Does food aid *really* have disincentive effects?
New evidence from sub-Saharan Africa*

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Abstract: Limited empirical evidence exists either to refute or confirm the pervasive belief that food aid creates significant disincentive effects on recipient food production. Using household level data from rural Ethiopia, we demonstrate that while simple descriptive statistics appear consistent with the disincentive effects hypothesis, once one controls properly for the endogeneity of food aid receipt, no empirical support remains for the hypothesis that food aid creates disincentive effects among recipient households. The macroeconomic evidence yields similar findings. Applying vector autoregression methods to national-level data, controlling for possible confounding variables, we find that food aid has no disincentive effect on food production.

Key words: Targeting, agricultural productivity, placement effects, emergency assistance, Ethiopia.

1. INTRODUCTION

Per capita food production in sub-Saharan Africa declined precipitously between the early 1970s and the mid-1980s. While there has been modest recovery over the past fifteen or so years, per capita food production in sub-Saharan Africa remains almost 20 percent below the levels observed thirty years ago. Over the same period that food production per capita declined, food aid flows into sub-Saharan Africa increased nearly fivefold. Food aid flows then became extremely volatile, but remaining in the 2.0-4.0 million metric tons per year range for the past decade (Figure 1). This juxtaposition of falling per capita food production and rising food aid receipts raises widespread concern among donors, government policymakers and development practitioners in the NGO community. Some observers interpret these data as a sign that food aid flows have caused a decline in African agriculture.

[FIGURE 1 HERE]

The logic behind this assertion is that food aid deliveries increase supply faster than they stimulate demand, depressing the food prices received by recipient country producers and traders, and thereby creating disincentives for producers to invest in improved technologies or for marketing intermediaries to invest in storage and transport capacity. Longstanding concerns about the food price effects of food aid deliveries – which date at least from Schultz (1960) – have been corroborated empirically in several cases in sub-Saharan Africa in recent years (Donovan et al. 1999, Barrett and Maxwell 2005). These concerns are echoed at the household level where concern is raised that the receipt of food aid causes households to reduce their labor supply, discourages household investment in agricultural production and crowds out private transfers and other means of

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informal responses to shocks. Special concerns have been voiced about the labor market effects of ill-conceived food-for-work (FFW) projects, which may distort local labor markets by attracting workers away from vital activities during the agricultural year, especially if the wages offered under FFW are at or above prevailing market wage rates (Maxwell and Singer, 1979). Further, critics assert, food aid permits recipient country governments to continue to neglect the needs of their agricultural sectors for adequate rural infrastructure, agricultural research and extension, as well as price and trade policies that are not biased against agriculture.

Given the importance of reversing productivity declines in African agriculture and the contested assessments of the role food aid plays in agricultural development, one would expect to find a considerable body of carefully analyzed empirical evidence on the dependency and disincentive effects of food aid on farm households and the agricultural sectors of recipient economies. This is not the case. Lentz (2003) provides a careful annotated bibliography covering papers on the dependency and disincentive of food aid published in the last 25 years. Her review shows that, strikingly, while a number of authors claim that such disincentive effects exist, these claims are based only on case studies and anecdotes.[1] While case studies can be highly informative, one would like some quantitative corroboration of such claims. No such evidence exists to date.

In this paper, we examine the impact of food aid at both the micro-level, using household survey data from Ethiopia, the largest recipient in Africa, and the macro-level, using national production data from across the continent. At the household level, when we examine descriptive statistics such as unconditional means, we find evidence that food aid has marked disincentive effects on a range of behaviors. These effects are large

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in magnitude and statistically significant. However, these comparisons do not control for other factors. To take a simple example, free food allocations are sometimes targeted towards individuals who are unable to work because they are infirm. A comparison of the unconditional means of labor supply between two groups, disabled individuals receiving free food and able-bodied individuals not receiving free food, would likely find labor supply lower amongst the former but arguably this is a consequence of physical disability that induces food aid receipt and is not a consequence of food aid receipt itself. The ever-important distinction between causality and correlation is critically important.

In our household level analysis, we find that when we control for characteristics such as age, sex and education of head, land holdings, size and location – attributes on which food aid distributions are sometimes targeted – food aid's apparent disincentive effects vanish. Our findings are even more marked when we examine the impact of food aid flows on per capita agricultural production across Africa. Once we introduce appropriate econometric controls, we find that contrary to received wisdom, food aid flows may even have stimulated (rather than depressed) per capita food production in sub-Saharan Africa in the short-term following receipt of food aid shipments.

As with any empirical study, care should be taken not to over-interpret these results. There may be other mechanisms by which food aid generates disincentive effects that we do not consider here. For example, it is possible that food-for-work projects crowd out participation in community efforts to manage common property resources. If poorly targeted food aid depresses local market prices, it may create disincentives for farmers who do not receive food aid, although our macro-level evidence suggests this effect is not large as a general phenomenon. At the national level, the impact of food aid

on recipient government effort to promote agricultural production remains an open research question. Given these and other lacuna, we stress that we see the paper's principal contribution as methodological, namely the importance of controlling for potentially confounding effects when assessing the impact of food aid.

Section 2 lays out the conceptual issues associated with evaluating the impact of food aid. Section 3 uses household-level data from Ethiopia to examine the impact of food aid on labor supply, on-farm investment and mutual aid. Section 4 then explores the dynamic relationship between food production and food aid at the macro-level of countries' aggregate food production per capita. The final section summarizes our findings and concludes with policy implications.[2]

2. ISSUES IN ASSESSING THE IMPACT OF FOOD AID

Consider the following simple model. Let y_{ivt} be a household behavior of interest – say the amount of labor supplied to agriculture – of household i living in village v at time t. Let A_{ivt} equal one if this household receives food aid, zero otherwise. There may also be some random factors that cause labor supply to vary and we denote these as v_{ivt}. Accordingly, we write.

$$y_{ivt} = \beta_A A_{ivt} + v_{ivt} \tag{1}$$

The parameter β_A offers an unbiased estimate of the impact of food aid on agricultural labor supply, provided that it is uncorrelated with v_{ivt}. This would be true, for example, if food aid were randomly distributed. However, this is unlikely to be true. Suppose, for example, we observe two villages. Village A is relatively rich, has fertile soils and good, reliable rainfall. Village B is drought-prone and soils are poor. We would expect, all other

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things held constant, that household labor supply is higher in Village A – where the returns to labor are higher – than in Village B. However, Village B is more likely to receive food aid if aid is being allocated on the basis of categorical characteristics such as poverty, the incidence of drought or agro-ecological potential. In cases such as this, equation (1) will generate biased estimates of β_A because the assumption underlying the regression analysis, that $E(A_{ivt}\upsilon_{ivt}) = 0$, is violated.

In the context of evaluation work in developing countries, Rosenzweig and Wolpin (1986) make this point more generally. They argue that when planners or project staff purposively site a project based on household or locality level characteristics, failing to take into account this endogenous program placement (as in equation (1)) will generate biased estimates of impact. A better representation of the links between current food aid and current household behaviors is therefore:[3]

$$y_{ivt} = \beta_A A_{ivt} + \beta_V V_{vt} + \beta_H X_{ivt} + v_{ivt} \tag{2}$$

Here, V_{vt} and X_{ivt} are respectively, village and household level characteristics at time t, and β_V and β_H give the impact of these characteristics on labor supply. β_A, β_V and β_H will give consistent estimates of these impacts provided that A_{ivt}, V_{vt} and X_{ivt} are uncorrelated with v_{ivt}.

3. EVIDENCE FROM ETHIOPIAN HOUSEHOLDS

Ethiopia is Africa's leading food aid recipient. As such, it is a good place to study the potential disincentive effects of food aid at the household level. We use three rounds of the Ethiopian Rural Household Survey (ERHS) collected in 1994 (twice) and 1995. The ERHS is a multi-purpose survey containing data on a wide range of assets, income,

consumption, schooling and health over time as well as the causal factors associated with these, such as changing access to public services, shocks, public and private interventions to offset such shocks, processes of agricultural intensification and diversification, entry into or exit from high value activities in agriculture and other sectors.

The sample consists of approximately 1470 households residing in 15 villages. Although these data are not nationally representative, the villages were selected so as to be representative of the principal agro-ecological zones found in rural Ethiopia. They include ox-plough agricultural systems found in the north and central highlands as well as coffee and enset (a root crop) producing households in the south. Households were randomly selected within each site and in numbers proportional to the population of the region relative to the national population. In 1994 and 1995, households were interviewed three times, with approximately five months elapsing between each round, by enumerators resident in the survey villages. Attrition over these three rounds was approximately 4 per cent and appears to have been random. Dercon and Krishnan (2000) provide further survey details and Bevan and Pankhurst (1996) provide ethnographies for each of the surveyed villages.

Access to food aid varied over time. In five villages, food aid is present in all survey rounds while several others had no or little access to food aid in this period. Another village had almost continuous access and accounts for nearly all households that received food aid in all three rounds. Just over one out of every five households received food aid prior to the first survey round, rising to 40 % prior to round two, then falling to 20 % prior to round three. Nearly half (48 %) of surveyed households did not report receiving any food aid and 28 % of households received it only once. Few households (6

%) reported receiving food aid at least once prior to all three survey rounds, a strikingly small proportion of the population if one firmly believes food aid creates dependency among recipient households. There is some targeting towards poor households but it is imperfect (Dercon and Krishnan, 2001).[4]

In this section, we explore whether three broad sets of household behaviors observed in 1995 vary by access to food aid.[5] Specifically, we consider whether past access to food aid, as measured by whether households had been recipients of food aid – either as participants in food-for-work programs or as recipients of free food transfers – in the periods prior to the two survey rounds conducted in 1994 reduced measures of labor supply, agricultural investments or mutual support in 1995. In addition, we explore whether access to food aid, observed contemporaneously with these behaviors, affects these activities. This distinction allows us to consider whether food aid received in the past (in 1994) affected subsequent activities (behaviors observed in 1995) as well as whether current (1995) receipt of food aid affected current (1995) behaviors.

The ERHS does not contain detailed data on all aspects of labor supply, so we cannot study how total labor supply behaves in response to food aid receipt. However, it does contain information on certain dimensions of labor supply. These include the number of days worked: (i) by family members on the cultivation, planting or harvesting of permanent or semi-permanent crops in the previous 30 days; (ii) for pay by adult males since the previous survey round. This includes farm work, professional or salaried work, piece or wage work undertaken by unskilled, semi-skilled or skilled laborers or work as domestic servants but not work on food-for-work or cash-for-work projects or days spent working in traditional labor sharing activities; (iii) for pay by adult females since the

previous survey round; (iv) in self-employed non-agricultural own business activities (e.g., weaving, spinning, milling, other handicrafts, trade) by adult males since the previous survey round; and (v) in self-employed non-agricultural own business activities by adult females since the previous survey round.

Table 1 considers these five different aspects of household labor supply, disaggregated by type of activity and individuals within the household undertaking these activities. Starting with the first column, Table 1 indicates that households who received food aid in early 1994, approximately one year before these activities were observed, spent considerably less time supplying labor to permanent and semi-permanent crops. Less time was also spent on non-agricultural own business activities by adult males and females. Summing across all groups within the household, on average, households not receiving food aid worked five times as many days in these activities than did households who did (9.5 versus 2.0). These statistics would seem to confirm the existence of a disincentive effect on food aid recipient labor supply.

[TABLE 1 here]

However, the disincentive effects are less clear when we compare households who received food aid in late 1994, approximately five months before these activities were observed. While less time was spent by food aid recipients on supplying labor to permanent and semi-permanent crops, there is no meaningful difference in labor supply to non-agricultural own business activities. When we consider the associations between current (1995) labor supply and current (1995) receipt of food aid, the disincentive effect on agricultural labor remain. However, while male labor supply to non-agricultural own business seems to fall with receipt of food aid, female labor time devoted to non-

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agricultural own business rises and time spent in wage work is higher for all household demographic groups amongst households receiving food aid.

Table 2 considers three different aspects of household investments in 1995: whether households purchased fertilizers or pesticides and whether they made investments in livestock, the principal savings mechanism in much of rural Ethiopia. It also considers two measures of informal, mutual support: transfers (in-cash and in-kind) received from other households since the previous survey round and the number of days the household contributes to traditional labor sharing parties called *debbo.* Households receiving food aid were less likely to report that they used fertilizers or pesticides and there is some indication that receipt of food aid reduces participation in labor sharing groups.

[Table 2 here]

We complement these descriptive statistics by estimating equation (1), the impact of food aid receipt on household behaviors with no additional controls. These are reported in Table 3. A number of the outcomes are dichotomous (0/1) variables and for these, we estimate (1) using a probit. The remainder contain a mix of observations, some of which have zero values, and some of which have positive values and for these, we estimate Tobits.[6] As coefficients reported for probits are not readily interpretable, marginal effects are also reported. Generally, the results reported here complement the descriptive materials provided in Tables 1 and 2. Access to food aid would seem to have significant disincentive effects on investments in agricultural activities and the magnitudes of some of these effects are large.[7] Receipts of food aid lower the likelihood that a household uses fertilizer by 11 to 18 percentage points and reduces family labor

supply on permanent or semi-permanent crops by about 20 to 26 days per month. Past access to food aid has mixed impacts on labor supplied to non-agricultural own business activities and contemporaneous access increases labor supplied to off-farm wage work.

[Table 3 here]

As noted in section 2, however, these results are conditional on the strong assumption that receipt of food aid and other household characteristics are uncorrelated. Table 4 reports results where household characteristics – location, age and sex of the household head, whether the head ever attended school, household size and households' holdings of arable land and oxen – are included as additional controls.[8] The results are considerably different from those reported in Table 3. All negative effects of food aid disappear with one exception. And while contemporaneous access to food aid reduces time spent on permanent and non-permanent crops, the magnitude of these effects is about 1/3 of that reported in Table 3.[9]

[Table 4 here]

There are, however, two limitations with the results reported in Table 4. First, we might wonder whether our results are biased by our choice of estimator for variables that have a mix of zero and positive values. Second, suppose receipt of food aid was correlated with a household characteristic – say a shock – that varied over time. If this were true, $E(A_{ivt}\upsilon_{ivt}) \neq 0$ and the parameter estimates reported in Table 4 would be inconsistent. For outcomes that are continuous rather than dichotomous, it is possible to address these concerns jointly. The dependent variables are five dimensions of labor supply (family labor supplied to permanent and semi-permanent crops; labor supply of adult males and females to off-farm wage work; and labor supply of adult males and

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females to non-agricultural own business activities) and two dimensions of mutual support (transfers received from other households and days contributed to traditional labor sharing groups). As before, we consider the impact of past access to food aid (food aid prior to 1995) as well as food aid received contemporaneously with these behaviors. Two sets of results are reported. The first column reports results based on using least squares estimation of equation (1) with the standard errors corrected for potential heteroscedasticity using the methods outlined by Huber (1967) and White (1980).[10] These results generally mimic those found in Table 4, suggesting that concern over possible bias resulting from the use of tobits in Table 4 is probably unfounded.

The second columns reported by round 2 address the concern that $E(A_{ivt} \upsilon_{ivt})$ might not equal zero even with the inclusion of locality and household characteristics as controls. They report the results of estimating Maddala's (1983) "treatments" regression model. Using maximum likelihood, we estimate the following model:

$$y_{ivt} = \beta_A A_{ivt} + \beta_V V_{vt} + \beta_H X_{ivt} + v_{ivt} \tag{2'}$$

$$A_{ivt} = \beta_{AV} V_{Vt} + \beta_Z Z_{iVt} + \varepsilon_{iVt} \tag{3}$$

Equation (2') is identical to (2) except that A_{ivt}, considered endogenous, is now determined by equation (3). Again a number of household characteristics are included as control variables or components of V_{ivt} (household location) and X_{ivt}: age and sex of the household head, whether the head ever attended school, household size, households' holdings of arable land and oxen, whether the household had experienced a shock in the last five years and whether it had recovered from that shock. Z_{ivt} contains five dummy variables that serve as instruments, past access to food aid – in the three years preceding the survey - as well as food aid received in the aftermath of the 1983/84 drought. These

instruments reflect the "inertia effect" of food aid targeting described by Jayne *et al*.

(2002). These variables are correlated with current access and do not appear to directly

affect the outcomes being examined.[11] The attraction of Maddala's estimator is that it

ensures that $E(A_{ivt}\upsilon_{ivt}) = 0$ so that our estimates of β_A – that is, our estimates of possible

disincentive effects – are consistent; and it provides a chi-squared test of whether the

error terms in (2') and (3) are correlated.[12]

      We begin by comparing OLS results ("food aid exogenous, no controls") to the

results from the treatment regressions ("food aid endogenous, controls incl.") of the

impact of past (1994) receipt of food aid on labor supply and mutual support observed in

1995. The results are somewhat varied. When we consider the impact of food aid

received about a year ago, it again appears that disincentive effects are pervasive.

However, these vanish in the cases of labor supply to non-agricultural activities or, fall

markedly in magnitude as in the case of labor supply to permanent and semi-permanent

crops. When we consider the impact of food aid received in late 1994, there are small

disincentive effects observed in the case of labor supply to permanent and semi-

permanent crops and in days contributed to traditional labor sharing groups.

      The last columns of Table 5 examine the impact of contemporaneous receipt of

food aid and these household behaviors. Here the results are less ambiguous. Controlling

for household and locality characteristics and the potential endogeneity of food aid, there

is no evidence of disincentive effects on labor supply.[13] If anything, receipt of food aid

appears to *increase* labor supply, particularly by males. There is a disincentive effect on

days contributed to traditional labor sharing groups, but this is small in magnitude, on

average, one day out of the previous five months.

[Table 5 here]

To summarize, comparisons of unconditional means and proportions, such as those found in Tables 1, 2 and 3 suggest that food aid has disincentive effects on the behaviors considered here. However, such findings appear to be confounded by targeting-related placement effects. When these are taken into account, as in Tables 4 and 5, these disincentive effects disappear. Put differently, the apparent negative correlation between food aid and different measures of labor supply, investment and mutual support – what appear, at first glance, disincentive effects of food aid – seem to reflect the response of food aid receipt to exogenous factors that likewise affect labor supply, investment and mutual support. The unconditional correlation does not appear to result from a causal relation from food aid to these household behavioral variables.

4. THE RELATIONSHIP BETWEEN FOOD AID AND FOOD PRODUCTION

The preceding analysis examines the micro-level evidence on claims of food aid's disincentive effects in an especially important case, Ethiopia. Household-level data cannot adequately address, however, macro-level critiques that food aid depresses food prices and discourages government investment in agriculture, thereby undermining the productivity of recipient countries food production sectors. Accordingly, in this section, we explore the dynamic relationship between food production and food aid by examining the vector autoregressive (VAR) characteristics of the two variables at national level. Following previous studies, we combine time series of food production and food aid across countries in sub-Saharan Africa to obtain a panel data set that contains sufficient observations to estimate the following VAR model (Lavy, 1990; Barrett et al., 1999):[14]

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$$FP_{it} = \alpha_0 + \sum_{k=1}^{n} \alpha_k FP_{it-k} + \sum_{k=1}^{n} \beta_k FA_{it-k} + \eta_i + \varepsilon_{it} \qquad (4)$$

$$FA_{it} = \alpha'_0 + \sum_{k=1}^{m} \alpha'_k FA_{it-k} + \sum_{k=1}^{m} \beta'_k FP_{it-k} + \xi_i + \mu_{it} \qquad (5)$$

where *FP* and *FA* are domestic food production and food aid inflows, respectively, while

FP_{t-k} and FA_{t-k} represent values of the variables lagged k years; α_0, α'_0, α_k, α'_k, β_k and

β'_k are parameters to be estimated, η_i and ξ_i are country-specific effects that summarize

the influence of unobserved variables such as infrastructure, period average climate, soils,

elevation, history and culture, and which are assumed to be distributed independently

across countries, with variance σ_η^2 and σ_ξ^2, and ε and μ are error terms for each country i

and year t. Given that ordinary least squares (OLS) and generalized least squares (GLS)

will yield biased estimates in the presence of correlations between the country-specific

effects and the lagged *FA* and *FP* variables, we employ a generalized method of moments

(GMM) estimator to obtain consistent parameter estimates (Holtz-Eakin et al., 1988).

Differencing away the country-specific fixed effects and using annual rainfall, RF_{it}, and

yearly dummies for disasters, Y_{it}, as control variables, we estimated the following

equations:

$$\Delta FP_{it} = \alpha_0 + \sum_{k=1}^{m} \alpha_k \Delta FP_{it-k} + \sum_{k=1}^{m} \beta_k \Delta FA_{it-k} + \sum_{k=1}^{s} \gamma_k RF_{it-k} + \sum_{k=1}^{2} \delta_k D_{itk} + \Delta \eta_i \qquad (6)$$

$$\Delta FA_{it} = \alpha_0 + \sum_{k=1}^{m} \alpha_k \Delta FA_{it-k} + \sum_{k=1}^{m} \beta_k \Delta FP_{it-k} + \sum_{k=1}^{s} \gamma_k RF_{it-k} + \sum_{k=1}^{2} \delta_k D_{itk} + \Delta \xi_i \qquad (7)$$

where *RF* represents annual rainfall and *D* is a dummy variable for disasters, and γ and δ

are parameters to be estimated. The yearly dummies for disasters equal one in years when

a country experienced a natural or human disaster, and zero otherwise.

Annual food aid flows data were obtained from the World Food Programme and food production data from the FAO's *Production Yearbook*. The data covered the period 1970-2000. Because cereals food aid accounts for more than 90% of total food aid shipments to sub-Saharan Africa, cereals serve as a reasonable proxy for overall trends in food aid. A total of 42 sub-Saharan African countries that received food aid were included in the analysis.[15] All volume figures were converted to a per capita basis using annual population data reported in the FAO's *Production Yearbook*. Country-level annual rainfall data were obtained from the Tyndall Centre for Climate Change (Mitchell et al., 2003). Given that the impact of rainfall volume varies between different geographical areas based on agro-ecology, altitude, etc., we measured rainfall in terms of standardized deviations from country-specific means, obtained by subtracting a country's mean annual rainfall from rainfall and dividing by the country-specific standard deviation. Year-specific dummies for disasters were constructed from data obtained from the Center for Research on the Epidemiology of Disasters (CRED) in Belgium. The CRED data base contains detailed information on natural disasters and conflicts on an annual basis for all countries, so we include two distinct types of disaster indicators to reflect each of these.

The primary motivation for this estimation is to establish the dynamic effects of food aid on food production. Thus, the main question is whether food aid stimulates or retards recipient country food production. We therefore first estimate a model with food production as the dependent variable, regressed on its own lags and on lags of food aid—the latter to capture persistence in food aid disbursement patterns—controlling for rainfall and disasters in both regressions.

Our specification does not include information on factors such as soil quality or production technology that may affect food production but for which we have no data. These factors must be treated as country-specific effects. However, given that the estimation approach employed here uses differencing, the time invariant component of these effects gets eliminated and thus cross-sectional differences in soil quality, irrigation infrastructure, etc. pose no problem in our estimation. Given controls for annual variation in rainfall and disasters, it is unlikely that there remain significant unobserved covariates that are correlated with both food aid and food production.

As Holtz-Eakin et al. (1988) emphasize, two important questions in this estimation are whether the data is consistent with a lag length m and whether food aid causes food production.[16] It therefore makes sense to test for non-causality conditional upon the outcome of a test for lag length. The appropriate lag length is determined by starting with an arbitrary but long lag, then re-estimating the equation with successively shorter lags. The lag length is a constraint that can be tested using the fact that the difference in the unconstrained and constrained sum of squared residuals has a chi-squared distribution. For the estimations using the whole sample, a lag length of four ($m = 4$) proved optimal for both food production and food aid.[17] In order to obtain consistent and efficient estimates, each regression employed instruments of one period lags, thus with $m = 4$ the instrument is lagged from t-2 to t-5.

Granger causality tests were then carried out for the entire sample to establish the direction of causality between food aid and food production. The F-statistic for the null hypothesis that changes in food aid do not affect changes in food production was 6.51 with four degrees of freedom, against a critical value of 2.37 at the five percent level of

statistical significance. The *F*-statistic for the null hypothesis that changes in food production do not affect changes in food aid was 5.28 (against the same critical value). Thus we can reject both null hypotheses. There appears to be bidirectional causality between food aid and food production, which reinforces the use of the generalized method of moments approach to estimate the VAR model.

The left column of Table 6 presents the parameter estimates for the food production equation (White robust *t*-statistics appear in parentheses). The most striking finding is the predominantly positive effect food aid has on food production, although only the coefficients on one and two year lags were statistically significant at the ten and five percent levels, respectively. The magnitudes of these estimates suggest that a one kilogram per capita increase in food aid shipments yields an expected net increase of about a one-third of a kilogram per capita in the subsequent year's food production. Food aid certainly does not appear to have depressed food production. On the contrary, if anything it appears to have exerted a modest positive impact on food production in sub-Saharan Africa over the past thirty years. Any disincentive effects due to depressed product prices induced by food aid shipments must be more than offset, on average, by positive risk management and factor price effects.

[Table 6 here]

The coefficients for the rainfall variable indicate that positive deviations from country level mean rainfall result in higher contemporaneous food production, as one would expect, while negative deviations tend to lower production. Natural and manmade disasters exert a statistically significant, negative effect on food production, as one would expect. Wald test statistics examining the joint significance of the food aid, rainfall and

disaster dummies suggest tat rainfall, disasters and food aid all jointly affect per capita food production in sub-Saharan Africa.

Estimates from the VAR food aid equation appear in the right column of Table 6. The results show that past values of food output affect current levels of food aid. Specifically, the negative and statistically significant coefficients of food production indicate that increases in food production tend to reduce food aid shipments in subsequent periods, while declines in food production are accompanied by increased supplies of food aid. These effects are quite modest in magnitude, however. Neither contemporaneous nor lagged deviations in rainfall significantly influenced food aid flows, while aid flows did respond significantly and positively to disasters.[18]

[FIGURE 2 HERE]

In order to make the food aid's net dynamic effects clearer, we compute impulse response functions to depict the time path of food production responses to a one-year increase in food aid shipments of one kilogram per capita. Figure 2 depicts the impulse response functions of both food aid and food production to a food aid shock over the whole period. In contrast to comparable impulse response function estimates using a smaller and more geographically diverse set of countries and only PL 480 food aid flows from the United States (Barrett et al. 1999), food aid (the dashed line) exhibits little persistence over time. This is consistent with the observation that the overwhelming majority of food aid shipments into Africa have been emergency flows and with the claim that these flows respond, albeit with a significant lag, to local production shocks. Of more immediate interest to this analysis, the effects of food aid on per capita food production (the solid line in Figure 2) appear positive and persistent for three years, with

negative effects in the fourth year and negligible impact thereafter. It would appear that resources of any sort are sufficiently scarce and food aid has been sufficiently well targeted overall in sub-Saharan Africa over the past thirty years that it has effectively relieved factor market constraints so as to at least neutralize the product price disincentives that naturally arise from food aid shipments.

5. CONCLUSIONS

This paper enters this contested debate on the impact of food aid on markets and households. Using more advanced econometric methods than have previously been applied to the relationship between food aid and food production, in Sub-Saharan Africa or anywhere else, we find that food aid does not appear to have created disincentives to local agricultural production, labor supply, investment or mutual support.

In rural Ethiopia, simple test statistics, such as a comparison of means, or simple regressions, suggest that the disincentive effects of food aid on household behaviors are many, large in magnitude and statistically significant. *However*, when we take into account household characteristics such as age, sex and education of head, land holdings, size and location – characteristics that can affect behaviors and on which food aid is commonly targeted – many of these adverse effects vanish. In fact, there is some suggestion in these data that food aid leads to increases in labor supply to agriculture, wage work and own business activities.

Similarly, national-level data from across sub-Saharan Africa suggest that whatever negative effects food aid may exert on food producer prices and on government incentives to invest in agricultural development must be at least offset by food aid's

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favorable effects on human nutrition, farmer seasonal liquidity and food security. Once one controls for factors such as rainfall and natural and human disasters and for country-specific characteristics such as colonial ties, geography and climate, the apparent negative correlation between food aid receipt and food production evident in simple descriptive statistics, such as Figure 1, likewise disappears. If anything, food aid appears to have been mildly stimulative to, rather than a drag on, African agricultural productivity.

One possible explanation for these findings– which differs from previous results using data from the rest of the developing world (e.g., Barrett et al. 1999), which find negligible food production effects, positive or negative, from food aid – is that financial liquidity constraints so severely limit food production and distribution in much of sub-Saharan Africa that the ameliorative effect of food aid on farmers' access to inputs compensates for the downward pressure food aid exerts on producer prices. This underscores the importance of factor market failures in limiting productivity in Africa as well as the centrality of food aid management modalities – especially targeting – to making it an effective instrument.

The crucial concern in food aid management is effective targeting of food aid to those who most need it (Barrett and Maxwell 2005). Yet, effective targeting necessarily implies a negative correlation between food aid receipt and household productivity and investment, low values of which typically reflect need. One must probe beyond that facile correlation in order to establish whether food aid has any of the undesirable disincentive effects so commonly attributed to it. The parallelism between our micro-level findings from Ethiopian household data and our macro-level findings from national

data provide the strongest evidence to date that observers should be cautious about uncritical acceptance of claims that food aid has disincentive and dependency effects.

As with any empirical study, these results should be treated with care. However, the consistency of the micro- and macro-level evidence suggests that the anecdotes and case studies upon which dependency and disincentive claims are frequently built are not generalizable and should be subjected to careful scrutiny as the critical distinction between correlation and causality is easily lost in qualitative analysis. In this sense, this paper's principal contribution is methodological: we demonstrate how misleading statements regarding disincentive effects can be when confounding effects are not taken into account.

6. ENDNOTES

[1] We note too that it is possible to construct an opposing perspective on the food aid – food productivity relationship, which holds that increased food aid has been and could prospectively be beneficial to African agriculture. This line of argument emphasizes food aid's role in increasing poor households' access to and consumption of food in the face of exogenous climatic or political shocks, thereby improving human nutritional status, health, labor productivity and income earning capacity relative to what would transpire in the absence of food aid. In Ethiopia, Quisumbing (2003) and Yamano et al. (2005) find that food aid plays an important role in protecting the nutritional status of pre-school children, particularly in the aftermath of shocks. Bezuneh et al. (1988), Barrett et al. (2001) and Holden et al. (forthcoming) provide evidence that in some localities, food aid may have relieved seasonal credit constraints that limit farmers' capacity to invest in mineral fertilizer, modern seeds and other productivity-enhancing inputs and to have sparked investment in terracing and other on-farm improvements that likewise increase yields with a lag (Holden et al. forthcoming). Von Braun et al. (1999) report on the multiplier effects of a FFW-built road in the Ethiopian lowlands, where improved market access directly attributable to that road led to the establishment of water mills and fruit plantations and the revival of traditional cotton spinning and weaving in the three years after the road was built. Well-conceived and managed FFW projects that invest in necessary materials to complement labor inputs can 'crowd in' private investment, as Holden et al. (forthcoming) find in the case of private investment in soil and water conservation structures in Tigray region of Ethiopia and Bezuneh et al. (1988) find in purchased farm inputs in Baringo District, Kenya.

[2] We are aware of the important debate over whether food aid is the best resource to use in promoting agricultural market development in recipient countries, particularly in sub-Saharan Africa. In the abstract, food aid seems an undeniably inferior resource because it is doubly tied, in the following sense. Food aid is inflexible in the form of the transfer – it comes only as food, not as cash or other useful goods or services – and it is typically inflexible in sourcing that food, relying on commodities procured in and shipped directly from the donor country. However, if the volume of international transfers depends on the form in which donors provide it, particularly if donor country governments are willing to provide food aid in part because it supports their own farm and agribusiness constituencies, then food aid that is relatively inefficient per

dollar transferred may nonetheless generate greater absolute transfers than less restricted cash appropriations. It is within this context that we place our analysis: whether food aid violates a necessary (but not sufficient) condition for its use in promoting recipient household and country development.

[3] Note that when, in section 3, we consider whether past receipt of food aid affects current behaviors, we would replace A_{ivt} with A_{ivt-1}.

[4] An interesting question is the extent to which household behaviors are shaped by expectations regarding receipt of food aid. Given the extent to which food aid seemingly appears and disappears from these villages without warning, our prior is that flows of assistance are too unpredictable to affect household behavior. That said, the question is not resolved in this paper; a credible empirical approach to this question requires a much longer time horizon than considered here.

[5] We do not, however, discuss the allocation processes that underlie how food aid is distributed. This is reviewed in Sharp (1997), Clay et al. (1999), Gilligan and Hoddinott (2004) and Jayne et al. (2001, 2002).

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

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

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

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

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

[11] In addition, the X_{ivt} found in (2') also appear in (3).

[12] Rejecting the null hypothesis that the error terms are uncorrelated implies that OLS estimates of (2') will produce biased coefficients.

[13] There is no direct analogue to an over-identification test in a treatments regression. As a specification check, we re-estimated these models using instrumental variables and estimating the first stage regression as a linear probability model. This approach allows us to construct a Sargan over-identification test for our instruments. In nearly all cases, we do not reject the Sargan test at the 5 per cent level, the exceptions being woman's labor supply to own business activities and the impact of lagged aid on participation in work parties.

[14] This imposes the assumption that the underlying structural relationship between food aid and food production is the same for each country in the sample.

[15] The countries include: Angola, Burundi, Benin, Burkina Faso, Botswana, Cape Verde, Central African Republic, Chad, Cameroon, Comoros, Congo, Cote d'Ivoire, Djibouti, Ethiopia, Equatorial Guinea, Guinea-Bissau, Gambia, Ghana, Kenya, Lesotho, Liberia, Madagascar, Mauritius, Mauritania, Mali, Malawi, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Somalia, Sudan, Swaziland, Tanzania, Togo, Uganda, Zaire, Zambia, and Zimbabwe.

[16] The Im, Pesaran and Shin (1997) panel unit root test could not reject the null hypothesis of unit root in the food aid and food production variables. The two variables were therefore specified in first differences.

[17] Details on the optimal lag length tests are available from the authors by request.

[18] We re-estimated the VAR for 1970-85 and 1986-2000 sub-samples in order to ensure that temporal pooling was not distorting our findings. The results (available from the authors by request), based on an optimal lag length of m=3, are qualitatively identical to those using the full pooled sample, although the coefficient estimates relating food aid to subsequent food production are slightly higher in the second period, perhaps reflecting some improvement in food aid programming due to advances in early warning systems and targeting methods. See Barrett and Maxwell (forthcoming) for detailed explanation of changes in food aid targeting, procurement and distribution modalities since the mid-1980s.

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Table 1: Paired t-tests of access to food aid and labor supply

	Received food aid in early 1994		Received food aid in late 1994		Received food aid in same recall period as labor supply (1995)	
	Yes	No	Yes	No	Yes	No
Mean family labor supply on permanent and semi-permanent crops, 1995	1.9	9.8	4.1	10.7	3.4	9.2
t test on difference in means	7.11**		7.00**		5.04**	
Mean labor supply of adult males on off-farm wage work, 1995	7.2	7.6	8.2	7.0	14.0	5.8
t test on difference in means	-0.16		-0.64		-3.75**	
Mean labor supply of adult males to non-agricultural own business activities, 1995	1.1	5.8	4.4	5.3	5.2	3.0
t test on difference in means	4.21**		-0.93		1.85*	
Mean labor supply of adult females on off-farm wage work, 1995	1.9	1.5	1.7	1.5	4.0	1.0
t test on difference in means	-0.62		-0.56		-5.09**	
Mean labor supply of adult females to non-agricultural own business activities, 1995	0.9	3.7	2.6	3.4	6.1	2.3
t test on difference in means	2.91**		0.99		-3.79**	

Table 2: Paired t-tests of access to food aid, agricultural investments and mutual support

	Received food aid in early 1994		Received food aid in late 1994		Received food aid in same recall period as labor supply (1995)	
	Yes	No	Yes	No	Yes	No
Agricultural Investments						
Proportion of households purchasing fertilizers, 1995	1.8	19.4	5.6	22.3	6.7	17.7
t test on difference in means	8.08**		9.13**		4.82**	
Proportion of households purchasing pesticides, 1995	0.6	4.1	1.4	4.6	0.6	4.0
t test on difference in means	3.18**		3.40**		2.94**	
Proportion of households purchasing livestock, 1995	18.1	21.0	21.0	19.9	19.6	20.4
t test on difference in means	1.16		-0.54		0.30	
Mutual support						
Mean value of transfers received from other households, 1995	181.3	158.0	188.6	146.1	251.3	139.6
t test on difference in means	-0.27		-0.58		-1.25	
Mean number of days contributed to traditional labor sharing groups, 1995	0.9	2.5	1.8	2.4	2.1	2.2
t test on difference in means	3.54**		1.58		0.27	

Table 3: Estimated impact of food aid on labor supply, agricultural activities, and mutual support in 1995, with no additional controls

	Dependent variable	Received food aid in early 1994	Received food aid in late 1994	Received food aid in same recall period
(1)	Family labor supply on permanent and semi-permanent crops, 1995	-26.55 (7.03)**	-20.48 (7.35)**	-21.46 (5.86)**
(2)	Labor supply of adult males on off-farm wage work, 1995	-1.80 (0.21)	27.72 (3.99)**	73.26 (9.01)**
(3)	Labor supply of adult males to non-agricultural own business activities, 1995	-56.73 (4.82)**	13.21 (1.94)*	-21.09 (2.26)**
(4)	Labor supply of adult females on off-farm wage work, 1995	15.44 (1.60)	-0.75 (0.09)	77.19 (6.82)**
(5)	Labor supply of adult females to non-agricultural own business activities, 1995	-58.54 (2.94)**	-10.85 (0.88)	56.65 (4.16)**
(6)	Households purchasing fertilizers, 1995	-1.24 (6.74)** *-0.18*	-0.82 (8.52)** *-0.17*	-0.56 (4.65)** *-0.11*
(7)	Households purchasing pesticides, 1995	-0.71 (2.71)** *-0.03*	-0.46 (3.08)** *-0.03*	-0.71 (2.70)** *-0.03*
(8)	Households purchasing livestock, 1995	-0.13 (1.30) *-0.03*	0.02 (0.32) *0.01*	-0.11 (1.11) *-0.03*
(9)	Value of transfers received from other households, 1995	-484.92 (0.85)	456.61 (1.05)	739.31 (1.43)
(10)	Days contributed to traditional labor sharing groups, 1995	-12.60 (5.15)**	-2.14 (1.30)	-1.34 (0.65)

Notes:
Rows (1)–(5) and (9)-(10) estimated using tobits; all others estimated using probits. Parentheses in rows (1)–(5) and (9)-(10) are absolute values of t statistics; parentheses in other rows are absolute values of z statistics. **, significant at the 5% level; *, significant at the 1% level. Numbers in italics in rows (6)-(8) are marginal effects. Sample sizes are approximately 1460.

Table 4: Estimated impact of food aid on labor supply, agricultural activities, and mutual support in 1995, with locality and household controls

	Dependent variable	Received food aid in early 1994	Received food aid in late 1994	Received food aid in same recall period
(1)	Family labor supply on permanent and semi-permanent crops, 1995	-4.52 (1.00)	1.57 (0.49)	-7.53 (1.86)*
(2)	Labor supply of adult males on off-farm wage work, 1995	-0.52 (0.03)	49.15 (4.88)**	67.92 (6.44)**
(3)	Labor supply of adult males to non-agricultural own business activities, 1995	-19.97 (0.91)	10.36 (1.00)	-10.56 (0.93)
(4)	Labor supply of adult females on off-farm wage work, 1995	31.47 (1.98)**	37.64 (3.00)**	65.48 (4.63)**
(5)	Labor supply of adult females to non-agricultural own business activities, 1995	0.03 (0.00)	38.14 (2.52)**	26.27 (1.60)
(6)	Households purchasing fertilizers, 1995	-0.28 (0.94) *-0.05*	-0.05 (0.27) *-0.01*	0.12 (0.56) *0.02*
(7)	Households purchasing pesticides, 1995	-0.39 (1.22) *-0.02*	-0.13 (0.53) *-0.01*	-0.02 (0.05) *-0.00*
(8)	Households purchasing livestock, 1995	-0.16 (0.69) *-0.04*	0.20 (1.61) *0.05*	-0.19 (1.44) *-0.05*
(9)	Value of transfers received from other households, 1995	945.4 (0.93)	553.5 (0.82)	561.9 (0.83)
(10)	Days contributed to traditional labor sharing groups, 1995	3.73 (1.09)	2.39 (1.03)	2.97 (1.18)

Notes:
Controls are village level dummy variables, age and sex of household head, whether the head ever attended school, log household size, household's holdings of arable land and a dummy variable equaling one if the household owns more than two oxen. Other notes as in Table 3.

Table 5: Estimated impact of food aid on labor supply and mutual support in 1995, treating access as endogenous

Dependent variable	Received food aid in early 1994		Received food aid in late 1994		Received food aid in same recall period	
	Food aid treated as		Food aid treated as		Food aid treated as	
	Exogenous, no controls	Endogenous, w/ controls	Exogenous, no controls	Endogenous, w/ controls	Exogenous, no controls	Endogenous, w/ controls
Family labor supply on permanent and semi-permanent crops	-7.48 (10.48)**	-5.33 (3.17)**	-5.98 (6.75)**	-4.52 (3.75)**	-5.54 (6.50)**	17.37 (10.95)**
Labor supply of adult males on off-farm wage work	0.95 (0.53)	19.59 (2.49)**	2.44 (1.75)*	6.68 (1.81)*	9.01 (4.85)**	8.56 (3.37)**
Labor supply of adult males to non-agricultural own business activities	-4.57 (5.77)**	20.39 (10.92)**	1.18 (1.15)	5.00 (1.24)	-1.99 (1.77)*	21.34 (5.30)**
Labor supply of adult females on off-farm wage work	0.69 (1.18)	-0.90 (1.31)	0.35 (0.61)	-1.35 (1.45)	3.44 (5.12)**	1.77 (1.98)**
Labor supply of adult females to non-agricultural own business activities	-2.74 (3.82)**	-3.48 (1.90)*	-0.72 (0.85)	-1.21 (0.94)	4.17 (3.01)**	0.88 (0.857)
Value of transfers received from other households	35.31 (0.30)	255.88 (0.64)	44.16 (0.57)	1149.48 (1.10)	110.37 (0.75)	87.15 (0.32)
Days contributed to traditional labor sharing groups	-1.73 (5.48)**	-1.93 (4.94)**	-0.56 (1.41)	-2.67 (2.75)**	0.02 (0.04)	-1.39 (2.24)**

Notes: See Table 4.

**Table 6: Vector Autoregressive Regression Parameter Estimates**

Regressors	Food Production	Food Aid
Change in food production lagged one year	-0.2287** (5.846)	-0.0041 (1.183)
Change in food production lagged two years	-0.1431** (4.649)	-0.0057** (2.083)
Change in food production lagged three years	0.0485* (1.663)	-0.0073** (2.591)
Change in food production lagged four years	0.0674** (2.271)	-0.0052 (1.327)
Change in food aid lagged one year	0.3128* (1.973)	-0.2705** (6.817)
Change in food aid lagged two years	0.3706** (2.533)	-0.1864** (4.852)
Change in food aid lagged three years	0.1698 (1.426)	-0.0537* (1.712)
Change in food aid lagged four years	-0.2359 (0.786)	0.0198 (0.941)
Rainfall	0.0351** (2.347)	-0.0204 (1.527)
Rainfall lagged one year	0.0183 (0.901)	-0.0130 (0.891)
Natural Disasters	-0.0643** (2.859)	0.0257** (3.102)
Conflict Disasters	-0.0381** (2.526)	0.0429** (2.733)
Wald tests for joint significance		
Lagged Food Production		14.260**
Lagged Food Aid	13.148**	
Rainfall	7.543**	2.143
Disasters	18.387**	13.952**

Notes:

The numbers in parentheses are absolute *t*-values. * (**) indicates statistical significance at the 10% (5%) level.

The Wald statistic tests the joint significance of the independent variables asymptotically distributed as χ_k^2 under the null of no relationship, where *k* is the number of coefficients estimated.

Figure 1: Sub-Saharan African Food Production vs. Food Aid Flows
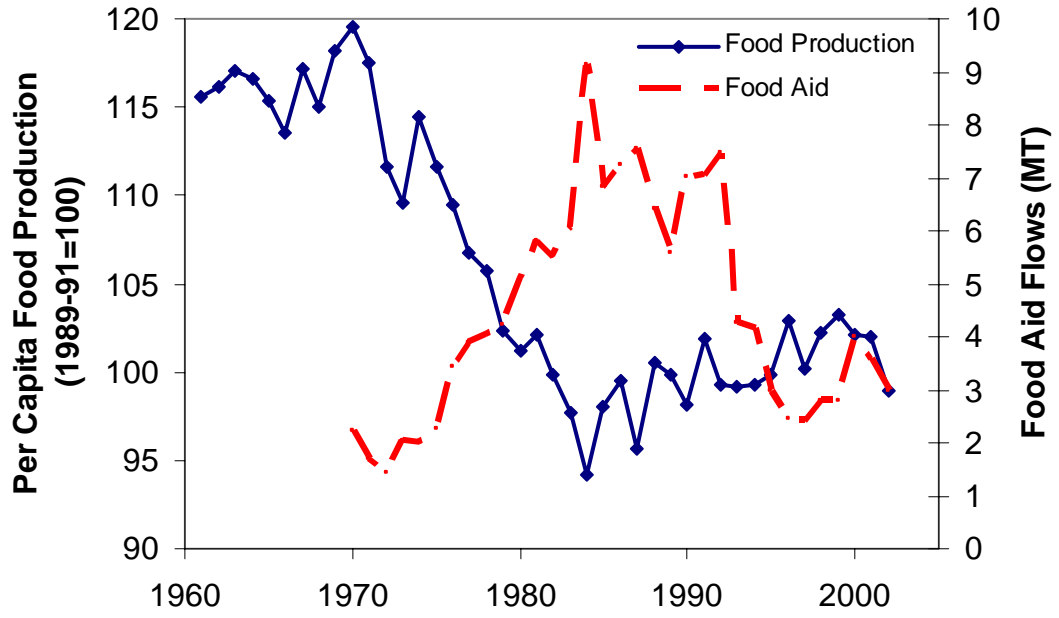
Data source: FAO

Figure 2: Estimated Impulse Response Functions