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Risk Sharing and Public Transfers

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We use public transfers in the form of food aid to test for the presence of risk sharing arrangements at the village level in rural Ethiopia. We reject perfect risk-sharing, but find evidence of partial risk-sharing via transfers. There is also evidence consistent with crowding out of informal insurance linked to food aid programmes.

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Risk Sharing and Public Transfers

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Abstract

We use public transfers in the form of food aid to test for the presence of risk sharing arrangements at the village level in rural Ethiopia. We reject perfect risk- sharing, but find evidence of partial risk-sharing via transfers. There is also evidence consistent with crowding out of informal insurance linked to food aid programmes.

In the wake of Townsend's (1994) seminal paper, much empirical work has focused on testing perfect risk-sharing. Most tests involve investigating whether idiosyncratic shocks contain any information to explain consumption growth. 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).

But the impact of such interventions cannot be assessed independently of existing informal mechanisms. Several standard transfer models 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). When informal risk-sharing arrangements are present, public transfers to specific households might be treated like positive idiosyncratic shocks and hence, shared across households. Alternatively, a formal safety net could undermine existing informal insurance: if informal arrangements must be self-enforcing, any scheme that changes the value of autarky relative to being in the scheme will affect the degree of risk-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 this paper, we investigate public transfers in the form of food aid for farm households in Ethiopia to test for the presence of community risk-sharing arrangements. We first test the predictions of the perfect risk sharing model: controlling for community resources, food aid should have no impact on household consumption. Under autarky, with no transfers between households, food aid to some households should not affect consumption of other households. If autarky is rejected then this test provides evidence that transfers take place between households. We find that this is indeed the case: both perfect risk sharing and autarky are rejected, suggesting partial risk-sharing using informal transfers. We also test a prediction of the constrained efficient (partial) risk-sharing model with enforceability constraints: that in areas with public safety nets, idiosyncratic shocks may be less insured. We present evidence that this is indeed the case.

1 Theoretical Framework

Consider an endowment economy consisting of a community of N households, each household j with time-separable expected utility defined over instantaneous utility $u(c_{ts}^j; z_{ts}^j)$ in which c^j is a single consumption good and z_{ts}^j 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. 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:

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

in which π_s is the probability of state s occurring. Denoting e_{ts}^j as the endowment of household j in state s in time t, and using c_{ts}^A and e_{ts}^A 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:

$$c_{ts}^A \equiv \sum_{j=1}^{N} c_{ts}^j \leq e_{ts}^A \equiv \sum_{j=1}^{N} e_{ts}^j \tag{2}$$

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 (Mace 1991; Cochrane 1991; 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, this implies the standard perfect risk-sharing result: that the growth path of marginal utilities of all households is the same and that it is only influenced by changes in the aggregate resource constraint. We will use this framework to discuss the impact of food aid when risk-sharing is present.

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.

2 Econometric specification

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:

$$\ln c_t^j = \frac{1}{\gamma}\ln\mu_t - \frac{1}{\gamma}\ln z_t^j - \frac{1}{\gamma}\ln\theta_j + \epsilon_t^j \tag{4}$$

Equation (4) 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 (4) 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.[1] Most neg-

[1]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.

ative shocks typically used in the literature such as illness, job loss and agricultural shocks arguably would satisfy this condition. In this paper, we use positive shocks, in the form of food aid given to individuals in the village, as one of the idiosyncratic income shocks. 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 (4). However, if placement is determined by characteristics that do not change over time, then estimating (4) by fixed effects removes the source of inconsistency.

We begin with an estimation of (5), 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 effects and the pareto weights.

$$\ln c_t^j = \alpha D_t + \beta Z_t^j + \delta Y_t^j + \lambda A_t^j + \vartheta^j + \epsilon_t^j \qquad (5)$$

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 (5) cannot clarify this point. To test this we can ask whether aid given to other people in the village affects a households' consumption. If so, this would provide strong evidence of some sharing arrangement. Furthermore, 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.

3 Data

The data come from three rounds of the Ethiopian Rural Household Survey (ERHS), a panel data survey consisting of three rounds collected at intervals of about 5 months between 1994 and 1995. It covers 15 villages, representative for different areas across the country, and a total of 1450 households were interviewed, across villages. The attrition rate in this panel is very low at about 3 percent per year. The survey has detailed information on households, including information on consumption, assets and income, as well as on shocks due to drought, pests and illnessd. Furthermore, it contains information on participation in food aid and food-for-work programs.

Consumption per adult equivalent[2] (in 1994 prices) is low: about 80 birr on average per month or $0.35 a day. The survey period was a good crop year on average, but a quarter of the villages were affected by drought, while diseases afflicted crops and livestock elsewhere. An average household loses several person days a month due to illness. Ability to cope with shocks is generally known to be quite limited, with life histories suggesting serious hardship linked to shocks due drought, illness, policy changes and other factors.

Ethiopia is one of the highest recipients of food aid in the world. The annual volume of cereal food aid has typically represented about 5 to 15 percent of production (Jayne et al., 2002). Much of this is distributed throughout the country directly to households: more than half via food-for-work programmes and the rest via direct distribution. The stated aim is to target the poor and vulnerable. Self-targeting of food-for-work is rarely used, and wages are usually higher than opportunity costs of time, resulting in more people applying to work than can be accommodated. In effect, this means that the distinction between food aid and food-for-work may not be useful for our purposes, and is not addressed in this paper. Previous studies suggest that there is some persistence in the placement of food aid but also of relatively poor targeting.[3]

Households were asked to list all assistance received since the previous survey round, about 5 months earlier. On average 27 percent of households received aid in each round. There is some persistence in the food aid allocation: the correlation between receiving aid today and in the preceding period is statistically significant but not high at 0.15. Two villages received no aid at all while in five of the villages, aid was only distributed once At the household level, about 50 % of households received aid at least once, 29% received it just once but only 5 % received it thrice. Finally, conditional on a village receiving aid, the percentage of households receiving aid is typically high - about 50 %. Overall, this suggests substantial variation over space and time, allowing us to perform the tests described earlier.

Table 1 displays the descriptive statistics, organised by whether households receive aid or not. Data definitions are provided below the table (details in Dercon and Krishnan, 2000b). It is striking that there is little difference between the characteristics of those who obtained aid and those who did not. Recipients have somewhat smaller household size, and suffered more illness and livestock disease, but experienced better rainfall and fewer crop shocks. Aid receipts are equivalent to about 13 percent of total consumption for those receiving it. Mean consumption levels are similar for both groups, with high standard deviations.

[2]The consumption data are based on summing all sources of food and non-food consumption, deflated by 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.

[3]Sharp (1997) finds little evidence of area targeting. Participants are selected at the community level but aid is spread over a large number of beneficiaries. This is confirmed by Jayne et al. (2002) who also find that the most important factor determining access to food aid was simply whether a programme existed in the area before.

Table 1: Descriptive Statistics: mean and standard deviation (in brackets) per household by food aid status

	Without food aid		With food aid		Full sample	
consumption per adult	89.08	(94.47)	89.80	(83.71)	89.27	(91.72)
household size (no.)	5.24	(2.18)	4.80	(2.14)	5.12	(2.18)
male headed? (%)	0.80	(0.40)	0.76	(0.43)	0.79	(0.41)
illness days per adult	0.52	(1.77)	0.77	(2.76)	0.59	(2.08)
rainfall index (normal=0)	0.09	(0.23)	0.16	(0.32)	0.11	(0.26)
% suffering below normal rain	0.32	(0.47)	0.31	(0.46)	0.32	(0.47)
crop shocks (%, best=1)	0.44	(0.39)	0.59	(0.43)	0.48	(0.41)
livestock disease (%, best=1)	0.85	(0.28)	0.79	(0.29)	0.83	(0.28)
aid per adult	0	(0.00)	11.95	(38.63)	4.20	(20.59)
village level consumption	89.89	(33.74)	93.09	(35.46)	89.27	(34.28)

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.

(Note that this consumption level is measured after receipt of aid.) By and large, food aid recipients live in villages with higher mean consumption.

4 Results

Tables 2 and 3 summarises the econometric tests. 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 1, 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[4]. 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 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 0, 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

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

Table 2: Regression results. Left hand side variable is log real consumption per adult. Fixed effects estimator with robust standard errors

	1		2		3	
	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
livestock shocks (%, best=1)	0.005	0.381	0.004	0.406	0.005	0.391
illness days per adult	0.061	0.116	0.062	0.108	0.002	0.963
rainfall deviation (normal=0)					0.465	0.000
rainfall deviation if bad (if <0)					0.316	0.000
aid dummy	0.130	0.000			0.090	0.001
ln aid per adult			0.082	0.000		
village aid dummy					0.090	0.000
ln village aid per adult						
ln relative price index						
sex head (1=male)	0.131	0.318	0.162	0.206	0.194	0.146
household size (no.)	-0.119	0.000	-0.110	0.000	-0.131	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	
observations	3987		3985		3987	

Table 3: Further regression results. 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)	0.036	0.130	0.021	0.379	0.034	0.328	-0.029	0.426
livestock shocks (%, best=1)	0.005	0.355	0.005	0.336	0.005	0.379	0.005	0.295
illness days per adult	0.046	0.208	0.032	0.337	0.066	0.123	0.052	0.251
rainfall deviation (normal=0)	0.377	0.000	0.433	0.000			0.425	0.000
rainfall deviation if bad (if <0)	0.293	0.000	0.240	0.000			0.324	0.000
aid dummy					0.111	0.000	0.053	0.054
ln aid per adult	0.040	0.005	0.051	0.009				
village aid dummy							0.075	0.196
ln village aid per adult	0.022	0.140	0.124	0.001				
ln relative price index	-0.641	0.000	-0.590	0.000			-0.596	0.000
sex head (1=male)	0.025	0.129	0.223	0.102	0.133	0.310	0.240	0.135
household size (no)	-0.118	0.000	-0.119	0.000	-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	0.059		0.059		0.080		0.061	
observations	3985		3987		3987		3985	

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, table 3, column 1 demonstrates that both individual and community level aid have a positive impact on consumption, but the latter is only significant at 14 per cent. However, we have 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 2, 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 2, 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. 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 2), there was evidence of crop shocks not being fully insured within the community. Column 3 (table 3) 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[5]. In fact, the evidence suggests that this lack of full insurance *only* occurs in villages receiving

[5]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,

food aid, while for villages without food aid the coeffiicient 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 4, 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.

5 Conclusions

We use public transfers in the form of food aid to test for the presence of risk sharing arrangements at the village level in rural Ethiopia. We reject perfect risk- sharing, but find evidence of partial risk-sharing via transfers. There is also evidence consistent with crowding out of informal insurance linked to food aid programmes.

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