Food Aid Targeting, Shocks and Private Transfers Among East African Pastoralists

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Abstract: Public transfers of food aid are intended largely to support vulnerable populations in times of stress. We use high frequency panel data among Ethiopian and Kenyan pastoralists to test the efficacy of food aid targeting under three different targeting modalities, food aid's responsiveness to different types of covariate shocks, and its relationship to private transfers. We find that, in this region, self-targeting food-for-work or indicator-targeted free food distribution more effectively reach the poor than do food aid distributed according to community-based targeting. Food aid flows do not respond significantly to either covariate, community-level income or asset shocks. Rather, food aid flows appear to respond mainly to more readily observable rainfall measures. Finally, food aid does not appear to affect private transfers in any meaningful way, either by crowding out private gifts to recipient households nor by stimulating increased gifts by food aid recipients.

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Keywords: drought, crowding out, pass through, safety nets, social insurance, targeting

Public transfers are intended to assist the poor, to insure against adverse shocks, or both. There has long been widespread concern about the efficacy of targeting transfers and the prospect that public transfers may be effectively neutralized by compensatory reductions in private transfers.

Food aid represents a primary form of transfers in many low-income, rural communities around the world, perhaps especially in East Africa. Ethiopia is now the largest food aid recipient worldwide and Kenya, Sudan and other states in the region rely disproportionately on international food aid for public transfers to rural inhabitants. However, the international development community has long expressed a range of concerns about food aid, including the fear that food aid breeds "dependency", commercial trade displacement, its misuse by warring parties in conflict settings, and its efficacy in reaching the poorest. Barrett (2002) argues that the root of these prospective problems lies in targeting errors in food aid distribution and operational agencies[1] show a growing interest in assessing and improving the efficacy of food aid targeting.

The efficacy of food aid targeting depends on at least three factors. First, how is the targeting done? A significant literature on different targeting modalities has emerged over the past fifteen years, with a push among operational agencies first to self-targeting and indicator targeting and, most recently, for community based targeting (Barrett and Maxwell 2005; Coady, Grosh, and Hoddinott 2003). As yet, there remains scant empirical evidence directly comparing performance under alternative targeting modalities.

Second, how do public transfers affect private flows? Is there "crowding out" of private flows by public ones, as some previous studies have found (Cox, Hansen, and Jimenez 2004; Dercon and Krishnan 2003; Cox and Jimenez 1995), or might there even be "pass through" wherein non-needy recipients of public transfers increase the private transfers they make to needy households in response to direct targeting errors? Current enthusiasm for community-based targeting, depends, in part, on an untested hypothesis that non-trivial "pass-through" occurs, i.e., that private transfers effectively redistribute public transfers so that resources passed through are in effect indirect transfers to the poor mediated through non-needy unintended beneficiaries.

Third, external assistance is arguably most necessary in response to (or in anticipation of) covariate shocks that limit the ability of households within a community to assist family, friends, and neighbors (Dercon 2004). Given the typically superior information households have about one another relative to the information readily available to outside operational agencies, private inter-household transfers (so-called "social insurance") are generally better instruments for addressing idiosyncratic, household-level shocks than are external injections of resources. But covariate economic shocks – i.e., the common covariance of assets or incomes – as distinct from potentially imprecise correlates of covariate shocks, such as rainfall, are difficult to observe and we know little about the responsiveness of transfers to covariate economic shocks.

This paper explores these three key topics: how efficacy varies by targeting modality, how food aid flows affect private transfers, and how food aid responds to covariate shocks. Our data cover nearly 300 households in ten northern Kenyan and southern Ethiopian communities interviewed quarterly between June 2000 and December 2001. As such, this study is one of the few panel data analyses of food aid anywhere and the only one at reasonably high frequency and with a significant number of repeated observations across households. Moreover, we focus on pastoral households in the arid and semi-arid lands (ASAL), the region's subpopulation that is both most subject to climatic shocks and of greatest current concern among donors regarding prospective food aid dependency. Panel data permit us to estimate shocks and to control effectively for both time-varying factors such as rainfall or violence in determining food aid flows and for observable and unobservable community-level factors (e.g., NGO presence, accessibility, leadership quality, social cohesion) that likely affect both external food aid transfers and inter-household redistribution within the community.

We also benefit from a quasi-natural experimental design as these data span three different targeting modalities, enabling direct exploration of differences due to targeting methods. In southern Ethiopia, food aid flowed to households through either self-targeting food-for-work schemes (FFW)[2] or free food distribution (FFD) relying on indicator targeting based on age and gender of the household head or the presence of children in the household, with no work requirement. Meanwhile, food aid distribution in our northern Kenya sites has moved to community-based targeting (CBT), wherein outside agencies eschew direct household level targeting, which is decided entirely by the recipient community. Generally, the northern Kenyan communities distribute food uniformly across households, pro-rated based on an often outdated[3] roster of registered household headcounts, due to pressures within communities to share resources equally among all residents.

Although equal division of transfers across households is not unique to this setting, it is not a necessary nor a ubiquitous feature of CBT. Assessments of other CBT programs have found that communities, schools, or religious organizations target the poorest households relatively well (see Conning and Kevane (2001) for a good review of the evidence). Because the form of CBT employed by communities in our study does not attempt to target the poor, the results of our analysis of CBT are directly applicable only to communities engaging in equal distribution of transfers.

Further, because of widespread poverty and heavy concentration of activity on herding, pastoral communities are often considered by donors organizations to be homogenous in spite of considerable within-community variability in income, risk exposure, etc.[4] Rather than incur the high costs of reaching difficult-to-identify poor households via FFW or FFD, donors may propose CBT to these communities. While CBT places the responsibility to target effectively on the shoulders of the community rather than the donors, it may be equally difficult for the community to target effectively. This non-random application of CBT to hard-to-target communities may impact its targeting performance. Thus, CBT's performance relative to FFW and FFD may be caused more by placement effects of communities that are difficult to target to rather than by inadequate targeting. We have no means to control for placement effects, so this key caveat must be borne in mind as we discuss empirical results. Finally, the existing literature offers no evidence as to whether community-based food aid targeting works better than more conventional methods, as some analysts claim it does for other forms of transfer in other settings (Alderman 2002).

Several recent studies have examined the efficacy of food aid targeting in Ethiopia, questioning both the determination of which communities should be eligible for food aid and which households within a community should be the food aid recipients (Clay, Molla, and Habtewold 1999; Jayne et al. 2001; Jayne, Molla, and Yamano 2002; Gebremedhin and Swinton 2001). In assessing the efficacy of food aid targeting, most previous studies have employed hurdle models (Jayne et al. 2001; Clay, Molla, and

Habtewold 1999; Gebremedhin and Swinton 2001; Dercon and Krishnan 2003).[5] We suggest, rather, that a household's decision of whether to accept food or not is made with foreknowledge of an approximate quantity that will be received and thus, that whether a household received food aid and the quantity received should be modeled jointly. We therefore opt for censored regression methods for estimating household food aid receipts as a function of household characteristics, including income and wealth, both potentially endogenous regressors for which we instrument, and community- level shocks. We also interact our variables with indicators for each of the three targeting regimes (community based targeting in northern Kenya, and food-for-work and free food distribution in southern Ethiopia) to establish whether targeting differs across distribution mechanisms.

The ultimate efficacy of targeting depends not only on the direct distribution of public aid but also on their impact on private transfers, which can either take the form of income effects, in which receiving food aid "frees up" resources that are then transferred to needy households or substitution effects in which food aid at least partly replaces private transfers. We refer to the former case as the "pass-through" of transfers and the latter case as the "crowding out" of transfers.

Anecdotal evidence from northern Kenya (Reed 2001; Aklilu and Wekesa 2001) suggests that social safety nets, particularly transfers between relatives and neighbors, provide an important coping mechanism for households.[6] Our data confirm the existence of extensive transfer networks. During the survey period, over 65 percent of Kenyan and nearly 30 percent of Ethiopian households surveyed report exchanging money, livestock, or uncooked food, not including items loaned or borrowed.

In a growing body of literature, some researchers have found that public transfers at least partly crowd out private transfers within communities receiving transfers (Albarran and Attanasio 2001; Cox, Hansen, and Jimenez 2004; Dercon and Krishnan 2003). However, the extant literature on crowding out of private transfers is hampered by lack of data which tracks both private transfer and public transfer information or it relies on limited transfer data. We have data on monetary and all major non-monetary transfers, such as food and livestock, in our survey communities. After including proper controls for a range of other covariates likely to affect inter-household transfers both given and received, we can test directly whether food aid receipts have any pass through or crowding out effects on private transfers. Further, we can break out food transfers from all transfers, which include cash and livestock, in order to test for possible limits to fungibility in the form of transfer.

Relatively little research explicitly examines how shocks impact private and public transfers. Theory clearly suggests that households use private transfers to address idiosyncratic shocks through social insurance schemes (Coate and Ravallion 1993). Yet, social insurance arrangements may not offer adequate protection to members of groups facing covariate shocks, and may break down during significant covariate shocks (Jimenez, Kang, and Sawada 2002). Public transfers can play an effective role in complementing private transfer arrangements in so far as public transfers can respond to covariate shocks that may limit local households' capacity to smooth consumption through social insurance. We adapt a method previously employed to study food aid's responsiveness to macro-level shocks (Barrett 2001; Barrett and Heisey 2002) to construct a measure of covariate shocks for each household in each survey round, enabling us to examine, for the first time, how public and private transfers respond to covariate shocks. Moreover, by interacting predicted food aid receipts with measures of covariate shocks, we can also establish whether the prospective crowding out or pass through effects of public transfers vary according to the nature of local income and asset shocks.

The plan for the remainder of the paper is as follows. In the next section, we explain our econometric strategy for tackling these issues. We then describe the data. The third section presents estimation results and the last section concludes.

Econometric Strategy

Our objective in this paper is to explore four interrelated issues regarding food aid as it is practiced among pastoralist communities in the arid and semi-arid lands of East Africa. First, we wish to take advantage of unprecedented availability of detailed panel data to look anew at the efficacy of household-level food aid targeting. Second, we want to take advantage of the quasi-natural experiment in our data to look for prospective differences in efficacy by targeting modality (CBT, FFD or FFW). Third, high frequency panel data enable us to study food aid's responsiveness to shocks in a way that has never been done at micro-level. Finally, we seek to test whether these data support the hypothesis that public aid flows crowd out private transfers as well as the more novel hypothesis relating public and private transfers, that unintended beneficiaries effectively "pass through" windfall aid receipts to other households, which could provide an indirect targeting correction for at least some direct targeting errors.

These objectives require addressing a host of econometric challenges related to the panel nature of the data, the potential endogeneity of income and assets with respect to food aid flows and of food aid receipts with respect to private transfers, the need to estimate unobservable covariate asset and income shocks, as well as the censored nature of the food aid receipts and private transfer gross inflows and gross outflows dependent variables we study. This section explains our strategy for resolving these challenges.

Estimating income, assets and shocks

Food aid receipts are likely codetermined with contemporaneously observed household income and assets. For example, food aid may improve nutrient intake, resulting in increased worker productivity and therefore increased income. Furthermore, many pastoralists do not visit towns often and may link a trip to a food distribution center with other in-town activities, such as trading or selling animals or animal products, so as to justify the fixed transaction costs associated with travel. Income and assets may thus be endogenous regressors in the determination of a household's food aid receipts. We use standard instrumental variables estimation methods to resolve this problem.[7] In so doing, we also create the covariate asset and income shock variables we need to test for food aid's responsiveness to community-level shocks.

We estimate separate instrumenting equations for income[8] and livestock holdings, the chief asset held by sample households, and then compute asset and income shocks based on the decomposed residuals from the instrumenting equations. Our model for instrumented income is:

$$Y_{ijt}=\alpha + \boldsymbol{\beta}_{ij}\mathbf{X}_{ijt} + \lambda_{jt} + \rho_{ijt} \qquad (1)$$

where Y_{ijt} is income for household *i* from community *j* at time *t*. The matrix of regressors $\mathbf{X}_{ijt}$ includes household size, gender of the household head, age and age-squared of the head of household, the number of children in a household, the previous and current quarter's rainfall (mm) and indicator variables for possession of a bank account, insecurity in the previous quarter, country of residence (Kenya=1), the previous quarter's income, and a vector of time-and-location specific fixed effects, λ_{jt}, one for each of the 60 quarter and region combinations (10 regions and 6 quarters, with the base case Wachille for the quarter ending September 2000). These fixed effects capture local supply and demand conditions that vary over space and season, such as forage availability, prices, crime and weather patterns, inter-clan or inter-ethnic disputes, etc. The residual, $\rho_{ijt,}$ is the mean zero residual portion of income not explained by these instruments.

In addition to time-and-location specific fixed effects, we also control for household-specific random effects. Random effects are unobserved effects uncorrelated with the explanatory variables, allowing the econometrician to control for "any remaining serial correlation due to unobserved time-constant factors" (Wooldridge 2002, p. 288).[9] Following standard panel data econometric techniques, the residual, ρ_{ijt}, can be decomposed into two parts:

$$\rho_{ijt} = \theta_{ijt} + \psi_{ijt} \tag{2}$$

where ψ_{ijt} is the universal random error for household *i* in community *j* during time *t* and θ_{ijt} is each household's random effect.

Beyond simply controlling for the panel nature of the data, we can also decompose the error term into household-specific (idiosyncratic) and community-specific (covariate) shocks.[10] Covariate shocks, ε_{jt}, reflect the period-specific mean deviation from expected income in community *j*:

$$\varepsilon_{jt} = (1/N_j)\sum_{i=1}^{N_j}(\theta_{ijt} + \psi_{ijt}) \tag{3}$$

The covariate shock estimate is thus the mean unexplained portion of income in each community each period. One could then define the idiosyncratic income shock as the remaining unexplained portion of household income, i.e., as from the difference between equations (2) and (3):

$$\varepsilon_{ijt} \equiv Y_{ijt} - \hat{\mathbf{Y}}_{\mathbf{ijt}} - \varepsilon_{jt} \equiv \rho_{ijt} - \varepsilon_{jt} \tag{4}$$

where $\hat{\mathbf{Y}}_{\mathbf{ijt}}$ is the fitted value from equation (1). The idiosyncratic shock estimate, ε_{ijt}, is thus the deviation of each household *i*'s income in community *j* at time *t* from its expected value conditional on the covariate shock estimate, ε_{jt}. Our primary interest is the relationship between food aid and covariate shocks to which food aid flows are meant to respond, thus we do not discuss idiosyncratic shocks further.[11]

We follow precisely the same process to instrument for asset holdings, measured in tropical livestock units (TLUs),[12] and to estimate the covariate asset shock, φ_{jt} for each community and time period. These covariate asset and income shocks, as well as predicted income and herd size values, are key regressors in our subsequent estimation of the efficacy of food aid targeting, its responsiveness to shocks and its effects on private transfers.

Estimating Household-Level Food Aid Receipts

Given our estimates of household-level expected assets and income, and covariate asset and income shocks, we can now study the efficacy of household-level food aid distribution conditional on targeting modality and food aid's responsiveness to shocks. We use a censored (Tobit) regression model to determine the expected value of food aid conditional on food aid receipt. Households who did not receive aid have left censored observations equal to zero while the value of food aid received is used for recipient households.[13]

Our regression model thus takes the standard form, with continuous latent food aid receipts, FA_{ijt}^{*}, a function of observable and instrumented regressors, with a censoring rule on observations of food aid receipts, FA_{ijt}:

$$FA_{ijt}^{*} = \boldsymbol{\beta}_{ij}^{CBT}(CBT_{ijt}\mathbf{X}_{ijt}) + \boldsymbol{\beta}_{ij}^{FFW}(FFW_{ijt}\mathbf{X}_{ijt}) + \boldsymbol{\beta}_{ijt}^{FFD}(FFD_{ijt}\mathbf{X}_{ijt}) + \zeta_{j}^{FA} + \boldsymbol{\beta}_{ij}^{ni}\mathbf{X}^{ni}_{ijt} + \rho_{ijt}^{FA} \qquad (5)$$

with

$$\begin{aligned} FA_{ijt} &= FA_{ijt}^{*} && \text{if } FA_{ijt}^{*} > 0 \\ FA_{ijt} &= 0 && \text{if } FA_{ijt}^{*} \leq 0 \end{aligned} \qquad (6)$$

The regressors, $\mathbf{X}_{ijt}$**,** include predicted income and assets, covariate income and asset shocks, household size, gender of the household head, age and age-squared of the head of household, the number of children in a household, last quarter's aid receipts, previous and current quarter's rainfall (in mm), as well as an intercept term. In order to understand how distinct food aid targeting modalities affect food aid receipt, we employ a partial switching regression specification, interacting each $\mathbf{X}_{ijt}$ with an indicator variable indicating whether the household resided in a community using community-based targeting (CBT), free food distribution (FFD) or food-for-work self-targeting (FFW) mechanisms during the period.[14] We only use a partial switching regression specification because three household attributes, $\mathbf{X}^{ni}$ – possession of a bank account, town-based employment, and insecurity in the previous quarter – are unrelated to targeting efforts and thus we impose the assumption that the effects of these variables do not vary across targeting modalities. We continue to use random effects, now in conjunction with location-specific fixed effects, ζ_j, to control for any remaining nonspherical errors.[15]

Estimating Private Transfers

In order to examine food aid's prospective impacts on private transfers, we regress the latter on predicted food aid receipts – thereby controlling for the obvious endogeneity of food aid – and predicted food aid interacted with covariate income and asset shocks.[16] The linear term allows us to test the crowding out and pass through hypotheses directly, while the interaction terms allow for prospective change in those effects due to shocks. This admits the possibility, for example, that food aid crowds out private transfers only in the presence of negative covariate shocks that leave most households in a community worse off. We estimate separate equations for gross transfers given and gross transfers received. Since both of these dependent variables are left-censored at zero, we again use the partial switching Tobit specification with location-specific fixed effects and household-level random effects.

We use two different, nested measures of transfers. The first is transfers of food, including uncooked grains, sugar, and milk.[17] Estimation results for this narrowly defined form of transfers show whether food aid affects private transfers to or from other households in effectively the same form in which the public transfer was received. The second measure aggregates the value of all non-loan transfers: cash, food, and livestock. This broader measure reveals whether food aid affects transfers in a more fungible way.

We estimate food transfers received (RFT_{ijt}), measured as a positive value, as

$$
\begin{aligned}
&RFT_{ijt}^{*} = \boldsymbol{\beta}_{ij}^{CBT}(CBT_{ijt}\mathbf{Z}_{ijt}) + \boldsymbol{\beta}_{ij}^{FFD}(FFD_{ijt}\mathbf{Z}_{ijt}) + \boldsymbol{\beta}_{ij}^{ni}\mathbf{Z}_{ijt} + \zeta_{it}^{RFT} + \rho_{ijt}^{RFT} \\
&RFT_{ijt} = RFT_{ijt}^{*} \qquad \text{if } RFT_{ijt}^{*} > 0 \\
&RFT_{ijt} = 0 \qquad \text{if } RFT_{ijt}^{*} \leq 0
\end{aligned} \tag{7}
$$

where $\mathbf{Z}_{ijt} \equiv \mathbf{X}_{ijt} \sim \hat{\mathbf{FA}}_{\mathbf{ijt}} \sim \hat{\mathbf{FA}}_{\mathbf{ijt}} \Psi_{jt}$; the vector $\Psi_{jt} \equiv \varphi_{jt} \sim \varepsilon_{jt}$ encompasses the covariate asset and income shocks, and RFT_{it}^{*} is the latent value of food transfers received. $\hat{FA}_{it}$ is the predicted values of food aid receipt from the direct targeting equation. The $\mathbf{X}_{it}$ regressors are the same as for the food aid Tobit, excluding the previous quarter's rainfall, which effectively serves as the identifying instrument for food aid receipts. The $\hat{\mathbf{FA}}_{\mathbf{ijt}} \Psi_{jt}$ element of $\mathbf{Z}_{ijt}$ allows for the effects of food aid to vary potentially with the shocks experienced by communities. Because very few transfers were made either to or by FFW recipient households, we have too few observations to estimate FFW interaction terms separately. We therefore allow only for an intercept shift associated with FFW participation.

We follow this same estimation strategy for the three remaining private gross transfer dependent variables: all transfers received, RAT_{ijt}, food transfers given, GFT_{ijt}, and all transfers given, GAT_{ijt}. The key variables of interest concern the relationship between $\hat{\mathbf{FA}}_{\mathbf{ijt}}$ and each of the private transfer dependent variables. The coefficient relating $\hat{\mathbf{FA}}_{\mathbf{ijt}}$ to transfers received addresses the crowding out hypothesis, which would imply a negative and statistically significant point estimate. The coefficient relating $\hat{\mathbf{FA}}_{\mathbf{ijt}}$ to transfers given speaks to the pass-through hypothesis, which would imply a positive and statistically significant point estimate. The terms interacting $\hat{\mathbf{FA}}_{\mathbf{ijt}}$ with different shocks allow for crowding out or pass through effects to vary with spatiotemporal conditions. This specification permits us to disentangle food aid's multiple prospective impacts on private transfers, controlling for crucial intertemporal variation in conditions and in key unobservable covariates at the community-level.

Data and Descriptive Statistics

Our data are unique among evaluations of food aid in that we have a panel of observations spanning two countries, three different targeting modalities, and eight quarters, March 2000 through December 2001, during which a severe drought affected the surveyed communities. The data were collected from both communities and households as part of the USAID Global Livestock Collaborative Research Support Program (GL CRSP) Improving Pastoral Risk Management on East African Rangelands (PARIMA) project.

We use household and community-level data collected during seven quarterly survey rounds between June 2000 and December 2001 following the baseline survey of these households in March 2000. All prices were reported in Kenyan shillings and Ethiopian birr, then converted to U.S. dollars using June 2000 exchange rates.[18]

The ten survey communities lie in a contiguous zone spanning arid and semi-arid lands (ASAL) in northern Kenya and southern Ethiopia lacking basic infrastructure and far removed from their respective capitals. Ethnic groups span communities on both sides of the border, with the ethno-linguistic and agro-ecological similarities making comparisons across the study region feasible, if imperfect. Food aid shipments have become a regular – and controversial – part of the landscape in these areas, which are regularly buffeted by droughts, disease outbreaks and armed violence. The five Kenyan locations – Kargi (KG), Logologo (LL), N'gambo (NG), North Horr (NH) and Suguta

Marmar (SM) – all used community based targeting to distribute food aid during the survey period. The five Ethiopian locations Dida Hara (DH), Dillo (DL), Finchawa (FN), Qorate (QR) and Wachille (WA) – all had both food-for-work and free food distribution programs in place at different times during the survey.

Our sample comprises an unbalanced panel of 1560 observations across 288 households. The average household was interviewed for 5.4 out of a possible six quarters,[19] with over 70 percent interviewed for all six quarters and over 97 percent for at least four quarters. There nonetheless was some survey attrition or interruption, most likely because households migrated out of the community. Because migration may be correlated with food aid receipts (e.g., due to rainfall quantity or timing, insecurity, changes in employment opportunities or status, livestock holdings, etc.), and with some of our regressors, non-random sample attrition could yield biased and inconsistent regression parameter estimation if we do not control for attrition through a selection equation. However, all of the candidate variables identifying the selection effect (whether they participated in a survey for a particular quarter) are also related to food aid receipts. Without suitable instruments to control for prospective attrition bias, we must simply rely on recent empirical findings from panel data sets in developing countries that "even when attrition is fairly high, … [it] is not a general and pervasive problem for obtaining consistent [parameter] estimates" (Alderman et al. 2000 p.23), and that "survey attrition does not have a major impact on the estimates of equations of schooling attainment, labor force participation, self-employment, wages and fertility" (Falaris 2002, p.133).

Before turning to the estimation results, we present descriptive statistics, first for Kenya and Ethiopia separately, and then differentiated by targeting modality (CBT, FFW, FFD) for food aid recipients. Household income was much lower in Ethiopia than in Kenya (table 1). The mean and median Kenyan household received over three times more income than the median Ethiopian household. Mean Ethiopian herds were also smaller than in Kenya, although the median Ethiopian household has a slightly higher herd size than the median Kenyan household. Private transfers are larger in Kenya. Although the median household in each country neither gave nor received food transfers, the median Kenyan household received some form or transfer. Finally, food aid appears more stable for Kenyan households, with the previous quarter's value similar to the current value. However, 85 percent of Kenyan households reported insecurity (i.e., violence in the area) in the previous quarter. Only 17 percent of Ethiopian households reported insecurity in the previous quarter.

Figure 1 shows the portion of total income attributable to public (food aid) and private (gifts) transfers to households across quarters. Total transfers comprised between 9 and 19 percent of total median income. While over 40 percent of Ethiopian observations receive no transfers, only 6 percent of Kenyan observations received no transfers, underscoring the breadth of food aid distribution through CBT in northern Kenya.

Further differences exist by targeting modality (table 2). The median recipients of CBT food aid are more likely to both receive and give higher valued transfers than either FFW or FFD recipients. FFW and FFD do not appear to be differently targeted by individual indicators such as age, gender of the household head, and number of children in the household. The median recipients of all three forms of food aid have lower incomes than the median household income in the general population (see table 1). This is not the case with respect to assets for CBT recipients, who hold more livestock than do the northern Kenyan households at large.

Econometric Results

Instrumental Variables

The instrumenting equations for income and assets do well, with r^2 of 0.54 and 0.91 percent, respectively. Income is positively and significantly related to the previous period's income, ownership of a bank account, and town-based employment. Female headed households are poorer, controlling for other household attributes. Household assets are statistically significantly increasing in the prior period's livestock holdings and, as expected, decreasing in livestock deaths during the previous period. See Appendix table 1 for further details on the instrumenting equations.

Food Aid Targeting

Table 3 reports the switching Tobit regression parameter estimates of equation (5), explaining the value of food aid received by households.[20] To aid in interpretation of the Tobit coefficients, we compute the marginal effect (ME) of each regressor on the expected value of food aid by multiplying our coefficient estimators by the probability of being uncensored, as shown in the left column. Further, we disaggregate the results into the marginal effect on the probability of receiving aid (second column) and the marginal effect on the value of aid conditional on receipt (third column) using the McDonald - Moffit (1980) decomposition technique.

The model fits these data reasonably well. A Wald test clearly rejects the null hypothesis that there is no relation between the regressors and food aid receipts, with a test statistic of 688.75, with a pseudo-r^2 of 0.29.[21]

Targeting modality indeed seems to matter to food aid distribution patterns. CBT and FFW recipients receive less aid, on average, about $10 less per month for CBT households and about $3 less for FFW households, as compared against FFD recipients. Other than household size, household-specific attributes – assets, income, idiosyncratic income and asset shocks, age and gender of the household head – had no discernible effect on CBT or FFW flows. A Wald test of the exclusionary restriction that income, assets, age, age squared, number of children, and gender of the household head all have coefficients equal to zero cannot be rejected for either CBT or FFW flows (with p-values of 0.4591 and 0. 4277, respectively, on the relevant χ^2 test statistics), indicating that food aid is not targeted based on household attributes for either of these modalities. However, we can readily reject that same joint exclusionary restriction null hypothesis for FFD (with a p-value of 0.0000). Only FFD flows appear strongly related to household attributes, as ought to be the case for effective household-level targeting of public transfers.

Household size matters to all food aid flows. CBT flows increase modestly with household size as rations were supposed to be based on the number of residents in each household. Note, however, that expected CBT food aid receipts are not increasing when household size increases due to the addition of young children, reflecting the fact that the rosters used for allocating food aid are often quite dated, missing many children. FFW flows likewise increase in household size, likely reflecting the negative effect household size exerts on household-specific shadow wages rates, inducing greater self-selection into FFW programs among larger households (Barrett and Clay 2003).

With proper controls in place for community-specific fixed effects, household level income and assets, as well as covariate shocks, there appears minimal inertia in food aid distribution, contrary to past findings from the region that had to rely on cross-sectional data (Jayne, Molla, and Yamano 2002). The previous quarter's food aid was positively and significantly related to current food aid receipts only for CBT households, and then only for about $0.02 more food aid per week per household.

Food aid appears to flow in response to observable community-level shocks. CBT aid is significantly, negatively related to lagged rainfall, consistent with our qualitative field-level observations that food aid shipments into northern Kenya were heavily influenced by recent drought. FFW flows, by contrast, were negatively and statistically significantly related to both lagged and current period rainfall, consistent with the principles of self-targeting under the assumption that lower rainfall reduced the opportunity cost (i.e., the shadow wage) of FFW project participants' time. Free food distribution was strongly negatively related to current period rainfall.

The fact that food aid flows in response mainly to easily observed rainfall shocks rather than to underlying covariate asset or income shocks to which it theoretically ought to respond is underscored by the positive and statistically significant coefficient estimates on covariate asset shocks for both FFD and FFW and on covariate income shocks for CBT distribution. If food aid played an effective insurance role in this setting, it would be negatively and significantly related to asset and income shocks. However, the magnitudes of the estimated effects are quite small under each targeting modality.

Moreover, if income or assets covary negatively with aid receipts due to unobserved common factors, spurious correlation with the instrumenting equation residuals ought to bias downwards the coefficient estimates on the shock variables. Therefore, the fact that we have only one economically and statistically significant negative estimate of food aid flows in response to shocks, i.e. FFD's covariate income shock, seems a strong signal.[22] In summary, food aid seems to flow mainly in response to observable rainfall events rather than as a proper safety net to compensate for covariate income or asset shocks.

Food aid flows were not statistically significantly related to household predicted income under any of the three targeting modalities. Predicted assets were significantly related to food aid flows for free food distribution only. That may, however, be due to correlation between household attributes used for indicator targeting in many field FFD and FFW programs (e.g., age and gender of household head, household size) and income or wealth or between locations, used in geographic targeting of all food aid, irrespective of targeting modality, and income and wealth. But by re-estimating the food aid flows Tobit without controls for household indicators[23] and location fixed effects, each of which may effectively proxy for income, wealth or asset or income shocks, we can establish whether food aid indeed flows progressively, i.e., to needier households.

Table 4 reports the estimates of the specification without controls for household attributes used for indicator targeting in this region or for location-specific fixed effects.[24] This specification enables us to check whether indicator targeting based on household attributes and geographic targeting based on time-invariant community attributes seem effective in reaching the relatively poor, in providing insurance against adverse shocks, or both. As one would expect, there is no significant change in the pattern of CBT food aid flows, since these do not employ indicator targeting, although food aid flows now appear to respond negatively but not significantly to covariate asset shocks. Nonetheless, dropping the household indicators has no discernible effect on CBT's overall targeting efficacy. Food aid distributed according to community-based targeting does not appear to reach the poor very effectively.[25]

By contrast, upon removing location-specific effects and household indicators used in targeting, both FFW and FFD flows now appear economically and statistically significantly progressive, FFW in response to assets and FFD in response to income. FFW also now seems to flow as intended with respect to covariate asset shocks. The geographic and household indicators used in targeting FFW and FFD in southern Ethiopia indeed appear effective proxies for income and asset measures of welfare such that food aid does flow mainly to poorer households and those suffering greater shocks in southern Ethiopia, although the volumes of food aid involved remain small. Households suffering sharp adverse shocks continue to need informal assistance through private networks.

Food Aid's Effects on Private Transfers

As previously discussed, we test the crowding out and pass through hypotheses by regressing private transfers received and given, respectively, on the fitted values of food aid receipts obtained from the regressions just discussed. We do this for both food transfers and for the broader set of all cash, food and livestock transfers. Furthermore, we interact predicted food aid receipts with covariate income and asset shocks in order to establish whether crowding out or pass through effects vary with shocks.

In this sample, food aid has no economically or statistically significant crowding out effect on private transfers. When one looks at all transfers received (table 5), the point estimates for the coefficients on predicted food aid receipts are positive and small, not negative and large, as implied by the crowding out hypothesis. Private transfers do appear to respond to FFD food aid receipts interacted with covariate asset shocks and to CBT food aid interacted with covariate income shocks, but the negative signs implies that crowding out only occurs in the presence of positive shocks, i.e., when it is of relatively less concern for neutralizing policy interventions. Moreover, the average effects are quite small. More generally, we reject the joint null hypothesis that all of the food aid terms' coefficients equal zero. A Wald test that the coefficients of food aid receipts, lagged aid, income and asset shocks interacted with food aid all equal zero can be rejected for both CBT food aid recipients (p-value = 0.0102) and FFD food aid receipts (p-value =.0275). But given the signs of the point estimates involved, this too offers no support for the crowding out hypothesis.

When we re-estimate the model using only food transfers received as the dependent variable, the resulting point estimates suggest, if anything, a modest positive, statistically significant relationship between CBT food aid receipts and receipts of private transfers, suggesting modest "crowding in" rather than crowding out of private transfers in response to food aid flows. As less than 15 percent of sample observations included receipt of private food transfers, however, we place less stock on those estimates.[26]

We likewise find no strong statistical support for the pass through hypothesis on which some advocacy of community based targeting rests. The estimated coefficients relating food aid receipts to private transfers given are indeed positive, consistent with the hypothesis that increased aid receipts get passed along to others in the form of increased outflows of private transfers from the recipient household. But the magnitudes of the point estimates are quite small and statistically insignificantly different from zero. Covariate shocks do not have any significant effect on pass through effects associated with food aid. Overall, the Wald test of the joint exclusionary restriction that all the variables involving food aid receipt jointly equal zero cannot be rejected at any reasonable level of statistical significance, with p-values of 0.3410 and 0.1499 for CBT and FFD receipts, respectively. By contrast, increases in expected income, expected assets and covariate asset shocks result in more transfers by households in FFD communities while higher income leads to greater gifts given in CBT communities.

The same qualitative results obtain when we restrict our attention to just food transfers given. In both FFD and CBT communities, estimated food transfers given increases in income, consistent with the views that contributions under social insurance schemes will increase with one's income and that altruistic gifts are a normal good. Food aid has statistically significant pass through effects for both FFD and CBT. However, the point estimates are small in magnitude and, in the case of FFD communities, negative. Shocks still have no discernible effect in these data on transfer patterns.

The overall pattern is that food aid receipts have no significant effect on recipient households' inflows or outflows of private transfers, i.e., there is no strong evidence of either crowding out or pass through. Cox, Hansen, and Jimenez argue that long term public transfers may render crowding out a "fait acompli" (2004, p. 2194). In other words, in areas that have long received public transfers, like those we study, perhaps households have already adjusted their transfer patterns to current public transfer levels, leaving only that portion of private transfers that do not respond to public transfers. If crowding out affects private transfers primarily when public transfers first begin and are largely irreversible thereafter, there may be large crowding out effects of de novo public transfer schemes that we cannot capture in this setting. The implication, of course, would be that, in the long-term, the ameliorative effect of public transfers to needy households from well-targeted food aid to areas already accustomed to inflows of aid is not cancelled out by compensatory reductions in private transfers to those households. It is equally true, however, that social networks do not provide an informal corrective mechanism for targeting errors in public distribution via pass-through effects. The net result is to underscore the importance of effective targeting of food aid distribution.

Conclusions

This paper addresses several critical but under-researched questions concerning the distribution of food aid and of public transfers more broadly. Our results corroborate previous findings by other authors that food aid is not especially well targeted by income or assets at household-level in this region. They contradict previous findings that public transfers crowd out private transfers. We find no evidence of such effects.

The availability of multiple periods in a panel permits us to look more carefully at several important hypotheses. We find that inertia in household-level food aid distribution, while significant, plays less of a role than prior, cross-sectional studies suggest. We also find that food aid flows do not respond significantly to community-level covariate income or asset shocks. Rather, food aid flows primarily in response to rainfall, a highly imperfect proxy for welfare among the population of interest.

Because food aid was distributed under three different targeting modalities in our survey region, we are also able to compare a bit across these methods. We find that free food distribution based on indicator targeting using household attributes seems more effective in reaching the poor than self-targeting through food-for-work schemes, which is in turn better targeted than food aid distributed following community-based targeting methods. However, because CBT's relatively poorer targeting may be due to program placement effects and local peculiarities of CBT distributions in northern Kenya, rather than to CBT as a targeting modality more generally, we encourage caution in interpreting these results. Rather, our findings underscore that targeting is terribly difficult, even by communities. We find mild evidence of "pass through" of food from CBT food aid recipient households, but the magnitudes involved are far too small to compensate for direct targeting errors in the initial distribution of food aid by operational agencies.

[1] We use the term "operational agencies," as is custom among field practitioners, to encompass both international non-governmental organizations (NGOs) and United Nations agencies (e.g. UNICEF and WFP), or government entities that distribute food to individual recipients.

[2] The FFW wage and length of work are exogenously determined by the project and thus can properly be taken as exogenous to the household's choice in this analysis.

[3] McPeak (personal communication 2003) notes that 1996 census figures were used in Kenyan regional center Marsabit for food aid allocations during 2000-2002.

[4] For example, Smith, Barrett, and Box (2001) demonstrate considerable inter-household variation in risk assessments in pastoral communities.

[5] Hurdle models are two step processes. First the probability of a household receiving food aid is estimated using a probit model. Then, for households receiving food aid, the quantity of food aid received is estimated using generalized least squares.

[6] In most of the research examining food aid targeting or public transfer's impacts on private transfers, including ours, a "community" is identified in geographic terms based on data collection protocols. This community may not be the same as a social insurance network defined by the households interviewed (Santos 2003). For example, clan or kin based networks may play a stronger role in buffering a household against shocks than do geographic neighbors. The data we use were not collected in a way that permits identification of non-geographic communities. This may well mute the effects of private transfers in this and all preceding analyses that likewise rely on geographic identification of transfer networks.

[7] We address the potential endogeneity of some other regressors by using just observations from the baseline survey round, which predates the flow measures of transfers we use as dependent variables. This is our strategy with respect to household composition or the household's possession of a commercial bank account, for example.

[8] Income is the sum of the value of production of milk and maize, cash income from non-livestock activities and enterprises (e.g. wages, salaries, and proceeds from charcoal production, firewood collection, hides, or crafts), and livestock sales proceeds and the value of livestock slaughtered for meat (whether for sale or for home consumption). We exclude private transfers and food aid receipts from income so as to avoid spurious correlation between income and those dependent variables.

[9] Note that we use household specific random effects because there is no unbiased parametric fixed effects estimator for Tobit models.

[10] One could alternatively try to include measures of observable shocks (e.g., rainfall, quarantines, raids) directly. But since conceptually transfers are meant to flow in response to welfare shocks experienced by households rather than observable, largely community-scale events that may be only weakly correlated with individual level welfare (Smith, Barrett, and Box 2001; Lybbert et al. 2004), the approach of using the unexplained component of income makes more sense, as Barrett (2001) argues. This seems borne out by our (unreported) results. When we estimate food aid receipts without the computed shock terms, substituting instead a vector of exogenous shock proxies (e.g., raids, quarantines), the results proved nonsensical.

[11] Because idiosyncratic shock estimates absorb the measurement error in income, were we to include it as a regressor, there would be bias towards zero in the coefficient

estimates relating idiosyncratic income shocks to food aid or to private transfers, similar to an errors-in-variables problem. Furthermore, computing a likelihood ratio test, we find that we cannot reject the null hypothesis that idiosyncratic shocks have no additional explanatory power over the specifications we employ (p-value = 0.8315).

[12] A TLU conversion assigns metabolic equivalence weights to each type of livestock where 1 TLU = 1 Cattle = 0.7 Camels = 10 Goats = 10 Sheep.

[13] We value food aid receipts for the primary goods received: maize and wheat. Food aid can be supplemented with very small quantities of oil, beans, and unimix (a blended fortified food). However, we lack price information for these products. Therefore, the value of food aid is slightly underreported. We use community maize prices to value wheat, for which prices were not collected, using an adjustment factor of wheat to maize prices for Ethiopia. In 1999-2000, using Ethiopian commodity price data supplied by Michigan State University for Ethiopia as a whole, the unconditional mean ratio of wheat/maize prices (i.e., the ratio of birr/kg prices) was 1.459.

[14] CBT was in force in our northern Kenya locations throughout the survey period. In southern Ethiopia, both FFD and FFW were available in each community at different points in time. No households simultaneously received both types of food aid. Households who received one form of aid in a period were assigned a zero for the other sort of aid that period, while all other households in southern Ethiopia were classified as eligible and thus were assigned an indicator value of one. There are obvious possibilities for program placement effects because operational agencies' choice of CBT versus FFW or FFD methods is not completely random as well as selection effects, because households choice to participate in FFW instead of FFD, or vice versa, need not be

random either. However, we have no suitable instruments in these data with which we could control for the selection effect within these communities, nor do we have data on other communities that could be used to identify the prospective placement effects.

[15] We do not use time-and-location fixed effects for the food aid and private transfers equations due to too few observations in each time-location subsample after breaking out food aid into three forms of targeting.

[16] For less than half of the censored households, 422 of 1050, the predicted value was negative. Because we do not observe negative food aid, we convert these negative values to zero predicted food aid.

[17] We are constrained to estimating food transfers as the values of sugar, milk, and maize received, due to lack of prices for other products, such as tea, legumes, and oil. But, these latter products are a very minor component of recorded inter-household flows.

[18] $0.123 = 1 Ethiopian Birr; $0.0129 = 1 Kenyan Shilling on June 15, 2000 (http://www.oanda.com/convert/fxhistory). Inflation was low during the survey period and no credible deflators are available for these regions of Ethiopia and Kenya. Therefore, we did not deflate nominal values.

[19] Since we use lagged values both in instrumenting for assets and income and of food aid, we must drop the June 2000 survey round from the estimation, reducing the sample to six usable panel rounds.

[20] Across the Tobit equations, N'Gambo is the omitted community in northern Kenya, and Dida Hara is the omitted community in southern Ethiopia. The omitted intercept is FFW for the food aid targeting equations. In the transfer equations, FFD is omitted.

[21] Bear in mind that the Tobit model does not maximize the R-squared value, but rather maximizes the log-likelihood function (Wooldridge, 2002. p. 529).

[22] We also tried specifications that included quadratic shock terms to allow for possible nonlinear effects, as might occur if flows respond only to relatively substantial shocks, but not to modest perturbations. We found no evidence that higher-order polynomial specification in shocks added any explanatory power to the simpler linear specification presented here.

[23] We retain attributes not commonly used in targeting (e.g., holding a bank account, in town work, receiving food aid in the previous quarter).

[24] The results are qualitatively very similar if we retain the location-specific fixed effects. A table of results is available from the authors by request.

[25] As discussed previously, the data do not allow us to discern whether the failure to reach the poor is due to targeting mechanism or program placement.

[26] We omit the tables reporting the regression results for food transfers received and food transfers given. These are available from the authors by request.

Table 1. Monthly Descriptive Statistics for Food Aid Recipients, by Country

Variable	Median	Mean	Standard Deviation
Ethiopia (n=863)			
Food aid value	$0.47	$3.00	$5.57
Food aid value during previous quarter	$2.73	$4.34	$5.99
Income	$8.20	$21.46	$38.41
Monthly Income during previous quarter	$8.42	$20.16	$35.87
Livestock holdings in TLUs	8.00	12.93	22.73
Previous quarter's livestock holdings	7.43	12.67	26.60
Food transfers received	$0.00	$0.08	$0.76
Food transfers given	$0.00	$0.11	$0.73
All transfers received	$0.00	$0.64	$4.00
All transfers given	$0.00	$0.66	$4.14
Rainfall (in mm)	9.87	11.08	5.27
Monthly rainfall during previous quarter (in mm)	11.11	11.35	5.31
Number of children age nine and under	3.00	2.75	1.93
Number of household members	7.00	4.25	4.25
Age of household head	45.00	48.93	16.42
Female headed households		0.29	
Households holding a bank account		0.00	
Households with member working town		0.06	
Insecurity in the community last quarter		0.17	
Kenya (n=697)			
Food aid value	$3.10	$4.38	$4.17
Food aid value during previous quarter	$2.97	$4.03	$3.87
Income	$30.50	$66.51	$130.48
Monthly Income during previous quarter	$30.01	$62.40	$102.22
Livestock holdings in TLUs	7.53	17.48	39.80
Previous quarter's livestock holdings	7.58	17.84	39.58
Food transfers received	$0.00	$0.09	$0.27
Food transfers given	$0.00	$0.14	$0.33
All transfers received	$0.32	$3.22	$15.44
All transfers given	$0.00	$1.25	$3.96
Rainfall (in mm)	6.43	7.65	7.13
Monthly rainfall during previous quarter (in mm)	3.93	7.12	7.21
Number of children age nine and under	2.00	1.93	1.48
Number of household members	6.00	6.23	2.63
Age of household head	45.00	46.29	13.52
Female headed households		0.34	
Households holding a bank account		0.06	
Households with member working town		0.51	
Insecurity in the community last quarter		0.73	

Table 2. Monthly Descriptive Statistics for Food Aid Recipients, by Targeting Modality

Variable	Median	Mean	Standard Deviation
Received aid from FFW (n=144)			
Food aid value	$5.97	$6.83	$8.41
Food aid value during previous quarter	$4.49	$5.69	$5.30
Income	$5.61	$15.20	$24.44
Monthly Income during previous quarter	$4.83	$12.59	$18.39
Livestock holdings in TLUs	4.55	7.40	12.08
Previous quarter's livestock holdings	4.64	7.17	11.42
Food transfers received	$0.00	$0.02	$0.18
Food transfers given	$0.00	$0.12	$0.39
All transfers received	$0.00	$0.77	$5.52
All transfers given	$0.00	$0.73	$3.93
Rainfall (in mm)	8.90	8.53	4.69
Monthly rainfall during previous quarter (in mm)	7.28	8.07	5.44
Number of children age nine and under	3.00	2.75	1.86
Number of household members	7.00	8.72	4.76
Age of household head	50.00	52.30	16.95
Female headed households		0.34	
Households holding a bank account		0.00	
Households with member working in town		0.07	
Insecurity in community last quarter		0.00	
Received aid from FFD (n=312)			
Food aid value	$3.49	$5.00	$5.39
Food aid value during previous quarter	$4.48	$6.04	$5.52
Income	$6.24	$11.80	$21.27
Monthly Income during previous quarter	$5.84	$15.65	$29.50
Livestock holdings in TLUs	8.00	10.53	15.30
Previous quarter's livestock holdings	6.79	11.22	29.19
Food transfers received	$0.00	$0.06	$0.37
Food transfers given	$0.00	$0.07	$0.22
All transfers received	$0.00	$0.58	$3.35
All transfers given	$0.00	$0.64	$5.07
Rainfall (in mm)	15.00	13.17	6.40
Monthly rainfall during previous quarter (in mm)	13.18	13.88	5.55
Number of children age nine and under	2.00	2.80	1.96
Number of household members	8.00	8.42	4.33
Age of household head	45.00	46.69	16.32
Female headed households		0.37	
Households holding a bank account		0.00	
Households with member working in town		0.05	
Insecurity in community last quarter		0.26	
Received aid from CBT (n=594)			
Food aid value	$3.87	$5.14	$4.07
Food aid value during previous quarter	$3.35	$4.34	$3.83
Income	$28.73	$66.29	$137.00
Monthly Income during previous quarter	$29.18	$61.12	$104.17
Livestock holdings in TLUs	8.86	19.07	42.73

Previous quarter's livestock holdings	8.88	19.53	42.45
Food transfers received	$0.00	$0.08	$0.22
Food transfers given	$0.00	$0.14	$0.34
All transfers received	$0.39	$3.39	$16.26
All transfers given	$0.04	$1.25	$3.91
Rainfall (in mm)	3.91	7.24	7.42
Monthly rainfall during previous quarter (in mm)	3.41	5.82	5.89
Number of children age nine and under	2.00	1.87	1.43
Number of household members	6.00	6.23	2.65
Age of household head	45.00	46.76	13.61
Female headed households		0.35	
Households holding a bank account		0.06	
Households with a member working in town		0.50	
Insecurity in community last quarter		0.78	

Table 3. Tobit Estimates for Quarterly Food Aid Receipts (US $)

Variable	Marginal effects on unconditional expected value of y y = $4.64	Marginal effects on probability of y being uncensored y = .44	Marginal effects on conditional expected value of y y = $10.43		Mean
CBT	-31.9652	-0.9882	-28.5206	***	0.446795
CBT* Income ‡	-0.0005	0.0000	-0.0004		89.15
CBT* Livestock Assets ‡	0.0004	0.0000	0.0003		7.81
CBT*Comm. Income Shocks ‡	904201	57695	679742	***	-0.00000005
CBT*Comm. Asset Shocks ‡	-2.7652	-0.1764	-2.0788		-0.005117
CBT*Lagged food aid receipts	0.0704	0.0045	0.0529	***	5.40
CBT*Previous quarters' rainfall	-0.0581	-0.0037	-0.0437	***	9.54
CBT*Rainfall (in mm)	0.0030	0.0002	0.0023		10.26
CBT*Number of children	-0.4727	-0.0302	-0.3554	*	0.863462
CBT*No. household members	0.4209	0.0269	0.3164	***	2.79
CBT*Age of household head	0.0752	0.0048	0.0565		20.68
CBT*Age^2 of household head	-0.0011	-0.0001	-0.0008		1038.90
CBT*Female headed households	-0.2363	-0.0152	-0.1784		0.152564
Kargi	0.5243	0.0326	0.3905		0.086538
Logologo	8.0779	0.3560	5.7687	***	0.067949
North Horr	7.0628	0.3276	5.0446	***	0.083333
Suguta Marmar	2.1755	0.1254	1.5875	*	0.098718
FFW	-11.1273	-0.6964	-9.4097	***	0.353205
FFW* Income ‡	0.0034	0.0002	0.0026		28.14
FFW* Livestock Assets ‡	-0.0493	-0.0031	-0.0371		4.93
FFW*Comm. Income Shocks ‡	-2284956	-145798	-1717738		0.00000004
FFW*Comm. Asset Shocks ‡	1704	109	1281	***	-0.000057
FFW*Lagged food aid receipts	-0.0203	-0.0013	-0.0152		3.57
FFW*Previous quarter's rainfall	-0.0927	-0.0059	-0.0697	**	10.50
FFW*Rainfall (in mm)	-0.2994	-0.0191	-0.2251	***	10.49
FFW*Number of children	-0.4608	-0.0294	-0.3464		0.958974
FFW*No. hshld members	0.1314	0.0084	0.0988		2.99
FFW*Age of hshold head	0.0884	0.0056	0.0664		17.73
FFW* Age^2 of household head	-0.0010	-0.0001	-0.0008		984.50
FFW*Female headed hsholds	-1.5763	-0.1091	-1.2311		0.087179
FFD* Income ‡	-0.0009	-0.0001	-0.0007		31.15
FFD* Livestock Assets ‡	0.0299	0.0019	0.0225		6.41
FFD*Comm. Income Shocks ‡	-2197525	-140219	-1652010	*	0.00000006
FFD*Comm. Asset Shocks ‡	2926	187	2199	***	-0.000035
FFD*Lagged food aid receipts	0.0305	0.0019	0.0229		5.54
FFD*Previous quarter's rainfall	-0.0638	-0.0041	-0.0480		16.49
FFD*Rainfall (in mm)	-0.4200	-0.0268	-0.3157	***	15.91
FFD*No. of children	0.2715	0.0173	0.2041		1.25
FFD*No. of household	-0.1378	-0.0088	-0.1036		3.83

members					
FFD*Age of household head	-0.4238	-0.0270	-0.3186	***	22.01
FFD* Age^2 of household head	0.0036	0.0002	0.0027	***	1181.24
FFD*Female headed hsholds	0.2645	0.0167	0.1980		0.128205
Dillo	5.7475	0.2841	4.1147	**	0.100000
Finchawa	-5.2450	-0.4315	-4.9532	***	0.115385
Qorate	-1.5357	-0.1056	-1.1953		0.107692
Wachille	9.1725	0.3995	6.5779	**	0.115385
Insecurity in comm. last quarter	0.0824	0.0053	0.0619		0.419872
Households with a bank account	-1.4839	-0.1035	-1.1630	*	0.030128
Households working in town	0.8659	0.0539	0.6452		0.258333

Wald χ^2 (55) = 688.75 Pseudo-r^r = 0.293

Prob > χ^2 = 0.0000 Proportion of observations censored = 0.327

Note: For dummy variables, dy/dx is for discrete change from 0 to 1.

*, ** and *** reflect statistical significance at the 10, 5 and 1 percent levels, respectively.

‡ indicates an instrumented regressor.

Marginal effects cannot be computed for the constant term.
Its coefficient from the Tobit estimation is 76.516***.

Table 4. Tobit Estimates for Quarterly Food Aid Receipts (US $), No Household or Location Indicators

Variable	Marginal effects on unconditional expected value of y y = $9.41	Marginal effects on probability of y being uncensored y = .58	Marginal effects on conditional expected value of y y = $16.15		Mean
CBT	-8.465157	-0.3129205	-6.064994	***	0.446795
CBT* Income ‡	-0.0032428	-0.0001182	-0.0022892		89.1532
CBT* Livestock Assets ‡	0.0262978	0.0009584	0.0185648		7.81395
CBT*Comm. Income Shocks ‡	-89483.8	-3261.139	-63170.58		-0.000000049
CBT*Comm. Asset Shocks ‡	-8.391798	-0.3058298	-5.924142	**	-0.005117
CBT* Lagged food aid receipts	0.2017402	0.0073522	0.1424173	***	5.39541
FFW	-14.69971	-0.586576	-11.09259	***	0.353205
FFW* Income ‡	0.0103214	0.0003762	0.0072863		28.1375
FFW* Livestock Assets ‡	-0.2147712	-0.0078271	-0.1516165	***	4.93289
FFW*Comm. Income Shocks ‡	-253737.9	-9247.2	-179124.9		0.000000044
FFW*Comm. Asset Shocks ‡	-2158.248	-78.65497	-1523.603	***	-0.000057
FFW*Lagged food aid receipts	0.2634456	0.009601	0.1859779	***	3.56911
FFD* Income ‡	-0.0399813	-0.0014571	-0.0282245	***	31.1462
FFD* Livestock Assets ‡	-0.0064554	-0.0002353	-0.0045572	*	6.41125
FFD*Comm. Income Shocks ‡	-1941420	-70752.91	-1370534		0.000000056
FFD*Comm. Asset Shocks ‡	203.0091	7.398442	143.3131		-0.000035
FFD*Lagged food aid receipts	-0.1521491	-0.0055449	-0.1074088	***	5.54378
Insecurity in comm. last quarter	-0.8313709	-0.0304587	-0.5872687		0.419872
Households with a bank account	-0.4010214	-0.0148437	-0.2835426		0.030128
Households working in town	0.2359815	0.0085599	0.1665199		0.258333

Wald χ^2 (26) = 398.70 Pseudo-r^r = 0.103

Prob > χ^2 = 0.0000 Proportion of observations censored = 0.327

Note: For dummy variables, dy/dx is for discrete change from 0 to 1.

*, ** and *** reflect statistical significance at the 10, 5 and 1 percent levels, respectively.

‡ indicates an instrumented regressor.

Marginal effects cannot be computed for the constant term.

Its coefficient from the Tobit estimation is 14.76***.

Table 5. Tobit Estimates for Value of All Transfers Received (US$)

Variable	Marginal effects on unconditional expected value of y y = $7.00	Marginal effects on probability of y being uncensored y = .21	Marginal effects on conditional expected value of y y = $32.68		Mean
CBT*Aid*Comm. Income Shocks ‡	-243298.8	-5768.093	-255838.1	***	-8.1E-07
CBT*Aid*Comm. Asset Shocks ‡	2.401221	0.0569278	2.524977		-0.111176
CBT*Aid ‡	0.1841007	0.0043646	0.193589		5.28512
CBT	1.475053	0.0348149	1.543015		0.446795
CBT* Income ‡	0.0035375	0.0000839	0.0037198		89.1532
CBT* Livestock Assets ‡	-0.0435875	-0.0010334	-0.0458339		7.81395
CBT*Comm. Income Shocks ‡	2148226	50929.83	2258943	*	-4.9E-08
CBT*Comm. Asset Shocks ‡	-29.93168	-0.7096159	-31.47432		-0.005117
CBT*Lagged food aid receipts	-0.0986733	-0.0023393	-0.1037588		5.39541
CBT*Rainfall (in mm)	0.0158181	0.000375	0.0166333		10.2602
CBT*Number of children	0.7112168	0.0168614	0.7478719		0.863462
CBT*No. household members	-0.4589279	-0.0108802	-0.4825804		2.78718
CBT*Age of household head	0.4613852	0.0109385	0.4851643		20.6814
CBT*Age^2 of household head	-0.0044004	-0.0001043	-0.0046272		1038.9
CBT*Female headed households	0.8222156	0.0192386	0.8499135		0.152564
Kargi	2.656624	0.0599958	2.632191		0.086538
Logologo	1.667329	0.0382739	1.683215		0.067949
North Horr	0.1116322	0.0026409	0.1170488		0.083333
Suguta Marmar	10.03313	0.2021365	8.956051	**	0.098718
FFW	1.072669	0.0252375	1.117004		0.353205
FFD*Aid*Comm. Income Shocks ‡	-450934.6	-10690.69	-474175.2		0.0000003
FFD*Aid*Comm. Asset Shocks ‡	-476.8174	-11.30432	-501.3919	**	-0.000551
FFD*Aid ‡	0.1138652	0.0026995	0.1197337		3.01198
FFD*Income‡	0.005321	0.0001262	0.0055953		31.1462
FFD* Livestock Assets ‡	-0.0308434	-0.0007312	-0.032433		6.41125
FFD*Comm. Income Shocks ‡	-2669102	-63278.68	-2806664		5.6E-08
FFD*Comm. Asset Shocks ‡	7215.976	171.0753	7587.877	**	-0.000035
FFD*Last quarter's food aid receipts	0.0248693	0.0005896	0.026151		5.54378
FFD*Rainfall (in mm)	0.0260992	0.0006188	0.0274443		15.9113
FFD*No. of children	-1.595193	-0.0378186	-1.677408	***	1.25321
FFD*No. of household members	0.8438386	0.0200056	0.8873289	***	3.83077
FFD*Age of household head	-0.1864828	-0.0044211	-0.1960939		22.0071
FFD* Age^2 of household head	0.0018317	0.0000434	0.0019261		1181.24
FFD*Female headed hsholds	4.25984	0.0941879	4.128611	*	0.128205
Dillo	0.1999269	0.0047224	0.2091997		0.1
Finchawa	-1.946429	-0.0478186	-2.155474		0.115385
Qorate	-3.82829	-0.0977046	-4.528764	**	0.107692

Wachille	4.862119	0.1062781	4.657306	0.115385
Insecurity in comm. last quarter	-0.6726352	-0.0159838	-0.709633	0.419872
Households with a bank account	1.536936	0.0352831	1.551522	0.030128
Households working in town	-1.588648	-0.0383222	-1.712474	0.258333

Wald χ^2 (41) = 199.50 Pseudo-r^r = .026

Prob > χ^2 = 0.0000 Proportion of observations censored =.659

Note: For dummy variables, dy/dx is for discrete change from 0 to 1.

*, ** and *** reflect statistical significance at the 10, 5 and 1 percent levels, respectively.

‡ indicates an instrumented regressor.

Marginal effects cannot be computed for the constant term.

Its coefficient from the Tobit estimation is -75.7136***.

Table 6. Tobit Estimates for Value of All Transfers Given (US$)

Variable	Marginal effects on unconditional expected value of y	Marginal effects on probability of y being uncensored	Marginal effects on conditional expected value of y		Mean
	y = $2.78	y = .21	y = $13.15		
CBT*Aid*Comm. Income Shocks ‡	-48943.54	-2889.116	-51809.18		-0.00000081
CBT*Aid*Comm. Asset Shocks ‡	-0.2287626	-0.0135038	-0.2421566		-0.111176
CBT*Aid ‡	0.1717404	0.0101378	0.1817957		5.28512
CBT	2.486769	0.1426361	2.56521		0.446795
CBT* Income ‡	0.004599	0.0002715	0.0048682	***	89.1532
CBT* Livestock Assets ‡	0.0014056	0.000083	0.0014879		7.81395
CBT*Comm. Income Shocks ‡	346676	20464.13	366973.8	*	-0.000000049
CBT*Comm. Asset Shocks ‡	3.675883	0.2169858	3.891106		-0.005117
CBT*Lagged food aid receipts	-0.0223882	-0.0013216	-0.023699		5.39541
CBT*Rainfall (in mm)	0.0027965	0.0001651	0.0029603		10.2602
CBT*Number of children	0.0160984	0.0009503	0.0170409		0.863462
CBT*No. household members	-0.066742	-0.0039398	-0.0706497		2.78718
CBT*Age of household head	-0.1016374	-0.0059996	-0.1075882		20.6814
CBT*Age^2 of household head	0.0009	0.0000531	0.0009527		1038.9
CBT*Female headed households	0.8405106	0.0479962	0.8531553		0.152564
Kargi	-0.4066738	-0.0244864	-0.4427384		0.086538
Logologo	-1.594584	-0.1027578	-1.950526		0.067949
North Horr	-1.997506	-0.1312142	-2.550771	**	0.083333
Suguta Marmar	0.2682321	0.0156399	0.2793374		0.098718
FFW	-1.130476	-0.0678351	-1.228165		0.353205
FFD*Aid*Comm. Income Shocks ‡	-83870.45	-4950.837	-88781.05		0.0000003
FFD*Aid*Comm. Asset Shocks ‡	178.6452	10.54535	189.1049	**	-0.000551
FFD*Aid ‡	0.0279153	0.0016478	0.0295498		3.01198
FFD*Income‡	0.0065155	0.0003846	0.006897	**	31.1462
FFD* Livestock Assets ‡	0.0503861	0.0029743	0.0533362	***	6.41125
FFD*Comm. Income Shocks ‡	-657546.1	-38814.66	-696045.3		0.000000056
FFD*Comm. Asset Shocks ‡	-1963.181	-115.8857	-2078.125	**	-0.000035
FFD*Last quarter's food aid receipts	0.0102507	0.0006051	0.0108509		5.54378
FFD*Rainfall (in mm)	-0.0128796	-0.0007603	-0.0136337		15.9113
FFD*No. of children	0.1296763	0.0076547	0.1372689		1.25321
FFD*No. of household members	0.1165558	0.0068802	0.1233801		3.83077
FFD*Age of household head	-0.089145	-0.0052622	-0.0943644		22.0071
FFD* Age^2 of household head	0.0005536	0.0000327	0.000586		1181.24
FFD*Female headed hsholds	-1.210954	-0.0754416	-1.393346	**	0.128205
Dillo	1.521445	0.0840388	1.487303		0.1
Finchawa	-1.808568	-0.1162887	-2.210751	***	0.115385

Qorate	-2.353071	-0.1559897	-3.08962	***	0.107692
Wachille	0.3587986	0.0208485	0.372008		0.115385
Insecurity in comm. last quarter	-0.7180032	-0.0426128	-0.7665923		0.419872
Households with a bank account	4.10644	0.2016327	3.611714	**	0.030128
Households working in town	0.3269143	0.0191204	0.3418677		0.258333

Wald χ^2 (49) = 236.07 Pseudo-r^r = 0.0625

Prob > χ^2 = 0.0000 Proportion of observations censored = 0.69

Notes: For dummy variables, dy/dx is for discrete change from 0 to 1.

*, ** and *** reflect statistical significance at the 10, 5 and 1 percent levels, respectively.

‡ indicates an instrumented regressor.

Marginal effects cannot be computed for the constant term.

Its coefficient from the Tobit estimation is -14.7894**.

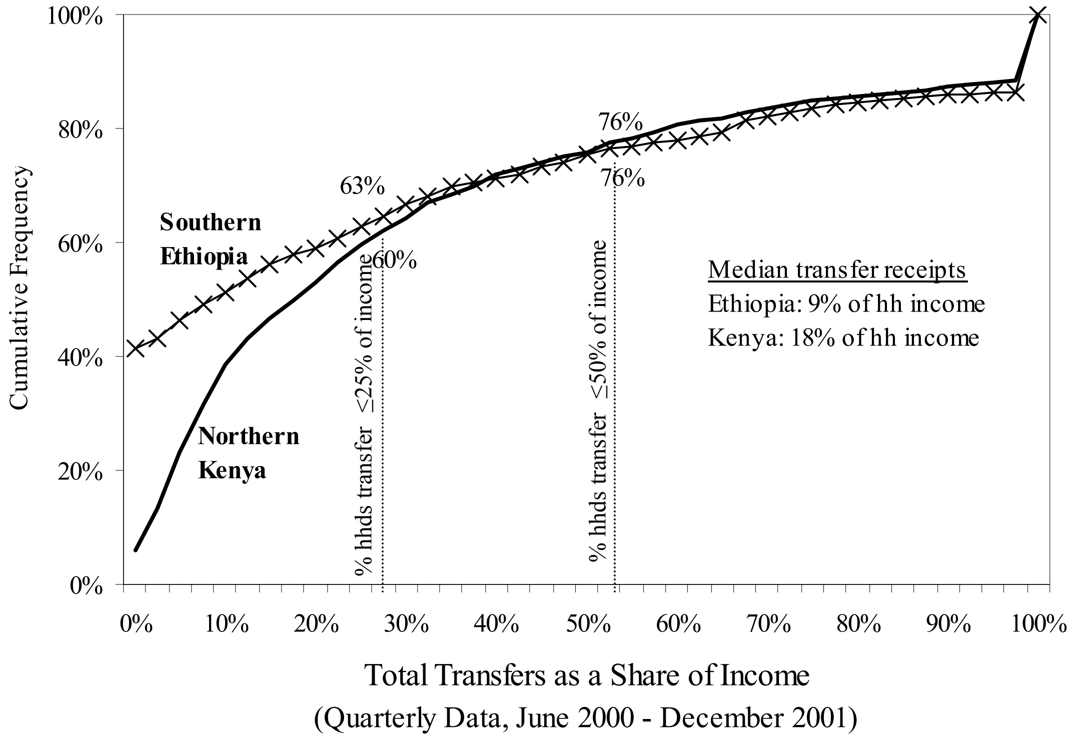

Figure 1. Food aid transfers and private transfers as a share of pastoralist household income, 2000-1 drought

Appendix Table 1. Instrumental Variable Estimates for Income and Assets

	Income			Assets		
Variable	**Coef.**		**z**	**Coef.**		**z**
Income during previous quarter	0.8391069	***	35.1	-		
Livestock holdings in previous quarter	-		-	0.8829061	***	92.79
Livestock deaths in previous quarter	-		-	-0.6137341	***	-7.66
Rainfall (in mm)	-2.95	***	-4.5	-0.0381225		-1.12
Number of children	-5.275965		-1.23	-0.2721433		-1.06
Number of household members	4.411206	*	1.89	0.2916555	**	2.1
Age of household head	1.130115		0.58	0.0756191		0.65
Age^2 of household head	-0.0119882		-0.67	-0.000726		-0.68
Female headed households	-18.98083	*	-1.66	-1.340281	*	-1.94
Households holding a bank account	68.69457	**	2.19	3.409243	**	1.97
Insecurity in the community last quarter	223.9972	**	2.22	11.84775	***	2.32
Households with member working in town	50.17859	***	3.4	0.7850939		1.02
Is the household in Kenya? (1=yes)	-37.6683		-0.81	-1.535462		-0.64

Note: Time-location effects (interaction terms for 10 locations and 6 quarters) are not reported.
T-statistics in parentheses

* Significant at the 10% level.	Overall $r^2 = 0.5426$	Overall $r^2 = 0.9067$
** Significant at the 5% level.	Wald χ^2 (64) = 1773.28	Wald χ^2 (65) = 10206.56
*** Significant at the 1% level.	Prob > χ^2 = 0.0000	Prob > χ^2 = 0.0000

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