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Food Aid and Informal Insurance

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Abstract

Households in developing countries use a variety of informal mechanisms to cope with risk, including mutual support and risk-sharing. These mechanisms cannot avoid that they remain vulnerable to shocks. Public programs in the form of food aid distribution and food-for-work programs are meant to protect vulnerable households from consumption and nutrition downturns by providing a safety net. In this paper we look into the extent to which food aid helps to smooth consumption by reducing the impact of negative shocks, taking into account informal risk-sharing arrangements. Using panel data from Ethiopia, we find that despite relatively poor targeting of the food aid, the programs contribute to better consumption outcomes, largely via intra-village risk sharing.

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1 Introduction

It is generally acknowledged that developing and transition economies need better safety nets (see e.g.Drèze and Sen (1990), World Development Report 2000/01). Current provision often takes the form of direct transfer program such as the distribution of food-aid and public employment programmes with in-kind wages. Their aim is to support the poor and vulnerable and to prevent current and future deprivation and insufficient nutrition[1]. Despite calls for more long-term safety nets, they remain largely relief programmes, in response to emergencies (Clay and Stokke (1991)).

The problems with food-based support programs and their efficiency in developing countries has received a lot of attention (for reviews see Ravallion (1991), Clay (1986), Barrett (2001)). Much of this literature has been concerned with questions about the programme objectives, their long-run sustainability and dependency problems (Maxwell and Singer (1979), Ruttan (1993), Stewart (1998), Barrett (2000)) or the incentive effects of transfers on labour supply, food production and other productive activities (Sahn and Alderman (1996), Dorosh et al. (1995), Maxwell (1991), Maxwell et al. (1994), Mohapatra et al. (1997), Barrett (1998a), Bezuneh et al. (1988)). Other studies try to quantify the net transfer benefits, taking into account the opportunity cost of time spent on food-for-work (Datt and Ravallion (1994)). There is a sizeable literature on the impact of ration systems and supplementary feeding programmes (Beaton and Ghassemi (1982), Kennedy and Alderman (1987), Alderman (1991)). A few papers have directly addressed the issue of nutrional impacts of food distribution and food-for-work programmes (Athanasios et al. (1994), Janyne, Strauss et al. (1999)), while some have focused on the indirect effects, such as on-farm investment (Bezuneh et al. (1988), Bezuhen and Deaton (1997)).

In recent years the focus has been on problems related to the targeting of transfers (Besley and Kanbur (1990), Sen (1995), van de Walle and Nead (1995)). Building on the success of the Maharastra Employment Guarantee Scheme, self-selection based employment schemes are often advocated as providing a partial solution to these targeting problems (Drèze and Sen (1990), Ravallion (1990), (Barrett (2001), Alderman and Lindert (2001)). Empirical work on rural data has tried to quantify these targeting issues and ask whether the poor do obtain the transfers(Jayne et al. (2001), Teklu and Asefa (1999), Ravallion (1990), von Braun (1995)).

To the extent that empirical research has focused on the impact of transfers on the welfare of the poor, much of this work suffers from at least four problems. First, many studies, especially those on food aid and food-for-work, focus on whether the poor are reached or not, without directly evaluating the

[1] Different terms tend to be used to describe the objectives of these programs. Often, they aim to promote food security, which can be defined as the freedom from the risk of insufficient nutrition, thereby avoiding current and future deprivation. Different authors use different definitions. Reduced 'nutritional risk' or 'vulnerability' are used in a similar sense (Beaton (1987), Morduch (1994), Maxwell (1996), Dercon and Krishnan (2000), Christiaensen (2000), Barrett (2001)).

impact on the livelihoods of the poor. Secondly, even if they do, they rarely address the issue of the impact in terms of nutritional *risk*, and focus only on the direct effect on current incomes and nutrition, partly due to the lack of longitudinal data that can document vulnerability and welfare dynamics (Barrett (2001)). Thirdly, and linked to this, they do not consider the presence of alternative ways of coping with consumption or nutrition shortfalls, such as running down assets and relying on informal support networks (Morduch (1995), Deaton (1992)). Any impact evaluation ought to take these alternatives into account for they will affect the sum total of support available: for instance, Attanasio and Rios-Rull (1998) demonstrate large crowding out effects. Fourthly, they fail to acknowledge the econometric problems related to program placement effects (see Rosenzweig and Wolpin (1994) for a classic treatment). In particular, the recipients of support may receive it due to certain characteristics, unobserved to the researcher, that also affect the recipients' nutrition or consumption outcomes thus biasing the results of the impact evaluation.

In this chapter, we study food aid distribution in rural Ethiopia, and try to address some of these problems. As in other studies, we distinguish two questions. First, what determines the allocation rule of food aid in Ethiopia? By studying the allocation rule, we can also address the issue of whether food aid is indeed used as a form of insurance and so, is responsive to negative shocks. Secondly, what is the impact on consumption of food aid transfers? For this question, we explicitly tackle the issue of how food aid might interact with informal sharing arrangements within the village. (Issues related to the long-term impact of the safety net, for instance, investment in productive assets, are discussed in the chapter by Barrett et al. in this volume.)

Per capita GDP is estimated at about 140 US dollars per annum, child malnutrition (stunting) is estimated at well-above 50 percent during the 1990s and adult malnutrition is about 25 percent (World Bank (1999), Christiaensen and Alderman (2001), Dercon and Krishnan (2000)). Furthermore, its population lives in a highly risky environment: drought is a recurrent phenomenon, requiring large public responses (as in 1994 and 1999) or, in conjunction with failing public policy and war, triggering a large scale famine in 1974 and in 1984-85. Ethiopia is also an important recipient of food aid, and is arguably increasingly dependent on it. The World Food Programme estimates for 1994-98 suggest that Ethiopia is the second largest recipient of food aid in the world (after Bangladesh). In the 1990s, volumes of food aid accounted for about 5 to 15 percent of production (Clay et al. (1998)). Food aid is largely distributed via food-for-work programmes - the best estimate puts its share at 63 percent of food aid, while the rest is largely distributed as direct (free) transfers. Food-for-work and direct food aid distribution are virtually the only publicly-provided safety net in rural Ethiopia. A few recent studies have documented and analysed the effectiveness of food aid delivery, uncovering important deficiencies, although the focus has largely been on targeting issues (Sharp (1997), Clay et al. (1998), Jayne et al. (1999, 2001), Barrett et al. (2001)).

In the next section, we discuss food aid distribution in Ethiopia and the data used in this chapter. In the third section, the village-level and within village

allocation rule for food aid is analysed. In section 4, we present a theoretical framework to test the impact of a safety net on households faced with income risk. An empirical model is developed in section 5 and this is used in section 6. Section 7 concludes.

2 Data and descriptive statistics

Ethiopia offers an obvious opportunity to study the impact of safety nets on the household's ability to keep consumption smooth. It is one of the highest recipients of food aid and faces a harsh and variable climate. Both donors and the government have committed themselves to forming a well-functioning safety net. The Food Security Strategy (Federal Democratic Republic of Ethiopia, 1996) distinguishes between food-for-work or other income generating labour schemes (supplementary employment and income schemes), aimed at able-bodied adults, and targeted interventions for especially vulnerable groups. They cover both interventions in large scale crisis and programmes designed to reach particular groups over longer periods. In practice, most interventions involve food aid. In turn, food aid has long contributed to food supplies in Ethiopia. This dependence has been exacerbated by the food shortages during the famine in 1984-85, the increasingly desperate situation in many rural areas in the late 1980s, linked to civil war and political turmoil. The annual volume of cereal food aid has typically been about 2,000 to 6,000 metric tons per year in the period 1986-1995, representing about 5 to 15 percent of production. Even in average years, the volume of cereal food aid in a given region can account for 25 percent or more of total marketed supply of grain, increasing to up to 50 percent in drought years (Clay et al. (1998)). A substantial portion (over 80 percent in bad years) of food aid has been used for emergency relief purposes.

Food aid is usually distributed across across regions and districts by the government while the actual allocation to beneficiaries at the local level is decided by local village officials. In rural areas, this will be the peasant association, which is a local government institution covering one or more villages. For food-for-work, self-targeting is not often used, but even if eligible, households must still make a decision of whether to work or not. Alternative opportunities are likely to influence this decision. Often, the workload related to food-for-work activities is not clear while wages are typically rather high, and probably higher than the opportunity cost of time. The result is that usually more people apply for food-for-work than can be accommodated. In effect, this means that the distinction between food aid and food-for-work is not as important as expected, even though allocation rules differ. This is investigated further below.

What is the evidence on the targeting of this food aid? Sharp (1997), who reviewed a large body of evaluation studies and conducted several new case studies, found that food aid has been spread too thinly over too many areas and too many people, particularly in recent years. There is little evidence of targeting specific areas. Furthermore, in most cases, participants are selected at the community level, but there is a clear reluctance to select some households while

excluding others, so that much larger numbers are involved in the programmes than intended. The result is that, locally, targeting errors of inclusion (providing aid to people who are not in the intended target group) are a greater problem than errors of exclusion (failing to provide aid to the people who need it most). The result is that often too little aid is provided to the poorest to make much difference. A similar result was found in the sub-sample of the large nationally representative HICES/WMS survey for 1995/96. A number of related studies (Clay et al. (1999), Jayne et al. (2001)) find that the most important factor determining access to food aid was whether there was a programme previously in the area, resulting in a serious regional bias of food aid allocation. Half the food aid distributed went to households with more than sufficient food from their own resources . It should be stressed that these types of findings are not uncommon across other developing countries. Nevertheless, they appear to have encouraged many donors to reassess their activities in these areas. Subsequent careful econometric studies of food aid in Ethiopia (Jayne et al.(2002)) seems to confirm these patterns.

The data used in this paper come from three rounds of the Ethiopian Rural Household Survey, collected in 1994 and 1995. This is a panel data survey collected by the Economics Department of Addis Ababa University in collaboration with the Centre for the Study of African Economies at Oxford University. It covers 15 villages, representative for different areas across the country, and a total of 1450 households were interviewed. The attrition rate in this panel is very low - about 2 percent per year. The survey has detailed information on households, including consumption, assets and income, as well as the shocks they faced. Furthermore, it contains detailed information on participation in food aid and food-for-work programs.

Consumption per adult equivalent[2] (in 1994 prices) is relatively low: about 80 birr on average, which was then about 12 US dollars per month per adult. Using a local nutritional poverty line of 2200 Kcal per adult, this suggests that about 40 percent do not get sufficient calories on average. Shocks are very common, even in this short period and even, given that this was a relatively good year. In about a quarter of the villages, a serious drought occurred while diseases affected crops and livestock in many others. The average household lost several person days a month due to serious illness (more details are in Dercon and Krishnan (2000)). Ability to cope with shocks is quite limited. Many households reported episodes of serious hardship linked to shocks in the last twenty years, related to drought, illness, policy changes and other factors: 80 percent of the sample suffered major economic hardship due to drought, mainly during the famine of the mid-1980s.

Efforts have clearly been made to supply the rural population with food aid. But how effective is it? First, in our sample we have a high percentage of

[2] The consumption data are based on summing and valuing food and non-food consumption obtained via own production, the market and via gifts. It is expressed in real terms by using a consumer price index, using the average household in the first round as a base. It is expressed in adult equivalent units using nutritional equivalence scales based on WHO data for East Africa. All data issues are discussed in Dercon and Krishnan (2000).

Table 1: Percentage of households receiving food aid per round in selected villages

village name	round 1 (1994a)	round 2 (1994b)	round 3 (1995)
Atsbi	64	6	52
Geblen	79	97	5
Ankober	0	98	0
Shumsha	96	80	62
Yetmen	0	0	0

Source: Ethiopian Rural Household Survey.

Table 2: Percentage of households receiving food aid by poverty status

	non-poor (t-1)	poor (t-1)	total (t-1)
no food aid (t,t-1)	52	22	74
food aid (t,t-1)	14	12	26

Source: Ethiopian Rural Household Survey.

households receiving food aid or benefiting from food-for-work in the six months before each round of the survey: about 20 percent in both rounds 1 and 3, and even 39 percent in round 2. However, the spatial and temporal spread is very large. In five villages (out of 15) programs are always present, while in 7 others, there was a program in place some of the time and in 3 other villages, there was no food aid at all. Furthermore, the coverage in terms of households changed considerably over time. Table 1 gives the details for a few villages.

Clearly, one village (Ankober) had a program with virtually complete coverage in the one period, to disappear in the other periods. In Geblen, the program, very active in 1994, almost vanished. Only in Shumsha, (not far from the tourist destination Lalibela - a preferred destination of heads of mission and evaluation teams of donors and NGOs), coverage was always high: in round 1, six agencies are identified as giving food aid. Tables 2 to 4 give some indication about the targeting of the programme. First, we look at whether the poor in preceding period, labelled t-1 received aid during the period between two rounds t and t-1. The poor are identified using a poverty line based on the cost of obtaining sufficient consumption to yield 2200 Kcal per adult and with some allowance for non–food expenditures. (Details are in Dercon and Krishnan (2000).) Effectively about 40 percent are poor in each round, although there is considerable mobilit amongst who might be labelled "poor". Table 1 shows that of the 26 percent of households receiving aid, more than half were non-poor in round t-1. In short, targeting is rather more inclusive than might be intended.

Table 3 examines the effect of common shocks, namely village level rainfall, and divides the sample into groups according to whether the rains in the six months preceding a round are in the lowest tercile (worst rains), middle tercile (median rains) and the best rain tercile, and by whether food aid was received. The table suggests that, of those receiving food aid, most food aid went to

Table 3: Percentage of households receiving food aid by rainfall experience.

	worst rains	median rains	best rains
no food aid (t,t-1)	28	27	21
food aid (t,t-1)	8	2	13

Source: Ethiopian Rural Household Survey.

Table 4: Is food aid targeted to the poor. Percentage of households receiving food aid by agricultural shock experience.

	worst shocks	median shocks	best shocks
no food aid (t,t-1)	27	27	21
food aid (t,t-1)	7	6	12

Source: Ethiopian Rural Household Survey.

those households in villages which experienced the best rains, while the people that did not receive food aid, also suffered worse weather. This description holds true even if the comparison is with rains in the previous period and current food aid. Finally, we also use information on overall shocks to crops, including plant diseases and other idiosyncratic shocks. Households were again divided into terciles by crop shocks and receipt of food aid and similar patterns emerge: using simple descriptive statistics, the evidence suggests that the poor and vulnerable are not clearly targeted. Of course, this is merely suggestive and a proper evaluation requires more analysis to which we now turn.

3 Food aid allocation

In this section, we present some regressions describing the allocation rule used for free food aid and food for work, by providing covariates of whether food aid is obtained and how much aid is received. We use a straightforward reduced-form regression, in which food aid is determined by by a number of household and community characteristics, some time-varying, such as shocks, and others fixed over time. Jayne et al. (2002) conduct a similar analysis using a relatively large nationally representative clustered sample collected in 1996. They have data on a large number of districts (343) and 2796 households. Within each woreda, a few clusters were selected from which households were chosen; these clusters are 5 on average and substantially larger than a village. The large number of districts allows detailed analysis of the allocation to districts but is less amenable in describing the allocation within villages, since only a relatively small number of households per cluster is selected. In contrast, our data consists of panel data on 1450 households from 15 villages. The relatively limited number of villages limits our ability to describe the allocation rules to villages but the rather large random samples within villages allow us to describe allocation within villages with more confidence. The fact that we have panel data allows us to focus more

closely on the role played by time-varying information. Since we ask the same questions, we can and do compare our findings to those in Jayne et al. (2002).

The variables of interest are, whether food-for-work aid has been received and whether free food aid has been received by the household. The model is specified as a probit. A subsequent set of regressions explores the amounts received (conditional on having received some aid). This is effectively a hurdle model[3]. The first set of regressions focus on within village allocation, controlling for village level effects. In particular, a set of village level time varying dummies allows us to focus on the within-village allocation: given that the village gets food aid, how is it allocated? The explanatory variables used are the same for all specifications. We use real consumption per adult and a set of household characteristics, such as the number of male and female adults, total household size, whether the head has completed primary education, and the sex and age of the household head. Since real consumption per adult may well be endogenous, not least since it is measured at the same time as the receipt of the food aid, we use asset variables (land and livestock per adult) as identifying instruments. The use of consumption or asset variables allows us to investigate the extent of income or wealth targeting observed in the data. Furthermore, we use a number of idiosyncratic (or household specific) shock variables to explore the responsiveness of aid targeting to shocks: illness days lost per adult in the preceding four months, an index of whether livestock suffered from disease (where "1" is best) and an index of whether crops suffered in general from poor growing conditions (again, "1" is best). These indices were constructed using a number of questions related to specific problems experienced in the latest growing season (details are in Dercon and Krishnan (2000)). A second set of regressions drops the time-varying village effects, and includes a number of characteristics at the village level. Also included are whether the village had any all-weather road and its distance to the nearest town, as well as the percentage deviation of rainfall in the particular year (normal is coded as "1"). Table 5 provides descriptive statistics.

The probability of receiving food aid or food-for-work is estimated using a standard probit model, using the pooled data, with robust standard errors corrected for village-cluster effects[4]. Since we have panel data, this was also estimated as a conditional fixed effect logit model. This allows a further focus on time-varying information, including whether food aid allocation is responsive to shocks. Where relevant, the results are discussed in the text. Table 6 provides the results for whether any aid is received.

[3] An alternative would be to jointly estimate whether aid has been received and how much, by using a Tobit model or a sample selection model. However, the former would constrain coefficients to be the same for both the decisions on whether and how much aid has been received, while the latter would require identifying instruments to credibly estimate the first stage of the model, which we do not think exist, or identification by functional form which is also not preferable.

[4] We estimated the models also as random effects probit models, and the results are virtually identical, including in terms of significance. Since the models with time-varying village level effects did not converge always using this estimation method, we report only the pooled regression results.

Table 5: Descriptive Statistics: mean and standard deviation (n=3981)

	mean	std dev.
does the household receive any aid?	0.29	0.45
does the household receive free transfers?	0.18	0.39
does the household receive food for work?	0.11	0.31
free food aid per adult (1994 prices)	2.25	15.82
food for work per adult (1994 prices)	0.76	5.66
consumption per adult (1994 prices)	91.61	96.71
livestock per adult (1994 prices)	333.97	501.63
land per adult (1994 prices)	0.36	0.44
household size (no.)	5.15	2.20
number of male adults 15-65 years (no.)	1.55	1.16
number of female adults 15-65 years (no.)	0.96	0.19
male headed? (%)	0.77	0.42
head completed primary education? (%)	0.10	0.29
age head (years)	46.23	15.79
illness days per adult	0.62	2.18
rainfall index (normal=1)	1.13	0.26
% suffering below normal rain	0.34	0.47
crop shocks (%, best=1)	0.48	0.41
livestock disease (%, best=1)	0.83	0.28
village distance to nearest town (km)	9.18	5.06
village has all weather road? (%)	0.53	0.50

Consumption and aid per adult equivalent per month, in birr of 1994; aid is valued at consumer prices; illness days suffered by adults per adult in the household; the rainfall index is calculated as rainfall in the preceding agricultural year relevant to the survey round divided by mean rainfall, minus one, and is measured at the nearest meteorological station - mean values based on typically about 20 years of data; below normal rain is defined as rainfall below the long-term mean; crop shocks is a subjective (self-reported) index of whether main crops suffered moderately or severely from any type of damage (including pests or weather related), where no problem equals 1 and 0 is total failure; livestock disease is a self-reported measure of whether livestock suffered from serious disease between survey rounds, where 1 means no problem. Note that this means that for ALL shocks variables higher variables mean better outcomes, with the exception of illness.

Table 6: Determinants of receiving food aid (marginal effects from probit model with village-time fixed effects, robuts standard errors).

	any free aid?		any food-for-work?	
	marg effect	p-value	marg effect	p-value
consumption per adult	-0.002	0.006	-0.002	0.183
male adults 15-65 years (no.)	-0.035	0.000	0.007	0.621
female adults 15-65 years (no.)	-0.017	0.783	0.012	0.904
male headed? (%)	0.006	0.737	0.014	0.728
household size (no.)	0.001	0.943	-0.009	0.439
head primary education?	0.082	0.106	0.041	0.437
age head (years)	0.000	0.969	-0.001	0.210
illness days per adult	0.001	0.545	-0.003	0.359
crop shocks (%, best=1)	-0.013	0.570	-0.040	0.014
livestock disease (%, best=1)	-0.025	0.621	-0.032	0.164
time varying village dummies	not reported		not reported	
	psuedo R^2	0.69	psuedo R^2	0.46
	n=2447		n=1595	

The underlying data set contains 3981 observations. However, since we use a binary left hand side variable and time-varying fixed effects, all observations from villages that have no food aid at all in a specific period would be perfectly collinear with the village dummies and therefore cannot contribute to understanding the household level determinants, and are excluded. The table reports marginal effects as the derivative of the cumulative normal distribution at the mean of the right hand side variables; for dummies the marginal effect is expressed as the discrete change from 0 to 1 is reported. Real consumption is treated as endogenous using a two-stage regression. Land per adult and livestock values per adult are identifying instruments.

There is evidence of income based targeting within villages (controlling for whether the village received aid). Evaluated at the mean of all other variables, the probability of receiving free food aid increases from 5.8 percent at the 75th percentile of consumption per adult, to about 18.6 percent at the 25th percentile. Note that these marginal effects are somewhat larger than in Jayne et al. (2002, p.273). One reason may be that they analysed *within-district* allocation, and that allocation from districts to villages was not as sensitive to incomes as the allocation within villages. The other significant effect is that households with more male adults are less likely to receive aid (with a 3.5 percent lower probability per male adult), suggesting targeting towards children, elderly and women. For food-for-work programs, the marginal effect of consumption per adult on participating is only very weakly significant, even though the marginal effect is relatively high again, suggesting some targeting (and contrary to Jayne et al. (2002))[5]. Participation in food-for-work is sensitive to idiosyncratic shocks to crops (at 1 percent) or to livestock (significant at 16 percent) - suggesting that food-for-work may perform some insurance function[6]. Whether this is because people are selected to participate on the basis of a recent misadventure or whether they are more likely to choose to participate if shocks occur cannot be assessed. We also offer two checks of the robustness of these results. First, the consumption data contain information on the source of food consumption items, including whether they were received as gifts from public sources. Since current consumption per adult (even if instrumented) may already be boosted by access to food aid, resulting in a spurious relationship, we used an alternative consumption measure, excluding all consumption from public gifts. The results were unaffected by this. Next, a conditional fixed effects logit model was run. The results related to livestock and crop shocks were confirmed; but we did not find a significant effect on consumption per adult. While this suggests that the effects on income targeting using pooled data are caused by missing 'fixed' attributes, correlated to consumption, the most plausible explanation is that these variables are wealth-related variables, and that targeting may still be sensitive to levels of wealth or income, but not sensitive to fluctuations in income[7].

Table 7 describes similar regressions but with the levels of aid received as the left hand side variable and the sample restricted to those households receiving some aid of the relevant type. For those receiving aid, the amounts on average are 13 percent of mean consumption in the case of free aid, and 10 percent if food-for-work. The results suggest that although there is evidence of some targeting in terms of whether people get support, in terms of amounts, the better-off households do get more. At the mean, a one percent increase in consumption results in one percent more aid. Male headed households get more

[5] Evaluated at the mean, the probability of receiving food-for-work goes up from about 2.5 percent at the 75th percentile to 16.4 percent at the 25th percentile.

[6] Including shocks without consumption per adult or the reverse in the regression did not affect the results.

[7] An alternative explanation is that a significant part of the consumption fluctuations are related to random measurement error.

Table 7: Determinants of how much food aid is received per adult (OLS with village-time fixed effects; only those receiving aid), robuts standard errors with cluster effects).

	how much food aid?		how much food-for-work?	
	coeff	p-value	coeff	p-value
consumption per adult	0.140	0.013	0.163	0.187
male adults 15-65 years (no.)	-0.553	0.028	0.240	0.600
fem adults 15-65 years (no.)	1.351	0.713	2.826	0.136
male headed? (%)	1.669	0.030	-0.329	0.744
household size (no.)	1.382	0.298	0.348	0.658
head primary education?	-4.007	0.150	1.733	0.647
age head (years)	0.190	0.160	0.119	0.155
illness days per adult	-0.210	0.188	0.303	0.247
crop shocks (%, best=1)	-1.294	0.043	0.740	0.470
lives disease (%, best=1)	2.149	0.010	-2.407	0.377
constant	-29.470	0.191	-7.385	0.640
time varying vill dummies	not reported		not reported	
	R^2 =	0.16	R^2 =	0.76
	mean=	13.01	mean=	6.95
	n=698		n=363	

Only for those households receiving aid. Real consumption is treated as endogenous using a two-stage regression. Land per adult and livestock values per adult are identifying instruments.

aid, although more male adults in the household reduces the transfer. For those participating in food for work, there is no evidence of income targeting either, in terms of the amounts received. The evidence suggests some targeting of households with more female adults, but this is weakly significant In short, even though there is evidence that within villages, food aid and possibly access to food-for-work displays some targeting, the amounts received are typically higher for the better-off households. These results are unaffected when using an alternative definition of consumption, excluding food received as gifts from public sources. Using a household fixed effects panel estimator, the fixed effects are not significant for free food aid, so that the pooled sample result of higher amounts to the better-off stands, while for food-for-work, a significant positive relationship between the amounts received and consumption levels are obtained as well, providing further evidence that among those receiving food-for-work, the relatively better-off receive more.

Recall that all these results are conditional on village level effects so that they only reflect the allocation rule conditional on a village receiving aid.Next, we consider the factors determining allocation across villages. We estimate the regressions investigating the determinants of who receives food aid or food-for-work again, but this time replacing the time-varying fixed effects with (instrumented) mean consumption per adult in the village in each period, village-level rainfall shocks and the means of the other shock variables[8]. Table 8 gives these results. The within-village effects are largely similar to those in table 6 - in any case, the results in that table offer the preferred interpretation, since they fully control for all possible village level effects. While within the village, there is evidence of income targeting in the case of free transfers and no clear targeting for food-for-work, the evidence here suggests that *better-off* villages are more likely to receive free aid, while there is only very weak evidence of targeting poorer villages via food-for-work (both in terms of significance and size of the effect). The probability of receiving free food aid increases for a household living in a village with mean consumption around the 75th percentile by 18 percentage points, compared to a household living in a village with mean consumption around the 25th percentile. Furthermore, there is no correlation with rainfall, and a positive correlation between (fewer) problems with crops in the village, and receiving food aid. Households seem to be more likely to have access to food-for-work if on average the village had more problems with crops - suggesting that they are offered as a safety net when problems occur. All these results appear robust to alternative specifications, such as changing the number of village level characteristics, defining consumption so as to strictly exclude food aid itself and fixed effects specifications. Replacing consumption by assets, such as land and livestock, showed that areas with more land per adult were significantly more likely to receive free transfers, while they were significantly less likely to receive food-for-work programs. In short, there is clearly no evidence in favour of targeting of free aid programs to poorer areas in our sample,

[8] Adding the means of the other household level variables did not change the interpretation of the results substantially.

Table 8: Determinants of receiving food aid (marginal effects from probit model with village-time fixed effects, robuts standard errors).

	any free aid?		any food-for-work?	
	marg effect	p-value	marg effect	p-value
village mean consumption	0.003	0.035	-0.001	0.198
rainfall (normal=1)	0.110	0.402	0.041	0.621
village mean crop shocks	0.454	0.000	-0.221	0.007
village mean livestock shocks	-0.177	0.403	0.055	0.623
village mean illness days	0.225	0.001	0.082	0.002
consumption per adult	-0.002	0.012	0.000	0.938
male adults 15-65 years (no.)	-0.015	0.027	0.008	0.037
female adults 15-65 years (no.)	-0.005	0.881	0.000	0.999
male headed? (%)	0.019	0.291	-0.011	0.465
household size (no.)	-0.018	0.093	0.001	0.833
head primary education?	-0.004	0.916	0.014	0.210
age head (years)	-0.001	0.073	0.000	0.891
illness days per adult	-0.001	0.243	-0.001	0.362
crop shocks (%, best=1)	-0.010	0.243	-0.014	0.000
livestock disease (%, best=1)	-0.012	0.450	-0.004	0.505
	psuedo R^2	0.41	psuedo R^2	0.19
	n=3981		n=3981	

The underlying data set contains 3318 observations. However, since we use a binary left hand side variable and time-varying fixed effects, all observations from villages that have no food aid at all in a specific period would be perfectly collinear with the village dummies and therefore cannot contribute to understanding the household level determinants, and are therefore excluded. The table reports marginal effects as the derivative of the cumulative normal distribution at the mean of the right hand side variables; for dummies the marginal effect expressed as the discrete change from 0 to 1 is reported. Real consumption is treated as endogenous using a two-stage regression. Land per adult and livestock values per adult are identifying instruments.

but there is some evidence of targeting food-for-work, albeit relatively weak.

These results obviously need careful interpretation. The sample of *villages* is relatively small, and although stratified to represent different typical types of villages, generalising about the village-level effects has to be done with caution. However, the samples within villages are random and relatively large, so that the within-village processes may be well captured in this sample. Jayne et al. (2002) have a much larger and random sample of districts, and their district-level findings are likely to be stronger. They find some income targeting in both food-for-work and free food aid, but they find it also to be rather weak with very small increases in the probability of receiving food aid in a village with mean income near the 25th percentile, compared to villages at the 75th percentile. They discover some sensitivity to non-weather related crop shocks. Both our results and the results in Jayne et al. (2002) suggest that targeting

is, at best, relatively weak, especially across villages. Within the villages, both data sets suggest targeting of (free) food aid, but the evidence on targeting of food-for-work is weaker. However, whether food aid is targeted or not does not necessarily measure fully the impact on households. In the rest of the paper, we analyse this further, with a focus on the contribution of food aid on the consumption path over time and its interaction with alternative means of keeping consumption smooth such as informal reciprocal transfers between households, expanding on Dercon and Krishnan (2003). To this end, we provide a discussion of the theoretical framework for the analysis in the next section.

4 Food aid and risk-sharing

In order to analyse the impact of the safety net provision, we focus on intertemporal behaviour in the face of risk and the existence of community and household based mechanisms to cope with the consequences of risk[9]. As a basis for this analysis, we use standard risk-sharing tests (Townsend (1994)). These tests investigate whether outcomes over time in a risky environment are consistent with those expected if markets were perfect, i.e. as if all risk is insured *ex-ante*. This literature is less concerned with *how* this full insurance occurs - it could occur through 'formalised' market mechanisms or via 'informal' sources, such as mutual support within families or villages. The basis of the most standard tests is to ask whether idiosyncratic shocks contain any information that could explain consumption growth - under perfect risk-sharing they should not. Typically, perfect risk-sharing is rejected: results from a variety of contexts, such as extended families in the United States, communities in India and nuclear households in Ethiopia have failed to find perfect risk sharing but do find evidence of partial risk sharing (Hayashi et al., 1996; Townsend, 1994; Dercon and Krishnan, 2000a). This in turn suggests that there might be a substantial role for interventions that might help households pool risk more effectively (Morduch, 1999). This would provide the support for more widespread protection mechanisms, including via food aid or food-for-work. Clearly, investigating whether these programs indeed contribute to smoother consumption and more risk-sharing is of importance.

However, standard models of transfers predict that private transfers will be reduced if public transfers are introduced and there is some empirical support for this proposition as well (Cox et al., 1998). The presence of informal risk-sharing arrangements has further consequences for the impact of formal transfers. If households share risk, public transfers to specific households might be treated like positive idiosyncratic shocks and hence, shared across households. If informal risk-sharing arrangements are self-enforcing, a formal safety net could undermine existing informal insurance: any scheme that changes the value of autarky relative to being in the scheme will affect the degree of risk-

[9] Since for the empirical application, we specifically use household level data, we do not consider intrahousehold issues in this model. For an analysis of these issues in the context risk using the same Ethiopian data, see Dercon and Krishnan (2000a).

sharing. The result may be less informal insurance and even result in making some households worse off (Ligon et al., 2002; Attanasio and Rios Rull, 2000). In short, introducing public safety nets might not have completely benign effects and hence taking explicit account of alternative mechanisms becomes critical.

In what follows, we focus on the impact of food aid on consumption. We do so using a specification that explicitly allows for the existence of a (perfect or imperfect) informal risk-sharing arrangement in place. The null-hypothesis is that there is perfect risk-sharing. In that case, food aid at the level of the community will have an impact, while, controlling for this community-level effect, transfers to specific households will have no impact, since they will be shared across the members of the community. If food aid at the community level has an impact after controlling for household level transfers, then this would be evidence in favour for the existence of some sharing scheme within the community. Indeed, it is difficult to see why the consumption of all households in a community would increase if positive transfers occur to some members, unless it involves some sharing of these transfers within the community.

To test this formally, consider an endowment economy consisting of a community of N households, each household j with time-separable expected utility defined over instantaneous utility $u(c^j_{ts}; z^j_{ts})$ in which c^j is a single consumption good and z^j_{ts} are taste shifters, varying across households; both c and z are defined across T periods t and S states s. Endowments in each period are assumed to be risky. There is no storage, or contracting outside the community. Let us assume that all households in this community efficiently share risk, without commitment or information constraints, so that the problem can be represented as if a social planner allocated weights θ_j to each household and maximises the weighted sum of expected utilities (ignoring time preference for simplicity), subject to the community-level resource constraint in each period t and state s. Formally, at period 0, we can write this weighted sum as:

$$\max \sum_{j=1}^{N} \theta_j \sum_{t=0}^{T} \sum_{s=1}^{S} \pi_s u(c^j_{ts}; z^j_{ts}) \tag{1}$$

in which π_s is the probability of state s occurring. Denoting e^j_{ts} as the endowment of household j in state s in time t, and using c^A_{ts} and e^A_{ts} to denote aggregate consumption and endowments in the community in each state and time period, the community resource constraint in each period and state can then be defined as:

$$c^A_{ts} \equiv \sum_{j=1}^{N} c^j_{ts} \leq e^A_{ts} \equiv \sum_{j=1}^{N} e^j_{ts} \tag{2}$$

More elaborate models including incomes, assets and production could be defined, but the key predictions from a perfect risk-sharing model would not be affected (Deaton 1992; Townsend 1994). Defining μ_{st} as the multiplier on the community resource constraint in each period and state, divided by the

probability of the state occurring (π_s), then the first order condition for optimal allocation of consumption from this problem for household j at period t can be stated as:

$$\theta_j u_c^j(c_{ts}^j; z_{ts}^j) = \mu_{st} \tag{3}$$

with $u_c^j(c_{ts}^j; z_{ts}^j)$ denoting the marginal utility of consumption of household j. Since the pareto weights are linked to a single consumption plan and since μ_{st} only depends on aggregate, not household consumption, then only considering interior solutions, this implies the standard perfect risk-sharing result: that the growth path of marginal utilities of all households ($u_{c_t}^{i'}$) is the same and that it is only influenced by changes in the aggregate resource constraint, or:

$$\frac{u_{c_{t+1}}^{i\prime}}{u_{c_t}^{i\prime}} = \frac{u_{c_{t+1}}^{j\prime}}{u_{c_t}^{j\prime}} \tag{4}$$

In other words, whatever state of the world materialised, relative marginal utilities are equal - with the lack of expectations operators the hallmark of full insurance. The other key prediction is that the relative marginal utilities of two households are a constant, the ratio of the pareto weights, irrespective of the state of nature.

This assumes that the risk-sharing arrangement is perfectly enforceable. There is a growing literature focusing on constrained efficient contracts, enforced by the threat to leave the arrangement and return to autarky (Ligon et al., 2002; Attanasio and Rios Rull, 2000). To characterise these arrangements, one could start from (1) and (2) above, but add an additional constraint for each household h, stating that in each period and state of the world, it must be in the interest of the household to stay in the arrangement rather than revert to autarky. These contracts still imply that risk-sharing will take place - so that changes in the community resource constraint will still affect the path of household marginal utilities. However, anything that increases the value of autarky relative to the value of staying in the contract will reduce the degree of risk sharing. This means that shocks to individual endowments and incomes may affect the ratio of marginal utilities across individuals, despite the presence of a risk-sharing arrangement.

These theoretical predictions have important implications about the impact of transfers from a formal safety net (see also Ligon and Schechter (2002)). Risk-sharing is not just concerned with negative shocks; the sharing predictions will hold for positive shocks as well. Transfers from outside, such as food aid delivery, are such positive shocks. Perfect risk-sharing implies that they are fully shared, based on the pareto weights; if risk-sharing is imperfect, transfers from outside will still be shared, but not necessarily fully. This has consequences for the impact of targeting the poor and needy in safety nets. Table 9 summarises these effects. Suppose schemes target in order to reach the poor. Evaluating targeting may be done by considering whether or not the poor are reached - if some of the poor are not reached, we will call this a problem of exclusion. An alternative way

Table 9: Targeting and informal risk-sharing).

	perfect targeting	imperfect targeting	no targeting
full risk-sharing	no exclusion full excess coverage	no exclusion full excess coverage	no exclusion full excess coverage
imperfect risk-sharing	possible exclusion possible excess coverage	possible exclusion possible excess coverage	possible exclusion excess coverage
no risk-sharing	no exclusion no excess coverage	some exclusion some excess coverage	no exclusion full excess coverage

Note: exclusion refers to poor households who did not benefit from the transfer scheme (in terms of higher living standards); excess coverage refers to non-poor households who benefited from the transfer scheme.

of evaluating targeting may be by considering the extent to which the non-poor are covered by the scheme - a problem of excess coverage. The table gives the matrix of possibilities: perfect targeting of the needy, imperfect targeting and no targeting, when support is simply equally divided among households. We consider the impact when perfect, imperfect and no risk-sharing takes place, assuming that imperfect risk-sharing is linked to enforcement problems.

If there is full risk-sharing, targeting becomes irrelevant: any transfer towards a household in the group, whether needy or not, is shared according to the sharing rule underlying the informal arrangement. If there is no risk-sharing, the 'standard' results related to targeting are obtained: perfect targeting results in full inclusion of all the needy in the scheme, and there are no non-poor included. If all households receive a transfer, so that there is no targeting, then there is no exclusion and full excess coverage. Under imperfect risk-sharing, the results are less clear-cut: if targeting is perfect, the risk-sharing arrangement may imply that some of transfer will need to be shared, although sharing will not be perfect. Under no targeting, partial risk-sharing may also imply that some of the transfer will be shared. In the end, who is covered or excluded is not clear a priori, especially if targeting is imperfect. However, there is a further issue: the incentives to leave the arrangement may result in exclusion still to take place even if there is full coverage of the poor as in both the perfect targeting or no targeting case, if some of the poor must use the positive transfer to pay some of the non-poor to remain in the arrangement. The conclusion is that the importance and consequences of targeting depend on the nature of the informal risk-sharing arrangements. For example, even if targeting is relatively poor, as appears to be the case in our data, and many of the needy are excluded, the impact of the transfer program may be more positive due to the existence of the informal sharing mechanisms.

5 Empirical model

Next, we derive an empirical model to investigate this further. Testable formulations of the perfect risk sharing model can be obtained by assuming specific utility functions. Using a standard CRRA formulation, $u^j(c_t^j; z_t^j) = z_t^j \frac{(c_t^j)^{1+\gamma}-1}{1+\gamma}$(in which subscript s is dropped so that conventional notation is used), using logarithms and allowing for measurement error ϵ_t^j in the logarithm of consumption, we can write (3) as:

$$\ln c_t^j = \frac{1}{\gamma} \ln \mu_t - \frac{1}{\gamma} \ln z_t^j - \frac{1}{\gamma} \ln \theta_j + \epsilon_t^j \tag{5}$$

Equation (5) can be estimated using within (fixed effects) estimators, or first differences, so that the unobservable fixed pareto weights do not affect estimation of the parameters of interest Equation (5) gives a useful basis for a standard test of perfect risk-sharing. Suppose one can identify a variable X_t^j that affects the income or endowment of household j, then provided X_t^j is cross-sectionally independent of z_t^j, θ_j or ϵ_t^j, then under the null of perfect risk-sharing, $\ln c_t^j$. should be cross-sectionally independent of X_t^j.

Idiosyncratic income shocks are thus useful candidates for testing risk-sharing, provided that they are independent of current consumption levels.[10] Most negative shocks typically used in the literature such as illness, job loss and agricultural shocks would arguably satisfy this condition (Udry (1995), Dercon and Krishnan (2000)). In this chapter, we use positive shocks, in the form of food aid given to individuals in the village, as one of the idiosyncratic income shocks[11]. Under perfect risk-sharing, positive shocks should also be shared and not affect household consumption directly, but only do so through aggregate village resources. However, food aid is typically not randomly distributed so the assumption of cross-sectional independence of aid A_t^j with z_t^j and particularly, θ_j is untenable. This is the standard program-placement problem of evaluating public programs. If aid is targeted to specific types of households - e.g. those in poor areas or those headed by females, then without further controls for program-placement, the impact of aid on $\ln c_t^j$ would be inconsistently estimated in (5). However, if placement is determined by characteristics that do not change over time, then estimating (5) by fixed effects removes the source of inconsistency.

We begin with an estimation of (6), and regress the logarithm of consumption on a set of time-varying community dummies D_t and a set of time-varying taste shifters Z_t^j (which will be defined below).ϑ^j is assumed to contain all time-invariant taste shifters, the fixed part of aggregate resources, fixed placement

[10] The advantage of using shocks to income, rather than just income is that in many alternative models, predictable changes would have been taken into account in the consumption path, and would therefore contain less information to reject perfect risk-sharing.

[11] In the evaluation of the impact of food aid, we do not distinguish between free food aid and food-for-work. While there may be different rules determining access to these different types of food aid, their impact on consumption is likely to be similar.

effects and the pareto weights.

$$\ln c_t^j = \alpha D_t + \beta Z_t^j + \delta Y_t^j + \lambda A_t^j + \vartheta^j + \epsilon_t^j \tag{6}$$

This regression is used to test perfect risk-sharing, using a set of variables measuring idiosyncratic events affecting income, such as illness, crop pests and livestock disease Y_t^j, as well as aid A_t^j. The coefficient, λ, should be zero under perfect risk sharing, as should be δ, the coefficient on idiosyncratic income. If the hypothesis that $\lambda = 0$ (or $\delta = 0$), is rejected, then perfect risk sharing is ruled out. Does this then mean that no risk-sharing arrangement exists? Not necessarily, but (6) cannot clarify this point. To test this we can ask whether aid given to other people in the village affects a household's consumption. If so, this would provide strong evidence of some sharing arrangement, whether perfect or imperfect. In particular, it is unlikely that this effect is caused by correlated effects - all households responding to higher food aid to some households by independently increasing consumption, for example by depleting assets. Actual transfers between households appears to be the most likely explanation in that case. Finally, a prediction of constrained efficient risk-sharing models, that a change in the value of autarky affects the degree of risk-sharing, can be tested by investigating whether the impact of a reduction in idiosyncratic income is higher in communities with substantial food aid compared to villages where there is little or no aid.

6 Results

Tables 10 and 11 summarises the econometric tests (discussed further in Dercon and Krishnan (2003)). We report the fixed effects estimates, with robust standard errors. Idiosyncratic income determinants included are whether aid was received by the household as well as other indices of shocks, including the self-reported measure of shocks to crops, livestock disease and illness. These alternative sources of shocks are introduced as control variables, to isolate the impact of aid. Household composition and the sex of the household head (with changes mainly due to seasonal migration or death) are used as taste shifters. We begin with a test of the perfect risk-sharing model, with all aggregate resources summarised as time-varying village level dummies. We measure the impact of aid in two different ways: as a dummy for whether the household received any aid, as well as the logarithm of the level of aid received. Table 10, columns 1 and 2, suggest that the perfect risk sharing model is rejected in either case, since controlling for time-varying community fixed effects, aid as well as other shocks affect consumption levels[12]. Column 2 suggests that a ten percent increase in aid increases consumption by 0.8 percent.

Next we test whether there is actually *any* risk-sharing taking place. To do this, we replace the community level variables by time-varying variables

[12] Note that the positive sign on crop shocks implies that worse crop conditions reduced consumption.

Table 10: Testing risk-sharing. Left hand side variable is log real consumption per adult. Fixed effects estimator with robust standard errors

	1		2		3		4	
	coeff	p-value	coeff	p-value	coeff	p-value	coeff	p-value
crop shocks (%, best=1)	0.086	0.000	0.076	0.001	0.025	0.293	0.021	0.379
livestock shocks (%, best=1)	0.005	0.381	0.004	0.406	0.005	0.391	0.005	0.336
illness days per adult	0.061	0.116	0.062	0.108	0.002	0.963	0.032	0.337
rainfall index (normal=1)					0.465	0.000	0.433	0.000
rainfall index if bad (if <1)					0.316	0.000	0.240	0.000
aid dummy	0.130	0.000			0.090	0.001		
ln aid per adult			0.082	0.000			0.051	0.009
village aid dummy					0.090	0.000		
ln village aid per adult							0.124	0.001
ln relative price index							-0.590	0.000
sex head (1=male)	0.131	0.318	0.162	0.206	0.194	0.146	0.223	0.102
household size (no.)	-0.119	0.000	-0.110	0.000	-0.131	0.000	-0.119	0.000
crop shocks*village aid dum								
livestock*village aid dummy								
time-varying village dummies	yes		yes					
R-squared	0.080		0.085		0.048		0.059	
observations	3987		3985		3987		3987	

Table 11: Testing risk-sharing and crowding out. Left hand side variable is log real consumption per adult. Fixed effects estimator with robust standard errors

	1		2	
	coeff	p-value	coeff	p-value
crop shocks (%, best=1)	0.034	0.328	-0.029	0.426
livestock shocks (%, best=1)	0.005	0.379	0.005	0.295
illness days per adult	0.066	0.123	0.052	0.251
rainfall index (normal=1)			0.425	0.000
rainfall index if bad (if <1)			0.324	0.000
aid dummy	0.111	0.000	0.053	0.054
ln aid per adult				
village aid dummy			0.075	0.196
ln village aid per adult				
ln relative price index			-0.596	0.000
sex head (1=male)	0.133	0.310	0.240	0.135
household size (no)	-0.120	0.000	-0.119	0.000
crop shocks*village aid dum	0.084	0.068	0.107	0.025
livestock*village aid dummy	-0.020	0.558	-0.069	0.243
time-varying village dummies	yes			
R-squared	0.080		0.061	
observations	3987		3985	

proxying changes in common resources. Deviations from normal rainfall levels were included, expressed as actual levels divided by long-term mean levels minus one. We allow for different effects on resources from 'better than normal' rainfall compared to 'worse than normal' levels. For example, if savings are possible, but credit markets suffer from imperfections, then it is easier to smooth in good years than in bad years (Deaton, 1991). Therefore the regression includes rainfall in general, as well as a separate measure of rainfall interacted with a dummy variable that takes the value of one when the rainfall index is below 1, or below normal levels. Hence, in bad years, the effect of rainfall on consumption is the sum of the coefficient on rainfall and the coefficient on rainfall interacted with this dummy; both coefficients are expected to be positive. All the regression results confirm this: below normal rainfall has a significantly larger impact than above normal rainfall. However, both effects are substantial, consistent with large weather-induced fluctuations in consumption.

A further community characteristic included is whether more than five per cent of the households from a particular village in the sample received food aid. Testing its effect gave a strongly significant, positive effect. However, it could be argued that in areas with poorly functioning food markets, where arbitrage happens slowly or not at all, the addition of substantial amounts of food aid to supplies in a village may simply have relative price effects, so that the impact measured by this food aid at the village level is merely a price effect, and not evidence for risk-sharing transfers between households. To control for this possibility, we include a measure of the level of local food prices, compared to the average in the full sample: in short, as a control for local price movements beyond inflationary trends in national food prices. Column 3 shows that this has a strongly significant effect, and lowers the impact of the village aid dummy, but the latter remains strongly significant. This would suggest that some risk-sharing is taking place and evidence in favour of the use of transfers between households.

We explored this further using levels of food aid given to individuals and to the community. To construct a measure of the latter, we calculated the total volume of aid coming into the village per adult per month, using all reported levels by the households in the sample. Obviously, since we work with a sample of households (even if it typically constitutes about a quarter of the village), measurement error may bedevil this estimate. Even so, we found that both individual and community level aid have a positive impact on consumption, albeit that the community level impact is small (0.022) but it is only significant at 14 per cent. To address measurement error we can make use of an alternative measure of aid flows that might be used as an instrument to tackle the measurement issue further. Our measure of food aid coming in is based on a question asking for receipts of aid between the survey rounds - typically about 5 months. However, in the consumption questionnaire, food aid received from public sources is recorded again with a recall period of 7 days. Since this is an independent measure of the food aid variable, obviously correlated with the measure from the consumption files, we can use this measure as an identifying instrument for household and community level food aid received. The results

are reported in column 4, and as expected, both the size and significance of the coefficients increases, especially the community level effect. In short, food aid coming into the village seems to be shared to some extent.

Finally, we test whether there is any evidence of pressure on informal risk-sharing arrangements due to the presence of food aid. To investigate this, we use the same regression as in columns 1 and 3 of table 10, but this time we include an interaction between idiosyncratic shocks that are clearly observable: viz. crop shocks and livestock shocks, and interact them with a measure of whether the village receives any food aid. Table 11 gives the results. The null hypothesis of no impact on informal arrangements from food aid is that the coefficient on the interaction term of food aid with the idiosyncratic shocks, is zero, i.e. there is no additional information in this extra term to explain consumption (controlling for idiosyncratic shocks as before). Recall that in column 1 (table 10), there was evidence of crop shocks not being fully insured within the community. Column 1 (table 11) shows that the coefficient on the interaction term of crop shocks with food aid is positive and significant at 7 percent, i.e. that there is a larger effect of idiosyncratic crop shocks in these communities than in those without food aid[13]. In fact, the evidence suggests that this lack of full insurance *only* occurs in villages receiving food aid, while for villages without food aid the coefficient is not significantly different from zero, as if in localities with safety nets some idiosyncratic shocks are not insured anymore. This result is confirmed (and significant at 3 percent) in column 2, where rainfall information is used as a direct measure of time varying village level variables. This supports the proposition that food aid crowds out local arrangements for insuring idiosyncratic risk.

7 Conclusions

The focus here has been the impact of food aid on household consumption in Ethiopia. We begin with whether there is any evidence of targeting of the poor or of those suffering bad shocks. Futhermore, we attempt to assess the impact of food aid on consumption outcomes. Our study is different in that it specifically tests the impact of food aid in a context of informal risk-sharing arrangements. In particular, if households form part of such arrangement, the issue of targeting of food aid is either irrelevant (if the arrangement is perfect) or its consequences complex (if risk-sharing is incomplete). More specifically, with imperfect risk-sharing, some of the aid coming into a community is likely to be shared in the community, but at the same time, it is possible that the food aid itself may contribute to the breakdown of these arrangements, since it changes the autarky situation, i.e. the outcomes of households if they were to be outside the informal arrangement. Furthermore, the fact that we have access to panel data with time-varying food aid distribution allows us to deal with

[13] Given the definition of the interaction term, in villages with food aid, the total effect of crop shocks on consumption is the sum of the coefficient on idiosyncratic shocks *and* the coefficient on the interaction term,

the econometric problems related to the non-random placement of programmes across households and communities.

The evidence suggests relatively poor targeting. Our results imply that targeting of free food aid is relatively sensitive to income within villages but the evidence of targeting of food-for-work is much weaker within villages. Across villages, there is some evidence of income sensitivity of targeting of food-for-work, but not of free food aid. Note that the nature of our sample means that our results across villages are less reliable than our within-village results.

Next, when looking at the impact of food aid, we find that there is some within village-sharing of this food aid. As a consequence, this implies that the relatively poor targeting is less a problem then standard analysis would have implied. We find that controlling for household level food aid, the fact that there is food aid to some in the community has an additional effect on consumption and the most likely interpretation of this effect that transfers indeed take place. Informal risk-sharing seems to result in better outcomes of the food aid distribution scheme, compensating for some of the poor targeting involved. Furthermore, we also evidence of some crowding out. There is evidence that villages with food aid seem to protect each other less for idiosyncratic risk, compared to communities without food aid schemes. In other words, this evidence is consistent with weakening informal arrangements because of the presence of a formal system.

This has some important implications for policy. In line with Jayne et al. (2002), it would be helpful to improve targeting. Our evidence suggests that the better targeting of communities will have to take precedence. Even if the actual local level distribution does not appear to be based on self-targeting, the presence of informal arrangements seems to contribute to some broader sharing of the benefits of food aid across a community. Note that this evidence of some shared benefits does not necessarily mean that the poor will in the end get most of the aid; rather that it will be spread over a relatively larger number of people than to whom it was initially given. Still, in our sample, we should remain aware that the problem of across-village targeting is also one of timing and of the volume of aid: the variation over time of food aid across communities (which community gets aid when and how much) suggests that there is much scope for improvement.

The issue of possible crowding out of informal insurance arrangements by formal support programs is complicated, not least to resolve via appropriate policy design. It is possible to design safety nets that would not induce welfare losses or to minimise crowding out of idiosyncratic insurance. We can think of at least two examples. Both involve finding ways to reduce the incentives of participants to deviate from the arrangement due to the increase in autarky values. The solution would be to make the safety net conditional on participation in the mutual mechanism, so that the safety net would be lost if the household were to leave the arrangement. The simplest form would be to give the aggregate protection to the mutual insurance group. However, this would require the safety net operator to be able to identify the group and monitor its continued existence as a group. If the group cannot be observed or monitored,

an optimal mechanism may be designed to induce the group to stay together by allowing other group members to punish deviators by denying them aggregate insurance. Attanasio and Rios-Rull (2000) discuss some mechanisms which could deliver this outcome. In any case, current practices are far removed far from these optimal mechanisms.

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