Asset Transfers and Household Neediness in South Sudan

Elliott Collins and Ethan Ligon

February 23, 2016

Abstract

What happens when you give an 'ultra-poor' household a productive asset, with training in how to use it? The answer depends on the ways in which markets are incomplete. Previous studies have found that in some settings this sort of program can have a significant impact on occupational choice and average income. Here we document the effects of such a program in South Sudan, but with a focus on the *welfare* of the household, using a measure related to the household's marginal utility of expenditures, or what Ligon (2016) calls the households' *neediness*.

This construction allows us not only to see if the the program has a significant effect on household welfare, but also allows us to draw inferences regarding which households would benefit most from a hypothetical cash transfer. We use the fact that neediness is related not only to consumption expenditures, but also to key variables such as the marginal product of labor, investment, and participation in both market- and self-employment.

We report the results of an experiment which randomly assigns participation in such a program, and find large and significant effects on expenditures and a 0.21 standard deviation reduction in average neediness. These improvements in welfare are mirrored by increases in the number and value of assets held; increases in self-employment and skilled market employment, these last compensated for by a marked decrease in casual agricultural labor (and, less confidently, by an increase in leisure).

Introduction

We consider a program in South Sudan which provides training and productive assets to women in very poor households, which is intended to encouraging these women to create a productive enterprise. We have reasonably good data on the cost of this program to the NGO that has implemented it. Our question: how can we best measure the benefit?

We consider this question from the point of view of a given household. We think of this household as solving a dynamic program by simultaneously making decisions regarding consumption, investment, occupation, and production. All of these decisions are tied together by a quantity Ligon (2016) calls 'neediness', which is simultaneously equal to the marginal benefit of additional consumption expenditures, time, investment, and inputs to production. We use data on disaggregate household expenditures and methods devised by Ligon (2016) to measure changes in the logarithm of household neediness.

We find that the program results in a statistically significant 0.21 standard deviation reduction in the average log neediness of the treatment group relative to a control group, mirroring a 6.5 SSP (South Sudanese Pounds; about $1.62 USD) increase in the subset of daily household expenditures we observe. Other changes can be interpreted using our model, even when they're not necessarily predicted by that model. Those changes include some increase in both the number and value of productive assets held by the treated households, and a substitution away from casual agricultural labor into more skilled forms of market labor, self-employment, and perhaps leisure. Importantly, our estimates of these other changes can use estimated household neediness as an additional household covariate, which gives us a simple way to distinguish between 'wealth' and 'substitution' effects due to the treatment.

Background on 'TUP' and Related Interventions

Impoverished women in underdeveloped regions tend to be involved in low-return occupations, and frequently face both financial and human capital constraints (A. V. Banerjee and Duflo, 2007). A set of programs designed specifically to reduce poverty typically aim to alleviate these constraints simultaneously is the "ultra-poor graduation" framework, in which very poor individuals are offered both physical capital and some form of training or education to promote a particular kind of microenterprise activity. Broad outcomes for similar programs are described in (A. Banerjee et al., 2015). Bandiera et al. (2017) describes a large ultra-poor graduation initiative implemented in Bangladesh known as the "Transfers to the Ultra-Poor" (TUP) program. The program was implemented in 2007 by BRAC (cf., `www.brac.org`). Exploiting the randomized pattern of expansion, the study found persistent impacts on productivity, earnings, and participation in microenterprise.

Subsequently, BRAC decided to pilot a TUP program near the town of Yei in South Sudan. This paper uses randomized enrollment to evaluate the effects of this pilot program over the course of a year.[1] In late 2013, the program gave 249 women start-up capital at a marginal cost of around $240. Participants received some form of livestock, agricultural material, or retail inventory. They then participated in training specific to the assets provided and were given periodic food support valued at $110.

The TUP program in South Sudan is a pilot program. As with other programs of its kind, it consists of four phases: targeting and selection, training and enterprise selection, asset transfers, and monitoring. Each of these phases is modeled after the process in the original program in Bangladesh, but has been modified in notable ways based on local conditions.

Targeting and Selection

The TUP program in Bangladesh targeted women based primarily on a participatory appraisal activity in which community members used subjective means to assign households to different wealth quantiles. By contrast, the TUP program in South Sudan relies primarily on a set of inclusion and exclusion criteria based on wealth correlates taken from a community-wide survey, de-emphasizing relative measures of poverty in favor of absolute criteria.

Targeting guidelines include characteristics correlated with poverty as both exclusion and inclusion criteria. Surveyed households are excluded on the basis of having a salaried worker in the household or participation in another NGO program. Participation is also limited to women with access to cultivable land, since this is necessary for some of the TUP enterprises. Of these women, BRAC identified as eligible 650 who fit at least three of the following criteria: (i) the household head works as a day laborer; (ii) the household has two or more children; (iii) at least one child is working; (iv) the household has fewer than three rooms; and (v) the household includes an adult female who has not completed secondary school.

Eligibility was established in a census conducted in April of 2013. A baseline survey was then conducted among eligible women in June and July, which provided stratification data for the random selection of 250 women into the TUP program, with 375 remaining as controls.

[1] A more complete description of the experiment and the program may be found in Chowdhury, Collins, et al. (2015)

Training and Enterprise Selection

Of the eligible households, 250 were randomly selected to participate in the TUP program. After a general orientation to familiarize them with the program overall, each client was asked their preference over a menu of possible business types, which included selling dry fish at market, raising goats, raising ducks, and growing maize. BRAC set the number of participants in each group beforehand, ensuring that many but not all participants received their preferred asset type. Next, clients were enrolled in business skill training. Some of this training is program-wide, such as basic and financial literacy, though most of it is specific to the type of asset provided. Training occurred over four days at BRAC's own office or demonstration farm.

Asset Transfer and Monitoring

The standard program then provided clients with productive assets, with an effort to keep the market value of transferred goods constant across enterprises. In late 2013, each client in each enterprise group received assets valued at roughly $240.

After transfers were made, BRAC also provided weekly food transfers (bags of maize or maize flour) during group meetings. This was intended to ease clients' household budgets, compensate them for their time at trainings, and encourage them not to sell productive assets before their businesses got off the ground. These food transfers continued until about a month before the follow-up survey, and were valued at roughly $110 per client, raising the value of physical transfers to $350. BRAC estimates a marginal cost for an additional client equal to the value of transfers plus 10–20% of this in delivery and administrative costs. Initial intensive training sessions later gave way to monitoring and mentorship from local staff, as well as small support groups consisting of 8–12 clients, such as those found in BRAC's microfinance programs. These group meetings were ongoing when the final round of data was collected.

Data and Selection

Our data comes from three principal sources. First is a census of adult women proximate to BRAC's regional office in Yei, which was conducted in April of 2013. From this census a subset of 745 'eligible' women was identified, who were then selected to be surveyed in a second 'baseline' survey conducted in June and July of the same year. This baseline identified

649 of the eligible women, who were stratified by baseline asset holdings, participation in small trade and agriculture, and number of income earners, with 250 households being randomly selected. A third follow-up survey was subsequently conducted in July of 2014.

The first round of data collection consisted of a census of women in households within a six kilometer radius of the regional BRAC office. These women typically live on small plots of land with several small, mud, one-room buildings with thatched roofs. Eighty percent of surveyed women are between the ages of 20 and 40, with between one and three children.

The census survey was designed to establish program eligibility. BRAC's approach of selecting on a range of 'correlates' of poverty is designed to be less costly than the more intensive community-based ranking exercise used in the Bangaladesh program, raising the question of targeting effectiveness. Do the eligibility requirements sucessfully separate out an especially poor group of women, and does it avoid excluding women who should be eligible? Of the 1,279 surveyed households, 58% met all of the eligibility requirements. A straightforward comparison of the sample averages between the selected and non-selected groups indicates that selected households are 17% less likely to have paid work, have fewer durable assets and less livestock, and are more likely to be eating sorghum, which is typically regarded as low-quality food. Most selected women work either as a housewife or in small-scale agriculture. Eighty percent lived in households with some agricultural output, 35% had some poultry or livestock, and roughly 36% were involved in small trade or retail. Average reported daily consumption expenditures amounted to roughly \$1.50 USD per person.

Summary statistics for surveyed eligible women are presented in Table **??**, by treatment group. The table provides means of various outcome variables at baseline. The column "N" indicates the number of non-zero values across the entire sample; the column "Diff." gives the difference in means across these two groups, while p is related to a test of the hypotheses that "Diff." is equal to zero.

Though the kind of information presented in Table **??** is more useful for thinking about magnitudes than it is for 'balance' between the two randomly assigned groups, it's nevertheless true that mean values for these groups are generally similar. Only one of the differences we compute is significant by the standard of a sequence of t-tests and 95% level of confidence, and this difference is instructive. It comes in the calculation of the average value of sheds, where the control group happens to have a total of 8 sheds, while treatment group has only four; further, though all of the households in the control group happen to report a that their sheds have a positive value, only one of the four shed-owning households in the treatment group does so. The probability of some kind of imbalance along these lines happening for *some* variable is quite high, and of course this is no kind of evidence against the quality of the random number generator used to manage the assignment. Nevertheless, the initial difference should be kept in mind, if only because (as we'll see in the results below) "Sheds" are one of the outcomes which seem to be affected by the TUP program.

An analysis of targeting effectiveness in Chowdhury and Morel (2014) employs a principle component index developed by the Consultative Group to Assist the Poor (CGAP). They find that that roughly half of the selected individuals are in the bottom quartile, and nearly all are poorer than average for their community. Exclusion criteria based on NGO participation and lack of land ownership exclude a significant number of relatively poor women, suggesting that this targeting method has sacrificed some targeting effectiveness for the sake of program structure.

After the original census, two surveys (a "baseline" and "follow-up") were conducted in the summers of 2013 and 2014, respectively. These surveys contained modules on enterprise and income-generating activity, household composition, food security, and consumption of a range of food and non-food goods.

Among the 745 households identified as eligible in the census, enumerators were able to locate and interview 649 in the baseline survey in July 2013. It was using this baseline that households were stratified by potentially important characteristics and randomly selected for enrollment. Asset transfers and training began in December of 2013. In total, 554 of these were located and interviewed in the follow-up survey in July 2014.

Since BRAC had kept in much closer contact with the TUP participants in the intevening months, attrition is a source of concern.

A Modest Model

Bandiera et al. (2017) offer a simple static model of the behavior of an individual. The model itself is a version of an agricultural household model, of the sort discussed in **singh-etal86** but with a focus on occupational choice, which Bandiera et al. identify as a critical feature in their study in Bangladesh.

Here we adopt the model of Bandiera et al. (2017) more or less wholesale, but extend it to allow for both time and uncertainty. The spirit of this extension is very similar to the "exogenously incomplete" model devised by

karaivanov-townsend14 However, we interpret it as a model of *household*, rather than individual behavior, since most of the data we have to test this model is observed at the household level. This turns it into a dynamic model involving both asset accumulation and occupational choice, and we show how this extension allows us to nicely tie together the production, consumption, and investment decisions made by the household.

Our notation is adapted from Bandiera et al. (2017), with modest changes to generalize and allow for time and uncertainty. Households are indexed by $j \in \mathcal{J} = \{1, 2, \ldots, J\}$. In each period the economy is in a state $s \in \mathcal{S} = \{1, \ldots, S\}$; these states evolve according to finite-state Markov process with the probability of transitioning from state s to state r given by π_{sr}. Time is discrete, and in each period t the household derives utility from consumption of an n-vector of consumption goods C and from leisure R. Utility within a period can also depend on household characteristics θ. Bandiera et al. (2017) interpret this θ as skills, but we'd interpret it more broadly to include, e.g., household size and composition. Then momentary utility is given by $U(C, R, \theta)$, with this utility function increasing, concave, and continuously differentiable. The household makes plans over an infinite horizon, with utility in the next period discounted by a factor $\beta \in (0, 1)$.

In each period the household allocates its time between leisure R, employment (by others) L, and self-employment S. All must be non-negative. We assume that no labor is hired in by the household (modifying the model to allow this would be straight-forward, but not empirically useful in our setting, as none of the households in our sample is observed to hire in labor). Earnings from employment depends on an individual and state-specific function $W_s^j(L, \theta)$. Income from self-employment involves a production process which depends not only on time allocated to this occupation, but also on the productive assets and a household-specific shock; household j's characteristics evolve according to a household-specific Markov process, so that $\theta_{t+1} = H_s^j(\theta_t)$ if the state at $t + 1$ is s.

Asset accumulation depends on initial assets K, the state-specific idiosyncratic price for new assets q_s^j, and stochastic, household-specific returns to holding those assets $Q_s^j(K)$ (e.g., think of livestock fertility and mortality). Borrowing is limited, but these limits may depend on the state and vary across households, so that $K_{t+1} \leq B_s^j(K_t)$ if the $t + 1$ state is s. The returns function Q_s^j is assumed to be weakly concave; both it and the borrowing limit functions B_s^j are also assumed to be increasing and continuously differentiable.

In any state s, given assets K, characteristics θ, and time spent in self-employment S, household j produces $F_{js}(K, S, \theta)$ units of the numéraire good, where we assume the F_{js} are increasing, weakly concave, and continuously differentiable. The cost of purchasing the consumption bundle C is taken to be $P^j_s(C)$ for household j in state s. In each period the cost of consumption plus net investment must not exceed income from employment and own production, so that household j faces the budget constraint

$$P^j_s(C) + q^j_s(K' - K) \leq F^j_s(K, S, \theta) + W^j_s(L, \theta), \tag{1}$$

where K' is a vector of the total assets invested for the next period.

Putting this altogether, we regard household j as solving the dynamic program

$$V^j_s(K, \theta) = \max_{C,S,L,K'} U(C, 1 - L - S, \theta) + \beta \sum_{r \in \mathcal{S}} \pi_{sr} V^j_r \left(Q^j_r(K'), H^j_r(\theta)\right) \tag{2}$$

subject to the budget constraint (1) (with which we associate the Karush-Kuhn-Tucker multipliers λ^j_s); to non-negativity constraints on consumption goods $i = 1, \ldots, n$, (with associated multipliers ν^j_i); non-negativity constraints for time allocation $S \geq 0$ and $L \geq 0$ (with multipliers η^j_S and η^j_L, respectively); and subject finally to the borrowing constraint $K' \leq B^j_s(K)$ (with multipliers μ^j_s).

Using lower case letters to indicate partial derivatives, the first order conditions then can be written

$$\begin{aligned}
C_i : u_i(C, R, \theta) - \nu^j_i &= p^j_{si} \lambda^j_s \qquad \text{for all } i = 1, \ldots, n \\
L : u_R(C, R, \theta) - \eta^j_L &= w^j_s \lambda^j_s \\
S : u_R(C, R, \theta) - \eta^j_S &= f^j_S \lambda^j_s \\
K' : \beta \sum_r \pi_{sr} v^j_r q^j_r + \mu^j_s &= q^j_s \lambda^j_s.
\end{aligned} \tag{3}$$

Here u_i denotes the marginal utility of consumption good i, and u_R is the marginal utility of leisure. Similarly f^j_s is the marginal product of S in production for household j, while $v^j_r = \frac{\partial V^j_r}{\partial K}(Q^j_r(K'), H^j_r(\theta))$ is household j's marginal valuation of an additional unit of realized capital in state r, and q^j_r is j's marginal return to investment in state r. In addition to these optimality conditions we have the envelope condition with respect to K,

$$v^j_s(K, \theta) - \mu^j_s b^j_s(K) = \lambda^j_s \left(q^j_s + f_{sK}(K, S, \theta)\right). \tag{4}$$

Now, the key variable which ties together all of these is the multiplier on the budget constraint, which measures the marginal benefit of having additional resources. Since this marginal value depends in turn on not only the state s but also the current values of (K, θ), we use (3) and (4) to implicitly write it as a function $\lambda_s^j(K, \theta)$. We have

$$\lambda_s^j(K,\theta) = \frac{u_i - \nu_i^j}{p_{si}^j} = \frac{u_R - \eta_L^j}{w_s^j} = \frac{u_R - \eta_S^j}{f_{sS}^j} = \frac{\beta \sum_r \pi_{sr} v_r^j q_r^j + \mu_s^j b_s^j}{q_s^j} = \frac{v_s^j - \mu_s^j b_s^j}{q_s^j + f_{sK}^j}. \tag{5}$$

In words, the household is allocating its resources to equate returns measured in terms of *utility* across different margins; none of these are returns in physical quantities that we can directly measure. "Utility return" would be an accurate way of describing these quantities: Taking each equality in (5) one at a time, λ_s^j is equal to household j's utility return of consuming an additional unit of good i (this holds for every $i = 1, \ldots, n$, of course); is equal to the utility return to taking an hour off from employment; is equal to the utility return to taking an hour off from self-employment; and is equal to the utility return to an additional unit of investment, which finally is equal to the utility value of having additional assets in the current state s.

But while "utility return to an additional unit of investment" may be accurate, we think the English language already has a suitable word: the variables λ_s^j measure the *neediness* of household j. When λ^j is high relative to those of other households, so is $(u_i^j - \nu_i^j)/p_i^j$, and household j stands in greater need of food; similarly when $\lambda_s^j > \lambda^j$ the household is particularly in need of labor; of investment; of consumption; of leisure.

The neediness variables λ_s^j have other interpretations as well. If we were to consider the static consumer's problem being solved by household j at each date state, then λ_s^j is equal to the partial derivative of the household's indirect utility function with respect to total consumption expenditures in state s.

Notice that the different expressions for neediness in (5) involve three different kinds of objects. First, there are some prices which may be directly observable in the data (e.g., prices of consumption goods; individuals' wages; purchase prices of assets such as livestock). Second, there are shadow prices that will *not* be directly observable; these include the key λ_s^j as well as multipliers on the non-negativity constraints and the multiplier on the borrowing constraint. Third, there are unknown functions, including the marginal utility functions (u_i, u_R) and the marginal productivities of assets and labor in the self-employment technology (f_{sS}^j, f_{sK}^j).

Modeling our experiment

We want to think now about how our experiment can be thought of in terms of the model of the households we've developed—only by putting the experiment "into" the model can we think coherently about how a household might react to the experimental treatments we introduce. Or as **rubin74** might put it, we think of putting the experiment into the model as the construction of a logical argument establishing circumstances under which only some particular variables should be expected to have a causal effect on particular dependent variables.

Accordingly, consider partitioning the space $\mathcal{S} = \mathcal{C} \cup \mathcal{E}$. Then for any state $s \in \mathcal{E}$ we begin our experiment (we can always specify S and choose the transition probabilities π_{sr} to ensure that we only start the experiment once). Further, let $T_0(s)$ and $T_1(s)$ be subsets of the index set of households, so that for $\hat{s} \in \mathcal{E}$ if $j \in T_0(\hat{s})$ then household j is assigned to a 'control group' in our experiment, while if $j \in T_1(\hat{s})$ then household j is assigned to a 'treatment group' which receives assets, training, and so on. Assignment is random if, for any pair of households $(j_0, j_1) \in T_0 \cup T_1$ each had an equal probability of being assigned to T_1.

In partitioning $\mathcal{S}$ into states where the experiment is conducted and states where it is not, we think of $\mathcal{C}$ as the set of 'counterfactual' states. Thus, for an 'experiment' state $\hat{s} \in \mathcal{E}$ there exists another 'counterfactual' state $\tilde{s} \in \mathcal{C}$ such that for any household $j \in T_1(\hat{s})$, the 'treatment' consists of an $\hat{K}$, a $\hat{\theta}$, and a $\hat{C}$ such that

$$Q^j_{\hat{s}}(K') = Q^j_{\tilde{s}}(K') + \hat{K}; \quad H^j_{\hat{s}}(\theta) = H^j_{\tilde{s}}(\theta) + \hat{\theta}; \quad \text{and } P^j_{\hat{s}}(C) = P^j_{\tilde{s}}(C - \hat{C}) \tag{6}$$

for all K', θ, and C. Note that we are *not* assuming that consumption or investment will be unchanged by the treatment; it would be surprising if they were not. The content of the assumption is that the technology producing returns to investment or the cost of a consumption bundle only be affected by the experiment in an additive way.

Further, we assume that for any household in the *control* group outcomes are the same in both the experimental state $\hat{s} \in \mathcal{E}$ and the counterfactual state $\tilde{s} \in \mathcal{C}$, or, for any $j \in T_0(\hat{s})$ that we have

$$Q^j_{\hat{s}}(K') = Q^j_{\tilde{s}}(K'); \qquad H^j_{\hat{s}}(\theta) = H^j_{\tilde{s}}(\theta); \qquad \text{and } P^j_{\hat{s}}(C) = P^j_{\tilde{s}}(C), \tag{7}$$

also for all K', θ, and C. Together, these two conditions just assert that our experiment only affects the treated, and give the effect of the treatment on treated households. Left unstated is a third assumption, that the treatment's effects on treated households are channeled solely through the transfers of $(\hat{K}, \hat{\theta}, \hat{C})$.

This notation may seem unnecessary, if our goal is simply to discuss what it means to have experimental treatments and random assignments. But now we ask—within the context of the model—what effects we'd expect from the experimental treatment. There turns out to be a very simple way to measure these. Equation (5) implies that changes in any aspect of the household's economic behavior (consumption, labor supply, production, credit constraints) will be reflected in the neediness λ_s^j, so one way of thinking about what we want to measure experimentally is the ratio $\lambda_{\hat{s}}^j/\lambda_{\tilde{s}}^j$ for $j \in T_1(\hat{s})$. This ratio would tell us the proportional difference in utility returns for a treated household due to the experiment.

Viewed through this lens, the expected "average treatment effect" on (the log of) neediness can be written as

$$\text{ATE} = \left(\frac{1}{\#T_1(\hat{s})} \sum_{j \in T_1(\hat{s})} \log \lambda_{\hat{s}}^j\right) - \left(\frac{1}{\#T_1(\hat{s})} \sum_{j \in T_1(\hat{s})} \log \lambda_{\tilde{s}}^j\right).$$

The problem, of course, is that we can't observe the λ^js in the counterfactual state $\tilde{s}$. But using the assumption (7) and the assumption of random assignment, it follows that

$$\left(\frac{1}{\#T_1(\hat{s})} \sum_{j \in T_1(\hat{s})} \log \lambda_{\tilde{s}}^j\right) = \left(\frac{1}{\#T_0(\hat{s})} \sum_{j \in T_0(\hat{s})} \log \lambda_{\hat{s}}^j\right),$$

so that we have the average treatment effect on the logarithm of neediness given by

$$\text{ATE} = \left(\frac{1}{\#T_1(\hat{s})} \sum_{j \in T_1(\hat{s})} \log \lambda_{\hat{s}}^j\right) - \left(\frac{1}{\#T_0(\hat{s})} \sum_{j \in T_0(\hat{s})} \log \lambda_{\hat{s}}^j\right). \tag{8}$$

This now only involves needing to observe outcomes in realized states.

Empirical Strategy

Notice that the utility returns in (5) involve three different kinds of objects. First, there are some prices which may be directly observable in the data (e.g., prices of consumption goods; individuals' wages; purchase prices of assets such as livestock). Second, there are shadow prices that will *not* be directly observable; these include the key λ_s^j as well as multipliers on the non-negativity constraints and the multiplier on the borrowing constraint. Third, there are unknown functions, including the marginal utility functions (u_i, u_R) and the marginal productivities of assets and labor in the self-employment technology (f_{sS}^j, f_{sK}^j).

These last unknown functions depend on variables which we may be able to observe. Consider in particular a good i of which household j consumes a positive quantity. This gives us the equality $u_i(C_s^j, R_s^j, \theta_s^j)/p_{si}^j(C_s^j) = \lambda_s^j$. This equation holds for all states and for every good $i = 1, \ldots, n$ with positive consumption, so it must hold in any realized state. To celebrate this fact we simplify notation, letting t indicate the state that occurs at that date, so that we have $u_i(C_t^j, R_t^j, \theta_t^j)/p_{ti}^j(C_t^j) = \lambda_t^j$ to indicate this relationship at date t and state s_t. With this simplified notation we also introduce some additional assumptions: first, that utility from leisure is additively separable from utility from consumption, or that $u_{iR} = 0$. Second, we partition the index set of households into sets of households that reside within m distinct areas; i.e., we take $\mathcal{J} = \mathcal{J}_1 \cup \mathcal{J}_2 \cdots \cup \mathcal{J}_m$. Then we assume that within each of these m areas households all face the same prices for consumption goods, or that $p_{ti}^j(C^j) = p_{ti}$.

Now, with this we return to the equation defining the expected average treatment effect (8). Using the fact that for goods consumed in positive amounts we now have $\lambda_t^j = u_i(C_t^j)/p_{ti}$, we substitute into (8), obtaining

$$\log u_i(C_t^j, \theta_t^j) = \log p_{ti} + \sum_g (j \in T_g) \overline{\log \lambda_t}^{T_g} + \epsilon_{ti}^j,$$

where $\overline{\log \lambda_t}^{T_g}$ is the average value of the $\log \lambda$s for treatment group T_g, ϵ_{ti}^j is a residual which, by (8) will be equal to $\lambda_t^j - \overline{\log \lambda_t}^{T_g}$ if household j is a member of treatment group g.

Estimating Marginal Utilities

If we observed prices p_{ti} and happened to know the values of $\log u_i(C_t^j, \theta_t^j)$ we could go ahead and straight-forwardly estimate the average treatment effect we're interested in. Of course we do not know the latter. However, we do observe expenditures on multiple kinds of food and other non-durable consumption. If we re-arrange the first equality in (5) and use our assumption that leisure is separable in utility then we can write the vector of marginal utilities of consumption as

$$u(C, \theta) = p\lambda.$$

Next, following the long line of work following **heckman-macurdy80** and **macurdy83** we parameterize the log of marginal utilities, assuming $\log u(C, \theta) = \Gamma \log C + \zeta\theta$, where Γ is an $n \times n$ matrix of parameters having full rank, and where ζ is an $n \times l$ matrix.[2]

With this parameterization, we can write

$$\Gamma \log C + \zeta\theta = \log p + \log \lambda.$$

This is getting close to something we can estimate, but we have data on the value, not quantity, of food consumption. Let $X_i = p_i C_i e^{\epsilon_i}$, where ϵ_i is some measurement error, be the value of expenditures on consumption good i. Then rearranging, we have the system of equations

$$\log X = (I + \Gamma^{-1}) \log p - \Gamma^{-1}\zeta\theta + \Gamma^{-1} \log \lambda + \epsilon. \tag{9}$$

This system is what we might call a Frischian expenditure system (**browning-etal85**). Ligon (2016) provides methods for estimating this system; showing that with data on at least some expenditures and household characteristics one can obtain not only estimates of the parameters but also of the neediness measures $\log \lambda$ (up to a normalization).

Differences in the mean of the inferred neediness $\log \lambda$ between treatment and control group will be equal to the average treatment effect that most interests us, but we can also obtain estimates of this effect directly from (9). Consider the following standard ANCOVA specification of the sort championed by McKenzie (2012). Key features of the standard specification include a set of fixed effects for time and place; linear covariates as controls; baseline values of the outcomes as additional controls; and finally a collection of average treatment effects, which are ordinarily the object of interest. We adopt just such a specification, letting X_{ti}^{jga} be expenditures on good i in period t for a household j in area a and in treatment group g. Then we can write

$$\log X_{ti}^{jga} = \alpha_{ti}^a + \tau_i^g + \delta_i(\theta_t^j - \bar{\theta}_t^g) + \gamma_i \log X_{t-1,i}^{jga} + u_{ti}^j. \tag{10}$$

Now, in the standard interpretation of this regression $\tau_i^1 - \tau_i^0$ will be the average treatment effect on expenditures on good i, while the terms involving the θ and the lagged outcomes improve power by accounting for covariance between household characteristics and expenditures (and perhaps accounting for unbalanced outcomes in the baseline). Because the latent variables α_{ti}^a capture differences in means across areas as well as goods and periods, it is

[2] The development and justification of this particular preference structure is discussed in Ligon (2015).

the variation that is within an area that is being exploited here to estimate the τ_i^g.

This ANCOVA specification has an intimate relationship with the Frischian expenditure system (9) which allows us to give a structural interpretation of the reduced-form ANCOVA. In particular, the good-area-time effects α_{ti}^a estimate the effects of changes in prices on expenditures, the vector $(I + \Gamma^{-1}) \log p_t$ in (9). The terms involving the idiosyncratic covariate characteristics $\delta_i \theta_t^j$ match up with the effects of characteristics on expenditure demand $\Gamma^{-1} \zeta \theta_t$, while the average treatment effect estimates $\tau_i^g = \beta_i (\overline{\log \lambda_t}^{T_1} + \zeta_i \bar{\theta}_t^g)$, where the β_i are equal to the row sums of the matrix Γ^{-1}.

So, the average treatment effect in these ANCOVA regressions with log consumption expenditures as outcomes can be interpreted as the product of a demand elasticity and neediness. Further, these can be decomposed, giving us both parameters useful for understanding demand systems and Engel curves and measures of neediness useful for measuring welfare. Even better, these neediness parameters are key to understanding the connections between consumption, investment, production, and occupational choice, and allow us to measure the extent to which an intervention operates via its effects on wealth versus effects it may have on production or occupational choice.

What assumptions have we had to make in order to give this 'structural' interpretation to our average treatment effects? There are only really four 'structural' assumptions we need to make. All pertain to the household's utility function, and seem fairly unobjectionable, or at least conventional in applied empirical work. The first two are that the household's utility function is intertemporally separable and von Neumann-Morgenstern; these allow us to think of the household as solving a 'two-stage' intertemporal budgeting problem (**gorman59**). The third is that the utility function is separable in consumption and leisure; the last that Frischian consumption expenditure elasticities are constant. This is much less restrictive than what is usually assumed in parametric Engel curve estimation.

Results

We offer results in three parts. First we discuss the average treatment effect on consumption expenditures, and use estimates of this effect across different consumption goods to estimate the average treatment effect on neediness, as well as the distribution of neediness in both treatment and control groups.

Second, we consider outcomes related to both the number and value of assets held by the household. The estimates of household neediness previously obtained can be used to control for the effects of treatment on wealth. The link between these holdings and the model is considerably looser than in the case of consumption, but certainly both the average number and value of assets we observe is positively affected by TUP. The distribution of resources *across* different assets is less easy to predict, but we see large average treatment effects on the value of livestock owned, consistent with the focus of TUP on increasing livestock ownership for treated households which choose this. We finally examine self-employment and occupation. There are quite large effects on participation in self-employment, broadly consistent with what one would expect from a purely administrative analysis (BRAC gave animals to so many treated households, of which a certain known number already had significant livestock holdings). Finally, we turn to a broader and more detailed notion of occupation: here we see members of treated households leaving housework and casual agricultural employment. Some of these people seem to enter non-agricultural day labor, but it's less clear what they're doing instead. However, one possibility consistent with both the evidence and the model is that people in the average treated household move out of low-skill market employment, instead increasing labor in more skilled market employment, and possibly increasing both household leisure and participation in home production.

Consumption Expenditures and Neediness

Our principal results may be found in Table 1. As suggested above, these are 'ANCOVA' estimates of the effects of being in either of the two groups "CTL" (Control) and "TUP" (targeted ultra-poor), the latter of which received assets, training, and food subsidies. Other household characteristics included as controls are the number of people in the household as well as the number of children. Baseline values of expenditure were included as an additional control, with a complete set of village/area fixed effects (constrained to sum to zero). Where recorded values of consumption expenditure are equal to zero, these are regarded as missing and dropped from the analysis. There are two motivations for this treatment of zeros: first, at an entirely practical level, our dependent variable is the logarithm of expenditures, which is undefined at zero. But second, if a household is at a corner when it chooses a particular consumption item, then the first order condition in (3) for that consumption good won't be correct (we'd be missing a multiplier related to non-negativity). By simply dropping observations for goods where consumption is zero we are effectively dropping observations where expenditures do not correctly reveal household neediness. In any event, treating zero consumptions as missing results in our 'panel' of goods by households being unbalanced, so we estimate the ANCOVA equations as a single system.

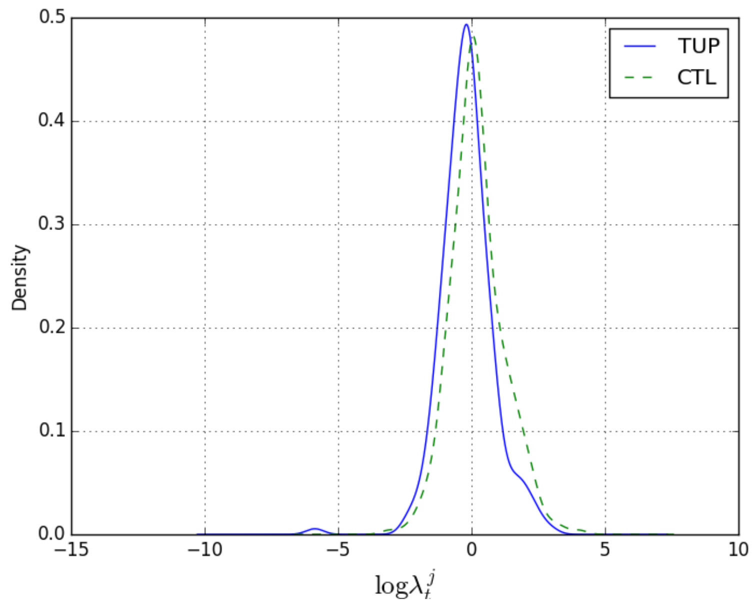
We see in the first instance that the average treatment effect for TUP participants on the value of these consumption goods are almost uniformly positive, and significantly positive (three stars indicates a 99% level of confidence, two stars 95%, and one star 90%) for 8 of 14 different goods. The exceptions are informative. The estimated sign for the difference in the value of salt consumption is negative, but very small and insignificant, consistent with the view that the income elasticity of salt is very small for this population. The other negative difference is for transportation expenditures. We've included transportation in this table with the idea that transportation services enter the utility function. But another view is that transportation is an expense associated with employment or production. One of the principal findings of Bandiera et al. (2017) is that TUP participants in Bangladesh switched from wage employment to self-employment, which one presumes may have reduced the demand for transportation, and it's very possible that something similar is happening here.

The differences in average treatment effects are also highly jointly significant: a test of the hypothesis that these are all zero yields a χ^2_{14} statistic of 75.43, with an associated p-value less than 10^{-9}.

#+end$_{\text{example}}$ #+end$_{\text{example}}$

Now recall that according to our model each treatment effect is equal to the product of an elasticity parameter β_i and average log neediness for the group. By redefining the 'treatment groups' so that there are 554 of them, each group consisting of exactly one household, we can obtain estimates of *individual* effects on the value of goods consumed, or $\beta_i \log \lambda^j_t$. We adopt the normalization that $\mathrm{var}(\lambda^j_t) = 1$, and scale the elasticities β_i so that their sum weighted by expenditure shares is equal to one. Scaled in this way these Frisch elasticities would be equal to Marshallian income elasticities provided each household had a coefficient of relative risk aversion of one. These Frisch elasticities are reported in the final column of Table 1. As the differences in estimated average treatment effects would suggest, all but salt appear to be normal goods. Because the scale is only identified by an arbitrary normalization, we can't say based on this evidence what goods are necessities or a luxuries. But we can say that fuel, transport, soap, and cosmetics (all the non-food items) appear to be the four most income elastic goods, followed by vegetables, sugar, cooking oil, and cereals. And whatever the scale, the least income elastic good seems to be salt, with an elasticity orders of magnitude smaller than that of the most income elastic goods.

We now turn our attention to the relationship between (log) neediness and treatment. The final row of Table 1 reports the mean values of neediness for both CTL and TUP groups. As with the individual goods, there's a highly significant difference between these means. Because the standard deviation of the pooled $\log \lambda_t^j$ is equal to one (because of our normalization), we can interpret the difference between these means as evidence that neediness for the treatment group fell by a highly statistically significant 0.21 standard deviations relative to the control.



Of course, knowing just that the mean neediness is less in the TUP group tells us little about how changes in welfare are distributed across households. Giving assets to would-be entrepreneurs might have very disparate effects on welfare, as many standard models of entrepreneurship predict (**banerjee-newman93**; **paulson-etal06**; **karaivanov-townsend14**) and as a number of recent experiments tend to confirm (**demel-etal08**; **mckenzie-woodruff08**; **fafchamps-etal11**). Perhaps some fortunate or skilled few benefit hugely while others experience little benefit.

To understand the distribution of benefits in our setting, consider Figure **??**, which presents kernel density estimates of the distribution of $\log \lambda_t^j$ across households conditional on whether they are members of either the treatment or control group. Two things are visually evident from the figure. The first is that average neediness for the TUP group is smaller than it is for the control group. Related, the second is that the distribution of welfare gains for the TUP group may first-order stochastically dominate the distribution for the control group: it's not just that mean neediness falls, it's that mean neediness appears to fall for *everyone*, save for the least needy (consistent with the idea that the utility function U is concave).

Other Testable Predictions of the Model

The model presented in model is written so as to be quite general in some dimensions, and we lack the data to construct structural estimates of the full model. However, with only fairly modest maintained assumptions we can estimate parts of this model, and test others. For example, the previous section has outlined methods for estimating a parametric utility function and corresponding demands for non-durable consumption, which we exploit below. We have also described an approach to measuring the effects of the TUP program on average household welfare.

With what we've been able to estimate, we'd like to be able to use the model to ask two counterfactual questions about the TUP program. The first: what size of cash transfer would yield the same welfare benefits as what we observe from the experiment? We'll call this the "welfare-equivalent cash transfer." The second: in what ways is the behavior induced by the TUP program different from what we'd expect from the welfare-equivalent cash transfer?

In principle, the welfare-equivalent cash transfer can be calculated by treating our estimated

Results

We offer results in three parts. First we discuss the average treatment effect on consumption expenditures, and use estimates of this effect across different consumption goods to estimate the average treatment effect on neediness, as well as the distribution of neediness in both treatment and control groups. Second, we consider outcomes related to both the number and value of assets held by the household. The estimates of household neediness previously obtained can be used to control for the effects of treatment on wealth. The link between these holdings and the model is considerably looser than in the case of consumption, but certainly both the average number and value of assets we observe is positively affected by TUP. The distribution of resources *across* different assets is less easy to predict, but we see large average treatment effects on the value of livestock owned, consistent with the focus of TUP on increasing livestock ownership for treated households which choose this. We finally examine self-employment and occupation. There are quite large effects on participation in self-employment, broadly consistent with what one would expect from a purely administrative analysis (BRAC gave animals to so many treated households, of which a certain known number already had significant livestock holdings). Finally, we turn to a broader and more detailed notion of occupation: here we see members of treated households leaving housework and casual agricultural employment. Some of these people seem to enter non-agricultural day labor, but it's less clear what they're doing instead. However, one possibility consistent with both the evidence and the model is that people in the average treated household move out of low-skill market employment, instead increasing labor in more skilled market employment, and possibly increasing both household leisure and participation in home production.

While A. Banerjee et al. (2015) do not estimate neediness according to our procedure, they both find comparable average treatment effects on the sum of consumption for the expenditure categories they measure. The consistency of average treatment effects across the distribution also mirrors the distributional results in A. Banerjee et al. (2015). Bandiera et al. (2017) find increases in consumption among the ultra-poor, but do not report short-term estimates like the ones we present here.

Assets

We have seen that the TUP treatment has a positive and significant effect on consumption expenditures and leads to a significant and sizable reduction in neediness. From (3), we might expect this reduction in neediness to also show up in investment and assets. Of course, since the TUP program revolves around actually giving assets to treated households, it may appear obvious that assets should increase. But in fact this is not at all a foregone conclusion. From (3) we have an indication that a decrease in neediness (such as the one we measured above) may decrease the marginal value of assets (consistent with an increase in the holdings of those assets). But the assets may be valued simply because they can be sold to finance increased consumption or leisure—a pure wealth effect, which would be reflected in a reduction in neediness λ_s^j. This use certainly improves welfare, and may help extend the benefits of the TUP program to future periods, but this is a role that would be played equally well by a (simpler) financial transfer. For the asset transfers to play an important role in *production*, we should look for the effects they may have on the production function, where a transfer of particular assets may either directly enter the production function, or may help to relax a borrowing constraint (perhaps by serving as a security), allowing the household to finance the purchase of other inputs to production should it wish.

Here we explore the effect of the TUP program on physical assets by estimating the average treatment effect on both the number (Table 2) and value (Table 3) of different sorts of assets.

Both sets of regressions are estimated just as the average treatment effects for consumption was, with the sole difference that reports of "zero" assets (whether count or value) were not treated as missing data. In particular, we include a complete set of village fixed effects, constrained to sum to zero; baseline (2013) values were included as controls, along with the number of people and number of children in the household.

Results for the *number* of assets are reported in Table 2. Consider first the column labeled "Diff. (no $\log \lambda$)", which excludes estimated neediness from the list of controls. In contrast to the case of consumption goods, few of these individual items are significant: at a 90% level of confidence the TUP program results in significant increases only in the number of chairs and tables, mobile telephones, poultry, and the number of sheds. The hypothesis that none of these differences is significant is rejected; it yields a χ^2_{15} statistic of 142.7 with an associated p-value less than 10^{-9}. The finding that treatment results in more poultry[3] and sheds is unsurprising, as some of the enterprises selected in the TUP program explicitly involved duck acquisition and shed construction. The finding that furniture or mobile phone purchases are significant is less expected, but it is perfectly possible that the operation of a small businesses might benefit from having a mobile or a table, of course. Sewing machines have obvious productive uses, but none directly related to the enterprises the TUP program was designed to encourage. Other surprises are that some other outcomes do *not* have significant treatment effects. In particular there is no significant effect of the TUP on the number of small animals owned—this is surprising as 35 of the treated women chose to rear goats (these out of a total of 246 treated households that received some kind of asset).

A deeper insight into the mechanisms behind asset acquisition can be gained by re-estimating the ANCOVA regression behind Table 2, but this

[3]Some care should be taken in interpreting the magnitudes of the effects on poultry, as the elicitation of both the number and value of poultry was handled slightly differently in the 2013 baseline and the 2014 follow-up survey.

time controlling for neediness. The coefficients on $\log \lambda$ are reported in the final column of the table; this allows us to see that less needy households are more likely to have more assets, as without exception the estimated coefficient on neediness is negative. Of these, 11 of 15 are significant at a 90% level of confidence. But perhaps more importantly, we can now re-interpret the effects of the TUP program on the number of assets held *controlling* for a measure of wealth; the relevant estimates are reported in the column labeled "Diff. (with $\log \lambda$)". When we control for neediness, we see that the increase in chairs and tables or mobile phones appears to be due only to the wealth effect of the TUP program ($\log \lambda$ is significant in these regressions, but the estimated average treatment effect is no longer significantly different from zero). The coefficients on poultry and small animals remain significant, as we'd expect. The coefficients on mosquito nets we do not understand: they suggest that less needy households are more likely to own mosquito nets, but that the TUP treatment resulted in fewer mosquito nets.

Referring to Table 3 may help to resolve the puzzle of the missing small animals; treatment is associated with a significant increase in the *value* of both poultry and small animals. No other differences are individually significant at the 95% confidence level, but we easily reject the hypothesis that *none* of these differences in value is significant. To summarize: the average treatment effect on the value of different assets is significant for poultry and small animals. Perhaps it would be surprising if this was *not* the case, since the treatment involves giving ducks and goats to more than half of the treated households, but the fact that those ducks and goats haven't been eaten or sold six months after the asset transfers provides suggests that the asset transfers affect production as intended, and serve as more than just a store of wealth. Both A. Banerjee et al. (2015) and Bandiera et al. (2017) find significant effects on total asset holdings in the short term, as well, with a similar emphasis on livestock and other productive assets.

Employment and Occupation

The model we've described above requires an explicit decision from the household about the allocation of time between leisure, production, and employment. Our results related to consumption expenditures and neediness tell us that TUP households' neediness falls, and our model suggests that we should expect this decrease in neediness to be related not only to consumption but also time allocation. Equation (3) describes the relation, with

$$\log \lambda_s = \log(u_R - \eta_L) - \log w_s = \log(u_R - \eta_S) - \log f_{sS}.$$

Suppose that labor and consumption are separable, as assumed in our calculation of neediness. There are four cases to consider.

First, it might be the case that the household supplies no labor at all, so that $\eta_L^j \eta_S^j > 0$. In this case a small decrease in neediness caused by an increase in K will not affect the marginal utility of leisure, u_R, and cannot affect the 'wage' w_s^j, so that the entire decrease in neediness will (from the point of view of employment) be reflected in the shadow cost of not being able to take *more* leisure. Taking the appropriate derivatives in this case yields $d\eta_L/d\lambda = w$; note that only 'shadow' quantities are changed in this case. For the second equality, the household in this case is still assumed to be at a corner in leisure, so u_R will remain unchanged, and changes in λ will be reflected in changes in η_S, but also in increases in the marginal product of labor f_{sS}. Under reasonable specifications of the production function F this makes perfect sense: the provision of greater capital inputs to home production are apt to yield exactly this sort of response.

Second, consider the case where the household is at a corner in L, because the wage w faced by the household is less than the marginal product of labor from own production f_S given assets K. In this case for the second equality we may expect to see increases in the marginal product of labor f_S. The effect on leisure is indeterminate, depending on the curvature of F.

Third, consider the case where the household is at a corner in S, because the wage w faced by the household exceeds the marginal product of labor from own production f_S. In this case there may be an increase in own production, but if wages are fixed there will certainly be an increase in leisure.

Finally we come to the fourth case, in which the household supplies labor both in the market and in own-production. As in the third case, if wages are fixed leisure must increase, resulting in a decrease in u_R. But the reduced labor previously supplied to the market can be divided between leisure and additional self-employment, though whether and how much time in self-employment increases will depend on the curvature of the production function.

Thus, the model at this level of generality leaves us with only some weak predictions about outcomes. The main prediction we have is that a small decrease in neediness will (weakly) increase leisure, *unless* the household is initially only self-employed, in which case the change in leisure is ambiguous. Note also that even this weak prediction hinges on the marginal product of labor in the market being taken as given—this may not be the case for, e.g., piecework labor, where decreasing marginal products may be the rule.

So, compared to the case of consumption the model gives us much less guidance regarding what to expect in terms of employment, and we can turn to the results without being surprised.

Table 4 provides the unsurprising results regarding the effects of the program (which we've established results in a decrease in neediness) on self-employment. The respondent is said to be "in business" if they claim to have been involved in any non-farm self-employment in the past year ("non-farm" here explicitly means agriculture, livestock, and poultry). They are "cultivating" if they report actively cultivating any land, whether owned, rented, or common. Being occupied in rearing livestock is taken here to mean that the respondent reports owning total livestock valued at more than 50 South Sudanese Pounds (roughly 12 USD at the time of the survey).

So how do the less-needy households of the TUP program change their self-employment? Noting that these questions elicit *participation* in different forms of self-employment, we see significant increases in participation both in business and livestock. In terms of the model, these changes have more to do with the multiplier η_S than they do with hours spent in self-employment, but the average treatment effects seem robust; we can think of this as being leading to roughly 20 per cent (5% plus 17% less some who leave cultivation) more of TUP households moving into self-employment than out of as a consequence of their participation in the program. The TUP program had a total of 216 participants, of which 116 chose to receive livestock, so our estimated participation rate here is slightly less than half of what the administrative data would tell us *provided* that none of these households had any livestock previously, but from our baseline data we can see that in fact 67 TUP households did previously have livestock, suggesting a reasonable match between the changes in participation expected from administrative data and the estimated average treatment effect. This may seem of little note, but evidence that people who were given ducks have chosen to raise them instead of eating or selling them is important to have.

In a separate part of the survey we elicit occupational information for all members of the household, where "occupation" can include not only various forms of self-employment, but also many other possible uses of one's time. Though it is possible to report several occupations for each person, in only three instances was more than one occupation reported for a single person. Of the 4304 individuals in the sample households in 2014, occupations are reported for 3886. The rather old or the very young are heavily over-represented among those with no reported occupation.

We add up the total number of people in each of 35 occupations in each household. Table 5 reports, in its first column, the total number of people in each occupation (for occupations with more than 30 people). This evidence on occupation paints either a disturbing picture of the economic environment in South Sudan or an encouraging testament to BRAC's ability to identify and target the 'ultra-poor': of people in the top twelve occupations listed, less than 22 per cent are engaged in what we might think of as remunerated productive work (students and housewives work, but aren't remunerated; beggars are remunerated but aren't productive). Of this 22 per cent, 61 percent are engaged in cultivating household land, either for home consumption or for sale. An additional 12 per cent are reported to work in their own small business, while the balance (26 per cent) sell their labor to others.

Because these occupations are reported for all household members they include children, and in the second column of Table 5 we show the number of children (under the age of 17) in different occupations. It's no surprise that most (three quarters) of students are children, and it's reassuring to see that 70 percent of those who are unemployed and not seeking work are young. Less reassuring is the fact that two thirds of the beggars in this sample are children. Twenty-two per cent of the unemployed looking for work are also under the age of 17.

Using data on reported occupations for all individuals, we estimate an ANCOVA regression, on the model we've used in earlier tables; the only important difference is that though we control for household size and numbers of children (and for neediness for figures reported in the last two columns), we do *not* control for baseline occupational counts (data on occupation in the baseline was elicited in a way not directly comparable to data in the later survey). Thus, this is an entirely "Post-treatment" comparison, and we are unable to control for any pre-treatment differences in occupation across groups.

The joint hypothesis that all differences are zero is soundly rejected, with a p-value less than 0.02. The principal finding from Table 5 is that the households in the TUP group are significantly less likely to be engaged in unpaid housework or as agricultural laborers (on someone else's land). This last finding seems to echo a result of Bandiera et al. (2017), who find that a TUP treatment seems to play an important role in causing women to shift from wage- to self-employment. A. Banerjee et al. (2015) similarly finds a significant increase in time spent working, without an increase in hours of wage labor. Those experiments find the increase in labor hours driven in part by increases in agriculture and livestock activities related to the program itself, which we will see is not the case in our setting. However, the other significant effect is an *increase* in employment as a non-agricultural laborers. We have no compelling explanation for this second finding, though we note that the total numbers of such workers is quite small. But though only the only individual occupations that demonstrate a significant treatment effect are casual agricultural and non-agricultural labor, *overall* there seems to be a quite significant effect of TUP on occupation (the hypothesis that all of the coefficients in either of the "Diff." columns of Table 5 are equal to zero is rejected with a very high degree of confidence).

One might have thought that we would see people reporting occupations related to the TUP program: increases in household land cultivation or vegetable farming; increases in poultry or livestock husbandry; or increases in the operation of a small business; all of these were explicit offered as possible occupations. But we do not see significant effects for any of these. Further, of the 83 women who were given ducks, and of the 35 women given goats, exactly one of each reports their occupation as "poultry husbandry" or "livestock husbandry". Possibly the participants in these programs regard the corresponding activity as something less than or different from an "occupation".

One clear prediction of our model was that treated households which were not initially active in both self- and market-employment would respond, in part, by increasing leisure. Table 5 offers some tantalizing but not conclusive evidence on this. Looking just at the point estimates in the first "Diff." column, it appears that the average treated household moves out of casual agricultural employment (this is significant), but also moves out of *all other* listed occupations save for casual *non*-agricultural labor (significant), skilled labor (not significant) and unemployment (both seeking employment and idle). Thus, a consistent account one can give to explain Table 5 is that the average treated households moves away from casual agricultural labor and perhaps some other unskilled occupations. The time freed is allocated to more skilled market employment, and perhaps to increased leisure.

To summarize: the introduction of the TUP program does induce a significant occupational response, with particular identifiable responses including a decrease in casual agricultural labor, an increase in casual non-agricultural labor, and an increase in unemployment. We do not, however, see direct evidence of *particular* TUP enterprises changing occupation. One tempting general conclusion is that role played by TUP in determining occupation may depend more on its general loosening of budget and borrowing constraints than it does on changing the relative returns of wage- and self-employment. However, given the lack of reliable baseline data on occupation this is largely speculative.

Conclusion

We finish by making a general observation. We have arranged an randomized control trial of a complicated intervention in a low income country. This is the kind of endeavor where theory and 'structural' approaches to estimation may not seem to have very much to contribute: the number of outcomes affected by the complicated intervention may be large and uncertain, and the demands made by a 'structural' model to explain all these outcomes may seem absurd. But combined with randomization, sometimes a little structure can go a long way. With only quite modest assumptions on household preferences we've developed a rather general model of household behavior. This model is not very structural in the sense that we've adopted very few assumptions about precise functional forms or laws of motion.

Our main approach to estimation is both conventional and modest: we identify a number of "outcomes", and use ANCOVA regression to estimate average treatment effects. The main methodological contribution of the paper is the recognition that when the outcomes include the logarithms of different consumption expenditures, then the average treatment effects can be interpreted within our modest model as the product of a price elasticity and the average value of the log of the multiplier on the household's budget constraint. With this recognition one sees that these average treatment effects can be easily decomposed, recovering estimates of those elasticities and of the welfare measure we're calling 'neediness.' These quantities are useful to know for a wide variety of purposes, as knowing these may allow one to conduct any of a number of interesting counterfactual exercises.

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Table 1: Average treatment effects for value of consumption of different goods from ANCOVA regression, along with estimated Frisch elasticities β_i (proportional to income elasticities). Controls include baseline values of expenditures, household size, and numbers of children. Asterisks indicate statistical significance at 90, 95, or 99% level of confidence.

Goods	N	CTL	TUP	Diff.	β_i
Beans	464.000	−0.034**	0.033**	0.067***	0.236***
		(0.017)	(0.015)	(0.022)	(0.021)
Bread	311.000	−0.015	0.014	0.029	0.252***
		(0.024)	(0.021)	(0.032)	(0.033)
Cosmetics	397.000	−0.079***	0.080***	0.160***	0.514***
		(0.028)	(0.027)	(0.039)	(0.032)
Egg	91.000	−0.048	0.050**	0.098**	0.186***
		(0.044)	(0.020)	(0.049)	(0.046)
Fish	420.000	−0.034*	0.036**	0.070***	0.224***
		(0.020)	(0.016)	(0.026)	(0.026)
Fruit	114.000	−0.028	0.028	0.056	0.234***
		(0.046)	(0.042)	(0.062)	(0.059)
Fuel	521.000	−0.032	0.030	0.062	0.627***
		(0.034)	(0.029)	(0.045)	(0.036)
Maize	308.000	−0.063*	0.063**	0.125***	0.233***
		(0.037)	(0.030)	(0.047)	(0.051)
Meat	169.000	−0.053	0.055	0.109*	0.210***
		(0.042)	(0.040)	(0.058)	(0.051)
Millet	59.000	−0.044	0.101	0.144*	−3.172***
		(0.048)	(0.070)	(0.085)	(0.268)
Oil	514.000	−0.024	0.022	0.045*	0.322***
		(0.021)	(0.016)	(0.026)	(0.024)
Rice	415.000	−0.016	0.016	0.032	0.252***
		(0.021)	(0.018)	(0.027)	(0.026)
Salt	535.000	0.002	−0.002	−0.004	−0.002
		(0.006)	(0.004)	(0.007)	(0.007)
Soap	543.000	−0.077***	0.080***	0.157***	0.635***
		(0.028)	(0.025)	(0.038)	(0.026)
Sorghum	211.000	−0.028	0.023	0.051	0.171***
		(0.031)	(0.027)	(0.041)	(0.039)
Sugar	513.000	−0.023	0.020	0.043	0.370***
		(0.021)	(0.016)	(0.027)	(0.023)
Sweetpotato	57.000	0.021	−0.036	−0.057	0.280***
		(0.035)	(0.060)	(0.069)	(0.091)
Transport	116.000	0.009	−0.026	−0.035	0.704***
		(0.061)	(0.060)	(0.086)	(0.068)
Vegetables	512.000	−0.054**	0.052***	0.106***	0.362***
		(0.023)	(0.018)	(0.029)	(0.026)
$\overline{\log \lambda}^g$	554	0.138***	−0.060***	−0.198***	—

Table 2: Average treatment effects for number of assets of different types from ANCOVA regression; controls include baseline values of dependent variable, household size, number of children, and log neediness. Asterisks indicate statistical significance at the 90, 95, or 99% level of confidence. Estimates in columns labeled "CTL" and "TUP" do not control for log λ.

Asset	CTL	TUP	Diff. (no log λ)	Diff. (with log λ)	log λ
Bed	-0.30	0.64	0.93	0.68	-1.28^{*}
	(0.37)	(0.61)	(0.72)	(0.72)	(0.66)
Bicycle	0.00	0.01	0.01	-0.01	-0.06^{***}
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Chairs & tables	0.04	0.24^{***}	0.20^{*}	0.09	-0.56^{***}
	(0.07)	(0.07)	(0.10)	(0.10)	(0.10)
Cows	0.07	-0.05	-0.12	-0.17	-0.26
	(0.17)	(0.05)	(0.17)	(0.17)	(0.16)
Fan	-0.01	0.01	0.02	0.01	-0.05^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mobile	-0.02	0.11^{**}	0.13^{**}	0.06	-0.33^{***}
	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)
Motorcycle	0.01	-0.00	-0.01	-0.02	-0.03^{*}
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Mosquito Net	0.14^{***}	0.05	-0.09	-0.14^{**}	-0.24^{***}
	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
Poultry	-1.13^{***}	1.40^{***}	2.53^{***}	2.33^{***}	-1.00^{***}
	(0.11)	(0.20)	(0.22)	(0.22)	(0.22)
Radio	0.02	0.02	0.00	-0.01	-0.08^{***}
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Sewing	-0.02	0.04	0.06^{*}	0.06	-0.04
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Shed	-0.02^{**}	0.03^{***}	0.06^{***}	0.04^{***}	-0.07^{***}
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Shop	-0.00	0.01	0.01	-0.00	-0.06^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Small animals	0.09	-0.02	-0.11	-0.22	-0.59^{*}
	(0.33)	(0.08)	(0.34)	(0.34)	(0.32)
Tv	0.01	-0.00	-0.01	-0.01	-0.02^{**}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Total	-1.21^{*}	2.46^{***}	3.67^{***}	2.73^{***}	-4.71^{***}
	(0.66)	(0.71)	(0.97)	(0.95)	(0.90)

Table 3: Average treatment effects for value of assets of different types from ANCOVA regression. Asterisks indicate statistical significance at the 90, 95, and 99% level of confidence. Estimates in columns labeled "CTL" and "TUP" do not control for $\log\lambda$.

Asset	CTL	TUP	Diff. (no $\log\lambda$)	Diff. (with $\log\lambda$)	$\log\lambda$
Bed	2.82	18.57**	15.75	0.78	−75.56***
	(9.36)	(9.23)	(13.14)	(12.80)	(12.27)
Bicycle	1.47	3.23	1.76	−1.57	−16.84**
	(5.69)	(4.78)	(7.43)	(7.40)	(7.26)
Chairtables	−0.29	13.53***	13.83**	7.53	−31.92***
	(4.67)	(4.72)	(6.64)	(6.52)	(6.31)
Cows	−12.54	18.22	30.76	14.12	−84.55***
	(16.69)	(18.63)	(25.01)	(24.77)	(24.68)
Fan	−0.07	0.66	0.73	0.46	−1.37
	(0.96)	(0.81)	(1.26)	(1.26)	(1.26)
Mobile	1.92	6.79**	4.87	−1.46	−32.05***
	(3.85)	(3.23)	(5.02)	(4.85)	(4.73)
Motorcycle	25.43	−11.31	−36.73	−54.23	−88.49***
	(29.04)	(18.70)	(34.54)	(34.32)	(33.87)
Net	1.13**	0.34	−0.79	−1.36*	−2.94***
	(0.54)	(0.46)	(0.71)	(0.70)	(0.69)
Poultry	−37.10***	46.50***	83.61***	76.89***	−33.97***
	(4.07)	(6.72)	(7.86)	(7.79)	(8.07)
Radio	1.59	1.84	0.24	−1.99	−11.30***
	(2.39)	(2.08)	(3.17)	(3.13)	(3.06)
Sewing	3.26	−1.99	−5.25	−6.32	−5.36
	(3.91)	(2.29)	(4.53)	(4.52)	(4.51)
Shed	−2.54	3.99*	6.53**	4.32	−11.28***
	(1.89)	(2.04)	(2.78)	(2.74)	(2.75)
Shop	2.41	0.02	−2.38	−9.77	−37.38***
	(7.16)	(5.19)	(8.84)	(8.69)	(8.67)
Small animals	−23.26**	32.79***	56.04***	46.61***	−48.66***
	(10.05)	(12.45)	(16.00)	(15.89)	(15.67)
Tv	2.73	−1.62	−4.35	−6.47*	−10.69***
	(3.25)	(2.17)	(3.91)	(3.88)	(3.88)
Total	−37.57	131.75***	169.32***	71.40	−494.94***
	(49.76)	(42.89)	(65.69)	(62.84)	(59.53)

Table 4: Average treatment effects for nature of self-employment, from ANCOVA regression. Asterisks indicate statistical significance at 95% level of confidence. Estimates in columns labeled "CTL" and "TUP" do not control for $\log \lambda$.

Self-employment	N	CTL	TUP	Diff. (no $\log \lambda$)	Diff. (with $\log \lambda$)	$\log \lambda$
In business	229	-0.02	0.03^{**}	0.05^{***}	0.05^{***}	0.01
		(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Cultivating	452	0.03^{***}	0.01	-0.02	-0.02	-0.01
		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Livestock business	229	-0.05^{***}	0.12^{***}	0.17^{***}	0.16^{***}	-0.07^{***}
		(0.01)	(0.01)	(0.02)	(0.02)	(0.02)

Table 5: Occupations of individuals in surveyed households, along with average effects for control and TUP groups. Occupations with fewer than 30 people are excluded. Estimates in columns labeled "CTL" and "TUP" do not control for $\log\lambda$.

Occupation	N	<17	CTL	TUP	Diff. (no $\log\lambda$)	Diff. (with $\log\lambda$)	$\log\lambda$
Student	1932	1484	0.16^{***}	0.09^{*}	-0.07	-0.10	-0.14^{*}
			(0.06)	(0.05)	(0.07)	(0.07)	(0.08)
Cultivation	357	34	0.04	0.00	-0.03	-0.04	-0.04
			(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Idle	308	212	-0.01	0.04	0.05	0.02	-0.14^{***}
			(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Beggar	278	184	0.05	-0.01	-0.05	0.03	0.41^{***}
			(0.04)	(0.03)	(0.05)	(0.05)	(0.05)
Housewife	193	8	0.02	0.00	-0.02	-0.04^{**}	-0.11^{***}
			(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Seeking employment	134	29	0.00	0.01	0.01	0.00	-0.03
			(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Vegetable farming	126	0	0.03	-0.01	-0.03	-0.01	0.13^{***}
			(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Small business	98	1	0.01	0.00	-0.00	0.00	0.02
			(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Ag. Laborer	78	4	0.03^{**}	-0.02^{***}	-0.05^{***}	-0.03^{**}	0.10^{***}
			(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Skilled labor	56	0	-0.00	0.01	0.01	0.01	-0.03^{**}
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Driver	41	1	0.01	-0.01	-0.02	-0.02	-0.00
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Non-ag Laborer	31	1	-0.01^{***}	0.02^{**}	0.03^{***}	0.03^{***}	-0.02^{*}
			(0.01)	(0.01)	(0.01)	(0.01)	(0.01)