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Is There Persistence in the Impact of Emergency Food Aid? Evidence on Consumption, Food Security, and Assets in Rural Ethiopia

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Executive Summary

The primary goal of emergency food aid after an economic shock is often to bolster short-term food and nutrition security. However, these transfers also act as insurance against other shock effects, such as destruction of assets and changes in economic activity, which can have lasting deleterious consequences. Although existing research provides some evidence of small positive impacts of timely food aid disbursements after a shock on current food consumption and aggregate consumption, little is known about whether these transfers play a safety net role by reducing vulnerability and protecting assets into the future.

We investigate this issue by exploring the presence of persistent impacts of two major food aid programs following the 2002 drought in Ethiopia: a food-for-work program known as the Employment Generation Schemes (EGS) and a program of free food distribution (FFD). Using rural longitudinal household survey data collected in 1999 and 2004, we estimate the impact of these programs on consumption growth, food security, and growth in asset holdings 18 months after the peak of the drought, when food aid transfers had substantially or entirely ceased in most program villages.

We measure persistent food aid impacts using a quasi-experimental methodology. The average treatment effect of each program is estimated using a difference-in-differences matching technique based on propensity score matching. Comparison households for the matching analysis are nonbeneficiaries drawn from the same villages. We undertake several robustness checks of estimated impacts to confirm that observed effects represent lagged or persistent program impacts, rather than contemporaneous program effects.

The results show a significant effect of EGS participation on growth in consumption and food consumption (in per adult equivalent terms) from 1999-2004, a period ending one-and-a-half years after the drought and at least six months after nearly all food aid disbursements ceased. EGS beneficiaries also experienced a significant reduction in perceived famine risk relative to five years ago, while famine risks increased

over this period for the group of matched nonbeneficiaries. Contrary to these positive effects, EGS beneficiaries experienced significantly slower growth of livestock holdings from 1999 to 2004. This is consistent with a program-induced reduction in demand for precautionary savings, though we found that the significance of this effect is also driven in part by outlier observations with very large growth in livestock holdings in the matched comparison group. For the FFD program, we find a significant average impact of the program on growth in food consumption, but, surprisingly, a negative impact on change in famine risk. Results show differences in the distribution of impacts by pre-drought household welfare. Participation in public works had a significant impact on growth in food consumption and food security for households in the middle and upper tail of the per capita expenditure distribution. The better-targeted FFD program had its largest impacts among the poor. These differences in program outcomes are consistent with evidence on program targeting that shows that the work requirements of the EGS make the poor less likely to participate.

Overall, these results suggest that emergency food aid played an important role in improving welfare, access to food, and food security for many households following the drought in 2002. However, improved targeting, especially in EGS, and larger, sustained transfers may be required to increase benefits, particularly to the poorest households. The impacts of food aid identified here indicate some persistence or accumulated effects of transfers on consumption growth over time. Although the time lag between receipt of transfers and observed consumption is not more than one year in most cases, the estimated impact on consumption growth relative to the size and timing of transfers suggests possible savings or multiplier effects of emergency food aid.

Key words: food aid, treatment effects, propensity score matching, Ethiopia

1. Introduction

Natural disasters, financial crises, and other economic shocks can have significant negative consequences for uninsured households (Skoufias 2003; Block et al. 2004). When the resulting destruction of assets and changes in economic activity are sufficient to prevent recovery, these shocks lead to poverty traps with lasting effects on household welfare (Barrett and Maxwell 2005). In this setting, food aid or other assistance given in the aftermath of an economic shock may insure households from deleterious shock effects. Emergency food aid intended primarily to sustain short-term food and nutrition security may also serve as a safety net, protecting welfare in the long run and possibly reducing the need for further assistance in the future.

There is a small body of research that assesses the impact of food aid programs on household food security and welfare and, to a more limited extent, nutrition (Barrett 2002). A common finding is that food aid programs such as general food distribution or food-for-work have at most a small impact on food consumption or nutrition and often only a short-run effect on aggregate consumption (see Yamano, Alderman, and Christiaensen 2005 and Quisumbing 2003 for evidence from Ethiopia; see Stifel and Alderman 2003 for evidence from Peru). However, there is little rigorous evidence about whether timely food aid distribution in response to a shock may play an important safety net role by reducing vulnerability and protecting assets (Barrett, Holden, and Clay 2004 is an exception). By preserving stocks of productive assets or savings during a crisis, emergency food aid may have a positive impact on future asset holdings and a persistent effect on welfare. A major challenge of identifying food aid impacts that has been ignored in much of the literature is to account for selection into the programs; failing to do so makes it impossible to attribute causation of welfare gains to food aid.

This paper examines this issue in the context of Ethiopia's experiences following the 2002 drought. While initial accounts suggested that poor rainfall was of concern primarily in northeast pastoral areas, rains started late in parts of the Amhara region and most crop-dependent areas received below-normal rainfall in August and September. By

December 2002, it was estimated that 11.3 million Ethiopians would face severe food shortages in 2003, with an additional 3 million people at risk of significant shortages, double the estimate only four months earlier. Cereal production was estimated to have fallen by 25 percent (FEWS NET 2002-03). The worst affected areas included much of the pastoral areas of Afar, parts of eastern Tigray, eastern Oromiya, parts of Amhara, and SNNPR. In response, the government expanded its two major food aid programs, an emergency food-for-work program called the Employment Generation Schemes (EGS) and free food distribution also known as "Gratuitous Relief" (FFD).[1]

This paper uses rural longitudinal household survey data collected in 1999 and 2004 to measure the effect of these programs on consumption levels, food security, and asset holdings 18 months after the peak of the drought, when food aid transfers had substantially or entirely ceased in most program villages. The data, the setting, and the methodology used in this analysis all provide the conditions for a rigorous evaluation. First, the timing of the data collection makes it possible to control for pre-drought household and farm characteristics and to observe key outcomes roughly two years after the onset of the drought. Second, several features of these data improve the quality and extent of our knowledge of food aid's effects. The household questionnaire used in 2004 included retrospective questions on the effects of the drought and on the timing and size of food aid participation and receipts. The questions on drought effects include information on perceived changes in famine risk, a useful summary measure of changes in household food security. Also, detailed information on livestock holdings provides useful measures of asset holdings in a country where livestock dominate all other assets as a form of investment. Finally, we measure the average treatment effect of the food aid program using a difference-in-differences matching technique based on propensity score matching. Heckman, Ichimura, and Todd (1997, 1998) and Heckman et al. (1998) show that under certain conditions on the data—all of which are satisfied in this study—propensity score matching estimators provide reliable estimates of program impact.

[1] Emergency drought assistance included both food aid (primarily wheat, maize and vegetable oil) and limited quantities of cash. For ease of exposition, we refer to all of this as food aid.

We find a large, significant effect of EGS participation on the growth in consumption and food consumption (in per adult equivalents) of recipients one-and-a-half years after the 2002 drought. EGS beneficiaries experienced a reduction in famine risk relative to five years ago, while a comparison group of nonbeneficiaries reported an increase in famine risk over the same period. We find a significant average impact of FFD participation on growth in food consumption, but, surprisingly, a negative impact on food security. After disaggregating impact estimates by pre-drought household consumption tertiles, we find significant impacts of public works participation on food consumption and food security for some households in the middle-to-upper tail of the expenditure distribution. The better-targeted FFD program showed greater benefits for the poor.

The paper is organized as follows. The next section presents the ERHS data and summarizes the effects of the drought and food aid receipts by sample households. We then present the methodology for measuring longer-term food aid impacts and provide the impact estimates. The final section discusses the implications of the impact estimates for food aid policy.

2. Evidence of Drought Effects and Food Aid Use

Our data come from the Ethiopia Rural Household Survey (ERHS), a longitudinal household data set collected in six survey rounds from 1994-2004 in 15 rural Ethiopian villages.[2] The sampling frame was stratified on the main agroecological zones (excluding pastoral and urban areas) and village sample sizes were chosen to generate an approximate self-weighting sample in terms of farming system. Given the relatively

[2] For further details on the ERHS, see Bevan and Pankhurst (1996), Dercon and Krishnan (2000), and Dercon and Hoddinott (2004).

small number of sampled villages, extrapolation of results to rural Ethiopia as a whole must be done with care.[3]

We use data from the 1999 and 2004 rounds of the ERHS to estimate food aid impacts after the drought.[4] The 2004 round captures a variety of information about the incidence of the 2002 drought among sample households, about the breadth and depth of drought effects, and about receipt of food aid through the EGS or FFD. Pilot testing suggested that the 18-month time gap between the peak of the drought and the 2004 survey enumeration was too long to capture immediate drought effects on yields, consumption, or assets. Instead, qualitative questions about the incidence and effects of the drought were asked in a detailed shocks module and in a separate drought module. Detailed information about the timing and size of transfers from each program were captured in survey modules on food aid, off-farm income, and food consumption. These data show that most food aid transfers were made in the first 12 months after the drought. Although food aid resumed in some villages in the period captured in the 2004 round of the survey, with the exception of one village, food aid transfers at that time were too small to account for the observed growth in consumption.[5]

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[3] Dercon (2004) recently compared variants of the welfare measures from the ERHS used here to those reported in Ethiopia's national Welfare Monitoring Survey and found that income grew faster for households in the ERHS villages in the 1990s than for households on average in Ethiopia. As a result, households in the ERHS have somewhat higher welfare levels in 1999 than elsewhere.

[4] We assessed the extent of sample attrition between 1999 and 2004. Among households in villages receiving food aid, the overall attrition rate was low, 6.5 percent or 1.3 percent per year. There is no correlation between observable household characteristics (for example, age, sex of the household head, household size, consumption, and livestock holdings) and attrition. There are some significant differences in attrition by village with one village, Shumsha, having a higher attrition rate than others in the sample. A careful examination of reasons for attrition in Shumsha recorded during data collection did not reveal any systematic explanation.

[5] The 2004 survey indicates only whether the household received the EGS or FFD programs during the first 6 months after the failed harvest in 2002 (September 2002 – March 2003), but provides more detail on food aid activities during the subsequent 12 months (April 2003 – March 2004). The number of households enrolled in both programs is generally higher in the first six months after the drought. Days worked in the EGS during the next 12 months gradually declined from April 2003, stopping during the harvest in September 2003. Food-for-work resumed in four of the nine villages in early 2004, though only about one fifth of recent EGS participants rejoined the program. An exception is Shumsha village, where about 60 percent of recent recipients joined. Nonetheless, almost no food aid receipts from either EGS or FFD were reported during the recall period for consumption.

Table 1 presents summary information on drought incidence and the food aid response for the 15 ERHS villages. Column 2 lists mean consumption per adult equivalent in 1999 as an indication of pre-drought welfare levels. In four villages, less than 3 percent of respondents reported a drought (self-defined) in 2002. This figure was less than 15 percent in two other villages (column 3). Drought incidence ranged from 30-85 percent in the remaining nine villages, which were the ones that received food aid from September 2002 to March 2004 (columns 4 and 5). The self-reports of drought are consistent with rainfall data from nearby weather stations: household self-reported drought incidence in 2002 was fairly closely correlated to deviations of 2002 rainfall during the main growing season (August-December) from long-run averages ($\rho = 0.27$). Table 1 also shows that the incidence of the drought was spread broadly across the expenditure distribution of villages in the sample. Indeed, receipt of food aid is more closely correlated with the share of drought-affected households in the village (Spearman correlation coefficient = 0.35) than with the wealth of the village in quintiles (Spearman correlation coefficient = -0.16). This pattern reflects the disaster relief motivation of food aid during this period.

Columns 6-8 of Table 1 summarize the size of food aid transfers during this period for villages receiving food aid. Column 6 shows that in most villages receiving food aid, 15-19 percent of respondents felt they received enough food aid during the drought. The outliers for this measure are the poorest village, Aze Deboa, in which fewer than 4 percent of households report receiving enough aid and Korodegaga, in which one-third of respondents claimed receiving enough aid. This relatively high level of satisfaction may have arisen because nearly all households in Korodegaga received some food aid for a limited period of several months and then all food aid was stopped. Columns 7 and 8 report the number of days worked under public works and the share of income per capita from public works in per capita household expenditure.[6] There is

[6] Transfers through public works comprised three-quarters of all transfers; the rest came through FFD. To save space, we only report the data on public works.

Table 1—Drought incidence and the food aid response

	Number of households in 1999-2004 ERHS panel (1)	Real consumption per adult equivalent in 1999 (2)	Share of households reporting a drought in 2002 (3)	Share of households participating in public works, 2002-04 (4)	Share of households receiving free food distribution, 2002-04 (5)	Share of households reporting receiving enough food aid (6)	Average days worked in public works, 2003-04 (7)	Income per capita from public works as a share of consumption per capita (8)	Average number of meals per day during drought (9)	Share of households that sold livestock to pay for food during drought (10)
		(birr/month)	(percent)	(percent)	(percent)	(percent)		(percent)		(percent)
Haresaw	81	91.1	50.6	56.8	58.0	18.9	40.5	3.2	1.8	55.6
Geblen	61	70.9	63.9	67.2	73.8	15.3	43.7	1.9	1.9	54.1
Dinki	79	75.0	50.6	55.7	58.2	16.9	5.6	0.5	2.2	38.5
Debre Berhan	168	138.7	7.7	4.8	0.6		0.0			
Yetemen	55	81.2	0.0	3.6	0.0		0.0			
Shumsha	121	149.9	35.5	80.2	77.7	17.1	42.8	0.5	1.6	49.2
Sirbana Godeti	83	160.8	2.4	2.4	1.2		0.0			
Adele Keke	88	87.8	85.2	35.2	42.0	17.3	9.6	0.3	2.0	25.0
Korodegaga	98	87.6	38.8	92.9	81.6	33.3	32.8	3.2	2.2	68.4
Turufe Kechemane	92	131.5	14.1	6.5	2.2		0.0			
Imdibir	65	58.6	0.0	0.0	0.0		0.0			
Aze Deboa	59	28.0	30.5	76.3	66.1	3.6	8.8	0.3	2.2	62.7
Adado	122	82.5	2.5	1.6	0.8		0.0			
Gara Godo	93	55.7	66.7	50.5	59.1	17.5	15.1	0.4	1.8	48.4
Doma	62	104.5	67.7	45.2	14.5	16.3	23.1	0.4	1.8	25.8
Total	1,327	99.8	32.3	36.9	34.4	18.4	14.4	0.7	2.1	40.0

considerable variation in the intensity of public works in these villages, with residents of several villages averaging more than 40 days of work on public works during the year. However, the value of resources transferred, even in these villages, represents a small fraction of per capita consumption. Gilligan and Hoddinott (2004) provide additional details about the operation of the two food aid programs.

Columns 9 and 10 summarize two of the drought coping mechanisms. Column 9 shows average number of daily meals consumed during the drought. From this table alone, there is no clear relationship of these data with drought or food aid incidence, but the sample average is low at 2.1 meals per day. Column 10 shows that livestock provided consumption insurance during the drought, with 40 percent of households selling livestock to pay for food during this period.

3. Estimating the Impact of Food Aid

A valid measure of the impact of food aid should compare outcomes in households that received food aid to what those outcomes would have been had the same households not received any food aid. The construction of this unobserved counterfactual is the basic dilemma of impact evaluation. Measuring impact as the difference in mean outcomes between all households receiving food aid and those not receiving food aid, even controlling for pre-program characteristics, may give a biased estimate of program impact. This bias arises if there are unobserved characteristics that affect the probability of participation in the program that are also correlated with the outcome of interest. Two important sources of this selection bias include targeting of the program to recipients based on characteristics unobservable to the researcher and self-selection into the program by eligible recipients. The difference-in-differences propensity score matching estimator used in this analysis helps to control for these sources of selection bias. The estimator constructs a plausible comparison group by matching food aid recipients to similar nonrecipients using a rich set of control variables, including whether the household met the specific targeting criteria for that food aid

program in its village, as described by local leaders in the ERHS survey. Then, changes in outcomes are compared across these two groups from before and after the food aid program to remove any remaining unobserved time-invariant differences between recipients and matched nonrecipients.

Following Heckman, Ichimura, and Todd (1997) and Smith and Todd (2001, 2005), let Y_t^1 be a household's outcome in time period t if it is a food aid recipient and let Y_t^0 be that household's outcome in time period t if it does not receive food aid. The impact of food aid is the change in the outcome caused by receiving food aid:

$$\Delta = Y_t^1 - Y_t^0 .$$

However, for each household, only Y_t^1 or Y_t^0 is observed in any period, t. Let D be an indicator variable equal to 1 if the household receives food aid and 0 otherwise. In the literature on evaluation of social programs, D is an indicator of receipt of the "treatment." We would like to construct an estimate of the average impact of food aid on those that receive it—the average impact of the treatment on the treated (ATT):

$$ATT = E\left(\Delta \mid X, D=1\right) = E\left(Y_t^1 - Y_t^0 \mid X, D=1\right) = E\left(Y_t^1 \mid X, D=1\right) - E\left(Y_t^0 \mid X, D=1\right), \qquad (1)$$

where X is a vector of control variables. Because $E\left(Y_t^0 \mid X, D=1\right)$ is not observed, we estimate the impact of food aid on consumption and asset holdings using propensity score matching as a method for estimating the counterfactual outcome for participants (Rosenbaum and Rubin 1983). Let $P(X) = \Pr(D=1 \mid X)$ be the probability of participating in the food aid program. Propensity score matching constructs a statistical comparison group by matching observations on food aid recipients to observations on nonrecipients with similar values of $P(X)$. The validity of this approach rests in part on two assumptions:

$$E\left(Y_t^0 \mid X, D=1\right) = E\left(Y_t^0 \mid X, D=0\right), \qquad (2)$$

and

$$0<P(X)<1. \tag{3}$$

Expression (2) assumes "conditional mean independence," that conditional on X nonparticipants have the same mean outcomes as participants would have if they did not receive the program. Expression (3) assumes that valid matches on $P(X)$ can be found for all values of X. Rosenbaum and Rubin show that if outcomes are independent of program participation after conditioning on the vector X, then outcomes are independent of program participation after conditioning only on $P(X)$. If expressions (2) and (3) are true, propensity score matching provides a valid method for estimating $E(Y_t^0 \mid X, D = 1)$ and obtaining unbiased estimates of ATT.

When panel data are available with information from before and after the delivery of food aid, the estimator in equation (1) can be improved by subtracting off the difference in pre-program outcomes between the food aid recipients and the matched comparison group of nonrecipients,

$$\begin{aligned} ATT = E(\Delta_t - \Delta_\tau \mid X_\tau, D=1) &= E\left(\left(Y_t^1 - Y_t^0\right) - \left(Y_\tau^1 - Y_\tau^0\right) \mid X_\tau, D=1\right) \\ &= E\left(Y_t^1 - Y_\tau^1 \mid X_\tau, D=1\right) - E\left(Y_t^0 - Y_\tau^0 \mid X_\tau, D=1\right), \end{aligned} \tag{4}$$

where τ and t represent time periods before and after the introduction of the program, respectively, and the indicator D refers to receipt of the program in an intervening period. This difference-in-differences estimator removes any bias due to unobservable, time-invariant differences between the treatment and comparison group not controlled for by conditioning on pre-program observables, X_τ. The version of this estimator based on matching was formalized in Heckman, Ichimura, and Todd (1997) and Heckman et al. (1998).

Through comparisons with experimental estimators, Heckman, Ichimura, and Todd (1997, 1998) and Heckman et al. (1998) show that propensity score matching provides reliable, low-bias estimates of program impact provided that (1) the same data

source is used for participants and nonparticipants, (2) participants and nonparticipants have access to the same markets, and (3) the data include meaningful *X* variables capable of identifying program participation and outcomes. The ERHS surveys clearly meet criterion (1). We ensure that criterion (2) is met by restricting the set of nonrecipients in the potential comparison group to households that did not participate in the relevant food aid program during 2002-04 in villages with the food aid program. The ERHS data also provide a very rich set of variables to identify program participation and consumption, food security, and asset holdings, as required by criterion (3). In the community surveys implemented as part of the ERHS in 2004, village leaders responsible for targeting food aid programs reported the criteria they used in targeting public works and FFD.[7] This enables us to identify the targeting component of the participation decision by including the specific targeting criteria as control variables in the participation regressions for each program. For EGS, we include the gap between the market wage rate and the public works wage, interacted with household characteristics, to identify household-specific self-selection. We also use several variables that indicate the breadth and depth of the household's social networks and its political connections to village officials to identify the role of these connections and access to information in program participation. A detailed retrospective shocks module in the 2004 round of the ERHS survey also allows us to construct control variables for death and illness shocks that occurred after the 1999 round of the survey (the last round before the drought), but before the drought occurred in 2002. Shocks such as these could be associated with program eligibility and with welfare outcomes. This rich set of control variables should capture many of the determinants of participation that are typically unobservable to the researcher, which helps to reduce a potentially significant source of bias in propensity score matching estimators. In general,

[7] The precise criteria used varied by village and by intervention (Gilligan and Hoddinott 2004). For food for work, community leaders generally used a loose "poverty" criterion as well as whether the individual was able-bodied. In practice, it appears that greater weight was given to the latter. In the case of free food distribution, being old or disabled was often listed as criterion that appears to have been applied more consistently.

we find that the estimate of food aid impact is sensitive to the choice of variables in X_τ, so we try various alternative specifications and present the results that appear most robust.

We estimate separate treatment effects for participation in EGS and in FFD because the two programs have different eligibility requirements in most villages. In the Employment Guarantee Scheme, recipients must work to obtain the food aid, so disabled or elderly household members typically would not qualify. Instead, these groups are often the target population for FFD. Also, the timing and size of transfers differ under EGS and FFD, so impact will likely differ as well.

However, estimating impacts separately introduces a new complication, namely that many households receive both EGS and FFD at some point during the 18-month period following the drought. Including households in the treatment or comparison group that receive another food aid program raises concerns that the impact estimates for the program of interest are "contaminated" in the sense that outcomes are confounded by transfers from a similar intervention at nearly the same time, which could bias the impact estimates.[8] However, restricting the set of EGS participants and potential comparison households to those that do not receive the other program can lead to other forms of bias. In the Appendix, we investigate these sources of bias and conclude that the impact estimates that do not exclude recipients of other program are more reliable.

For each outcome and each food aid program, we estimate the propensity score for participation in the program by a probit model including pre-drought observable variables, X_τ, that include both determinants of participation in the program and factors that affect the outcome. We match treatment and comparison observations using kernel matching, and estimate standard errors for the impact estimates by a bootstrap. Following Heckman, Ichimura, and Todd (1997) and Smith and Todd (2005) and omitting time subscripts, the kernel matching estimator takes the form

[8] Positive spillovers from recipients of either program to its own comparison group of nonrecipients in the form of program-induced transfers would create another form of contamination of the comparison group. This form of bias is likely to be small in this setting as it is elsewhere (see Lentz and Barrett 2004).

$$ATT = \frac{1}{n}\sum_{i \in T}\left\{ Y_i^1 - \frac{\sum_{j \in C} Y_j^0 K\left(\frac{P_j(X) - P_i(X)}{a_n}\right)}{\sum_{k \in C} K\left(\frac{P_k(X) - P_i(X)}{a_n}\right)} \right\}, \tag{5}$$

where T is the treatment group of program participants, C is the comparison group of nonparticipants, K is a kernel function, and a_n is a parameter determining the kernel bandwidth. Abadie and Imbens (2005) show that using the bootstrap after nearest neighbor matching, until recently a common approach to estimating standard errors in evaluation studies, does not yield valid estimates. Bootstrapping standard errors for kernel matching estimators is not subject to this criticism because the number of observations used in the match increases with the sample size.[9]

4. Results

Propensity Score Matching

For both the EGS and FFD programs, probit models were estimated using a broad set of control variables to construct propensity scores used to match program recipients to nonrecipients. In propensity score matching, it is desirable to over-parameterize the probit from the point of view of a model of the determinants of food aid participation in order to find the closest match possible. It is also important to condition the match on variables that are highly associated with the outcome variables, such as lagged values of the outcomes (Heckman and Navarro-Lozano 2004). In a series of t-tests, we tested the "balancing property" of the probit specification to ensure that the treatment sample and the sample of comparison observations have similar mean propensity scores and observables at various levels of the propensity scores. Results reported here are based on a preferred specification for which we could not reject equality of the average propensity

[9] We are grateful to a reviewer for alerting us to this finding, which was relevant to our results based on nearest neighbor matching in a previous draft.

score, nor equality of the mean of each control variable, between treatment and comparison observations within quantiles of the propensity score.

Smith and Todd (2005) note that there is little guidance available to researchers on how to select the set of conditioning variables used to construct the propensity score. In particular, *t*-tests on the significance of individual parameters and goodness-of-fit measures like the share of dependent variable observations correctly predicted can be misleading (Heckman and Navarro-Lozano 2004). We focused on finding a set of conditioning variables that, on theoretical grounds and according to information in the ERHS survey, should be highly associated with the probability of participating in each food aid program and with the outcomes of interest. Although we do not place a causal interpretation on the parameter estimates from this model, the estimates demonstrate association.

For EGS, the control variables chosen include lagged changes in log consumption per adult equivalent between previous rounds of the ERHS survey; pre-drought (1999) land area owned and its square; pre-drought household demographics variables (household size (in 2002), dependency ratio, female headship, log head age); whether the household head had any formal education; whether all household members were too weak, sick, young, or old to work; the wage differential between EGS and the agricultural labor market interacted with household demographic variables; an indicator for whether the household met any targeting criteria for EGS in its village; whether the household reported experiencing a drought from 2000-2002 or a death or serious illness shock from 1999-2002; and measures of the household's political and social connections in the village.[10]

[10] We investigated alternative specifications for the probit model of EGS participation associated with each outcome variable. In particular, we considered including lags of the outcome variable rather than lagged changes in log consumption in the models of changes in log food consumption and livestock holdings. Using lagged outcome variables did not change the results significantly. In the livestock model, including lagged changes in log consumption and lagged changes in livestock holdings did not satisfy the balancing property.

Table 2 presents the model of participation in the EGS used to create propensity scores for the matching algorithm. While recognizing that t-statistics provide only partial guidance, the results suggest that the probability of participating in EGS is declining in household head age. This may be evidence of screening by program managers for younger, more productive workers or that older household heads have a higher opportunity cost of their time. There is a very large negative association with participation in the program for households in which all members have an age- or work-related disability for EGS, showing that the work requirements of EGS played an important role in excluding this group from the program. These estimates also show some evidence of favoritism in awarding positions on EGS teams, in that having a parent who plays an important role in village social life is associated with a higher probability of participating in the EGS. These results also suggest that having access to detailed information on program eligibility and social position can be important in matching households in impact evaluation based on propensity score matching.

For the FFD program, the estimated propensity scores were based on many of the same variables used for EGS, including lagged changes in log consumption per adult equivalent between previous rounds of the ERHS survey; pre-drought (1999) land area owned and its square; the pre-drought household demographics variables; whether all household members were too weak, sick, young or old to work; controls for drought, death and illness shocks from 1999-2002; and measures of the household's political and social connections in the village. Some control variables were included or changed specification based on tests of the balancing property and robustness checks for estimated impacts. These include the household head's highest grade completed in school; whether the household head's primary job was farming; and whether a parent of the respondent holds a local official position (interacted with regional dummies). As before, we also included an indicator for whether the household met any targeting criteria for FFD in its village.

Table 2—Probit estimates for participation in Employment Generation Schemes (EGS) or receipt of Free Food Distribution (FFD)

	Mean	EGS	FFD
Difference in ln real consumption per adult equivalent, 1997-1999	-0.039	0.031	-0.071**
		(1.053)	(2.364)
Difference in ln real consumption per adult equivalent, 1995-1997	0.315	0.037	-0.020
		(1.068)	(0.575)
Difference in ln real consumption per adult equivalent, 1994-1995	-0.213	0.033	-0.023
		(1.076)	(0.698)
Land area owned (hectares)	1.167	-0.048	-0.057
		(0.638)	(0.797)
Land area owned squared	2.633	0.027	-0.001
		(1.490)	(0.067)
Ln household size in 2002	1.646	0.041	-0.057
		(0.842)	(1.179)
Dependency ratio	1.279	0.002	-0.004
		(0.108)	(0.228)
Ln of household head age	3.833	-0.213***	0.049
		(2.717)	(0.549)
Household head is female[a]	0.291	-0.073	0.005
		(1.454)	(0.076)
Household head has any formal education[a]	0.193	-0.026	
		(0.404)	
Household head's highest completed grade in school	1.017		-0.006
			(0.502)
If household head primary job is farmer[a]	0.758		0.028
			(0.393)
Household members weak/sick/young/old[a]	0.085	-0.490***	0.203*
		(5.443)	(2.506)
Market-EGS wages differential × number of male household members age 15-64	-2.391	-0.002	
		(0.591)	
Market-EGS wages differential × number of household members age 0-64	-2.198	-0.002	
		(0.756)	
Market-EGS wages differential × number of household members age 65 and up	0.023	0.007	
		(1.062)	
Household met at least one community targeting criterion for EGS[a]	0.790	0.087	
		(1.511)	
Household met at least one community targeting criterion for FFD[a]	0.451		-0.034
			(0.598)
Household experienced drought between 2000-2002[a]	0.798	0.033	0.028
		(0.636)	(0.511)
Household member died, 2002-2005[a]	0.230	0.010	0.045
		(0.207)	(0.854)
Male household member had serious illness, 1999-2002[a]	0.089	-0.092	0.003
		(1.131)	(0.040)
Female household member had serious illness, 1999-2002[a]	0.075	0.024	0.019
		(0.293)	(0.227)
Household head born in this PA[a]	0.710	-0.062	
		(1.164)	
Parent holds official position in kebele, Tigray region[a]	0.018		0.024
			(0.152)
Parent holds official position in kebele, Amhara region[a]	0.033		0.045
			(0.385)
Parent holds official position in kebele, Oromia region[a]	0.035		0.047
			(0.392)
Parent holds official position in kebele, SNNPR region[a]	0.063		0.140
			(1.600)
Father or mother important in PA social life[a]	0.675	0.105**	-0.037
		(2.289)	(0.799)
Number of *iddir* household belonged to prior to drought	0.770	-0.021	-0.017
		(0.530)	(0.389)
Number of people that will help in time of need (network size)	7.693	-0.002	-0.006**
		(0.964)	(2.070)
Network size has declined in last five years	0.349	0.027	
		(0.530)	
Network size has grown in last five years	0.310	-0.056	
		(1.104)	
N		644	639
Pseudo R-square		0.212	0.156
Observed probability		0.634	0.596
Predicted probability at means of X		0.678	0.602

Notes: Dependent variable equals one if household received that food aid program (EGS or FFD) between September 2002 and March 2004 in a village with drought-related food aid, and zero otherwise. Household demographics, consumption and asset variables are from 1999 unless otherwise stated. Absolute value of z statistics are in parentheses. * = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level. Estimates included village (PA) dummy variables (not shown).

[a] Results are presented as the change in the probability for an infinitesimal change in each continuous X variable, and as the discrete change in the probability from changing the value from 0 to 1 for dummy X variables.

Table 2 presents estimates of the model of receipt of FFD. The FFD model identifies a number of relationships that are different from those for EGS participation, providing some justification for treating these food aid programs separately. The estimates show that the probability of receiving FFD falls with faster growth in consumption in the period before the drought, from 1997-1999. There is also evidence that, in contrast to the EGS, households with disabled, elderly, or sick members are significantly *more* likely to receive FFD, which shows that the two programs are effectively targeting on this characteristic albeit in opposite directions. Also, having a larger informal insurance network is associated with a lower probability of receiving free food, an indication that village officials use their knowledge of a household's social capital in determining whether it receives the program.

Using estimated propensity scores for each program from the models in Table 2, we generated samples of matched program participants and nonparticipants for the EGS and FFD separately using kernel matching.[11] For each program, recipients with estimated propensity score above the maximum or below the minimum propensity score for the comparison group were treated as not having "common support" in the comparison group and so were dropped from the matched sample (see Smith and Todd 2005). We use the resulting separate samples of matched participants and nonparticipant households for the EGS and FFD programs to calculate the impact of each program roughly 18 months after the drought. For consumption, food consumption, and livestock holdings, we compute the difference-in-differences estimated average treatment effect as the difference in the change in the mean of the outcome variable between 1999 and 2004 between participants and nonparticipants in the matched sample. The estimated average treatment effect of the programs on food security is based on respondents' recall in 2004 of the change in their perceived famine risk over the last five years, as either "less, same, or more." Since this variable is retrospective, rather than being based on responses to a question about current perceived famine risk obtained separately in 1999 and in 2004, we do not regard the

[11] We conducted the propensity score matching using the psmatch2 procedure in Stata (Leuven and Sianesi 2003). An epanechnikov kernel was used with bandwidth set at 0.06.

impact measures as difference-in-differences estimates. As a result, these estimates may be more likely to suffer from bias due to omitted household fixed effects.

Average Impact of Participation in EGS

Table 3 presents estimates of the average impact of participation in the EGS in the period after the 2002 drought on welfare by March 2004.[12] The consumption outcomes considered include growth in household consumption or food consumption per adult equivalent, measured for equation (5) as

$$Y^1 = \ln\left(C^1_{2004} / C^1_{1999}\right)$$

for program participants and

$$Y^0 = \ln\left(C^0_{2004} / C^0_{1999}\right)$$

for nonparticipants, where C_t is consumption or food consumption in period t (in per adult equivalent terms). The results show a large, significant effect of participation in the EGS after the 2002 drought on both average growth in log consumption per adult equivalent and on average growth in log food consumption per adult equivalent from 1999 to 2004. While consumption for nonparticipants in the matched sample stagnated over this period (row 2), EGS participants experienced strong growth in average consumption (row 1). The estimated treatment effect of 0.215 is large. It is equivalent to 24 percent growth in the ratio of average real consumption per adult equivalent of EGS participants to matched nonparticipants over this period (a 4.4 percent annual growth rate). The impact on food consumption is even greater. The estimated average treatment effect of 0.289 in column 2 is equivalent to a 33.5 percent increase in the ratio of average

[12] Although the impact estimates were somewhat sensitive to the specification of the participation probit model, and less so to the propensity score matching technique, the results presented here generally held up under most specifications considered.

Table 3—Difference-in-difference estimates of the impact of participation in Employment Generation Schemes (EGS)

	Consumption								
	EGS past 18 months		Excluding Shumsha		EGS past 6 months				
Outcome variable[a]	**Total (1)**	**Food (2)**	**Total (3)**	**Food (4)**	**Total (5)**	**Food (6)**	**Famine risk (7)**	**Livestock (8)**	**Livestock trimmed[b] (9)**
Mean impact									
Average outcome, EGS participants	0.178	0.277	0.213	0.354	0.323	0.437	1.884	0.723	0.696
Average outcome, nonparticipants	-0.037	-0.012	0.043	0.145	0.254	0.308	2.023	1.253	0.776
Difference in average outcomes ATT	0.215*	0.289**	0.170	0.209	0.069	0.129	-0.140	-0.530**	-0.080
	(1.879)	(2.252)	(1.436)	(1.580)	(0.488)	(0.780)	(1.229)	(1.970)	(0.501)
Impact by tertiles of real consumption per adult equivalent, 1999									
ATT in tertile 1	-0.041	-0.040					0.145	-0.223	0.063
	(0.309)	(0.250)					(0.956)	(0.581)	(0.265)
ATT in tertile 2	0.247*	0.394***					-0.313*	-0.108	0.114
	(0.309)	(2.866)					(1.849)	(0.364)	(0.630)
ATT in tertile 3	0.254*	0.295*					-0.299	-1.008**	-0.369
	(1.880)	(1.888)					(1.439)	(2.334)	(1.284)

Notes: Absolute values of t statistics on ATT are in parentheses. These are based on bootstrapped standard errors using 1,000 replications of the sample. * = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level.

[a] Outcome variables for consumption are change in monthly log real total consumption per adult equivalent, 1999-2004, and change in monthly log real food consumption per adult equivalent, 1999-2004. The famine risk variable is an indicator of the household's perceived famine risk in 2004 relative to 1999, where 1 = less, 2 = same, 3 = more. The livestock variable is change in the real value of livestock in thousands of Ethiopian birr, 1999-2004.

[b] Results based on trimmed distribution of change in real value of livestock holdings, with the top 2.5 percent and bottom 2.5 percent of the distribution removed.

real food consumption per adult equivalent of EGS participants to that of matched nonparticipants from 1999-2004.

These estimates represent the average impact of receiving food-for-work at any time in the 18-month period from September 2002 – March 2004. Although most of the EGS transfers during that period occurred in the first 12 months after the drought, food aid transfers are likely to have declining effects through time. We want to explore whether these estimated impacts are due mostly to very recent EGS transfers or whether they reflect some persistence in effects from the large transfers received in the period immediately following the 2002 drought. We devised two tests to inform this investigation (see columns 3-6 of Table 3). First, we dropped Shumsha village from the analysis, since it was the only village in our sample with substantial EGS employment during the period of consumption recall.[13] With Shumsha village removed, the ATT on consumption growth remained large at 0.170, but was no longer statistically significant (p-value = 0.155), and the ATT for food consumption growth fell modestly to 0.209 and was no longer significant (p-value = 0.115). This suggests that contemporaneous EGS transfers in Shumsha may be responsible for some of the impact reported in columns 1 and 2 of Table 3, though Shumsha may have also experienced lagged benefits from significant food aid transfers there immediately after the drought. Second, we estimated the average treatment effect of participating in EGS only during the last six months (from September 2003-March 2004), adding measures to capture receipt of EGS in the first 12 months after the drought to the set of control variables for the matching model.[14] These models produce much smaller point estimates of the impact of EGS participation, with an ATT of only 0.069 and 0.128 on the log ratio of consumption and the log ratio of food consumption, respectively. Neither estimate was significant. This suggests that the much

[13] Shumsha undertook a large expansion of its EGS program in March 2004 when the household data were collected. In that month, 74 households, or 61.2 percent of households in the village sample, took part in food-for-work, working a total of 1,672 days on the EGS program.

[14] These variables include a dummy variable indicating any participation in EGS from September 2002-March 2003, and a second variable for the number of days worked by any household member under the EGS from March 2003-September 2003.

larger and significant average impact estimates from columns 1 and 2 of Table 3 must be capturing some of the impact of EGS transfers received more than six months ago. These results lend considerable support to the hypothesis that food aid has persistent impacts on consumption.

We estimated the impact of the EGS on changing food security during this period, based on respondents' qualitative recall in 2004 of perceived famine risk relative to five years ago. Responses were coded as an increasing index: 1 = less, 2 = same, 3 = more. Column 7 of Table 3 shows that households working in public works after the drought reported somewhat lower famine risk on average than five years ago, while average famine risk was nearly unchanged over the same period for those not involved in public works. However, the resulting impact estimate is insignificant.

Livestock are the most important asset for most households in this sample, both as a source of savings and, in the case of cattle, as a source of draft power. We investigated the possibility that, by bolstering food consumption after the drought, food aid substitutes for livestock assets as a source of *ex post* consumption insurance. We compared the change in the value of all livestock holdings (including cattle, sheep, goats, and large ruminants) from 1999-2004 between EGS participants and nonparticipants in the matched sample. Column 8 of Table 3 shows that the average growth in livestock holdings was 530 birr smaller for EGS participants than for matched nonparticipants 18 months after the peak of the drought, and this difference is significantly different from zero.

There are several possible explanations for this negative estimated impact of food-for-work on growth of livestock holdings. One is that this effect demonstrates reduced demand for precautionary savings by EGS participants, who were more convinced than nonparticipants that food aid would be made available during future food crises. Alternatively, the smaller increase in livestock holdings could be evidence of binding labor constraints made worse by participation in the EGS, since labor is a compliment to livestock in animal husbandry. Another possibility is that EGS participants had to increase their food consumption to meet higher food energy requirements derived from the effort of working on EGS projects. If these additional

program-induced food requirements were large enough, EGS participants may still have had to draw down livestock assets in order to meet their food needs. However, per capita food consumption jumped more than 30 percent for EGS participants from 1999-2004, which is probably too large an effect to be accounted for entirely by work-related increases in food energy demand.

Two other possible explanations include bias in impact measurement and the role of outliers. The smaller growth in asset levels for EGS participants could indicate measurement bias if program targeting or self-selection took place based on an unobservable household characteristic not controlled for in the matching that was correlated with growth in livestock holdings. Though we have a rich set of control variables, we cannot rule out this possibility. We also investigated whether this negative impact estimate was being driven by a small number of observations on nonparticipants that reported very large increases in livestock holdings. Column 9 of Table 3 reports the average treatment effect estimated on a modified sample in which observations in the top 2.5 percent and bottom 2.5 percent of the distribution of the change in real livestock holdings from 1999-2004 were removed. Trimming the outcome variable in this way removed 35 observations from the data set and lead to a substantial reduction in the estimated growth of livestock holdings for matched nonparticipants in the sample. Using this trimmed sample, the estimated impact of EGS participation on growth in livestock holdings is no longer significant (p-value = 0.617), suggesting that a relatively small number of comparison observations are responsible for most of the estimated impact in the full sample.[15]

Heterogeneous Impacts of Participation in EGS by Consumption Tertiles

The average impact of participation in EGS may mask significant impacts of the program on some households. To investigate this possibility, we estimated the impact of

[15] We also estimated EGS impact on growth in consumption and food consumption using trimmed samples removing the top and bottom 1 percent, 2.5 percent, and then 5 percent of the outcome variable, respectively. In each case, the impact of the program remained positive and significant.

EGS participation on relative food security and on growth in consumption, food consumption, and asset holdings by tertiles of 1999 real household consumption per adult equivalent. Results are presented in the bottom portion of Table 3 for the main models for each outcome.

These estimates show considerable variation in impacts of EGS participation across the distribution of 1999 household expenditure. The program has no effect on the growth of household consumption or food consumption for households in the poorest tertile, but it has large, positive, and significant effects on both outcomes for households in tertiles 2 and 3. This pattern at least partly reflects differences in the distribution of days worked in the EGS as described in Gilligan and Hoddinott (2004). EGS participants in the poorest tertile worked 32.4 days on average over the past 12 months, while those in the middle tertile worked 46.4 days on average and those in the top tertile worked 41.5 days on average. One reason for this observed difference in intensity of participation may be tighter labor constraints in poor households (Barrett and Clay 2003).

The magnitude of the difference in impacts between households in tertile 1 and those in tertiles 2 and 3 suggests factors other than participation intensity must also play a role. One explanation is that the pattern of impacts across the consumption distribution reflects differences in program impact across villages, which differ substantially in levels of welfare. However, adding controls for village fixed effects to the estimates of impact by consumption tertile did not alter the basic pattern of impacts on consumption or food consumption in Table 3. Another possible explanation is that relatively wealthier households have access to more complementary sources of capital that can be used to convert transfers from the EGS program into more lasting effects on consumption. However, these effects would have to exist outside the substantial set of control variables used for matching participants to nonparticipants.

There is further evidence of heterogenous impacts of EGS participation for changes in famine risk. Among households in the middle consumption tertile, EGS participants report significantly larger reductions in famine risk than non-participants. The negative effects of participating in public works on growth in livestock holdings are

limited to the highest tertile of the consumption distribution. As with the average impact, this effect disappears in the trimmed sample.

Average Impact of Participation in FFD

Table 4 presents estimates of the impact of participating in FFD on consumption, food consumption, food security, and livestock holdings in the matched sample. The effects of receiving free food through FFD after the 2002 drought on average growth in household consumption per adult equivalent and on growth in livestock holdings are not significant. However, free food receipt has a large and significant effect on the growth in log food consumption per adult equivalent. The treatment effect is equal to a 28.5 percent increase in the ratio of participant to nonparticipant food consumption from 1999-2004. This impact of the free food distribution program on growth in food consumption

Table 4—Difference-in-difference estimates of the impact of receipt of Free Food Distribution (FFD)

	Consumption, FFD past 18 months			
Outcome variable[a]	**Total**	**Food**	**Famine risk**	**Livestock**
Mean impact				
Average outcome, FFD participants	0.151	0.285	1.891	0.790
Average outcome, nonparticipants	0.021	0.034	1.732	0.541
Difference in average outcomes, ATT	0.129	0.251**	0.159*	0.249
	(1.531)	(2.579)	(1.705)	(1.090)
Impact by tertiles of real consumption per adult equivalent, 1999				
ATT in tertile 1	0.111	0.257*	-0.129	0.060
	(0.925)	(1.652)	(0.445)	(0.450)
ATT in tertile 2	0.133	0.207	0.624	0.115
	(1.231)	(1.638)	(1.492)	(0.750)
ATT in tertile 3	-0.063	0.084	-0.121	0.180
	(0.468)	(0.576)	(0.307)	(1.146)

Notes: Absolute values of t statistics on ATT are in parentheses. These are based on bootstrapped standard errors using 1,000 replications of the sample. * = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level.

[a] Outcome variables for consumption are change in log real total consumption per adult equivalent, 1999-2004, and change in log real food consumption per adult equivalent, 1999-2004. The famine risk variable is an indicator of the household's perceived famine risk in 2004 relative to 1999, where 1 = less, 2 = same, 3 = more. The livestock variable is change in the real value of livestock in thousands of Ethiopian birr, 1999-2004.

is only five percentage points lower than the average impact of the much larger food-for-work program, which transferred 90 percent more resources to households in the ERHS sample than FFD. This provides some evidence that the free food distribution program is more cost effective as a strategy for raising food consumption.

It is not possible to test whether this large impact of FFD on growth in food consumption reflects persistence of food aid received immediately after the drought because the data on FFD receipts are reported over the entire period rather than on a monthly basis. Still, we know that the bulk of food aid was disbursed in the first 12 months after the drought, which suggests that these measured impacts may reflect some persistent effects of transfers received several months before the survey.

Despite the large impact of the FFD program on growth in food consumption, results show that receipt of free food distribution causes a significant increase in perceived famine risk. One possible explanation for this unexpected result is that households receiving free food after the drought who were not recent food aid recipients may treat the program as a signal of a decline in their food security. As a simple test for the plausibility of this explanation, we used the sample of households receiving free food in 2004 and regressed the variable for perceived famine risk on an indicator for whether the household received free food in 1999, controlling for 1999 food consumption per adult equivalent, the growth in food consumption from 1999-2004, and village fixed effects. On average, free food recipients in 2004 that did not receive free food in 1999 reported a significantly higher increase in perceived famine risk.[16]

Heterogeneous Impacts of Participation in FFD by Expenditure Quintiles

Following the drought, free food distribution was generally better targeted to the poor and to other eligible groups than were public works (Gilligan and Hoddinott 2004). We present evidence on whether this targeting effectiveness translated into better

[16] Of course, some of this effect may be measuring effective targeting of actual increased relative famine risk among those not previously receiving food aid that is not captured by controlling for food consumption and food consumption growth.

outcomes for the poor by comparing impacts of receiving free food across tertiles of 1999 consumption per adult equivalent. Estimates are presented in Table 4.

Looking across the pre-drought welfare distribution, we still find no significant impacts of the FFD program on either growth in household consumption or growth in livestock holdings. For growth in food consumption, the significant average effects in column 2 are shown to be targeted towards households in the poorest tertile, in contrast to the EGS that disproportionately benefited those in the middle and upper tail of the consumption distribution. This result is consistent with the objectives of the free food program to reach households with limited labor endowments. These households tend to be poorer, with more elderly and disabled members.

The significant positive average effect of the program on famine risk is not found in the tertiles of the consumption distribution. However, this effect appears to derive from households in the middle of the distribution. This is somewhat surprising, given that the point estimate on the impact of FFD transfers on food consumption growth for these households, while imprecisely measured, is close to that for the poorest households who show no effects of the program on famine risk.

5. Conclusions

Using a propensity score matching estimator, we find large significant average treatment effects of EGS participation on growth of total consumption per adult equivalent and food consumption per adult equivalent 18 months after the 2002 drought in rural Ethiopia for the sample from the ERHS panel. Results disaggregated by tertiles of the pre-drought consumption distribution show that these benefits are skewed toward households in the middle and upper tail of the distribution. This is consistent with the evidence on program targeting that shows that the work requirements of the EGS make the poor less likely to participate (Gilligan and Hoddinott 2004). Results also show that EGS participants had significantly slower growth of livestock holdings from 1999-2004 and that this effect was strongest among relatively wealthier households. This finding is

consistent with reduced demand for precautionary savings as recipient households gain greater confidence in the reliability of food aid transfers as a form of insurance. However, the significance of this effect is also driven in part by outlier observations with very large growth in livestock holdings in the matched comparison group.

The free food distribution program demonstrated fewer and smaller effects than the EGS, which derives in part from the more narrow coverage and smaller transfers from the FFD. The program had significant average impacts on growth in food consumption per adult equivalent, and these benefits were better targeted toward the poor. Surprisingly, receipt of FFD also contributed to higher perceived famine risk relative to five years ago.

Overall, these results suggest that emergency food aid played an important role in improving welfare, access to food, and food security for many households following the drought in 2002. However, improved targeting, especially in EGS, and larger, sustained transfers may be required to increase benefits, particularly to the poorest households.

The impacts of food aid identified here indicate some persistence or accumulated effects of transfers on consumption growth over time. Although the time lag between receipt of transfers and observed consumption is not more than one year in most cases, the estimated impact on consumption growth relative to the size and timing of transfers suggests possible savings or multiplier effects of emergency food aid. There are several possible explanations for these effects. One possibility is an efficiency wage argument. Food aid transfers over a number of months following the drought may have assisted adults in conserving body mass. When good rains appeared the following year, food aid beneficiaries were physically better able to take advantage of this opportunity when planting and harvesting their crops. Although our estimates suggest that many food aid recipients, particularly wealthier EGS participants, did not respond by investing in livestock, they may have used other forms of savings or may have invested some of the transfers on their farms or elsewhere. However, we stress that these alternative explanations are speculative. Investigating them, and other possibilities, is the subject of ongoing research.

Appendix

This appendix addresses whether it is better to estimate the impact of each food aid program (EGS or FFD) with or without the recipients of the other program in the sample. Including beneficiaries of the other program could contaminate impact estimates. For example, impact estimates would be biased downward if the potential comparison group is more likely to receive the other program. Alternatively, removing all beneficiaries of the other program from both the treatment and comparison groups can also lead to bias. In general, dropping treatment or comparison households from concentrated portions of the outcome distribution will also create biased estimates of mean outcomes. Also, shrinkage of the potential comparison group may cause many treatment households to be dropped from the analysis due to lack of a suitable matched comparison households. Such treatment households are said to lack "common support." Heckman, Ichimura, and Todd (1997) note that dropping a large number of treatment observations due to lack of common support leads to biased estimates of the average impact of the program.

We tried several approaches to dealing with this problem and investigating which source of bias was greater. First, for each program, we tried including an indicator for whether the household received the *other* program as a control variable in the propensity score matching and found that adding this control did not change the results. Also, for EGS, we argue that FFD transfers are unlikely to create an upward bias in estimates of EGS impact because kernel-weighted average FFD transfers to the comparison group of non-EGS participants are nearly double the FFD transfers received by EGS participant households in the matched sample (p-value on equality of FFD transfers is 0.024).

Next, we estimated the impact of each program excluding households that received the other program from both the treatment and comparison groups. Because 40 percent of households in villages with food aid received both EGS and FFD, this substantially reduced the size of both the treatment and comparison groups, making it difficult to find matches with common support. For the EGS propensity score matching

model, dropping households that did not receive the FFD program greatly reduced the sample, from 704 to 276 households. The share of households participating in the EGS fell from 63.4 in the full sample to 59.4 in the sample without FFD participants. Using this restricted sample, the estimated impact of the program on growth in consumption and food consumption fell sharply, to 0.068 and 0.176, respectively, and neither impact estimate was significant (columns 1 and 2 of Appendix Table 5). The smaller impacts in the restricted sample arose from somewhat smaller growth in consumption for EGS participants than in the full sample, but more so from considerably higher consumption

Table 5—Difference-in-difference estimates of the impact of participation in EGS and FFD, restricting recipients of the other program from the sample

	For EGS		For FFD	
	Consumption, without FFD		Consumption, without EGS	
Outcome variable[a]	**Total (1)**	**Food (2)**	**Total (3)**	**Food (4)**
Mean impact				
Average outcome, participants	0.153	0.234	0.185	0.409
Average outcome, nonparticipants	0.085	0.059	0.182	0.264
Difference in average outcomes, ATT	0.068	0.176	0.003	0.145
	(0.362)	(0.817)	(0.013)	(0.580)

Notes: Absolute values of t statistics on ATT are in parentheses. These are based on bootstrapped standard errors using 1000 replications of the sample. * = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level.

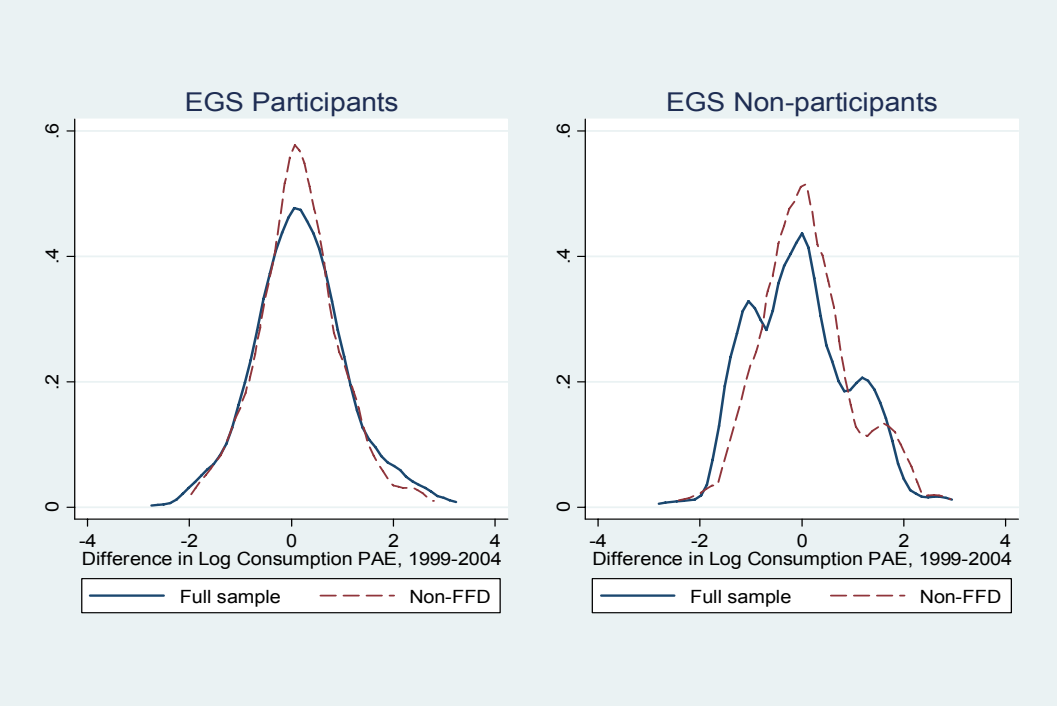
[a] Outcome variables for consumption are change in monthly log real total consumption per adult equivalent, 1999-2004, and change in monthly log real food consumption per adult equivalent, 1999-2004.

growth for households not in the EGS. This pattern is presented in greater detail in Appendix Figure 1. The figure compares the kernel density of the change in log consumption from 1999-2004 for EGS participants and nonparticipants in the matched full sample to those from the matched restricted sample without FFD recipients.[17] The distribution of consumption growth is similar for EGS participants in the two samples, but nonparticipants have very different distributions with a much fatter lower tail in the

[17] Observations are weighted using weights given to matched observations in the kernel matching algorithm run on the two samples.

full sample than the restricted sample. Again, concerns that FFD transfers may go disproportionately to EGS participants and lead to overestimates of EGS impact appear to be unfounded, given that the EGS distribution is fairly robust to removing households that also receive the FFD. However, the differences in distributions for the comparison group suggest that the restricted, non-FFD sample will not provide reliable estimates of the average impact of the EGS program. In particular, restricting FFD recipients from the sample creates a new form of bias by eliminating many of the poorest households from the comparison group. Keeping FFD recipients in the sample appears unlikely to create substantial bias in the impact estimates for the EGS, while removing them may introduce significant new sources of bias.

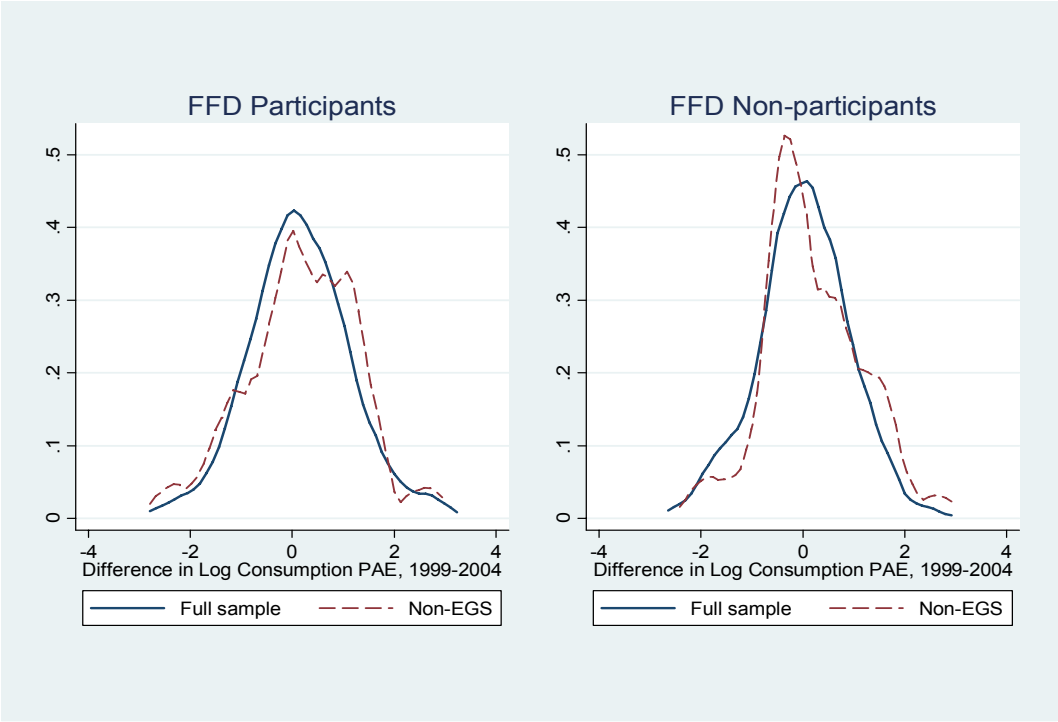
Figure 1—Changes in the kernel-weighted distribution of consumption growth when FFD recipients are excluded from the sample



We also explored potential bias in the FFD impact estimates from inclusion of EGS participants in the matched sample. In t-tests, we could not reject the hypothesis that kernel-weighted average EGS transfers were the same size between FFD recipients

and nonrecipients in the matched sample (p-value = 0.985), so EGS transfers are unlikely to contribute to over- or underestimates of FFD program impacts. We also tested whether the impact estimates were robust to removing EGS participants from the matched sample. Dropping all EGS participants from the list of FFD recipients and the comparison group reduced the sample for the FFD model from 718 to 263 households. Based on this restricted sample, consumption growth for FFD recipients was nearly identical to that of matched nonrecipients over the period (column 3 of Appendix Table 5). Food consumption growth was much higher for FFD recipients and nonrecipients in the sample with EGS participants removed than in the full sample, but the difference-in-difference impact estimate is 0.145 and is insignificant (column 4 of Appendix Table 5). This estimate is considerably smaller than the estimate of 0.251 on the full sample. Appendix Figure 2 shows how the distribution of the difference in log consumption changes for FFD recipients and matched nonrecipients when EGS participants are

Figure 2—Changes in the kernel-weighted distribution of consumption growth when EGS recipients are excluded from the sample



removed from the sample. These changes to the distribution, particularly the shift to the right in this distribution for FFD recipients when EGS recipients are removed, suggest that the presence of the EGS is not determining the impact estimates in the full sample. We conclude that the impact estimates from the full sample are more reliable.

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