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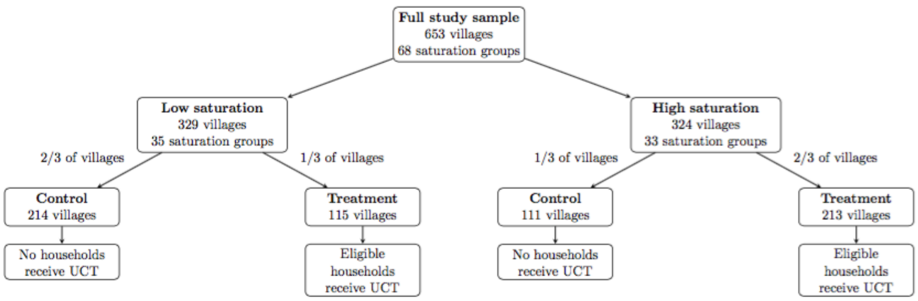
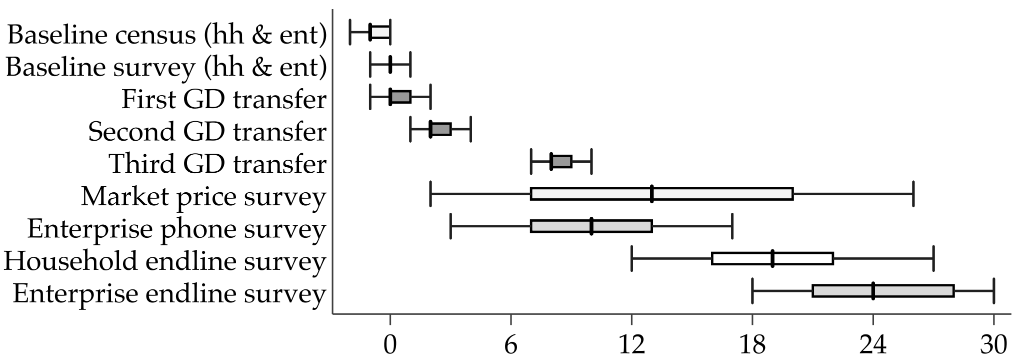
A Supporting figures & tables

Figure A.1: Study design and timeline

(a) Randomization

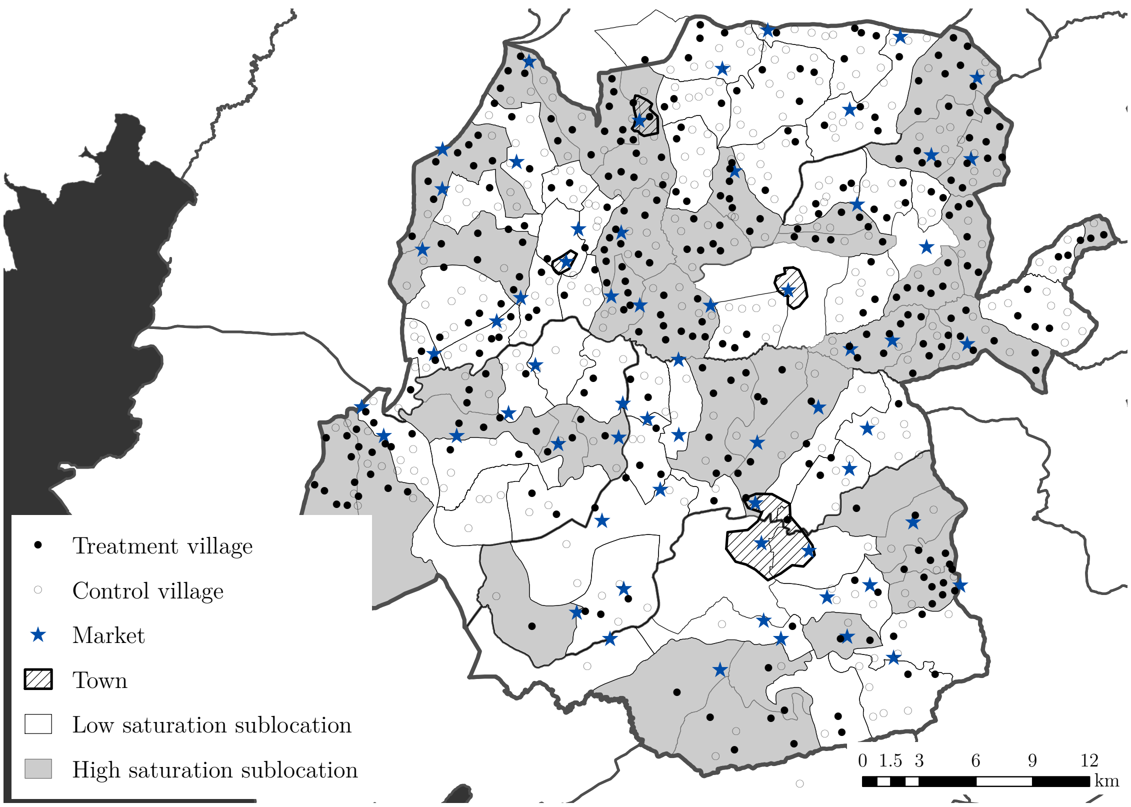


(b) Timing relative to experimental start



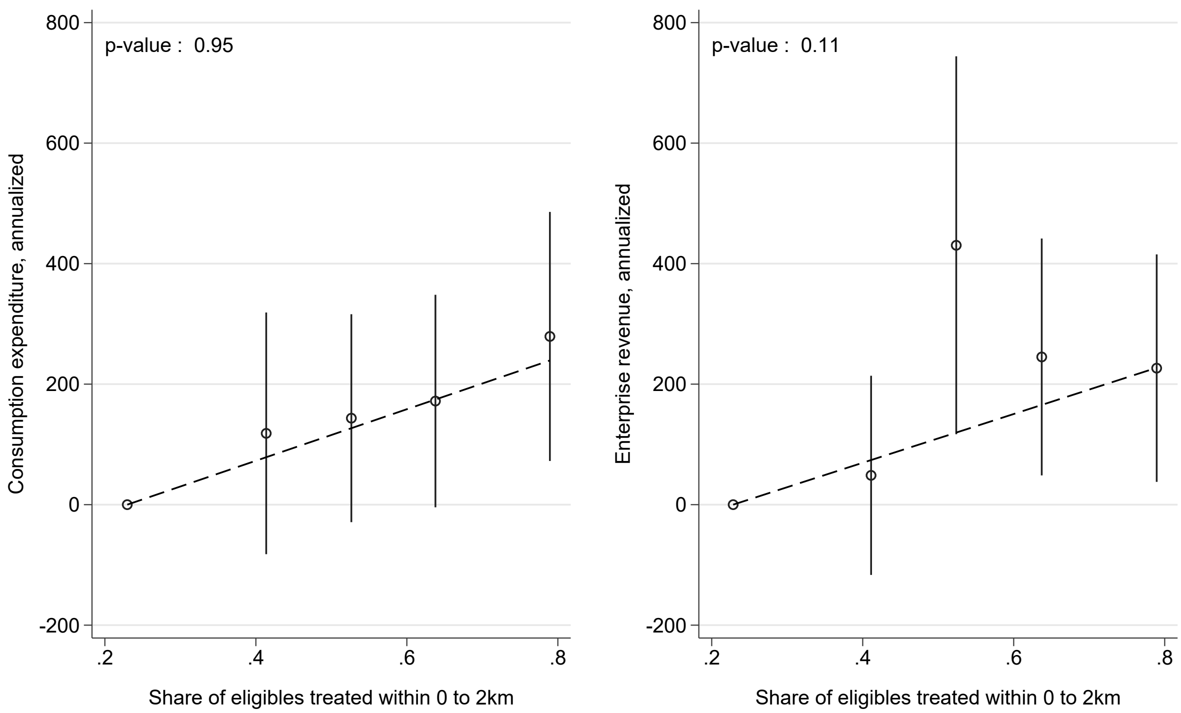
Notes: Panel A illustrates the two-level randomized controlled trial experimental design. 653 villages were grouped into 68 saturation groups based on the sublocation (the administrative unit directly above the village level) in which they are located. Saturation groups were then randomly assigned to either high or low saturation status. In the 33 high saturation groups, two-thirds of villages were assigned to treatment status, while in the 35 low saturation groups, one-third of villages are assigned to treatment status. In the 328 treatment villages, all eligible households received an unconditional cash transfer, while no households within control villages received a transfer. Panel B plots the 5th, 25th, 50th, 75th and 95th percentiles of study activities. Timing is reported relative to the anticipated start of activities in each village (the "experimental start"). The experimental start for a village is calculated based on the random ordering of treatment and control villages that both GD and research team field enumerators worked in, as well as GD's mean monthly pace of enrolling villages in the subcounty in which the village is located. As markets were not assigned to treatment, we use the first date transfers were distributed within the subcounty in which the market is located. The value of the first GD transfer is USD 151 PPP, while the second and third are both USD 860 PPP.

Figure A.2: Study area



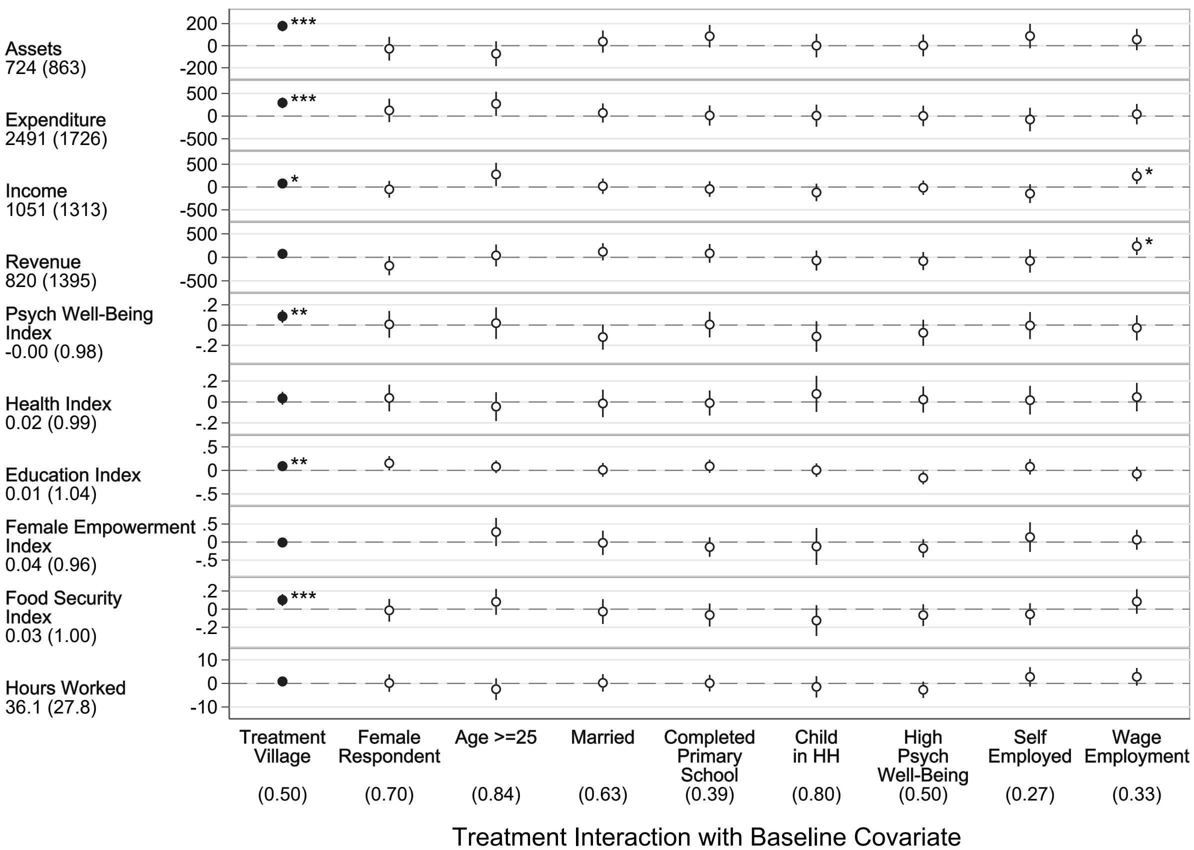
Notes: This figure plots study villages, sublocation boundaries, and weekly markets in the study area in Siaya County, Kenya. Control villages are denoted by hollow circles, treatment villages are denoted by solid circles, and blue stars indicate the locations of markets. High saturation sublocations are shaded in gray, while low saturation sublocations are those in white. Town boundaries are shaded with diagonal lines.

Figure A.3: Non-linear Spillover Estimates



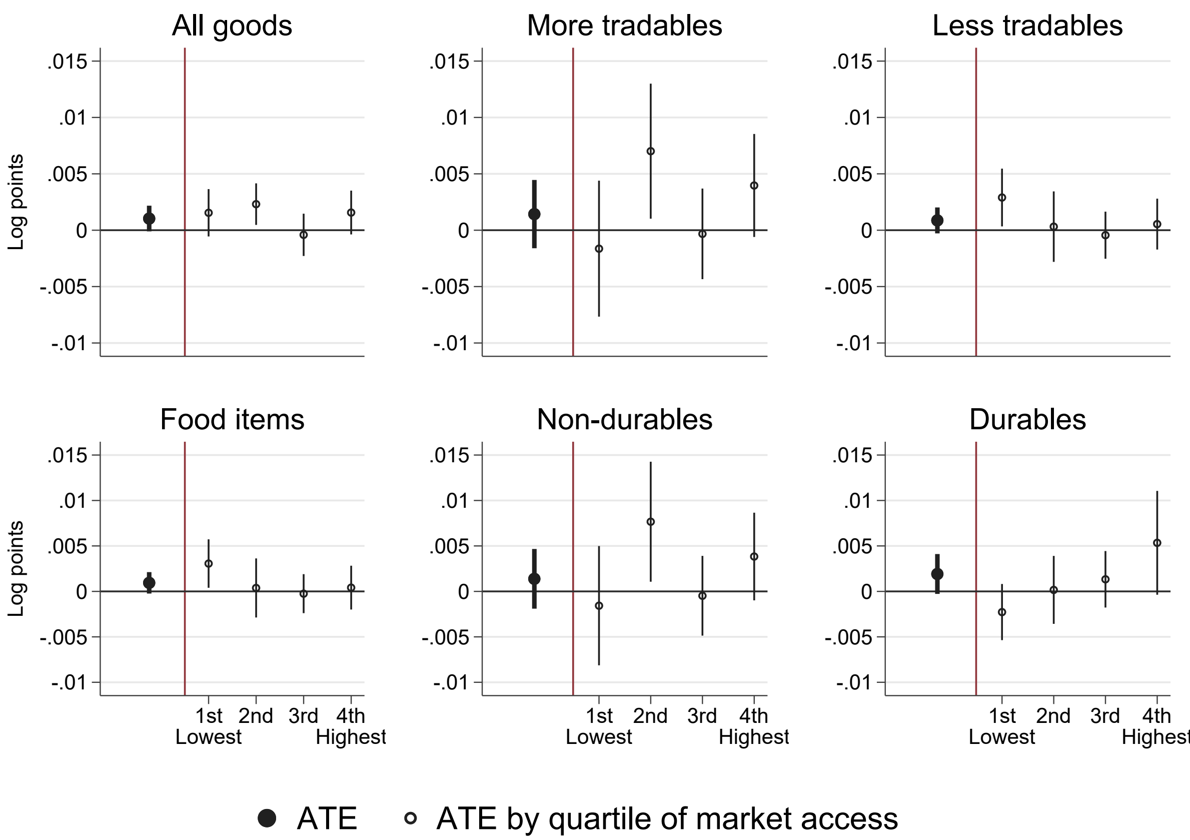
Notes: Each panel represents a reduced-form regression of household consumption and enterprise revenue on 5 quintile bins of the share of eligibles assigned to treatment 2km around each household / enterprise. Effects are relative to the lowest quantile. For consumption, we control for direct effects by including eligibility and a dummy for treatment status of each household. For enterprises, we include interactions with dummies for 3 enterprise types (within homestead, outside homestead, and own-farm). We then sum and weight coefficients to obtain total revenue effects per household in our study area. We control for baseline revenue at the village-by-enterprise type level and use inverse sampling weights. 95% confidence intervals are obtained using Conley (2008). Dashed lines start at zero, the slope coming from the same regression, with quantile bins of treatment intensity replaced by a linear term (and weighted across enterprise types as above). We cannot formally reject that our estimated non-linear regression is linear, i.e. that $\frac{\beta_2-\beta_1}{\Delta X_2} = ... = \frac{\beta_n-beta_{n-1}}{\Delta X_n}$. The p-values of this test are 0.95 and 0.11 for consumption and revenue respectively. We did the same test for all 10 pre-specified primary outcomes and treated / untreated households separately; we cannot reject linearity at the 10% level for any of them.

Figure A.4: Little heterogeneity in pre-specified primary outcomes



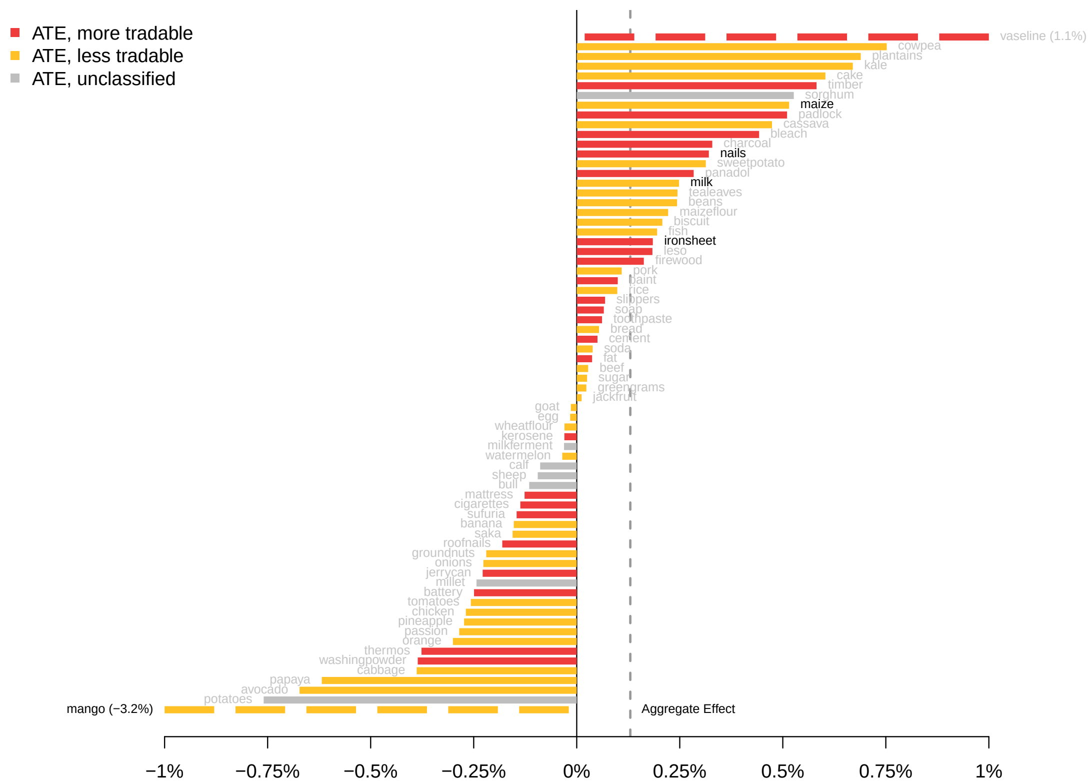
Notes: This figure presents estimates for treatment effect heterogeneity for eligible households in pre-specified primary outcomes along 8 pre-specified dimensions of heterogeneity (Haushofer et al. 2017a). Each plotted coefficient is from a separate regression. Each row represents a separate primary outcome; the mean (SD) for eligible households in control, low saturation villages is reported below the outcome label. The first column (Treatment Village) plots estimated effects for the coefficient on an indicator for being in a treatment village from Equation (1), where the sample is restricted to eligible households. Columns 2 through 8 plot the coefficient on the interaction term of the listed baseline covariate with the treatment village indicator; this interaction term and baseline covariate are added to Equation (1). Values in parentheses on the x-axis denote the mean of the baseline covariate. Standard errors are clustered at the village level. Reported significance levels correspond to FDR q-values, calculated following Benjamini, Krieger, and Yekutieli (2006). * denotes significance at 10 pct., ** denotes significance at 5 pct., and *** denotes significance at 1 pct. level.

Figure A.5: Output price effects by market access



Notes: Each panel represents a regression of the logarithm of a price index on the "optimal" number of lags and distance buffers of per capita GiveDirectly transfers in each buffer, as calculated for the overall price index. The number of radii bands and lags is chosen sequentially by minimizing the BIC, as pre-specified, for the overall price index. Regressions include a full set of market and month fixed effects. We report the implied ATE, calculated by evaluating the "optimal" regression specification at the average level of treatment intensity between September 2014 and March 2017, the time during which transfers went out. Bold markers correspond to the ATE across all markets. Hollow markers break down this average by quartiles of market access (with low market access referring to more remote markets), defined as $MA_m = \sum_{r=1}^{10} r^{-\theta} N_r$, where $\theta = 8$ and N_r is the population in in the $r-1$ to r km buffer around each market. Bars represent 95% confidence intervals based on standard errors as in Conley (2008), where we allow for spatial correlation up to 10km and autocorrelation up to 12 months.

Figure A.6: Output price effects at the product level



Notes: Each bar represents a regression of the logarithm of a median price index for each good, using a 4km distance buffer and no lags (the "optimal" number of lags and distance buffers of per capita GiveDirectly transfers for the overall price index). The number of radii bands and lags is chosen sequentially by minimizing the BIC, as pre-specified. Regressions include a full set of market and month fixed effects. Colors denote our classification into more tradable vs. less tradable goods. For each good, we report the implied ATE, calculated by evaluating the regression specification at the average level of treatment intensity between September 2014 and March 2017, the time during which transfers went out. Bold product names indicate significance at the 95% level.

Table A.1: Household Assets by Productivity Status

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Assets (non-land, non-house), net borrowing	178.09***	182.01***	132.63*	1,132.15
	(24.61)	(44.25)	(78.32)	(1,420.22)
Productive Agricultural Assets	4.26***	4.16**	−0.38	32.51
	(0.93)	(1.96)	(2.47)	(38.94)
Potentially Productive Assets	89.97***	52.78	35.86	700.68
	(25.86)	(49.31)	(65.83)	(1,025.41)
Livestock Assets	50.61***	44.82	−7.01	462.24
	(17.04)	(27.90)	(35.76)	(723.43)
Non-Ag Assets	37.03***	24.60	25.63	219.04
	(10.43)	(22.85)	(23.15)	(424.05)
Non-Productive Assets	78.81***	92.59***	52.33*	449.30
	(9.28)	(14.28)	(29.60)	(468.64)

Notes: This table presents results on household asset ownership based on classifications of assets by productivity status. Productive agricultural assets include agricultural tools. Potentially productive assets include livestock and non-agricultural assets, made up of the following: bicycle, motorcycle, car, boat, kerosene stove, sewing machine electric iron, computer, mobile phone, car battery, solar (panels or system), and generators. Non-productive assets include: radio/cd player, kerosene lantern, bed, mattress, bednet, table, sofa, chair, cupboards, clock, television, iron sheets. Column 1 reports the coefficient on an indicator for treatment village from a regression using data from eligible households (as classified by the GE census team), and includes an indicator for saturation status of the sublocation (Equation 1). Column 2 reports the total effect on treated households (eligible recipients) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a household's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the household (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer), as in Equation 2). For this analysis, the sample is restricted to eligible households, including 5,420 observations. Column 3 presents the average spillover effect on eligible households in control villages as well as ineligible households, coming from a stacked spatial regression of each outcome on the amount transferred per capita GDP to each 2km radii band around each household (instrumented by the share of eligibles assigned to treatment in each buffer), as in Equation 3). We have 5,505 observations. The reported average effect comes from a population-share-weighted average effect experienced by those two groups, and is representative of the average untreated household. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC for total assets (Row 1). Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across eligible and ineligible households). Each regression is weighted by inverse sampling weights and contains baseline values of the outcome when available. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.2: Enterprise revenue effects by sector

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Retail revenue	70.95*	140.92**	65.14	236.20
	(42.22)	(70.75)	(57.15)	(415.34)
Manufacturing revenue	−30.40	109.34**	109.58**	81.27
	(50.46)	(51.97)	(55.57)	(177.27)
Services revenue	−77.63*	17.77	52.70*	115.19
	(40.10)	(43.30)	(28.93)	(175.93)
Agriculture revenue	20.64	41.78*	16.38	308.18
	(13.91)	(23.95)	(19.83)	(365.39)

Notes: Column 1 reports the coefficient on an indicator for treatment village, and includes an indicator for saturation status of the sublocation (Equation 10). Column 2 reports the total effect on enterprises in treatment villages (own-village effect plus across-village spillover) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a enterprise's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the enterprise (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer). Column 3 reports the total effect on enterprises in control villages (across-village spillover only). For non-agricultural sectors (retail, services and manufacturing), we stack 2 separate regressions for non-agricultural enterprises operated within the household, and non-agricultural enterprises operated outside the household, due to our independent sampling across these enterprise categories (as in Equation 11). We have 1,300 observations for retail enterprises, 576 for manufacturing, 400 for services and 7,896 for agriculture. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across all enterprise categories). Each regression is weighted by inverse sampling weights and contains village-level baseline averages of the outcome variable by enterprise category when available. We convert effects to a per-household level by multiplying the average effect per enterprise in each enterprise category by the number of enterprises in that category, dividing by the number of households in our study area, and summing over all enterprise categories. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.3: Enterprise outcomes by owner eligibility

	(1)	(2)	(3)	(4)
	Recipient Owners		**Non-Recipient Owners**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Panel A: All enterprises				
Enterprise profits, annualized	12.03	29.36	49.61**	323.39
	(13.44)	(22.01)	(24.30)	(691.12)
Enterprise revenue, annualized	71.90***	148.44	197.78***	758.52
	(26.40)	(97.10)	(51.67)	(2,493.40)
Enterprise costs, annualized	29.55***	46.83***	45.82**	147.73
	(9.98)	(15.41)	(18.37)	(550.11)
Enterprise wagebill, annualized	25.29***	43.83***	43.63**	120.62
	(9.17)	(14.40)	(17.73)	(492.11)
Enterprise profit margin	−0.04**	−0.07**	−0.03	0.44
	(0.02)	(0.03)	(0.03)	(0.61)
Panel B: Non-agricultural enterprises				
Enterprise inventory	2.88	7.74	5.58	192.98
	(2.79)	(7.33)	(5.56)	(504.76)
Enterprise investment, annualized	−5.15	−15.61	5.50	178.25
	(5.35)	(16.17)	(8.25)	(640.98)
Panel C: Village-level				
Number of enterprises	0.00	0.03***	0.00	1.12
	(0.01)	(0.01)	(0.01)	(0.14)

Notes: Column 1 reports the coefficient on an indicator for treatment village, and includes an indicator for saturation status of the sublocation, among matched eligible enterprise owners (Equation 10). Column 2 reports the total effect on enterprises with a treated owner relative to eligible owners in control villages (own-village effect plus across-village spillover) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a enterprise's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the enterprise (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer). We have between 5,424 and 5,555 owner-matched observations for all enterprises, and 415 for non-ag outcomes. Column 3 reports the total effect on enterprises with untreated owners (spillover only), where we have between 6,584 to 6,739 observations for all enterprises, and 1,454 to 1,459 for non-ag outcomes. For each column, we stack 3 separate regressions for own-farm enterprises, non-agricultural enterprises operated within the household, and non-agricultural enterprises operated outside the household, due to our independent sampling across these enterprise categories (Equation 11. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across all enterprise categories). Each regression is weighted by inverse sampling weights and contains village-level baseline averages of the outcome variable by enterprise category when available. We convert effects to a per-household level by multiplying the average effect in each enterprise category by the number of enterprises in that category, dividing by the number of households in our study area, and summing over all enterprise categories. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.4: Input prices and quantities: additional labor supply outcomes

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Panel A: Quantities				
Household hours worked on own farm	2.07*	0.98	−6.28**	35.33
	(1.15)	(2.30)	(2.61)	(38.81)
Individual hours worked in self-employment	1.80	4.23**	−1.37	26.80
	(1.14)	(1.96)	(1.76)	(23.54)
Individual hours employed last week	0.52	−1.38	2.49	23.62
	(0.98)	(2.32)	(2.67)	(25.96)
Individual hours employed last week in agriculture	−1.54***	−2.28***	0.33	6.01
	(0.56)	(0.75)	(1.11)	(12.78)
Individual hours employed last week not in agriculture	1.67	0.62	1.91	17.09
	(1.03)	(2.31)	(2.65)	(26.41)
Panel B: Prices				
Hourly wage earned by employees	0.11***	0.04	0.19*	0.70
	(0.03)	(0.04)	(0.10)	(0.89)
Hourly wage earned by employees in agriculture	0.15**	0.21**	−0.06	0.67
	(0.06)	(0.08)	(0.13)	(0.67)
Hourly wage earned by employees not in agriculture	0.04	0.08	0.20	1.09
	(0.08)	(0.10)	(0.23)	(1.45)

Notes: Column 1 reports the coefficient on an indicator for treatment village from a regression using data from eligible households (as classified by the GE census team), and includes an indicator for saturation status of the sublocation (Equation 1). Column 2 reports the total effect on treated households (eligible recipients) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a household's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the household (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer), as in Equation 2. For this analysis, the sample is restricted to eligible households, including between 5,420 observations at the household level, and between 1,201 and 4,085 observations for individual-level outcomes. Column 3 presents the average spillover effect on eligible households in control villages as well as ineligible households, coming from a stacked spatial regression of each outcome on the amount transferred per capita GDP to each 2km radii band around each household (instrumented by the share of eligibles assigned to treatment in each buffer), as in Equation 3. We have between 5,505 household observations, and between 1,019 and 3,486 individuals. The reported average effect comes from a population-share-weighted average effect experienced by those two groups, and is representative of the average untreated household. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across eligible and ineligible households). Each regression is weighted by inverse sampling weights and contains baseline values of the outcome when available. In addition, prices are quantity-weighted. That is, wages are weighted by the number of hours worked. Standard errors are clustered at the sublocation in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.5: Input prices and quantities: additional land outcomes

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Panel A: Quantities				
Acres of land owned	−0.19	−0.10	0.08	1.42
	(0.14)	(0.09)	(0.10)	(2.37)
Acres of land rented out	−0.04	−0.05	0.06	0.93
	(0.11)	(0.21)	(0.18)	(0.91)
Acres of land rented in	0.03	0.04	0.08	0.70
	(0.03)	(0.06)	(0.07)	(0.64)
Acres of land used for crops	0.03	−0.03	0.09	0.96
	(0.02)	(0.04)	(0.06)	(1.18)
Panel B: Prices				
Land price per acre	166.84	365.44	556.83	3,952.86
	(201.20)	(290.86)	(412.34)	(3,148.52)
Monthly land rental price per acre	−0.05	−0.02	1.80	9.71
	(0.56)	(0.96)	(1.41)	(8.33)
Total ag land rental costs	6.95***	8.97*	10.14	51.76
	(2.47)	(5.21)	(9.39)	(39.67)

Notes: Column 1 reports the coefficient on an indicator for treatment village from a regression using data from eligible households (as classified by the GE census team), and includes an indicator for saturation status of the sublocation (Equation 1). Column 2 reports the total effect on treated households (eligible recipients) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a household's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the household (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer), as in Equation 2. For this analysis, the sample is restricted to eligible households, including between 352 and 5,418 observations (indicating land markets are often thin). Column 3 presents the average spillover effect on eligible households in control villages as well as ineligible households, coming from a stacked spatial regression of each outcome on the amount transferred per capita GDP to each 2km radii band around each household (instrumented by the share of eligibles assigned to treatment in each buffer), as in Equation 3. We have between 348 and 5,505 observations. The reported average effect comes from a population-share-weighted average effect experienced by those two groups, and is representative of the average untreated household. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across eligible and ineligible households). Each regression is weighted by inverse sampling weights and contains baseline values of the outcome when available. In addition, prices are quantity-weighted. That is, land prices and rental rates are weighted by land size. Standard errors are clustered at the sublocation in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.6: Non-market Outcomes and Externalities

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Psychological well-being index	0.09***	0.12*	0.08	0.01
	(0.03)	(0.07)	(0.06)	(1.01)
Health index	0.04	0.06	0.01	0.03
	(0.03)	(0.06)	(0.05)	(1.01)
Food security index	0.10***	0.05	0.08	0.01
	(0.03)	(0.07)	(0.06)	(1.00)
Children food security	0.13***	0.17**	0.09	−0.04
	(0.04)	(0.08)	(0.09)	(1.12)
Education index	0.09**	0.09*	0.10*	0.01
	(0.04)	(0.05)	(0.06)	(1.02)
Female empowerment index	−0.01	0.08	0.09	0.05
	(0.07)	(0.14)	(0.15)	(0.94)
Security index	0.11***	−0.02	−0.02	0.03
	(0.04)	(0.07)	(0.07)	(0.96)

Notes: Outcome indices in each row are calculated as weighted, standardized indices of multiple survey questions, as described in detail in Appendix C.1. Column 1 reports the coefficient on an indicator for treatment village from a regression using data from eligible households (as classified by the GE census team), and includes an indicator for saturation status of the sublocation (Equation 1). Column 2 reports the total effect on treated households (eligible recipients) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a household's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the household (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer), as in Equation 2. For this analysis, the sample is restricted to eligible households, including between 4,121 and and 5,423 observations (and a subset of 1,118 for female empowerment). Column 3 presents the average spillover effect on eligible households in control villages as well as ineligible households, coming from a stacked spatial regression of each outcome on the amount transferred per capita GDP to each 2km radii band around each household (instrumented by the share of eligibles assigned to treatment in each buffer), as in Equation 3. We have between 4,048 and 5,509 observations (and a subset of 978 for female empowerment). The reported average effect comes from a population-share-weighted average effect experienced by those two groups, and is representative of the average untreated household. The number of radii bands included in Columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across eligible and ineligible households). Each regression is weighted by inverse sampling weights and contains baseline values of the outcome when available. Standard errors are clustered at the sublocation in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.7: Inequality

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Panel A: Expenditure				
Gini coefficient	0.7	0.8	0.2	32.4
	(0.7)	(1.3)	(1.1)	(7.8)
Counterfactual Gini coefficient	−1.1*	−2.2	0	32.4
	(0.7)	(1.3)		(7.8)
P-value: effect = counterfactual effect	p=0.08	p=0.05	p=0.84	
Panel B: Assets				
Gini coefficient	−1.1	2.1	2.8**	45.4
	(0.9)	(1.6)	(1.4)	(10.1)
Counterfactual Gini coefficient	−7.5***	−6.9***	0	45.5
	(0.8)	(0.5)		(10.8)
P-value: effect = counterfactual effect	p=0.00	p=0.00	p=0.04	

Notes: This table reports results on village level inequality as measured by Gini coefficients (0-100). Panel A presents expenditure-based Gini coefficients and Panel B presents assets-based Gini coefficients. For each panel, the first row presents results on actual Gini coefficients measured from our data. The second row estimates the same specifications as the first row, but using counterfactual Gini coefficients assuming that only recipient households gained from the cash transfers, and untreated households experienced no spillovers. We construct a hypothetical consumption expenditure and assets distribution from its baseline distribution (for assets) or by imputing a baseline distribution based on endline non-missing values in control and low-saturation villages (for expenditure). We add in the associated gain, assuming recipients spend 66% of the transfer on consumption, and 34% on assets, following the relative magnitude of the point estimates on expenditure and assets in Table 1. This is also in line with our preferred dynamic MPC estimates, where we find recipients spent 93% of the transfer in the first 29 months, 63% on non-durables and 30% on durable assets (see Appendix 4.2 for details). The p-value reported in the third row tests if the actual effect (Row 1) equals the counterfactual effect (Row 2). Gini estimates and effect estimates are weighted by inverse sampling probabilities and village size. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.8: Expenditures, Savings and Income: Extended version

	Recipient households		**Non-recipient households**			
	(1)	(2)	(3)	(4)	(5)	(6)
	1(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control Eligibles	Ineligibles	Control, low-saturation mean (SD)
Panel A: Expenditure						
Household expenditure, annualized	292.98***	338.16***	333.73***	21.11	410.25***	2,536.86
	(60.09)	(109.36)	(123.24)	(83.76)	(147.86)	(1,934.09)
Non-durable expenditure, annualized	186.96***	226.74**	316.62***	24.77	388.06***	2,471.49
	(58.55)	(99.62)	(119.79)	(79.04)	(144.91)	(1,877.82)
Food expenditure, annualized	71.61*	133.55**	132.84**	10.64	162.75**	1,578.43
	(36.93)	(63.98)	(58.58)	(50.09)	(71.29)	(1,072.31)
Temptation goods expenditure, annualized	6.51	5.88	−0.71	10.65	−3.49	37.10
	(5.79)	(8.82)	(6.50)	(8.02)	(7.80)	(123.59)
Durable expenditure, annualized	95.18***	109.07***	8.41	5.68	9.07	59.44
	(12.64)	(20.23)	(12.50)	(16.83)	(14.99)	(230.90)
Panel B: Assets						
Assets (non-land, non-house), net borrowing	178.09***	182.01***	132.63*	−12.39	168.13*	1,132.15
	(24.61)	(44.25)	(78.32)	(39.90)	(98.03)	(1,420.22)
Housing value	372.78***	480.68***	72.58	27.18	83.69	2,033.72
	(25.25)	(38.88)	(215.70)	(37.31)	(268.14)	(5,030.37)
Land value	50.86	153.09	572.07	186.11	666.54	5,030.72
	(186.08)	(262.48)	(458.28)	(290.34)	(543.51)	(6,607.61)
Panel C: Household balance sheet						
Household income, annualized	77.62*	134.02	229.46***	81.25	265.74**	1,023.45
	(43.66)	(93.83)	(88.59)	(59.37)	(108.12)	(1,634.70)
Net value of household transfers received, annualized	−1.68	−7.44	8.75	−6.83	12.56	130.18
	(6.81)	(13.06)	(19.10)	(10.27)	(23.18)	(263.75)
Tax paid, annualized	1.95	−0.09	1.66	−0.92	2.29	16.93
	(1.28)	(2.02)	(2.02)	(1.65)	(2.39)	(36.51)
Profits (ag & non-ag), annualized	24.70	33.73	44.08	−6.23	56.39	485.20
	(23.18)	(48.95)	(45.35)	(37.99)	(56.34)	(787.10)
Wage earnings, annualized	42.51	73.72	182.99***	90.02**	205.75**	495.37
	(32.24)	(60.83)	(65.44)	(39.12)	(80.09)	(1,231.56)

Notes: See Table 1 for a description of Columns 1 to 3 and 6. Columns 4 and 5 break out the total effects from Column 3 separately for eligible households in control villages and ineligible households (in both treatment and control villages), respectively. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 - 5. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table A.9: Expenditures, savings and income results excluding respondents that migrated

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	𝟙(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Panel 1: Full Sample				
Respondent migrated	0.01	−0.03	0.00	0.05
	(0.01)	(0.02)	(0.01)	(0.21)
Net change in household members since baseline	0.03	0.02	−0.18**	−0.10
	(0.04)	(0.07)	(0.08)	(1.30)
Household size	0.02	0.03	−0.10	4.05
	(0.05)	(0.08)	(0.08)	(2.35)
Panel 2: Non-Migrant Sample				
Panel 2.A: Expenditure				
Household expenditure, annualized	311.86***	376.53***	325.42***	2,511.93
	(61.09)	(109.97)	(124.71)	(1,927.10)
Non-durable expenditure, annualized	200.13***	260.62***	307.06**	2,445.76
	(59.08)	(99.57)	(121.09)	(1,869.34)
Food expenditure, annualized	80.71**	152.03**	123.95**	1,572.86
	(37.98)	(67.87)	(57.68)	(1,069.81)
Temptation goods expenditure, annualized	4.45	1.89	−0.63	37.93
	(6.15)	(8.97)	(6.71)	(125.55)
Durable expenditure, annualized	102.17***	113.42***	8.54	60.04
	(13.19)	(21.60)	(12.85)	(231.72)
Panel 2.B: Assets				
Assets (non-land, non-house), net borrowing	174.46***	172.13***	136.47*	1,145.65
	(25.24)	(47.36)	(80.18)	(1,414.90)
Housing value	399.83***	478.90***	37.51	2,097.78
	(26.38)	(39.92)	(222.81)	(5,133.46)
Land value	52.15	83.35	559.27	5,141.46
	(193.27)	(286.26)	(471.63)	(6,687.63)
Panel 2.C: Household balance sheet				
Household income, annualized	37.82	84.56	201.11**	993.03
	(43.18)	(93.36)	(91.13)	(1,600.50)
Net value of household transfers received, annualized	0.68	−10.88	10.52	135.91
	(7.03)	(14.56)	(19.75)	(266.53)
Tax paid, annualized	1.59	−0.94	1.41	16.66
	(1.32)	(2.13)	(2.00)	(35.72)
Profits (ag & non-ag), annualized	11.55	−2.98	23.14	488.95
	(23.08)	(51.93)	(45.16)	(786.46)
Wage earnings, annualized	16.07	67.20	175.65***	461.17
	(31.58)	(58.98)	(65.43)	(1,185.26)

Notes: Panel 1 presents estimates of migration impacts on 3 indicators of migration: Whether the respondent themselves migrated out of the study area, the net change in household members since baseline, and the endline household size. Panel 2 reports results from Table 1 for respondents that have not migrated, where migration is defined as living in another administrative sublocation for over 4 months. See Table 1 for a descriptions of Columns 1-4. In Panel A, we have between 5,403 and 5,422 observations for columns 1-2 and 5,489 and 5,508 for column 3. In Panels B and C, we have 4,982 to 5,024 observations in columns 1-2 and 5,170 to 5,220 observations in column 3. Standard errors are clustered at the village level in Column 1, and calculated following Conley (2008) using a uniform kernel out to 10 km in Columns 2 - 5. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

B Details on study design and intervention

B.1 Cash transfer intervention

The NGO GiveDirectly (GD) provides cash transfers to poor households, and for the purposes of this study, enrolled households with grass-thatched roofs in villages assigned to treatment. GD worked on a rolling basis across villages in the study area. The village order was randomized. GD's enrollment process in treatment villages consisted of the following 6 steps:

1. Village meeting (*baraza*): Before beginning work in a village, GD held a meeting for all households in the village to inform residents that GD would be working in their village and explain the program and GD as an organization. To prevent gaming, the eligibility criteria were not disclosed.

2. Census: GD staff then conducted a household census of the village, collecting names of household members, contact information, and information about housing materials. The information on housing materials was used to determine program eligibility.

3. Registration: Households identified as eligible based on the household census were visited by GD's registration team. During these visits, GD staff confirmed the eligibility of the household, informed the household of their eligibility, and registered the household for the program. Households could select the member that they wished to be registered for the program. This visit was the point at which households learned they would be receiving transfers, as well as the amount of the transfers, the transfer schedule, and the fact that the transfer was unconditional.[51] Households were instructed and coached on how to register for M-Pesa, which was a prerequisite for being able to receive transfers. Households that did not have a mobile phone were given the option to purchase one from GD staff, the cost of which was deducted from the transfer amount.

4. Back-check: All registered households were back-checked to confirm eligibility in advance of transfers being sent. Importantly, the census, registration, and back-check teams consisted of separate staff members; this fact, and the multiple eligibility confirmations, were security measures to prevent gaming by households and field staff.

5. Transfers: Transfers were made in a series of three payments via M-Pesa, according to the following schedule: (i) A token transfer of KES 7,000 (USD 151 PPP) was sent once a majority of eligible households within the village had completed their backchecks, to ensure that the system was working properly, to ensure that the system was working properly. (ii) Two months after the token transfer, a first large installment of KES 40,000 (USD 860 PPP) was sent. (iii) Six months later (eight months after the token transfer), a second and final large installment of KES 40,000 was sent. If households elected to receive a mobile phone from GD, this cost (KES 1600 or USD 34 PPP) was subtracted from the second large installment. Transfers were typically sent at a

51. To emphasize the unconditional nature of the transfer, households were provided a brochure that listed a large number of potential uses of the transfer.

single time per month (usually around the 15th) to all households scheduled to receive transfers.

6. Follow-up: After transfers were sent, GD staff followed up by phone with transfer recipients to ensure that transfers were received. In addition, recipients could contact a GD helpline with questions. If GD staff learned that household conflicts had arisen as a result of the transfers, transfers were occasionally delayed while these problems were worked out.

B.2 Randomization details

Villages were randomly assigned to treatment status following the two-level randomization design described in Figure A.1a. The randomization was conducted in two batches as GD expanded its operations, with the first batch covering villages in Alego subcounty, and the second batch covering villages in Ugunja and Ugenya subcounties.

In Alego, we compiled a list of rural villages eligible for GD expansion. We then grouped sublocations into 23 saturation groups, ensuring that each saturation group was formed from contiguous sublocations, had at least three study villages, and (where possible) the number of study villages was a multiple of three (given that either one-third or two-thirds of villages are assigned to treatment within each sublocation). In 11 sublocations, we declared the sublocation itself as the saturation group. The remaining 13 saturation groups were formed by combining contiguous sublocations into saturation groups. In this manner, the 39 sublocations in Alego were allocated to 23 saturation groups, which were later randomized into high- and low-saturation status.

GD had worked in 193 villages in Alego prior to the start of this study. To account for previous participation in GD's program, we stratified assignment of high and low saturation by the level of previous exposure to the GD program within the saturation group, measured as the share of villages covered by a previous GD campaign, splitting the exposure level at the median.

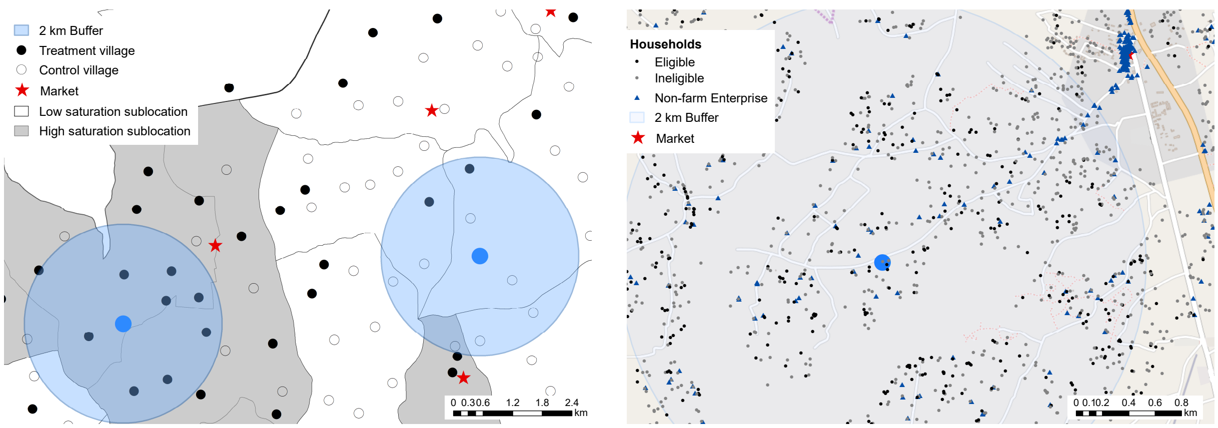
We then randomly assigned villages to three groups, and randomly assigned these groups to either a) treatment, b) treatment in high saturation, control in low saturation, and c) control. In Alego, 12 saturation groups were assigned to high saturation status, covering 98 villages (65 treatment, 33 control), and 11 saturation groups were assigned to low saturation status, covering 105 villages (37 treatment, 68 control). Across these 203 villages, a total of 7,891 households were classified as eligible by the GE census team (37 percent of households), with 3,950 of these households in treatment villages. We randomly generated an order for GD program expansion by first randomly ordering the saturation groups, and then villages within saturation groups.

The second randomization batch included villages in Ugunja and Ugenya subcounties. GD had not previously worked in any villages in these subcounties, so we did not stratify on any variables related to previous exposure for these villages. These subcounties had a larger number of villages per sublocation than Alego on average; as a result, we randomized saturation at the sublocation level. We assigned villages to one of three groups as above, pooled the "residual" villages that were not a multiple of 3, and randomly assigned one third of these to treatment, one third to treatment in high saturation sublocations and control in low saturation sublocations, and one third to control. GD worked first in Ugunja and then Ugenya. Across Ugunja and Ugenya, 115 sublocations covering 227 villages (148 treatment, 79 control) were assigned to high saturation status, while 79 sublocations covering 224 villages (78 treatment, 146 control) were assigned to low saturation status. These 450 villages had a total of 13,846 households classified as eligible by the GE census team (31 percent), with 7,105 of these households in treatment villages. We generated a random order within these subcounties by first ordering locations (the administrative unit above the sublocation), then sublocations within the location, then villages within the sublocation.

B.3 Illustrating spatial variation in treatment

Figure B.1: Spatial variation of data and treatment

(a) Comparing 2 villages | (b) Households and enterprises



Notes: This figure provides an example of the spatial variation that we use to identify spillover effects. Both panels provide zoomed-in views on a selection of villages from Figure A.2. Panel A illustrates variation in the density of treatment villages around 2 treated villages. It plots village centers for treatment (filled circles) and control (open circles) villages, as well as a 2 km radius around the village center. While both villages themselves are not treated, the share of treated villages around them varies considerably. Panel B zooms in on one of these villages and plots eligible and ineligible households, as well as non-farm enterprises. Market centers are plotted as red stars.

We construct the amount per capita GDP in each buffer around a village or market i ($Amt_{it,r}$) as the per capita transfers in each buffer r, divided by per capita GDP. We obtain amount transferred into each buffer r at time t from the GPS location of recipients, as well as information from GiveDirectly (GD) on transfers sent to each recipient. Per capita GDP is calculated as the population-weighted average, across all households in the study area, of our expenditure-based measure of GDP (see Section 5). To convert stock values into annual flows, we assume a 10% depreciation rate. This yields an average per capita GDP of 637 USD PPP (or 2727 USD PPP per household). For $Amt^{\neg v}_{vt,r}$, we exclude households in buffer r but located in the same village v.

The population in each radius band around each market or village is determined using the GPS location of each household in our baseline household census data. Each household is then multiplied by the average number of people per household from the baseline household survey. This provides a population measure for each village in our study sample. To account for villages not included in our sample, but within radii bands of study markets or villages, we take two approaches. First, in villages that were not part of our sample but where GD had worked previously, we use household GPS locations provided to us by GD. For areas which were neither in our sample nor had been visited by GD previously, we calculate the population by uniformly distributing the sublocation population from the 2009 Kenyan census, net of the population in study area or GD census villages, over the area of the sublocation that was not already covered by a village in our study or a village where GD had worked previously. Village areas are defined as convex hulls around GPS coordinates of all village households. 2009 Kenyan census numbers are inflated by the overall average population growth rate in Kenya between 2009 and 2014.

C Household data appendix

C.1 Construction of index outcomes

Our index variables are constructed from the following components:

1. Psychological well-being index: Weighted, standardized average of depression (10 question CES-D scale), happiness, life satisfaction, and perceived stress (PSS-4), appropriately signed so that positive values represent better psychological well-being.

2. Health index: weighted, standardized average of self-reported health (on a scale of 1 to 5), an index of indicators for common health indicators, and an indicator for whether the respondent has experienced a major health problem since the date of baseline surveys, appropriately signed so that positive values represent better health.

3. Food security index: weighted, standardized index of the number of days a) adults and b) children i) skipped or cut meals, ii) went to bed hungry, iii) went entire days without food out of the last 7 days, appropriately signed so that higher values represent better food security. The Children food security index is made up of the child-related food security questions.

4. Education index: weighted, standardized average of total education expenditure and proportion of school-aged children in school, appropriately signed so that higher values represent better education outcomes.

5. Female empowerment index: weighted, standardized average of a violence index and attitudes index, appropriately signed so that positive values reflect more female empowerment/less domestic violence. The violence index is calculated as from the frequency of physical, emotional as sexual violence over the last 6 months. The attitudes index is calculated from an index of male-oriented attitudes and an index on the justifiability of domestic violence.

6. Security index: a weighted, standardized index of the number of times victimized by i) theft or ii) assault, arson or witchcraft in the last 12 months, an indicator for experiencing but not reporting a crime, and an indicator for reporting to be worried about crime or safety in the neighborhood.

C.2 Tracking and attrition

We achieved high tracking rates at endline, reaching over 90 percent of both treatment and control households. To assess levels of attrition, and whether attrition at endline is affected by treatment status and hence might confound our results, we estimate Equation (1) using as an outcome an indicator r_{hvs} for whether household h in village v in sublocation s is observed at endline, and do this separately for eligible and ineligible households. We investigate whether this indicator of non-attrition varies with treatment status in Table C.1.

We observe high tracking rates of 90.3 and 90.8 in the two types of households, respectively, in low-saturation control villages. These rates are very similar in other villages and sublocations: We observe broadly insignificant treatment coefficients in both tables, suggesting that attrition does not systematically vary with treatment status. This result is robust to defining r_{hvs} as an indicator for being reached at both baseline and endline (Column 2). It is also robust to restricting the sample to only households reached at endline (Panel B) or only households surveyed at baseline (Panel C). The one significant coefficient is for ineligible households in high-saturation sublocations: these are significantly less likely to be reached twice (Panel A, Column 4).

Table C.1: Household survey tracking and attrition

	(1)	(2)	(3)	(4)
	Eligible		**Ineligible**	
	Surveyed at endline	Surveyed at baseline and endline	Surveyed at endline	Surveyed at baseline and endline
Panel A: All households targeted at endline				
Treatment Village	0.006	0.006	0.011	0.017
	(0.009)	(0.013)	(0.011)	(0.016)
High Saturation Sublocation	−0.003	−0.019	−0.015	−0.035*
	(0.009)	(0.015)	(0.011)	(0.018)
Control, Low Sat Mean (SD)	0.903	0.815	0.905	0.811
	(0.296)	(0.389)	(0.293)	(0.392)
Observations	5,992	5,992	3,121	3,121
Panel B: Among households surveyed at endline				
Treatment Village		0.001		0.008
		(0.011)		(0.015)
High Saturation Sublocation		−0.019		−0.024
		(0.014)		(0.017)
Control, Low Sat Mean (SD)		0.902		0.895
		(0.297)		(0.306)
Observations		5,385		2,822
Panel C: Among households surveyed at baseline				
Treatment Village	−0.004	−0.004	0.012	0.012
	(0.009)	(0.009)	(0.011)	(0.011)
High Saturation Sublocation	0.000	0.000	−0.021*	−0.021*
	(0.009)	(0.009)	(0.011)	(0.011)
Control, Low Sat Mean (SD)	0.927	0.927	0.933	0.933
	(0.260)	(0.260)	(0.251)	(0.251)
Observations	5,150	5,150	2,661	2,661

Notes: This table reports tracking and attrition rates for households, by classification as eligible or ineligible to receive GD transfers by GE project field staff. Each Column represents a regression of an indicator for being surveyed at endline, or at both baseline and endline on an indicator for being in a treatment village, and and an indicator for the saturation status of the sublocation. Panel A includes all households that were targeted for endline surveys. Panel B looks at households that completed endline surveys, and serves as our main analysis sample. Panel C looks at households that completed baseline surveys, and provides information on households that attrited from baseline to endline. Standard errors are clustered at the village level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

C.3 Baseline balance

We re-estimate our main specifications using baseline demographic and outcome data, following the format of Table 1.[52] Panel A presents household demographics, while Panel B presents outcomes from Tables 1 and 2 for which we have baseline data (we did not collect consumption expenditure data at baseline). We are generally balanced across a wide range of variables. In our main specifications, we include baseline values of the outcome variable as a control when available to improve statistical precision.

C.4 Household weights

We weight household-level analyses with inverse sampling probability weights to ensure results are representative of the full population. In each village, we have baseline census data that provides the total number of households, classified by transfer eligibility status (based on research team reports). We targeted 8 eligible households and 4 ineligible households for surveys at baseline, and at endline targeted households surveyed at baseline, as well as those targeted and missed at baseline. The number of eligible households varies across villages; we thus weight households surveyed at endline by the inverse of the share of eligible households surveyed within the village. We do the same for ineligible households.

For hourly earnings, land prices and household interest rates, we interact these household level weights with the number of hours worked, acres of land owned, and total loan amounts, respectively, to make price effects interpretable as unit price effects.

52. We pre-specified a different set of balance checks that did not incorporate spatial variation; these are available in Egger et al. (2020). These checks also show the experiment is well-balanced.

Table C.2: Household balance

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	𝟙(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation mean (SD)
Panel A: Respondent demographics				
Female	0.02	0.02	−0.01	0.75
	(0.02)	(0.03)	(0.02)	(0.43)
Respondent aged 25 or older	0.00	0.01	−0.01	0.92
	(0.01)	(0.02)	(0.01)	(0.28)
Is married	0.03	0.02	0.01	0.50
	(0.02)	(0.03)	(0.04)	(0.50)
Completed primary school	0.02	0.02	0.05*	0.33
	(0.02)	(0.03)	(0.03)	(0.47)
Has child	0.01	0.02	0.04*	0.73
	(0.01)	(0.02)	(0.02)	(0.44)
Self-employed	−0.01	−0.01	0.00	0.28
	(0.02)	(0.02)	(0.03)	(0.45)
Employed in wage work	−0.02	−0.01	0.01	0.25
	(0.02)	(0.05)	(0.03)	(0.43)
Panel B: Household assets				
Assets (non-land, non-house), net borrowi	3.95	−16.49	−41.27	1,017.56
	(23.06)	(36.24)	(100.34)	(1,391.45)
Housing value	14.47	−22.64	−0.21	1,579.57
	(14.43)	(19.55)	(342.18)	(4,219.38)
Land value	−221.59	−237.81	−199.16	4,366.48
	(162.63)	(339.89)	(451.87)	(5,819.84)
Panel C: Household cash flow				
Household non-ag income, annualized	−4.60	29.66	−14.39	197.28
	(15.82)	(32.74)	(29.24)	(461.78)
Self-employment profits, annualized	2.26	10.08	−2.57	89.07
	(7.48)	(13.67)	(18.83)	(288.60)
Wage earnings, annualized	−10.27	8.47	−5.97	97.14
	(12.68)	(26.09)	(13.80)	(309.63)
Tax paid, annualized	1.97	3.40**	3.27	16.33
	(1.20)	(1.71)	(2.44)	(44.73)
Panel C: Input Prices				
Land price per acre	−52.49	199.16	268.08	3,303.33
	(94.75)	(168.56)	(262.23)	(2,985.13)
Acres of land owned	35.61	71.96	−0.32**	1.36
	(35.66)	(72.82)	(0.15)	(2.39)
Total loan amount	1.65	6.44	−3.90	54.14
	(3.17)	(4.79)	(12.37)	(162.42)

Notes: This table presents regression specifications from Table 1 using baseline demographic and outcome variables. We did not collect consumption expenditure data at baseline. We have 4,674 to 4,768 observations for columns 1 and 2 (3,962 for land price) and 4,696 to 4,831 observations for column 3 (4,201 for land price). * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

C.5 Constructing average effects from coefficient estimates

Table C.3 presents the coefficient estimates underlying our reported average effects shown in Table 1. These average effects are constructed using the average values of each of the regressors included in the selected specification, denoted $\bar{X}$ and presented in the bottom row. For instance, the recipient household total effect for household expenditure (Table 1, row 1, column 2) is found by multiplying the coefficient on the amount going into households' own village Amt_v (row 1, column 2) by the mean amount (relative to village GDP) going into own village $\overline{Amt_v}|i$ is an eligible recipient (last row, column 2) and adding the coefficient on the amount going to other villages within 0-2km $Amt_{v,2}^{\neg v}$ (row 1, column 3) times the mean amount going into this radii band for treated villages $\overline{Amt}_{v,2}^{\neg v}$ (last row, column 3). We proceed in the same manner for the other tables.

Table C.3: Coefficient estimates for Expenditures, Savings and Income

	Recipient households			Non-recipient households		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\mathbb{1}$(Treat village) Reduced form	Amt Own Village IV	Amt Other Villages 0-2km IV	Amount, Control Eligibles 0-2km IV	Amount, Ineligibles 0-2km IV	Control, low-saturation mean (SD)
Panel A: Expenditure						
Household expenditure, annualized	292.98***	1,159.08***	375.10	265.50	4,419.08***	2,536.86
	(60.09)	(230.42)	(1,024.59)	(1,053.14)	(1,592.72)	(1,934.09)
Non-durable expenditure, annualized	186.96***	753.99***	318.11	311.50	4,180.12***	2,471.49
	(58.55)	(213.51)	(936.58)	(993.89)	(1,560.89)	(1,877.82)
Food expenditure, annualized	71.61*	352.01***	451.97	133.84	1,753.13**	1,578.43
	(36.93)	(128.50)	(584.12)	(629.77)	(767.95)	(1,072.31)
Temptation goods expenditure, annualized	6.51	23.71	−3.65	133.92	−37.57	37.10
	(5.79)	(22.07)	(94.11)	(100.81)	(83.98)	(123.59)
Durable expenditure, annualized	95.18***	368.21***	137.16	71.42	97.73	59.44
	(12.64)	(57.53)	(194.97)	(211.63)	(161.52)	(230.90)
Panel B: Assets						
Assets (non-land, non-house), net borrowing	178.09***	674.84***	55.46	−155.73	1,811.04*	1,132.15
	(24.61)	(87.84)	(376.80)	(501.75)	(1,055.96)	(1,420.22)
Housing value	372.78***	1,456.55***	1,082.00***	341.81	901.54	2,033.72
	(25.25)	(87.55)	(354.50)	(469.08)	(2,888.39)	(5,030.37)
Land value	50.86	330.17	728.74	2,340.15	7,179.88	5,030.72
	(186.08)	(478.95)	(2,920.78)	(3,650.75)	(5,854.60)	(6,607.61)
Panel C: Household balance sheet						
Household income, annualized	77.62*	333.58*	510.04	1,021.65	2,862.47**	1,023.45
	(43.66)	(185.90)	(746.45)	(746.49)	(1,164.68)	(1,634.70)
Net value of household transfers received, annualized	−1.68	−5.94	−64.41	−85.85	135.35	130.18
	(6.81)	(25.92)	(115.95)	(129.15)	(249.65)	(263.75)
Tax paid, annualized	1.95	7.31	−21.99	−11.57	24.72	16.93
	(1.28)	(4.50)	(19.59)	(20.76)	(25.79)	(36.51)
Profits (ag & non-ag), annualized	24.70	115.74	37.09	−78.33	607.47	485.20
	(23.18)	(97.59)	(388.53)	(477.68)	(606.86)	(787.10)
Wage earnings, annualized	42.51	196.44	243.31	1,131.87**	2,216.33**	495.37
	(32.24)	(119.53)	(557.31)	(491.88)	(862.71)	(1,231.56)
$\bar{X}$		0.26	0.09	0.08	0.09	

Notes: This table reports the coefficient estimates that underlie the average effects reported in Table 1, see corresponding table note for more details. $\bar{Amt}$ reports the average of each RHS variable for the sample studied (recipients or non-recipients), which we multiply with the coefficient to get the average effects reported.

D Enterprise data appendix

D.1 Enterprise census and survey details

We conducted a baseline enterprise census in each village on the same day as the baseline household census. The household census included a question on whether the household was running an enterprise from their homestead or from a fixed kiosk/shop. The enterprise census targeted enterprises operating outside of homesteads. We then returned to survey enterprises operating outside of the homestead and open on the day of our visit, coincident with baseline household surveys. In villages with over 20 enterprises operating outside of homesteads, e.g., those that overlapped a market center, we randomly selected 20 enterprises to survey.

Our endline enterprise census sought to re-identify all enterprises operating from within or outside homesteads, both those identified at baseline and any new enterprises. In order to maintain a representative sample, we randomly sampled up to 2 enterprises operating from within homesteads and up to 3 outside of homesteads to be surveyed, including those in market centers in villages containing a market.

Enterprise surveys cover profits, revenues, and a subset of costs (including the wage bill), and at endline collected information on inventories and investment. We measure (annualized) revenues and profits for non-agricultural enterprises directly by asking respondents about these quantities with a one month recall period (de Mel, McKenzie, and Woodruff 2009). We calculate costs as the sum of the employee wage bill, rent and security costs; this is not a comprehensive measure of all costs, and hence we do not expect the revenue measure to equal our measure of profits plus measured costs. In particular, we do not directly measure expenditure on intermediate inputs such as materials or supplies.

Information on agricultural enterprises comes from our household surveys. Baseline household surveys did not include sufficient detail to construct measures of agricultural revenue and profit, so we only use endline measures for these outcomes. For agricultural enterprises, total revenue is calculated as the sum of crop output (measured at the crop level) plus the value of pastoral and poultry output sold, and the value of the household's own consumption of pastoral and poultry output. When crop output was reported in non-monetary units, we convert these to monetary values using the 2016 mean of the median crop output price measured in the market price surveys in the household's sub-county. Agricultural costs are the wage bill, all agricultural inputs (e.g., seed and fertilizer), and land rental costs. We then calculate agricultural profits as total agricultural revenue minus agricultural costs.

D.2 Enterprise specifications

We estimate the following equations for enterprises:

$$y_{ivs} = \alpha_1 Treat_v \cdot X_{ivs} + \alpha_2 HighSat_s \cdot X_{ivs} + X_{ivs}\gamma + \delta_1 y_{ivs,t=0} \cdot X_{ivs} + \delta_2 M_{ivs} \cdot X_{ivs} + \varepsilon_{ivs}, \tag{10}$$

$$y_{iv} = \beta Amt_v \cdot X_{ivs} + \sum_{r=2}^{R} \beta_r Amt_{v,r}^{\neg v} \cdot X_{ivs} + X_{ivs}\gamma + \delta_1 \bar{y}_{iv,t=0} \cdot X_{ivs} + \delta_2 M_{iv} \cdot X_{ivs} + \varepsilon_{iv}. \tag{11}$$

Here, y_{ivs} is an outcome for enterprise i in village v (and sublocation s), $X_{iv(s)}$ is a vector of indicators for enterprise type (agricultural, non-agricultural operating outside the homestead, non-agricultural operating from the homestead), and other terms are defined as in Section 3. We interact our treatment indicator and transfer amount variables with this vector of enterprise types, effectively estimating a stacked version of Equations 1 and 2. This allows treatment effects and controls to vary flexibly across enterprise type. Table D.1 reports the share of enterprises by sector weighted by count and by revenue. Since enterprise surveys were conducted as repeated cross-section rather than a panel, we control for the village-level baseline mean of the outcome variable where available in our main specification. Results are similar if we omit this control (Table D.2).

We use our endline enterprise census data to construct weights that are representative of the full population of enterprises. In particular, we weight enterprises by the inverse of the share of surveyed enterprises of a particular type (agricultural, operating from homesteads, operating outside homesteads) within each village. For hourly wages, we interact these enterprise-level weights with the total hours worked to make wage effects interpretable as the average effect per hour worked.

D.3 Tracking, balance and attrition

Our enterprise samples are repeated cross-sections, so we do not report attrition rates between baseline and endline. We do check baseline balance for enterprises, taking the same approach as in Table 3 but using baseline values for outcomes that are available. (We did not collect enterprise investment or inventories, nor do we have revenue and profit measures for non-agricultural enterprises at baseline.) The baseline sample generally appears balanced; there are no statistically significant differences at the 5% level (Table D.3).

D.4 Matching enterprise owners

Through our integrated approach to enterprise and household censusing, we are able to match all agricultural enterprises (as found via household surveys), and 56% of non-agricultural enterprises, for a total of 93% of all enterprises. To match non-agricultural enterprises to the households that own them we apply both automatic and manual procedures to our detailed name, phone number and GPS data. As we relied heavily on the reported operating location, we excluded enterprise census data without this information. The proportion of matched enterprises are relatively evenly split by treatment status for both eligible and ineligible households: 52% of matched eligible enterprise owners and 51% of matched ineligible owners are in treatment villages.

Patterns with respect to the eligibility status of the owner are generally sensible: 28% of non-agricultural enterprises are owned by an eligible household, slightly below their share in the population (33%), and enterprises owned by ineligible (and thus on average somewhat richer) households have 9% higher profits and 21% higher revenues on average than those owned by eligibles.

Table D.1: Composition of enterprises by sector

	Overall		**Non-Ag**	
Sector	Count Share	Revenue Share	Count Share	Revenue Share
Retail	0.09	0.34	0.54	0.52
Manufacturing	0.04	0.16	0.24	0.24
Services	0.03	0.16	0.21	0.24
Agriculture	0.84	0.34		

Notes: This table describes enterprise shares by sector, both in terms of counts and shares of total revenue. Data on counts comes from the endline enterprise census (for non-agricultural enterprises) and the baseline household census (for agricultural enterprises). Data on revenue shares for the non-agricultural sectors comes from endline enterprise surveys, while data on agricultural revenue shares comes from endline household surveys.

Table D.2: Enterprise outcomes without baseline controls

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Panel A: All enterprises				
Enterprise profits, annualized	0.02	63.01	36.52	323.39
	(30.07)	(163.80)	(141.74)	(691.12)
Enterprise revenue, annualized	−82.06	376.54**	303.94**	758.52
	(129.11)	(148.36)	(139.85)	(2,493.40)
Enterprise costs, annualized	−12.61	106.19**	78.85	147.73
	(34.91)	(42.42)	(53.53)	(550.11)
Enterprise wagebill, annualized	−15.49	91.51***	77.86*	120.62
	(30.84)	(33.73)	(40.73)	(492.11)
Enterprise profit margin	—	—	—	—
Panel B: Non-agricultural enterprises				
Enterprise inventory	—	—	—	—
Enterprise investment, annualized	—	—	—	—
Panel C: Village-level				
Number of enterprises	0.01	0.01	0.00	1.12
	(0.01)	(0.02)	(0.02)	(0.14)

Notes: This table replicates Table 3 but without village level baseline control variables. We omit outcomes for which baseline controls were not available in the original table, as results for those outcomes are unaffected. See notes to Table 3 for further details. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table D.3: Enterprise Balance

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	$\mathbb{1}$(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Panel A: Non-agricultural enterprises				
Enterprise profits, annualize	–10.29	6.96	12.99	1,141.19
	(16.29)	(21.84)	(20.63)	(1,848.50)
Enterprise revenue, annualized	–99.79	65.88	118.63	4,919.61
	(87.63)	(99.52)	(103.40)	(11,430.88)
Panel B: All enterprises				
Enterprise costs, annualized	3.78	16.15	6.79	57.09
	(6.05)	(10.37)	(9.32)	(237.07)
Enterprise wagebill, annualized	3.39	15.75	6.95	53.08
	(5.91)	(10.00)	(8.94)	(233.12)
Panel C: Village-level				
Number of enterprises	0.00	–0.01	–0.01	1.07
	(0.01)	(0.02)	(0.02)	(0.14)

Notes: This table presents regression specifications from Table 3 using corresponding baseline enterprise outcomes where available. We did not collect enterprise inventories and investment data at baseline. We also exclude baseline agricultural revenues and profits, as these were not collected in the same manner as at endline. We have between 4,125 and 4,193 observations in Panel A, 9,245 to 9,264 in Panel B, and 653 in Panel C. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct.

E Price data appendix

E.1 Categorizing market survey products

Our market surveys included questions about 84 commodities. As outlined in our pre-analysis plan, we excluded products that are not present in at least 5 percent of market-month observations; this affects 11 products (bicycle, bull (grade), calf (grade), donkey, duck, piglet, turkey, goat (meat), lamb, milk powder, and mosquito net). Three products (Waterguard, fertilizer, and improved maize seeds) do not have consumption expenditure analogues in the KLPS-3 surveys that we use to construct expenditure weights, so these are also excluded, leaving a final list of 70 products.

Table E.1 presents the classification of the products we use in our price analysis into more and less tradable categories, and the subcategories (food, livestock, (non-food) non-durables, durables and temptation goods) shown in Table 4.

Table E.1: List of market products by category

Less tradable (locally produced)			**More tradable**		
Food		Livestock	Non-Food Non-Durables	Durables	Temptation Goods
Cassava	Papaya	Bull (local)	Bar soap	1 Iron sheet (32 gauge)	Cigarettes
Irish potato	Pineapple	Calf (local)	Toothpaste	Cement	
Maize	Water Melon	Chicken (hen)	Vaseline/lotion	Large Padlock	
Millet	Jackfruit	Goat	Washing powder	Nails (3 inch)	
Plantains	Passion Fruit	Sheep	Bleach	Roofing Nails	
Rice	Beef		Panadol/aspirin	Timber (2x2)	
Sorghum	Fish (Tilapia)		Cooking fat	Water Paint	
Sweet potato	Pork		Batteries (3-volt)	20L Jerry can	
Beans	Eggs		Firewood	Thermos flask	
Cabbage	Milk (Fresh)		Kerosene	3 1/2 X 6 Mattress	
Cowpea leaves	Biscuits		Charcoal		
Green grams	Bread		Leso		
Groundnuts	Cake		Small sufuria		
Kales	Maize flour		Slippers		
Onions	Wheat flour				
Saka (Local Vegetable)	Milk (Fermented)				
Tomatoes	Soda				
Avocado	Sugar				
Banana-sweet	Tea				
Mango					
Orange					

Notes: This table presents the classification of the 70 products used in our analysis of output prices. The classification follows our midline pre-analysis plan (Appendix I). The market survey collected information on 85 products. As outlined in our pre-analysis plan, we exclude any product that, at the market-product level, is missing for more than 95% of cases, a total of 11 products. We also drop three products that do not match items in our expenditure share data.

E.2 Price analyses robustness checks

E.2.1 Alternative definition of market access

Our main specifications separates price effects by market access as defined in Donaldson and Hornbeck 2016: $MA_m = \sum_{r=0}^{10} r^{-\theta} N_r$, where $\theta = 8$ and N_r is the population in in the $r-2$ to r km buffer around each market. Here we present alternative results based on a definition of market access as the inverse distance from the closest 'main' road, where we define a main road as any road in Open Street Maps classified as motorway, trunk, primary, secondary or tertiary road (excluding residential streets, tracks, paths, and unclassified roads). While price effects were concentrated in low-market-access areas using our main population-density-based market access measure, they seem to be fairly similarly small when splitting by road access. In a context where most people walk to their nearest market, this may not be surprising. However, we cannot reject that results are the same as our main results.

Table E.2: Output Prices using distance to main road as market access measure

		(1)	(2)	(3)	(4)
		Overall Effects		**ATE by road access**	
		ATE	Average maximum effect (AME)	below median	above median
All goods		0.0010*	0.0042	0.0010	0.0011
		(0.0006)	(0.0031)	(0.0008)	(0.0008)
By tradability	More tradable	0.0014	0.0062	0.0006	0.0021
		(0.0015)	(0.0082)	(0.0021)	(0.0021)
	Less tradable	0.0009	0.0034	0.0012	0.0007
		(0.0006)	(0.0032)	(0.0009)	(0.0009)
By sector	Food items	0.0009	0.0036	0.0014	0.0007
		(0.0006)	(0.0033)	(0.0009)	(0.0010)
	Non-durables	0.0014	0.0061	0.0005	0.0020
		(0.0017)	(0.0089)	(0.0023)	(0.0022)
	Durables	0.0019*	0.0070	0.0012	0.0031
		(0.0011)	(0.0061)	(0.0013)	(0.0019)
	Livestock	−0.0008	−0.0027	−0.0023*	0.0012
		(0.0010)	(0.0052)	(0.0013)	(0.0013)
	Temptation goods	−0.0011	−0.0112	−0.0035	0.0022
		(0.0026)	(0.0143)	(0.0036)	(0.0041)

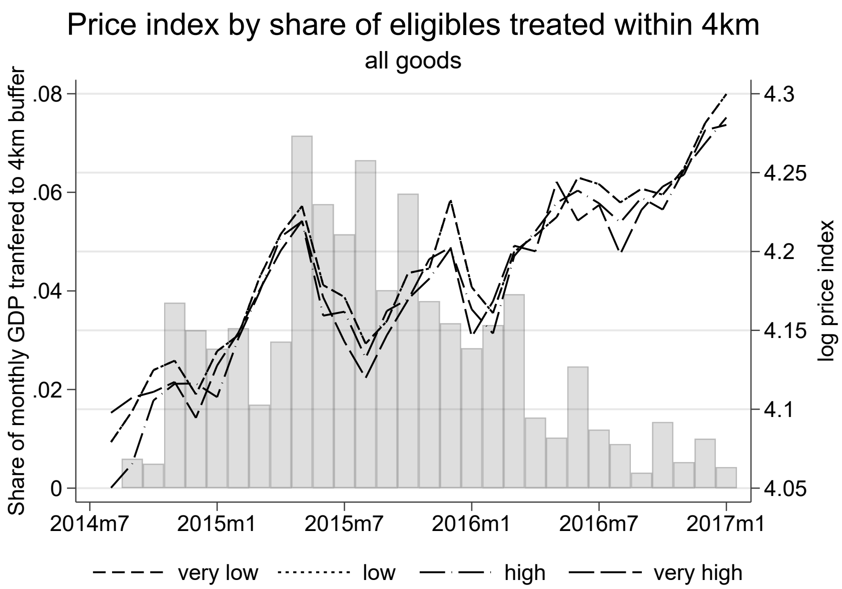
Notes: This table replicates Table 4. See notes for details. The only difference is the definition of market access of each market in Columns 3 and 4: It is defined as the inverse distance from the closest main road, classified by Open Street Map as motorway, trunk, primary, secondary or tertiary road (excluding residential, tracks, paths, and unclassified roads): $MA_m = \frac{1}{distance_m}$. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

E.2.2 Spatial and temporal analysis of price effects

Our main analysis in Section 4.5 follows our pre-specified algorithm, which selects the number of lags and distance buffers by minimizing the Schwarz BIC. While we allow for up to 18 months lags, and 20km spatial dependence, the algorithm selects a specification that includes only contemporaneous transfers up to 4km. In this section, we present three pieces of additional exploratory analysis that serve as robustness checks on our primary pre-specified results and explore the spatial and temporal dimensions of price effects in turn.

First, we show our price data in raw form: Figure E.1 shows that prices in more vs. less exposed markets as measured by the share of eligible households within 4km that were assigned to treatment evolved very similarly over the course of the study period and afterwards. We can visually reject large differences in the evolution of prices in response to treatment.

Figure E.1: Price index by treatment intensity



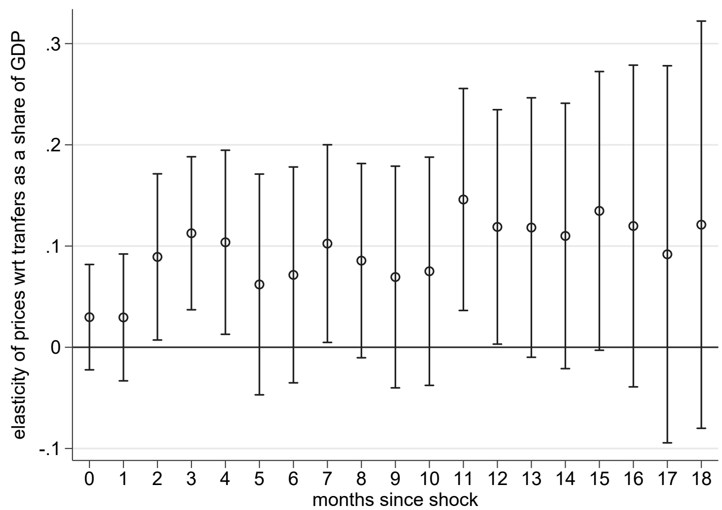
Notes: The figure shows the log price index across all goods in more vs. less exposed markets as measured by quartiles of the the share of eligible households within 4km that were assigned to treatment. Bars represent average transfer amounts relative to monthly GDP going to the 4km buffer across all 61 markets in each month.

Second, we estimate Equation (4) for a range of outer radii R from 2km to 6km while fixing the number of temporal lags at the (BIC selected) value of 0 months. This allows us to test whether our algorithm is indeed picking up the relevant spatial horizon, or whether we might be missing part of the effect. Table E.3 shows that price effects are robust to including additional radii bands. For none of the price indices can we reject that adding an additional buffer on top of that selected by our pre-specified algorithm leads to significantly different average price effects as those in the main specification.

Third, we do the analogous exercise temporally, estimating Equation (4) for lag structures of up to $M = 18$ months while fixing the maximum radius at $R = 4$km. We then calculate the cumulative effect of a shock of 100% of monthly GDP in each month up to L months. Figure E.2 shows that prices adjust rapidly. We cannot reject that lags of treatment beyond 3 months have no additional effect on prices, and the elasticity of prices with respect to transfers as a share of GDP stabilizes around 0.1.

The economic implication is that any part of the price response we miss using our pre-specified algorithm to select a temporal horizon does not make a meaningful difference quantitatively. In the most intense 12 month period, 3.9% of annual GDP was transferred to the 0 - 4km buffer around the average market (see Figure E.1). With an elasticity of 0.1, this implies a price effect of 0.4% in the average market over the most intense period of transfers (nearly identical to the 0.4% we arrive at using our pre-specified algorithm).

Figure E.2: Cumulative price effects



Notes: The figure is based on estimating Equation 5, where we impose a maximum lag or price effects up to $M = 18$ months and a maximum spatial radius of $R = 4$km. We then calculate the cumulative effect of a shock of 100% of monthly GDP in each month over L months on the overall logarithmic price index ($= \sum_{r=2km}^{R=4km} \sum_{l=0}^{L} \hat{\beta}_{rl}.$). Confidence intervals are as in Conley (2008) and we allow for spatial correlation up to 10km and autocorrelation up to 12 months.

E.2.3 IV specification for market price effects

In our preferred estimates we identify effects on market prices using a different method than the IV strategy we use for identifying effects on households and firms (i.e. specifications 2, 3 and 11). This is because, unlike for households and firms, we have rich panel data on market prices including prices both before and after treatment onset. This lets us identify effects using a difference-in-differences design (as described in section 3.7) and leveraging the random roll-out of transfers into different buffers around each market over time. Specifically, conditional on market fixed effects, which control for the share of eligible households around each market as well as other time-invariant characteristics of markets, treatment roll-out is exogenous.

For comparability, however, we can also estimate price effects using the same IV strategy we use for firm and household outcomes. Concretely, we run the same pre-specified radii and buffer selection algorithm (specifications 4 and 5) based on the BIC for each price effect. But instead of including market fixed effects and $Amt_{m(t-l),r}$, we drop market fixed effects, and instrument $Amt_{m(t-l),r}$ with share $s^{e,t}_{m,r}$ of eligible households assigned to treatment in the buffer r around market m multiplied by the share of transfers going to that buffer in month $t-l$. Note that this approach is analogous to our dynamic IV specification 7 we use to estimate the impulse response functions for flow variables underlying our multiplier estimates. After selecting radii $\bar{R}$ and lags L to be included for each price index, we run:

$$p_{mt} = \sum_{r=2}^{\bar{R}} \sum_{l=0}^{L} \beta_{rl} Amt_{m(t-l),r} + \lambda_t + \varepsilon_{mt} \tag{12}$$

Table E.4 reports average and average maximum effects as in Table 4 resulting from this strategy. Effects are broadly in line with those in our main specification. Although none of the effects are statistically significant when using the IV strategy, we are still able to reject large effects on prices.

E.3 Enterprise price analyses

In addition to prices collected as part of our market price surveys, we also collected some price data as part of our enterprise surveys. We make use of enterprise price data collected via seven rounds of phone surveys of enterprises between August 2015 and June 2016. These surveys were conducted with four types of enterprises: small retailers, hardware stores, maize grinders, and tailors. We focus on prices for services provided by the latter two, as hardware and retail prices are well-covered by our market price data. To ensure consistent quality, unit size and availability we collected prices for a small number of services these enterprises commonly provide. In particular, we focus on the price of grinding 1kg of maize at a posho mill, and for patching a small hole at a tailor shop.

Phone surveys overlapped with an intense period of treatment rollout. During those 11 months the share of overall transfers sent went from 52% to 92%, and the variation in transfers was substantial, both across space and time: The 10-90 percentile range of per capita GDP transferred within 2km of a village over the period is $[0.5\%, 9.6\%]$, and the average village experienced 1.8% of GDP more inflows in the most intense month compared to the least intense month.

We analyze these prices analogously to our market prices, running the following specification:

$$p_{evt} = \sum_{r}^{\bar{R}} \sum_{l=0}^{M} \beta_{rl} Amt_{v(t-l),r} + \alpha_v + \lambda_t + \varepsilon_{evt} \tag{13}$$

where p_{evt} is the logarithm of the price from enterprise e in village v in month t, α_v are village fixed effects, γ_t are month fixed effects. We select the included radii bands $\bar{R}$ and the number of treatment lags M using the same pre-specified algorithm as for market prices. Table E.5 reports the average treatment effect across the intervention period (ATE) as well as the average maximum effect across villages (AME) from the optimal specification, and investigates heterogeneity by market access (see Section 3.7 for details on the methodology).

We find limited effects on these two selected services, with magnitudes in the range of product-specific effects for our our market price measures. Tailoring prices rise by 0.02% on average, and 0.1% in the month of most intense transfer, though those coefficients are not statistically significant. As with market prices, the effects are concentrated in more remote areas. Maize grinding prices fall, if anything, but the estimated effects are not statistically significantly different from zero.

Table E.3: Robustness to fixing alternative radii bands: Output Prices

		Overall Effects			
		(1) ATE Optimal Radius	(2) ATE $\bar{R} = 2$	(3) ATE $\bar{R} = 4$	(4) ATE $\bar{R} = 6$
All goods		0.0010*	0.0001	0.0010*	0.0014*
		(0.0006)	(0.0004)	(0.0006)	(0.0008)
By tradability	More tradable	0.0014	0.0003	0.0014	0.0021
		(0.0015)	(0.0010)	(0.0015)	(0.0020)
	Less tradable	0.0009	0.0001	0.0009	0.0012
		(0.0006)	(0.0004)	(0.0006)	(0.0008)
By sector	Food items	0.0009	0.0001	0.0009	0.0012
		(0.0006)	(0.0005)	(0.0006)	(0.0009)
	Non-durables	0.0014	0.0003	0.0014	0.0021
		(0.0017)	(0.0011)	(0.0017)	(0.0021)
	Durables	0.0019*	0.0000	0.0019*	0.0027
		(0.0011)	(0.0008)	(0.0011)	(0.0016)
	Livestock	−0.0008	0.0001	−0.0008	−0.0011
		(0.0010)	(0.0006)	(0.0010)	(0.0011)
	Temptation goods	−0.0011	−0.0022	−0.0011	0.0002
		(0.0026)	(0.0019)	(0.0026)	(0.0034)

Notes: This table replicates Column 1 of Table 4, and then estimates the same ATE based on specifications where the maximum radius is imposed to be at $R \in [2\text{km}, 6\text{km}]$. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table E.4: Output Prices - IV Specification

		(1)	(2)	(3)	(4)
		Overall Effects		**ATE by market access**	
		ATE	Average maximum effect (AME)	below median	above median
All goods		0.0005	0.0035	0.0007	0.0004
		(0.0007)	(0.0050)	(0.0008)	(0.0010)
By tradability	More tradable	0.0002	0.0018	0.0010	0.0003
		(0.0012)	(0.0088)	(0.0019)	(0.0014)
	Less tradable	0.0006	0.0043	0.0006	0.0004
		(0.0008)	(0.0057)	(0.0011)	(0.0012)
By sector	Food items	0.0006	0.0046	0.0007	0.0005
		(0.0008)	(0.0060)	(0.0011)	(0.0012)
	Non-durables	0.0003	0.0019	0.0013	0.0003
		(0.0013)	(0.0094)	(0.0020)	(0.0015)
	Durables	0.0001	0.0006	−0.0021	0.0013
		(0.0015)	(0.0112)	(0.0025)	(0.0021)
	Livestock	−0.0005	−0.0038	0.0001	−0.0015
		(0.0009)	(0.0067)	(0.0006)	(0.0017)
	Temptation goods	−0.0029	−0.0214	−0.0033	−0.0029
		(0.0020)	(0.0147)	(0.0028)	(0.0022)

Notes: Each row represents an IV regression of the logarithm of a price index on the "optimal" number of lags and distance buffers of per capita Give Directly transfers in each buffer (Equation 12). Price indices are based on 311,138 non-missing price quotes for 70 commodities and products. For each product, we take the logarithm of the median price quote in a market-month, and create our market price indices as an expenditure weighted average of these median price quotes across all goods in that market-month. Regressions include a panel of 1,734 market-by-month observations. The number of radii bands and lags is chosen sequentially by minimizing the BIC, as pre-specified, for the overall price index, which selects 2km. Treatment amounts for each buffer-month are instrumented by the share of eligible households assigned to treatment in that buffer, multiplied by the share of all transfers in that buffer going out in that month. Regressions include a full set of month fixed effects. Column 1 reports the implied ATE, calculated by evaluating the "optimal" regression specification at the average level of treatment intensity between September 2014 and March 2017, the time during which transfers went out. Column 2 reports the average maximum effect, calculated at the average across all markets of the month in which the largest per capita transfers went into a market's neighborhood (up to the largest buffer selected by the algorithm). Columns 3 and 4 break down the ATE by market access, defined as $MA_m = \sum_{r=1}^{10} r^{-\theta} N_r$, where $\theta = 8$ and N_r is the population in in the $r-2$ to r km buffer around each market. Standard errors (in parentheses) are as in Conley (2008) and we allow for spatial correlation up to 10km and autocorrelation up to 12 months. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table E.5: Local manufacturing and services prices

	(1)	(2)	(3)	(4)
	Overall Effects		**ATE by market access**	
	ATE	Average maximum effect (AME)	below median	above median
Tailor, patch small hole	0.0002	0.0011	0.0042	−0.0020
	(0.0034)	(0.0192)	(0.0033)	(0.0046)
Posho mill, grind 1kg of maize	−0.0011	−0.0061	−0.0022	−0.0010
	(0.0017)	(0.0097)	(0.0026)	(0.0014)

Notes: Each row represents a regression of the logarithm of a price on the "optimal" number of lags and distance buffers of per capita GiveDirectly transfers in each buffer (Equation13). We include 2,347 monthly price observations for tailors (simple patch), and 4,577 observations from posho mills (grinding 1kg of maize) collected between Aug 15 - Jun 16, around the time of peak transfer intensity. The number of radii bands and lags is chosen sequentially by minimizing the BIC, as pre-specified. Regressions include a full set of market and month fixed effects. Column 1 reports the implied ATE, calculated by evaluating the "optimal" regression specification at the average level of treatment intensity between September 2014 and March 2017, the time during which transfers went out. Column 2 reports the average maximum effect, calculated at the average across all markets of the month in which the largest per capita transfers went into a market's neighborhood (up to the largest buffer selected by the algorithm). Columns 3 and 4 break down the ATE by market access, defined as $MA_m = \sum_{r=1}^{10} r^{-\theta} N_r$, where $\theta = 8$ and N_r is the population in in the $r-2$ to r km buffer around each market. Regressions are weighted by inverse sampling weights. Standard errors (in parentheses) are as in Conley (2008) and we allow for spatial correlation up to 10km and autocorrelation up to 3 months. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

F Robustness to alternative spatial modelling approaches

In this section we examine the robustness of our statistical inferences and overall conclusions to several alternative ways of dealing with the issue of radius selection in our spatial models.

F.1 Fixed radii

We first examine results holding the spatial radius fixed at 2km (the maximal radius our BIC procedure usually selects) as well as at 4km and 6km. Generally speaking we expect to capture additional spillovers using the larger radii that we might miss at the shorter 2km radius, but at the cost of precision. Tables F.1, F.2, and F.3 mirror our main Tables 1, 2 and 3 but using this approach. In each table Column 1 reproduces our main estimate while Columns 2, 3 & 4 report estimates using fixed 2km, 4km and 6km radii, all for recipient households (or in Table F.3 for treated villages). Columns 5-8 then repeat this exercise for non-recipient households and control villages.

At a fixed 2km radius our results are (not surprisingly) similar if not identical to those from our default specification, but with the advantage that inference post-model selection is less of a concern. At higher radii the point estimates are generally similar to (or in some cases larger than) our benchmark estimates, though as expected the precision of our estimates decreases at higher radii. In almost all cases we cannot reject that the fixed-radius estimates are the same as our benchmark estimates.

F.2 Split-sample estimation

We next examine robustness to selecting a radius and estimating coefficients using different splits of the data. Specifically, we select 200 random 50-50 splits of our data, stratified by village treatment assignment and (for households) eligibility status, into training and estimation samples. For each split we use the training sample to select a radius and the estimation sample to estimate parameters. We repeat this exercise, using the estimation sample as the training sample and vice-versa. We record the proportion of splits in which we calculate the same optimal radius band as when using our full dataset; we take the mean of the two point estimates and report the proportion of cases in which the resulting estimate effect lies within the 95% confidence interval reported in the paper.

Tables F.4, F.5, and F.6 presents results for the outcomes found in Tables 1, 2 and 3, respectively. For Tables F.4 and F.5, columns 1 and 2 reproduce the estimates and radii selection for recipient households. Column 3 reports the fraction of these splits that produce estimates for non-recipients falling into the 95%-CI of the initial estimate, and Column 4 the proportion that select the same radius as when using the full dataset. Columns 5-8 do the same for non-recipient households. Note that as in producing our main estimates we do not separately estimate an optimal radius for subcomponents of larger totals or indices. For enterprises, TableF.6 columns 1 and 3 reproduce the main estimates for treatment and control villages, respective. Columns 2 and 4 report the share of mean estimates falling within the 95%-CI of the initial estimate. As we use a common radius for treatment and control villages, column 5 reports the selected radius, and column 6 reports the share selecting the same radius.

Overall we see congruence between the full data and the subsamples regarding the optimal radius over which to estimate effects, with most agreement rates in the 90%s. We also see good coverage, with 95% or more of the mean replicate point estimates falling within our original 95% confidence interval in most cases.

F.3 Heterogeneous radii

We next examine whether our BIC algorithm selects different maximal radii for different geographic sub-groups of villages. Specifically, we (i) allow the BIC to select a different radius for markets with above versus below median market access, and then (ii) allow the BIC to select a different radius for each of the three sub-counties in which our study is set. Tables F.7, F.8 and F.9 report the maximal radius selected in each case, with the full sample radius selected for comparison. For enterprise results, optimal radii bands were selected only once for each outcome across treatment and control villages, as the enterprises were not direct recipients of the cash transfers. In the great majority of cases we end up selecting the same radius (which in almost every case is 2km). Specifically, out of 190 radii selected (15 outcomes * 2 treatment status groups * 5 geographic subgroups for households + 8 outcomes * 5 geographic subgroups for enterprises) we select a different radius than in the corresponding pooled approach 10 times, or 5.3% of the total. Overall we conclude that, while there are surely are differences in the relevant radii or more generally the relevant "catchment areas" for different units, our data do not reveal systematic differences.

F.4 Randomization Inference

Finally, we examine the sensitivity of our conclusions to randomization inference. This approach sidesteps concerns about model selection; we simply interpret the coefficients we obtain from the entire model selection and estimation procedure as a statistic whose distribution should be invariant to reassignments of treatment and control status under the null of no treatment effects for any unit. Specifically, we generate 500 replicates in each of which we re-assign each village and household's treatment status using the same algorithm with which actual treatment was assigned, recalculate the our derived spatial exposure measures using these assignments, and then re-estimate total effects.

Tables F.10, F.11 and F.12 report results for the outcomes in Tables 1, 2 and 3, respectively. Table F.13 does the same but also simulating the randomized rollout of the transfer program in order to conduct randomization inference for output price outcomes in Table 4. Randomization inference yields very similar substantive conclusions to our main analysis, rejecting the null of no treatment effects for almost exactly the same outcomes as our main tests reject the null of no average effect.

Table F.1: Robustness to fixing alternative radii bands: Expenditures, Savings and Income

	Recipient households				Non-recipient households				
	(1) Total Effect IV Optimal Radius	(2) Total Effect IV $\bar{R}=2$	(3) Total Effect IV $\bar{R}=4$	(4) Total Effect IV $\bar{R}=6$	(5) Total Effect IV Optimal Radius	(6) Total Effect IV $\bar{R}=2$	(7) Total Effect IV $\bar{R}=4$	(8) Total Effect IV $\bar{R}=6$	(9) Control, low-saturation mean (SD)
Panel A: Expenditure									
Household expenditure, annualized	338.16***	338.16***	516.73***	644.26*	333.73***	333.73***	448.96**	−31.50	2,536.86
	(109.36)	(109.36)	(178.89)	(382.56)	(123.24)	(123.24)	(182.86)	(408.82)	(1,934.09)
Non-durable expenditure, annualized	226.74**	226.74**	459.92***	476.74	316.62***	316.62***	296.15**	235.08*	2,471.49
	(99.62)	(99.62)	(169.26)	(382.52)	(119.79)	(119.79)	(119.53)	(120.56)	(1,877.82)
Food expenditure, annualized	133.55**	133.55**	323.17***	220.53	132.84**	132.84**	123.56**	101.04*	1,578.43
	(63.98)	(63.98)	(94.08)	(197.79)	(58.58)	(58.58)	(59.18)	(60.55)	(1,072.31)
Temptation goods expenditure, annualized	5.88	5.88	14.88	28.63	−0.71	−0.71	−2.20	−5.09	37.10
	(8.82)	(8.82)	(11.87)	(25.23)	(6.50)	(6.50)	(6.54)	(7.27)	(123.59)
Durable expenditure, annualized	109.07***	109.07***	59.95***	147.41***	8.41	8.41	8.21	13.19	59.44
	(20.23)	(20.23)	(22.77)	(40.73)	(12.50)	(12.50)	(12.89)	(11.66)	(230.90)
Panel B: Assets									
Assets (non-land, non-house), net borrowing	182.01***	182.01***	204.08***	291.61**	132.63*	132.63*	244.56*	123.84	1,132.15
	(44.25)	(44.25)	(66.29)	(141.20)	(78.32)	(78.32)	(142.59)	(244.33)	(1,420.22)
Housing value	480.68***	480.68***	479.76***	595.96***	72.58	72.58	543.67	−402.46	2,033.72
	(38.88)	(38.88)	(48.66)	(103.95)	(215.70)	(215.70)	(474.16)	(845.95)	(5,030.37)
Land value	153.09	153.09	511.79	239.44	572.07	572.07	816.20	−261.31	5,030.72
	(262.48)	(262.48)	(357.15)	(672.45)	(458.28)	(458.28)	(728.50)	(1,477.50)	(6,607.61)
Panel C: Household balance sheet									
Household income, annualized	134.02	134.02	228.45	405.02	229.46***	229.46***	315.10**	171.59	1,023.45
	(93.83)	(93.83)	(152.75)	(311.97)	(88.59)	(88.59)	(143.05)	(334.25)	(1,634.70)
Net value of household transfers received, annualized	−7.44	−7.44	−6.50	−21.59	8.75	8.75	57.10*	7.45	130.18
	(13.06)	(13.06)	(15.69)	(32.14)	(19.10)	(19.10)	(30.66)	(58.05)	(263.75)
Tax paid, annualized	−0.09	−0.09	0.20	3.56	1.66	1.66	2.74	0.94	16.93
	(2.02)	(2.02)	(2.77)	(4.97)	(2.02)	(2.02)	(3.77)	(6.47)	(36.51)
Profits (ag & non-ag), annualized	33.73	33.73	90.18	183.24	44.08	44.08	91.17	125.89	485.20
	(48.95)	(48.95)	(84.27)	(149.09)	(45.35)	(45.35)	(77.81)	(154.16)	(787.10)
Wage earnings, annualized	73.72	73.72	96.93	201.44	182.99***	182.99***	180.03	−10.81	495.37
	(60.83)	(60.83)	(99.23)	(189.83)	(65.44)	(65.44)	(123.70)	(273.07)	(1,231.56)

Notes: Columns 1 and 5 replicate columns 2 and 3 from Table 1, selecting the number of radii bands included using our pre-specified algorithm as described in Section 3. The optimal radius selected is 2km for all outcomes. Columns 2-4 estimate the Total Effect (IV) for treated households, imposing a maximum radius $\bar{R}$ of 2, 4 and 6km respectively. Similarly, Columns 6-8 replicate Column 5, while including more spatial buffers. Standard errors are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.2: Robustness to fixing alternative radii bands: Input Prices and Quantities

	Recipient households				Non-recipient households				
	(1) Total Effect IV Optimal Radius	(2) Total Effect IV $\bar{R} = 2$	(3) Total Effect IV $\bar{R} = 4$	(4) Total Effect IV $\bar{R} = 6$	(5) Total Effect IV Optimal Radius	(6) Total Effect IV $\bar{R} = 2$	(7) Total Effect IV $\bar{R} = 4$	(8) Total Effect IV $\bar{R} = 6$	(9) Control, low-saturation mean (SD)
Panel A: Labor									
Hourly wage earned by employees	0.04	0.04	0.04	0.14	0.19*	0.19*	0.02	−0.11	0.70
	(0.04)	(0.04)	(0.07)	(0.10)	(0.10)	(0.10)	(0.16)	(0.30)	(0.89)
Household total hours worked, last 7 days	1.41	1.41	5.77	11.02	−4.70	−4.70	−3.16	−4.73	63.20
	(3.69)	(3.69)	(5.01)	(8.99)	(3.17)	(3.17)	(5.15)	(11.35)	(54.14)
Panel B: Land									
Land price per acre	365.44	365.44	694.99	815.15	556.83	556.83	556.44	−228.76	3,952.86
	(290.86)	(290.86)	(543.95)	(1,100.55)	(412.34)	(412.34)	(904.21)	(1,777.35)	(3,148.52)
Acres of land owned	−0.10	−0.10	0.02	0.10	0.08	0.08	0.12	0.04	1.42
	(0.09)	(0.09)	(0.16)	(0.55)	(0.10)	(0.10)	(0.17)	(0.40)	(2.37)
Panel C: Capital									
Loan-weighted interest rate, monthly	0.01	0.01	0.04**	0.02	−0.01	−0.01	−0.02	0.00	0.06
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.04)	(0.07)
Total loan amount	3.13	3.13	20.55*	59.32**	6.36	6.36	29.65	32.64	80.61
	(8.34)	(8.34)	(11.88)	(25.06)	(13.21)	(13.21)	(19.62)	(33.18)	(204.36)

Notes: Columns 1 and 5 replicate Columns 2 and 3 from Table 2, selecting the number of radii bands included using our pre-specified algorithm as described in Section 3. The optimal radius selected is 2km for all outcomes for both recipients and non-recipientsand groups. Columns 2, 3 and 4 estimate the Total Effect (IV) for treated households, imposing a maximum radius R of 2, 4 and 6km respectively. Similarly, Columns 6-8 replicate Column 5, while including more spatial buffers. Standard errors are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.3: Robustness to fixing alternative radii bands: Enterprise Outcomes

	Treatment Villages				Control Villages				
	(1) Total Effect IV Optimal Radius	(2) Total Effect IV $\bar{R} = 2$	(3) Total Effect IV $\bar{R} = 4$	(4) Total Effect IV $\bar{R} = 6$	(5) Total Effect IV Optimal Radius	(6) Total Effect IV $\bar{R} = 2$	(7) Total Effect IV $\bar{R} = 4$	(8) Total Effect IV $\bar{R} = 6$	(9) Control, low-saturation mean (SD)
Panel A: All enterprises									
Enterprise profits, annualized	67.53	67.53	156.38**	130.79	32.91	32.91	115.06	89.98	323.39
	(41.62)	(41.62)	(78.24)	(165.55)	(37.27)	(37.27)	(71.76)	(147.69)	(691.12)
Enterprise revenue, annualized	356.81**	356.81**	749.04***	831.13	244.27**	244.27**	596.71***	667.56	758.52
	(144.21)	(144.21)	(247.61)	(531.30)	(108.96)	(108.96)	(202.05)	(449.43)	(2,493.40)
Enterprise costs, annualized	96.82**	96.82**	93.50	−78.50	77.02	77.02	72.72	−97.34	147.73
	(40.92)	(40.92)	(63.04)	(145.28)	(48.95)	(48.95)	(68.60)	(145.39)	(550.11)
Enterprise wagebill, annualized	81.69**	81.69**	71.73	−55.13	70.49*	70.49*	63.91	−47.54	120.62
	(33.76)	(33.76)	(56.97)	(124.37)	(36.52)	(36.52)	(52.40)	(104.56)	(492.11)
Enterprise profit margin	−0.06**	−0.06**	−0.10*	−0.10	−0.06***	−0.06***	−0.10**	−0.11	0.44
	(0.03)	(0.03)	(0.05)	(0.09)	(0.02)	(0.02)	(0.05)	(0.09)	(0.61)
Panel B: Non-agricultural enterprises									
Enterprise inventory	34.68**	34.68**	33.39	−41.12	16.91	16.91	16.09	−49.24	192.98
	(14.73)	(14.73)	(20.68)	(48.61)	(10.80)	(10.80)	(14.79)	(41.76)	(504.76)
Enterprise investment, annualized	13.58	13.58	9.46	−22.43	6.82	6.82	3.22	−24.52	178.25
	(15.39)	(15.39)	(26.77)	(50.12)	(8.65)	(8.65)	(18.59)	(41.39)	(640.98)
Panel C: Village-level									
Number of enterprises	0.02	0.02	0.02	−0.04	0.01	0.01	0.01	−0.05	1.12
	(0.01)	(0.01)	(0.02)	(0.04)	(0.01)	(0.01)	(0.02)	(0.04)	(0.14)

Notes: Columns 1 and 5 replicate Columns 2 and 3 from Table 3, selecting the number of radii bands included using our pre-specified algorithm as described in Section 3. The optimal radius selected is 2km for all outcomes and groups. Columns 2, 3 and 4 estimate the Total Effect (IV) for enterprises in treated villages, imposing a maximum radius R of 2, 4 and 6km respectively. Similarly, Columns 5 and 6 replicate Column 4, while including more spatial buffers. Standard errors are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.4: BIC split sample approach for household expenditure, savings and income outcomes

	Recipient Households				Non-Recipient Households			
	Main Estimate		200 Split Sets		Main Estimate		200 Split Sets	
	(1) Total Effect (IV)	(2) Selected Radius	(3) Share in 95% CI	(4) Share selecting same radius	(5) Total Effect (IV)	(6) Selected Radius	(7) Share in 95% CI	(8) Share selecting same radius
Panel A: Expenditure								
Household expenditure, annualized	338.16***	2km	100%	100%	333.73***	2km	100%	100%
	(109.36)				(123.24)			
Non-durable expenditure, annualized	226.74**	2km	100%	100%	316.62***	2km	100%	100%
	(99.62)				(119.79)			
Food expenditure, annualized	133.55**	2km	100%	100%	132.84**	2km	100%	100%
	(63.98)				(58.58)			
Temptation goods expenditure, annualized	5.88	2km	100%	100%	−0.71	2km	100%	100%
	(8.82)				(6.50)			
Durable expenditure, annualized	109.07***	2km	100%	100%	8.41	2km	100%	100%
	(20.23)				(12.50)			
Panel B: Assets								
Assets (non-land, non-house), net borrowing	182.01***	2km	100%	99%	132.63*	2km	100%	100%
	(44.25)				(78.32)			
Housing value	480.68***	2km	100%	89%	72.58	2km	100%	100%
	(38.88)				(215.70)			
Land value	153.09	2km	100%	100%	572.07	2km	100%	99%
	(262.48)				(458.28)			
Panel C: Household balance sheet								
Household income, annualized	134.02	2km	100%	100%	229.46***	2km	100%	98%
	(93.83)				(88.59)			
Net value of household transfers received, annualized	−7.44	2km	100%	100%	8.75	2km	100%	100%
	(13.06)				(19.10)			
Tax paid, annualized	−0.09	2km	100%	100%	1.66	2km	100%	100%
	(2.02)				(2.02)			
Profits (ag & non-ag), annualized	33.73	2km	100%	100%	44.08	2km	100%	100%
	(48.95)				(45.35)			
Wage earnings, annualized	73.72	2km	100%	100%	182.99***	2km	100%	94%
	(60.83)				(65.44)			

Notes: Columns 1 and 2 reproduce the total effect estimates and radii selection for recipient households in Table 1. Columns 3 and 4 show the fraction of 200 random 50-50 splits of the sample for which the mean point estimates (across the two estimation samples within each split) for recipients falling into the 95%-CI of the initial estimate (Column 3) and select the same radius as the initial selection (Column 4). Columns 5 - 8 do the same for non-recipient households. See Table 1 for more details on variable construction and regression specification. Standard errors in Columns 1 and 5 are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.5: BIC split sample approach for input prices and quantities

	Recipient Households				Non-Recipient Households			
	Main Estimate		200 Split Sets		Main Estimate		200 Split Sets	
	(1) Total Effect (IV)	(2) Selected Radius	(3) Share in 95% CI	(4) Share selecting same radius	(5) Total Effect (IV)	(6) Selected Radius	(7) Share in 95% CI	(8) Share selecting same radius
Panel A: Labor								
Hourly wage earned by employees	0.04	2km	100%	100%	0.19*	2km	100%	93%
	(0.04)				(0.10)			
Household total hours worked, last 7 days	1.41	2km	100%	100%	−4.70	2km	100%	100%
	(3.69)				(3.17)			
Panel B: Land								
Land price per acre	365.44	2km	100%	99%	556.83	2km	100%	95%
	(290.86)				(412.34)			
Acres of land owned	−0.10	2km	100%	99%	0.08	2km	100%	100%
	(0.09)				(0.10)			
Panel C: Capital								
Loan-weighted interest rate, monthly	0.01	2km	99%	100%	−0.01	2km	100%	97%
	(0.01)				(0.01)			
Total loan amount	3.13	2km	100%	100%	6.36	2km	100%	100%
	(8.34)				(13.21)			

Notes: Columns 1 and 2 reproduce the total effect estimates and radii selection for recipient households in Table 2. Columns 3 and 4 show the fraction of 200 random 50-50 splits of the sample for which the mean point estimates (across the two estimation samples within each split) for recipients falling into the 95%-CI of the initial estimate (Column 3) and select the same radius as the initial selection (Column 4). Columns 5 - 8 do the same for non-recipient households. See Table 2 for more details on variable construction and regression specification. Standard errors in Columns 1 and 5 are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.6: BIC split sample approach for enterprise outcomes

	Treatment Villages		Control Villages		Radii Selected	
	(1) Total Effect (IV)	(2) Share in 95% CI	(3) Total Effect (IV)	(4) Share in 95% CI	(5) Main Estimate	(6) Share selecting same radius
Panel A: All enterprises						
Enterprise profits, annualized	67.53	100%	32.91	100%	2km	97%
	(41.62)		(37.27)			
Enterprise revenue, annualized	356.81**	100%	244.27**	100%	2km	97%
	(144.21)		(108.96)			
Enterprise costs, annualized	96.82**	90%	77.02	96%	2km	85%
	(40.92)		(48.95)			
Enterprise wagebill, annualized	81.69**	83%	70.49*	89%	2km	80%
	(33.76)		(36.52)			
Enterprise profit margin	−0.06**	100%	−0.06***	100%	2km	98%
	(0.03)		(0.02)			
Panel B: Non-agricultural enterprises						
Enterprise inventory	34.68**	99%	16.91	100%	2km	99%
	(14.73)		(10.80)			
Enterprise investment, annualized	13.58	100%	6.82	100%	2km	99%
	(15.39)		(8.65)			
Panel C: Village-level						
Number of enterprises	0.02	92%	0.01	92%	2km	94%
	(0.01)		(0.01)			

Notes: Columns 1 and 3 reproduce the total effect estimates for enterprises located in treatment and control villages from Table 3. Column 5 reports the radii selection (which is done across all enterprises jointly, since enterprises are not direct recipients of cash transfers). Columns 2 and 4 show the fraction of 200 random 50-50 splits of the sample for which the mean point estimates (across the two estimation samples within each split) produce estimates falling into the 95%-CI of the initial estimates in Columns 1 and 3 respectively. Column 6 shows the share of these splits where the algorighm selects the same radius as the initial selection in Column 5. See Table 3 for more details on variable construction and regression specification. Standard errors in Columns 1 and 3 are calculated following Conley (2008) using a uniform kernel out to 10 km. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table F.7: Maximum Radius Chosen by the BIC Algorithm (in km), expenditure, saving and income outcomes

	Recipient Households						Non-recipient Households					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Full Sample	Low market access	High market access	Alego	Ugunja	Ukwala	Full Sample	Low market access	High market access	Alego	Ugunja	Ukwala
Panel A: Expenditure												
Household expenditure, annualized	2	2	2	2	2	2	2	2	2	2	2	2
Panel B: Assets												
Assets (non-land, non-house), net borrowing	2	2	2	2	2	2	2	2	2	2	2	2
Housing value	2	2	2	2	**4**	**4**	2	2	2	2	2	2
Land value	2	2	2	2	2	2	2	2	2	**4**	2	2
Panel C: Household balance sheet												
Household income, annualized	2	2	2	2	2	2	2	2	2	2	2	2
Net value of household transfers received, annualized	2	2	2	2	2	2	2	2	2	2	2	2
Tax paid, annualized	2	2	2	2	2	2	2	2	2	2	2	2
Profits (ag & non-ag), annualized	2	2	2	2	2	2	2	2	2	2	2	2
Wage earnings, annualized	2	2	2	2	2	2	2	2	2	2	2	2

Notes: This table reports the maximum radius selected minimizing a Baysian Information Criterion within different sub-samples of our data for outcomes in Table 1. Columns 1-6 report the radius selected when estimating effects on recipient households and when using the full sample (Column 1), when restricting the sample to villages with higher and lower than median market access (Columns 2 and 3), and to households in the three sub-counties Alego, Ugunja and Ukwala (Columns 4 - 6). Columns 7-12 report the same for non-recipient households.

Table F.8: Maximum Radius Chosen by the BIC Algorithm (in km), input prices and quantities

	Recipient Households						Non-recipient Households					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Full Sample	Low market access	High market access	Alego	Ugunja	Ukwala	Full Sample	Low market access	High market access	Alego	Ugunja	Ukwala
Panel A: Labor												
Hourly wage earned by employees	2	2	2	2	**4**	2	2	2	2	2	2	2
Household total hours worked, last 7 days	2	2	2	2	2	2	2	2	2	2	2	2
Panel B: Land												
Land price per acre	2	2	2	2	**16**	2	2	2	2	2	2	2
Acres of land owned	2	2	2	2	2	2	2	2	2	2	2	2
Panel C: Capital												
Loan-weighted interest rate, monthly	2	2	2	2	2	2	2	2	2	2	2	**4**
Total loan amount	2	2	2	2	2	2	2	2	2	2	2	2

Notes: This table reports the maximum radius selected minimizing a Baysian Information Criterion within different sub-samples of our data for outcomes in Table 2. Columns 1-6 report the radius selected when estimating effects on recipient households and when using the full sample (Column 1), when restricting the sample to villages with higher and lower than median market access (Columns 2 and 3), and to households in the three sub-counties Alego, Ugunja and Ukwala (Columns 4 - 6). Columns 7-12 report the same for non-recipient households.

Table F.9: Maximum Radius Chosen by the BIC Algorithm (in km), enterprise outcomes

		Market Access		**Subcounty**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Low market access	High market access	Alego	Ugunja	Ukwala
Panel A: All enterprises						
Enterprise profits, annualized	2	**4**	2	2	2	2
Enterprise revenue, annualized	2	**4**	2	2	2	2
Enterprise costs, annualized	2	**8**	2	2	2	2
Enterprise wagebill, annualized	2	2	2	2	2	2
Enterprise profit margin	2	2	2	2	2	2
Panel B: Non-agricultural enterprises						
Enterprise inventory	2	**8**	2	2	2	2
Enterprise investment, annualized	2	2	2	2	2	2
Panel C: Village-level						
Number of enterprises	2	2	2	2	2	2

Notes: This table reports the maximal radius selected by minimizing a Baysian Information Criterion within different sub-samples of our data for enterprise outcomes in Table 3. We report the radius selected when using the full sample (Column 1), when restricting the sample to villages with higher and lower than median market access (Columns 2 and 3), and to enterprises in the three sub-counties Alego, Ugunja and Ukwala (Columns 4 - 6).

Table F.10: Randomization inference for expenditure, savings and income outcomes

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	Total Effect IV	Spatial RI p-value	Total Effect IV	Spatial RI p-value
Panel A: Expenditure				
Household expenditure, annualized	338.16***	[0.00]***	333.73***	[0.02]**
	(109.36)		(123.24)	
Non-durable expenditure, annualized	226.74**	[0.03]**	316.62***	[0.02]**
	(99.62)		(119.79)	
Food expenditure, annualized	133.55**	[0.07]*	132.84**	[0.07]*
	(63.98)		(58.58)	
Temptation goods expenditure, annualized	5.88	[0.59]	−0.71	[0.90]
	(8.82)		(6.50)	
Durable expenditure, annualized	109.07***	[0.00]***	8.41	[0.43]
	(20.23)		(12.50)	
Panel B: Assets				
Assets (non-land, non-house), net borrowing	182.01***	[0.00]***	132.63*	[0.24]
	(44.25)		(78.32)	
Housing value	480.68***	[0.00]***	72.58	[0.80]
	(38.88)		(215.70)	
Land value	153.09	[0.61]	572.07	[0.29]
	(262.48)		(458.28)	
Panel C: Household balance sheet				
Household income, annualized	134.02	[0.16]	229.46***	[0.07]*
	(93.83)		(88.59)	
Net value of household transfers received, annualized	−7.44	[0.51]	8.75	[0.59]
	(13.06)		(19.10)	
Tax paid, annualized	−0.09	[0.97]	1.66	[0.43]
	(2.02)		(2.02)	
Profits (ag & non-ag), annualized	33.73	[0.46]	44.08	[0.47]
	(48.95)		(45.35)	
Wage earnings, annualized	73.72	[0.36]	182.99***	[0.06]*
	(60.83)		(65.44)	

Notes: This table presents randomization inference results for outcomes in Table 1. Column 1 reproduces the total effect for recipient households (Column 2 in Table 1), and Column 3 reproduces the total effect for non-recipient households (Column 3 in Table 1). Columns 2 and 4 report randomization inference p-values from 500 repetitions (i.e. the share of replications where the estimated total effect is larger than the main estimate), where the cross-sectional distribution of cash distributed in radii bands across villages was randomly shuffled.

Table F.11: Randomization inference for input prices and quantities

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	Total Effect IV	Spatial RI p-value	Total Effect IV	Spatial RI p-value
Panel A: Labor				
Hourly wage earned by employees	0.04	[0.55]	0.19*	[0.21]
	(0.04)		(0.10)	
Household total hours worked, last 7 days	1.41	[0.73]	−4.70	[0.15]
	(3.69)		(3.17)	
Panel B: Land				
Land price per acre	365.44	[0.33]	556.83	[0.14]
	(290.86)		(412.34)	
Acres of land owned	−0.10	[0.64]	0.08	[0.56]
	(0.09)		(0.10)	
Panel C: Capital				
Loan-weighted interest rate, monthly	0.01	[0.62]	−0.01	[0.62]
	(0.01)		(0.01)	
Total loan amount	3.13	[0.77]	6.36	[0.68]
	(8.34)		(13.21)	

Notes: This table presents randomization inference results for outcomes in Table 2. Column 1 reproduces the total effect for recipient households (Column 2 in Table 2), and Column 3 reproduces the total effect for non-recipient households (Column 3 in Table 2). Columns 2 and 4 report randomization inference p-values from 500 repetitions (i.e. the share of replications where the estimated total effect is larger than the main estimate), where the cross-sectional distribution of cash distributed in radii bands across villages was randomly shuffled.

Table F.12: Randomization inference for enterprise outcomes

	(1)	(2)	(3)	(4)
	Treated Villages		**Control Villages**	
	Total Effect IV	Spatial RI p-value	Total Effect IV	Spatial RI p-value
Panel A: All enterprises				
Enterprise profits, annualized	67.53 (41.62)	[0.23]	32.91 (37.27)	[0.43]
Enterprise revenue, annualized	356.81** (144.21)	[0.05]**	244.27** (108.96)	[0.06]*
Enterprise costs, annualized	96.82** (40.92)	[0.03]**	77.02 (48.95)	[0.03]**
Enterprise wagebill, annualized	81.69** (33.76)	[0.04]**	70.49* (36.52)	[0.03]**
Enterprise profit margin	−0.06** (0.03)	[0.12]	−0.06*** (0.02)	[0.03]**
Panel B: Non-agricultural enterprises				
Enterprise inventory	34.68** (14.73)	[0.03]**	16.91 (10.80)	[0.12]
Enterprise investment, annualized	13.58 (15.39)	[0.33]	6.82 (8.65)	[0.47]
Panel C: Village-level				
Number of enterprises	0.02 (0.01)	[0.23]	0.01 (0.01)	[0.56]

Notes: This table presents randomization inference results for outcomes in Table 3. Column 1 reproduces the total effect for enterprises in treated villages (Column 2 in Table 3), and Column 3 reproduces the total effect for enterprises in control villages (Column 3 in Table 3). Columns 2 and 4 report randomization inference p-values from 500 repetitions (i.e. the share of replications where the estimated total effect is larger than the main estimate), where the cross-sectional distribution of cash distributed in radii bands across villages was randomly shuffled.

Table F.13: Randomization inference for price outcomes

		(1)	(2)	(3)	(4)
		Overall Effects		**ATE by market access**	
		ATE	Average maximum effect (AME)	below median	above median
All goods		0.0010*	0.0042	0.0017*	0.0007
		(0.0006)	(0.0031)	(0.0009)	(0.0007)
		[0.070]	[0.110]	[0.098]	[0.500]
By tradability	More tradable	0.0014	0.0062	0.0023	0.0021
		(0.0015)	(0.0082)	(0.0023)	(0.0018)
		[0.268]	[0.370]	[0.264]	[0.382]
	Less tradable	0.0009	0.0034	0.0015	0.0001
		(0.0006)	(0.0032)	(0.0011)	(0.0008)
		[0.268]	[0.460]	[0.536]	[0.856]
By sector	Food items	0.0009	0.0036	0.0016	0.0002
		(0.0006)	(0.0033)	(0.0012)	(0.0008)
		[0.248]	[0.434]	[0.528]	[0.796]
	Non-durables	0.0014	0.0061	0.0026	0.0019
		(0.0017)	(0.0089)	(0.0026)	(0.0019)
		[0.310]	[0.422]	[0.234]	[0.416]
	Durables	0.0019*	0.0070	-0.0009	0.0034**
		(0.0011)	(0.0061)	(0.0011)	(0.0016)
		[0.052]	[0.152]	[0.630]	[0.094]
	Livestock	-0.0008	-0.0027	-0.0008*	-0.0017
		(0.0010)	(0.0052)	(0.0004)	(0.0020)
		[0.224]	[0.534]	[0.288]	[0.500]
	Temptation goods	-0.0011	-0.0112	-0.0008	-0.0003
		(0.0026)	(0.0143)	(0.0036)	(0.0035)
		[0.734]	[0.522]	[0.810]	[0.946]

Notes: This table presents randomization inference results for outcomes in Table 4. Randomization inference p-values from 500 repetitions (i.e. the share of replications where the estimated total effect is larger than the main estimate) are reported in brackets. In each iteration, we randomly re-generate cross-sectional and temporal roll out of transfers according to our actual treatment randomization as described in Section 2.2.

G Estimating the marginal propensity to consume and spend locally

This appendix section provides details on the marginal propensity to consume (MPC) estimates reported in section 4.2.

We focus on recipients' propensity to spend as a result of the transfer, which is directly relevant for the first-round spending impacts in the local economy. To the extent that recipient households generate additional earned income as the result of the transfer, and also spend out of this income, the main marginal propensity to consume estimate may be an overestimate. Below, we therefore also present recipient expenditure effects relative to the transfer amount received plus any additional income generated as a result of the transfer. (We are also able to obtain an estimate of the marginal propensity to consume among transfer non-recipients, by taking the ratio of spending impacts relative to income effects over the same time period. In fact, the estimates for non-recipients are quantitatively similar to those estimated among cash transfer recipients. Since income is likely to be imperfectly measured relative to expenditure in this context, see Deaton and Zaidi (2002), and because first-round spending impacts are particularly important, we focus on the MPC among transfer recipients.)

In rural African settings like ours, formal sector financial savings (e.g., in bank accounts) or cash savings are limited. Only 10% of households in our study area report having a bank account at endline. In ongoing work in a similar Kenyan context, total savings in mobile money, cash and bank accounts amounted to roughly 100 USD PPP in the control group, a small share of total assets. The effect on total savings of a 1000 USD PPP transfer (which is roughly half the size of the transfer in our study) after 14 months was only roughly 25 USD PPP, or 2.5% of the transfer. Instead, most household saving comes in the form of purchases of relatively liquid durable assets such as livestock or even housing materials. In what follows, we separately present recipient spending on durable assets and non-durable consumption goods. From an intertemporal decision-making perspective, the latter represents pure "consumption", while the former is likely have both a "consumption" and a "savings" component.

Whether they are "consumed" or "saved", expenditures on both durables and non-durables are predominantly local: over 95% of respondents report shopping locally for both types of goods. In a context where financial savings options are limited, high marginal propensities to spend — which as noted above, is not necessarily the same as to consume — should not be unexpected. From the perspective of quantifying the transfer multiplier, it is this marginal propensity to *spend* that matters, as spending on both "consumption goods" and "savings goods" show up as revenue for local firms, and have a similar stimulus effect on the local economy. Our main estimate of the MPC (MPC total) therefore includes both components.

Importantly, recipient expenditures only enter the local economy, and thus generate a local multiplier, if they occur locally and contribute to the income of another local agent. We call the measure of this type of expenditure the marginal propensity to spend locally (MPC local). Since the vast majority of individuals in the study sample work locally and firms are overwhelmingly locally owned (as noted in the main text), we expect nearly all factor payments to remain in the local economy. The main reason why local revenue might not end up as local income is the importing of intermediate goods. In Appendix Section

H.1, we calculate that up to 19% of non-durable consumption and 20% of durable purchases indirectly reflect imports of intermediate goods from outside the study area. Our preferred measure of the MPC local adjusts the overall marginal propensity to spend (MPC total) to account for such imports, leading the MPC local to be smaller in magnitude than MPC total.

Table G.1: Estimates of recipients' marginal propensity to consume

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Transfer					**Transfer + Income Gains**	
	MPC non-durables		**MPC durables**	**MPC total**	**MPC local**	**MPC total**	**MPC local**
	q1-q3	q4-q10					
Our data only	-0.22	0.29	0.30	0.37	0.30	0.33	0.27
	(0.22)	(0.12)	(0.05)	(0.22)	(0.18)	(0.18)	(0.15)
Rarieda data q1-3, our data q4-10	0.35	0.29	0.30	0.93	0.76	0.84	0.68
	(0.11)	(0.12)	(0.05)	(0.16)	(0.13)	(0.12)	(0.10)

Notes: This table presents estimates of recipients' marginal propensity to consume. Columns 1 and 2 report total effects on non-durable expenditure over the first 3 and the next 7 quarters after the transfer respectively relative to the average treatment amount received, and estimated dynamically according to Equation 7. Column 3 adds the effect on accumulated durable assets (including house value) for recipients at endline, estimated using Equation 2. Column 4 sums up Columns 1-3, presenting our main marginal propensity to spend (MPC total). Column 5 adjusts Column 4 by accounting for an estimated 20% of durables and 18% of non-durables expenditure that reflects imports of intermediates, yielding the marginal propensity to spend locally (MPC local). See Appendix H.1 for details. Columns 6 and 7 present these effects relative to the average transfer amount plus the average additional income recipients' generated over the 27 months after the transfer, again estimated using Equation 2. The first row estimates the MPC using only data from this study. The second row estimates the first 3 quarters of the non-durable expenditure effect in Column 1 using midline expenditure data from households in neighboring Rarieda county that received similar transfers as part of Haushofer and Shapiro (2016), which has the advantage of capturing the immediate expenditure response to the transfer. It is estimated analogously using Equation 7, but excluding spillover terms. Standard errors (in parentheses) come from 2000 iterations of a wild-bootstrap, clustered at the sublocation level for our data, and the village level for Rarieda data.

Row 1 in Table G.1 presents MPC estimates using data only from this study. We estimate a marginal propensity to spend on non-durables of 0.07 over the first 10 quarters after the initial transfer, and 0.30 on on durables. Combined, this yields a marginal propensity to spend of 0.37. We are thus able to directly account for 37% of the transfer. Adjusted for imported intermediates, the MPC local is 0.30.

As noted in the main text, this estimate faces the important limitation that the endline data collection started about 9 months after transfers in a village went out (see Figure A.1b). Recall periods for non-durable consumption range from a week to a month, making it hard to convincingly estimate *direct* spending effects for recipients on non-durables in the initial months after the transfer. Regarding measures from our data, we show in Figure 1 that the observed variation in transfer amounts received in the 3 quarters prior to our surveys is limited, leading to imprecise estimates (that are also small or even negative). This limitation is less relevant for the estimation of across-village spillovers because transfers to surrounding villages may have randomly gone out earlier or later relative to the timing of the survey, thus providing ample variation to estimate early spillover effects over a short time horizon. Note that estimates of durables expenditure effects do not face the same problem as non-durable consumption, since we measure these as the difference in the stock of durables at endline (between treatment and control), rather than as an integral of flows.

To overcome this limitation, the second row of Table G.1 brings in additional evidence using data collected as part of a closely related project in the neighboring sub-county of Rarieda that collected more detailed data on household spending in the months immediately after receipt of similar GiveDirectly transfers (Haushofer and Shapiro 2016). Endline surveys were conducted an average of 9 months after transfers. In addition, a random subset of this sample was surveyed in each of the first 6 months after transfers went out. Here, we use the combined midline and endline data for households which were surveyed in both rounds to estimate the short-run impacts of transfers on recipient spending[53]. The setting of the Rarieda study is remarkably similar to the one studied in this paper: same implementing partner, same eligibility criteria, similar geography and economic structure and only 3 years between them. However, there are two key features that differed and warrant discussion.

First, the Rarieda study randomized treatment among eligibles *within* villages, while in our study, all eligibles within a village are treated. Moreover, the Rarieda study design did incorporate geographic density of treatment across villages. Thus, the Rarieda data allows us to obtain only estimates of the *direct* impact of cash transfers on recipient spending, not including within-village spillovers or across-village spillovers. We expect the bias from excluding spillovers to be small for the initial non-durable spending impacts on recipients. Table 1 shows that across-village spillover effects for recipient non-durable spending are small 18 months after transfers (compare columns (1) and (2)). As we expect spillovers to increase over time, as money begins circulating, they are likely to be even smaller in the initial months. Moreover, Haushofer and Shapiro (2016) show that within-village spillovers in their setting were small and not statistically significant over the first 9 months.

Second, average transfer amounts in Rarieda were only about half the size of transfers in our study – recipients randomly received either 404 USD PPP or 1525 USD PPP – and transfers were randomly either sent as a lump-sum or monthly installments over 9 months. In our study, transfers were sent in 3 instalments over 8 months, a schedule that lies somewhere inbetween the two Rarieda transfer schedules. For estimation, we assume that recipient spending effects are linear in transfer amounts, and do not vary with the scheduling of transfers. Haushofer and Shapiro (2016) show that although initial spending impacts increase slightly less than linearly with the transfer amount, there is also a larger increase in early purchases of large, expensive items in the lump-sum arm. While the former may lead estimates from Rarieda to be overstated compared to our larger transfers, the latter may lead to a bias in the opposite direction.

Although we cannot exactly estimate the potential bias resulting from differences in study design, we can test whether estimated impacts on recipients' spending path are comparable between Rarieda and our data at a time horizon where we have sufficient data in both studies. The p-value for the hypothesis that the impact of cash on recipient non-durable spending 4-5 quarters after transfer are the same in our data and in Rarieda is $p = 0.31$. Together with the considerations above, we view these two studies as broadly comparable.

In our preferred estimate of the marginal propensity to consume, we therefore estimate the non-durable spending impact for recipients in the first 3 quarters from Rarieda data, and use our own data thereafter. Specifically, we estimate the dynamic impact of transfers on recipient spending according to Equation 7 as we do for our data, but excluding spillover

53. Note that Haushofer and Shapiro (2016) focus solely on endline data.

terms as discussed above. We deflate monetary values using the overall Kenyan CPI for Rarieda, and our own market price indices for the GE data. Using per-dollar coefficient estimates from Rarieda data, we then simulate the initial spending impact from transfers sent according to the schedule in our study, i.e., 3 transfers totalling USD 1,871 PPP (USD 1,000 nominal) over 8 months based on the Rarieda coefficients.

Column 1 shows that initial direct spending impacts on non-durable goods in Rarieda were indeed far higher than what we estimate in our data, at 0.35. Combined with our data on non-durable expenditure in the quarters thereafter, we estimate that recipients' spend 64% of the transfer on non-durables over the first 10 quarters. Adding in durable expenditure yields our preferred estimate of the marginal propensity to spend (MPC total) of 0.93. This indicates that we are close to accounting for the entire transfer amount being spent, and highlights that the study population can be characterized as largely hand-to-mouth consumers. Even when we account for increased income generated by recipients over the same period in Column 6, the estimate of the total marginal propensity to spend remains very high, at 0.84. This is again in line with the observation that savings in formal financial products or even in cash are unlikely to be substantial in this context.

The preferred estimate of the marginal propensity spend locally, which accounts for imports of intermediate goods is presented in Column 5, and yields an estimated MPC local of 0.76. An alternative estimate that accounts for any additional income generated (among transfer recipients) is similar, at 0.68 (Column 7). These calculations illustrate that a large share of transfer is spent by recipient households within our study period, and roughly three quarters re-enters the local economy and ends up as income of another local agent. In a simple static Keynesian framework, an MPC local in the range of 0.68 to 0.76 implies a local economy transfer multiplier $\frac{MPC}{1-MPC}$ between 2.1 to 3.2

H Transfer multiplier - robustness

This section conducts three main robustness checks regarding the multiplier analysis. In the first subsection, we attempt to account for transactions between agents in our study area and those located outside it. Using a combination of household and enterprise data, and conservative assumptions on import shares by type of enterprise, we provide an upper bound on the share of the expenditure multiplier that may reflect increased imports from outside the study area. The second subsection makes alternative assumptions about the expenditure effects in the initial months after transfers, which as noted in the main text are noisily estimated in our data because the average endline survey took place 18 months after the first transfers were received. Third, we present estimates in nominal terms (rather than real terms).

H.1 Accounting for imports of intermediate goods

As described in Section 5, the main expenditure multiplier incorrectly includes imports which are not part of local value added. There are many reasons to believe that any resulting bias is relatively small. From household shopping patterns, we know that only 10% of households report ever shopping at a market outside our study area. Non-farm businesses report only 5% of customers coming from outside the study area. In addition, the estimated effects on household consumption and enterprise revenue are fairly similar, suggesting that consumer spending was quite localized and direct imports by households are relatively small. The main concern is therefore imported intermediate goods.

To gauge whether this bias is quantitatively important, we first assign each component of our non-durable expenditure and durable asset measures to one of 48 enterprise types where it is most likely to be purchased. When there are multiple possible types of enterprises, we use overall revenue shares of different enterprise types to distribute expenditure between them. Reassuringly, this correspondence implies expenditure shares by enterprise type that match their revenue shares from the enterprise survey fairly well (correlation coefficient of 0.62). For each enterprise type, we then obtain an upper-bound for the share of intermediate inputs in overall value added as: $1 - \frac{1}{N}\sum_i w_i \frac{cost_i + profit_i}{revenue_i}$ (where we first winsorize at the 1% and 99% cut-offs, then average across enterprises of each type using revenue weights, and cap at 0 and 1), and where i denotes a firm and N is the total number of firms of that type, and w_i the revenue weight of firm i (re-scaled to sum to 1). This is clearly an upper bound, since the enterprise survey cost measure only contains selected components of firm costs.

Next, we make assumptions based on an understanding of the local context about what share of intermediate inputs is imported from outside the study area. In doing so, we try to err on the side of an import share that is too high. The total share of imports in consumption expenditure and assets is then calculated as the expenditure-weighted share of imports of intermediate goods for each expenditure and asset category. For the exact correspondence between each consumption good or asset and enterprise types, consult Tables H.1 and H.2.

Using this methodology, the upper bound estimate of the share of imports in non-durable consumption goods is 18%, and for assets, the figure is 20%. This shows that imports of intermediate goods may be non-negligible, but that a large majority of spending still reflects local economic activity (and recall that these figures are upper bounds). To get a sense

Table H.1: Non-durable expenditure: Intermediate input and import shares

Item	Bought atenterprise type	(1) Expenditure share (data)	(2) Intermediate input share (data)	(3) Intermediate import share (assumed)	(4) Overall import share
Cereals	Cereals	5.9%	60%	50%	30%
	Posho mill	5.9%	26%	0%	0%
	Small retail	2.9%	65%	75%	49%
Roots and tubers	Food stall / Raw food and fruits vendor	2.6%	44%	25%	11%
Pulses	Food stall / Raw food and fruits vendor	3.7%	44%	25%	11%
Vegetables	Food stall / Raw food and fruits vendor	8.6%	44%	25%	11%
Fruits	Food stall / Raw food and fruits vendor	2.9%	44%	25%	11%
Meat	Butcher	4.2%	58%	0%	0%
	Livestock / Animal (Products) / Poultry Sale	0.5%	20%	50%	10%
Fish	Fish Sale / Mongering	6.0%	41%	0%	0%
Dairy and eggs	Food stall / Raw food and fruits vendor	4.6%	44%	25%	11%
Other animal products	Livestock / Animal (Products) / Poultry Sale	0.5%	20%	50%	10%
Cooking fat	Small retail	3.7%	65%	75%	49%
Sugar products	Jaggery	2.6%	54%	0%	0%
	Small retail	2.6%	65%	75%	49%
Jam, honey, sweets, candies	Small retail	0.2%	65%	75%	49%
Tea, coffee	Small retail	1.5%	65%	75%	49%
Salt, pepper, condiments, etc.	Small retail	0.7%	65%	75%	49%
Food eaten outside the house	Food stand / Prepared food vendor	0.8%	56%	25%	14%
	Restaurant	0.6%	48%	50%	24%
Alcohol, tobacco	Bar	0.2%	41%	100%	41%
	Homemade alcohol / liquor	1.0%	52%	0%	0%
	Small retail	0.5%	65%	75%	49%
Other foods	Small retail	0.3%	65%	75%	49%
Clothing and shoes	Clothes / Mtumba / Boutique	1.0%	37%	100%	37%
	Tailor	1.8%	18%	100%	18%
Personal items	Barber shop	0.8%	0%	100%	0%
	Beauty shop / Salon	0.2%	12%	100%	12%
	Photo studio	0.0%	0%	100%	0%
	Small retail	1.0%	65%	75%	49%
Household items	Small retail	2.3%	65%	75%	49%
Transport, travel	Guesthouse/ Hotel	0.5%	18%	75%	14%
	Petrol station	2.3%	86%	100%	86%
	Piki driver	1.9%	26%	100%	26%
Airtime and phone expenses	M-Pesa	2.7%	54%	100%	54%
Internet	Cyber café	0.1%	18%	100%	18%
Firewood, charcoal, kerosene	Charcoal sale / burning	1.6%	16%	0%	0%
	Kerosene	0.1%	36%	100%	36%
	Timber / Firewood	0.1%	45%	50%	22%
Electricity	Local	0.3%		0%	0%
Water	Local	0.3%		0%	0%
Recreation	Bookshop	0.0%	21%	100%	21%
	Small retail	0.1%	65%	75%	49%
	Video Room/Football hall	0.0%	57%	100%	57%
Lottery tickets and gambling	Small retail	0.1%	65%	75%	49%
Religious expenses	Local	0.6%		0%	0%
Weddings, funerals	Local	1.0%		0%	0%
Charitable expenses	Local	0.1%		0%	0%
House rent / mortgage	Local	0.5%		0%	0%
School expenses	Local	10.7%		0%	0%
Medical expenses	Chemist	2.3%	27%	100%	27%
Other expenses	Local	4.2%		0%	0%
Total	**100.0%**				**18%**

Notes: Each row corresponds to an item in the expenditure module of our household surveys. We match each expenditure item to the enterprise type (from our enterprise census) at which it was most likely purchased (using revenue shares where possible to distribute expenditure where a good may be purchased in multiple enterprise types). Column 1 contains expenditure shares (sample-weighted across all households). Column 2 reports the upper bound of the share of intermediates in value added for each enterprise type, calculated from our enterprise surveys as $1 - \frac{1}{N}\sum_i w_i \frac{cost_i + profit_i}{revenue_i}$ (where we first winsorize at the 1% and 99% cut-offs, then average across enterprises of each type using revenue weights w_i, and cap at 0 and 1). Column 3 contains our upper-bound assumptions about the share of intermediate goods for each enterprise type that is imported, and Column 4 displays the implied import share for this item (multiplying Columns 2 and 3). The last row is expenditure-weighted across all categories.

Table H.2: Durable assets: Intermediate input and import shares

Item	Bought at enterprise type	(1) Asset share (data)	(2) Intermediate input share (data)	(3) Intermediate import share (assumed)	(4) Overall import share
Bicycle	Import	0.5%		100%	100%
	Bicycle repair / mechanic shop	0.5%	0%	100%	0%
Motorcycle	Motorcycle Repair / Shop	0.5%	45%	100%	45%
	Import	1.9%		100%	100%
Car	Import	2.5%		100%	100%
Boat	Import	0.0%		100%	100%
Bed	Carpenter	2.0%	10%	75%	7%
Chair	Carpenter	1.1%	10%	75%	7%
Table	Carpenter	1.3%	10%	75%	7%
Cupboard	Carpenter	1.5%	10%	75%	7%
Sofa	Carpenter	4.2%	10%	75%	7%
Mattress	Import	1.8%		100%	100%
Bednet	Hardware store	0.1%	41%	100%	41%
Solar energy system	Electric accesory/repair	0.3%	6%	100%	6%
	Import	1.0%		100%	100%
Generator	Hardware store	0.1%	41%	100%	41%
Car battery	Hardware store	0.2%	41%	100%	41%
Kerosene	Kerosene	0.1%	36%	100%	36%
Lantern	Hardware store	0.2%	41%	100%	41%
Clock	Electric accesory/repair	0.1%	6%	100%	6%
Radio	Electric accesory/repair	0.6%	6%	100%	6%
Sewing machine	Electric accesory/repair	0.4%	6%	100%	6%
Electric Iron	Electric accesory/repair	0.0%	6%	100%	6%
Mobile phone	Electric accesory/repair	0.7%	6%	100%	6%
	Import	0.7%		100%	100%
Television	Electric accesory/repair	0.7%	6%	100%	6%
Computer	Electric accesory/repair	0.0%	6%	100%	6%
	Import	0.0%		100%	100%
Cattle	Livestock / Animal (Products) / Poultry Sale	11.4%	20%	50%	10%
Pig	Livestock / Animal (Products) / Poultry Sale	0.3%	20%	50%	10%
Sheep	Livestock / Animal (Products) / Poultry Sale	0.6%	20%	50%	10%
Goat	Livestock / Animal (Products) / Poultry Sale	0.6%	20%	50%	10%
Chicken	Livestock / Animal (Products) / Poultry Sale	1.4%	20%	50%	10%
Other birds	Livestock / Animal (Products) / Poultry Sale	0.1%	20%	50%	10%
Farm tools	Hardware store	0.6%	41%	100%	41%
Ox plow	Hardware store	0.1%	41%	100%	41%
Wheel barrow	Hardware store	0.3%	41%	100%	41%
Hand cart	Hardware store	0.0%	41%	100%	41%
Iron sheets	Hardware store	0.4%	41%	100%	41%
House value (maintenance, improvement)	Carpenter	12.3%	10%	75%	7%
	Welding / metalwork	12.3%	0%	100%	0%
	Hardware store	18.4%	41%	100%	41%
	Local	18.4%		0%	0%
Total	**100.0%**				**20%**

Notes: Each row corresponds to an item in the asset module of our household surveys. We match each asset to the enterprise type (from our enterprise census) at which it was most likely purchased (using revenue shares where possible to distribute assets where a good may be purchased in multiple enterprise types). Column 1 contains asset shares (sample-weighted across all households). Column 2 reports the upper bound of the share of intermediates in value added for each enterprise type, calculated from our enterprise surveys as $1 - \frac{1}{N}\sum_i w_i \frac{cost_i + profit_i}{revenue_i}$ (where we first winsorize at the 1% and 99% cut-offs, then average across enterprises of each type using revenue weights w_i, and cap at 0 and 1). Column 3 contains our upper-bound assumptions about the share of intermediate goods for each enterprise type that is imported, and Column 4 displays the implied import share for this item (multiplying Columns 2 and 3). The last row is asset-share-weighted across all categories.

of how this impacts the multiplier estimate, we further assume that (i) all inventories are in the form of intermediate goods rather than final goods (leading us to err on the side of overstating their import share, at 62%), (ii) the import share of enterprise investment is the same as that of household assets (in our context, household and enterprise assets are often comparable or even shared), and (iii) imports scale linearly with expenditure. We then compute the share of the expenditure-based multiplier that is spent locally (see Table H.3). Even under the set of conservative assumptions discussed above, the transfer multiplier for local expenditure remains similar at 2.01.

Table H.3: Transfer Multiplier Estimates: Adjusting for Imported Intermediates

	(1) $\mathbb{M}$ Estimate	(2) Share imported	(3) Import adjusted
Panel A: Expenditure multiplier	2.53	0.20	2.01
Household non-durable expenditure	1.17	0.18	0.96
Household durable expenditure	0.81	0.20	0.65
Enterprise investment	0.48	0.20	0.38
Enterprise inventory	0.07	0.62	0.03

Notes: Results from the joint estimation of the expenditure multiplier (as in Table 5). Column 1 reports our main point estimates of the expenditure multiplier components. Each component is estimated individually and the multiplier is obtained by aggregating components as described in the main text. Column 2 presents our upper-range estimates of the share of imports captures for each expenditure component, and Column 3 presents the import-share adjusted estimate of the multiplier on local expenditure only.

H.2 Alternative assumptions for initial expenditure responses

Figure A.1 illustrates the timing of endline household surveys and enterprise surveys, and the substantial time lag between when the first transfers were scheduled in each village and survey administration (with time lags of 9 and 18 months, respectively). This limitation implies that treatment effects on expenditures in the three quarters post-transfer are quite imprecisely estimated, as discussed in detail in Appendix Section G.

We conduct two additional robustness tests to partially address these concerns. First, from the perspective of minimizing mean squared error, it may be preferable to exclude the noisily estimated initial quarters for all components of each multiplier that rely on flow values. This almost certainly leads to a substantial downward bias, since all early spending, profits and investment are excluded, but may improve statistical precision. The results of this exercise are presented in Panel A of Table H.4. As expected, the estimated expenditure and income multipliers are both lower compared to the main specifications, with the average of both multipliers falling to 1.75. The standard standard error on this estimate also declines substantially, by more than half. When testing both multipliers jointly, we reject a multiplier smaller than one with a p-value of 0.04.

Second, we utilize data from a closely related project in a neighboring county Rarieda that collected more detailed data on recipient household spending in the months immediately after they received similar GiveDirectly transfers a few years prior to this experiment (Haushofer and Shapiro 2016). While this project did not collect data on ineligible households, its data complements our data precisely where we think the timing of surveys and transfers imposes the most significant limitation for us, namely for estimating the direct impacts of transfers on *recipients* in the initial period post-transfer. In this exercise, we replace the noisily estimated consumption impacts among recipient households in the first 3 quarters post-transfer with estimates from the Rarieda data. Specifically, we estimate the same equation 7 as we do for our data, but exclude across-village spillover terms (see Appendix G for more details). For all other components, and for responses among non-recipients, the inputs into the multiplier estimate are unchanged.

Panel B of Table H.4 shows that augmenting the spending impact estimates with the data from Haushofer and Shapiro (2016) leads to a larger expenditure multiplier estimate of 3.09 (that is also slightly more precisely estimated than our main estimate). When testing both multipliers jointly, we reject a multiplier smaller than one with a p-value of 0.04. In Table H.5 we take this augmented estimate of the expenditure multiplier, and additionally adjust for imported intermediates using the same methodology as in H.1. Combining these adjustments, the expenditure multiplier is estimated to be 2.48.

H.3 The nominal transfer multiplier

The main multiplier estimate is based on real GDP, in which transfer amounts and all outcome measures are deflated to January 2015 US Dollars using the overall consumer price index in the geographically closest market to each household or enterprise (see Section 3.6 for a description of the price data). Table H.6 presents the same exercise in nominal terms. Since we estimate small treatment effects on prices, the difference between the real and nominal measures is mainly driven by overall inflation in the study area. As shown in Figure E.1, prices in the study area rose by about 10% per year on average. Roughly in line with this, the nominal multiplier over the first two years after transfers went out is roughly 5% larger than the real multiplier (2.66 versus 2.53) on the expenditure side, and approximately 12% larger (2.55 versus 2.28) on the income side.

Table H.4: Transfer Multiplier: Alternative Assumptions for the Initial Spending Impact

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main estimate	**Alternative Specification I: Setting initial 3 quarters = 0**			**Alternative Specification II Initial 3 quarters from Haushofer & Shapiro (2016)**		
		$\mathbb{M}$ Estimate	H_0: $\mathbb{M} < 0$ *p*-value	H_0: $\mathbb{M} < 1$ *p*-value	$\mathbb{M}$ Estimate	H_0: $\mathbb{M} < 0$ *p*-value	H_0: $\mathbb{M} < 1$ *p*-value
Panel A: Expenditure multiplier	2.53 (1.42)	2.04 (0.67)	0.00***	0.06*	3.09 (1.38)	0.01**	0.06*
Household non-durable expenditure	1.17 (1.32)	0.99 (0.63)	0.06*		1.73 (1.25)	0.08*	
Household durable expenditure	0.81 (0.05)	0.81 (0.05)	0.00***		0.81 (0.05)	0.00***	
Enterprise investment	0.48 (0.42)	0.17 (0.11)	0.06*		0.48 (0.44)	0.15	
Enterprise inventory	0.07 (0.03)	0.07 (0.03)	0.02**		0.07 (0.03)	0.02**	
Panel B: Income multiplier	2.28 (1.73)	1.45 (0.65)	0.01***	0.23	2.28 (1.76)	0.12	0.24
Enterprise profits	1.47 (1.28)	0.00 (0.35)	0.48		1.47 (1.28)	0.14	
Household wage bill	0.68 (1.15)	1.34 (0.54)	0.01***		0.68 (1.15)	0.28	
Enterprise capital income	0.09 (0.17)	0.10 (0.06)	0.05*		0.09 (0.17)	0.32	
Enterprise taxes paid	0.04 (0.03)	0.01 (0.01)	0.03**		0.04 (0.03)	0.08*	
Panel C: Expenditure and income multipliers							
Average of both multipliers	2.40 (1.38)	1.75 (0.58)	0.00***	0.09*	2.69 (1.39)	0.03**	0.12
Joint test of both multipliers			0.00***	0.04**		0.01***	0.04**

Notes: Results from the joint estimation of expenditure and income multipliers. Column 1 reports our main point estimates of both multipliers and their respective components from Table 5. Columns 2 - 4 repeat this exercise, imposing that the impact of each dynamically estimated flow component is zero in the first 3 quarters after the transfer. Columns 5 - 7 estimate the initial 3 quarters of the impact on non-durable consumption expenditure for recipients using data from a related project that collected more detailed data for recipient expenditure in the initial months after the transfer (Haushofer and Shapiro 2016). All other components remain the same as in our main specification. Transfer amounts and outcome variables are deflated to January 2015 using the overall consumer price index in the geographically closest market. Standard errors are computed by 2,000 replications of a clustered wild clustered bootstrap. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table H.5: Transfer Multiplier Estimates: Adding initial Quarters from Haushofer and Shapiro (2016) and Adjusting for Imported Intermediates

	(1) $\mathbb{M}$ Estimate	(2) Share imported	(3) Import adjusted
Panel A: Expenditure multiplier	3.09	0.20	2.48
Household non-durable expenditure	1.73	0.18	1.42
Household durable expenditure	0.81	0.20	0.65
Enterprise investment	0.48	0.20	0.38
Enterprise inventory	0.07	0.59	0.03

Notes: Results from the joint estimation of the expenditure multiplier, using data from Haushofer and Shapiro (2016) for the expenditure response of recipients in the first 3 quarters (as in Table H.4). Column 1 reports our main point estimates of the expenditure multiplier components. Each component is estimated individually and the multiplier is obtained by aggregating components as described in the main text. Column 2 presents our upper-range estimates of the share of imports captures for each expenditure component, and Column 3 presents the import-share adjusted estimate of the multiplier on local expenditure only.

Table H.6: Nominal Transfer Multiplier

	(1) $\mathbb{M}$ Estimate	(2) H_0: $\mathbb{M} < 0$ p-value	(3) H_0: $\mathbb{M} < 1$ p-value
Panel A: Expenditure multiplier	2.66	0.04**	0.12
	(1.48)		
Household non-durable expenditure	1.22	0.18	
	(1.37)		
Household durable expenditure	0.89	0.00***	
	(0.06)		
Enterprise investment	0.47	0.15	
	(0.43)		
Enterprise inventory	0.08	0.02**	
	(0.04)		
Panel B: Income multiplier	2.55	0.08*	0.19
	(1.80)		
Enterprise profits	1.47	0.13	
	(1.30)		
Household wage bill	0.94	0.22	
	(1.17)		
Enterprise capital income	0.10	0.29	
	(0.18)		
Enterprise taxes paid	0.04	0.07*	
	(0.03)		
Panel C: Expenditure and income multipliers			
Average of both multipliers	2.60	0.04**	0.12
	(1.44)		
Joint test of both multipliers		0.01**	0.06*

Notes: This table is analogous to Table 5 (see table notes for detail). The only difference is that here, monetary values are nominal, whereas in Table 5 transfer amounts and outcome variables are deflated to January 2015 using the overall consumer price index in the geographically closest market. Standard errors and test statistics are computed from 2,000 replications of a wild clustered bootstrap. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

I Study pre-analysis plans

We filed a series of pre-analysis plans as part of this study. These include: i) Haushofer et al. (2017a), and a follow-up amendment outlining spillover analyses, both of which focus on household outcomes; ii) Haushofer et al. (2016), which covered midline market price and enterprise data; and iii) Haushofer et al. (2018), which focused on macroeconomic quantities of interest. All pre-analysis plans can be accessed on the AEA trial registry: https://www.socialscienceregistry.org/trials/505. In this paper, we focus on primary outcomes for households, enterprises and prices, collected as part of our baseline and endline household and enterprise censuses and surveys, as well as our midline market price surveys.

Less relevant to this paper are: i) Walker (2017), which forms the basis of Walker (2018) on local taxes and public goods; and ii) Haushofer et al. (2017b), which conducts a separate exercise to study potential transfer targeting.

In the interest of space, we do not present an exhaustive list of every outcome component and analysis mentioned across these pre-analysis plans. A supplemental appendix containing the full set of pre-specified outcomes for these plans is available online at https://osf.io/r5q6v/.

Table I.1 presents the 10 primary household outcomes that we pre-specified as part of a single table, including FDR q-values accounting for multiple testing across these ten outcomes. In addition to the specifications reported in the main tables, we also report the pooled saturation effect, the average effect of being in a high saturation sublocation across all eligibility and village types. As outlined in Section 3, we prefer our spatial estimates as they take advantage of the full variation in treatment intensity in our data, but present these saturation results for completeness.

To calculate the pooled saturation effect, we use coefficient estimates from the following equation:

$$\begin{aligned} y_{hvs,t=1} = {} & \beta_0 + \beta_1 T_{vs} + \beta_2 E_{hvs} + \beta_3 H_s + \beta_4 T_{vs} \times E_{hvs} + \beta_5 T_{vs} \times H_s \\ & + \beta_6 E_{hvs} \times H_s + \beta_7 T_{vs} \times E_{hvs} \times H_s + \delta_1 y_{hvs,t=0} + \delta_2 M_{hvs} + \varepsilon_{ihvs}. \end{aligned} \tag{14}$$

Here, h indexes the household, v indexes the village, s indexes the sublocation, and t indicates whether the variable was measured at baseline or endline. T_{vs} is an indicator for households residing in a treated village, E_{hvs} is an indicator for whether the household is eligible for transfers, and H_s is an indicator for living in a high-saturation sublocation; $\times$ denote interaction terms. Standard errors are clustered at the saturation group level. The pooled saturation effect is then a weighted average of $\beta_3, \beta_5, \beta_6$ and β_7 using population weights of all households across high-saturation sublocations.

We make two additional notes. First, in Haushofer et al. (2016), we were not clear whether we would focus on a balanced panel of market survey data or an unbalanced panel. For simplicity, we present results using a unbalanced panel, but results are robust to using a balanced panel. Second, our reduced form equations cluster standard errors at the village level, as pre-specified in Haushofer et al. (2017a), but results are also robust to clustering at the sublocation level. (Both sets of results are available upon request).

Table I.1: Pre-specified primary outcomes, household welfare plan

	(1)	(2)	(3)	(4)	(5)
	Recipient Households		**Non-Recipient Households**		
	𝟙(Treat village) Reduced form	Total Effect IV	Total Effect IV	Pooled saturation effect	Control, low saturation mean (SD)
Assets (non-land, non-house), net borrowing	178.09***	182.01***	132.63*	41.96	1,132.15
	(24.61)	(44.25)	(78.32)	(42.33)	(1,420.22)
	[0.00]***	[0.00]***	[0.22]	[0.48]	
Household expenditure, annualized	292.98***	338.16***	333.73***	138.26*	2,536.86
	(60.09)	(109.36)	(123.24)	(71.29)	(1,934.09)
	[0.00]***	[0.01]***	[0.05]*	[0.27]	
Household income, annualized	77.62*	134.02	229.46***	111.85*	1,023.45
	(43.66)	(93.83)	(88.59)	(59.47)	(1,634.70)
	[0.07]*	[0.16]	[0.05]*	[0.27]	
Household revenue, annualized	73.62	175.46*	54.32	117.04**	933.27
	(51.59)	(90.98)	(109.21)	(57.86)	(1,698.65)
	[0.12]	[0.13]	[0.38]	[0.27]	
Psychological well-being index	0.09***	0.12*	0.08	0.04	0.01
	(0.03)	(0.07)	(0.06)	(0.03)	(1.01)
	[0.01]**	[0.13]	[0.27]	[0.27]	
Health index	0.04	0.06	0.01	−0.01	0.03
	(0.03)	(0.06)	(0.05)	(0.03)	(1.01)
	[0.14]	[0.26]	[0.41]	[1.00]	
Education index	0.09**	0.09*	0.10*	0.03	0.01
	(0.04)	(0.05)	(0.06)	(0.03)	(1.02)
	[0.02]**	[0.13]	[0.22]	[0.50]	
Female empowerment index	−0.01	0.08	0.09	0.02	0.05
	(0.07)	(0.14)	(0.15)	(0.08)	(0.94)
	[0.35]	[0.34]	[0.37]	[1.00]	
Food security index	0.10***	0.05	0.08	0.01	0.01
	(0.03)	(0.07)	(0.06)	(0.03)	(1.00)
	[0.01]***	[0.34]	[0.25]	[1.00]	
Hours worked last week (respondent)	1.27	−1.87	−1.79	1.05	34.05
	(1.02)	(1.85)	(1.41)	(0.95)	(27.11)
	[0.14]	[0.26]	[0.29]	[0.48]	

Notes: Each row represents regressions of a pre-specified primary outcome on different regressors. Column 1 reports the coefficient on an indicator for treatment village from a regression using data from eligible households (as classified by the GE census team), and includes an indicator for saturation status of the sublocation (Equation 1). Column 2 reports the total effect on treated households (eligible recipients) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a household's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the household (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer), as in Equation 2. For this analysis, the sample is restricted to eligible households. We have 5,168 to 5,423 observations (1,118 for the female empowerment index) for these columns. Column 3 presents the average spillover effect on eligible households in control villages as well as ineligible households, coming from a stacked spatial regression of each outcome on the amount transferred per capita GDP to each 2km radii band around each household (instrumented by the share of eligibles assigned to treatment in each buffer), as in Equation 3. The reported average effect comes from a population-share-weighted average effect experienced by those two groups, and is representative of the average untreated household. We have 5,230 to 5,509 (978 for female empowerment) for column 3.. The number of radii bands included in columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the pooled saturation effect, the average saturation effect experienced by households in high-saturation sublocations, derived as a weighted average of $\beta_3, \beta_5, \beta_6$ and β_7 in Equation 14. We have to 7,832 to 8,239 (1,535 for female empowerment). Column 5 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across eligible and ineligible households). Each regression is weighted by inverse sampling weights and contains baseline values of the outcome when available. Standard errors are clustered at the village in column 1, at the sublocation level in column 4, and calculated following Conley (2008) using a uniform kernel out to 10 km in columns 2 and 3. Minimum FDR q-values are reported in brackets. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

J Additional welfare analysis

We provide here an illustrative mapping between household welfare and aggregate output, emphasizing that any increase in aggregate output must reflect some combination of (i) an increase in the *employment* of factors of production, which comes at an opportunity cost, and (ii) an increase in their aggregate *productivity*, which does not. We also illustrate how household welfare differs from household expenditure.

Consider a household i whose market interactions at time t involve the (net) purchase of a vector of commodities c_{it} at prices p_t, the supply of a vector l_{it} of labor services at wages w_t, and the supply of (net) savings to support capital investment equal to the difference between current-period income and expenditure. In addition, the household receives profit π_{it} from owned enterprises.[54] The household's problem is

$$\max_{\{c_{it}, l_{it}\}} u(\{c_{it}, l_{it}\}, \{c_{-it}, l_{-it}\}) + \lambda_i \left(T_i + \sum_{t=0}^{\infty} \delta^t (\pi_{it} + w_t \cdot l_{it} - p_t \cdot c_{it}) \right) \tag{15}$$

where λ_i is the Lagrange multiplier on the budget constraint and $\delta \equiv 1/r$ the discount rate on future funds.[55] The economy's capital stock at the beginning of period t is $k_t = \sum_i k_{it} = \sum_i \sum_{\tau=-\infty}^{t-1} \delta^\tau (\pi_{i\tau} + w_t \cdot l_{i\tau} - p_t \cdot c_{i\tau})$. The household's contribution to (real) output in period t measured using the income approach is equal its claims on firm profits plus the factor payments it receives, or $\pi_{it} + w_t \cdot l_{it} + r \cdot k_{it}$.[56]

Overall output is the sum of these contributions which is simply total enterprise value added, and the period-t contribution to the transfer multiplier is the effect of \$1 of transfers on this quantity. Whether distributed to households in the form of higher profits, wages, or interest payments, real output gains can be achieved only through increases in (i) the supply of labor or capital, or (ii) of productivity. In the case of labor supply this comes at a utility cost, since $\frac{\partial u}{\partial l_{it}} < 0$, so that a dollar increase in output must be worth less than a dollar in equivalent variation terms. Similarly in the case of capital, an increase in the period-t capital stock implies a decrease in consumption in some other period(s), so that again a dollar increase in output is worth less than a dollar in equivalent variation terms. In the case of a pure productivity gain, on the other hand, a dollar of output is worth a full dollar to the household(s) that receive it.

To contrast expenditure with welfare, assume for simplicity that first-order conditions are necessary and sufficient for a solution to the household's problem defined by (15), and

54. The term π_{it} could also capture other (net) transfers e.g. from peer households and from the government. We ignore these terms here as the estimated treatment effects on them in our data are negligible.

55. One can generalize this formulation to allow for non-separable household production using non-marketed inputs such as family labor without changing the basic message. It is also straightforward to allow for discount rates to vary across agents, reflecting capital market imperfections.

56. Its contribution measured using the consumption approach is its expenditure $p_t \cdot c_{it}$ plus its attributable share of firm investment which (assuming a closed economy) must equal household savings, i.e., $p_t \cdot c_{it} + (\pi_{it} + w_t \cdot l_{it} + r \cdot k_{it} - p_t \cdot c_{it})$, which is evidently equivalent.

for simplicity we ignore externalities. The envelope theorem then implies

$$\frac{dv_i}{dT} = \lambda_i \sum_{t=0}^{\infty} \delta^t \left(\frac{t}{\delta} \frac{\partial \delta}{\partial T} s_{it} + \frac{\partial \pi_{it}}{\partial T} + \frac{\partial w_t}{\partial T} \cdot l_{it} - \frac{\partial p_t}{\partial T} \cdot c_{it} \right) \tag{16}$$

where $s_{it} = \pi_{it} + w_t \cdot l_{it} - p_t \cdot c_{it}$ is period t savings. In comparison, the welfare effect of a marginal change in T_i holding other transfers fixed is simply

$$\frac{\partial v_i}{\partial T_i} = \lambda_i \tag{17}$$

The (marginal) equivalent variation dEV_i/dT is the ratio of these expressions. To see how this relates to household expenditure, define (the present discounted value of) household expenditure as $e_i = \sum (1/(1+r))^t p_t \cdot c_{it}$. Differentiating the budget constraint, we have

$$\frac{de_i}{dT} = \sum_{t=0}^{\infty} \delta^t \left(\frac{t}{\delta} \frac{\partial \delta}{\partial T} (\pi_{it} + w_t \cdot l_{it}) + \frac{\partial \pi_{it}}{\partial T} + \frac{\partial w_t}{\partial T} \cdot l_{it} + w_t \cdot \frac{\partial l_{it}}{\partial T} \right) \tag{18}$$

Comparing the equations above, we see that

$$\frac{dEV_i}{dT} = \frac{de_i}{dT} - \sum_{t=0}^{\infty} \delta^t \left(\frac{t}{\delta} \frac{\partial \delta}{\partial T} (p_t \cdot c_{it}) + \frac{\partial p_t}{\partial T} \cdot c_{it} + w_t \cdot \frac{\partial l_{it}}{\partial T} \right) \tag{19}$$

This expression shows that changes in expenditure are closely related to changes in equivalent variation, but with several intuitive (and correctable) sources of bias. First, (nominal) expenditure incorrectly counts appreciation of the price of the household's planned time path of consumption, whether due to appreciation of intra-period prices ($\frac{\partial p_t}{dT} \cdot c_{it}$) or of the inter-period interest rate $\frac{t}{\delta} \frac{\partial \delta}{\partial T} (p_t \cdot c_{it})$, as a welfare gain. This is why constant-dollar expenditure measures are preferable. Second, it incorrectly counts income gains due to behavioral responses such as increased labor supply ($w_t \cdot \frac{\partial l_{it}}{\partial T}$) as a welfare gain. Finally, if (more realistically) we were to examine expenditure over any finite period of time this would introduce a third bias, as this metric would count as a welfare gain any increases in current expenditure that were driven by decreases in future expenditure (i.e., by dis-saving).

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