Appendix A is contained in the Main Appendix (in print with the article). Appendices B, C and D are in the Online Supplemental Appendix. All appendices A - K, including A - D, are also contained in a single file with the Online Replication Material.

Online Appendix to: “General equilibrium effects of cash transfers: experimental evidence from Kenya”

Dennis Egger Johannes Haushofer Edward Miguel
Paul Niehaus Michael Walker

June 7, 2022

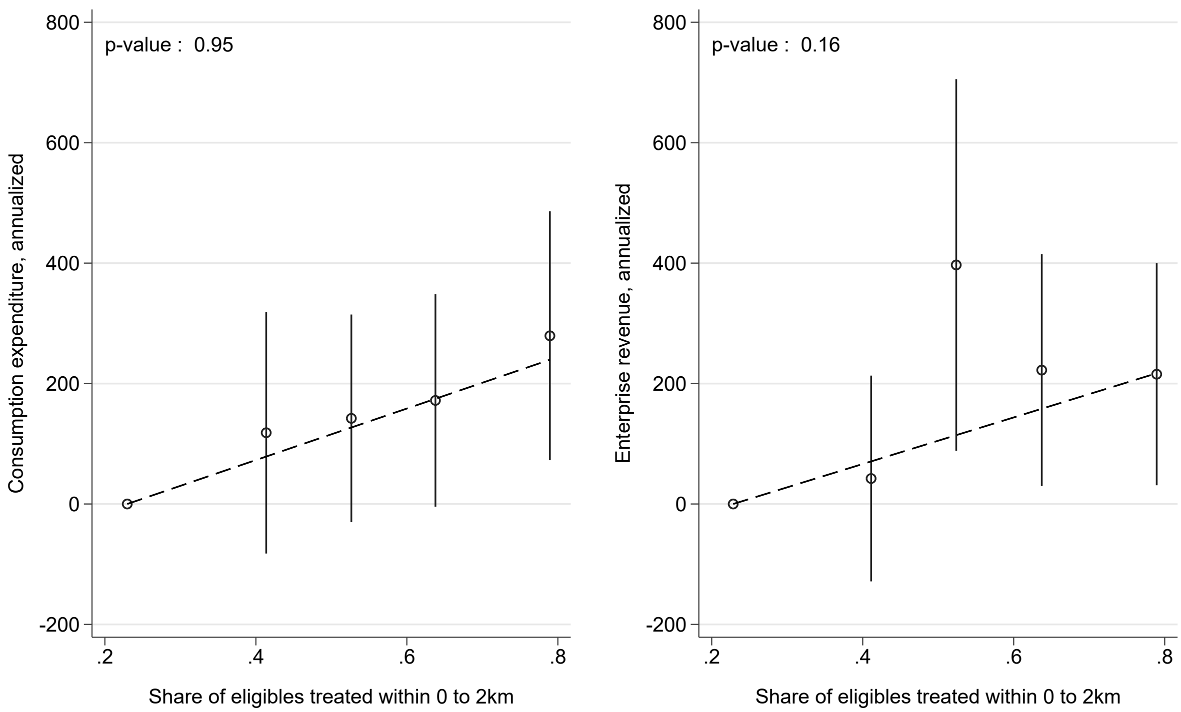
Egger: University of California, Berkeley; Haushofer: Princeton University, NBER, Busara Center for Behavioral Economics, and Max Planck Institute for Collective Goods; Miguel: University of California, Berkeley, NBER, and CEGA; Niehaus: University of California, San Diego, NBER, and CEGA; Walker: University of California, Berkeley, and CEGA. Niehaus is a co-founder, former president (2012 - 2017) and chairman of the board of GiveDirectly.

Online Appendix

Table of Contents

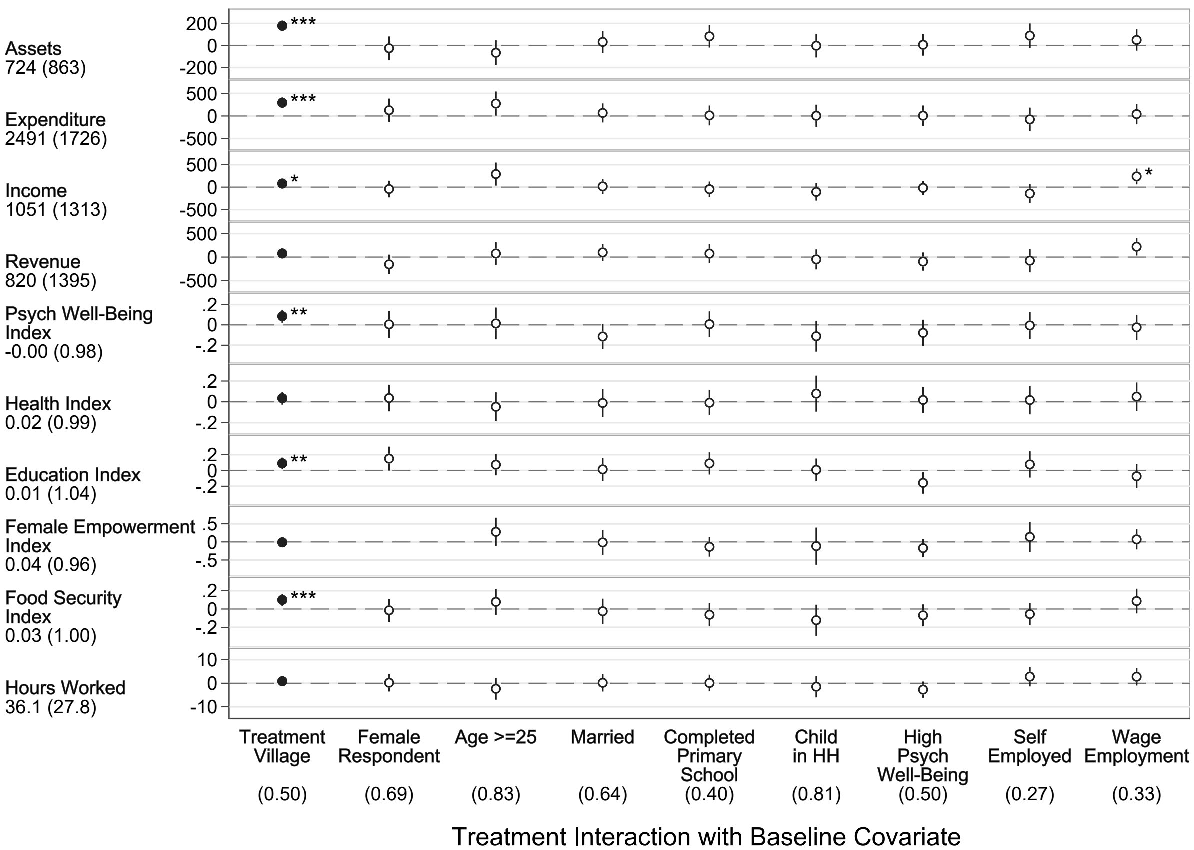
B Supporting figures & tables

Figure B.1: Non-linear Spillover Estimates



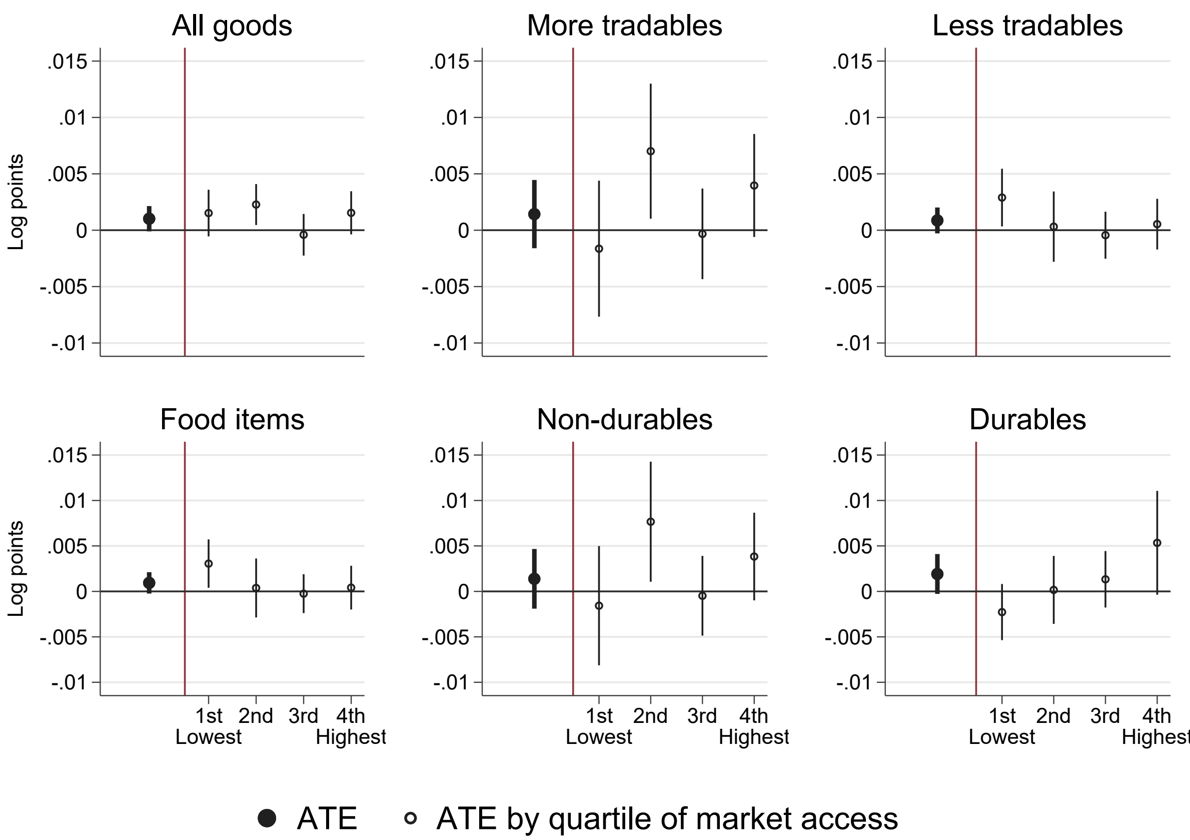
Notes: Each panel represents a reduced-form regression of household consumption and enterprise revenue on 5 quintile bins of the share of eligibles assigned to treatment 2km around each household / enterprise. Effects are relative to the lowest quantile. For consumption, we control for direct effects 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 B.2: Little heterogeneity in pre-specified primary outcomes



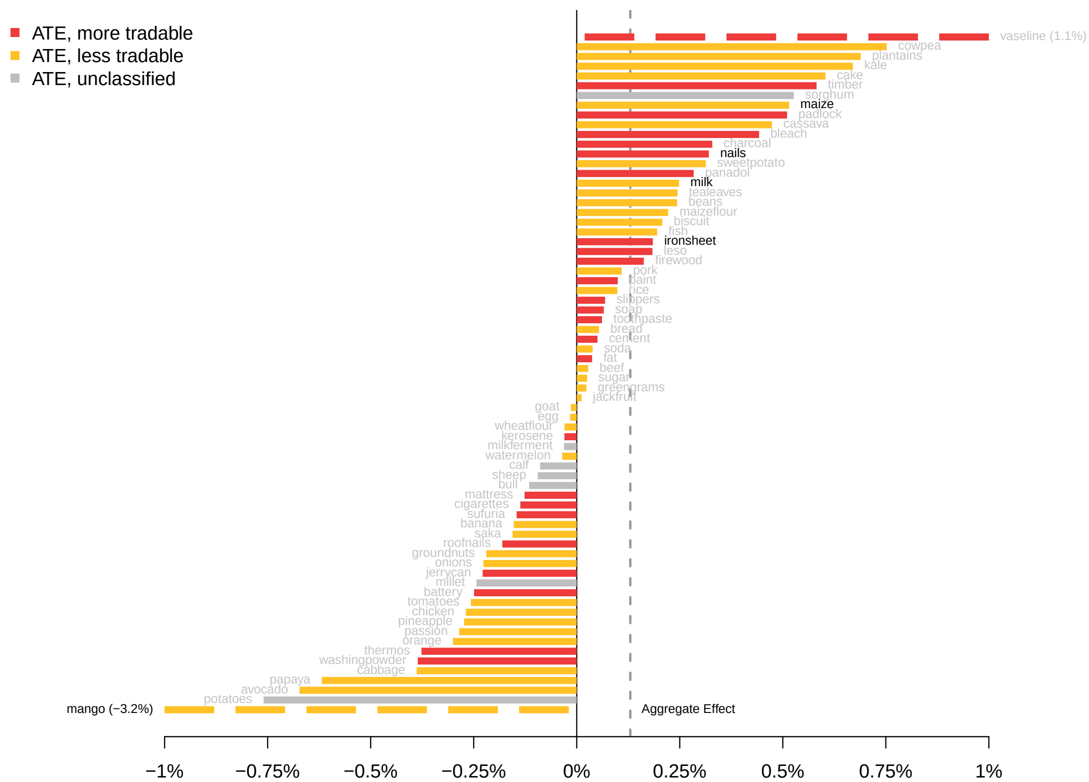
Notes: This figure presents estimates for treatment effect heterogeneity for eligible households in pre-specified primary outcomes along 8 pre-specified dimensions of heterogeneity (Haushofer et al. 2017). Each plotted coefficient is from a separate regression. Each row represents a separate primary 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 B.3: 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 B.4: 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 B.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)	174.49***	175.62***	151.53*	1,205.22
	(25.51)	(46.95)	(82.92)	(1,459.67)
Productive Agricultural Assets	4.26***	4.16**	−0.37	32.50
	(0.93)	(1.96)	(2.47)	(38.93)
Potentially Productive Assets	90.03***	52.80	36.46	700.16
	(25.85)	(49.31)	(65.84)	(1,025.10)
Livestock Assets	50.60***	44.81	−6.88	461.88
	(17.03)	(27.90)	(35.77)	(723.23)
Non-Ag Assets	37.10***	24.64	25.71	218.90
	(10.43)	(22.85)	(23.15)	(423.88)
Non-Productive Assets	79.00***	92.71***	52.49*	449.32
	(9.32)	(14.28)	(29.60)	(468.53)

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 B.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	65.46	160.21**	81.50*	235.98
	(41.84)	(68.09)	(43.38)	(414.95)
Manufacturing revenue	−49.59	92.74**	108.51	81.19
	(73.46)	(46.42)	(70.22)	(177.10)
Services revenue	−77.25*	7.20	43.37	115.09
	(40.75)	(46.57)	(31.35)	(175.76)
Agriculture revenue	3.11**	5.51***	2.15*	37.91
	(1.27)	(1.43)	(1.29)	(46.39)

Notes: Column 1 reports the coefficient on an indicator for treatment village, and includes an indicator for saturation status of the sublocation (Equation 8). 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 9). 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 B.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	6.78	18.51*	43.55***	156.79
	(7.39)	(11.08)	(14.44)	(292.84)
Enterprise revenue, annualized	51.79**	100.98	171.83***	494.45
	(22.82)	(86.46)	(42.78)	(1,223.07)
Enterprise costs, annualized	24.04**	28.11	37.27**	117.22
	(9.41)	(17.39)	(17.18)	(263.46)
Enterprise wagebill, annualized	21.13**	27.71	36.93**	97.35
	(8.69)	(17.48)	(17.10)	(237.01)
Enterprise profit margin	−0.05**	−0.05	−0.01	0.33
	(0.02)	(0.05)	(0.04)	(0.30)
Panel B: Non-agricultural enterprises				
Enterprise inventory	2.88	7.74	5.58	50.41
	(2.79)	(7.47)	(3.91)	(131.86)
Enterprise investment, annualized	−5.15	−15.61	5.49	46.57
	(5.34)	(15.75)	(8.36)	(167.44)

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 8). 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 9. 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 B.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)
Household hours worked on own farm	2.07*	0.97	−6.26**	35.32
	(1.15)	(2.30)	(2.61)	(38.79)
Individual hours worked in self-employment	1.80	4.23**	−1.38	26.82
	(1.14)	(1.96)	(1.76)	(23.53)
Individual hours employed last week	0.52	−1.37	2.51	23.60
	(0.98)	(2.32)	(2.67)	(25.95)
Individual hours employed last week in agriculture	−1.53***	−2.28***	0.33	6.00
	(0.56)	(0.75)	(1.11)	(12.78)
Individual hours employed last week not in agriculture	1.67	0.62	1.93	17.08
	(1.03)	(2.31)	(2.65)	(26.40)
Hourly wage earned by employees	0.10***	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 B.5: Input prices and quantities: additional land outcomes

	(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)
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)
Land price per acre	168.02	366.46	557.44	3,952.48
	(201.18)	(290.85)	(412.34)	(3,147.29)
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	6.97***	8.99*	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 B.6: Non-market Outcomes and Externalities

	(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)
Psychological well-being index	0.09***	0.12*	0.08	0.01
	(0.03)	(0.07)	(0.06)	(1.01)
Health index	0.03	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 E.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 B.7: Inequality

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	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	32.3
	(0.7)	(1.3)	(1.1)	(7.8)
Counterfactual Gini coefficient	−1.1*	−2.1	0	32.3
	(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	2.2	2.8**	45.4
	(0.9)	(1.6)	(1.4)	(10.1)
Counterfactual Gini coefficient	−7.6***	−6.7***	0	45.8
	(0.8)	(0.5)		(10.7)
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 C 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 B.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	293.59***	338.57***	334.77***	21.03	411.55***	2,536.01
	(60.11)	(109.38)	(123.20)	(83.76)	(147.81)	(1,933.51)
Non-durable expenditure, annualized	187.65***	227.20**	317.62***	24.68	389.31***	2,470.69
	(58.59)	(99.63)	(119.76)	(79.05)	(144.86)	(1,877.23)
Food expenditure, annualized	72.04*	133.84**	133.30**	10.59	163.33**	1,578.05
	(36.96)	(63.99)	(58.56)	(50.09)	(71.26)	(1,072.00)
Temptation goods expenditure, annualized	6.55	5.91	−0.68	10.65	−3.46	37.07
	(5.79)	(8.82)	(6.50)	(8.02)	(7.80)	(123.54)
Durable expenditure, annualized	95.09***	109.01***	8.44	5.69	9.12	59.41
	(12.64)	(20.24)	(12.50)	(16.83)	(15.00)	(230.83)
Panel B: Assets						
Assets (non-land, non-house), net borrowing	178.78***	183.38***	133.06*	−12.25	168.63*	1,131.66
	(24.66)	(44.26)	(78.33)	(39.93)	(98.04)	(1,419.70)
Housing value	376.92***	477.29***	80.65	26.90	93.80	2,032.11
	(26.37)	(38.80)	(215.81)	(37.33)	(268.31)	(5,028.27)
Land value	51.28	158.47	544.85	192.35	631.12	5,030.03
	(186.22)	(260.91)	(459.57)	(291.51)	(545.93)	(6,604.66)
Panel C: Household balance sheet						
Household income, annualized	79.43*	135.70	224.96***	83.37	259.61**	1,023.36
	(43.80)	(92.10)	(85.98)	(58.32)	(105.27)	(1,634.02)
Net value of household transfers received, annualized	−1.68	−7.43	8.85	−6.84	12.69	130.08
	(6.81)	(13.06)	(19.11)	(10.27)	(23.18)	(263.65)
Tax paid, annualized	1.94	−0.09	1.68	−0.92	2.31	16.92
	(1.28)	(2.02)	(2.02)	(1.65)	(2.39)	(36.50)
Profits (ag & non-ag), annualized	26.24	35.85	36.37	−1.74	45.70	485.56
	(23.67)	(47.66)	(44.88)	(36.54)	(55.63)	(786.92)
Wage earnings, annualized	42.43	73.66	182.63***	90.01**	205.30**	494.95
	(32.23)	(60.82)	(65.53)	(39.13)	(80.22)	(1,231.12)

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

	(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 1: Full Sample				
Respondent migrated	0.01	−0.03	0.00	0.05
	(0.01)	(0.02)	(0.01)	(0.22)
Net change in household members since baseline	0.03	0.02	−0.18**	−0.10
	(0.04)	(0.08)	(0.08)	(1.30)
Household size	0.02	0.03	−0.10	4.05
	(0.05)	(0.09)	(0.08)	(2.35)
Panel 2: Non-Migrant Sample				
Panel 2.A: Expenditure				
Hou	312.43***	376.89***	325.86***	2,511.75
	(61.11)	(113.62)	(120.61)	(1,926.63)
Non-durable expenditure, annualized	200.77***	261.03**	307.48***	2,445.60
	(59.11)	(101.60)	(117.66)	(1,868.88)
Food expenditure, annualized	81.11**	152.28**	124.06**	1,572.87
	(38.00)	(67.15)	(60.61)	(1,069.55)
Temptation goods expenditure, annualized	4.50	1.91	−0.61	37.91
	(6.15)	(9.51)	(6.83)	(125.53)
Durable expenditure, annualized	102.07***	113.36***	8.56	60.03
	(13.19)	(20.84)	(12.63)	(231.69)
Panel 2.B: Assets				
Assets (non-land, non-house), net borrowing	175.10***	173.60***	136.72	1,145.54
	(25.28)	(50.55)	(84.10)	(1,414.55)
Housing value	403.73***	473.12***	44.72	2,096.91
	(27.55)	(39.64)	(216.19)	(5,132.21)
Land value	51.69	87.92	525.14	5,141.36
	(193.43)	(279.70)	(464.96)	(6,685.90)
Panel 2.C: Household balance sheet				
Household income, annualized	39.63	84.96	197.25**	992.84
	(43.31)	(95.51)	(87.03)	(1,600.14)
Net value of household transfers received, annualized	0.69	−10.87	10.58	135.84
	(7.03)	(14.04)	(20.71)	(266.48)
Tax paid, annualized	1.58	−0.95	1.42	16.65
	(1.32)	(2.28)	(2.21)	(35.72)
Profits (ag & non-ag), annualized	13.45	−3.07	14.64	488.97
	(23.56)	(52.34)	(41.43)	(786.27)
Wage earnings, annualized	16.03	67.17	175.24***	460.98
	(31.57)	(59.78)	(67.66)	(1,185.04)

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.

C 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 11% 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

D.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 C.1: Estimates of recipients' marginal propensity to consume

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Transfer					**Transfer + Income Gains**	
	MPC non-durables q1-q3	q4-q10	**MPC durables**	**MPC total**	**MPC local**	**MPC total**	**MPC local**
Our data only	-0.21	0.29	0.30	0.38	0.30	0.34	0.27
	(0.22)	(0.12)	(0.05)	(0.21)	(0.17)	(0.17)	(0.14)
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)	(0.15)	(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 D.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 C.1 presents MPC estimates using data only from this study. We estimate a marginal propensity to spend on non-durables of 0.08 over the first 10 quarters after the initial transfer, and 0.30 on on durables. Combined, this yields a marginal propensity to spend of 0.38. We are thus able to directly account for 38% 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 C.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[1]. 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 $p = 0.26$. 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

1. 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

D 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).

D.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 D.1 and D.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 D.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 D.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	Bicycle repair / mechanic shop	0.5%	0%	100%	0%
	Import	0.5%		100%	100%
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	Import	0.7%		100%	100%
	Electric accesory/repair	0.7%	6%	100%	6%
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)	Welding / metalwork	12.3%	0%	100%	0%
	Carpenter	12.3%	10%	75%	7%
	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 D.3). Even under the set of conservative assumptions discussed above, the transfer multiplier for local expenditure remains similar at 2.01.

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

	(1) $\mathbb{M}$ Estimate	(2) Share imported	(3) Import adjusted
Panel A: Expenditure multiplier	2.58	0.20	2.05
Household non-durable expenditure	1.20	0.18	0.98
Household durable expenditure	0.84	0.20	0.67
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.

D.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 C.

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 D.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 0.03.

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 C 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 D.4 shows that augmenting the spending impact estimates with the data from Haushofer and Shapiro (2016) leads to a larger expenditure multiplier estimate of 3.14 (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 D.5 we take this augmented estimate of the expenditure multiplier, and additionally adjust for imported intermediates using the same methodology as in D.1. Combining these adjustments, the expenditure multiplier is estimated to be 2.52.

D.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 D.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 G.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 3% larger than the real multiplier (2.66 versus 2.58) on the expenditure side, and approximately 10% larger (2.71 versus 2.47) on the income side.

Table D.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	2.58	2.07	0.00***	0.05*	3.14	0.01***	0.06*
	(1.44)	(0.67)			(1.41)		
Household non-durable expenditure	1.20	1.00	0.06*		1.76	0.08*	
	(1.31)	(0.64)			(1.28)		
Household durable expenditure	0.84	0.84	0.00***		0.84	0.00***	
	(0.05)	(0.05)			(0.05)		
Enterprise investment	0.48	0.17	0.08*		0.48	0.14	
	(0.43)	(0.11)			(0.42)		
Enterprise inventory	0.07	0.07	0.02**		0.07	0.02**	
	(0.03)	(0.03)			(0.03)		
Panel B: Income multiplier	2.47	1.44	0.01***	0.25	2.47	0.08*	0.20
	(1.71)	(0.61)			(1.80)		
Enterprise profits	1.68	0.01	0.49		1.68	0.10	
	(1.27)	(0.32)			(1.32)		
Household wage bill	0.69	1.34	0.00***		0.69	0.28	
	(1.09)	(0.51)			(1.12)		
Enterprise capital income	0.06	0.08	0.09*		0.06	0.36	
	(0.17)	(0.06)			(0.18)		
Enterprise taxes paid	0.04	0.01	0.04**		0.04	0.09*	
	(0.03)	(0.01)			(0.03)		
Panel C: Expenditure and income multipliers							
Average of both multipliers	2.52	1.75	0.00***	0.09*	2.80	0.02**	0.10*
	(1.39)	(0.56)			(1.43)		
Joint test of both multipliers			0.00***	0.03**		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 D.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	3.14	0.20	2.52
Household non-durable expenditure	1.76	0.18	1.44
Household durable expenditure	0.84	0.20	0.67
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 D.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 D.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	0.03**	0.13
	(1.49)		
Household non-durable expenditure	1.22	0.18	
	(1.35)		
Household durable expenditure	0.90	0.00***	
	(0.06)		
Enterprise investment	0.47	0.16	
	(0.44)		
Enterprise inventory	0.08	0.02**	
	(0.04)		
Panel B: Income multiplier	2.71	0.06*	0.17
	(1.75)		
Enterprise profits	1.66	0.10	
	(1.26)		
Household wage bill	0.94	0.20	
	(1.12)		
Enterprise capital income	0.07	0.34	
	(0.17)		
Enterprise taxes paid	0.04	0.08*	
	(0.03)		
Panel C: Expenditure and income multipliers			
Average of both multipliers	2.69	0.03**	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.

References

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. 2006. "Adaptive Linear Step-up Procedures That Control the False Discovery Rate." *Biometrika,* 491–507.

Conley, Timothy G. 2008. "Spatial Econometrics." In *The New Palgrave Dictionary of Economics,* Second Edition, edited by Steven N. Durlauf and Lawrence E. Blume, 7:741–47. Houndsmills: Palgrave Macmillan.

Deaton, Angus, and Salman Zaidi. 2002. *Guidelines for constructing consumption aggregates for welfare analysis.* Vol. 135. World Bank Publications.

Haushofer, Johannes, Edward Miguel, Paul Niehaus, and Michael Walker. 2017. "GE Effects of Cash Transfers: Pre-analysis plan for household welfare analysis." July.

Haushofer, Johannes, and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." *QJE* 131 (4): 1973–2042.