Examining the incentive effects of food aid on household behavior in rural Ethiopia

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1. Introduction

The last ten years has seen a marked increase in World Bank lending on Social Protection - measures that include targeted income support, publicly mandated unemployment or old-age insurance and interventions that improve or protect human capital. Not only do these interventions improve equity and welfare by reducing vulnerability to shocks, particularly amongst poor households, they enhance economic efficiency by preserving capital and allowing households to shift income efficiently over the life-cycle, thus financing consumption when needed. While these interventions convey benefits, they also have both direct and indirect costs. Direct costs reflect the resources devoted to providing the interventions. Indirect costs reflect, in part, the changes in behavior that such programs might induce. For example, subsidies or transfers might cause some households to reduce their labor supply, increase consumption of subsidized commodities, crowd out of private transfers or affect relocation and migration decisions.[1] Further, these short-term disincentive effects may create longer-term dependency on publicly provided social protection.

While debates over the potentially adverse consequences of social protection have a long history in developed countries, there has been much less study of these issues in developing countries. One exception would seem to be debates over the impact of food aid which appear to echo these concerns over the benefits and costs of social protection interventions. Food aid resources are often used as a form of social protection in that they are transferred to individuals and households to mitigate the impact of shocks such as drought, flood or civil conflict. But while food aid is credited with saving literally

[1] Moffitt (1992, 2003), Cox and Jimenez (1992) and Jensen (1998) discuss a number of these issues in more depth.

millions of lives over the last several decades, it is surrounded by controversy.[2] At the household level for example, it is claimed to have disincentive effects - reducing labor supply and crowding-out private transfers and other means of informal responses to shocks while in the longer-term creating dependency by reducing investment in income generating activities such as agriculture. However, it is often argued that quantities of food aid provided are too small given recipient needs, and that considerable uncertainties surround the timing and delivery of food aid. The juxtaposition of these frequently-made claims is weird. Collectively, they suggest that food aid recipients in developing countries make major behavioral changes that are perceived to be costly to their own long-term well-being in response to food aid flows that are small and unpredictable.

Further, much of this literature ignores the possibility that food aid has different effects on different types of households. For example, the responsiveness of labor supply to increases in income changes as households become wealthier. For poor households, the opportunity cost of consuming leisure – in terms of the utility of foregone consumption – is higher than it is for wealthier households. This implies that disincentive effects may rise with household income or wealth. It also implies, as Barrett and Maxwell (2003) point out, that observed disincentive effects of food aid may reflect poor targeting. Second, recent work – both theoretical and empirical – has focused on the possibility of 'poverty traps' in developing countries. Ravallion (2003) points to a simple example from the efficiency wage literature (Mirlees, 1975, Dasgupta and Ray, 1986, Dasgupta, 1993) to illustrate this. Suppose that worker productivity is a function of past consumption and that consumption above a certain threshold is required in order to be

[2] The vast literature on food aid and food aid related issues is reviewed in Barrett (2002), Barrett and Maxwell (2003) and Hoddinott, Cohen and Bas (2003).

productive. A shock occurs, such as illness, which reduces consumption below this threshold rendering the worker permanently unproductive. Alternatively, consider an environment where access to credit relies on collateral. A shock occurs, say a flood, which destroys the stock of collateral and thus denies the household further access to credit. Again, permanent poverty results as the more formal models of Banerjee and Newman (1993) and Galor and Zeira (1993) illustrate. Alternatively, lowered asset holdings cause households to restrict themselves to low return activities or inferior technologies, see Dercon and Krishnan (1996) and Barrett (2003) for examples. In all three cases, food assistance can play a role in: increasing consumption and thus worker productivity; relieving credit constraints; or allowing households to take greater risks.

Given the strength of feelings these claims generate, one would expect that there is a considerable body of carefully analyzed empirical evidence on the disincentive and dependency effects of food aid on household behavior. But this is not the case. Lentz (2003) provides a careful annotated bibliography covering papers on the dependency and disincentive of food aid published in the last 25 years. Her review shows that, strikingly, while a number of authors claim that such disincentive effects exist, these claims are often based on case studies and anecdotes. Our own review of this literature reaches the same conclusion as do Barrett and Maxwell (2003, p. 3-76), namely that, "No one has published any solid empirical evidence of "dependency" on food aid. People simply assume and assert dependency to exist, perhaps explain why it could occur in theory, or offer an anecdote or two rather than any systematic evidence."

Accordingly, this paper enters this contested debate drawing on data on household level data on food aid flows and several dimensions of household and inter-household

behavior in selected areas of rural Ethiopia in the mid-1990s. Specifically, it asks whether access to food aid – both food-for-work and food assistance - was causally related to changes in labor supply, investments in a variety of agricultural activities and in the provision of mutual, informal support. When simple test statistics are constructed, such as a comparison of means, or when simple regressions are run, the disincentive effects of food aid on household behaviors are many, large in magnitude and statistically significant. *However*, when household characteristics such as age, sex and education of head, land holdings, size and location are taken into account, many of these adverse effects vanish. These results strongly suggest that the anecdotes and case studies upon which these dependency and disincentive claims are built are fatally flawed because they take correlations and ascribe causal links to them.

2. Uncovering causal links between food aid receipts and household behavior

In order to understand the causal links between food aid receipts and household behavior, it is helpful to begin with a simple model. Let y_{ivt} be the behavior of interest – say the amount of labor supplied to agriculture – of household i living in village v at time t. Let A_{ivt} equal one if this household receives food aid, zero otherwise. We could also imagine that there may be some random factors that also cause labor supply to vary from time-to-time and we denote these as $?_{ivt}$. With this notation in mind, we can write.

$$y_{ivt} = ß_A \bullet A_{ivt} + ?_{ivt} \qquad (1)$$

$ß_A$ tells us how much agricultural labor supply - y_{ivt} – changes when a household receives food aid, i.e. when A_{ivt} equals one. Anecdotes and case studies are qualitative representations of (1). They claim that when a household receives food aid, that this

causes a change in household behavior, the y_{ivt} variable. Statistically, the relationship in (1) can be examined by comparing, say, the mean labor supply of households receiving, or not receiving, food aid or by estimating $ß_A$ using linear regressions.

However, there is a strong assumption underlying this approach, namely that no other factors influence household labor supply or whatever behavior is being represented by y_{ivt}. This is a strong assumption. Suppose, for example, we have two villages. Village A has fertile soils and good, reliable rainfall. Village B is drought-prone and soils are poor. We would expect, all other things held constant, that household labor supply is higher in Village A – where the returns to labor are higher – than in Village B and that Village B should be targeted for food aid distribution. However, using equation (1) as our guide, we would conclude that food aid deliveries have disincentive effects – that they are reducing labor supply, when in fact it is some other factor – poor agro-ecological potential – which is the true causal factor. To take a second example, farmers are often told *not* to use fertilizers during droughts since fertilizers often only induce increases in yield when water supply is adequate. If a drought also leads to food aid deliveries, we will seem to have observed, *a la* equation (1), that food aid has discouraged the use of fertilizers.

More generally, taking estimates derived from (1) at face value assumes that $E(A_{ivt}?_{ivt}) = 0$, that is, access to aid and other household characteristics are assumed to be uncorrelated. As Rosenzweig and Wolpin (1986) point out, this condition is likely to be violated when planners or project staff purposively site a project based on household or locality level characteristics. In the context considered here, we might believe that food aid goes to more marginal or drought-prone rural areas. Estimation of (1) ignores such a

possibility. All of this is to say that it is critically important to distinguish correlations and causation. A better representation of the links between food aid and household behaviors is therefore:

$$y_{ivt} = ß_A \bullet A_{ivt} + ß_V \bullet V_{vt} + ß_H \bullet X_{ivt} + ?_{ivt} \qquad (2)$$

Here, V_{vt} and X_{ivt} are, respectively, village and household level characteristics at time t, and $ß_V$ and $ß_H$ give the impact of these characteristics on labor supply. $ß_A$, $ß_H$ and $ß_H$ will give consistent estimates of these impacts provided that A_{ivt}, V_{vt} and X_{ivt} are uncorrelated with $?_{ivt}$.

3. Data

We draw on three rounds of the on-going Ethiopian Rural Household Survey (ERHS) collected in 1994 (twice) and 1995. The ERHS is a multi-purpose survey containing rich data on a wide variety of dimensions of welfare and their evolution over time. These include, but are not limited to, data on a wide range of assets, income, consumption, schooling, health as well as the causal factors associated with these such as changing access to public services, shocks, public and private interventions to offset such shocks, processes of agricultural intensification and diversification, entry into or exit from high value activities in agriculture and other sectors.

The sample consists of approximately 1470 households residing in 15 villages. It is important to recognize that these data are not nationally representative. However, the villages were selected so as to be representative of the principal agro-ecological zones found in rural Ethiopia. They include ox-plough agricultural systems found in the North and Central highlands as well as coffee and enset (a root crop) producing households in

the South. Households were randomly selected within each site and in numbers proportional to the population of the region relative to the national population. Virtually all households are farmers have received land from local government authorities. In 1994 and 1995, households were interviewed three times, with approximately five months elapsing between each round, by enumerators resident in the survey villages. Attrition over these three rounds was approximately 4 per cent and appears to have been random. Dercon and Krishnan (2000) provide further survey details and Bevan and Pankhurst (1996) provide ethnographies for each of the surveyed villages.

As Table 1 shows, in this sample access to food aid varied over time. Just over one out every five households received food aid prior to the first survey round, with this figure rising to 40 per cent prior to round two, then falling to 20 per cent prior to round three. Nearly half of surveyed households did not report receiving any food aid and 28 per cent of households received it once. Few households, 5 per cent, reported receiving food aid at least once prior to all three survey rounds. Table 2 shows how access to food aid varies by village and round. There are several villages, Yetemen, Aze Deboa and Debre Berhan with no or little access to food aid in this period. Another village, Shumsha, has almost continuous access and accounts for nearly all households that received food aid in all three rounds. For the remaining villages, however, there is no clear pattern over time. In some, such as Korodegaga, it appears, becomes nearly universally available, than disappears. In others, such as Doma, it appears and remains present while in Geblen, access falls from nearly universal coverage (prior to round 2) to being almost largely absent.

One reason why these variations might be observed relates to the targeting of food aid. If food aid goes to poorer villages, or villages receiving shocks, then would expect to see patterns such as those described in the previous paragraph. However, there seems to be little *prima facie* evidence to support such an argument. Table 3 reports correlation coefficients between the proportion of households receiving food aid and two measures of welfare, median real village per capita consumption (excluding food aid) and median village per capita holdings of cultivable land. While all of these are correctly – negatively – signed, none of these are statistically significant.

Jayne, Strauss, Yamano and Molla (2002) and Sharp (1997) provide useful reviews of the targeting of food aid in rural Ethiopia. In addition to a significant amount of mistargeting, Jayne *et. al*. (2002) show that "inertia effects" in food aid allocations are powerful. A critical factor in explaining whether a locality receives food aid is whether it received it in previous years. Table 3 looks at this possibility by calculating correlation coefficients between the proportion of households receiving food aid and the proportion of households receiving food aid in the aftermath of the catastrophic 1982-84 droughts. The signs are positive and the correlations have a markedly higher level of statistical significance, consistent with the "inertia effects" argument.

4. Food aid, dependency and disincentive effects

We now turn to our empirical examination of the disincentive effects of food aid on household behaviors in rural Ethiopia. We begin with some descriptive results before turning to the results of estimating equations (1) and (2).

4.1 Basic results

We begin by considering whether the means or proportions of three broad sets of household behaviors observed in 1995 vary by access to food aid. Specifically, we consider whether past access to food aid, as measured by whether households had been recipients of food aid – either as participants in food-for-work programs or as recipients of food transfers – in the periods prior to the two survey rounds conducted in 1994 – reduced measures of labor supply, agricultural investments or mutual support in 1995. In addition, we explore whether access to food aid, observed contemporaneously with these behaviors, affects these activities. This distinction allows us to consider whether food aid received at one point in time (i.e. in 1994) affects future activities (behaviors observed in 1995) as well as whether food aid currently received affects current activities.

Table 4 considers seven different aspects of household labor supply, disaggregated by type of activity and individuals within the household undertaking these activities. It indicates that households who received food aid prior to round 1, ie. approximately one year before these activities were observed, spent considerably less time supplying labor to permanent and semi-permanent crops. Less time was also spent on non-agricultural own business activities by adult males, females and children. Summing across all groups within the household, on average, households not receiving food aid worked five times as many days in these activities than did households who did (11 versus 2.1). Conversely, children in households receiving food aid spent more time in wage work than children in households not receiving food aid. However, the disincentive effects are less clear when we compare households who received food aid prior to round 2, ie. approximately five months before these activities were observed. While less time

was spent by food aid recipients on supplying labor to permanent and semi-permanent crops, there is no meaningful difference in labor supply to non-agricultural own business activities. When we consider the associations between labor supply and current receipt of food aid (ie, whether the household receives food aid in round 3), the disincentive effect on agricultural labor remains. However, while male labor supply to non-agricultural own business seems to fall with receipt of food aid, female labor time devoted to non-agricultural own business rises and time spent in wage work is higher for all household demographic groups amongst households receiving food aid.

Table 5 considers five different aspects of household investments in 1995: whether households grew coffee or enset, whether they purchased fertilizers or pesticides, or whether they made investments in livestock, the principal savings mechanism in much of rural Ethiopia. Here, the results are unambiguous. Apart from livestock purchases, households with access to food aid – either in the recent past or currently – are less likely to undertake any of these investments. Note too that some of the magnitudes of these differences appear to be substantial. For example, households not receiving food aid would seem to be 2.5 to ten times more likely to grow coffee than households not receiving aid.

Households in rural Ethiopia, as do households elsewhere in developing countries, engage in a variety of informal, mutual support, activities. Table 6 considers whether access to food aid displaces these two measures of these informal links, transfers (in-cash and in-kind) received from other households since the previous survey round and the number of days the household contributes to traditional labor sharing parties called *debbo* in Ethiopia. There does not appear to be any evidence that receipt of food aid leads to

lower receipts of private transfers but there is some suggestion that past receipt of food aid reduces participation in labor sharing groups. Even here, however, the magnitude of the difference (1.5 days) does not appear large.

Table 7 reports the results of estimating equation (1), where the dependent variables are the 14 household behaviors described above. A number of these are 0/1 variables and for these, we estimate (1) using a probit. The remainder contain a mix of observations, so of which have zero values and some of which have positive values and for these, we estimate tobits.[3] The coefficients reported for the probits are not readily interpretable and so marginal effects are also reported. Generally, the results reported here complement the descriptive materials provided in Tables 4 through 6. Access to food aid would seem to have significant disincentive effects on investments in agricultural activities and the magnitudes of some of these effects are large.[4] Receipts of food aid lower the likelihood that a household grows coffee by 11 to 20 percentage points and the likelihood of using fertilizer by 11 to 18 percentage points. As before, the impact on labor supply is more varied. Food aid seems to lower family labor supply on permanent or semi-permanent crops by about 20 to 26 days per month. Past access to food aid has mixed impacts on labor supplied to non-agricultural own business activities and contemporaneous access increases labor supplied to off-farm wage work.

As noted in section 2, these purported disincentive effects are all based on the assumption that receipt of food aid and other household characteristics are uncorrelated.

[3] Given the well-known limitations associated with estimating tobits, in preliminary work we attempted to estimate Powell's (1984) censored least absolute deviation model but, as have others, encountered severe difficulties in getting these estimates to converge.

[4] Note that while the values of food aid received can be computed from the survey data, we do not use this variable here. If these values are measured with error, for example because of difficulties remembering how many days were worked in the past several months, then inclusion of this variable will produce a coefficient biased downwards towards zero. Put another way, using a dummy variable representation of access to food aid makes it more likely that we will pick up disincentive effects.

Table 8 reports the results of equation (2), where household characteristics – location, age and sex of the household head, whether the head ever attended school, household size and households' holdings of arable land –that might plausibly also affect these behaviors are also included as additional controls.[5] The results are considerably different from those reported in Table 7. All negative effects of food aid disappear with two exceptions. Food aid received a year ago (in round 1) reduces the likelihood of growing enset, but by a trivial amount, one percentage point. While contemporaneous access to food aid reduces time spent on permanent and non-permanent crops, the magnitude of these effect – a reduction of eight days – is about 1/3 of that reported in Table 7 and arguably, this is offset by the increased amount of labor on off-farm labor that food aid receipt induces.[6]

Tables 4 through 7 are essentially statistical representations of anecdotes and case studies and would seem to support a claim of marked disincentive effects of food aid on household behavior in rural Ethiopia. Taken at face value, the results of Table 8 suggest that such claims are fatally flawed because they take correlations and ascribe causal links to them. When we control for household characteristics such as location, virtually all these negative effects disappear. However, there are two limitations with the results reported above. First, we might wonder whether our results are biased by our choice of estimator for variables that have a mix of zero and positive values. Second, suppose receipt of food aid was correlated with a household characteristic – say a shock – that varied over time. If this were true, $E(A_{ivt}?_{ivt})$? 0 and the parameter estimates reported in Table 8 would be inconsistent.

[5] The village dummies capture all time-invariant characteristics that differ across villages as well as the impact of (slightly) different amounts of time that elapse between the second and third round interviews across the sample.

[6] Few children actually participate in off-farm wage work relative to the number of regressors we include and so results for this behavior are not reported.

Table 9 addresses these concerns jointly. The dependent variables are six dimensions of labor supply (family labor supplied to permanent and semi-permanent crops; labor supply of adult males and females to off-farm wage work; and labor supply of adult males, females and children to non-agricultural own business activities) and two dimensions of mutual support (transfers received from other households and days contributed to traditional labor sharing groups). As before, we consider the impact of past access to food aid (food aid received in rounds 1 and 2) as well as food aid received contemporaneously with these behaviors in round 3. For each round, two sets of results are reported. The first column reports results based on using least squares estimation with the standard errors corrected for potential heteroscedasticity using the methods outlined by Huber (1967) and White (1980).[7] These results generally mimic those found in Table 7, suggesting that concern over possible bias resulting from the use of tobits in Table 7 is probably unfounded.

The second columns reported by round address the concern that $E(A_{ivt}?_{ivt})$ might not equal zero even with the inclusion of locality and household characteristics as controls. They report the results of estimating Maddala's (1983) "treatments" regression model. Using maximum likelihood, we estimate the following model:

$$y_{ivt} = ß_A \bullet A_{ivt} + ß_V \bullet V_{vt} + ß_H \bullet X_{ivt} + ?_{ivt} \qquad (2')$$

$$A_{ivt} = ß_{AV} \bullet V_{vt} + ß_Z \bullet Z_{ivt} + e_{ivt} \qquad (3)$$

Equation (2') is identical to (2) except that A_{ivt} , considered endogenous, is determined by equation (3). As in Table 8, a number of household characteristics are included as control variables or components of V_{vt} (household location) and X_{ivt}: age and

[7] Correcting these standard errors to control for cluster level correlations – sometimes called design effects – produces similar results.

sex of the household head, whether the head ever attended school, household size, households' holdings of arable land, whether the household had experienced a shock in the last five years and whether it had recovered from that shock. Z_{ivt} contains five dummy variables that serve as instruments, past access to food aid – in the three years preceding the survey - as well as food aid received in the aftermath of the 1983/84 drought. These instruments reflect the "inertia effect" of food aid targeting described by Jayne *et. al*. (2002). As noted in Table 3, they are correlated with current access and do not appear to directly affect the outcomes being examined here.[8] The attraction of Maddala's estimator here is twofold: it ensures that $E(A_{ivt}?_{ivt}) =0$ so that our estimates of $ß_A$ – that is, our estimates of possible disincentive effects – are consistent; and it provides a chi-squared test of whether the error terms in (2') and (3) are correlated.[9]

We begin by comparing the simple OLS results ("food aid treated as exogenous, no controls") to the results from the treatment regressions ("food aid treated as endogenous, controls incl.") of the impact of past receipt of food aid in the two 1994 survey rounds (rounds 1 and 2) on labor supply and mutual support observed in round 3. Results are somewhat varied. When we consider the impact of food aid received in round 1, the simple OLS suggest that disincentive effects are pervasive. However, these vanish in the cases of labor supply to non-agricultural activities or, fall markedly in magnitude as in the case of labor supply to permanent and semi-permanent crops. When we consider the impact of food aid received in round 2, there are small disincentive effects observed in the case of labor supply to permanent and semi-permanent crops and in days contributed to traditional labor sharing groups.

[8] In addition, the X_{ivt} found in (2') also appear in (3).
[9] Rejecting the null hypothesis that the error terms are correlated implies that OLS estimates of (2') will produce biased coefficients.

The last columns of Table 9 examine the impact of contemporaneous receipt of food aid and these household behaviors. Here the results are less ambiguous. Controlling for household and locality characteristics as well as the potential endogeneity of food aid, receipt of food aid *increases* labor supply, particularly by males. There is a disincentive effect on days contributed to traditional labor sharing groups, but this is small in magnitude, on average, one day out of the previous five months.

5. Conclusions

Food aid, like other safety net and transfer programs, continues to be controversial. In Ethiopia, work by Quisumbing (2003) and Yamano, Christiaensen and Alderman (2003) indicates that food aid plays an important role in protecting the nutritional status of pre-school children, particularly in the aftermath of shocks. Yet, in Ethiopia as elsewhere in the developing world, there is a perception that food aid fosters dependency and creates disincentive effects at the household level and that these effects are inimical to longer-term development.

This paper enters this contested debate drawing on data on household level data on food aid flows and several dimensions of household and inter-household behavior in selected areas of rural Ethiopia in the mid-1990s. It asks whether access to food aid – both food-for-work and food assistance - was causally related to changes in labor supply, investments in a variety of agricultural activities and in the provision of mutual, informal support. Simple test statistics, such as a comparison of means, or simple regressions, suggest that the disincentive effects of food aid on household behaviors are many, large in magnitude and statistically significant. *However*, when we take into account household

characteristics such as age, sex and education of head, land holdings, size and location, many of these adverse effects vanish. In fact, there is some suggestion in these data that food aid leads to increases in labor supply to agriculture, wage work and own business activities.

As with any empirical study, these results should be treated with care. There may be other dimensions of household behavior not considered here that food aid adversely affects. There may be adverse market effects or adverse effects on government policies. These data pertain to only 15 of the thousands of villages in rural Ethiopia. And these data pertain to household behaviors in the mid-1990s that may have subsequently changed. However, they strongly suggest that the anecdotes and case studies upon which dependency and disincentive claims are frequently built are methodologically flawed. In this sense, the paper's principal contribution is methodological – emphasizing how misleading statements regarding disincentive effects can be when confounding effects are not taken into account.

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Table 1: Access to food aid by round

Percentage of households receiving food aid:	
Prior to	
Round 1	22%
Round 2	40
Round 3	20
Prior to all rounds	5.7
Prior to two rounds	18.2
Prior to one round	28.1
Never	48.0

Table 2: Proportion of households receiving food aid by village and by round

Region	Village	Round 1	Round 2	Round 3
Tigray	Haresaw	67.4	13.3	52.6
	Geblen	77.7	97.7	3.5
N. Shoa	Dinki	0	97.7	0
	Debre Berhan	1.1	7.1	1.7
Gojjam	Yetemen	0	0	0
S. Wollo	Shumsha	95.9	80.5	63.5
Shoa	Sirbani Godeti	1.0	13.3	0
	Imdibir	7.5	23.9	61.2
	Aze Deboa	0	2.4	2.4
S. Shoa	Trirufe Ketchema	5.8	23.1	14.3
Hararghe	Adele Keke	27.9	15.4	1.0
Arssi	Korodegaga	1.0	100.	17.3
Sidamo	Adado	0.8	28.4	0
	Gara Godo	2.7	5.4	10.6
Doma	Gama Gofa	1.3	96.0	74.7

Table 3: Village level correlations between selected village characteristics and access to food aid

	Round 1	Round 2	Round 3
Correlation between percentage of households receiving food aid and:			
Mean household per capita consumption	-0.13	-0.37	-0.05
	(0.60)	(0.13)	(0.83)
Mean household per capita holdings of arable land	-0.26	-0.10	-0.21
	(0.30)	(0.69)	(0.41)
Proportion of households receiving food aid in 1984	0.34	0.40	0.29
	(0.17)	(0.10)*	(0.25)
Proportion of households receiving food aid in 1985	0.67	0.62	0.48
	(0.00)**	(0.00)**	(0.04)**

Table 4: Paired t-tests of access to food aid and aspects of labor supply in 1995

Aspect of labor supply	Description	Received food aid in round 1?		Received food aid in round 2?		Received food aid in round 3?	
		Yes	No	Yes	No	Yes	No
Family labor supply on permanent and semi-permanent crops	Number of days worked by family members on the cultivation, planting or harvesting of permanent or semi-permanent crops in the previous 30 days	1.9	9.8	4.1	10.7	3.4	9.2
t test on difference in means		7.11**		7.00**		5.04**	
Labor supply of adult males on off-farm wage work	Number of days worked for pay by males, aged 18 and older, since the previous survey round. This includes farm work, professional or salaried work, piece or wage work undertaken by unskilled, semi-skilled or skilled laborers or work as domestic servants but excludes days worked on food-for-work or cash-for-work projects or days spent working in traditional labor sharing activities.	7.2	7.6	8.2	7.0	14.0	5.8
t test on difference in means		-0.16		-0.64		-3.75**	
Labor supply of adult females on off-farm wage work	Number of days worked for pay by females, aged 18 and older, since the previous survey round. It includes and excludes the same set of activities described for adult males.	1.9	1.5	1.7	1.5	4.0	1.0
t test on difference in means		-0.62		-0.56		-5.09**	
Labor supply of children on off-farm wage work	Number of days worked for pay by children aged less than 18 since the previous survey round.	2.3	0.5	1.5	0.5	3.6	0.3
t test on difference in means		-3.66**		-2.49**		-6.81**	

Labor supply of adult males to non-agricultural own business activities	Number of days worked in self-employed non-agricultural own business activities by males, aged 18 and older, since the previous survey round. This includes activities such as weaving, spinning, milling, pottery, other handicrafts, trade and transportation.	1.1	5.8	4.4	5.3	5.2	3.0
t test on difference in means		4.21**		-0.93		1.85*	
Labor supply of adult females to non-agricultural own business activities	Number of days worked in self-employed non-agricultural own business activities by females, aged 18 and older, since the previous survey round.	0.9	3.7	2.6	3.4	6.1	2.3
t test on difference in means		2.91**		0.99		-3.79**	
Labor supply of children to non-agricultural own business activities	Number of days worked in self-employed non-agricultural own business activities by children aged less than 18 since the previous survey round.	0.1	1.5	1.1	1.2	0.9	1.3
t test on difference in means		2.99**		0.27		0.73	

Table 5: Paired t-tests of access to food aid and agricultural investments in 1995

	Received food aid in round 1?		Received food aid in round 2?		Received food aid in round 3?	
	Yes	No	Yes	No	Yes	No
Proportion of households growing coffee	1.5	22.2	6.3	25.2	8.3	20.0
t test on difference in means	9.04**		9.91**		4.87**	
Proportion of households growing enset	1.2	15.2	8.5	14.8	11.9	12.6
t test on difference in means	7.08**		3.75**		0.34	
Proportion of households purchasing fertilizers	1.8	19.4	5.6	22.3	6.7	17.7
t test on difference in means	8.08**		9.13**		4.82**	
Proportion of households purchasing pesticides	0.6	4.1	1.4	4.6	0.6	4.0
t test on difference in means	3.18**		3.40**		2.94**	
Proportion of households purchasing livestock	18.1	21.0	21.0	19.9	19.6	20.4
t test on difference in means	1.16		-0.54		0.30	

Table 6: Paired t-tests of access to food aid and mutual support in 1995

	Received food aid in round 1?		Received food aid in round 2?		Received food aid in round 3?	
	Yes	No	Yes	No	Yes	No
Mean value of transfers received from other households	181.3	158.0	188.6	146.1	251.3	139.6
t test on difference in means	-0.27		-0.58		-1.25	
Mean number of days contributed to traditional labor sharing groups	0.9	2.5	1.8	2.4	2.1	2.2
t test on difference in means	3.54**		1.58		0.27	

Table 7: Estimates of the impact of access to food aid on labor supply, agricultural activities, and mutual support in 1995 – no additional controls

	Dependent variable	Round 1	Round 2	Round 3
(1)	Family labor supply on permanent and semi-permanent crops	-26.55	-20.48	-21.46
		(7.03)**	(7.35)**	(5.86)**
(2)	Labor supply of adult males on off-farm wage work	-1.80	27.72	73.26
		(0.21)	(3.99)**	(9.01)**
(3)	Labor supply of adult females on off-farm wage work	15.44	-0.75	77.19
		(1.60)	(0.09)	(6.82)**
(4)	Labor supply of children on off-farm wage work	17.79	26.20	82.55
		(1.52)	(2.48)**	(5.67)**
(5)	Labor supply of adult males to non-agricultural own business activities	-56.73	13.21	-21.09
		(4.82)**	(1.94)*	(2.26)**
(6)	Labor supply of adult females to non-agricultural own business activities	-58.54	-10.85	56.65
		(2.94)**	(0.88)	(4.16)**
(7)	Labor supply of children to non-agricultural own business activities	-76.96	-0.80	-15.95
		(2.63)**	(0.07)	(1.05)
(8)	Households growing coffee	-1.41	-0.84	-0.51
		(7.09)**	(8.99)**	(4.52)**
		-0.20	*-0.18*	*-0.11*
(9)	Households growing enset	-1.17	-0.34	-0.02
		(5.87)**	(3.75)**	(0.17)
		-0.14	*-0.07*	*-0.01*
(10)	Households purchasing fertilizers	-1.24	-0.82	-0.56
		(6.74)**	(8.52)**	(4.65)**
		-0.18	*-0.17*	*-0.11*
(11)	Households purchasing pesticides	-0.71	-0.46	-0.71
		(2.71)**	(3.08)**	(2.70)**
		-0.03	*-0.03*	*-0.03*
(12)	Households purchasing livestock	-0.13	0.02	-0.11
		(1.30)	(0.32)	(1.11)
		-0.03	*0.01*	*-0.03*
(13)	Value of transfers received from other households	-484.92	456.61	739.31
		(0.85)	(1.05)	(1.43)
(14)	Days contributed to traditional labor sharing groups	-12.60	-2.14	-1.34
		(5.15)**	(1.30)	(0.65)

Notes:
1. Rows (1)–(7) and (13)-(14) estimated using tobits; all others estimated using probits.
2. Parentheses in rows (1)–(7) and (13)-(14) are absolute values of t statistics; parentheses in other rows are absolute values of z statistics.
3. **, significant at the 5% level; *, significant at the 1% level.
3. Numbers in italics in rows (8)-(12) are marginal effects.
4. Sample sizes are approximately 1460.

Table 8: Estimates of the impact of access to food aid on labor supply, agricultural activities, and mutual support in 1995 with locality and household controls

	Dependent variable	Round 1	Round 2	Round 3
(1)	Family labor supply on permanent and semi-permanent crops	-3.49	1.62	-8.04
		(0.77)	(0.50)	(1.98)**
(2)	Labor supply of adult males on off-farm wage work	-0.01	48.75	67.52
		(0.00)	(4.86)**	(6.42)**
(3)	Labor supply of adult females on off-farm wage work	31.61	38.44	66.38
		(1.97)*	(3.06)**	(4.65)**
(4)	Labor supply of children on off-farm wage work	-	-	-
(5)	Labor supply of adult males to non-agricultural own business activities	-18.45	10.60	-10.79
		(0.84)	(1.02)	(0.95)
(6)	Labor supply of adult females to non-agricultural own business activities	-0.45	38.42	25.41
		(0.02)	(2.58)**	(1.57)
(7)	Labor supply of children to non-agricultural own business activities	-54.46	8.06	-24.98
		(1.09)	(0.51)	(1.41)
(8)	Households growing coffee	-0.60	-0.52	-0.34
		(1.38)	(2.77)**	(1.53)
		-0.03	*-0.03*	*-0.02*
(9)	Households growing enset	-1.27	0.16	-0.29
		(2.35)**	(0.63)	(0.81)
		-0.01	*0.00*	*-0.00*
(10)	Households purchasing fertilizers	-0.28	-0.07	0.11
		(0.96)	(0.36)	(0.53)
		-0.05	*-0.01*	*0.02*
(11)	Households purchasing pesticides	-0.37	-0.13	-0.002
		(1.15)	(0.52)	(0.01)
		-0.02	*-0.01*	*-0.00*
(12)	Households purchasing livestock	-0.16	0.20	-0.19
		(0.68)	(1.60)	(1.46)
		-0.04	*0.06*	*-0.05*
(13)	Value of transfers received from other households	1032.3	585.80	578.96
		(1.01)	(0.86)	(0.85)
(14)	Days contributed to traditional labor sharing groups	4.27	2.42	3.02
		(1.25)	(1.05)	(1.20)

Notes:
1. Rows (1)–(7) and (13)-(14) estimated using tobits; all others estimated using probits.
2. Parentheses in rows (1)–(7) and (13)-(14) are absolute values of t statistics; parentheses in other rows are absolute values of z statistics. **, significant at the 5% level; *, significant at the 1% level.
3. Numbers in italics in rows (8)-(12) are marginal effects.
4. Sample sizes are approximately 1460. Controls are village level dummy variables, age and sex of household head, whether the head ever attended school, household size and household's holdings of arable land.

Table 9: Estimates of the impact of access to food aid on labor supply and mutual support in 1995, treating access as both exogenous and endogenous

	Round 1			Round 2			Round 3		
	Food aid treated as			Food aid treated as			Food aid treated as		
Dependent variable	Exogenous, no controls	Endogenous, controls incl.	Chi-square statistic	Exogenous, no controls	Endogenous, controls incl.	Chi-square statistic	Exogenous, no controls	Endogenous, controls incl.	Chi-square statistic
Family labor supply on permanent and semi-permanent crops	-7.48 (10.48)**	-4.88 (3.08)**	8.15**	-5.98 (6.75)**	-3.91 (2.85)**	0.02	-5.54 (6.50)**	17.30 (10.97)**	92.52**
Labor supply of adult males on off-farm wage work	0.95 (0.53)	19.52 (2.69)**	3.00*	2.44 (1.75)*	6.51 (1.77)*	0.40	9.01 (4.85)**	9.32 (3.37)**	0.02
Labor supply of adult females on off-farm wage work	0.69 (1.18)	-0.71 (1.18)	9.71**	0.35 (0.61)	-1.07 (1.60)	6.83**	3.44 (5.12)**	1.78 (2.05)**	8.88**
Labor supply of adult males to non-agricultural own business activities	-4.57 (5.77)**	20.48 (10.85)**	64.83**	1.18 (1.15)	5.41 (1.31)	0.65	-1.99 (1.77)*	21.65 (5.77)**	6.70**
Labor supply of adult females to non-agricultural own business activities	-2.74 (3.82)**	-2.91 (1.62)	1.29	-0.72 (0.85)	-0.89 (0.76)	3.25*	4.17 (3.01)**	1.24 (0.85)	9.18**
Labor supply of children to non-agricultural own business activities	-1.24 (4.54)**	7.59 (5.58)**	37.71**	0.19 (0.44)	0.89 (1.60)	0.09	-0.15 (0.31)	8.29 (4.05)**	7.38**
Value of transfers received from other households	35.31 (0.30)	317.85 (0.90)	0.13	44.16 (0.57)	1138.72 (1.10)	0.87	110.37 (0.75)	123.12 (0.47)	0.14
Days contributed to traditional labor sharing groups	-1.73 (5.48)**	-1.70 (4.60)**	4.65**	-0.56 (1.41)	-2.39 (2.84)**	6.04**	0.02 (0.04)	-1.29 (2.10)**	25.72**

Notes:
1. Specifications treating access to food aid as exogenous are estimated using OLS with standard errors corrected using the method outlined in Huber (1967) and White (1980).
2. Specifications treating access to food aid as endogenous are estimated using Maddala's (1983) treatment regression estimator with standard errors corrected using the method outlined in Huber (1967) and White (1980). Controls are regional level dummy variables, age and sex of household head, whether the head ever attended school, household size, household's holdings of arable land, whether the household experienced a shock in the last five years and whether it had recovered from that shock. Past access to food aid in the three years preceding the survey as well as food aid received in the aftermath of the 1983/84 drought serve as instruments for current access to food aid.
3. Figures in parentheses are absolute values of t statistics.
4. **, significant at the 5% level; *, significant at the 1% level.
5. Sample sizes are approximately 1460.