

General Equilibrium Effects of Cash Transfers: Pre-Analysis Plan*

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1 Introduction

1.1 Motivation and overview

This document outlines outcomes and regression equations to measure effects of unconditional cash transfers provided through the General Equilibrium Effects of Cash Transfers in Kenya (GE) project on aggregate economic outcomes in the treated areas. These transfers are “helicopter drops of cash,” and as such their effect on the local economy is relevant for several literatures in economics, particularly work on agglomeration Glaeser and Gottlieb [2008], Hsieh and Moretti [2015], Kline and Moretti [2013], Moretti [2010], Krugman [1991] and on fiscal stimuli [Ramey, 2011, Buiter, 2014, Farhi and Werning, 2017, Nakamura and Steinsson, 2014]. Relative to earlier work on stimuli, we are uniquely able to provide estimates that both capture the effects of a macroeconomically significant influx of cash (a common form of stimulus distinct from government purchases; Oh and Reis, 2012), and are credibly identified using experimental variation. Specifically, our transfers constitute a shock of about 15% of local GDP, which is large in comparison both to many government programs and to the transfers studied in most previous program evaluations of cash transfers, which have typically focused on the responses of individual households as opposed to the aggregate economy. Our effects are also well-identified relative to typical work on fiscal stimulus, which must contend with the fact that government transfers are not experimentally assigned and may be anticipated [Parker et al., 2011].

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We first examine how recipients used transfers, organizing the analysis around a simple household accounting identity. A household that receives a “helicopter drop” of money B allocates this between (net) remittances to other households, R ; spending on consumption goods, E ; and saving, S :

$$B = \Delta R + \Delta E + \Delta S$$

Consumers may spend on goods and services produced either locally (i.e. “non-tradeables”) or non-locally (i.e. “tradeables”). Similarly, invested savings may be used to purchase either traded or non-traded capital goods, and these may be used either locally or non-locally. We distinguish between these categories as much as possible as theory predicts they may have different effects on the local economy.

We then examine how these patterns of expenditure, remittances, and saving affect local producers. We focus on aggregate output Y , defined as value-added produced by farm and non-farm enterprises in the affected area. We think of output in each sector as produced using a function $Y = f(K, L, Z)$ of variable factors of production capital K and labor L and a fixed factor land Z , and examine impacts on the utilization of each of these factors both across and within sectors. Within labor we further examine both the intensive and extensive margins (particularly migration) of labor supply. We also examine effects on the levels of prices, rental rates, and wages, both to ensure we properly measure real effects and also because these effects are of independent interest.

In general we focus on measuring aggregate quantities for the entire affected regions without distinguishing between the effects on recipient and non-recipient households. We define specifications that are relevant for examining distributional impacts, however, and which we intend to use in separate analysis of impacts on household well-being (Haushofer et al. [2017a]).

1.2 Relationship to other pre-analysis plans

This pre-analysis plan relates to four previously filed plans for the GE project:

1. A midline PAP [Haushofer et al., 2016, filed May 19, 2016] focused on impacts on market prices.
2. A local public finance PAP [Walker, 2017, filed February 12, 2017] focused on impacts on local taxation, spending, and participation in public projects.

3. A household welfare PAP [Haushofer et al., 2017a, filed July 6, 2017] focused on impacts on treated households. An amendment filed in conjunction with this pre-analysis plan outlines how we will look at saturation effects and spillover effects onto untreated households for the same outcomes as treated households.
4. A targeting PAP Haushofer et al. [2017b, filed September 1, 2017] focused on understanding heterogeneity in the impacts of the treatment on treated households.

Relative to these previous plans, this GE PAP focuses on aggregate economic impacts, which along with the results from the household welfare pre-analysis plan (including the aforementioned amendment) provide a picture of distributional impacts. The GE PAP also builds on the midline PAP in several ways: it uses a similar approach to study the spatial and temporal structure of effects, and draws heavily on market price and enterprise data. After the filing of the local public finance and household welfare pre-analysis plan, Miguel and Walker were involved in estimating treatment effects outlined in these pre-analysis plans; their comments were treated as suggestions by colleagues by Haushofer and Niehaus in finalizing this GE pre-analysis plan.

The rest of this document is organized as follows: Section 2 describes the data collected and outlines variable construction. Sections 3 and 4 describe the economic literatures and main outcome variables that are the focus of this PAP. Section 5 describes the econometric approach. For details on the intervention and experimental design, please see the household welfare PAP [Haushofer et al., 2017a].

2 Sampling and data collection

To measure economic aggregates such as spending, saving, output, and factor supply, we require data on representative samples of both households and firms. We collected these data as follows.

2.1 Household Data

In advance of the distribution of transfers to treatment villages, we conducted baseline household censuses and household surveys in both treatment and control villages. The household census was designed to be comparable to the census conducted by the partner organization GiveDirectly (GD) to determine eligibility within treated villages so that

we could predict which households within these villages would be treated. The census also served as a sampling frame for baseline household surveys. We predicted household eligibility based on the census data and randomly selected 8 households predicted to be eligible and 4 predicted to be ineligible to survey in each village; we refer to these as “initially-sampled” households. For couples, we randomly selected either the male or female to be the “target” respondent; if we could not reach the target, but the spouse/partner was available, we surveyed the spouse/partner. If an initially-sampled household was not available to be surveyed on the day we visited the village for baseline surveys, we replaced this household with another randomly-selected household to ensure that we surveyed 12 households in each village. We refer to these households as “replacement” households. Lastly, we refer to households that were initially-sampled but unable to be surveyed as “missed baseline” households.

Endline surveys targeted all “initially sampled” and “replacement” households. For households that were baselined, we attempt to survey the same respondent that was surveyed at baseline. Endline surveys began at the end of May 2016 and ran through May 2017. The median survey date was about 1.5 years after the baseline surveys. See Haushofer et al. [2017a] for more details on endline data collection.

2.2 Enterprise Data

We collected data on enterprises at both baseline and endline.¹ As part of baseline activities, we conducted an enterprise census in addition to the household census described above. These censuses were designed to be jointly comprehensive, with the enterprise census designed to capture enterprises operating from outside the homestead while the household census captured those operating within homesteads by asking whether the household was running an enterprise. These censuses were generally conducted on the same day in a village, between August 2014 and and July 2015. We returned later to conduct enterprise surveys. Enterprise surveys were conducted on a single day and usually covered all enter-

¹In addition to these sources, we also conducted phone surveys of a sample of enterprises between August 2015 and June 2016. The data collection and analyses from those surveys are outlined in the GE midline data PAP [Haushofer et al., 2016]. In those surveys, we focused on four types of enterprises: i) small retail shops/dukas (kiosk shops), one of the most common types of enterprises and a seller of a variety of common products; ii) cereal grinders (known as posho mills), one of the most common enterprises and a staple producer; iii) tailoring shops, one of the top 10 most common enterprises and anecdotally a provider of luxury goods; and iv) hardware shops, given that Haushofer and Shapiro (2016) find important effects on home construction.

prises from the enterprise census (i.e. those operating outside of the homestead) open on that day. We made an exception in villages with more than 20 enterprises (which occurred when a village overlapped with a market center) where we randomly selected 20 enterprises to survey, the maximum we anticipated a single enumerator would be able to cover in a day. In all cases, baseline censuses and enterprise surveys were conducted prior to the distribution of any cash transfers to the village.

We conducted a second enterprise census at endline in order to capture entry and exit. In addition, our endline enterprise census enumerated enterprises whether they operated within or outside a homestead; this is because we did not conduct a second household census at endline and wanted to be sure to capture all enterprises. We used information from the baseline household and enterprise census as a starting point for enumerators in each village, and enumerators then consulted with the village elder and the enterprise owners and employees that took part in the census to ensure that all enterprises within the village were censused. The endline enterprise census was conducted between November 2016 and February 2017. After the conclusion of the census, we randomly sampled up to five enterprises per village to be targeted for surveys: up to two enterprises operating from within homesteads, and three enterprises operating from outside homesteads. If a target enterprise was closed, enumerators sought to replace this enterprise with another randomly-selected enterprise of the same type. Enterprises operating outside of the home are on average larger than enterprises operating from homesteads, and this sampling strategy ensures that we cover a range of enterprise types. Enterprise surveys began in February 2017 and concluded in April 2017.

We faced a particular challenge capturing “abedo” enterprises, which typically operate from a homestead during the day but then move to a shopping center in the late afternoon to offer a small assortment of goods to people finishing their daily work. We capture information on abedoss as follows: at baseline, we captured them as part of the household census (and not the enterprise census) to avoid double-counting. At endline we capture them as part of the comprehensive enterprise census, since we did not conduct a separate household census.

3 Macroeconomic aggregates

We next state our primary outcome concepts of interest. In some cases it is obvious how to measure these concepts using variables in the survey instrument, while in others there

is additional work required to construct the best available measures. In the interest of brevity we focus here on defining the concepts of interest and defer a full specification of the measures we will construct.² We use nominal values as our primary outcomes, as analysis conducted prior to writing this plan indicated that transfers had no meaningful impact on local prices; in brief, we constructed an index of market prices using data from 61 weekly markets in our study area and found little impact of transfers on this index using a variety of spatial and temporal specifications [see Haushofer et al., 2016, for full details]. As a robustness check we also conduct analysis using real values, which we construct by deflating nominal values using the consumer goods price index described below.

3.1 (Net) remittances

A transfer of cash to households in the treated area may trigger a change ΔR in net remittances to households outside the treated area. For any given economic aggregate X , there are then arguably two outcomes of interest: the change as a proportion of gross transfers ($\Delta X/\Delta B$) and the change as a proportion of net transfers ($\Delta X/(\Delta B - \Delta R)$) to the treated area. The latter is the “structural” effect we would expect to observe if transfers were delivered to all regions (in which case the net effect on remittances would on average be 0). To estimate both quantities we must first estimate impacts on net remittances, which we therefore specify as a primary outcome.

- **Primary outcome: net remittances sent.** The value of both cash and in-kind remittances sent, net of value received.

3.2 Expenditure

Overall expenditure is a primary outcome. In addition, we further distinguish between goods with different degrees of tradeability. As in the midline PAP [Haushofer et al., 2016]

²For example, one recurring issue concerns timing. For stock outcomes such as assets we are typically interested in impacts on the stock $X(T)$ at the time T that outcomes were measured. For a flow variable $X(t)$ such as expenditure, on the other hand, we are typically interested in the effect of treatment on the integral

$$\int_{t=0}^T \Delta X(t) dt$$

over the outcome from the time $t = 0$ of treatment to the time $t = T$ of measurement. Our surveys did not measure $X(t)$ for all the relevant concepts and time periods, however, so that for each outcome we must construct the best available approximation.

we categorize goods into three groups, treating food and livestock as a distinct category because it is difficult to judge whether they are traded or not.

- **Primary outcome: expenditure.** Total expenditure on consumption goods.
- **Secondary outcome: expenditure on tradeable goods** (durables and non-food non-durables). These are tobacco, cigarettes, bhang, snuff, khatt, miraa; petrol; clothing and shoes; books/magazines, music/CDs, videos; toiletries, cosmetics, combs, soap; household items: soap, cleaning agents, toilet paper/tissues, air freshener, shoe polish, insecticide, matches, candles; firewood, charcoal, kerosene; household durables: furniture, cutlery, lamps, kitchen equipment, vases and mirrors.
- **Secondary outcome: expenditure on food and livestock.** These are cereals; roots and tubers; pulses; vegetables; meat; fish; dairy products and eggs; other animal products; oils and fats; fruits; sugar products; jam, honey, sweets, candies; non-alcoholic drinks; alcoholic drinks; salt, pepper, spices and condiments; other foods.
- **Secondary outcome: expenditure on non-tradables** (services). These are food eaten outside the house; airtime, other phone expenses; internet; tolls, transport fare, hotel stays; lottery tickets/gambling; tickets to any entertainment; haircuts; electricity; water; house rent/mortgage; home maintenance and repair; religious expenses or other ceremonies; charitable donations; weddings and funerals; medical expenses; dowry/bride price.

3.3 Saving

To measure impacts on total household savings over the time period from the intervention until our survey, we examine the impact on the stock of household's net assets. Relative to the alternative of measuring impacts on savings during each month (say) from the moment of treatment onwards and then aggregating, this has the advantage that a single measure collected once without recall concerns should reflect all saving done previously. The major disadvantage is that for non-financial assets, which comprise the bulk of most households' portfolios, this measure also captures depreciation. For example, if a household saved money in the form of livestock which subsequently died, we would count this as dissaving. For any given rate of depreciation δ , however, we can bound the bias as follows. Let S_t

be (net) savings at time t and A_t be assets, and let $t=0$ denote the time treatment was delivered. Then the treatment effect ΔA_T on assets measured at some subsequent time T is given by

$$\Delta A(T) = \int_{t=0}^T \Delta S(t) e^{-\delta(T-t)}$$

If (as convex models would predict) the impact on savings is weakly positive at every point, then this implies

$$\Delta A(T) e^{\delta T} = \int_{t=0}^T \Delta S(t) e^{\delta t} dt \geq \int_{t=0}^T \Delta S(t) dt \geq \Delta A(T)$$

In other words, the treatment effect on total savings from time $t=0$ to time $t=T$ is bounded by the effect on assets and the effect on assets depreciated as if all savings had occurred instantaneously at time $t=0$. We therefore propose to estimate both these quantities using locally plausible estimates of the rate of depreciation on commonly held assets.

- **Primary outcome: net assets.** Total net asset holdings.

3.4 Output

We define output as the sum across enterprises of enterprise value added (including from small enterprises such as a retail kiosk operated within homesteads, which were included in our endline survey of enterprises) and the sum across households of agricultural output. We measure enterprises both through our enterprise survey and via a household survey module on non-agricultural self-employment; we use all available data on enterprises to estimate effects.³ This definition does not include the value of household production such as cooking, domestic work, etc. In addition, it does not include the public and NGO sector, which is likely a small share because our study area does not include larger towns.

We define value added as usual as the market value of goods sold (i.e. revenue), plus the value of any changes in final goods inventories, less spending on intermediate inputs

³One limitation of the self-employment data measured in households surveys for these purpose is that we do not observe inventories of final goods produced via self-employment. While we do not expect these to be significant, we may also examine the robustness of our conclusions to excluding value add from self-employment.

(raw materials, electricity, services to other firms, and rented capital). Note that payments to labor and owned capital are not subtracted.

For enterprises there are some nuances involved in calculating spending on intermediate inputs. We include payments to rent premises, market fees, and security fees. We do not include the investment variable we collected, which is a sum of spending on both renting and purchasing capital goods. In principle we should include rental fees and exclude capital expenditure; in practice based on our experience working in this area it is rare for enterprises to rent capital goods, and we believe that the bulk of this spending represents capital expenditure.

For household agricultural production, we observe revenue from the sale of agricultural outputs (excluding meat and milk) as well as quantities sold and total quantities produced, from which we can infer the market value of the total quantities produced. We do not observe changes in final goods inventories which we expect to be negligible in the context of agricultural production. To measure spending on raw materials we include all expenditures except salaries and capital expenditure on tools and own machinery, which leaves expenditure on animal medicine; fertilizer; irrigation; improved/hybrid seeds; and formal agricultural insurance. We do not measure rental costs of capital equipment but again expect these to be relatively minor in this context.

To look at aggregate effects, we consider each farm and enterprise as a “productive unit”. We outline our methodology for this in Section 5.2.

- **Primary outcome: value added in enterprise and agricultural production**
- **Secondary outcome: value added in enterprise**
- **Secondary outcome: value added in agricultural production**

We also distinguish further between enterprise production of more and less tradeable goods, using a similar delineation as the one outlined above for expenditure. Specifically, we distinguish between production of tradables (durables and non-food non-durables), food and livestock, and non-tradables (services). Note that retail enterprises are always considered non-tradable because their value added consists of retailing, not production. The classification of enterprises is therefore as follows:

- **Secondary outcome: tradeable value added in enterprise.** Durables and non-food non-durable production including the sale or brewing of homemade alcohol / liquor; carpenter; non-food producer.

- **Secondary outcome: food and livestock value added in enterprise.** Tea buying centre; livestock / animal / poultry sale; fishing; fish sale / mongering; cereals; agrovet; jaggery; butcher; food stand / prepared food vendor; food stall / raw food and fruits vendor.
- **Secondary outcome: non-tradeable value added in enterprise.** Services including small retail (incl. dukas); large retail; non-food vendor; hardware store; bookshop; chemist; petrol station; restaurant; bar; barber shop; beauty shop / salon; M-Pesa agent; bank agent; mobile charging; video room/football watching hall; cyber café; tailor; posho mill; welding / metalwork; guesthouse/ hotel (lodging); motor vehicles mechanic shop; motorcycle mechanic repair / shop; bicycle repair / mechanic shop; piki driver; boda driver; oxen / donkey / tractor ploughing; photo studio

Lastly, we look specifically at whether any changes in value-added are coming from the extensive or intensive margin. First, we look at whether treatment villages have a higher number of enterprises than control villages, using data from our enterprise census. Second, we look at value-added by enterprises separately for new enterprises (those that started after distribution of the transfers) versus incumbent enterprises.

- **Secondary outcome: Number of enterprises per village from enterprise census data.**
- **Secondary outcome: Value-added in enterprise by new enterprises**
- **Secondary outcome: Value-added in enterprise by incumbent enterprises**

3.5 Labor

An increase in output implies an increase in the utilization of variable inputs, which we examine in turn. For labor we use data from our household survey; we include work in agriculture, wage employment, own-farm self employment and non-agricultural self-employment. We implicitly exclude time spent on home production (e.g. cooking), as is typical.

- **Primary outcome: labor utilization.** Total hours worked in agricultural self-employment, non-agricultural self-employment, and wage employment.

We examine labor mobility across sectors using data on agricultural and non-agricultural employment. We are interested here in testing for a structural shift, i.e. for a disproportionate increase in employment in one sector relative to the other. We test for such a shift by estimating impacts on employment in each sector separately and then testing whether these changes are proportionate to control group mean employment levels.

- **Secondary outcome: labor utilization in non-agricultural enterprise**, including both self- and wage-employment.
- **Secondary outcome: labor utilization in agriculture.**

We examine labor mobility across space using data on the number of working-age adults present in surveyed households at endline. We measure impacts on net in-migration by measuring effects on the overall count of working-age adults. To separate out gross flows in each direction, we measure impacts on the number of working-age adults present at endline who were not present at baseline; impacts on this measure capture gross in-migration. Similarly, we construct a measure of the number of working-age adults that were present at baseline but moved away at endline. Note that there is one important limitation to this analysis. Because we did not conduct a second household census at endline we do not observe entirely new households that may have moved into the area; this will tend to lead us to understate impacts on net or gross in-migration. However, given that land markets are thin, we expect the largest component of migration flows to be changes in the number of household members.

- **Secondary outcome: number of working-age household members (net migration).**
- **Secondary outcome: number of working-age household members not present at baseline (in-migration).**
- **Secondary outcome: number of working-age household members present at baseline, but moved away at endline (out-migration).**

3.6 Productive capital

For enterprises, we measure changes in the stock of capital goods using reported investment. As discussed above, this measure in principle includes both capital expenditure on owned

equipment and also rental expenditure on rented equipment, but our experience of the context suggests that the latter are likely to be a small share of total expenses. For household agricultural production, we measure the change in the stock of capital goods as either the flow value of investments made (e.g. expenditure on tools and durable goods) or the change in the stock value of owned assets (e.g. livestock) depending on the way these topics were covered in the survey. In both cases we exclude expenditure on any rented capital, which should appear as owned capital on the balance sheet of the firm that owns the asset. When our best measure of investment is the past flow value of capital expenditure we face a depreciation problem opposite to that discussed above in the context of household savings: we will tend to over-estimate impacts on the capital stock to the extent any incremental assets acquired in the past have depreciated. We will therefore explore the sensitivity of our conclusions to reasonable assumptions about the rate of depreciation on these assets.

We treat land as a distinct factor of production; we expect the stock of land to stay fixed in the short run, but it is possible that it could adjust if for example treatment induced the clearing and cultivation of previously marginal lands. We therefore include it as a secondary outcome.

- **Primary outcome: capital employed.** The sum of the change in capital use in enterprise and in agricultural production.
- **Secondary outcome: capital employed in agricultural enterprise.**
- **Secondary outcome: capital employed in non-agricultural enterprise.**
- **Secondary outcome: total land employed in agricultural production.**

We also look at responses from enterprise owners to questions about overall business sentiment, following questions commonly used to construct indices of market sentiment which are thought to be predictive of investment behavior. We calculate a diffusion index (the share of positive responses + 50% of the share of neutral responses) for feelings about overall business conditions.

- **Secondary outcome: view whether overall business conditions today are improving, staying the same or worsening.**
- **Secondary outcome: view whether overall business conditions in one year will be better, the same or worse.**

4 Prices

We examine impacts on the (log) prices of final consumer goods (which we additionally use to construct real values of other shilling-denominated outcomes) as well as the (log) prices of labor, capital and land inputs.

4.1 Consumer goods prices

To examine impacts on consumer goods prices we follow the same methods defined in in Haushofer et al. [2016], taking the mean log index over the period of endline data collection (June 2016 to January 2017) as our primary outcome. We also separately estimate impacts on the prices of the traded and non-traded subcomponents of the commodity basket using the categorization defined above for consumer goods.

- **Primary outcome: consumer goods prices.** Average log price of a standard basket of goods from June 2016 to January 2017 using the basket and specifications defined in [Haushofer et al., 2016].
- **Secondary outcome: tradable consumer goods prices.** As above, for goods defined as tradeable in our expenditure analysis.
- **Secondary outcome: food and livestock prices.** As above, for goods defined as food and livestock in our expenditure analysis.
- **Secondary outcome: non-tradable consumer goods prices.** As above, for goods defined as non-tradeable in our expenditure analysis.

To construct real-valued outcomes, we need to assign a price level to each observation of a nominal outcome. We do this by calculating a consumer price index for each household and each enterprise in the month in which they were surveyed. Specifically, for households living in (or near) our study area at endline, we assign households the market price index of their nearest market, based on the as-the-crow-flies distance between households' location and markets. For households missing GPS coordinates that are still resident in study villages we use the geometric mean of households within the same village from baseline household census data. For households that have migrated outside of the study area to another rural area, we use the mean market price across all markets in our study area as a rural price index. For households that have migrated to an urban area, we inflate our rural price

index by the mean urban/rural price difference in market price surveys collected as part of the Kenya Life Panel Survey project, a longitudinal study of nearly 10,000 Kenyan youth involved in two previous randomized controlled trials [Kremer et al., 2009, Miguel and Kremer, 2004], during our endline survey period. For enterprises we follow an analogous procedure except that we do not track any enterprises that have relocated outside the study area. We may also examine robustness to adjusting figures for the national inflation rate.

4.2 Wages

Estimating the effect of transfers on wages is complicated by potential compositional effects on employment: treatment may induce workers to select into different kinds of jobs, and may induce firms to employ different kinds of workers. Neither of these potential biases is easy to sign ex ante. We therefore plan to examine effects on wages as reported by both sides of the market, and also to examine effects on the socio-economic and demographic characteristics of employed adults. We do not have data on job attributes other than wages and therefore do not report corresponding effects on the composition of jobs.

- **Primary outcome: mean wage paid by enterprises**, weighted by firm employment.
- **Primary outcome: mean wage earned by working-age adults.**
- **Secondary outcome: characteristics of employed working-age adults.**

We also examine how any effects vary by sector (agriculture vs. non-agriculture) and skill level.

- **Secondary outcome: mean wage paid by agricultural enterprises**, weighted by firm employment.
- **Secondary outcome: mean wage paid by non-agricultural enterprises**, weighted by firm employment.
- **Secondary outcome: mean wage paid to unskilled workers**, as measured by household surveys. We restrict attention to individuals working jobs in the following occupations that we consider unskilled labor: agricultural laborer; livestock care/shepherd; fishing; selling agricultural products; hawking/ selling clothes, food, other items; working in other person's shop (retail); domestic work (house boy/girl); hotel,

restaurant or tourism job; watchman/ guard/ cleaner; vehicle taxi work (matatu tout/conductor, not driver; and local brewer.

- **Secondary outcome: mean wage paid to skilled workers**, as measured by household surveys. We define occupations as skilled that are not listed above as unskilled with the exception of “other” responses in the endline data, which will be handled on a case-by-case basis consistent with the above definition.

Finally, we examine the robustness of our conclusions to the use of alternative measures of local wages. Specifically, we collected information on agricultural wages in a survey of village elders and collected information about skilled and unskilled wages in a subset of market surveys.⁴

- **Secondary outcome: mean wage paid to agricultural workers**, as measured in surveys of village elders. These surveys asked about agricultural wages for a variety of agricultural activities (such as ploughing, clearing, harvesting, etc.). We take the median wage across activities to generate a village-level agricultural wage measure.
- **Secondary outcome: mean wage paid to unskilled workers**, as measured in market surveys. Market surveys conducted between July 2016 and January 2017 collected information about daily wages for basic unskilled labor.
- **Secondary outcome: mean wage paid to skilled workers**, as measured in market surveys. Market surveys collected similar information on skilled labor (masonry, etc) as with unskilled labor.

4.3 Rental rates

To measure effects on the prevailing rental rate of capital we use interests rates on loans held by households. Specifically, we calculate the loan size weighted average annualized interest rate on loans from ROSCAs, money lenders, family members and non-family members. We exclude loans from banks and from M-Shwari (a service offered by Safaricom) as these institutions typically set rates that do not vary finely with geography. We did not collect information on the terms of enterprise loans.

- **Primary outcome: interest rates.** The loan size weighted average interest rate on informal loans held by households.

⁴For market survey outcomes, we follow regression specifications outlined in Haushofer et al. [2016]

- **Secondary outcome: lending rates.** The loan size weighted average interest rate on informal loans charged by households lending money.

4.4 Land and building prices

Our primary measure of land prices comes from household self-reports and in many cases represents households’ best guesses as to the price that land would command if sold, as land markets are relatively thin. We restrict our sample to households that have not migrated from our study area, and classify household locations on the basis of where they are living at the time of the endline survey, as these are the relevant housing and land prices for this analysis.

- **Primary outcome: land prices.** Price per acre of land in village in which the respondent lives.
- **Secondary outcome: monthly rent charged per acre of land.** Calculated from total rent charged, acres rented, and months rented in household survey.

We do not use data on household expenditure renting homes, or enterprise expenditures renting commercial facilities, as our data only include total expenditure and not quantity information that would let us calculate unit costs.

5 Empirical Specifications

5.1 Household Regressions

We use data from all households – “initially-sampled” households (both those that were baselined and missed at baseline) and “replacement” households – as part of our main specifications. We use the following main specification for data from the household survey:

$$\tilde{y}_{ihvs,t=1} = \beta_0 + \beta_1 \tilde{A}mt_v + \beta_2 \tilde{A}mt_s + \delta_1 \tilde{y}_{ihv,t=0} + \delta_2 M_{ihv,t=0} + \zeta_1 \pi_v + \zeta_2 \pi_s + \varepsilon_{ihvs} \quad (1)$$

Here, h indexes the household, v indexes the village, s indexes the sublocation, and t indicates whether the variable was measured at baseline ($t = 0$) or endline ($t = 1$). For individual-level variables, i indexes members in the household roster. Amt_v is the total amount transferred to a village, Amt_s is the total amount delivered to other villages

in the same sublocation, and \tilde{y} and \tilde{Amt} are the per-capita analogues of these variables. Per-capita transfers to control villages are zero. Importantly, random assignment does not guarantee that the Amt variables are exogenous as they also depend on the share of households in a given location that are eligible. We therefore instrument for these measures using assignment to treatment at the village level, and assignment to high or low saturation at the sublocation level, and estimate the model using two-stage least squares. Following McKenzie [2012], we condition on the baseline values of the outcome variable $\tilde{y}_{ihvs,t=0}$ to improve statistical power. When $\tilde{y}_{ihvs,t=0}$ is missing for an observation, we include an indicator term for missingness, M_{ihvs} , and replace $\tilde{y}_{ihvs,t=0}$ with its mean. We further control for the share of eligible households at the village (π_v) and sublocation (π_s) level. In both treatment and control villages, we surveyed eligible and ineligible households. To account for the relative frequency of these household types in each village, we will use inverse probability weights.

One drawback of estimating the effects of village and sublocation treatment intensity simultaneously is that it requires us to cluster standard errors at the sublocation level, which may yield relatively inefficient estimates of village effects. We therefore plan to estimate village effects by estimating Equation 1 restricting $\beta_2 = \zeta_2 = 0$ and clustering at the village level, while estimating sublocation effects by estimating it without these restrictions and clustering at the sublocation level. For calculations that require estimates of both effects we will use the results from the latter estimation. We may adapt this procedure, however, if we are subsequently able to identify more statistically efficient ones.

5.2 Enterprise regressions

We use the following regression as our primary specification to study outcomes collected as part of the endline enterprise survey:

$$y_{evs,t=1} = \beta_0 + \beta_1 \tilde{Amt}_v + \beta_2 \tilde{Amt}_s + V_{evs} \sigma + \delta_1 y_{evs,t=0} + \delta_2 M_{evs,t=0} + \zeta_1 \pi_v + \zeta_2 \pi_s + \varepsilon_{evs} \quad (2)$$

Here, y_{evs} is the outcome of interest for enterprise e in village v and sublocation s , V_{evs} is a vector of indicator variables for the survey instrument and sampling strategy described below, and other variables are as above.⁵ Again the per-capita amount of trans-

⁵We use per-capita values of y_{evs} where appropriate.

fers is instrumented using treatment assignment at the village level. Baseline outcomes are included whenever they are available. Our primary sample includes both “target” and “replacement” enterprises, as described in section 2.2. When looking at enterprises, V_{evs} contains an indicator equal to one for enterprises surveyed as part of the enterprise survey and operating outside the homestead and an indicator equal to one for enterprises surveyed as part of the household survey self-employment section. We will use inverse probability weights to obtain results that are representative of the universe of enterprises. As above for households we will estimate this specification dropping the sublocation variables and clustering by village to estimate the effects of village treatment intensity, and also estimate it with all variables and clustering at the sublocation level to estimate the effects of sublocation treatment intensity.

In order to look at total output, we combine data from households and enterprises to look at effects on “productive units”. We consider each household a productive unit for agricultural production; in baseline data, 97 percent of households report being engaged in agricultural activity, and we code households not engaged in agriculture as having zero output. (Note that this means that we use specification 2 when analyzing effects on agricultural production, and we omit V_{evs} as all data come from the household survey.) We also consider each enterprise as a productive unit. This includes household self-employment. When looking at productive units, V_{evs} contains the indicators equal to one for productive units surveyed as part of the enterprise survey and operating outside the homestead, an indicator for household survey self-employment units, and an indicator for household survey agricultural units. We use weights based on baseline household census data (our only household census) and endline enterprise census data, both of which served as sampling frames for surveys. We sampled households on the basis of their eligibility status, and enterprises on the basis of whether they operated from homesteads or outside of homesteads. We thus develop village-level weights for each of the following categories of productive units as the inverse share of surveyed units in each village: eligible households, ineligible households, homestead enterprises and outside-homestead enterprises.

5.3 Village-level regressions

For outcomes defined at the village level (e.g. the number of active enterprises), we estimate a specification analogous to those above:

$$y_{vs,t=1} = \beta_0 + \beta_1 \tilde{A}mt_v + \beta_2 \tilde{A}mt_s + \delta_1 y_{v,t=0} + \delta_2 M_{v,t=0} + \zeta_1 \pi_v + \zeta_2 \pi_s + \varepsilon_{vs} \quad (3)$$

As above we estimate versions of this with only village-level effects as well as the full model.

5.4 Inference

As described above, we calculate standard errors clustered at either the village or sublocation level, depending on whether we are estimating the model without (with) sublocation effects.

We will account for multiple inference by focusing our analysis on a set of primary outcomes listed above. As a further adjustment, we calculate sharpened q -values over the primary outcomes following Benjamini et al. [2006], to control the false discovery rate (FDR). Rather than specifying a single q , we report the minimum q -value at which each hypothesis is rejected, following Anderson [2008]. We will report both standard p -values and minimum q -values in our analysis. We will apply the adjustment separately for each coefficient of interest. Whether one focuses on p -values or q -values in interpreting the results depends on the implicit loss function the reader is optimizing. For instance, a reader making a decision that whose returns depend on whether transfers affect any particular macroeconomic outcome (e.g. prices) should focus on the p -value, not the q -value. In contrast, a reader making a decision whose merits depend on whether transfers affect a high proportion of macroeconomic indicators should focus on q -values.

5.5 Effects of interest

In addition to the main effects described above, we also calculate several related or derived statistics that involve some degree of extrapolation. First, we estimate the **total treatment effect**

$$\beta_{PTTE} = E[y_{ihvs,t=1} \mid \tilde{A}mt_v = \tilde{A}mt_s = z] - E[y_{ihvs,t=1} \mid \tilde{A}mt_v = \tilde{A}mt_s = 0] = (\beta_1 + \beta_2) * z$$

that we predict would result if all villages were treated with transfers per capita of z as opposed to transfers per capita of 0. This calculation implicitly assumes that the effect

of treating additional neighboring villages is linear and does not interact with the effect of own-village treatment.

Second, for spending and saving outcomes we estimate households’ **marginal propensity to consume**. We cannot estimate this quantity directly because we do not have experimental variation in the amount of money each household received independent of the amount their village received, which may also have affected their outcomes. We therefore estimate the MPC by assuming that the effects of funds received by neighbors on own behavior are the same for recipients and non-recipients of transfers. Under this assumption, we can calculate the MPC by interacting equation 1 with an indicator for household eligibility for transfers and interpreting differential impacts on eligible households as their MPCs.

5.6 Estimating the spatial horizon of treatment effects

The analyses outlined so far posit that spillover effects occur within villages and within sublocations. However, it is possible that spillovers are better modelled as occurring not within administrative units but as a function of geodesic distance. We therefore estimate models similar to those in the midline PAP [Haushofer et al., 2016] in which effects are a function of cash delivered at various radii. The main difference comes from the fact that the midline data was collected in the midst of the distribution of a large amount of transfers, whereas endline household and enterprise survey data collection occurred after all or a large part of the distribution of transfers concluded. We therefore look at the effects of the total amount distributed per capita, $\tilde{Amt}_i^{RR'}$, within a series of donuts with inner radius R kilometers and outer radius R' around household or enterprise i . Analogously to the approach used above, we instrument for this quantity with the proportion of eligible households in the same donut whose villages were assigned to treatment. We examine distance ranges from 0-1km up to 9-10km. We then estimate the following model for both enterprise and household data, with $R' = R + 1$:

$$y_{ivs} = \sum_{R=0km}^{9km} \gamma_{RR'} \tilde{Amt}_i^{RR'} + \varepsilon_{ivs}. \quad (4)$$

Here, y_{ivs} is the outcome of interest for household or enterprise i in village v in sublocation s , and $\tilde{Amt}_i^{RR'}$ is the per-capita amount transferred to households within a given radius band relative to unit i , instrumented as described above. We use the Schwartz Bayesian

Information Criterion to select the nested model with the optimal number of radius terms, while imposing weak monotonicity [see Haushofer et al., 2016, for full details]. Standard errors are calculated via Conley [1999, 2008] using a uniform kernel up to the maximum radius of the nested model. We then test i) whether the $\gamma_{RR'}$ terms are jointly different from zero and ii) whether the $\gamma_{RR'}$ terms are equal to one another.

One potential concern with this strategy is that part of the variation in the treatment density around a location comes from the assignment of sublocations to high- and low-saturation. As a robustness check, we will estimate models that fully interact an indicator for being in a high saturation sublocation with the amount variables, and will present estimates with and without the restriction that the effects are the same.

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