Development Economics: Lecture 2

Poverty Traps and Policy Scale-up

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Structure of the course (days 1–5)

Topics 1-5 (Moneke)

- Topic 1 (Mon 09/09): Econ. Growth and Transformation
- Topic 2 (Tue 10/09): Poverty Traps and Policy Scale-up
- Topic 3 (Wed 11/09): Infrastructure and Spatial Development
- Topic 4 (Thu 12/09): Energy Access and Electrification Puzzle
- Topic 5 (Fri 13/09): Climate Change, Environment and Dev.

The case for policy/intervention in development economics

- limited role for policy under competitive markets, can only:
  1. temporarily increase growth rate, e.g.
     - increase TFP growth rate by subsidising innovation
     - reduce labour force
     - decrease depreciation rate
  2. introduce wedges and distortions, e.g.
     - subsidise factors (labour, capital, fertilizer, etc.), misallocation
     - re-distribute resources for equity
     - tax otherwise efficient allocations

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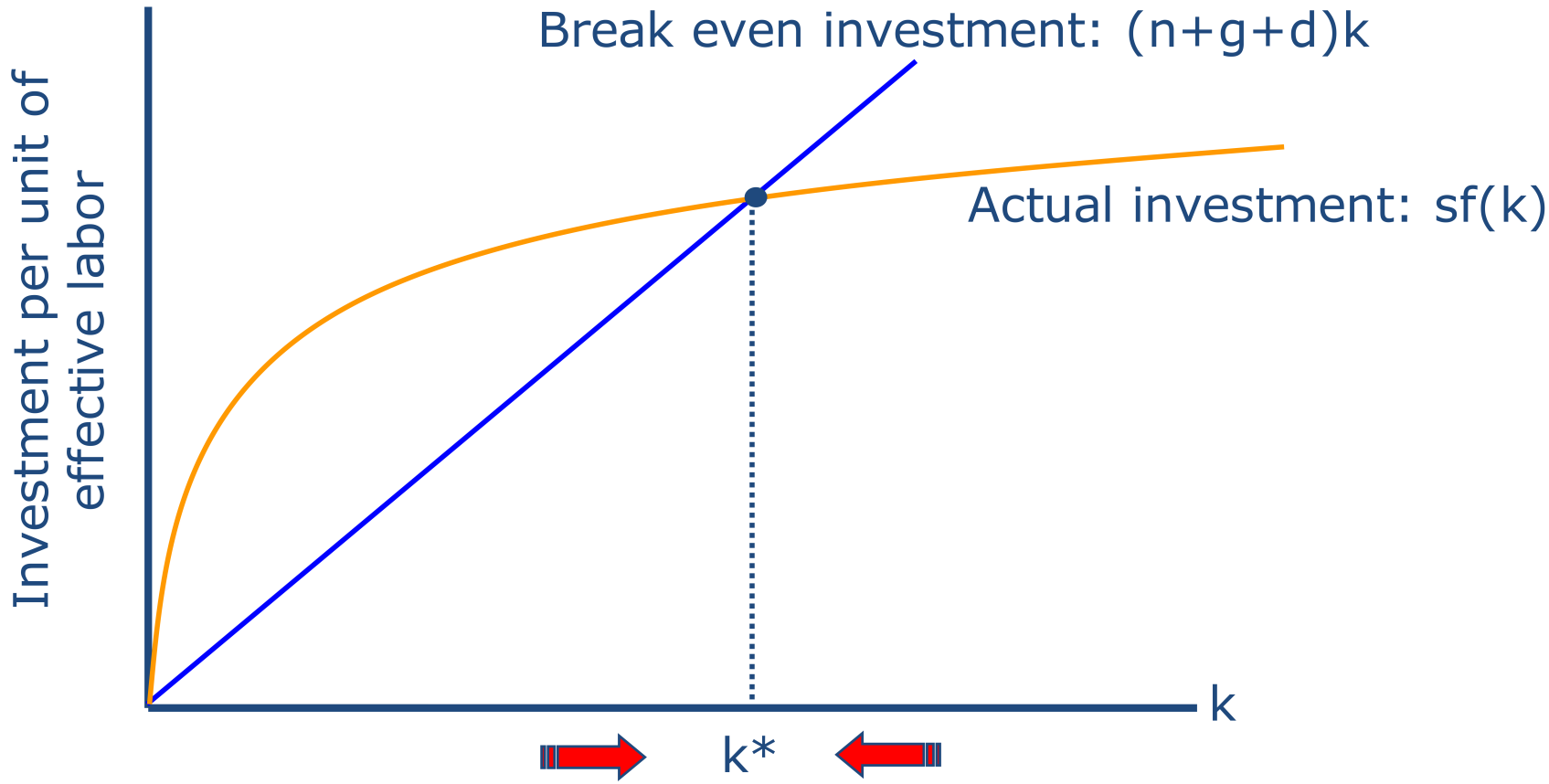
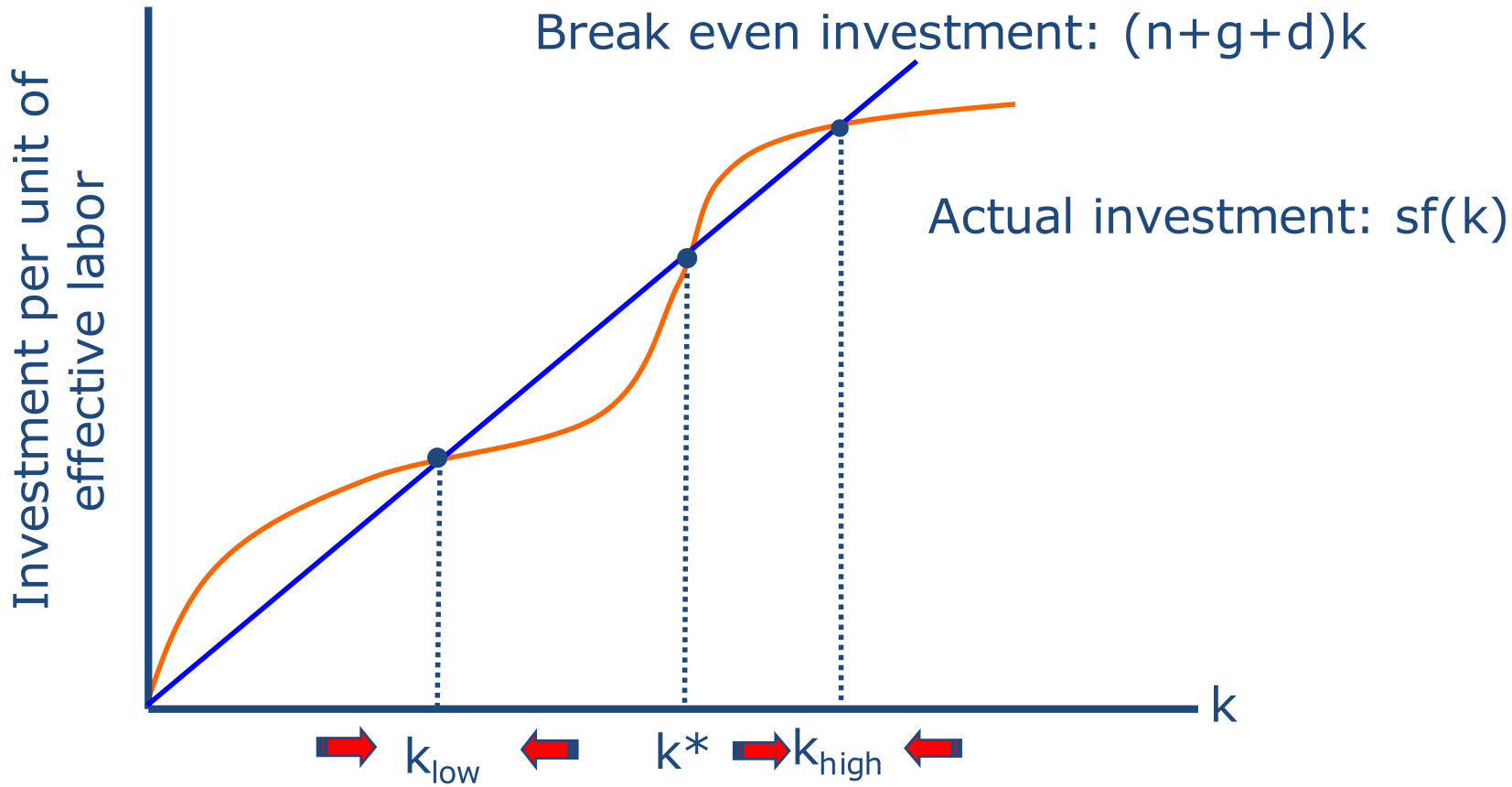
- however, in the presence of market failures or non-convexities, policy can have large, long-term effects
- development economists recommend evidence-based policy:
  - experimentally vary treatment across groups (e.g. RCT)
  - estimate average treatment effect
  - measure cost/benefit ratio
  - scale-up policy?

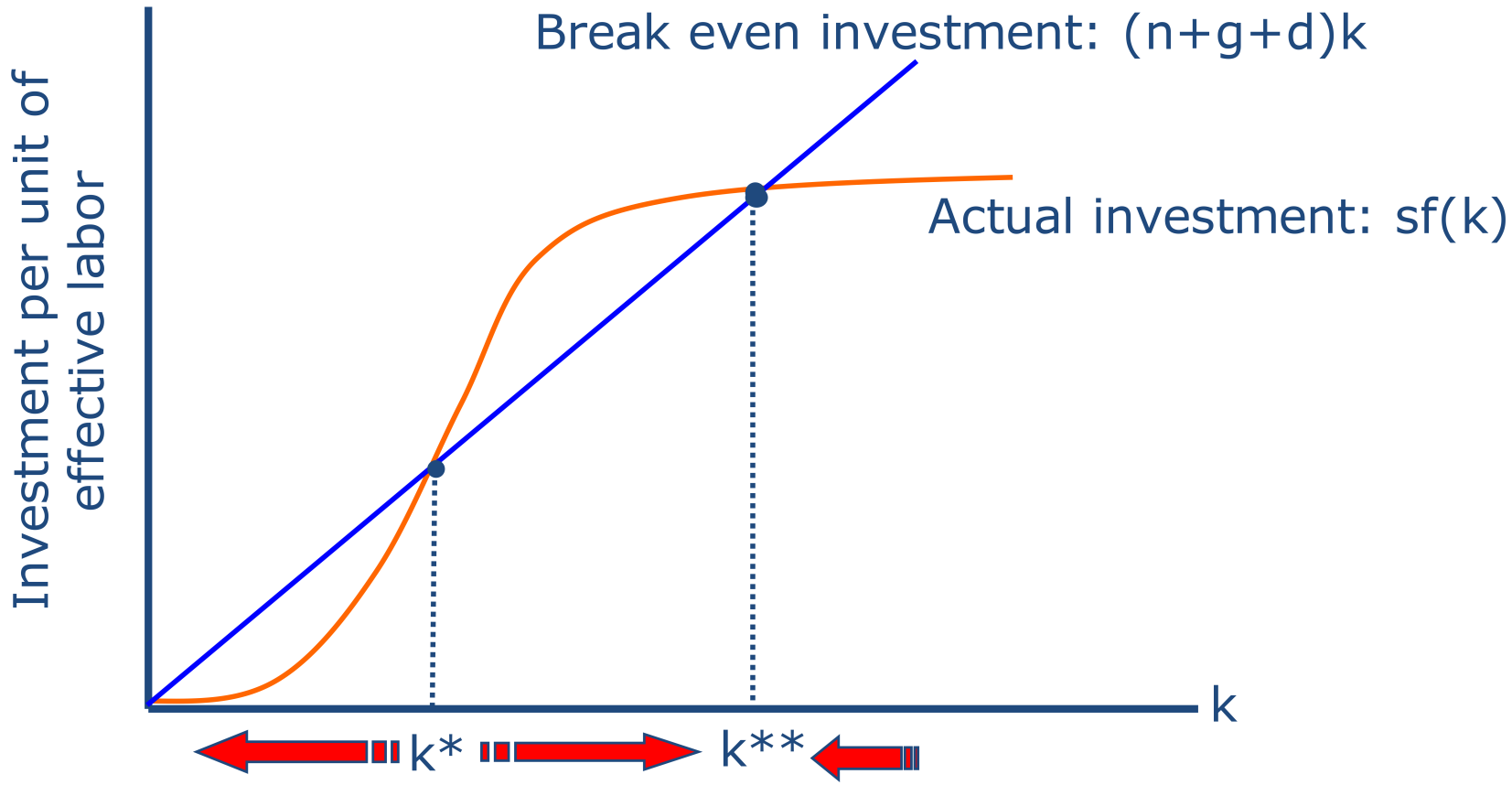
Solow (1956): steady state



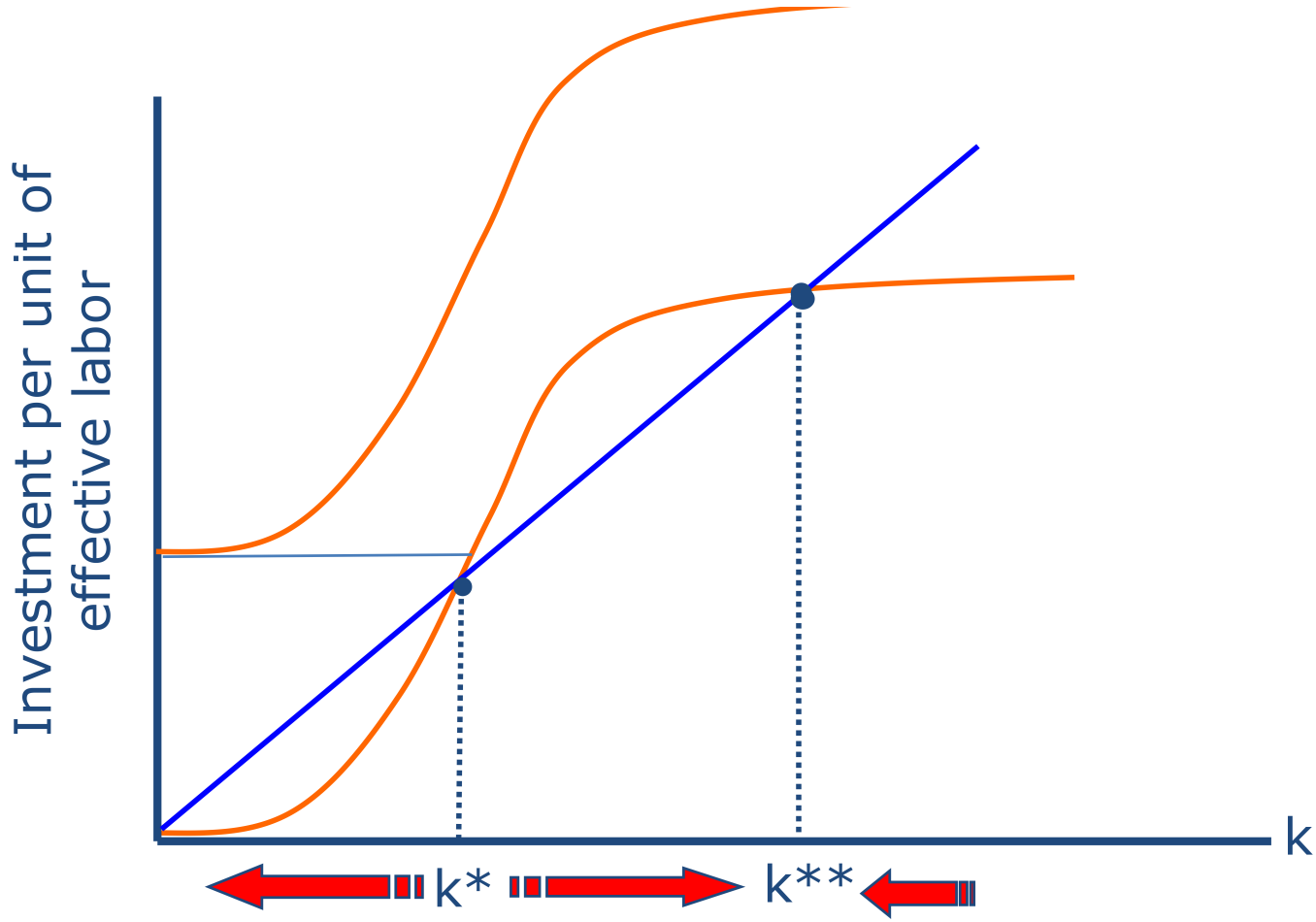
Solow (1956): beyond CRS – poverty trap at k_{low}



Solow (1956): beyond CRS – poverty trap at k_0



Solow (1956): one-off policy shock to escape poverty trap



Why do people stay poor?

- labour is the sole endowment of the poor
  - $\rightarrow$ we need to understand what determines earnings
    - earnings equal wage times hours worked
- recap: large productivity differences across sectors, occupations, jobs

$\rightarrow$ occupational choice, i.e. choice of job, becomes highly relevant

Features of Village Labour Markets

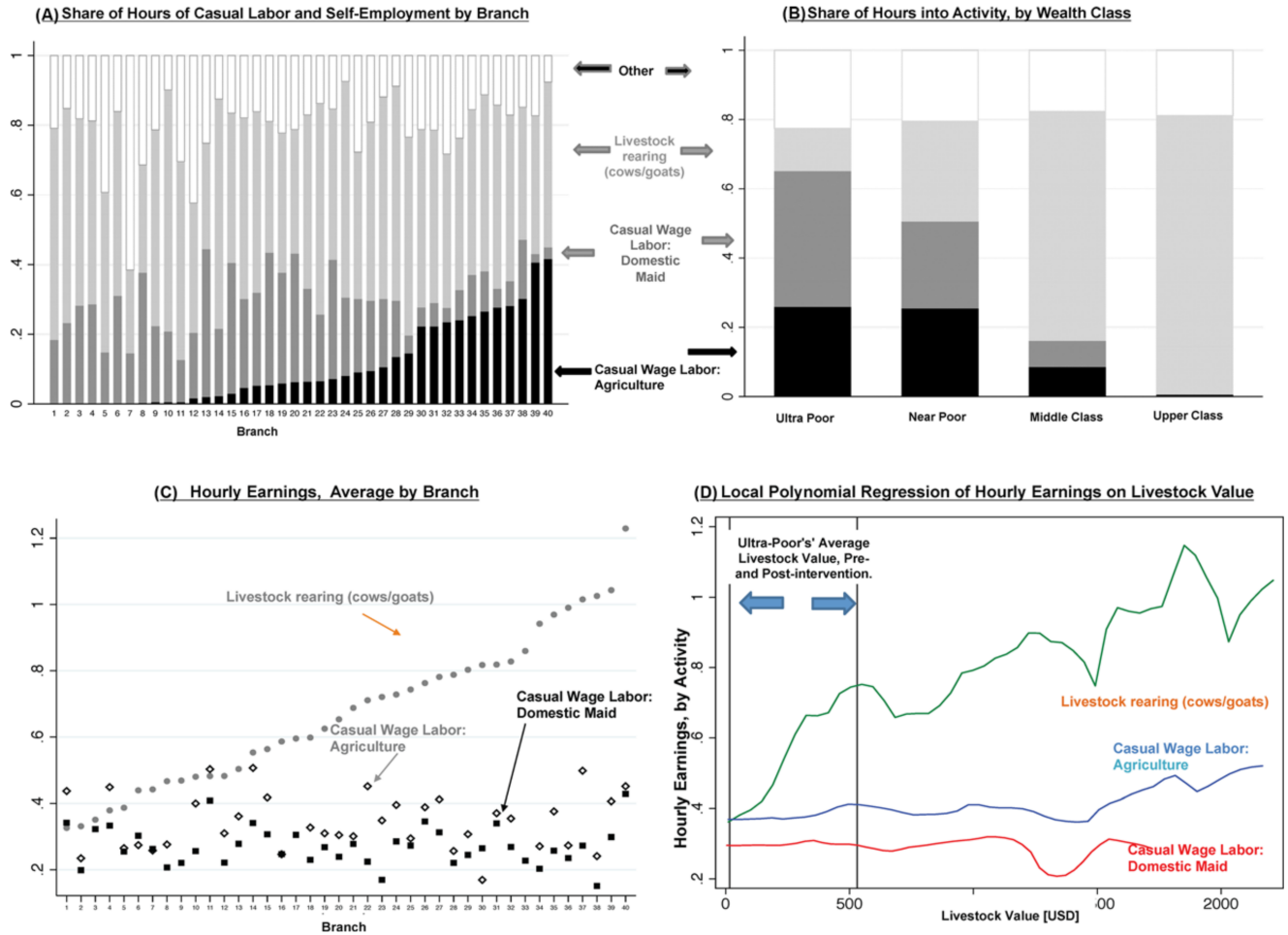


FIGURE I

Features of Rural Labor Markets for Women

Source: Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor markets and poverty in village economies. *The Quarterly journal of economics*, *132*(2), 811–870.

Features of Village Labour Markets (II)

TABLE II

LABOR MARKET ACTIVITIES OF WOMEN, BY WEALTH CLASS

Means (std. dev.)	(1) Ultra-poor	(2) Near-poor	(3) Middle class	(4) Upper class
Engaged in any income-generating activity	0.843	0.810	0.863	0.903
Total hours worked in the past year	991	769	553	502
	(894)	(812)	(596)	(502)
Total days worked in the past year	252	265	302	325
	(137)	(142)	(123)	(103)
Casual wage labor				
Hours devoted to agricultural labor	258	196	47.7	3.05
	(533)	(467)	(236)	(49.9)
Hours devoted to domestic maid	388	193	41.9	0.648
	(708)	(516)	(251)	(22.7)
Capital-intensive activities:				
Hours devoted to livestock rearing (cows/goats)	121	221	366	404
	(265)	(341)	(390)	(370)
Number of sample households	6,732	6,743	6,328	2,036

Notes. All statistics are constructed using baseline household data from both treatment and control villages. Wealth classes are based on the participatory rural assessment (PRA) exercise: the ultra-poor are ranked in the bottom wealth bins (fourth if four bins are used, fifth if five are used) and meet the program eligibility criteria, the near-poor are ranked in the bottom wealth bins and do not meet the program eligibility criteria, the middle-class are ranked in the middle wealth bins (second and third if four are used; second, third, and fourth if five are used), and the upper classes are those ranked in the top bin. The number of households in each wealth class at baseline is reported at the bottom of the table. Engagement in any income-generating activity covers all potential activities.

Source: Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor markets and poverty in village economies. *The Quarterly journal of economics*, *132*(2), 811–870.

Informal/causal jobs akin to modern ‘zero hour’ contracts

- characterised by low wage
- unpredictable, offered on a daily/hourly basis
- uninsured, no guaranteed minimum hours
- unstable, wage sensitive to production shocks

$\rightarrow$ probably hide a lot of underemployment

Targeting the Ultra-Poor: a large-scale intervention

- eligible: poor women, identified by communities themselves, verified by BRAC employees
- on average, 6 women per community (7% of HHs) eligible
- treatment: menu of assets
  (e.g. livestock, small crafts, small retail, etc.)
- recipient commits to retain asset for 2 years, free to sell after

→ revealed preference: almost all choose a livestock combination

→ value of transfer (TK9500 = USD140)

  - e.g. one year of per capita expenditure
  - or twice yearly earnings
  - nine times annual savings

→ receive asset specific training, intensive over first year

Targeting the Ultra-Poor: implementation of RCT

- randomise programme roll-out across 40 BRAC branch offices (1309 communities) in the poorest areas of Bangladesh
  - stratified by subdistrict
  - 20 branch office areas treated in 2007, 20 branch office areas treated in 2011
  - matched pair randomisation
- randomise at the branch rather than community level to minimise contamination
- beneficiaries selected in both treatment and control communities
- sampling: beneficiaries and all other poor and a sample of other wealth classes surveyed in 2007, 2009, 2011, 2014
- final sample:
  - 6,732 eligible beneficiaries
  - 16,297 HHs from other classes

Targeting the Ultra-Poor: experimental results

TABLE III

TREATMENT EFFECTS ON THE LABOR SUPPLY AND EARNINGS OF ULTRA-POOR WOMEN

	Livestock		Agriculture		Maid		All activities	
	(1) Hours	(2) Days	(3) Hours	(4) Days	(5) Hours	(6) Days	(7) Hours	(8) Days
Panel A: Labor supply								
Program impact after 2 years	488***	205.5***	−42.3	−3.54	−57.4	−8.45	341***	72.4***
	(30.7)	(11.1)	(53.0)	(7.02)	(42.9)	(5.88)	(67.9)	(10.0)
Program impact after 4 years	415***	171.6***	−46.2	−4.77	−117**	−16.77***	206***	61.1***
	(38.9)	(10.9)	(42.7)	(5.43)	(45.0)	(5.82)	(73.0)	(12.5)
Control mean at 4-year follow-up	191.00	94.76	278.14	35.40	447.05	63.97	1,217.00	277.40
4-year impact: % change	217%	181%	−17%	−13.5%	−26%	−26%	17%	22%
2-year impact = 4-year impact [p-value]	0.111	0.023	0.930	0.831	0.125	0.125	0.080	0.179
Adjusted R-squared	0.335	0.367	0.184	0.183	0.067	0.061	0.072	0.069
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732	6,732	6,732
Number of observations	20,196	20,196	20,196	20,196	20,196	20,196	20,196	20,196
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)

Source: Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor markets and poverty in village economies. *The Quarterly journal of economics*, *132*(2), 811–870.

Targeting the Ultra-Poor: experimental results (II)

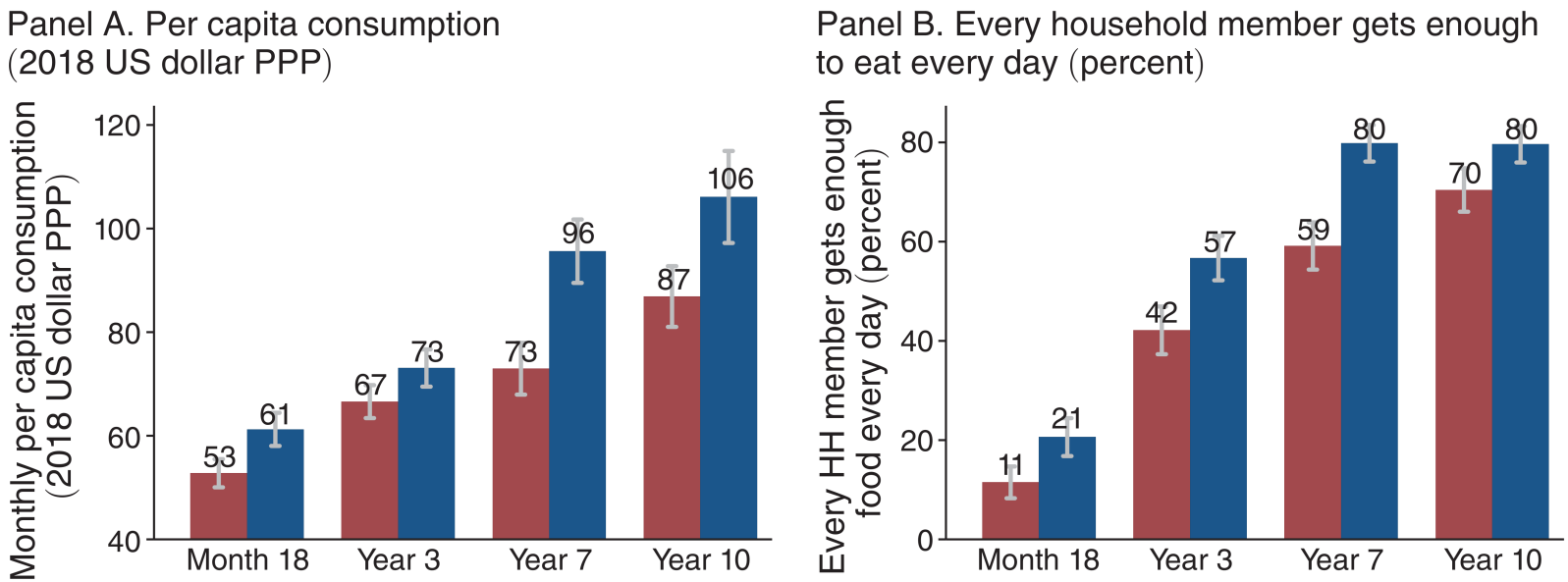
TABLE III
(CONTINUED)

	Livestock	Agriculture		Maid		All activities
	(9) Earnings	(10) Wage	(11) Earnings	(12) Wage	(13) Earnings	(14) Earnings
Panel B: Earnings						
Program impact after 2 years	80***	0.028	−9.99	0.034	−11.48	62.3**
	(14.0)	(0.021)	(13.98)	(0.022)	(11.36)	(30.17)
Program impact after 4 years	115***	0.053**	−3.89	0.074***	−25.25**	87.8***
	(14.1)	(0.024)	(13.97)	(0.019)	(11.57)	(28.58)
Control mean at 4-year follow-up	18.48	0.441	96.44	0.354	112.84	410.92
4-year impact: % change	16%	12%	−4%	21%	−22%	21%
2-year impact = 4-year impact [p-value]	0.049	0.219	0.701	0.080	0.205	0.455
Adjusted R-squared	0.127	0.486	0.178	0.241	0.095	0.088
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732
Number of observations	20,120	5,227	19,883	5,833	19,796	20,135
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. Sample: ultra-poor women (std. err. in parentheses), clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Intent-to-treat estimates are reported based on a difference-in-difference specification estimated using OLS. This regresses the outcome of interest for woman i in village v in survey wave t on a constant, a dummy for whether the woman resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms. Standard errors are clustered by BRAC branch area. All outcomes are measured at the individual level (for the ultra-poor woman in the household) and defined for the year prior to survey date. We report the mean of each dependent variable as measured at baseline in treated villages. In all columns we report the p-value on the null hypothesis that the two- and four-year ITT impacts are equal. The number of ultra-poor is the number of eligible women observed at baseline and in both follow-up survey waves. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

Source: Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor markets and poverty in village economies. *The Quarterly journal of economics*, *132*(2), 811–870.

Targeting the Ultra-Poor: results from elsewhere



Source: Banerjee, A., Duflo, E., & Sharma, G. (2021). Long-term effects of the targeting the ultra poor program. *The American economic review. Insights*, *3*(4), 471–486.

Targeting the Ultra-Poor: results from elsewhere (II)

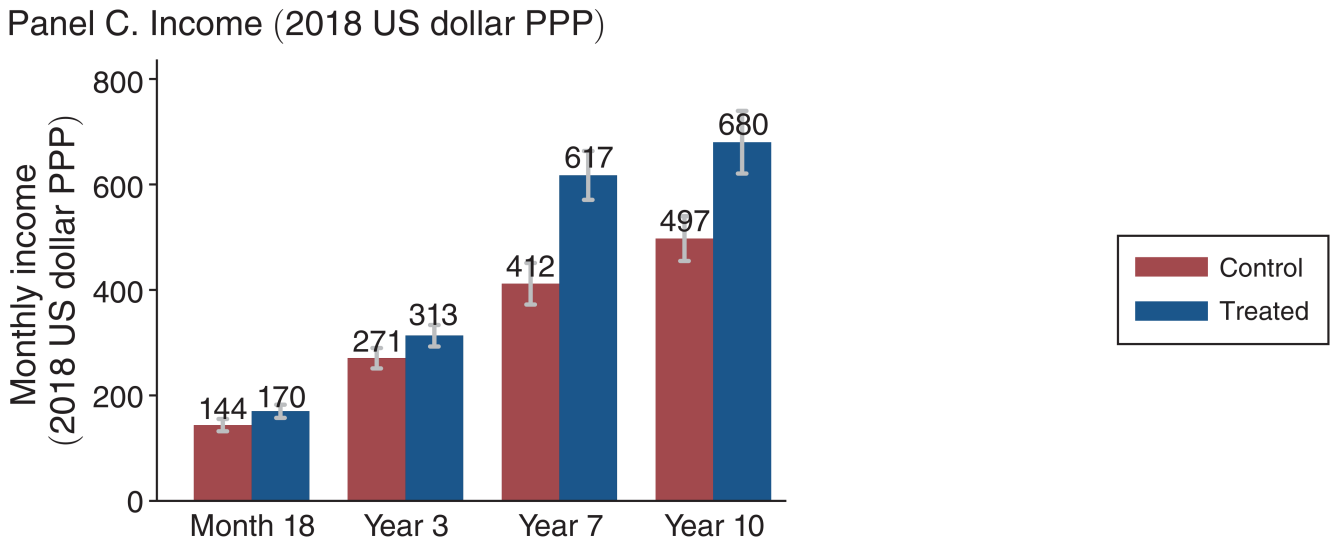


FIGURE 1. CONSUMPTION AND INCOME OVER TIME

Source: Banerjee, A., Duflo, E., & Sharma, G. (2021). Long-term effects of the targeting the ultra poor program. *The American economic review. Insights*, *3*(4), 471–486.

Targeting the Ultra-Poor: quantile treatment effects results

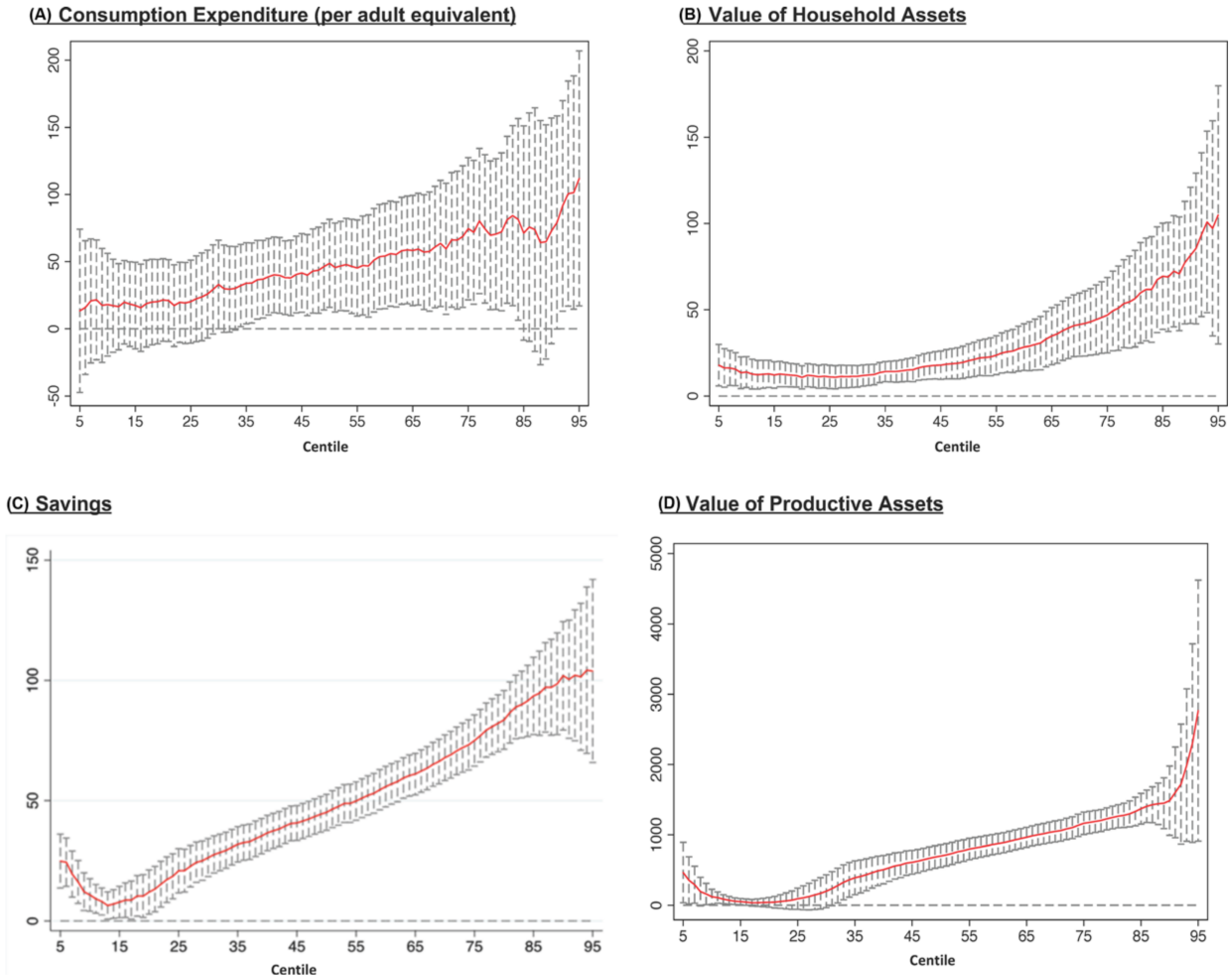
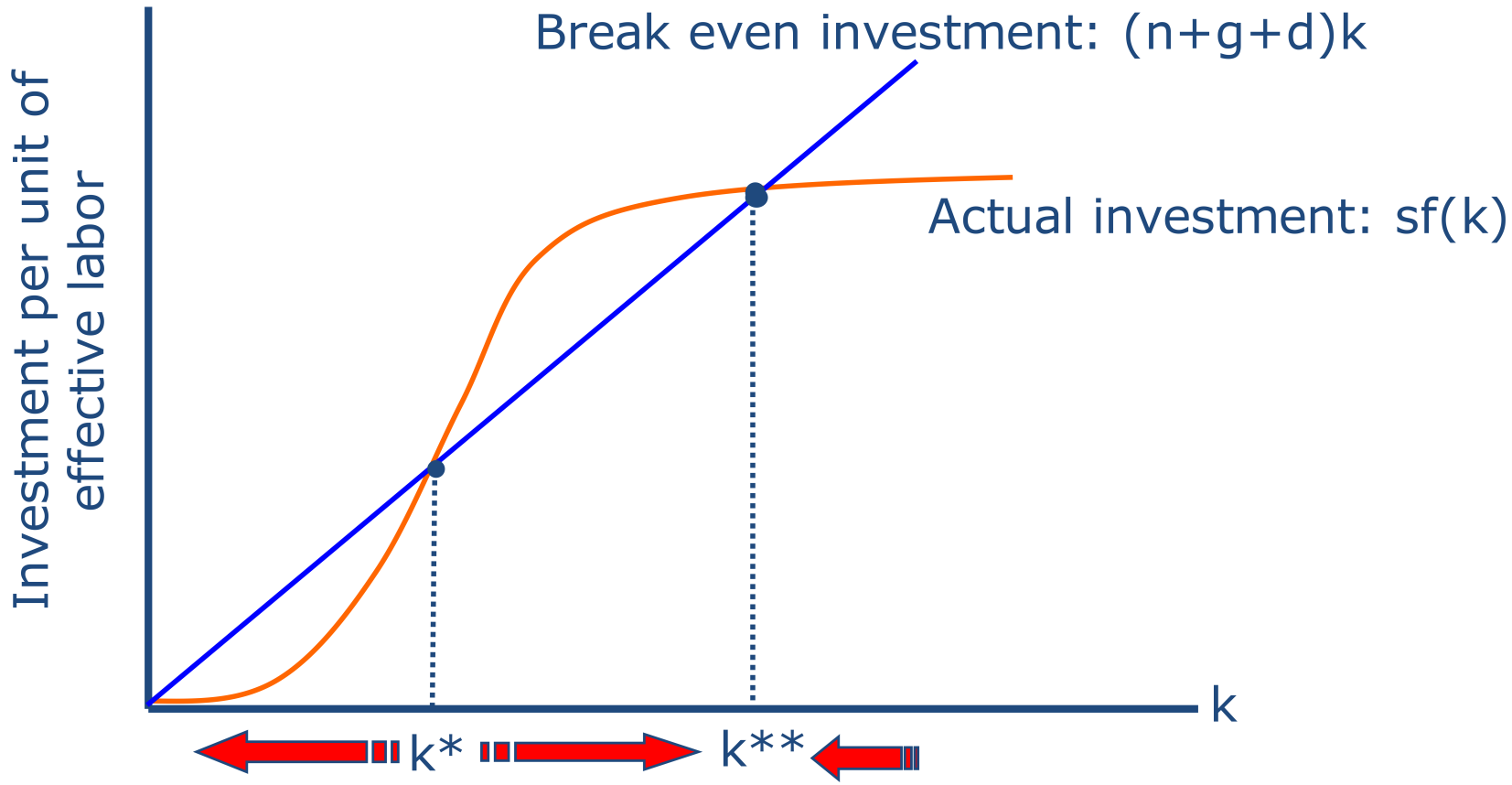


FIGURE II

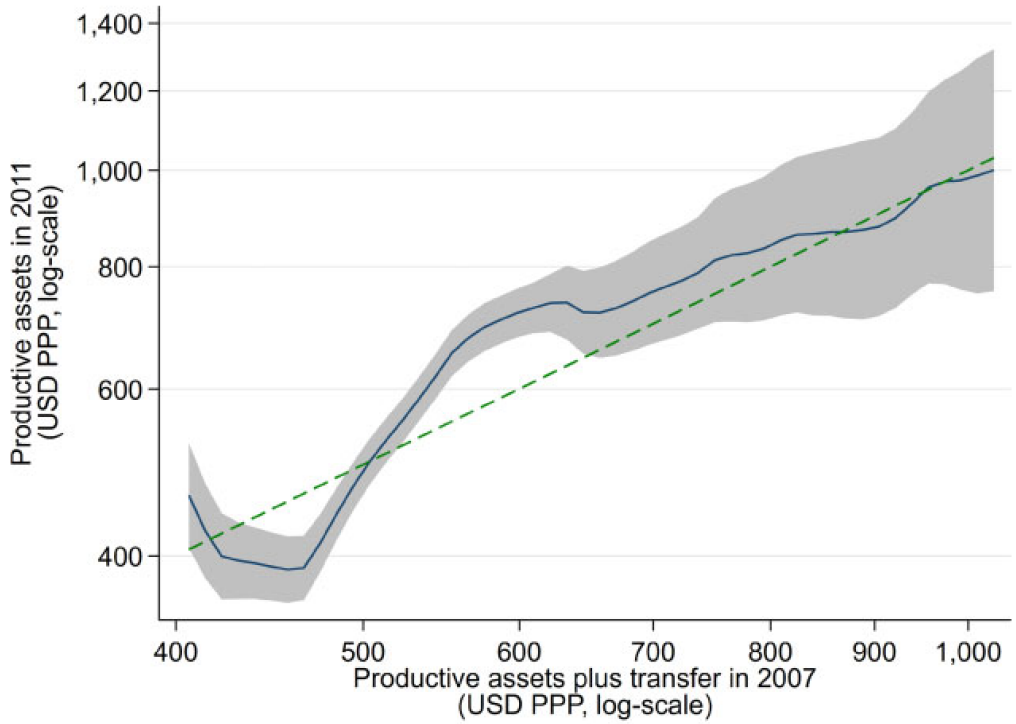
Four-Year Quantile Treatment Effects

Source: Bandiera, O., Burgess, R., Das, N., Gulesci, S., Rasul, I., & Sulaiman, M. (2017). Labor markets and poverty in village economies. *The Quarterly journal of economics*, *132*(2), 811–870.

Recap: poverty trap at k_0



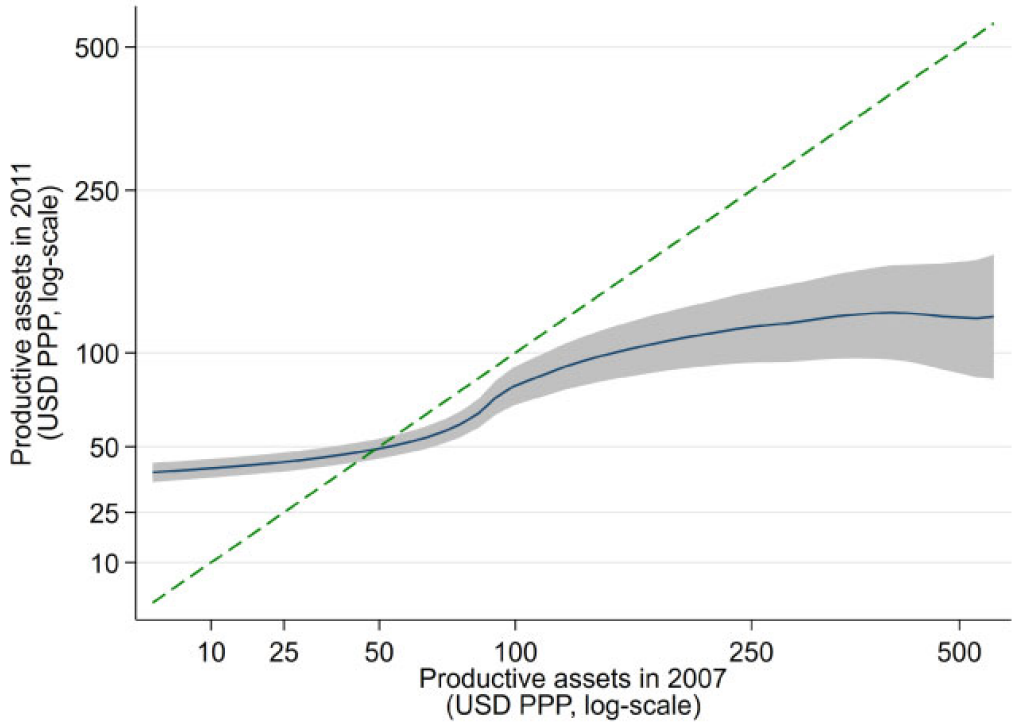
Estimating $\hat{k}$, the unstable steady state



(A) Treatment villages

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

Estimating $\hat{k}$, the unstable steady state (control)



(B) Control villages

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

Divergence in asset accumulation

TABLE II

SHORT-TERM RESPONSES TO THE ASSET TRANSFER

	Dependent variable: log change of productive assets 2007–2011				
	Panel A		Panel B		
	Treatment (1)	Treatment (2)	Control (3)	Control (4)	Both (5)
Above $\hat{k}$	0.297***	0.475***	−0.020	−0.097	−0.020
	(0.043)	(0.070)	(0.052)	(0.598)	(0.057)
Baseline assets		−2.199***		−0.463*	
		(0.698)		(0.266)	
Above $\hat{k}$ × baseline assets		1.969***		−0.097	
		(0.729)		(0.269)	
Treatment					−0.483***
					(0.059)
Above $\hat{k}$ × treatment					0.318***
					(0.070)
Constant	−0.138***	−0.282***	0.345***	−0.680	0.345***
	(0.033)	(0.057)	(0.046)	(0.592)	(0.050)
N	3,292	3,292	2,450	2,450	5,742

Notes. * $p < .1$; ** $p < .05$; *** $p < .01$. Standard errors are in brackets. Sample: ultra-poor households in treatment and control villages with log baseline productive assets below 3 (observations from control households are excluded if their baseline productive assets were above 3 had they received the transfer). The dependent variable is the difference between log productive assets in 2011 and log of productive assets in 2007, where productive assets are defined as the total value of livestock, poultry, business assets (e.g., tools, vehicles, and structures), and land. Above $\hat{k}$ equals 1 if the baseline asset stock plus the imputed transfer is larger than 2.333, and 0 otherwise. In treatment, this represents households' actual posttransfer asset stock. In control, where no transfer was received, Above $\hat{k}$ indicates whether the household would be above 2.333 if it had received a transfer. Baseline assets always refers to the actual asset stock, that is, in control without the imputed transfer. Baseline assets are centered at 2.333, such that the coefficient on Above $\hat{k}$ reflects the log change at the threshold. Treatment was assigned at the village level.

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

Testing the mechanism: capital constraints?

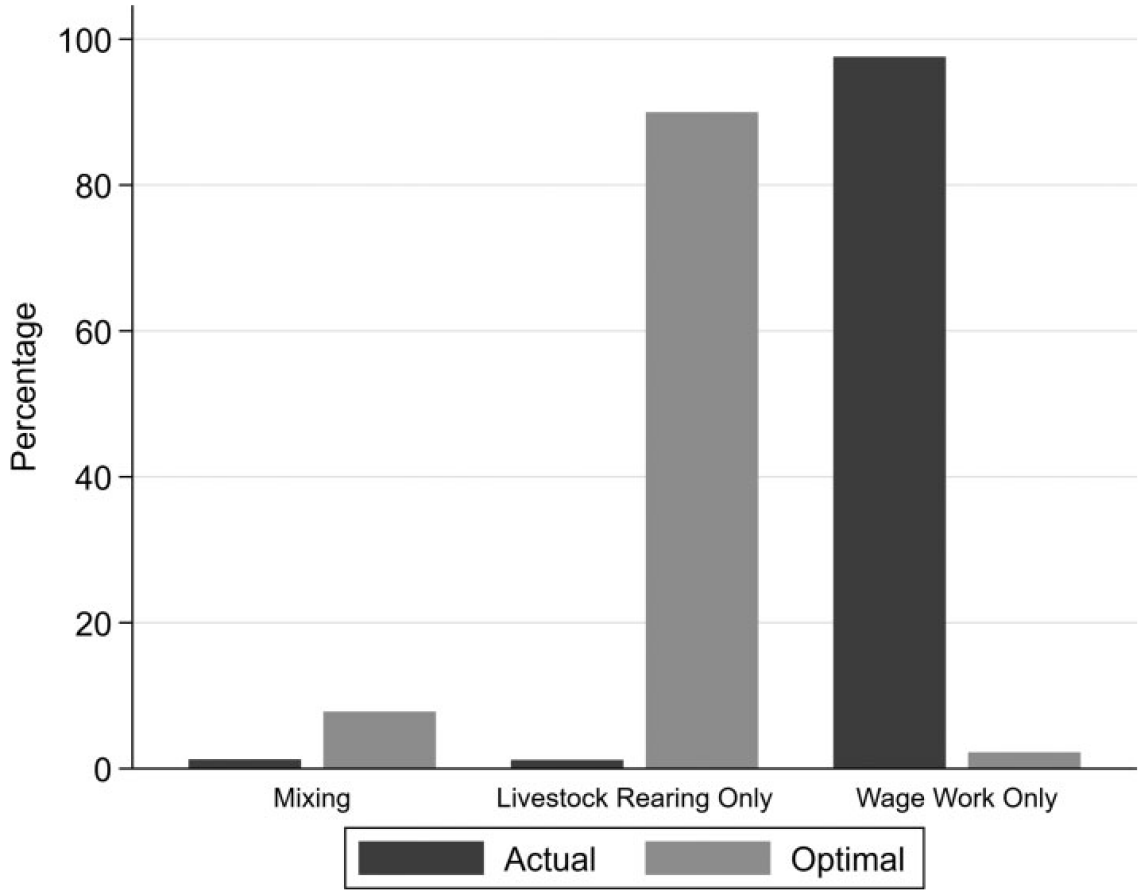


FIGURE VIII

Occupational Choice: Actual versus Model Prediction in the Absence of Capital Constraints

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

Policy implications

- existence of a poverty threshold implies that only transfers large enough to push beneficiaries past the threshold will reduce poverty in the long run
- smaller transfers might increase consumption for a short period but will have no long lasting effects
  - BRAC asset transfer worth USD515 (1 year of PCE) was enough for 66% of beneficiaries
  - micro-loans are typically <USD200, which might explain the disappointing effects of microfinance (Banerjee et al., 2010)

Crucial that intervention is 'big enough'

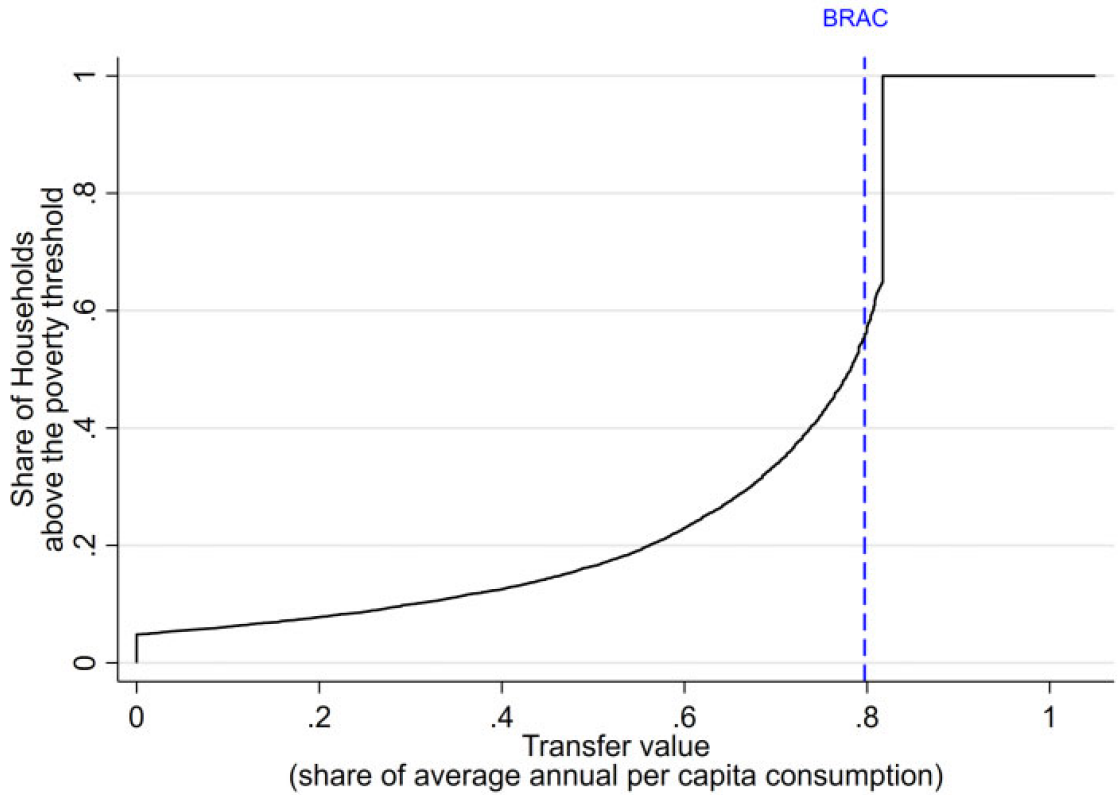
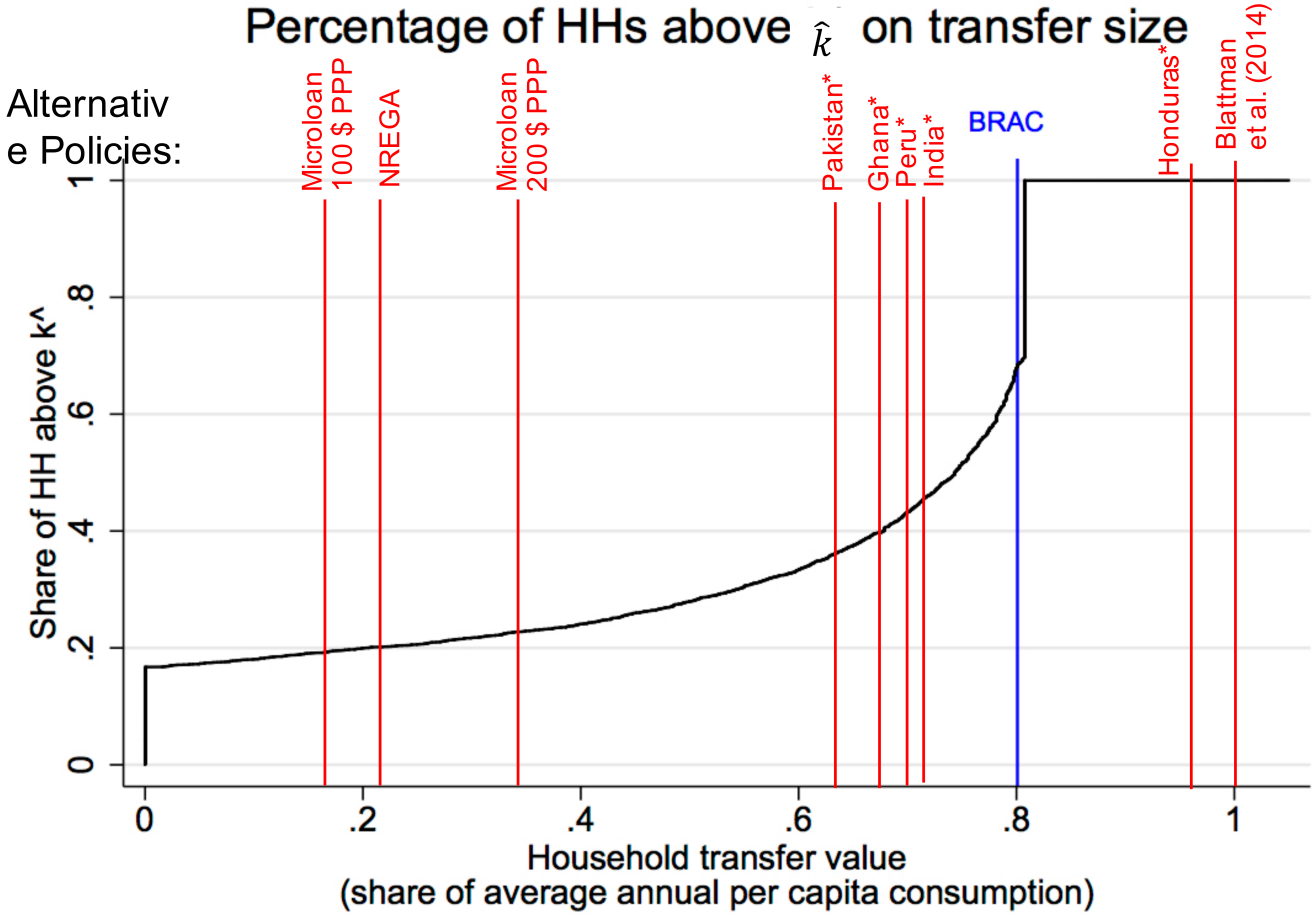


FIGURE IX

Share of Ultra-Poor Households above the Poverty Threshold as a Function of the Transfer Size

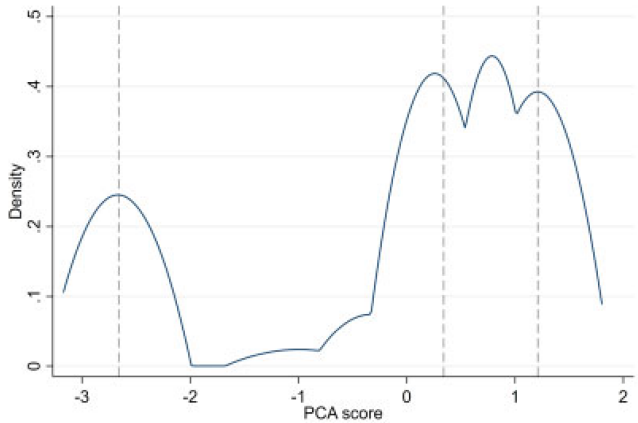
Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

. . . and explains why other interventions fall short

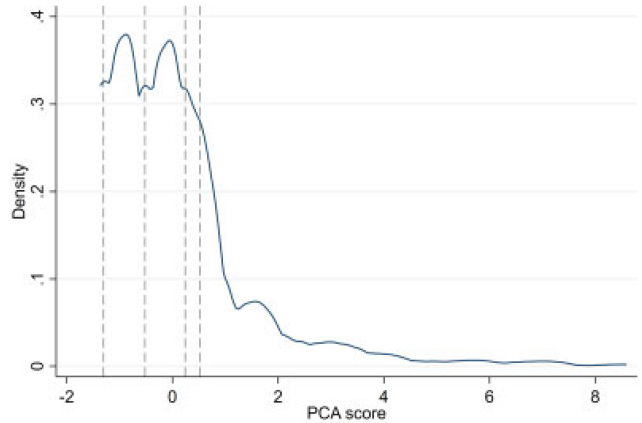


Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

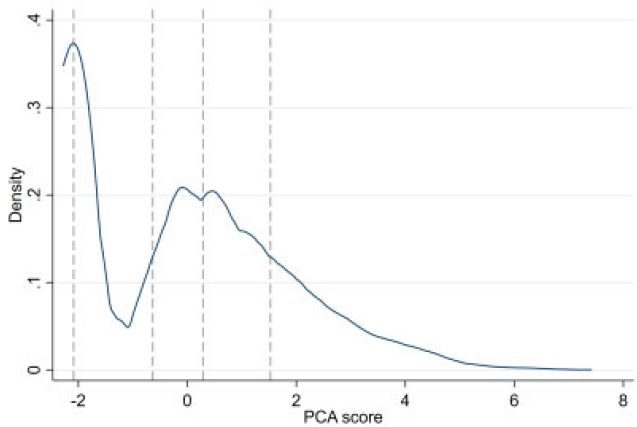
External validity: poverty traps in other countries?



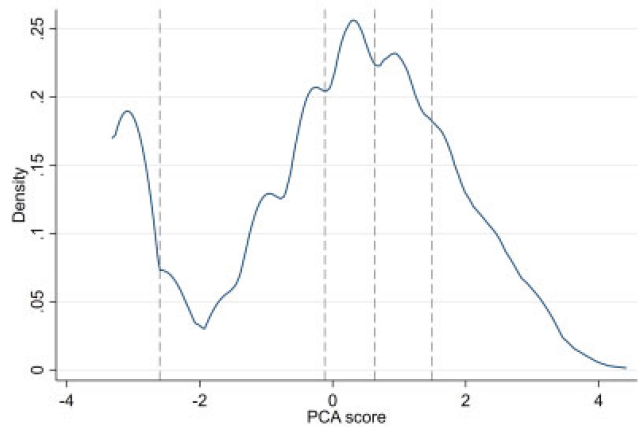
(A) Bangladesh, 2014



(B) India, 2015



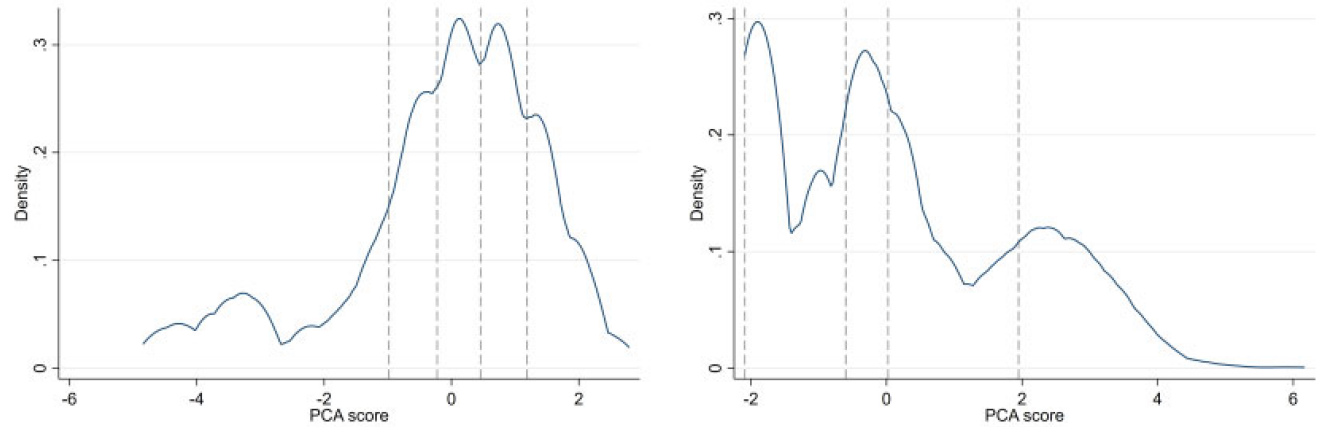
(C) Pakistan, 2017



(D) Afghanistan, 2015

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

External validity: poverty traps in other countries? (II)



(E) Nepal, 2016 (F) Myanmar, 2015

FIGURE X

Distribution of Agricultural Assets for Rural Households across South Asia

The graphs show kernel density plots of wealth scores for six South Asian countries, based on microdata from harmonized IPUMS and DHS household surveys. The wealth scores are constructed by performing a principal component analysis (PCA) at the household level using a full list of agricultural assets. The list of specific assets varies across countries. The first component of the PCA is used to compute the wealth index. All kernel density estimates use an Epanechnikov kernel with a bandwidth of 0.3. The vertical dashed lines denote quintiles of the wealth distribution.

Source: Balboni, C., Bandiera, O., Burgess, R., Ghatak, M., & Heil, A. (2021). Why do people stay poor? *The Quarterly journal of economics*, *137*(2), 785–844.

Conclusions from Balboni et al. (2021)

- key insight – misallocation of talent
- poor people are not unable to take on more productive employment activities – they just lack the needed capital
- program releases this constraint – those closer to the threshold cross it and move out of poverty, those further away sink back into poverty
- key policy conclusion – need big push policies to tackle persistent poverty
- such policies need to focus on tapping into abilities and talents of the poor rather than just propping up their consumption

Recap: structural transformation and growth

- large low-income country employment share in agriculture
- historical precedence of agriculture shrinking as countries grow
- agriculture shockingly low productivity
- agricultural productivity gaps large
- unclear productivity effects of moving individuals

$\rightarrow$ gaps vs wedges: do frictions prevent structural transformation?

$\rightarrow$ why are not more people moving into non-agr. jobs in cities?

Addressing migration frictions directly

Bryan et al. (2014): test policy to support (seasonal) migration

- experimentally vary incentives for seasonal migration to determine micro-foundations of frictions
- seasonal migration to non-agricultural jobs in city may help in avoiding seasonal monga famine in rural areas

$\rightarrow$ free bus ticket dramatically increases migration adoption

$\rightarrow$ gains from migration so large, previous lack of uptake hard to rationalise

$\rightarrow$ impressive results of a small policy intervention

$\rightarrow$ cost/benefit ratio hugely favourable

Bryan et al. (2014): free bus ticket induces migration ↑

TABLE II

PROGRAM TAKE-UP RATES[a]

	Incentivized	Cash	Credit	*Not Incentivized*	Info	Control	*Diff. (I – NI)*
Migration rate in 2008	58.0%	59.0%	56.8%	36.0%	35.9%	36.0%	22.0***
	(1.4)	(1.9)	(2.1)	(2.0)	(2.8)	(2.8)	(2.4)
Migration rate in 2009	46.7%	44.6%	49.1%	37.5%	34.4%	40.5%	9.2***
	(1.4)	(1.9)	(2.1)	(2.0)	(2.8)	(2.9)	(2.5)
Migration rate in 2011[b]	39%			32%			7.0**
	(2.1)			(2.5)			(3.3)

[a]Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Diff. Incentivized − Not Incentivized tests the difference between migration rates of incentivized and non-incentivized households, regardless of whether they accepted our cash or credit. No incentives were offered in 2009.

[b]For re-migration rate in 2011, we compare migration rates in control villages that never received any incentives to the subset of 2008 treatment villages that did not receive any further incentives in 2011. Note that migration was measured over a longer period (covering the main monga season) in 2008 and 2009, and a different time period (the mini-monga season) in 2011.

Source: Bryan, G., Chowdhury, S., & Mobarak, A. M. (2014). Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh. *Econometrica*, *82*(5), 1671–1748.

Bryan et al. (2014): consumption ↑, low migration puzzling

TABLE III

EFFECTS OF MIGRATION BEFORE DECEMBER 2008 ON CONSUMPTION AMONGST REMAINING HOUSEHOLD MEMBERS[a]

	ITT								
	Cash	Credit	Info	ITT	ITT	IV	IV	OLS	Mean
				Panel A: 2008 Consumption					
Consumption of food	61.876**	50.044*	15.644	48.642**	44.183*	280.792**	260.139**	102.714***	726.80
	(29.048)	(28.099)	(40.177)	(24.139)	(23.926)	(131.954)	(128.053)	(17.147)	
Consumption of non-food	34.885***	27.817**	22.843	20.367**	16.726*	115.003**	99.924*	59.085***	274.46
	(13.111)	(12.425)	(17.551)	(9.662)	(9.098)	(56.692)	(51.688)	(8.960)	
Total consumption	96.566***	76.743**	38.521	68.359**	60.139**	391.193**	355.115**	160.696***	1000.87
	(34.610)	(33.646)	(50.975)	(30.593)	(29.683)	(169.431)	(158.835)	(22.061)	
Total calories	106.819*	93.429	−85.977	142.629***	129.901***	842.673***	757.602***	317.495***	2090.26
(per person per day)	(62.974)	(59.597)	(76.337)	(47.196)	(48.057)	(248.510)	(250.317)	(41.110)	

Source: Bryan, G., Chowdhury, S., & Mobarak, A. M. (2014). Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh. *Econometrica*, *82*(5), 1671–1748.

Potential conclusion: scale up, free bus tickets for all?

Bryan et al. (2014): test direct support for (seasonal) migration

- → free bus ticket dramatically increases migration adoption
- → gains from migration so large, previous lack of uptake hard to rationalise
- → impressive results of a small policy intervention
- → cost/benefit ratio hugely favourable

Would you recommend scale-up of subsidised/free bus tickets during seasonal famine season to the Bangladeshi government?

Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): limited scale-up and GE effects

- limited scale-up: free bus ticket to 133 villages (1.3x scale), 5792 potential migrants (5x scale)
- saturation design: randomise saturation by targeting either 14% or 70% of eligible population (landless, poor) per village

Akram et al. (2018): limited scale-up and GE effects

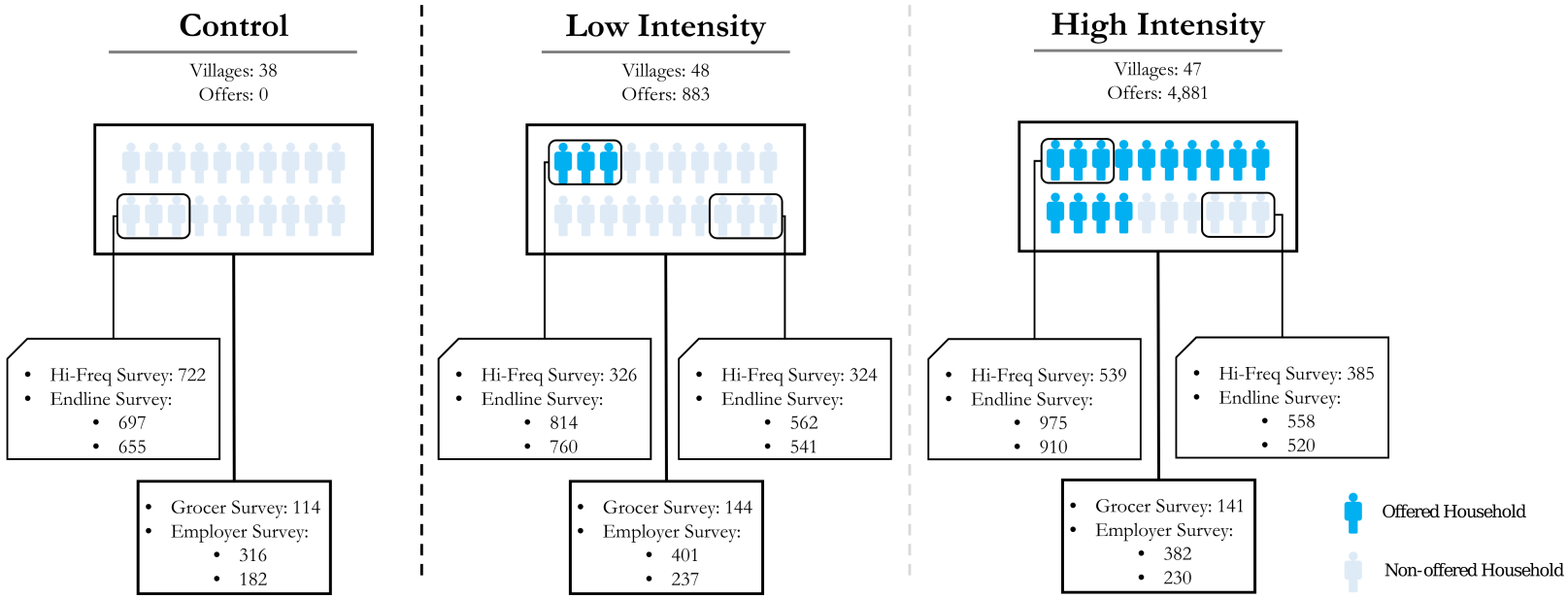
- limited scale-up: free bus ticket to 133 villages (1.3x scale), 5792 potential migrants (5x scale)
- saturation design: randomise saturation by targeting either 14% or 70% of eligible population (landless, poor) per village

$\rightarrow$ test for model-implied origin village GE effects:

1. strategic complements: if risk aversion deters seasonal migration, migration decisions could be strategic complements (travelling together)
2. strategic substitutes: larger number of migrants from village makes employment in village more attractive (if landholders cannot change production technology in short-run)
3. market integration: if food markets are not well integrated, local food prices may change with fewer people or additional village income

Akram et al. (2018): saturation design to vary intensity

Figure 1: Data Collection and Experimental Design



Boxes in upper-half denote experimental design, with light-blue figures representing eligible villagers not offered the travel grant and dark-blue representing eligible villagers offered the grant. Boxes in the bottom-half of the picture denote data collection. Boxes with a notched top-corner specify sample sizes for high-frequency and endline household surveys (of both offered and non-offered households). A detailed endline was administered in 2015 and a second, compact, endline was administered in 2016; the sample size for each of these is shown in the two sub-bullets under "Endline Survey". Unnotched boxes specify sample sizes for the grocer (shopkeeper) and employer surveys. The employer survey was administered twice – once in 2015 and once in 2016, with the respective sample sizes for each year specified by each sub-bullet under "Employer Survey".

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): migration decisions complementary

Table 1. Migration in 2014-15 and Remigration in 2015-16 in Response to Treatments in 2014

VARIABLES	(1) At least one migrant (2014-15)	(2) Number of migrants (2014-15)	(3) Migration episodes (2014-15)	(4) Re-migration in 2016, at least one migrant
Offered Grant in Low Intensity Treatment Village	0.248***	0.260***	0.390***	0.188***
	(0.0366)	(0.0405)	(0.0666)	(0.0341)
Not Offered Grant in Low Intensity Treatment Village	0.0333	0.0314	0.0759	0.0282
	(0.0388)	(0.0442)	(0.0720)	(0.0347)
Offered Grant in High Intensity Treatment Village	0.398***	0.412***	0.626***	0.293***
	(0.0333)	(0.0376)	(0.0630)	(0.0352)
Not Offered Grant in High Intensity Treatment Village	0.0965**	0.111**	0.127*	0.127***
	(0.0397)	(0.0463)	(0.0723)	(0.0371)
Observations	3,600	3,600	3,600	3,382
R-squared	0.137	0.119	0.124	0.089
Control Mean	.342	.367	.499	.378
Upazila FE	YES	YES	YES	YES
p-value: Offered High = Offered Low	0	0	0	.003
p-value: Non-Offered High = Non-Offered Low	.127	.101	.53	.009

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): migration decisions complementary

Table 3. Accomodation Sharing and Traveling with Companions Among Migrants (2015-16)

VARIABLES	(1) Number of companions with whom sharing accomodation	(2) Number of travel companions
Offered Grant in Low Intensity Treatment Village	-0.123	0.586
	(0.778)	(0.583)
Not Offered Grant in Low Intensity Treatment Village	-0.164	1.007
	(1.017)	(0.642)
Offered Grant in High Intensity Treatment Village	1.293	2.819***
	(0.892)	(0.708)
Not Offered Grant in High Intensity Treatment Village	-0.286	2.434***
	(0.781)	(0.641)
Observations	1,678	1,756
R-squared	0.052	0.091
Control Mean	10.123	6.17
Upazila FE	YES	YES
p-value: Offered High = Offered Low	.116	.002
p-value: Non-Offered High = Non-Offered	.906	.041

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): group migration more profitable

Table 6. LATE (IV) Estimates to Study the Differential Effects of Migration from Low-Intensity and High-Intensity Villages

	(1)	(2)
VARIABLES	Migration income	Migration income
Migrated, Low Intensity Treatment Village	4,672***	6,294***
	(1,138)	(1,030)
Migrated, High Intensity Treatment Village	6,173***	7,520***
	(946.7)	(1,032)
Observations	3,600	3,600
R-squared	0.150	0.171
Upazila FE	YES	YES
Instruments	High/Low Intensity	High/Low Intensity, Offered/Nonoffered
chi2-test High Intensity=Low Intensity	1.760	1.240
Prob > chi2	0.185	0.266
First Stage Partial R^2	0.393	0.422
First Stage F-test Statistic	305.4	165.4
First Stage p-value	0	0

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): origin labour market wages ↑

Table 10. LATE (IV) Estimates of the Effects of Emigration on Wages Paid in the Home Village as Reported by Employers

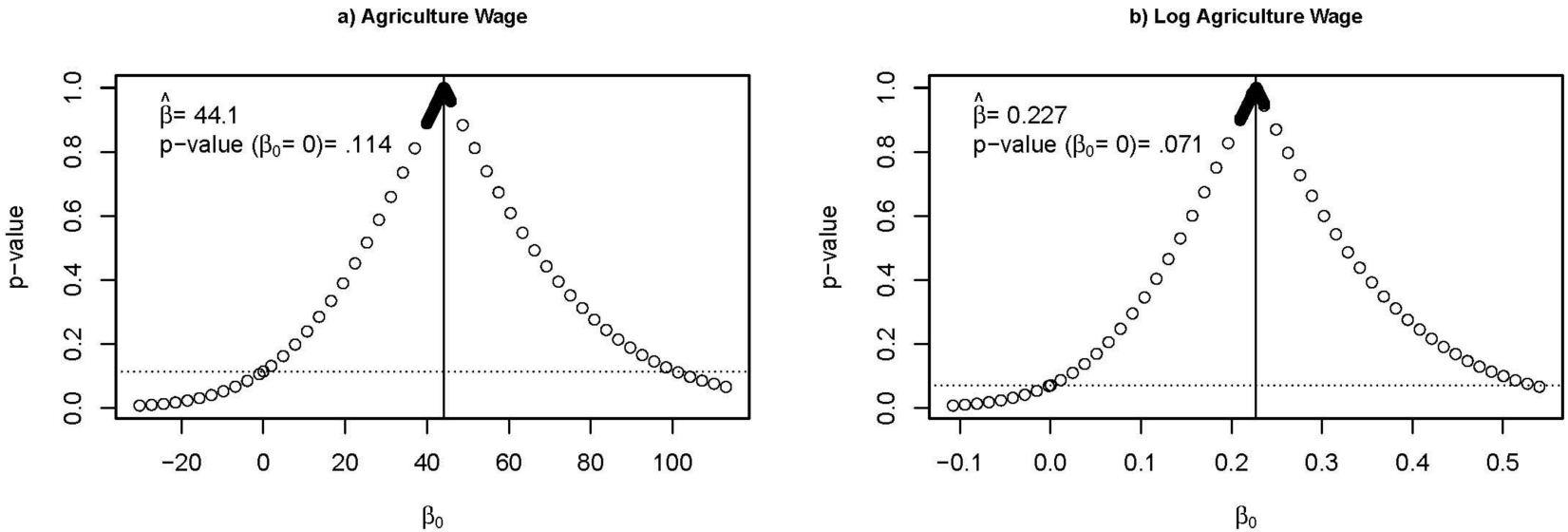
VARIABLES	(1) Male wage for agricultural work	(2) Male wage for non-agricultural work	(3) Male wage for agricultural work (log)	(4) Male wage for non-agricultural work (log)	(5) Male wage for agricultural work	(6) Male wage for non-agricultural work	(7) Male wage for agricultural work (log)	(8) Male wage for non-agricultural work (log)
Share of eligible villagers who migrated in 2015-2016	41.36*	2.682	0.216**	0.0647	28.75	-7.485	0.153*	0.0153
	(23.87)	(31.70)	(0.108)	(0.134)	(19.94)	(24.65)	(0.0889)	(0.103)
Observations	338	247	338	247	385	276	385	276
R-squared	0.518	0.259	0.503	0.260	0.557	0.260	0.547	0.265
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
1st-Stage	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity
Sample	117 villages	117 villages	117 villages	117 villages	133 villages	133 villages	133 villages	133 villages
First Stage Partial R^2	0.463	0.377	0.463	0.377	0.525	0.437	0.525	0.437
First Stage F-test Statistic	57.60	31.67	57.60	31.67	83.32	43.11	83.32	43.11
First Stage p-value	1.04e-10	4.70e-07	1.04e-10	4.70e-07	0	8.52e-09	0	8.52e-09

Standard errors clustered at the village level reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses data from the employer survey which interviewed agricultural and non-agricultural employers across all villages in the sample, and asked about wages paid during the period of out-migration. The survey asked separately about male and female wages, and about agricultural and non-agricultural wages.

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): origin labour market wages ↑

Figure 3: Randomization Inference P-Values for the Effect of Emigration on Agricultural Wages (117 Village Sample)



Figures show p-values produced via the randomization inference procedure outline in Appendix 2 using the partial sample of 117 villages. The x-axis, β_0, is the assumed effect of emigration on the (log) agricultural wage, analogous to the coefficient estimates in Table 10. The solid vertical line in each panel marks the point estimate, $\hat{\beta}$. The dotted horizontal line marks the p-value for the null hypothesis H_0: $\beta_0 = 0$.

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): origin labour market profits ↓

Table 15. LATE (IV) Estimates of the Effect of Emigration on Employer Costs, Revenues and Profits

VARIABLES	(1) Wage bill per decimal (Aman 2015)	(2) Non-wage costs per decimal (Aman 2015)	(3) Costs per decimal (Aman 2015)	(4) Change in Costs per decimal from 2013 to 2015	(5) Revenues per decimal Aman 2015 (current)	(6) Change in Revenues per decimal from 2013 to 2015	(7) Profits per decimal Aman 2015 (current)	(8) Change in Profits per decimal from 2013 to 2015
Share of eligible villagers who migrated in 2015-2016	81.41	64.17	145.6	224.1**	-163.1	-83.04	-254.9**	-19.55
	(79.42)	(108.0)	(174.6)	(103.3)	(232.4)	(119.4)	(124.8)	(72.23)
Observations	626	626	626	626	626	626	626	626
R-squared	0.108	0.095	0.108	0.030	0.119	0.063	0.086	0.040
Control Mean	149.011	139.553	288.564	-25.507	367.521	1.68	83.361	-23.578
Control Median	122.125	103.634	232.33	4.962	254.545	-10.714	45.454	-18.399
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES
First Stage Partial R^2	0.305	0.305	0.305	0.305	0.305	0.305	0.305	0.305
First Stage F-test Statistic	24.56	24.56	24.56	24.56	24.56	24.56	24.56	24.56
First Stage p-value	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The IV results in these tables were generated with the 2016 Follow-up Employer Survey, combined with 2015 migration rates per village derived from the 2016 Follow-up Household Survey. Analysis is conducted at the village level. All money-related variables are measured in taka.

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

Akram et al. (2018): origin food prices ↗

Table 16. LATE (IV) Estimates of the Effect of Emigration on Local Food Prices

VARIABLES	(1) log Rice (kg)	(2) log Flour (kg)	(3) log Pulses (kg)	(4) log Edible oil (liter)	(5) log Fish (kg)	(6) log Meat (kg)	(7) log Egg (per egg)	(8) log Milk (liter)	(9) log Salt (kg)	(10) log Sugar (kg)	(11) log Beverages	(12) log Prepared Food	(13) log Laspeyres index for 12 goods
Share of eligible villagers who migrated in 2014-2015	-0.0085	-0.0277	-0.0022	0.0321*	0.147*	0.0430	-0.0278	-0.0250	-0.0066	0.0121	-0.146*	-0.0459	0.0884**
	(0.00855)	(0.0171)	(0.0205)	(0.0190)	(0.0827)	(0.0456)	(0.0301)	(0.0324)	(0.0269)	(0.00977)	(0.0779)	(0.0565)	(0.0450)
Observations	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375
R-squared	0.667	0.267	0.594	0.376	0.749	0.267	0.890	0.502	0.396	0.256	0.930	0.954	0.756
Mean	3.452	3.505	4.613	4.704	5.299	4.725	2.107	3.669	2.368	3.838	1.415	2.288	4.175
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
Firststage_R2partial	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398
Firststage_Ftest	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12
Firststage_Pvalue	0	0	0	0	0	0	0	0	0	0	0	0	0

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using shopkeeper (grocery store) level data from the high frequency survey which interviewed them 6 times between 22nd December 2014 to 28th February 2015. The dependent variable in each specification is the price per unit of a given item of food in the local village market, measured in logs. The dependent variable in column (13) is log of the Laspeyres index of the preceding 12 items, defined as:

Source: Akram, A. A., Chowdhury, S., & Mobarak, A. M. (2018). Effects of emigration on rural labor markets. *Yale University mimeo*.

A rural poverty trap?

- cross-sectional data show that wages much higher in urban areas than in rural areas (see Lecture 1)
- Bryan et al. (2014) find that subsidies for seasonal migration raise income and consumption of migrants
- Akram et al. (2018) highlight that:
  - origin labour market conditions also improve from migration
  - migrants strategic complements, implies high risk of migration

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$\rightarrow$ are many rural workers stuck in poverty traps?

- credit constraints prevent adoption of profitable technology
- high income risk prevents reaping of spatial wage arbitrage

$\rightarrow$ estimate model that nests experimental results, featuring:

- credit constraints (that expose households to shocks)
- seasonal migration as insurance motif

Lagakos, D., Mobarak, A. M., & Waugh, M. (forth.). The welfare effects of encouraging rural-urban migration. *Econometrica*.

Lagakos et al. (2022): welfare effects of insurance via bus

Table 8: Welfare Effects of One-Time Migration Subsidies

		Migration Subsidy Migration Endogenous		Migration Subsidy Migration Policy Fixed		Unconditional Transfer Migration Endogenous	
		Welfare	Migr. Rate	Welfare	Migr. Rate	Welfare	Migr. Rate
Income Quintile	1	1.17	85	0.77	48	1.05	45
	2	0.45	63	0.31	38	0.56	37
	3	0.29	52	0.20	34	0.40	33
	4	0.20	46	0.15	31	0.32	31
	5	0.12	40	0.10	31	0.20	31
Average							
Rural & Low Assets		0.44	57	0.30	36	0.51	35
All Rural		0.22	41	0.15	31	0.25	30

Note: The first two columns report the lifetime consumption-equivalent welfare gains and migration rates for rural assets with low assets from one-time conditional migration subsidies. The next two columns report the same when the migration policies are held fixed for every agent. The final two columns report the welfare gains and migration rates from a one-time unconditional transfer costing the same total amount as the migration subsides. The rows are for different income quintiles of the rural households eligible for the subsidy, with 1 being the poorest and 5 being the richest. All three experiments are in partial equilibrium, meaning that the rural wage is held fixed, and without financing the subsidies in equilibrium.

Source: Lagakos, D., Mobarak, A. M., & Waugh, M. (forth.). The welfare effects of encouraging rural-urban migration. *Econometrica*.

Lagakos et al. (2022): permanent vs temporary subsidies

Table 9: Welfare Effects of Permanent Migration Subsidies

	Migration Fixed No Taxation	Migration Fixed Tax Financed	Migration Endogenous Tax Financed (G.E.)
Rural & Low Assets	2.06	1.62	2.26
All Rural	1.62	1.19	1.85
All Urban	0.15	-0.29	-1.26
All Households	1.03	0.59	0.80
% in Rural Area	60	60	66
% of Rural Seasonally Migrating	31	31	56
% of Rural with Low Assets	50	50	74
Tax Rate (% of labor income)	0	0.4	1.3

Note: The first column reports the effects of permanently offering conditional migration subsidies to rural households with sufficiently low assets, as in the migration experiments, but with migration policies held fixed and without any taxation to pay for the transfers. The second column is the same, but the migration subsidies are financed through labor taxation. The third column allows migration to be endogenous and finances the transfers through labor taxation.

Source: Lagakos, D., Mobarak, A. M., & Waugh, M. (forth.). The welfare effects of encouraging rural-urban migration. *Econometrica*.

Lagakos et al. (2022): experiment-consistent model

- alternative model that fits data & experimental results better
- ↯ different interpretation: shock insurance, not poverty trap

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- however, similar welfare gains from one-off unconditional transfer at same cost, albeit worse for the poorest

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- welfare gains of one-off migration high for the poorest HHs
- however, similar welfare gains from one-off unconditional transfer at same cost, albeit worse for the poorest

→ poorest HHs not benefiting from migration *per se*, but from targeting to needy HHs willing to undergo ordeal of migration

→ similar for permanent migration subsidies: most of welfare gains arise from targeting resources to vulnerable rural HHs

→ not relaxing credit constraints for those stuck in rural areas, but providing better insurance

Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Meghir et al. (2022): risk sharing and migration

- faced with shocks, informal risk sharing and migration may be alternative, imperfect responses to same problem

$\rightarrow$ interested in interaction between risk sharing and migration

$\lightning$ require full village network to track risk sharing

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Meghir et al. (2022) pursue three objectives:

1. estimate causal effect of migration subsidies on risk sharing, exploiting Bryan et al.'s (2014) RCT
2. model endogenous risk sharing & endogenous migration in GE
3. estimate model with experimental variation, quantify welfare effect of temporary vs permanent subsidies vs UCTs

Meghir et al. (2022): informal insurance ↑ with migration

	Treatment effect	Control mean
Willingness to help		
Community member would help you	0.030	0.85
	(0.020)	
... and you would ask for help	0.025	0.83
	(0.020)	
Community member would ask you for help	0.109***	0.57
	(0.033)	
... and you would help them	0.109***	0.53
	(0.032)	
Actual transfers		
Receive any transfer from community member	-0.024	0.57
	(0.022)	
Amount, if any transfer received (Tk)	1821***	4808
	(678)	
Give any transfer to community member	0.036**	0.15
	(0.018)	
Amount, if any transfer given (Tk)	1310**	2001
	(558)	

TABLE 2: Treatment effect on transfers within the community

Note: The sample includes households from the 2011 survey. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described. Each regression also controls for upazila (county). Standard errors, clustered by village, are in parentheses, and the mean of the control group is in square brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Source: Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Meghir et al. (2022): income–consumption link weaker

	Round 4				Diff in Diff	
	(1)	(2)	(3)	(4)	(5)	(6)
Log income (round 4)	0.157***	0.169***	0.130***	0.140***	0.112**	0.109**
	(0.027)	(0.028)	(0.028)	(0.029)	(0.054)	(0.046)
Treatment effect on log income	-0.073***	-0.066**	-0.072***	-0.061**	-0.077	-0.099**
	(0.027)	(0.027)	(0.027)	(0.026)	(0.061)	(0.046)
Village-round FE	X	X	X	X	X	X
Household FE					X	X
Household head controls		X		X		
Resource controls			X	X		
Includes baseline					X	X
Includes 2013						X
Observations	1857	1857	1857	1857	2166	4371
R squared	0.186	0.221	0.217	0.267	0.791	0.721

TABLE 4: Effect of migration incentives on the exposure of consumption to income

Source: Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Meghir et al. (2022): model setup

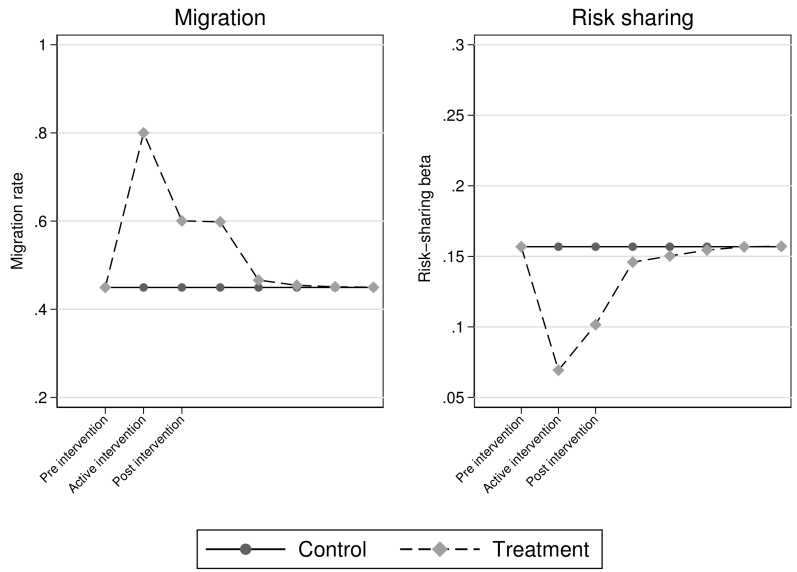
Model period	Activities	State of the world
Start of period t —	Start-of-period job contacts (A_t)	$h_t = \{s_t, A_t\}$
Before migration —	Observe village state (s_t)	
	Migration decision (j_t)	
After migration —	Exogenous creation of job contacts ($\hat{A}_t$)	$\hat{h}_t = \{h_t, j_t, q_t, \hat{A}_t(h_t, j_t)\}$
	Observe migration state (q_t)	
	Make risk-sharing transfers	
	Consume	
	Return to village	
	Exogenous separation of job contacts (A_{t+1})	
Start of period $t+1$ —	Start-of-period job contacts (A_{t+1})	$h_{t+1} = \{s_{t+1}, A_{t+1}\}$
	Observe village state (s_{t+1})	

Figure 1: Model timeline

Source: Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Meghir et al. (2022): migration as risk-sharing trade-off

(a) Combined effect



(b) Financial component

(c) Utility component

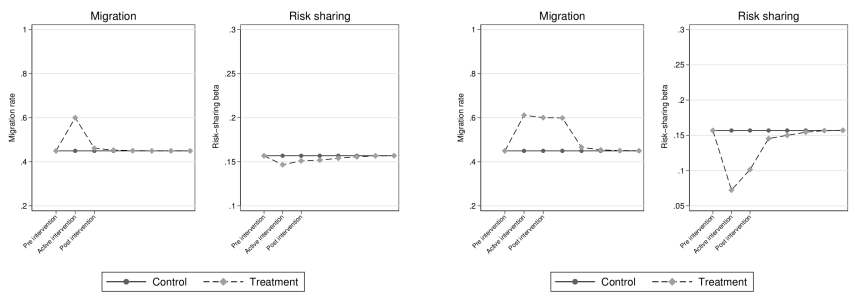
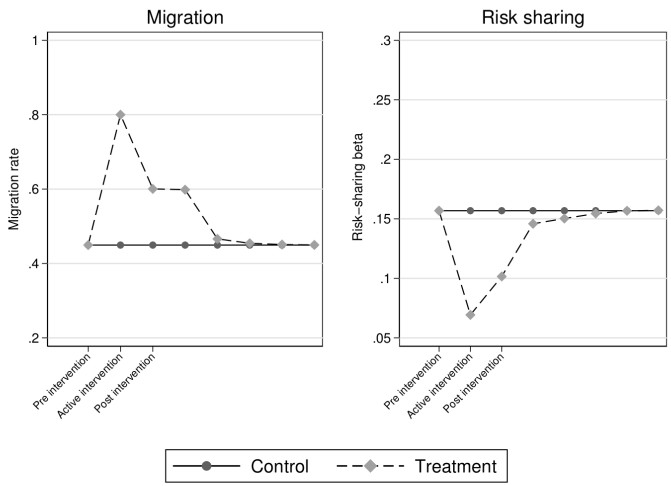


Figure 3: Effect of the experiment on migration and risk sharing

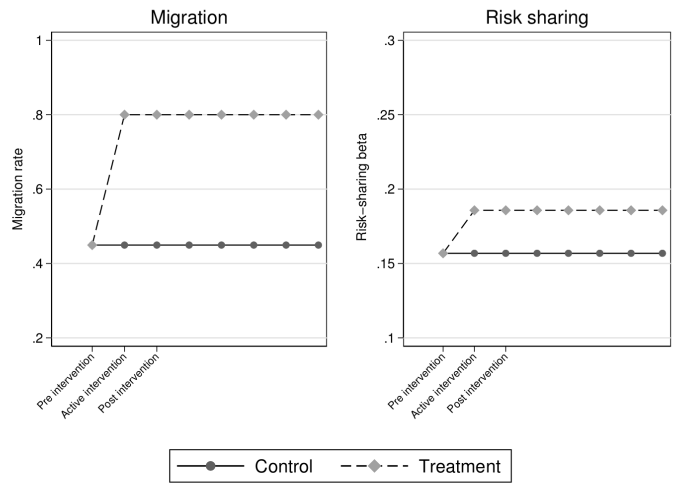
Source: Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Meghir et al. (2022): temporary vs permanent subsidies

(a) Temporary subsidy



(b) Permanent subsidy



Note: Shock is both financial and utility shock.

Figure 4: Temporary vs permanent subsidy

Source: Meghir, C., Mobarak, A. M., Mommaerts, C., & Morten, M. (2022). Migration and informal insurance: Evidence from a randomized controlled trial and a structural model. *Review of Economic Studies*, *89*(1), 452–480.

Mobarak, A. M. (2022). Assessing social aid: The scale-up process needs evidence, too. *Nature*, *609*, 892–894.

Mobarak (2022): scaling lessons learned (I)

We scaled up the programme in stages, each time expanding the observations we made: these included risk of divorce, changes in prices of goods and the costs of family separation. These data helped us to capture the unintended consequences of more migrants leaving their villages and entering urban labour markets.

Results continued to look promising, and a large microcredit organization in Bangladesh received philanthropic support to offer seasonal-migration loans to hundreds of thousands of households. But the outcome was disappointing – subsidies mainly reached those who would have migrated anyway, and the programme was promptly discontinued. (Mobarak (2022), pp. 892)

Mobarak (2022): scaling lessons learned (II)

When programmes enter a 'scaling stage', the focus often immediately shifts to solving the practical issues of broader implementation of the programme (such as how to teach government staff about an innovation, distribute subsidies to tens of thousands of people, instead of hundreds, or integrate a programme across government systems).

All that work, although essential, overlooks the crucial question of whether exciting pilot results still hold. Many – if not most – development programmes encounter uncertainties and complexities that emerge only at scale. These are rarely observed – and therefore cannot be analysed – during the initial pilots. Simply repeating interventions on the same scale at multiple locales is not enough. (Mobarak (2022), pp. 892)

Mobarak (2022): scaling lessons learned (III)

1. consider effects beyond those reaching direct beneficiaries: spillovers, feedback loops and GE effects

2. pay attention to broader social changes beyond the outcome that the original programme targeted

3. anticipate political and operational risks as new players get involved with a programme

4. scale up in reasonable increments

5. expand methodologies to track the full range of welfare effects

*** Egger, D., Haushofer, J., Miguel, E., Niehaus, P., & Walker, M. (2022). General equilibrium effects of cash transfers: Experimental evidence from Kenya. *Econometrica*, *90*(6), 2603–2643.

General equilibrium effects of cash transfers: Experimental evidence from Kenya

Dennis Egger[1] Johannes Haushofer[2] Edward Miguel[1]
Paul Niehaus[3] Michael Walker[1]

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[2]Princeton University & IFN

[3]University of California San Diego

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Intellectual context: macroeconomics

- Tracing out the pattern of transactions in an integrated economy and their contributions to aggregates such as overall output or well-being has long been a fundamental task of economic analysis
    - E.g. effects of fiscal stimulus, including Keynes (1936) and more recently Chodorow-Reich (2019), Nakamura and Steinsson (2014), Farhi and Werning (2016), Auerbach et al (2019), Corbi et al (2019)
- These issues generally have not, however, been subjected to experimental examination

Intellectual context: development

- There is also renewed interest in behavioral responses to cash transfers with the rise of large-scale government programs
    - Cash transfers make up the majority of social safety net spending (World Bank 2018)
    - A large literature documenting effects among recipients on a broad range of behavioral responses, including consumption, earnings, assets, food security, child growth and schooling, self-reported health, female empowerment, and psychological well-being
    - Generally no evidence of spending on "temptation goods", e.g., alcohol (Evans & Popova 2017) or reductions in work effort
- Yet we know much less about the aggregate consequences, even though cash transfers seem quite likely to have broader effects
    - Because cash functions as a medium of exchange, $1 a recipient uses to transact will mechanically show up on someone else's balance sheet
    - In a few cases, experimentation at larger scales finds meaningful effects (Angelucci & di Giorgi, 2009; Cunha et al, 2018; Filmer et al, 2018)

This project

- We aim to unite these two literatures, bringing experimental methods to the study of aggregate economic issues

- In particular, we evaluate a large-scale cash transfer experiment in rural Kenya. Four methodological advances:

  1. **A large influx of cash:** $11M, or 25% of annual GDP in treated areas delivered over 24 months, and 17% over the peak 12 months
  2. **Randomization across large units** generating spatial variation in the intensity of exposure both at and above the village level
  3. Unusually **extensive measurement** of outcomes for both recipients and non-recipients, nearby enterprises and markets, local government, etc., including high-frequency consumer goods prices. Census 65,385 households (with nearly 300,000 individuals), 12,095 non-farm enterprises
  4. A **simple theoretical framework** to organize results and interpret implications for welfare

Tracing out the flow of funds

1. Substantial expenditure increases for both recipient (+13%) and non-recipient (+13%) households
2. Quantitatively similar increases in sales at local enterprises
3. Increased earnings for non-recipients driven primarily by labor earnings, mirroring higher enterprise wage bills; no change in reported total hours worked
4. Small changes (+0.1-0.2%) in final goods prices, concentrated in more remote communities; some evidence of increased prices of non-tradeable inputs (labor, land) but not of capital

Aggregate implications

1. Estimate a local transfer multiplier of 2.5 using *either* expenditure or income data
    - Contrast to recent US local fiscal multiplier estimates (range 1.5-2.0)
    - Consistent with *marginal propensity to spend locally* of $\approx$ 0.7 - 0.75
    - Increase in real output without substantial increase in employment of inputs suggest roles for local demand and factor under-utilization ("slack"), as opposed to constraints on investment
2. Interpreted through the lens of our framework, the results suggest welfare gains for non-recipients, driven by two forces
    - Expansions in household's real budget sets, not (or not solely) driven by increased labor supply
    - Non-market effects (externalities) are mostly null or positive, both between and within households (e.g., public goods, domestic violence)

Agenda

1. Context, design, and empirical specifications
2. Empirical results
   - Tracing out the flow of funds
   - Transfer multiplier
3. Welfare framework, externalities & interpretation
4. Discussion: production capacity utilization
5. Conclusion

Setting: rural western Kenya

653 villages in Siaya County

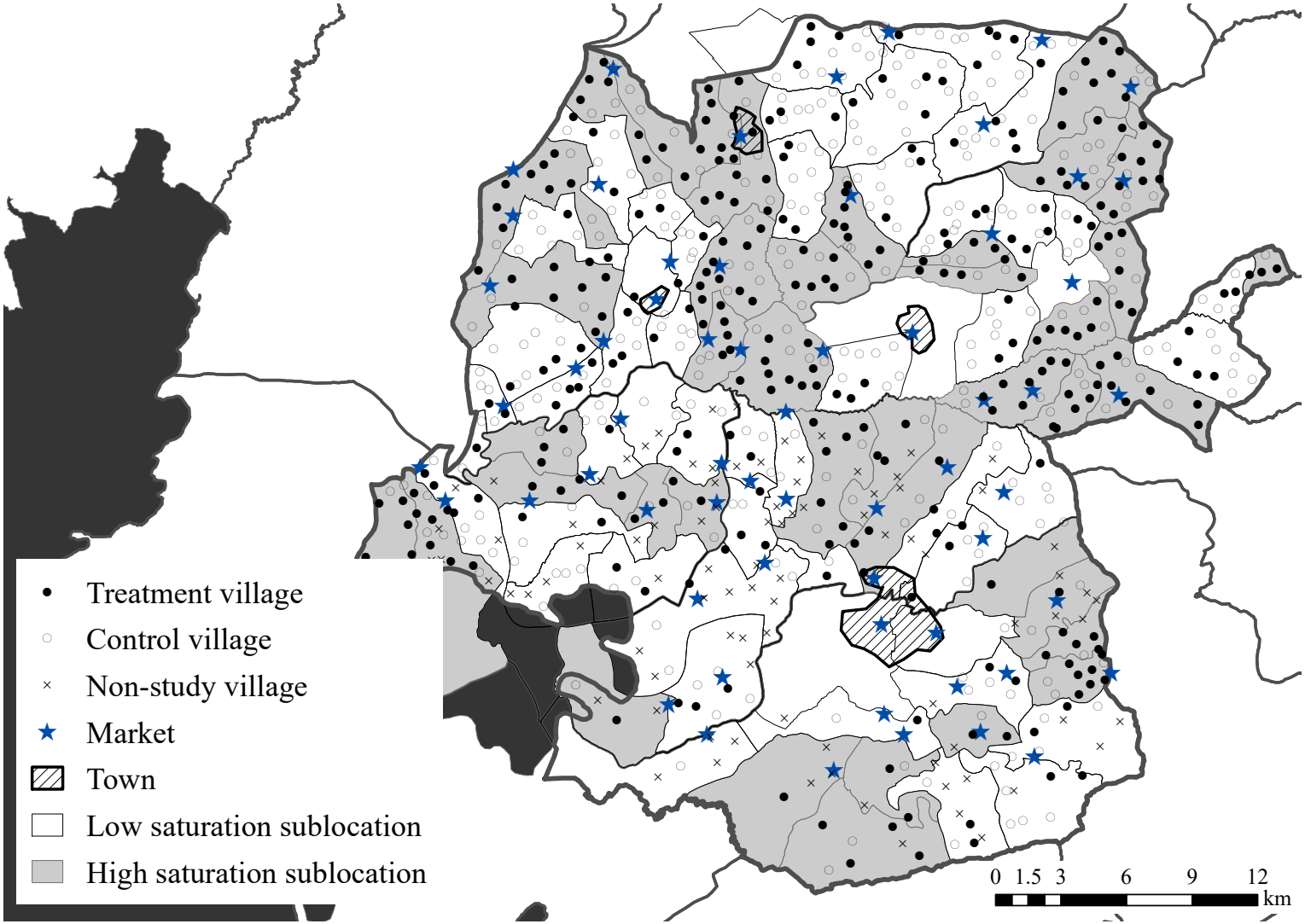
- ~100 households per village
- 4.4 household members and 2.3 children on avg
- 97% of HH's in agriculture, 45% in self-employment, and 60% in wage work
- Survey respondent mean age is 48 years, 6 years of schooling
- Steady economic growth, no national elections during study period (2014-17)

The GiveDirectly (GD) Program

GD distributes unconditional cash transfers as follows:

- Enrolls roughly the poorest 1/3 of households in each village using a simple proxy means test (here, having a grass-thatched roof)
- Coaches recipients to register for mobile money system (M-Pesa)
- Distributes payments via M-Pesa in 3 tranches over 8 months: a test payment, then two larger payments
- Transfer are large: USD 1,000 nominal / USD 1,871 PPP
  - Equivalent to 75% of mean annual HH expenditure $\Rightarrow$ $\sim$**17% of annual GDP** in treated areas during peak 12 months
  - Recipients typically withdraw the full amount and spend in cash

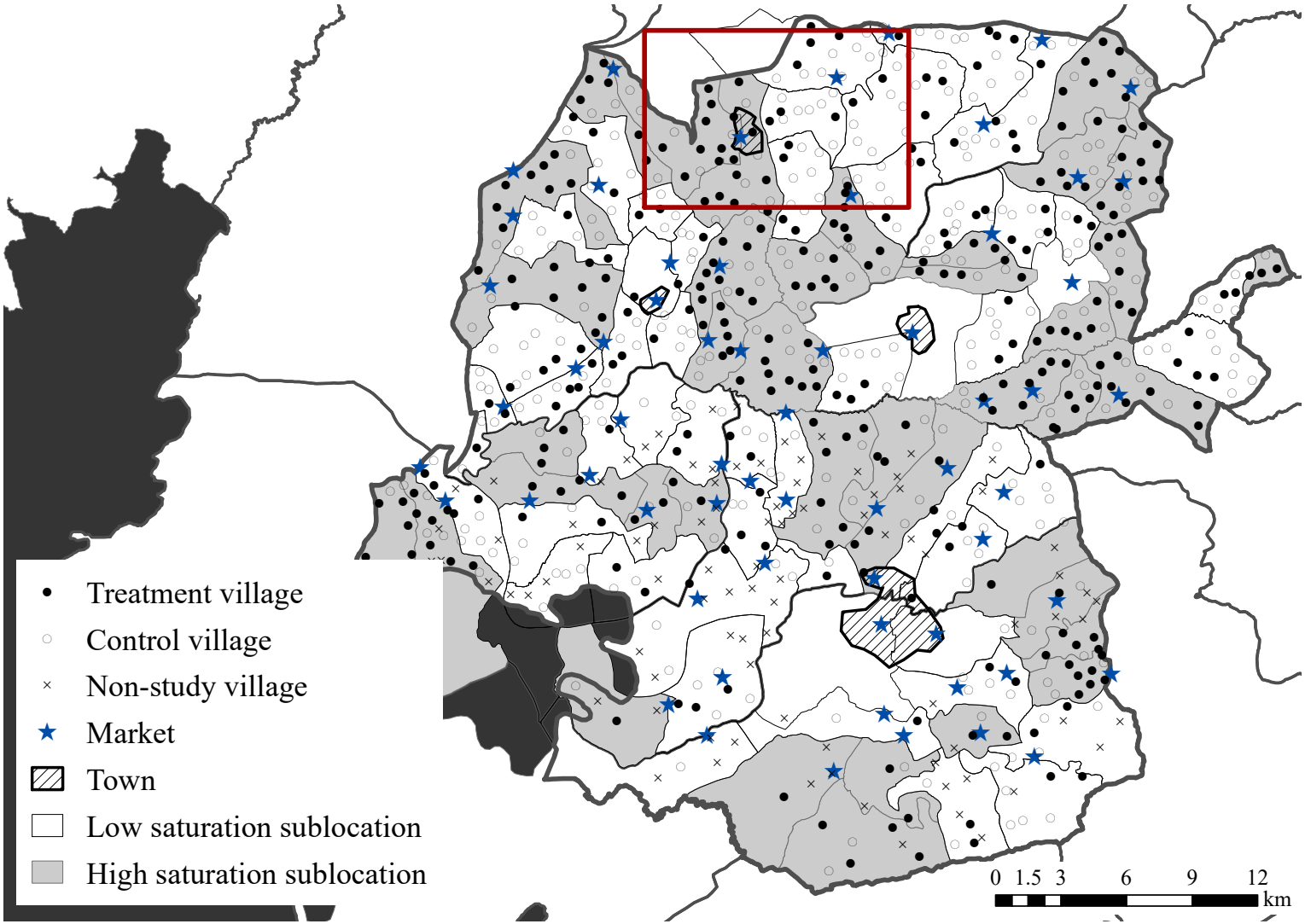
Spatial exposure to treatment (1)



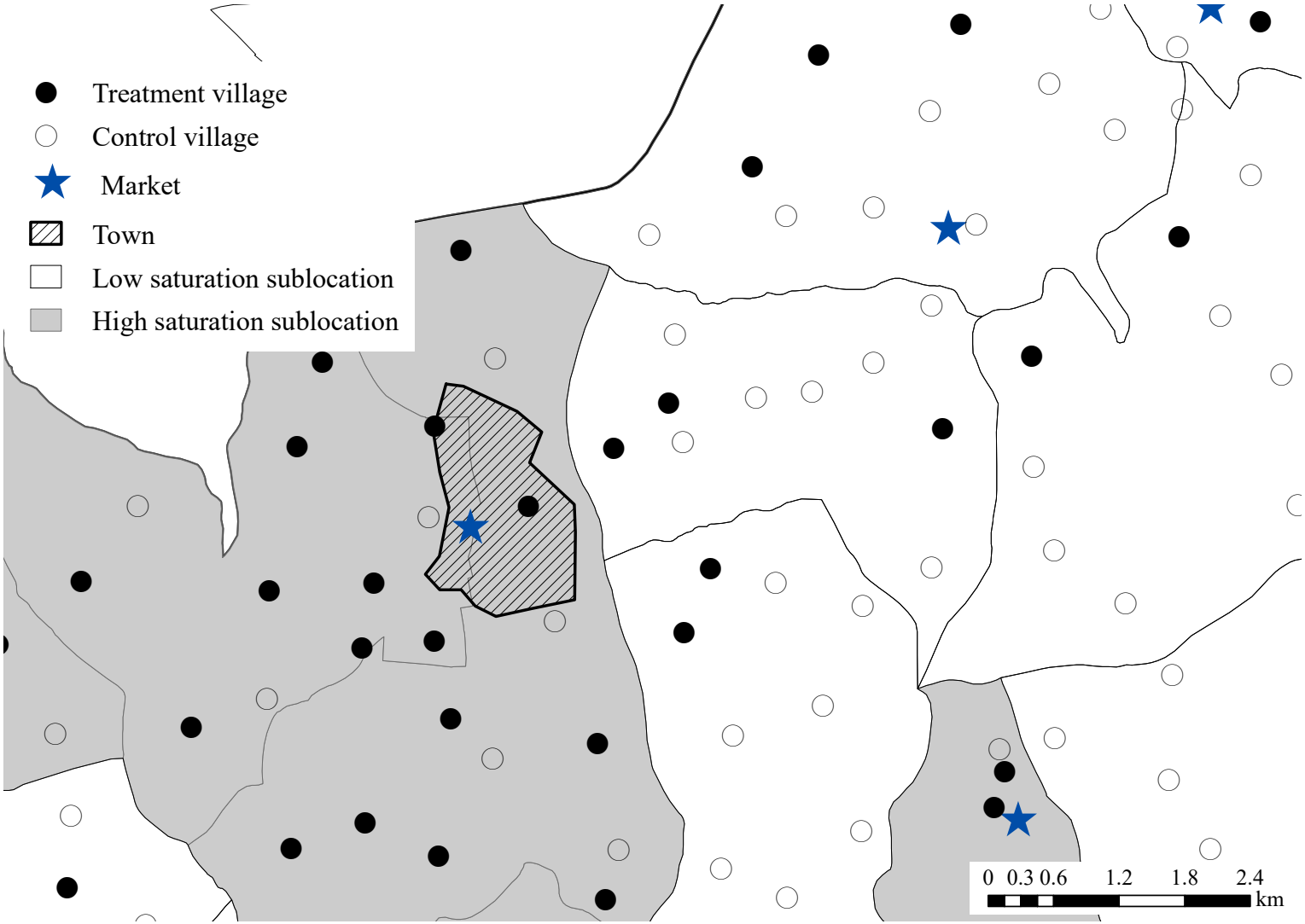
Densely populated area, with many proximate markets

Details Timing

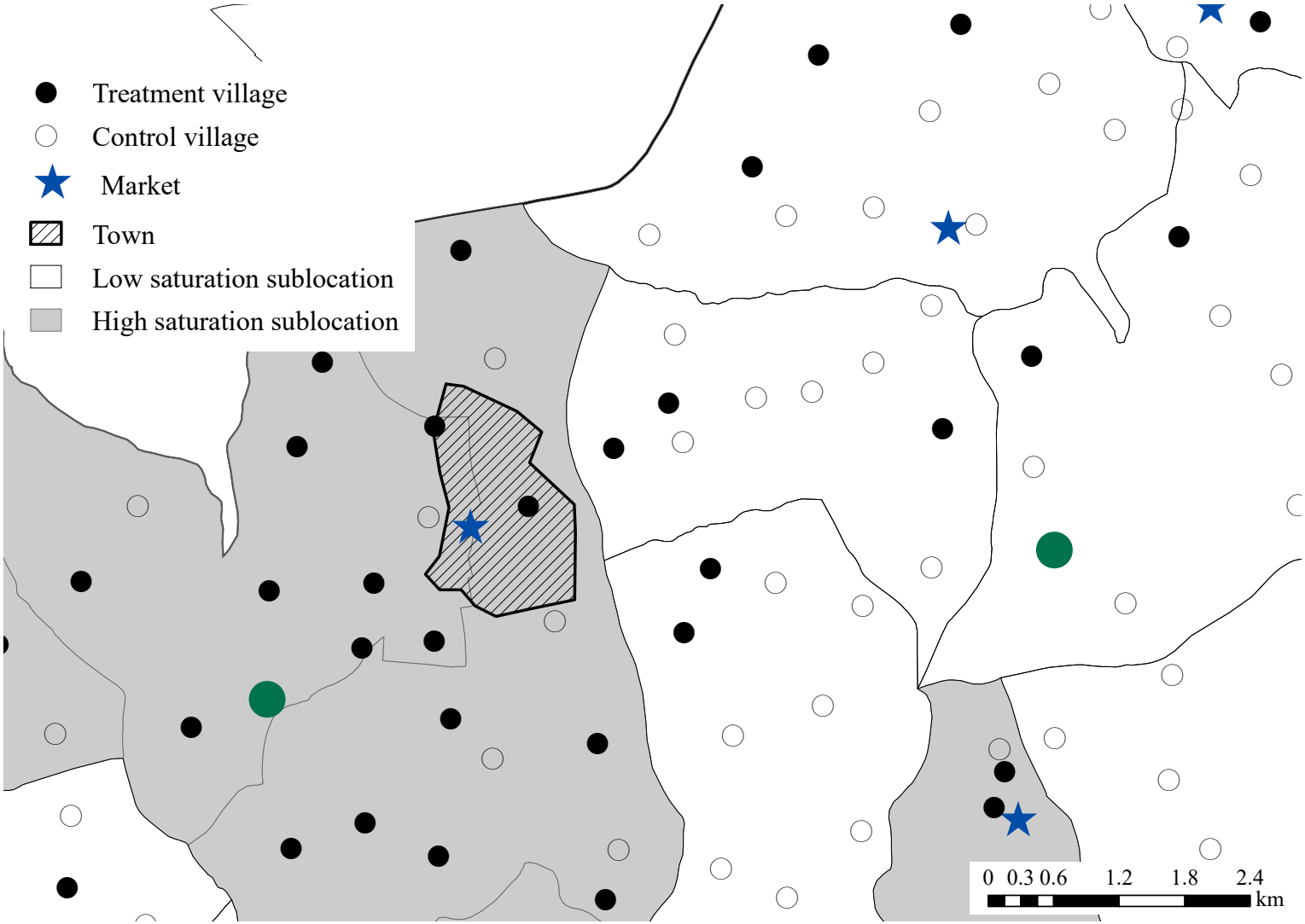
Spatial exposure to treatment (1)



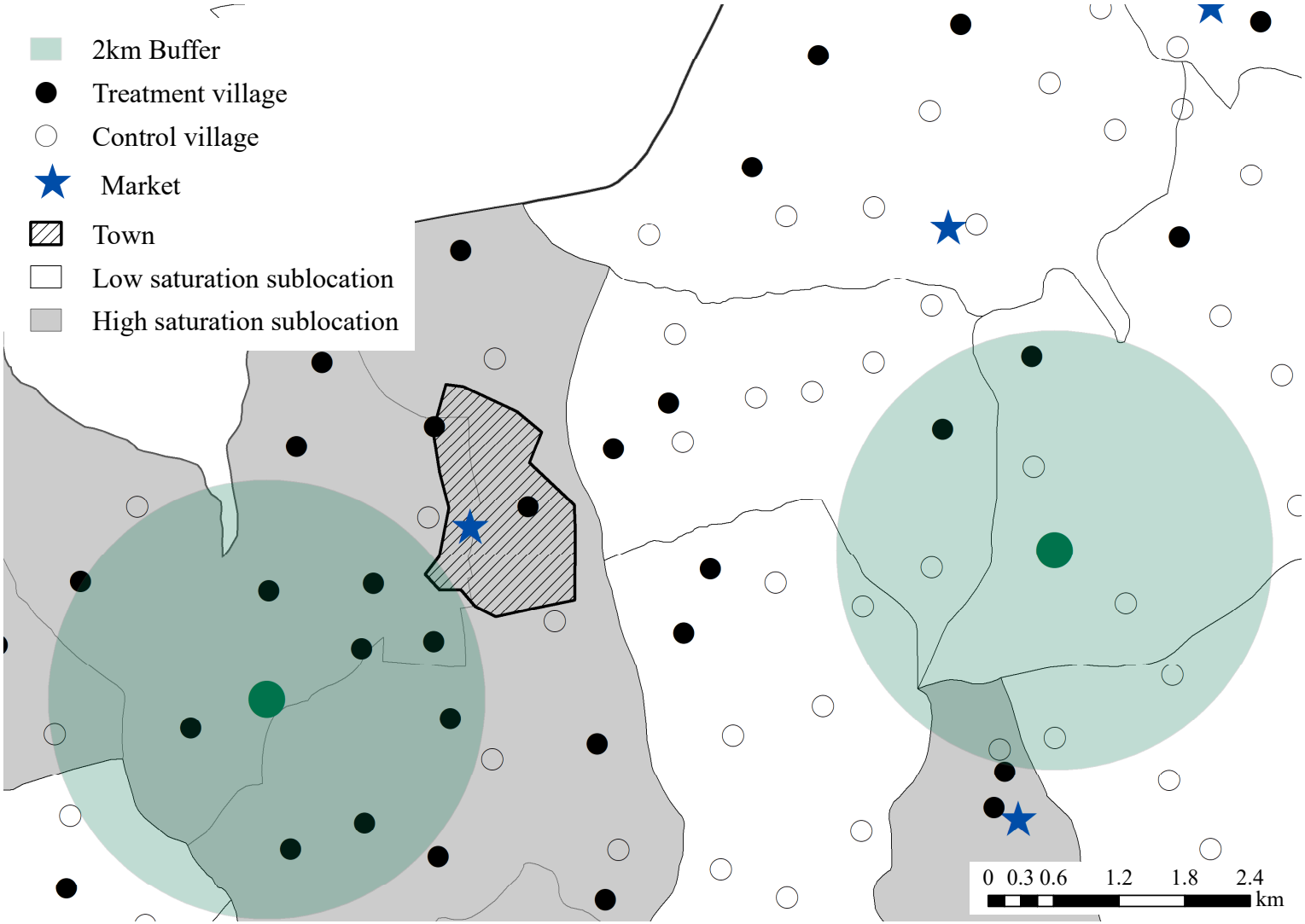
Spatial exposure to treatment (2)



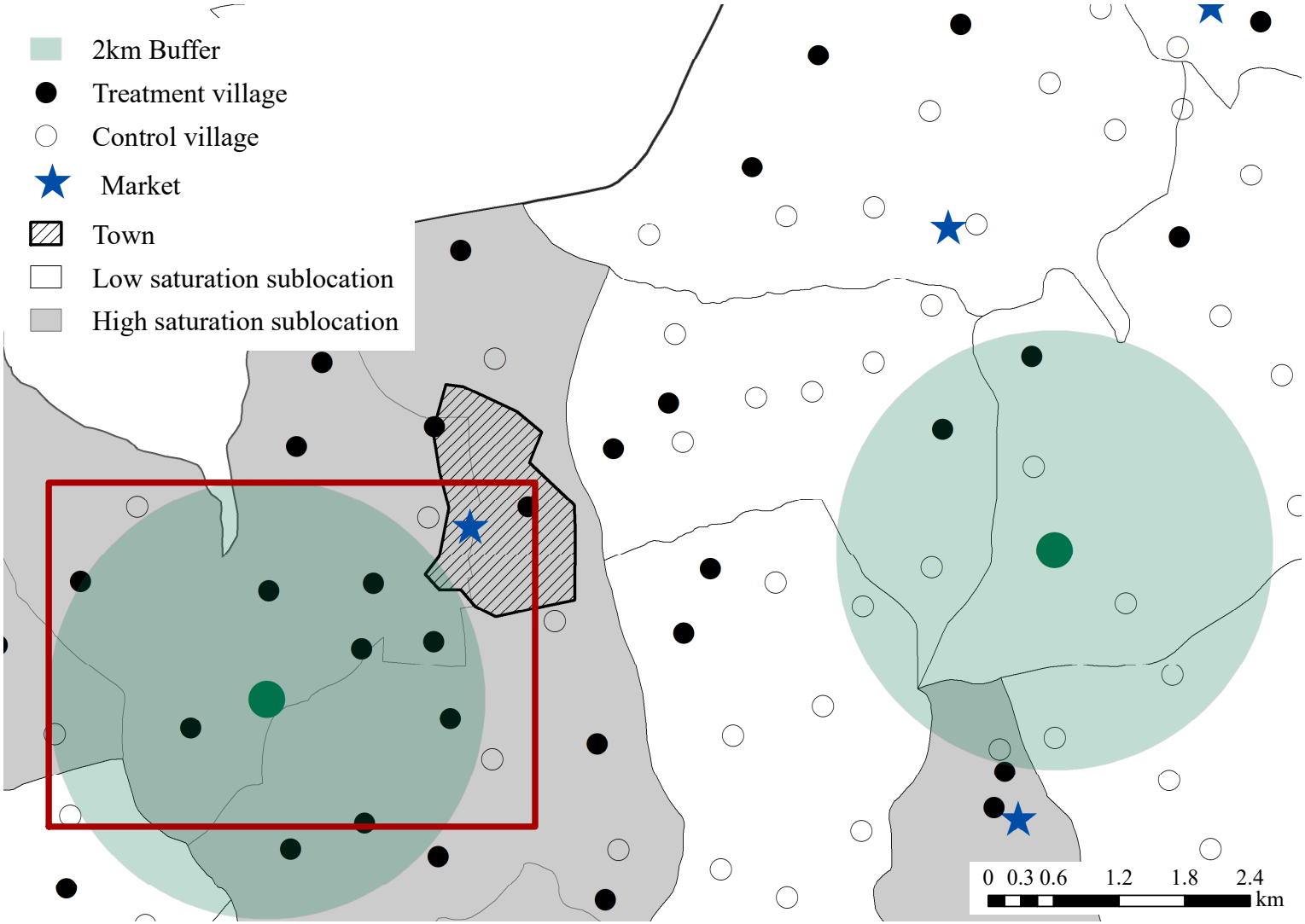
Spatial exposure to treatment (2)



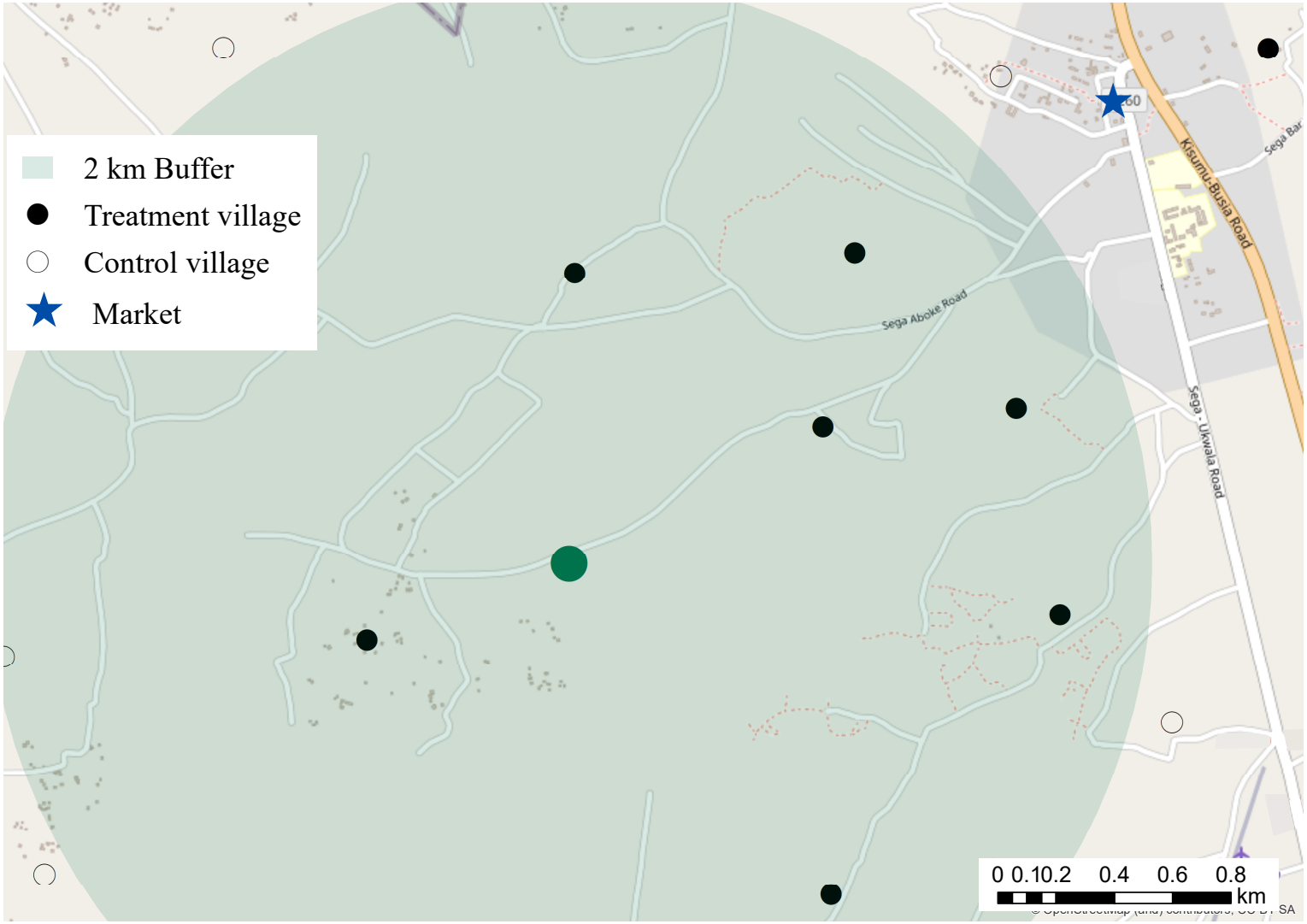
Spatial exposure to treatment (2)



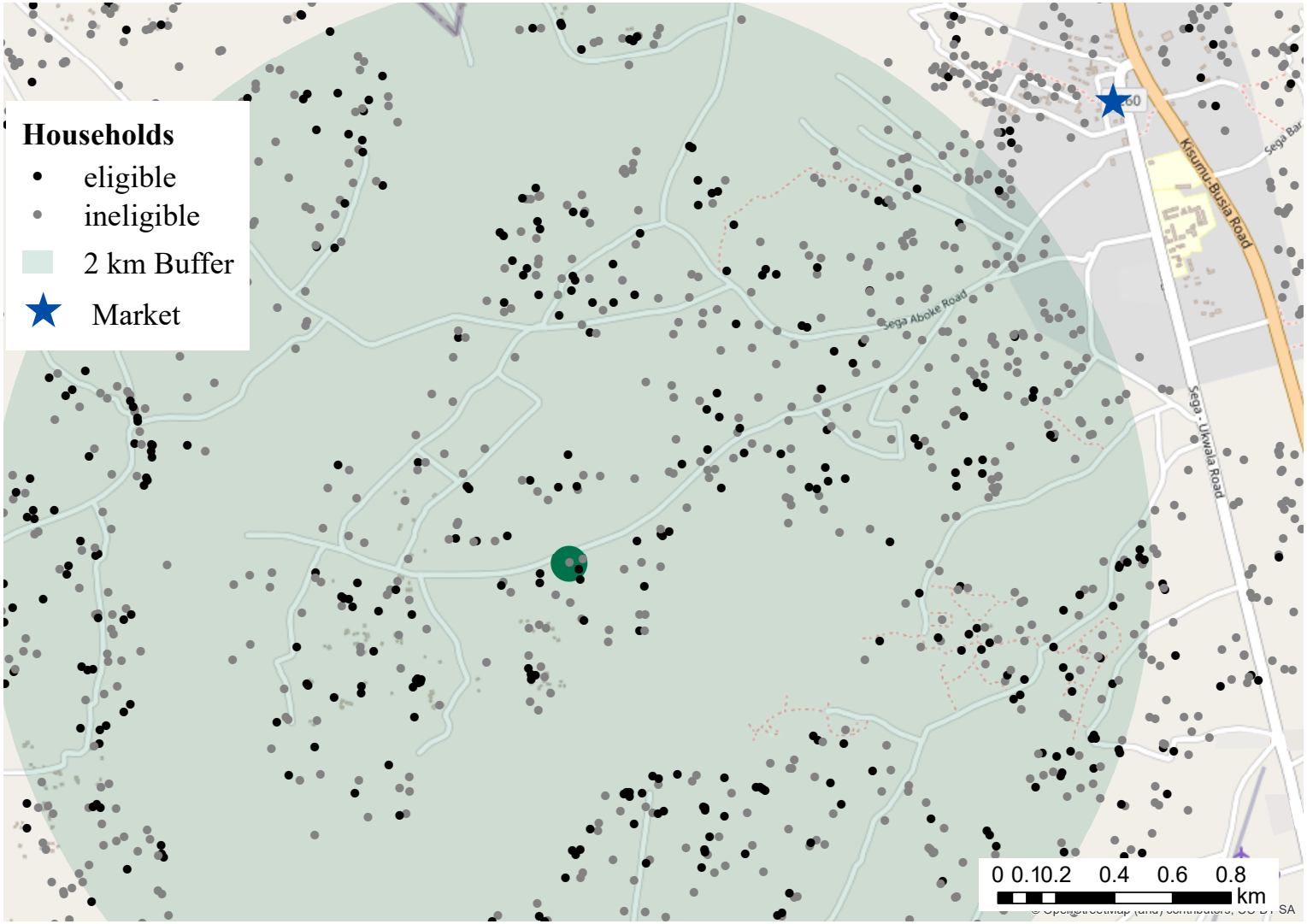
Spatial exposure to treatment (2)



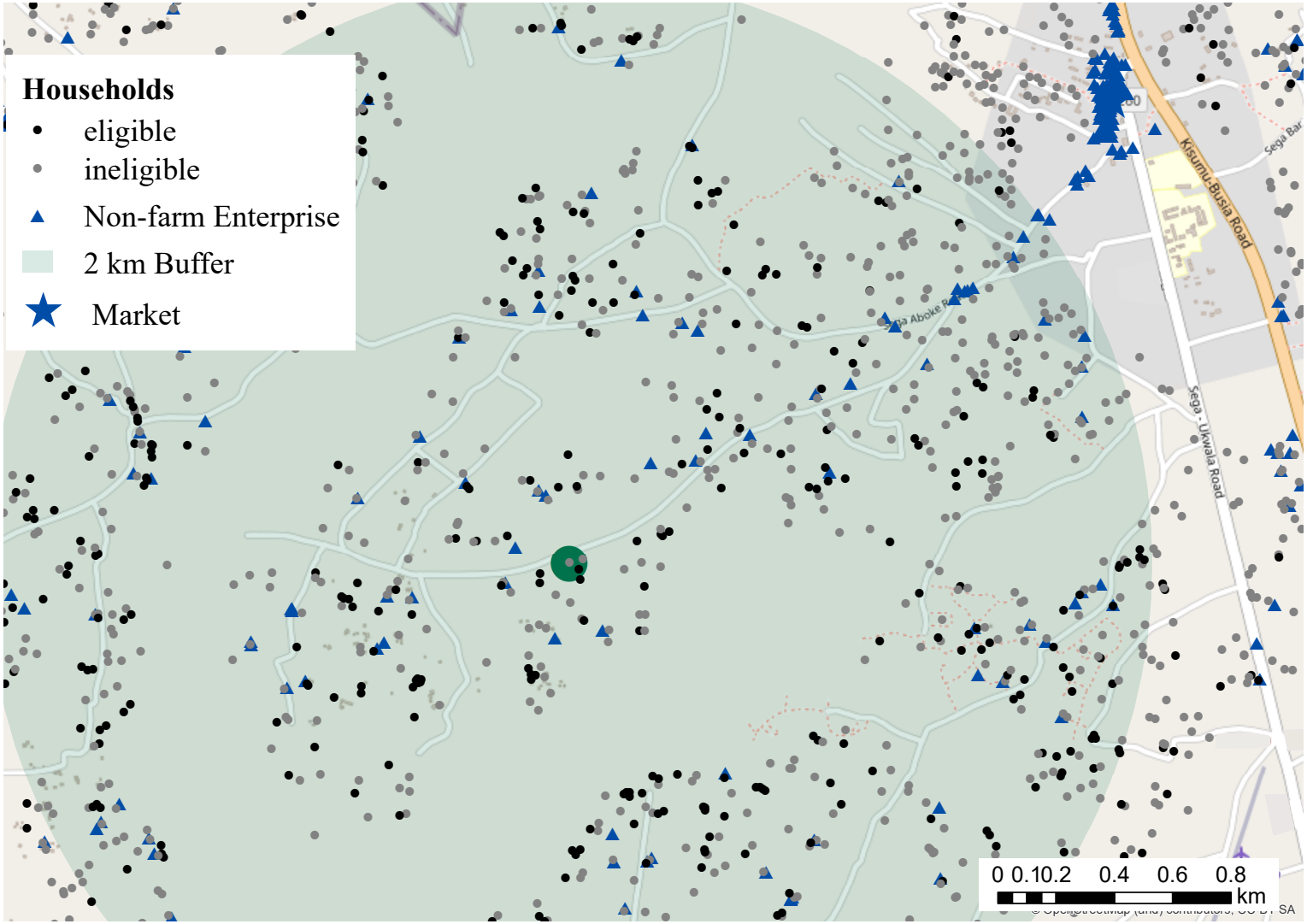
Spatial exposure to treatment (3)



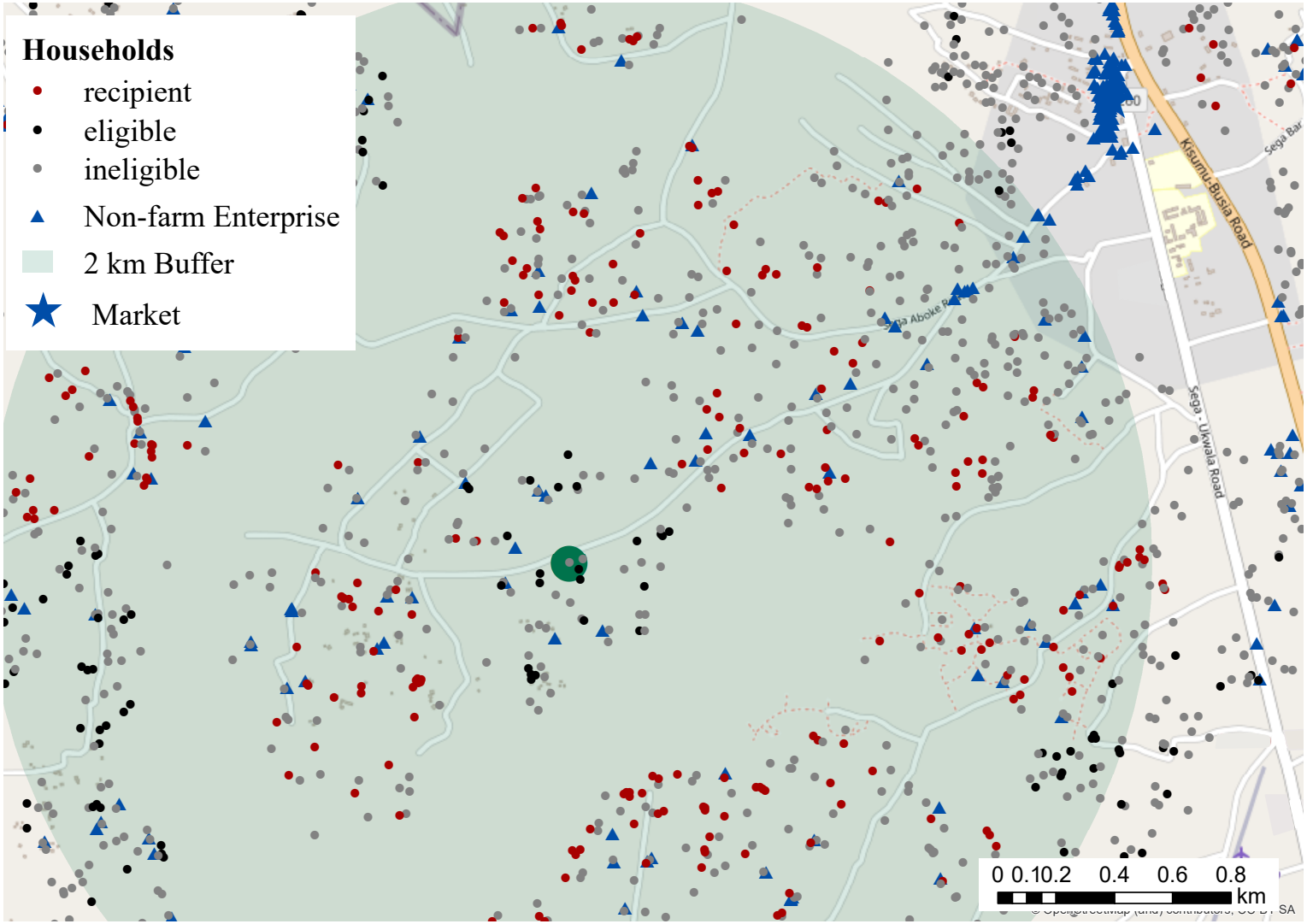
Spatial exposure to treatment (3)



Spatial exposure to treatment (3)



Spatial exposure to treatment (3)



Original field data sources

1. **Household surveys:** at endline, 8,200 households across 653 villages, surveyed 9-31 months after first transfer. 90% survey rate, no difference by treatment Tracking Balance Timeline
2. **Enterprise surveys:** from both household surveys (ag and non-ag self-employment modules) a distinct census and survey of 5 enterprises per village, (mostly) matched to owning households Balance
3. **Market price surveys:** 61 markets $\times$ 72 major commodities $\times$ 3 vendors $\times$ 30 months
4. (**Local government** official surveys)

Effects of interest

- We are primarily interested in total effects, i.e., comparing observations to a counterfactual with no intervention, and estimate:

  1. The **average total effect** on outcomes for treated and untreated households and firms, including
     - Direct effects (for households) of own (village) treatment
     - Neighborhood effects (for households and firms) of treatment intensity, which we estimate within 2 km bands (selected to minimize a Bayesian Information Criterion)
  2. The **reduced form (ITT) treatment effect** on treated households, as a benchmark that assumes no neighborhood effects
  3. Neighborhood effects on monthly prices, including (i) average effects and (ii) average effect in the month of maximum local transfers

- Report monetary values in PPP USD, with flow outcomes annualized unless otherwise reported and with enterprise outcomes normalized per household in that village (for comparability)

Example spatial specification

For household i in village v, we estimate

$$y_{iv} = \alpha + \beta Amt_v + \sum_{r=2}^{R} \beta_r Amt_{v,r}^{\neg v} + \varepsilon_{iv}$$

- Use the (cumulative) amount per capita transferred over course of the study to own village (Amt_v) and other villages in the r to $r-2$ km buffer ($Amt_{v,r}^{\neg v}$)
- Instrument respectively by $Treat_v$, and share $s_{\neg v,r}^{e,t}$ of eligible HH's in villages (other than v) assigned to treatment (by buffer)
- Report ATotE ($\hat{\beta} \cdot \overline{X}$) using mean transfer amount per village/buffer
- Two modifications depending on sample:
  1. Untreated households: use $Amt_{v,r}$, so spillovers work entirely through β_r
  2. Market prices: use amount distributed last quarter, add in month and market fixed effects (instead of instrumenting)
- Conley SE's (1999, 2008); randomization inference very similar.

Reduced form specification | Testing for linearity

Agenda

1. Context, design, and empirical specifications
2. Empirical results
   - Tracing out the flow of funds
   - Transfer multiplier
3. Welfare framework, externalities & interpretation
4. Discussion: production capacity utilization
5. Conclusion

Results Outline

1. Tracing out the flow of funds
    - **Recipient households**
    - Enterprises
    - Non-recipient households
    - Output & input prices
2. Transfer multiplier

Recipient HHs: expenditure, saving

	(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: Expenditure				
Household expenditure, annualized	293.59***	338.57***		2,536.01
	(60.11)	(109.38)		(1,933.51)
Non-durable expenditure, annualized	187.65***	227.20**		2,470.69
	(58.59)	(99.63)		(1,877.23)
Food expenditure, annualized	72.04*	133.84**		1,578.05
	(36.96)	(63.99)		(1,072.00)
Temptation goods expenditure, annualized	6.55	5.91		37.07
	(5.79)	(8.82)		(123.54)
Durable expenditure, annualized	95.09***	109.01***		59.41
	(12.64)	(20.24)		(230.83)
Panel B: Assets				
Assets (non-land, non-house), net borrowing	178.78***	183.38***		1,131.66
	(24.66)	(44.26)		(1,419.70)
Housing value	376.92***	477.29***		2,032.11
	(26.37)	(38.80)		(5,028.27)
Land value	51.28	158.47		5,030.03
	(186.22)	(260.91)		(6,604.66)

Recipient HHs: income

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	𝟙(Treat village)	Total Effect	Total Effect	Control, low saturation
	Reduced form	IV	IV	mean (SD)
Panel C: Household balance sheet				
Household income, annualized	79.43*	135.70		1,023.36
	(43.80)	(92.10)		(1,634.02)
Net value of household transfers received, annualized	−1.68	−7.43		130.08
	(6.81)	(13.06)		(263.65)
Tax paid, annualized	1.94	−0.09		16.92
	(1.28)	(2.02)		(36.50)
Profits (ag & non-ag), annualized	26.24	35.85		485.56
	(23.67)	(47.66)		(786.92)
Wage earnings, annualized	42.43	73.66		494.95
	(32.23)	(60.82)		(1,231.12)

Results Outline

1. Tracing out the flow of funds
    - Recipient households
    - **Enterprises**
    - Non-recipient households
    - Output & input prices
2. Transfer multiplier

Enterprise outcomes

	(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: All enterprises				
Enterprise profits, annualized	−2.27	55.77	35.08	156.79
	(21.42)	(36.73)	(37.36)	(292.84)
Enterprise revenue, annualized	−29.61	322.16**	237.16**	494.45
	(102.74)	(138.17)	(112.72)	(1,223.07)
Enterprise costs, annualized	−13.32	89.35**	73.08	117.22
	(28.63)	(38.51)	(46.77)	(263.46)
Enterprise wagebill, annualized	−15.90	75.99**	66.57*	97.35
	(25.49)	(30.64)	(35.86)	(237.01)
Enterprise profit margin	0.01	−0.11*	−0.12**	0.33
	(0.02)	(0.06)	(0.05)	(0.30)
Panel B: Non-agricultural enterprises				
Enterprise inventory	11.02	34.69***	16.90	50.41
	(9.14)	(13.39)	(10.66)	(131.86)
Enterprise investment, annualized	4.00	13.58	6.82	46.57
	(7.05)	(13.10)	(7.96)	(167.44)
Panel C: Village-level				
Number of enterprises	0.01	0.02	0.01	1.12
	(0.01)	(0.01)	(0.01)	(0.14)

Results Outline

1. Tracing out the flow of funds
   - Recipient households
   - Enterprises
   - **Non-recipient households**
   - Output & input prices
2. Transfer multiplier

Non-recipient HHs: income

	(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 C: Household balance sheet				
Household income, annualized	79.43*	135.70	224.96***	1,023.36
	(43.80)	(92.10)	(85.98)	(1,634.02)
Net value of household transfers received, annualized	−1.68	−7.43	8.85	130.08
	(6.81)	(13.06)	(19.11)	(263.65)
Tax paid, annualized	1.94	−0.09	1.68	16.92
	(1.28)	(2.02)	(2.02)	(36.50)
Profits (ag & non-ag), annualized	26.24	35.85	36.37	485.56
	(23.67)	(47.66)	(44.88)	(786.92)
Wage earnings, annualized	42.43	73.66	182.63***	494.95
	(32.23)	(60.82)	(65.53)	(1,231.12)

by eligibility

Non-recipient HHs: expenditure, saving

	(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: Expenditure				
Household expenditure, annualized	293.59***	338.57***	334.77***	2,536.01
	(60.11)	(109.38)	(123.20)	(1,933.51)
Non-durable expenditure, annualized	187.65***	227.20**	317.62***	2,470.69
	(58.59)	(99.63)	(119.76)	(1,877.23)
Food expenditure, annualized	72.04*	133.84**	133.30**	1,578.05
	(36.96)	(63.99)	(58.56)	(1,072.00)
Temptation goods expenditure, annualized	6.55	5.91	−0.68	37.07
	(5.79)	(8.82)	(6.50)	(123.54)
Durable expenditure, annualized	95.09***	109.01***	8.44	59.41
	(12.64)	(20.24)	(12.50)	(230.83)
Panel B: Assets				
Assets (non-land, non-house), net borrowing	178.78***	183.38***	133.06*	1,131.66
	(24.66)	(44.26)	(78.33)	(1,419.70)
Housing value	376.92***	477.29***	80.65	2,032.11
	(26.37)	(38.80)	(215.81)	(5,028.27)
Land value	51.28	158.47	544.85	5,030.03
	(186.22)	(260.91)	(459.57)	(6,604.66)

Results Outline

1. Tracing out the flow of funds
    - Recipient households
    - Enterprises
    - Non-recipient households
    - **Output & input prices**
2. Transfer multiplier

Consumer prices in markets

raw data | by product

		(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)
By tradability	More tradable	0.0014	0.0062	0.0023	0.0021
		(0.0015)	(0.0082)	(0.0023)	(0.0018)
	Less tradable	0.0009	0.0034	0.0015	0.0001
		(0.0006)	(0.0032)	(0.0011)	(0.0008)
By sector	Food items	0.0009	0.0036	0.0016	0.0002
		(0.0006)	(0.0033)	(0.0012)	(0.0008)
	Non-durables	0.0014	0.0061	0.0026	0.0019
		(0.0017)	(0.0089)	(0.0026)	(0.0019)
	Durables	0.0019*	0.0070	−0.0009	0.0034**
		(0.0011)	(0.0061)	(0.0011)	(0.0016)
	Livestock	−0.0008	−0.0027	−0.0008*	−0.0017
		(0.0010)	(0.0052)	(0.0004)	(0.0020)
	Temptation goods	−0.0011	−0.0112	−0.0008	−0.0003
		(0.0026)	(0.0143)	(0.0036)	(0.0035)

Input prices and quantities

	(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: Labor				
Hourly wage earned by employees	0.10***	0.04	0.19*	0.70
	(0.03)	(0.04)	(0.10)	(0.89)
Household total hours worked, last 7 days	2.44	1.41	−4.69	63.19
	(1.71)	(3.69)	(3.17)	(54.12)
Panel B: Land				
Land price per acre	168.02	366.46	557.44	3,952.48
	(201.18)	(290.85)	(412.34)	(3,147.29)
Acres of land owned	−0.19	−0.10	0.08	1.42
	(0.14)	(0.09)	(0.10)	(2.37)
Panel C: Capital				
Loan-weighted interest rate, monthly	−0.01	0.01	−0.01	0.06
	(0.01)	(0.01)	(0.01)	(0.07)
Total loan amount	5.53	3.12	6.12	80.57
	(4.95)	(8.34)	(13.23)	(204.28)

More labor supply results More land results

1. Tracing out the flow of funds
    - Recipient household effects
    - Enterprise outcomes
    - Untreated household effects
    - Output & input prices
2. **Transfer multiplier**

Transfer multiplier

Define the transfer multiplier as:

$$\mathbb{M} = \frac{1}{T}\left(\int_{t=0}^{\bar{t}} \Delta GDP_t\right)$$

Two approaches to estimating real GDP:

- Expenditure: $\mathbf{GDP_t} = \mathbf{C_t} + \mathbf{I_t} + \mathbf{G_t} + NX_t$
  - $\mathbf{C_t}$ = Consumption (non-durables) + accumulated assets (durables)
  - $\mathbf{I_t}$ = Enterprise investment + accumulated inventories
  - $\mathbf{G_t}$ = Local government expenditure (effect ≈ 0, Walker 2018)
  - NX_t = Net exports (including intermediate goods)
- Income: $\mathbf{GDP_t} = \mathbf{W_t} + \mathbf{R_t} + \mathbf{\Pi_t} + \mathbf{Tax_t} - NFI_t$
  - $\mathbf{W_t}$ = Household wage bill
  - $\mathbf{R_t}$ = Enterprise rental income
  - $\mathbf{\Pi_t}$ = Enterprise profits
  - $\mathbf{Tax_t}$ = Enterprise taxes
  - NFI_t = Net income from abroad

Transfer multiplier - dynamic estimation

Dynamic version of spatial regression for flow variable x:

$$x_{it,v} = \alpha_t + \sum_{s=0}^{9} \beta_s \tilde{Amt}_{v(t-s)} + \sum_{s=0}^{9} \gamma_s \tilde{Amt}^{\neg v}_{v(t-s),0-2km} + \varepsilon_{it,v}$$

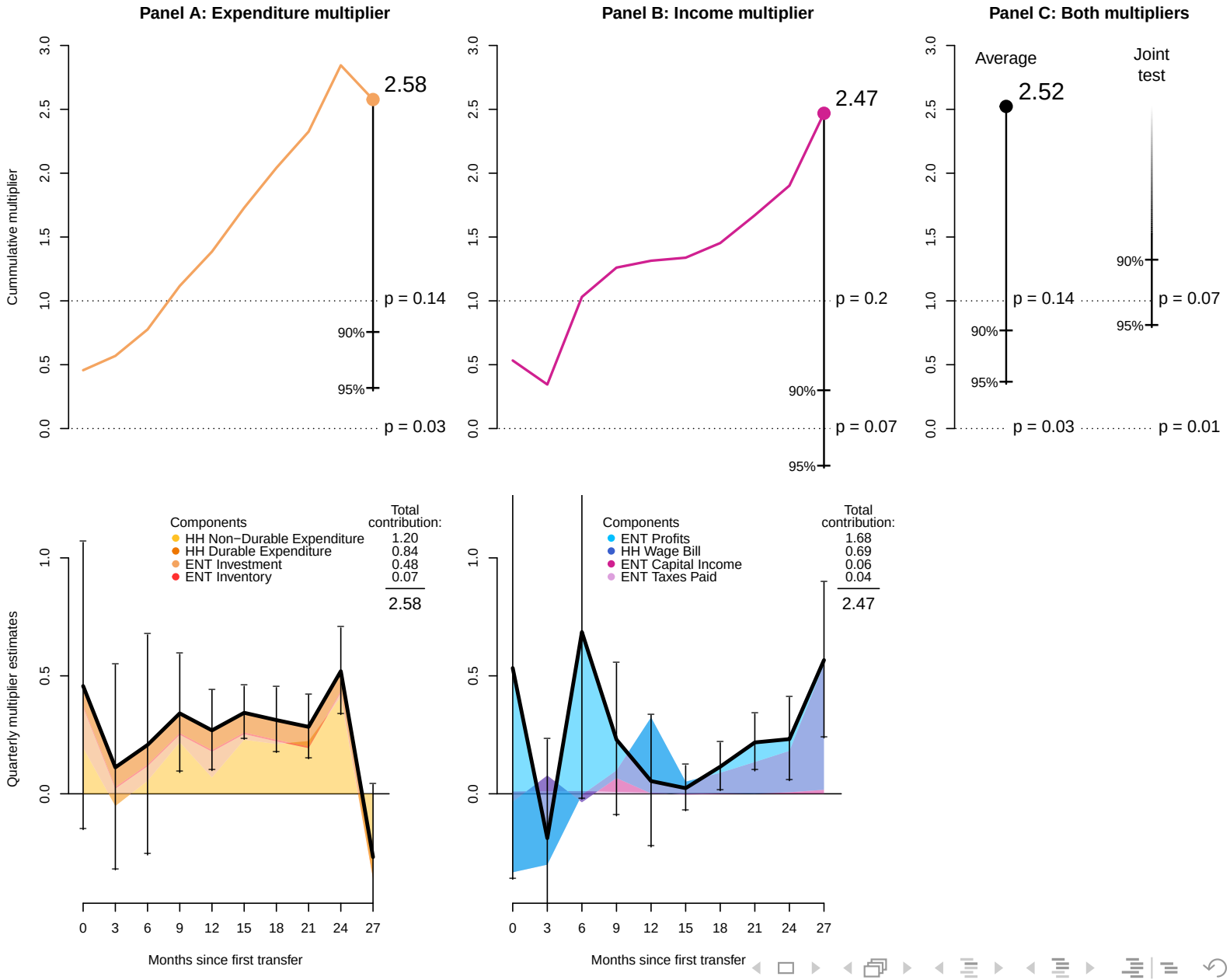
- Instrument lagged treatment in quarter $t-s$ by share of eligibles assigned to treatment $*$ share of transfers going out in $t-s$ (as order of both transfer and measurement rollout was randomized)
- Construct dynamic response to hypothetical treatment of everyone at time 0, using planned roll-out of transfers in months 0, 2 and 8. Integrate over time, and sum up across components using sampling weights from household and enterprises censuses
- Transfers and outcomes deflated to January 2015 USD PPP using the overall consumer price index in the nearest market
- Inference using wild bootstrap (with 2000 runs)

The marginal propensity to spend locally

	(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	-0.21	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)

- Static Keynesian benchmark: $\mathbb{M} = \frac{\text{MPC}}{1-\text{MPC}} \approx 2.3 - 3$
- Savings predominantly through asset purchases $\Rightarrow$ what matters for aggregate output is spending on locally produced goods (MPC local)
- Recall window misses a lot of early spending $\Rightarrow$ data from related study in neighboring Rarieda for the first 9 months after transfers (Haushofer & Shapiro (2016))

The real transfer multiplier



Transfer multiplier extensions

- *Real* multiplier of ≈ 2.5 using income and expenditure data, in line with a high MPC

- We refine the expenditure multiplier in two ways Details:

  1. Improving noisy estimates for expenditure in the first 3 quarters after transfers using data from experiment in adjacent Rarieda county (Haushofer & Shapiro (2016))

  2. Accounting for imports: Conservative estimates imply *at most* 20% of expenditure and 59% of inventories reflect imported value added

	(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

Agenda

Objectives

- A transfer multiplier is not a welfare multiplier
  - Classic derivations (e.g. the "Keynesian cross") lack microfoundations
  - Recent studies have largely focused on estimation (Ramey, 2019)
  - Exceptions have pointed out that multipliers need not be sufficient statistics for welfare (Mankiw & Weinzerl 2011, Sims & Wolff 2018)
- We aim here to describe the broad channels through which transfer could affect welfare and how these relate to the multiplier

Household value function

- Let $v_i(T_i, T)$ be the indirect utility attained by a household that receives a (possibly zero) transfer T_i while other eligible households in the area receive T

- We want to know how changes in T affect i's equivalent variation (EV) T_i^* defined by

$$v_i(T_i^*, 0) = v_i(T_i, T) \tag{1}$$

- If no general equilibrium effects, then T is irrelevant and we simply have $T_i^* = T_i$, i.e., the tautology that a dollar is worth a dollar.

Household value function (2)

- We think of v_i as the value of some underlying optimization problem

$$v_i(T_i, T) = \max_{x_i} u_i(x_i, x_{-i}(T)) \text{ s.t. } x_i \in X(T_i, T) \tag{2}$$

- u_i captures preferences over own choices, which are constrained to lie in X, and choices x_{-i} of others (which may matter if there are externalities, public goods, preferences over inequality, etc.)

- Changes in T thus affect utility (and hence T_i^*) in two broad ways:

  1. Effects on market outcomes that alter the constraint set X, for example, by changing the prices facing i, or its income from various sources.
  2. Effects on non-market outcomes that directly affect i's well-being independent of its constraint set (or if we interpret i as an individual, changing intra-household externalities or allocation)

Mapping to the multiplier

- Increases in (real) output must show up as expansion of budget sets
  - If due to productivity gains, this is a pure welfare gain
  - If due to increased employment of factors of production, this comes at some opportunity cost (e.g. disutility of labor)
- The multiplier summarizes market activity and so does *not* capture effects on non-market outcomes

Interpreting the results

1. Expansions in budget sets (whether measured by income or expenditure) do not seem to have been driven by factor employment
    - Land is in fixed supply (and households do not report owning or renting more of it)
    - No significant changes in overall labor supply, though some reallocation
    - Modest increases in capital (inventories), and output gains are if anything larger for enterprises owned by non-recipients Details
2. For (arguably) non-market outcomes we observe, effects are generally null or positive with the possible exception of inequality

Non-market outcomes and externalities

	(1)	(2)	(3)	(4)
	Recipient Households		**Non-recipient Households**	
	$\mathbb{1}$(Treat village)	Total Effect	Total Effect	Control, low saturation
	Reduced form	IV	IV	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)

- In more detail: Heterogeneity Psychological well-being Health Child details Education Female Empowerment Security Public Goods

Inequality

Details

	(1)	(2)	(3)	(4)
	Treatment Villages		**Control Villages**	
	$\mathbb{1}$(Treat village)	Total Effect	Total Effect	Control, low saturation
	Reduced form	IV	IV	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	

Agenda

What drove increases in local output?

- Any explanation must apply to the retail and manufacturing sectors where gains are concentrated By sector
- In accounting terms, the value of increased real output must reflect some mix of
  1. Higher throughput of intermediates and finished goods produced elsewhere – **seems likely** given the large retail share though not directly measured
  2. Value added through increased use of factors of production – **little evidence** of this for labor and capital, and land is in relatively fixed supply
  3. Value added through increased utilization of existing capacity – **some evidence** of low baseline utilization in "steady-state"

Factor under-utilization

- A large share of the (non-ag) economy operates "on-demand"
  - Retail: e.g. a barbershop
  - Manufacturing: 60% of revenue to grain ("posho") mills and welding shops
  - In Uganda, Bassi et al (2019) find that employees in similar industries (welding, furniture-making) spend 25% of time "waiting for customers," "eating and resting"
- These examples suggest inputs whose costs are fixed over the relevant ranges – a building, milling machinery, an employee to "mind the shop"
  - Non-ag enterprises have an average of just 1.7 customers per hour
  - A majority (72%) have one employee, suggesting that integer constraints often bind Data

$\Rightarrow$ Harkens back to classic theory in development economics on surplus labor (Lewis 1954), and may also be relevant for rich countries, esp. during recessions (e.g., Michaillat and Saez 2015; Murphy 2017).

Why might there be slack in steady-state? (speculative)

- The small scale of local market activity
  - It may be profitable to operate a standard grain milling machine with one employee, but capacity could be larger than local demand, and production easy to expand
  - Poor roads and high transport costs as an underlying cause
  - Slack may be lower in denser areas
- Frictions and institutions affecting local market structure
  - Bassi et al (2019) document multiple nearly identical manufacturing firms (e.g., carpenters) located on the same block, all with 1-2 workers and excess labor capacity
  - Consolidation into fewer, larger firms – each with more machinery and workers – would presumably reduce "slack" in labor and capital utilization. The existence of too few large firms is a well-known empirical pattern in low income economies

Agenda

Closing thoughts

- We document meaningful increases in aggregate local economic activity in response to a large inflow of cash transfers
    - Increases in expenditure and assets of recipients, revenue for nearby enterprises, and earnings, expenditure and assets of non-recipients
    - Minimal, precisely estimated consumer price inflation
    - A local transfer multiplier of 2.5
- A counter-example to the critique that experimental trials are not well suited to studying the "big questions" in economics (Bardhan 2005, Easterly 2006, Deaton 2010)
- Concerns about negative spillovers were not borne out in this setting; rather, unadjusted T-C estimates would doubly under-count welfare gains (as in Miguel & Kremer 2004)

Egger et al. (2022): price effects by market access

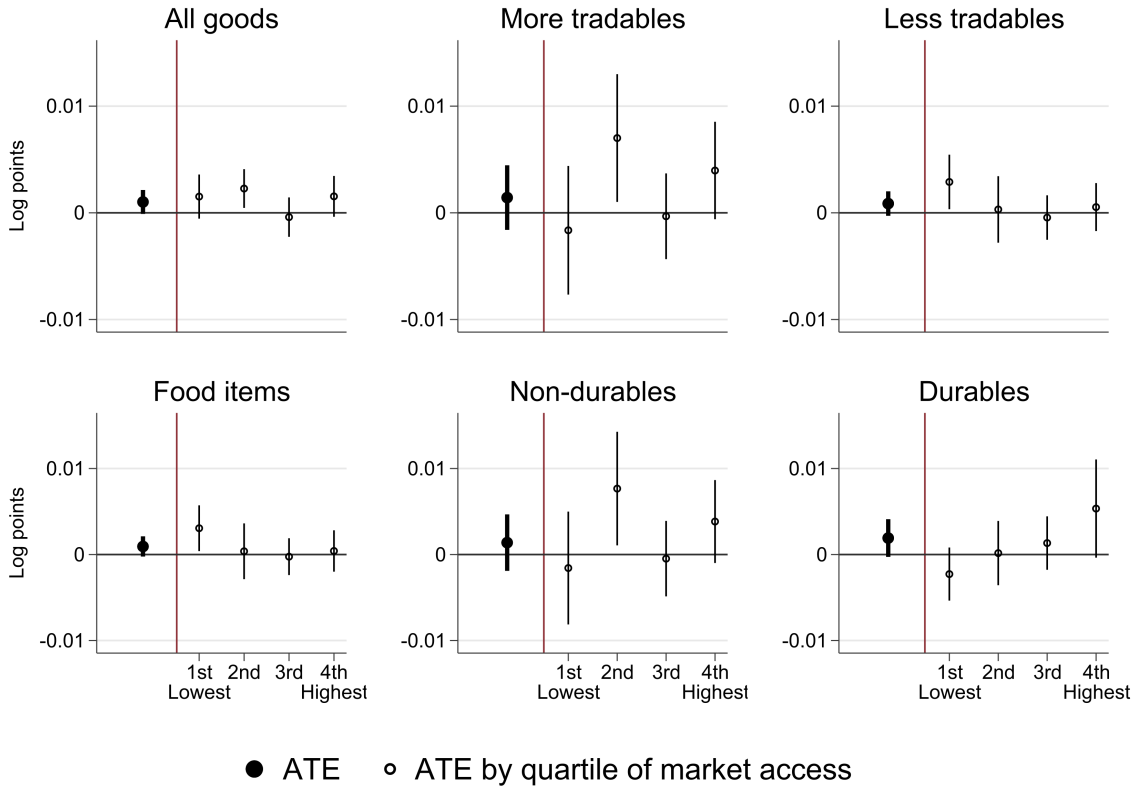


FIGURE B.3.—Output price effects by market access. *Notes*: Each panel represents a regression of the logarithm of a price index on the "optimal" number of lags and distance radii bands of per capita GiveDirectly transfers in each band, as calculated for the overall price index. The number of radii bands 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 the $r-1$ to r km band around each market. Bars represent 95% confidence intervals based on standard errors as in Conley (2008), where we allow for spatial correlation up to 10 km and autocorrelation up to 12 months.

Source: Egger, D., Haushofer, J., Miguel, E., Niehaus, P., & Walker, M. (2022). General equilibrium effects of cash transfers: Experimental evidence from Kenya. *Econometrica*, *90*(6), 2603–2643, Online Appendix.

Conclusion

- market failures imply crucial role for policy
- poverty traps and non-convexities make policy design hard:
  - treatment must be 'large enough' without being wasteful (i.e. focus on marginal recipients)
  - if treatment sufficiently large, intervention can become one-off
  - small, repeated interventions may generate transitory effects
  - success would enable bootstrapping out of poverty
- scale-up of policy challenging – analytically and practically:
  - unintended and GE effects abound, increasing in treatment size
  - factor markets, output markets, profits and market integration potentially affected
  - shocks to social fabric, informal institutions and quality of life
  - treatment multiplier effects may be large if idle capacity exists

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