female household head; Respondent is widow
Age of household head	KW, A	1	1	Household has elderly member	Household head age; Household has elderly member
Dependency ratio	KW, A	1	1		
<i>Panel C: Household assets</i>					
Own home	KW, A	1	0		
Wall material	KW, A	1	0		
Roofing material	KW, A	1	0		
Floor material	A	1	0		
Number of rooms / floor space per-capita	KW, A	1	1		
Type of latrine / toilet	KW, A	1	0		
Water source	A, N	1	1		
Access to electricity	KW, A	1	0		
Gas connection	N	0	–		
Type of cooking fuel	KW, A	0	–		
Radio, television	KW, N	1	1	Indicator for owning TV or radio	Indicator for owning TV or radio
Telephone / Mobile phone	KW, N	1	1		Indicator for mobile phone
Cooker, heater, fan, air conditioning	KW	1	1		Above median appliance value
Furniture	KW	1	1		Above median furniture value
Bicycle, car, motorcycle	KW, N	1	1		Indicator for owning bicycle
Access to microcredit	A	1	1		
<i>Panel D: Productive assets</i>					
Landholding size	KW, N	1	1	Household owns land; Household owns more than 0.25 acres of land	
Livestock	KW	1	1	Indicator for owning livestock	Indicator for owning livestock
Use of fertilizer	KW	1	1		
<i>Panel E: Livelihood options</i>					
Agricultural or non-farm wage labor	KW	1	1	Respondent employed	Respondent employed
Non-farm independent business	KW	1	1	Respondent self-employed	Respondent self-employed
Agricultural production of cash or staple crops	KW	1	0		
Receipt of foreign remittances	KW	0	–		
Sector of work (informal, industry, or agriculture)	KW, A	1	1		
Annual income threshold	N	1	1		Above median total income
Government employee	N	0	–		
Food security (adults, children)		1	1	Number of meals eaten yesterday; Number of meals with protein yesterday	
<i>Total variables</i>		31	24	16	18

Notes: This table outlines variables that have been included in proxy means tests (PMTs) and their overlap with variables in this study's baseline survey. Column (1) reports the measures from the sources in Column (2), namely [Kidd and Wylde \(2011\)](#) (KW), [Alatas et al. \(2012\)](#) (A), and [Niehaus et al. \(2013\)](#) (N). Columns (3) and (4) denote whether or not similar variables were collected as part of the GE baseline survey, and if so, whether there is meaningful variation in the variable. Column (5) includes our preferred list of PMT-like variables, and column (6) reports the set of variables selected via LASSO among those in column (4).

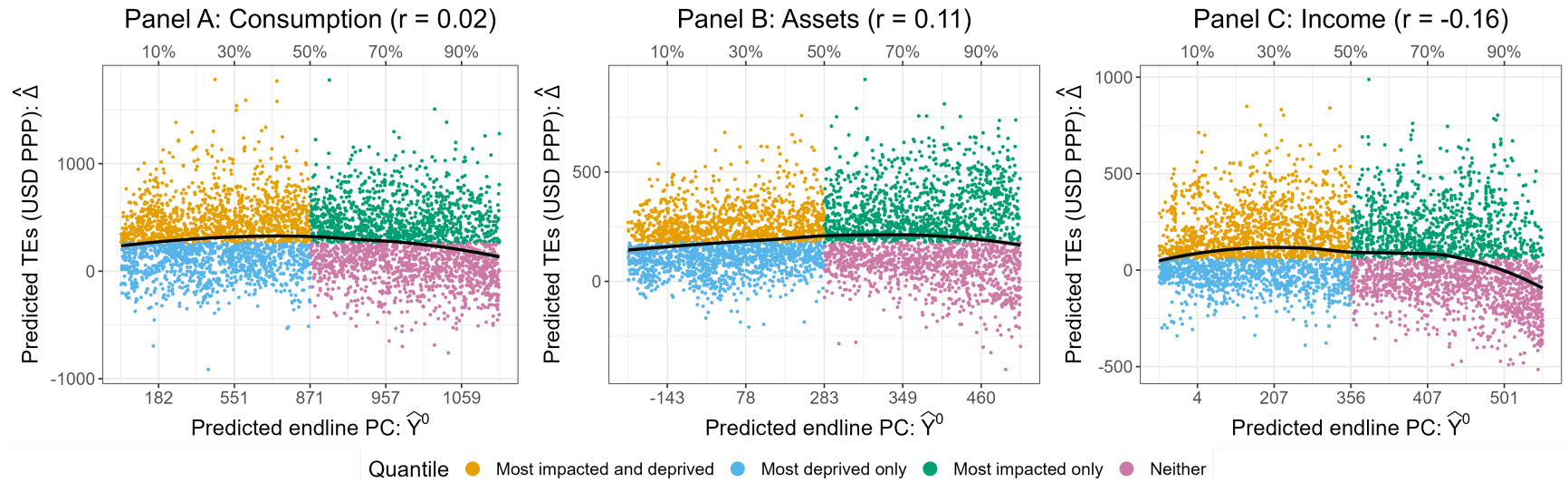
C Appendix: Alternative learning models

Figure C.1: Predicted treatment effects ($\hat{\Delta}_h$) plotted against the predicted untreated per capita values ($\hat{y}_h^0$) for OLS and LASSO predictions

1. OLS



2. LASSO



Notes: These figures re-create Figure 1 using OLS (Row 1) and LASSO (Row 2) as prediction methods in contrast to generalized random forests. Monetary values are in USD PPP (2016).

Table C.1: Overlap of socially optimal households with most impacted and deprived using alternative methods for prediction of consumption

	(1)	(2)	(3)
CARA: α CE	Most deprived	Most impacted	
<i>Panel A: OLS</i>			
0.0000	\$50	0.59	1.00
0.0005	\$49	0.61	0.97
0.0010	\$49	0.63	0.95
0.0075	\$41	0.69	0.84
0.0150	\$33	0.70	0.79
<i>Panel B: LASSO</i>			
0.0000	\$50	0.52	1.00
0.0005	\$49	0.57	0.95
0.0010	\$49	0.60	0.92
0.0075	\$41	0.75	0.76
0.0150	\$33	0.77	0.72

Notes: This table reproduces the social welfare analysis of Table 1 but with OLS and LASSO predictions (from Tables C.2 and C.3). See Table 1 for more details.

Table C.2: Welfare inputs using OLS for prediction: predicted per capita untreated outcomes (y_h^0) and ATEs (Δ_h) by group

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: Consumption</i>										
Predicted	722	392	626	667	-275	263	450	815	821	-371
Actual	729	517	700	750	-233	310	335	346	351	-16
					[-271,-199]					[-123,97]
<i>Panel B: Assets</i>										
Predicted	214	19	179	208	-189	195	236	448	450	-214
Actual	213	69	224	255	-186	182	147	175	179	-32
					[-208,-165]					[-95,40]
<i>Panel C: Income</i>										
Predicted	298	101	247	246	-146	55	236	486	487	-251
Actual	297	187	290	294	-107	85	136	149	146	-11
					[-127,-87]					[-91,63]

Notes: This table reproduces Table 2 but replaces GRF predictions for each fold by ordinary least squares (OLS) predictions. We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. See Table 2 for more details. Monetary values are in USD PPP (2016).

Table C.3: Welfare inputs using LASSO for prediction: predicted per capita untreated outcomes (y_h^0) and ATEs (Δ_h) by group

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: Consumption</i>										
Predicted	725	467	644	730	-264	272	290	487	494	-204
Actual	729	491	658	760	-269	310	322	324	314	7
					[-310,-231]					[-103,124]
<i>Panel B: Assets</i>										
Predicted	213	37	177	242	-206	192	179	298	303	-123
Actual	213	54	185	254	-200	182	158	197	210	-53
					[-223,-177]					[-121,25]
<i>Panel C: Income</i>										
Predicted	297	151	270	283	-131	76	103	220	221	-117
Actual	297	176	282	302	-126	85	89	105	103	-14
					[-153,-104]					[-108,73]

Notes: This table reproduces Table 2 but with LASSO predictions replacing GRF predictions for each fold. We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. See Table 2 for more details. Monetary values are in USD PPP (2016).

D Appendix: Robustness checks

Table D.1: Robustness checks for main social welfare analysis: consumption

	(1)	(2)	(3)
CARA: α	CE	Most deprived	Most impacted
<i>Panel A: Machine-selected covariates</i>			
0.0000	\$50	0.32	1.00
0.0005	\$49	0.51	0.75
0.0010	\$49	0.61	0.66
0.0075	\$41	0.91	0.40
0.0150	\$33	0.96	0.36
<i>Panel B: Using untreated data for endline predictions</i>			
0.0000	\$50	0.28	1.00
0.0005	\$49	0.46	0.78
0.0010	\$49	0.57	0.67
0.0075	\$41	0.91	0.37
0.0150	\$33	0.96	0.32

Notes: This table reproduces the social welfare analysis of Table 1 but with features selected via LASSO (Panel A), as in Table D.2 and using untreated data for endline predictions (Panel B), as in D.3. See Table 1 for more details.

Table D.2: Welfare inputs using machine-selected covariates: predicted per capita untreated outcomes (y_h^0) and ATEs (Δ_h) by group

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: Consumption</i>										
Predicted	752	548	611	914	-366	259	240	278	298	-59
Actual	729	500	540	912	-412	310	308	398	397	-89
					[-468,-357]					[-241,65]
<i>Panel B: Assets</i>										
Predicted	234	78	128	332	-254	181	171	186	197	-26
Actual	213	37	83	328	-290	182	176	167	181	-5
					[-320,-265]					[-86,93]
<i>Panel C: Income</i>										
Predicted	303	186	252	315	-129	61	61	81	83	-22
Actual	297	155	231	323	-168	85	95	117	102	-7
					[-218,-135]					[-110,108]

Notes: This table reproduces Table 2 but with covariates (features) selected via LASSO (see Table B.1). We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. See Table 2 for more details. Monetary values are in USD PPP (2016).

Table D.3: Welfare inputs using untreated data for endline predictions: predicted per capita untreated outcomes (y_h^0) by group

Statistic	Predicted per capita untreated outcomes (y_h^0)					Average Treatment Effects (Δ_h)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All	(D)	(SO)	(I)	(D) - (I)	All	(D)	(SO)	(I)	(D) - (I)
<i>Panel A: Consumption</i>										
Predicted	724	511	581	896	-385	277	250	303	326	-76
Actual	729	505	554	910	-405	310	274	429	405	-132
					[-466,-338]					[-285,21]
<i>Panel B: Assets</i>										
Predicted	214	66	106	325	-259	189	178	195	207	-28
Actual	213	54	97	337	-282	182	151	164	188	-37
					[-310,-251]					[-127,52]
<i>Panel C: Income</i>										
Predicted	294	177	246	303	-126	69	69	92	94	-25
Actual	297	171	251	323	-151	85	89	103	94	-4
					[-180,-125]					[-113,97]

Notes: This table reproduces Table 2 by generating prediction models for endline outcomes using only data from transfer-eligible households in control villages. We report the 95% BCa CI for the actual difference statistic through empirical bootstrap conditional on the GRF model predictions, as described in Section 3. See Table 2 for more details. Monetary values are in USD PPP (2016).

E Appendix: Prediction

Algorithm E.1:

Result: mean statistic of interest, for a given W

for i *in* $1...150$ **do**

 Split data randomly into 5 folds*;

for k *in* $1...5$ **do**

 test data $\leftarrow$ fold(k);

 train data $\leftarrow$ fold(-k);

 rf $\leftarrow$ randomforest(train data);

 cf $\leftarrow$ causalforest(train data);

$\hat{y}^{0,K}(X_h, t_h) \leftarrow$ predict(rf, test data);

$\hat{\Delta}^K(X_h, t_h) \leftarrow$ predict(cf, test data);

$d\hat{W}^K(X_h, t_h) \leftarrow n_h \times \left[W(\hat{y}^{0,K}(X_h, t_h) + \hat{\Delta}^K(X_h, t_h)/n_h) - W(\hat{y}^{0,K}(X_h, t_h)) \right]$

$d\hat{W}_h^K \leftarrow \frac{1}{\bar{t}} \sum_{t=0}^{\bar{t}} d\hat{W}^K(X_h, t_h);$

$HH_k \leftarrow \text{Households} \cap \text{fold}(k);$

for h *in* HH_k **do**

if $d\hat{W}_h^K \geq \text{median}(d\hat{W}_{h'}^K)$ **then**

 sociallyoptimal[h] = 1

end

end

end

 results[i] $\leftarrow$ statistic(sociallyoptimal, data);

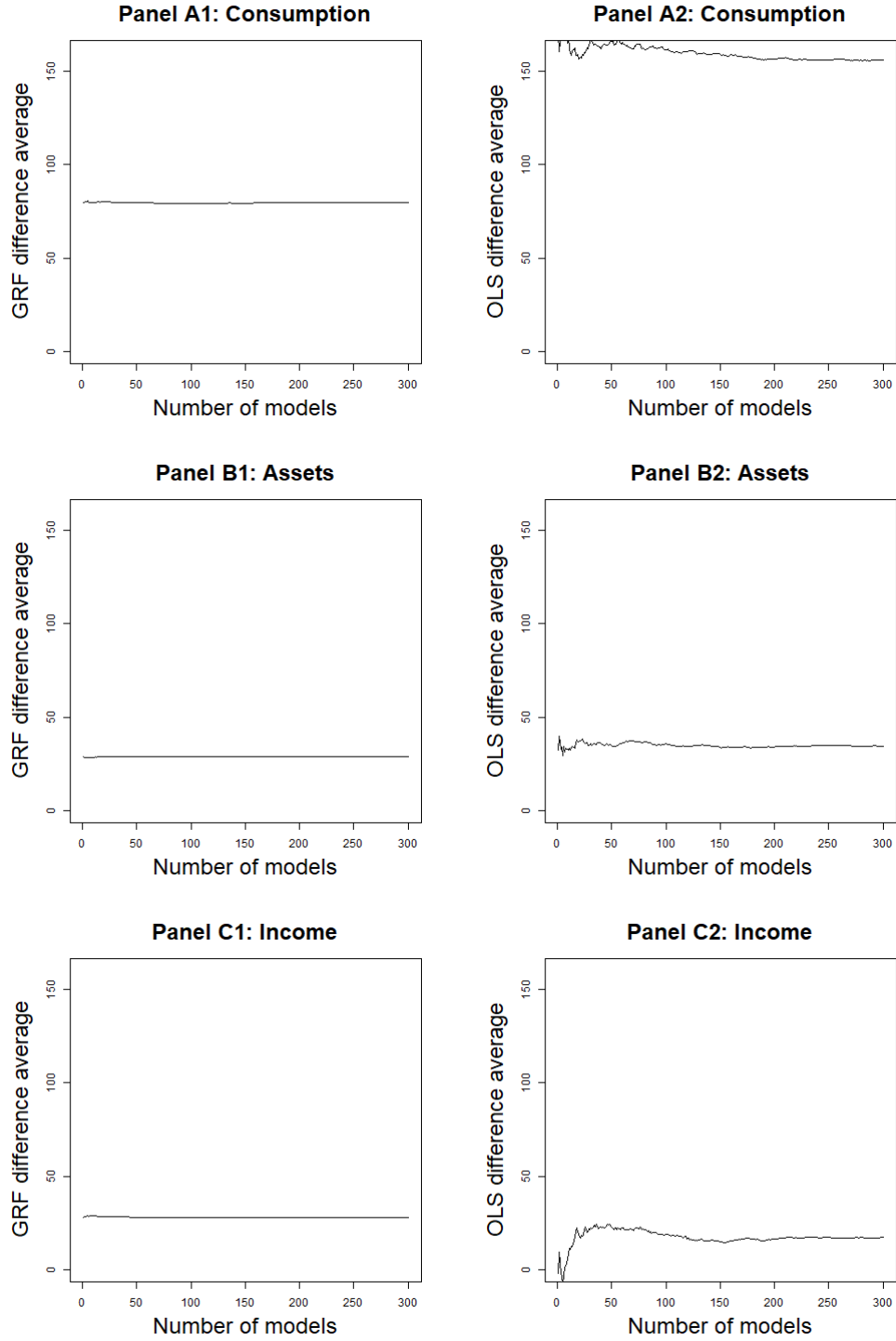
end

result $\leftarrow$ mean(results);

Figure E.1: Model convergence

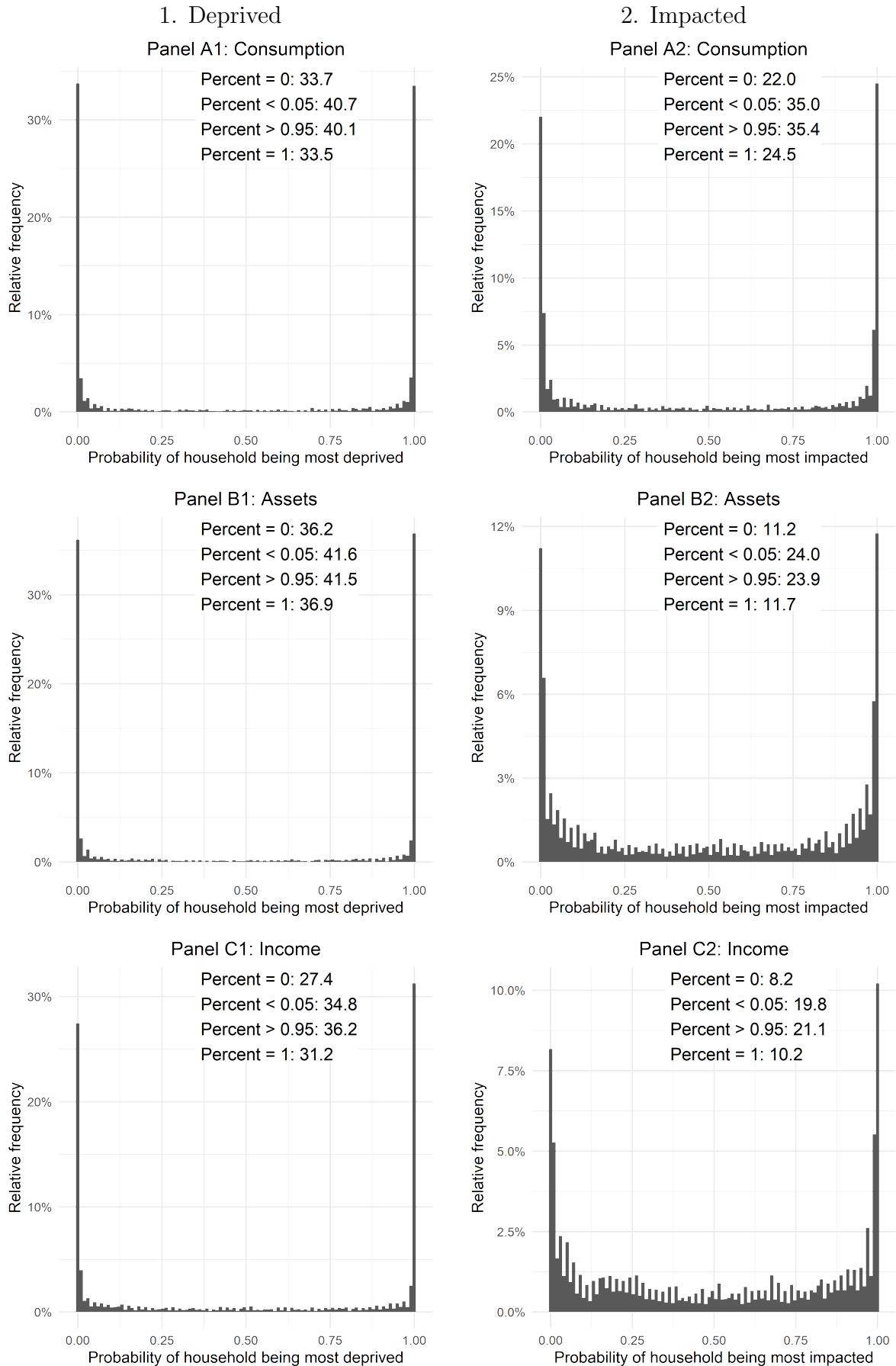
1. Predicted difference (GRF)

2. Actual difference (OLS)



Notes: This figure presents the convergence of the difference of the predicted and actual average treatment effect between groups I, D as a function of the number of models being trained. Note that this statistic remains roughly constant between 150 and 300 models. Monetary values for the y-axis are in USD PPP (2016).

Figure E.2: Stability of most deprived and impacted classification



Notes: This figure presents the relative frequency of the probability that a household is classified as either most deprived (column 1) or most impacted (column 2) across 150 models. The large mass at/around 0 and 1 indicate that most households are being consistently classified to a group across models.

Table E.1: Previous Heterogeneity results (EHMNW), p-values

		Interaction of treatment village indicator with indicator for:						
	Female Respondent	Age $\geq$ 25	Married	Completed Primary School	Child in HH	High Psych Well-being	Self-employed	Wage employment
Consumption	0.34	0.05	0.52	0.91	0.95	0.96	0.57	0.70
Assets	0.61	0.21	0.47	0.10	0.99	0.90	0.12	0.27
Income	0.66	0.03	0.86	0.59	0.28	0.83	0.17	0.01

Notes: This table presents estimates for treatment effect heterogeneity for eligible households for the main targeting outcomes along 8 pre-specified dimensions of heterogeneity. Each reported p-value is from a separate regression, and reports the p-value on the interaction term of the listed baseline covariate with the treatment village indicator; this interaction term and baseline covariate are added to the regression equation. Each row represents a separate outcome. Standard errors are clustered at the village level.

F Spillovers

This appendix illustrates formally one way in which one might incorporate spillovers into an analysis of optimal targeting, some of the insights and issues that arise in doing so, and what the data from our specific application have to say about these. To limit superfluous notation we collapse time to a single period. Let household h 's outcome as a function of the full treatment assignment vector $\mathbf{T}$ be given by

$$Y_h = Y_h^0 + T_h \cdot \Delta_h + \sum_{h'} S_{hh'} T_{h'} \quad (\text{F.1})$$

where the parameter $S_{hh'}$ captures the spillover effect on household h of treating household h' . The policy-maker again seeks to learn a scalable targeting rule that is a function of only observable characteristics X , and to this end forms a prediction

$$\hat{S}(X_h, X_{h'}) \text{ of } \mathbb{E}[S_{hh'} | X_h, X_{h'}] \quad (\text{F.2})$$

analogous to its predictions of Y_h^0 and Δ_h . In principle this prediction might depend on the characteristics of both households. Moreover, these characteristics might include their location, so that $\hat{S}(X_h, X_{h'})$ could depend on the distance between the households.

To explore whether own-household characteristics X_h (other than distance from h') might predict the spillover effects experienced by household h in our Kenyan setting we conduct two separate exercises. The first examines eligible households in villages that were not treated, and examines variation in their exposure to indirect effects from *outside* those villages. The appeal of this exercise is that these are exactly the same class of households for whom the main analysis predicts treatment effects, exposed to experimental variation in neighborhood treatment intensity. To make the exercises as directly comparable as possible we construct a binary indicator of exposure, calculating for each household whether or not its neighborhood treatment intensity, as defined in [Egger et al. \(2022\)](#), is above the median. We then re-run the analysis replacing the own-village treatment indicator with this indicator.³⁷

The second examines treatment effects on *ineligible* households. The drawback of this exercise is that effects on these households may well have been different, as they tend to be somewhat (though not enormously) better-off ([Figure A.2](#)). If they are more likely to own small businesses, for example, then we might have better luck predicting heterogeneous spillover effects for them via mechanisms like that described above. The advantage is that we can take advantage of the same experimental variation as in the main analysis (village-level treatment assignment), with the interpretation unambiguous in this case: because ineligible households did not receive transfers themselves, any predictable heterogeneity we find in the (time-averaged) effects of assigning their *village* to treatment must reflect heterogeneous indirect effects.

³⁷For this analysis we train “static” models on time-demeaned data, rather than allowing for treatment effects to vary over time, since time since transfer onset nearby is not well-defined (transfers began in different neighboring villages at different times). This seems reasonable in any case given that our core results are quite stable with respect to time since own transfer receipt. It also allows us to calculate BCa confidence intervals bootstrapping the full procedure, since the static model is less computationally demanding.

Broadly speaking, neither approach reliably predicts heterogeneous effects. Consider first the results of the cross-village exercise (Columns 1–4 of Table F.1). For all three outcomes, the algorithm identifies a most-impacted group that is actually substantially *less* impacted than average, and far less impacted than it is predicted to be. Consequently targeting those predicted to be most impacted actually leads in most cases to less impact than targeting the most deprived. Turning to the within-village exercise for ineligibles (Columns 5–8), we see the same inverted pattern for consumption—which is precisely where the main exercise was *most* successful at identifying high-impact households. For assets as well actual impacts on the most-impacted group are below the overall average (though above that for the most-deprived). Income is the only outcome for which the most-impacted group does meaningfully better than average, and here the confidence intervals do not quite let us reject that they gain as much as the most-deprived. It is certainly possible that, for these ineligible households, there is some close-to-predictable heterogeneity in what they earn from their treated neighbors. And to be clear it is of course entirely possible that such heterogeneity exists more generally. But overall there is little to suggest that the methods and covariates we use are able to detect it. This gives us more confidence that the main results are primarily picking up heterogeneity in direct effects.

What does this then imply for welfare analysis? In the case where the spillover effect onto household h is a function of (at most) the characteristics of the other treated households within its community, the social welfare function for that community is

$$\sum_h W \left(\hat{Y}_h^0 + T_h \cdot \hat{\Delta}_h + \sum_{h'} \hat{S}(X_{h'}) \right) \quad (\text{F.3})$$

When $W(\cdot)$ is our preferred CARA welfare function, one can show that this expression factors as

$$\underbrace{W \left(\sum_{h'} \hat{S}(X_{h'}) \right)}_{\text{Spillover adjustment}} \underbrace{\left[\sum_h W \left(\hat{Y}_h^0 + T_h \cdot \hat{\Delta}_h \right) \right]}_{\text{Original SWF}} \quad (\text{F.4})$$

In other words, social welfare in the presence of spillovers is equal to social welfare in the absence of spillovers multiplied by a term that is a function of the total spillover effect. This gives us an intuitively appealing result: the planner should deviate from the treatment assignment that would be optimal in the absence of spillovers (thus reducing the second term) only if this leads to an increase in aggregate spillovers (thus increasing the first term) that outweighs the decrease in the second term.

This leaves open the question what kinds of household $X_{h'}$ the planner might wish to target in order to generate larger spillovers. Our design does not allow us to identify this type of heterogeneity experimentally, since the rule GiveDirectly used to determine who was eligible for transfers was the same everywhere. And designing an alternative experiment to address this question would be difficult. Even if it were politically feasible to vary the targeting rule—with different eligibility criteria in different villages, say—and measure the impact this had on other households (independent of their own eligibility status), the sample

required to learn to learn informative predictions $\hat{S}(X_{h'})$ would be implausibly large.³⁸ It seems unlikely to us that an approach like this will be feasible in the foreseeable future.

One imperfect but feasible way to explore this issue with our data is to examine whether the spillover effects experienced by ineligible households covary predictably with the characteristics of the eligible households in their village. This is a conservative test in the sense that those characteristics were not experimentally assigned, so that statistically significant relationships might reflect the influence of other unmeasured factors rather than true causal effects of the eligibility rule. Even so, we do not see strong evidence of significant relationships. Table F.2 reports the results of OLS regressions estimated on the sample of ineligibles in which an own-village treatment indicator is interacted with the average values of the predictors among eligibles, as well as own-household characteristics.³⁹ We report the coefficients from the interaction terms, which are the terms of interest. For each of the three economic outcomes, a Wald test of the joint null that none of the interaction terms is significant fails to reject ($p = 0.19$, $p = 0.38$, and $p = 0.18$ for consumption, assets, and income, respectively). The data thus do not provide a strong case that a planner could generate larger spillovers by targeting any particular type of household—though power is of course limited here, since the average characteristics of eligibles do not vary dramatically across villages.

Assuming that experimental tests of the spillovers generated under different targeting rules are unlikely to be available in the foreseeable future, one practicable alternative a planner might consider is to build more economic logic in the analysis. For example, the spillover effects one would expect to get from treating a household with characteristics $X_{h'}$ typically depend on its behavioral response to the transfer, which is easier to measure and predict. This is effectively how the targeting of macroeconomic stimulus packages in high-income countries is analyzed: economists do not attempt to directly estimate how delivering transfers (say) to a given category of household will affect other households, but instead focus on estimating their marginal propensity to consume (MPC), taking this as a sufficient statistic for the impact their spending will then have on the rest of the economy.

In our rural, low-income setting the MPC out of initial transfers is probably not the relevant consideration, since households generally spend essentially all of their transfer purchasing *some* items locally, whether these are consumption goods or tangible assets better thought of as a form of saving or investment. Instead the important source of heterogeneity is likely the different impacts that the same initial transfers have on *subsequent* spending, since it is this spending—purchases in local shops, hiring local workers, etc.—that drives economic spillovers. In other words, the magnitude of the spillovers that treating household h' generates for its neighbors is likely to be directly related to the magnitude of the direct effect of transfers on h' 's economic outcomes (e.g. expenditure).

³⁸For example, consider powering an experiment to detect the spillover effects of changing the eligibility status of a subgroup that makes up 4% of the population, or around 1/10 of the share that was eligible in our setting. Assume (generously) that the spillover effects of treating 40% of one's neighbors are similar to the direct effect of own treatment, which is a generous read of the evidence in Egger et al. (2022). Then we would need an experiment 100 times larger to detect the spillover effects generated by this subgroup than to detect the main effects of treatment, since required sample size is inversely proportional to the square of effect size.

³⁹We use OLS for simplicity here as the GRF framework does not readily lend itself to testing the predictive power of one set of covariates conditional on another set.

If this line of reasoning is correct then it implies that the optimal targeting rule in the presence of spillovers should put even more weight on targeting impact, relative to deprivation, than in the absence of spillovers. Formally, suppose that spillovers are proportional to impact:

$$S(X_{h'}) = s\Delta_{h'} \quad (\text{F.5})$$

for some $s > 0$. Then the objective function (F.4) above becomes

$$\underbrace{W\left(\sum_{h'} s\hat{\Delta}_{h'}\right)}_{\text{Spillover adjustment}} \underbrace{\left[\sum_h W\left(\hat{Y}_h^0 + T_h \cdot \hat{\Delta}_h\right)\right]}_{\text{Original SWF}} \quad (\text{F.6})$$

and we have the result that the planner deviates from the allocation that maximizes welfare without spillovers (i.e. when $s = 0$) only if this leads to a larger average treatment effect.

There are of course a number of caveats to this reasoning, and a comprehensive analysis of all the alternatives is beyond the scope of this paper. The clean multiplicative separability of spillover and direct effects in (F.4), for example, is not preserved under welfare functions other than CARA which require the planner to take into account a complex array of wealth effects. That said, the basic intuition that spillovers in our setting are likely a function of the direct effects, and that optimal targeting policy should therefore put *more* emphasis on targeting impact once spillovers are accounted for, seems likely to us to be robust.

Table F.1: Cross-village and within-village spillover effects

	Cross-village spillover effects on untreated eligible households				Within-village spillover effects on ineligible households			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Statistic	All	Most deprived (D)	Most impacted (I)	Difference (D)-(I)	All	Most deprived (D)	Most impacted (I)	Difference (D)-(I)
<i>Panel A: Consumption</i>								
Predicted	9	5	32	-27	93	90	113	-24
Actual	32	-22	-119	97	147	104	53	50
				(-175,200) [-74,260]				[-158,266]
<i>Panel B: Assets</i>								
Predicted	27	21	40	-19	84	72	107	-36
Actual	27	-12	5	-18	105	17	90	-73
				(-47,191) [-124,100]				[-286,160]
<i>Panel C: Income</i>								
Predicted	101	102	116	-14	-9	-19	18	-37
Actual	110	97	-20	117	25	-32	80	-111
				(-84,210) [5,225]				[-351,110]

Notes: This table reproduces the average treatment effect estimates (Δ_h) of Table 2 for untreated eligible households in control villages (columns 1-4) and ineligible households located within treatment villages (columns 5-8), in order to look at cross-village and within-village spillover effects respectively. For columns 1-4, treatment is defined as whether the share of treated eligible households in a 2 kilometer radius (the instrument used in Egger et al. (2022) to study cross-village spillover effects) for a household is above the median. For columns 5-8, while ineligible households were not treated themselves, we make use of the treatment status of their village to estimate models in the same method as Table 2. Given that there is no clear definition for the timing of treatment for cross-village effects, a non-dynamic version of our procedure was estimated. Moreover, in addition to the conditional bootstrap confidence intervals we report the full procedure bootstrap confidence intervals for the cross-village spillover effects since the static model is less computationally intensive. Standard errors in parentheses are BCa bootstrap confidence intervals for the full procedure, while those in brackets are BCa bootstrap confidence intervals conditional on trained models. $N = 2,367$ for columns 1-4, and $N = 2,434$ for columns 5-8. Monetary values are in USD PPP (2016).

Table F.2: Heterogeneity of spillover effects on ineligible households by average eligible household characteristics

	Consumption	Assets	Income
CATE	-7.20 (16.22)	-5.95 (27.97)	-4.29 (20.70)
HH size	-208.45 (322.59)	95.83 (175.79)	-24.14 (165.61)
Widow	-172.56 (612.29)	256.78 (589.54)	254.29 (531.71)
Female head	5.26 (478.08)	-460.75 (480.68)	325.83 (434.20)
Has children	806.79 (975.39)	-839.39 (754.59)	-360.75 (863.98)
Has children in school	1209.42 (721.41)	1256.62 (539.35)	47.60 (475.23)
Has child under 3	778.25 (748.59)	573.23 (625.73)	1164.69 (642.07)
Has child under 6	58.11 (804.60)	-20.99 (656.28)	-400.01 (677.05)
Has elder member	1390.55 (776.58)	136.11 (688.11)	195.72 (610.72)
Has any livestock	225.98 (734.03)	315.25 (532.22)	-92.83 (454.12)
Owens land	-977.45 (1979.70)	-635.18 (1214.26)	-3025.80 (1597.60)
Owens 1/4 acre	1543.10 (2006.37)	642.13 (1145.72)	3247.20 (1546.84)
Owens TV or radio	-1473.32 (561.67)	-581.40 (406.51)	-9.62 (433.40)
Meals yesterday	69.07 (377.72)	-248.39 (406.06)	33.48 (275.73)
Meals with protein yesterday	271.13 (321.90)	-115.47 (307.88)	-307.27 (429.45)
Self employed	-231.63 (423.52)	-139.32 (519.21)	-500.05 (425.76)
Employed	14.80 (364.24)	-618.73 (406.01)	747.95 (738.26)
Ineligible baselines as controls	Yes	Yes	Yes
Wald test	0.19	0.38	0.18

Notes: This table shows the interaction terms between treatment and average eligible household characteristics in a village in a linear regression that estimates heterogeneous spillovers effects on the ineligible. The models control for the characteristics of the ineligible households, as well as for non-interacted terms. We present tests for the joint significance of the interaction terms excluding the predicted CATE. N = 2413.

G Sensitivity analysis for social welfare: weights

Pareto weights

As discussed in Section 1, the baseline welfare maximization problem we study can readily be extended to include Pareto weights that capture factors other than deprivation (in the sense of low per-capita consumption) which society thinks should influence the allocation of transfers. Information about such social preferences is not available for our Kenyan setting, but is available for otherwise fairly similar communities in Indonesia in which [Alatas et al. \(2012\)](#) document the factors that correlate with community rankings used to determine who received a small cash transfer. In this section we show formally how to use data from the Indonesia study to estimate Pareto weights, and then apply these to our Kenyan setting to see how much they affect the socially optimal allocation.

[Alatas et al. \(2012\)](#) collected data on each households' size n_h , consumption Y_h^0 , other characteristics X_h , and two measures of their priority ranking r_h for receiving transfers: the rank assigned to them in a public village meeting held for that purpose, and the (average) rank assigned to them in private surveys of their neighbors. We interpret the ranking data as resulting from maximization of a social welfare function of the form (3) above, i.e.

$$\max \sum_h \pi(X_h) n_h W(y_h^0 + \Delta/n_h) \quad (\text{G.1})$$

where $\pi(X_h)$ is the Pareto weight the community assigns to households of type X_h and as usual $y_h^0 = Y_h^0/n_h$. Note that we assume homogenous treatment effects, as [Alatas et al. \(2012\)](#) do not estimate heterogenous effects.

To estimate $\pi(X_h)$ from the ranking data, let

$$U_h^* \equiv \pi(X_h) n_h [W(y_h^0 + \Delta/n_h) - W(y_h^0)] \quad (\text{G.2})$$

be the increment to social welfare obtained by targeting household h . The observed rank r_h is then the rank of this unobserved latent variable. Let $W(\cdot)$ have the CARA form with coefficient α as above, and specify the weights using the parametric form

$$\pi(X_h) \equiv e^{X_h \beta} \quad (\text{G.3})$$

Taking logarithms and introducing an error term to capture the influence of any unobserved factors, we can then write $u_h^* \equiv \log(U_h^*)$ as

$$u_h^* = \log(1 - e^{-\alpha \Delta/n_h}) + \log n_h - \alpha y_h^0 + X_h \beta + \epsilon_h \quad (\text{G.4})$$

Since we are interested primarily in the β 's, we estimate models based on a simplified version of this equation in which household-size fixed effects $\{\delta_{n_h}\}$ absorb the first two terms, leaving us with a linear latent variable:

$$u_h^* = \delta_{n_h} - \alpha y_h^0 + X_h \beta + \epsilon_h \quad (\text{G.5})$$

If ϵ_h follows an extreme-value type I distribution this is the ranked-order logit model of [Beggs et al. \(1981\)](#), and can be estimated using standard packages. We include in X_h the variables that were influential in the Indonesian community rankings (see Table 12 of [Alatas et al.](#)) and which we also have as PMT-like predictors in our Kenyan data. In practice there are two such variables: an indicator for whether the household has any children, and one for whether the household is headed by a widow.

Table [G.1](#) reports estimates $(\hat{\alpha}, \hat{\beta})$ of the key parameters as well as the implied weights $e^{X\hat{\beta}}$. Whether we use the community rankings elicited in public or the peer rankings elicited in private, the pattern that emerges is very similar. The estimates of $\hat{\alpha}$ are roughly 0.0004, at the lower end of the range we consider when analyzing the Kenya data but significantly different from zero. Per-capita consumption is not the only factor taken into account, however; conditional on this, having children or being headed by a widow also significantly predict allocations (consistent with the reduced-form linear regressions in Table 12 of [Alatas et al.](#)). Interpreted as Pareto weights, the effect sizes are substantial: all else equal, households with children receive 17%–29% higher weight than those without, depending on which ranking we use, and households headed by widows receive 47%–60% higher weight.

With these results in hand we now return to our Kenya data and examine how much they affect the welfare-maximizing allocation of transfers, now taking into status as a widow as well as heterogeneity in impact and deprivation. Specifically, we find the allocation that solves the weighted social welfare maximization problem (3) using the weights estimated from Indonesia and for a range of values of α including the estimated value (i.e. 0.0004) as well as some of the others we consider in our main results.

We find that upweighting widows in this way has noticeable effects on targeting, but does not alter the conclusion that optimal targeting depends on impact as well as deprivation. Table [G.2](#) illustrates this, reporting the overlap between the groups selected and the benchmark most deprived and most impacted groups. For any given value of α a larger share of the most deprived and a smaller share of the most impacted are selected relative to the baseline scenario in Table 1. At the same, the share of the most deprived selected is always less than 50%, and the share of the most impacted selected always substantially greater than 50%.

Generalized social marginal welfare weights

As an alternative to social welfare maximization as in (1), [Saez and Stantcheva \(2016\)](#) propose evaluating policies according to a weighted sum of the money metric gains and losses they induce for different individuals. In this approach the weights could capture a wide variety of considerations, including deprivation in the sense of low per-capita consumption as well as any other factors that society deems relevant. In our setting and using our notation (and suppressing time variation for expositional simplicity), this would mean allocating transfers to maximize

$$\sum_h \pi(n_h, y_h^0, X_h) \Delta_h \tag{G.6}$$

They emphasize that this approach is reasonable when the changes Δ_h are small, which is certainly not the case in our setting—transfers and treatment effects were very large relative to annual consumption, for example. It is nevertheless interesting to consider how different the solution to (G.6) is to the one we get from standard welfare maximization.

The key issue is of course how to determine the weights $\pi(\cdot)$. [Saez and Stantcheva \(2016\)](#) provide several examples in which one can construct defensible weights—by inverting actual tax policies which are hard to reconcile with standard welfare maximization, for example—but do not provide any general theory of the weights which we can apply here. In light of the analysis above, however, it is natural to wonder what results we obtain if we estimate marginal weights using the Indonesia data, i.e. interpreting the ranking data as resulting from maximization of [\(G.6\)](#).

To that end, again set $\Delta_h = \Delta$ and specify an exponential form for π . To facilitate comparability between this exercise and the previous one, let

$$\pi(n_h, y_h^0, X_h) = \exp\{\tilde{\delta}_{n_h} - \alpha y_h^0 + X_h \beta\} \quad (\text{G.7})$$

This means that (after adding an error term) the latent variable determining the order in which households are ranked is again [\(G.5\)](#), just as before (with $\delta_n = \tilde{\delta}_n + \log \Delta$). We therefore work with the parameter estimates from [Table G.1](#) as before. The key difference lies in how we apply these estimates to the Kenyan data; now (i) all the terms in [\(G.5\)](#), not just $X_h \beta$, are used to calculate welfare weights, and (ii) households are ranked for inclusion according to [\(G.6\)](#) rather than [\(1\)](#). Notice that in this approach the same factors that mattered above—household size, per capita income, and the other characteristics in X_h —are still allowed to influence the allocation, but their influence now works solely through the marginal weights.

The results ([Table 1](#), Panel C, final row) are quite similar to those we obtain from the Pareto weights approach above (penultimate row). Specifically, the share of the most deprived selected is 42%, and the share of the most impacted selected is 77%—almost exactly the figures we obtain when using Pareto weights and the value $\alpha = 0.0004$ estimated from the Indonesian data.

Table G.1: Pareto Weights using Alatas et al. (2012) data

	(1) Community ranking	(2) Survey ranking
Indicator for having children	0.161 (0.0928)	0.257 (0.0603)
Indicator for widow	0.472 (0.138)	0.383 (0.100)
Annual per-capita consumption (100s PPP), winsorized	-0.0412 (0.00376)	-0.0413 (0.00256)
HH size FEs	Yes	Yes
Implied weights ($\exp(X\beta)$)		
Has children	1.174	1.293
Widow	1.602	1.466
N Obs	1637	3429

Notes: This table presents estimates of a rank-order logit model of the parameters in equation G.5 with data from Alatas et al. (2012). Standard errors in parentheses.

Table G.2: Overlap of socially optimal households to target with most deprived and most impacted, Pareto weights for widow indicator

	(1)	(2)	(3)
CARA: α	CE	Most deprived	Most impacted
<i>Panel A: Consumption</i>			
0.0000	\$50	0.44	0.74
0.0004	\$50	0.50	0.69
0.0010	\$49	0.59	0.61
<i>Panel B: Assets</i>			
0.0000	\$50	0.43	0.71
0.0004	\$50	0.53	0.65
0.0010	\$49	0.65	0.56
<i>Panel C: Income</i>			
0.0000	\$50	0.53	0.87
0.0004	\$50	0.56	0.84
0.0010	\$49	0.59	0.82

Notes: This table reproduces Table 1 with pareto weights estimated with data from [Alatas et al. \(2012\)](#) for various values of α and various outcomes. For additional details, please see Table 1.