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The Impact of Safety Nets on Technology Adoption

A Difference-in-Differences Analysis

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The Impact of Safety Nets on Technology Adoption: A Difference-in-Differences Analysis *

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Abstract

This paper contributes to a growing body of empirical literature relating credit constraints and incomplete insurance to investment decisions. We use panel data from rural Ethiopia to investigate whether participation in a safety net program enhances fertilizer adoption. Using a difference-in-difference estimator and inverse propensity score weighting, we find that participation in Ethiopia's food-for-work (FFW) program increased fertilizer adoption. Results also indicate that the intensity of fertilizer usage increased with livestock holdings for food-for-work-participant households, providing some evidence that the intervention helped asset-rich farm households more than asset-poor households. We find no significant effects of free distribution on fertilizer adoption or intensification. Our results are consistent with the hypothesis that safety nets can be viewed as mechanisms that allow households to take on more risk to pursue higher profits. The paper highlights important policy implications related to the inter-related dynamics of safety nets and extension services that aim at promoting productivity-enhancing modern agricultural technologies.

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1 Introduction

There is a rich literature investigating why poor farm households in developing countries are unlikely to adopt risky technologies (Feder et al., 1985). The availability of inputs, uncertainty about profitability, and credit constraints are a few of the explanations that have received the most attention in academic research and policy discussions. However, few studies have empirically looked at the role of insurance, as it relates to the ability to smooth consumption, in explaining technology usage. Imperfections in the insurance market, with the result that households are unable to protect against downside shocks, may lead to risk rationing, where farmers are less willing to take on risk and voluntarily withdraw from the credit market (Binswanger and Sillers, 1983; Boucher et al., 2008; Dercon and Christiaensen, 2011). Poor households that are ill-equipped to handle negative shocks may engage in less risky, less profitable activities.[1] In this paper, we add to the literature on technology adoption by investigating the role safety nets play in the likelihood of a household taking on more risk. Identifying mechanisms that allow households to overcome the initial uncertainty of adopting a potentially profitable technology has important policy implications for poverty alleviation.

In a recent study, Dercon and Christiaensen (2011) investigate the ability of households to take on risky production technologies in the presence of credit and insurance constraints. A key prediction of their model is that households will adopt fewer risky inputs when they face higher *ex-post* downside consumption risk. Using data from rural Ethiopia, we investigate this further and explore the role that food aid can play in the adoption and usage of fertilizer. Food aid programs are implemented to protect against ex-post downside consumption risk and, in essence, can mitigate the adverse effects of shocks, allowing households to engage in higher-return, higher-risk activities.[2]

At the same time, it is plausible to argue that food aid may provide a disincentive to households to invest in productive assets and may lead households to become dependent upon aid. However, previous studies that have investigated the impact of safety nets on household behavior have found no such disincentive effects of receiving aid. For instance, Bezuneh et al. (1988) use a linear

[1] These environments can lead to risk-induced poverty traps, where households that are able to insure their consumption against income shocks engage in more profitable activities and escape poverty, while others are stuck with low-return, low-risk activities that trap them in poverty.

[2] Other papers have explored the demand for formal insurance among farmers (Giné and Yang, 2009; Giné et al., 2008). Unlike these papers, we explore an insurance mechanism, in the form of food aid, which does not require a direct cost to purchase.

programming method and document a positive impact of food aid on investment on farm capital by small-holder farmers in rural Kenya. Similarly, Covarrubias et al. (2012) document that the Malawi Social Cash Transfer scheme had positive effects on agricultural and nonagricultural production decisions.

Studies specific to Ethiopia have also failed to find that Ethiopia's food aid programs have disincentive effects. Little (2008) failed to find evidence of food aid dependency in Northeastern Ethiopia. Abdulai et al. (2005) used cross-sectional data from rural Ethiopia and found a positive impact of food aid on the supply of labor to own agriculture, wage work and own business activities. More recently, providing additional evidence against the "dependency on food aid" hypothesis, Andersson et al. (2011) investigate the impact of the Ethiopian Productive Safety Net Program on rural household's livestock wealth and tree planting. They find a positive impact of the program on the number of trees planted by households but no impact on livestock holdings.

A key limitation of previous studies investigating the impact of safety nets on agricultural investments and production outcomes is the inability to control for selection into the program based on both observable and unobservable characteristics.[3] Identifying the effect of food aid on household behavior is hindered due to selection into the program when assignment into the program is not random. In this paper, we address the non-random assignment of aid allocation by using inverse-propensity score weighting and a difference-in-difference estimator. In doing so, we build on previous studies and add to the literature on the effects of food aid on household behavior by exploring how it affects fertilizer usage, a critical productivity-enhancing modern agricultural input. Understanding how this intervention affected technology adoption is important to have a full picture of its impact on various household outcomes.

The impact of food aid on fertilizer adoption in rural Ethiopia was also studied by Bezu and Holden (2008). These authors use a Heckman selection model on cross-sectional data from the Tigray region of Ethiopia and document that food aid improved fertilizer adoption. Additionally, Gilligan et al. (2009) and Gilligan et al. (2012) found that income transfers under Ethiopia's new Food Security Program (FSP), along with additional support aimed at improving agricultural productivity, led to increased fertilizer use. The present paper differs from these previous studies by exploiting the unanticipated expansion in Ethiopia's food aid program during the 2002 drought and its impact on

[3]A few exceptions include Gilligan and Hoddinott (2007); Giné and Yang (2009); Giné et al. (2008); Andersson et al. (2011); Boone et al. (2013) and Covarrubias et al. (2012).

future behavior.

This paper also differentiates between the impact of food-for-work and free distribution (FD), Ethiopia's two main food aid programs. Investigating this is important for understanding which of the two aid programs is effective in having an impact on future technology adoption. The selection criteria differ between the two programs, leading to possible differences in how the individual programs are related to fertilizer adoption. The theoretical literature evaluates households that have access to credit markets and technology but may choose a suboptimal level of risk due to the inability to insure against adverse shocks. The sample of households eligible for free distribution differs from the sample of households eligible for food-for-work in terms of household characteristics, wealth, and access to credit markets and technology. Therefore, production decisions and risk preferences may be different between households eligible for free distribution and those eligible for food-for-work. Additionally, differences in expected impacts between the two programs may occur due to differences in the work requirements for food-for-work. For example, labor requirements in the food-for-work program may crowd out labor input in other productive activities.

This paper therefore contributes to a growing empirical literature in development economics relating credit constraints and incomplete insurance to investment decisions. In recent years, a number of studies have tested the theoretical literature linking farmers' inability to smooth consumption over time to suboptimal risk levels. Evidence on the effects of insurance on the adoption of improved technologies have produced conflicting results. On one hand, Hill and Viceiza (2012) conducted a framed field experiment in rural Ethiopia and found that insurance had a positive effect on fertilizer purchases. Dercon and Christiaensen (2011) found that downside consumption risk led to lower fertilizer usage in rural Ethiopia. Similarly, Lamb (2003) found that fertilizer demand increased with the depth of the off-farm labor market, measured by the unemployment rate and the share of employment in nonagricultural activities, which suggested that off-farm work and farm production may be complements.[4] However, Giné and Yang (2009) found that farmers who were offered an insured loan to purchase high-yielding hybrid maize and groundnut seeds for planting in Malawi had lower take-up rates than farmers who were offered an uninsured loan.

Many of the previous studies have taken advantage of controlled experiments and/or randomized control trials. Although this paper uses observational data, the richness of the data and the timing of the survey rounds allow us to utilize an identification strategy to investigate how the Ethiopian

[4] Lamb (2003) shows how off-farm labor can serve the purpose of income smoothing *ex post*.

government's response to the 2002 drought impacted farmers' decisions to use fertilizer a year after the intervention.

Difference-in-difference and inverse-propensity score weighting regression results suggest that participation in Ethiopia's food-for-work program increased fertilizer adoption. Results also show that the intensity of fertilizer usage increased with livestock holdings for food-for-work-participant households, providing some evidence that the intervention helped asset-rich farm households more than asset-poor households. We find no significant effects of free distribution on fertilizer adoption or intensification. Our results are consistent with the hypothesis that safety nets can be viewed as mechanisms that allow households to take on more risk to pursue higher profits. The paper highlights important policy implications related to the inter-related dynamics of safety nets and extension services that aim at promoting productivity-enhancing modern agricultural technologies. Furthermore, the results highlight the importance of safety net programs, their effectiveness in ensuring farmers that they will be protected against uninsured shocks, and how that assurance can translate into productivity-enhancing behavior.

In 2005, the Ethiopian Government along with several donor agencies established the Productive Safety Nets Programme (PSNP). The aim of the PSNP was to address chronic food insecurity by using food aid to promote economic development. Prior to 2005, food aid was primarily administered in response to emergency appeals during times of droughts and other unpredictable shocks. One of the main goals of the PSNP is to ensure that chronically food insecure households can meet their basic food needs while encouraging them to engage in productive activities and build up assets. Although our analysis covers the period prior to the implementation of the PSNP, our findings have important implications for the role that the PSNP can play in encouraging productive agricultural activities to help improve food security.

The rest of the paper is structured as follows. Section 2 presents a conceptual framework for the role food aid can play in technology adoption. Section 3 discusses the data. Section 4 presents the identification strategy and the econometric model. Section 5 presents mean treatment effects and Section 6 explores additional issues related to food aid and fertilizer usage. Section 7 concludes.

2 Conceptual Framework

Standard microeconomic theory of technology adoption in agriculture suggests that, in a setting with perfect credit and insurance markets, farm households can borrow to acquire productivity-enhancing agricultural inputs and effectively deal with the risk of crop failure. However, as is well documented in the empirical literature, markets in rural areas of developing countries - including credit and insurance markets - are characterized by imperfections (de Janvry et al., 1991). Additionally, informal risk-sharing mechanisms provide only partial insurance against adverse shocks (Morduch, 1995; Townsend, 1995). Consequently, covariate shocks such as rainfall shocks - which are not insured through the formal insurance market and which simultaneously affect all participants in informal risk-sharing mechanisms - can have significant and long-lasting adverse welfare impacts (Dercon, 2004).

As a result of the market imperfections faced by farmers, farm households make decisions on production and input use *ex ante* in order to minimize the possible impact of an adverse shock (Dasgupta, 1995). For example, Eswaran and Kotwal (1990) show that the inability to smooth consumption over time decreases agents' risk-bearing capacity. Similarly, Boucher et al. (2008) show that households may voluntarily withdraw from the credit market when it entails high-collateral contracts that provide lower expected utility than engaging in a risk-free, subsistence activity. In other words, households will choose to adopt less-risky technologies to avoid permanent damage to their welfare. Their model also suggests that, conditional on having access to credit markets, households will under-utilize modern inputs. As discussed previously, Dercon and Christiaensen (2011) develop an intertemporal model that links *ex post* consumption credit constraints (insurance) to *ex ante* risk-taking behavior. They show that poor farmers are less likely to adopt risky inputs due to credit constraints and the inability to smooth consumption.

The theoretical literature on technology adoption in developing countries therefore suggests that mechanisms that protect against downside shocks could encourage risk-averse farmers to take on more risk and adopt more profit-maximizing production technologies by relaxing liquidity constraints and helping to smooth consumption. In a setup characterized by risk and uncertainty, there are both *ex-ante* and *ex-post* impacts of insurance on technology adoption. The first aspect refers to the decision to adopt risky productivity-enhancing agricultural technologies, which is a central feature of insurance. The latter - the *ex-post* - might be achieved by programs that are not traditional insurance schemes - for example, their payout cannot be anticipated by participants, but the payout affects subsequent investment behavior.[5]

Food aid is a form of insurance that is less likely to be anticipated by farmers and, in principle, could encourage future adoption of modern agricultural inputs among risk-averse farmers. This depends on whether receiving food aid affects recipients' beliefs regarding their likelihood of accessing program benefits in the future. Importantly, there are no direct financial costs incurred by the farmer, which is different than the insured loan described in Giné and Yang (2009); instead, the purpose of food aid is to protect against downside consumption shocks. As discussed above, empirical results generally disprove the hypothesis of disincentive effects from food aid.

In this paper, we empirically test the effects that food aid has on the adoption of fertilizer after a shock has been realized. We focus on fertilizer for a number of reasons. One is that the Ethiopian Government has demonstrated its commitment to agricultural development by introducing policies that promote the use of modern inputs in order to intensify crop yields. Another is that the relatively large number of smallholder farms that have adopted chemical fertilizer gives us a large enough sample to investigate the adoption of a particular modern input. Further, Dercon and Christiaensen (2011) showed that fertilizer use is a high-return but high-risk technology; returns to fertilizer are high when rains fluctuate around their median levels, but it loses its profitability when rainfall is at the extremes. Additionally, examining the adoption of a single technology allows us to avoid the difficulty in controlling for differences across technologies in potential profitability and risk.[6]

3 Context and Data

To investigate the role of the Ethiopian Government's food aid safety net program in fertilizer usage, we exploit the government's response to the 2002 drought, using longitudinal data from the

[5] These payouts can affect investment behavior either through a pure income effect or by changing future beliefs about risks to consumption.

[6] Based mainly on standard non-separable agricultural household models which assume missing and imperfect factor markets, various papers have investigated the determinants of chemical fertilizer adoption. According to previous studies, these determinants can be farmer characteristics (e.g., education, gender, age, and farming experience), household characteristics (e.g., area of cultivable land, assets, and labor), farm biophysical characteristics (such as soil fertility and type), and access to agricultural extension, credit, and markets (Holden et al., 2001; Pender and Gebremedhin, 2008; Freeman and Omiti, 2003; Adesina, 1996; Waithaka et al., 2007; Alem et al., 2010; Doss, 1999; Dercon and Christiaensen, 2011).

Ethiopian Rural Household Survey (ERHS). The ERHS was conducted in 15 peasant associations across rural Ethiopia.[7] The survey was administered by the International Food Policy Research Institute (IFPRI) in collaboration with the department of economics at Addis Ababa University (AAU) and the Center for the Study of African Economies (CSAE) at the University of Oxford.

The panel survey evolved from an initial survey conducted in 1989 administered in seven villages to study the response of households to food crises. At the time of the survey, there was no intention of creating a longitudinal data set. The 450 households within the seven peasant associations were randomly selected, while the villages located in the regions of Amahara, Oromiya, and SNNPR, in southern and central Ethiopia, were primarily ones that had suffered from the 1984-1985 famine and other droughts that followed between 1987 and 1989. In 1994, CSAE and AAU conducted a panel survey incorporating six of the seven villages surveyed in 1989, plus an additional nine villages, to give approximately 1500 households surveyed. The villages were chosen to account for the diversity among the major farming systems. The attrition rate from 1989-1994 in the six villages used in the 1989 survey was less than 7 percent. The lost households were replaced by households which were considered by village elders and officials as being similar, in demographic and wealth terms, to the households that could not be traced. Households formed out of households interviewed in 1989 were also interviewed, usually sons or daughters who after marriage formed their own household. The ERHS interviewed 1,477 households in seven rounds between 1994 and 2009.

We make use of the 1999, 2004, and 2009 rounds of the ERHS.[8] The sample attrition was low between 1999 and 2004; only 5.2 percent of the sample was lost (Dercon and Hoddinott, 2011).[9,10] We restrict our analysis to 6 of the 15 peasant associations because these are the only peasant associations where a nontrivial share of households report using fertilizer and receiving food aid. Six of the excluded villages were not affected by the 2002 drought and therefore did not receive government assistance (refer to Gilligan and Hoddinott (2007)'s Table 1 for a list of these villages) and the other three excluded villages had fewer than ten households reporting using fertilizer.[11] As a result, the external validity of this study is confined to villages with a history of fertilizer usage.

[7]The peasant associations are the lowest administrative unit in rural Ethiopia, consisting of several villages. Throughout the paper we will refer to a village and a peasant association interchangeably.

[8]In 1999, three additional villages were added to the survey; our analysis excludes these additional villages.

[9]The attrition rate between 1994 and 2004 was 13.2 percent.

[10]Refer to Dercon and Hoddinott (2011) for a detailed description of the ERHS panel data set.

[11]Note that, although only some villages received aid, there is no selection bias here. The paper is interested in the effect of food aid on fertilizer usage conditional on the village receiving aid.

The 2002 drought decreased cereal production by over 25 percent and left over 12.3 million Ethiopians in need of food assistance. The government responded to the drought by expanding its food aid program, which primarily consists of food-for-work and free distribution. The ERHS was administered before and after the drought, which will allow us to identify the effect of food aid on fertilizer adoption and intensification.[12] In essence, we are able to identify the effect of the Ethiopian government's response to the 2002 drought.[13] Given that the 2004 wave of the ERHS was conducted almost eighteen months after the peak of the drought and most food aid transfers were made in the first twelve months after the drought (Gilligan and Hoddinott (2007)), we believe the 2004 survey represents a reasonable post-drought panel to investigate the impact of the food aid programs on fertilizer adoption and use.

The 2004 round of the ERHS included a module containing detailed questions about the 2002 drought, its impact on household wellbeing, consumption, food security, and coping mechanisms, the government's response to the drought, and participation in the safety net program.[14] Table 1 provides information on the impact the drought had on the sampled households and households' perceptions of the government's response.[15] The surveyed households reported consuming on average two meals a day during the worst period of the drought (Column 1). Column 2 shows that approximately 50 percent of the households reported needing to sell livestock to pay for food during the drought. Columns 3 and 4 provide the share of households that received food aid under the respective food aid programs. The share of households that participated in a food-for-work scheme was 61 percent. The share of households that received free distribution was also 61 percent.

The survey asked several questions about households' perceptions of the government's response to the 2002 drought. Households were asked whether they felt there was enough food assistance provided to their community; if the household received food assistance, households were asked whether the aid came on time and whether the amount received was sufficient. Columns 5-7

[12] According to USAID's Famine Early Warning System Network (FEWS NET), in 2003 the country experienced normal to above normal rainfall and cereal retail prices were stable.

[13] This is important, as we are not investigating changes in food aid receipt over time. The intervention we are exploiting is the one-time response to the 2002 drought.

[14] As is the case in many household surveys, the information on the drought and participation in the food aid programs was self-reported and no objective checks were done to verify households participation in the programs. Other than the standard measurement errors applicable to any other household survey in a developing country context, we do not expect systematic error in reporting that would affect the analysis. Previous authors (e.g., Gilligan and Hoddinott (2007)) used the same data to investigate the impact of the programs on consumption and food security. Additionally, Gilligan and Hoddinott (2007) state that self-reports of the drought were consistent with rainfall data from nearby weather stations.

[15] A similar table is presented in Gilligan and Hoddinott (2007)

of Table 1 summarize how households perceived the government's response to the 2002 drought. Approximately 34 percent of households felt that the community received enough food assistance during the drought. However, less than 15 percent of households in Aze Doboa and Gara Godo felt that their community received enough food assistance. Of the households that received some food assistance, only 20 percent reported receiving enough food assistance. More than 30 percent of the households in Korodegaga reported receiving enough food aid. Of the households that received food assistance, approximately 54 percent felt that the food aid arrived on time. This share varied between a low of 36 percent of households in Aze Doboa and a high of 73 percent of households in Haresaw. These variations in the timeliness and sufficiency of food aid illustrate the risks to consumption faced by farming households in the event of a shock to food production.

There may exist concern that aid recipient households are fundamentally different from non-recipient households. In essence, if aid recipients believed they were more likely to be protected by the food aid program, then *ex-ante* risk behavior would be different for aid recipients and non-aid recipients and therefore bias impact estimates. Prior research on food aid targeting in Ethiopia has shown that there exist substantial errors of inclusion and exclusion in household targeting (Sharp, 1997; Clay et al., 1999; Jayne et al., 2002; Broussard et al., 2014). Of the villages used in the analysis, only 22 percent of households never received food aid during the 8 years prior to the 2002 drought and only 5 percent of households had received food aid in every round. Most households cycle on and off food aid (conditional on the village receiving aid) and between free distribution and food-for-work.[16]

The survey also asked detailed questions about inputs used for crop agriculture. Households were asked about the type and amount of fertilizer used during the previous main season.[17] Households that report using chemical fertilizer make up our sample of fertilizer-adopting households.[18] Table 2 provides a summary of fertilizer usage by village for 1999 and 2004. Column 1 lists the number of households used in the analysis by village. Columns 2 and 3 provide the share of households using fertilizer and Columns 4 and 5 give the intensity of fertilizer usage. In 2004, 45 percent of the households used fertilizer (Column 3), a decrease from the 56 percent of households that used fertilizer in 1999 (Column 2). Although not shown, many households switched in and out of using

[16]The cycling between free distribution and food-for-work is primarily due to the fact that villages rarely received both programs in a given period, with 2002 being an exception.

[17]The public sector and cooperative unions are the main distributors of fertilizer. Most farmers purchase fertilizer from their local cooperative, placing orders up to eight months in advance (Dercon and Christiaensen, 2011).

[18]DAP and UREA are the two main types of fertilizer used.

fertilizer over the 10-year period during which the ERHS was administered. Application rates in 1999 and 2004 were approximately 50 kilograms of fertilizer per hectare.[19] Most households in the survey reported that cost was the main constraint to using modern inputs, including fertilizer, with few households reporting availability of the inputs as a constraint (Dercon and Christiaensen, 2011).

4 Identification Strategy and Econometric Model

To identify the effect of the Ethiopian Government's response to the drought on fertilizer adoption, we employ difference-in-differences and inverse propensity score weighting.[20] We compare adoption behavior of aid recipient households to households that did not receive aid under the relevant food aid program. The ERHS contains a rich set of variables used by village representatives to select aid recipient households that will allow us to control for observable differences between recipient and non-recipient households (Sharp, 1997; Clay et al., 1999; Jayne et al., 2002; Broussard et al., 2014). Additionally, our empirical strategy will allow us to account for time-invariant unobservable differences between recipient and non-recipient households. By comparing fertilizer usage of aid participant and non-participant households before and after the 2002 drought, we are able to capture the effect of the Ethiopian government's food aid safety net program. The naive difference-in-differences estimator is estimated from the following regression:

$$\Delta Y_i = \beta_0 + \beta_1 D_i + \epsilon_i \tag{1}$$

where ΔY_i is the change in the outcome of interest between 1999 and 2004 for household i, and D is an indicator variable equal to 1 if the household received food aid and 0 otherwise.[21] Our measure of aid comes from self-reported measures of aid received from the government or a non-governmental organization.[22] The estimate of β_1 captures the mean difference in the relevant outcome variable

[19] Only the 1999 round collected data on fertilizer use by crop and plot; the other rounds collected data on fertilizer usage at the farm level. Therefore, we are unable to accurately calculate application rates. We approximate application rates by dividing the amount of fertilizer used by the total amount of cultivated land owned by the household. This is not ideal, given that farmers do not use fertilizer on all their plots of land and for all crops. Therefore, application rates will be underestimated.

[20] A similar method has been used by Gilligan and Hoddinott (2007), who investigate the impact of the program we consider in this paper on future consumption of farm households in rural Ethiopia.

[21] Given that one of the outcomes, fertilizer usage, is a binary variable, this implies running a regression on a dependent variable that takes only three values: -1, 0 , 1.

[22] The intervention is a dummy variable equal to 1 if the household reports receiving aid between September 2002 and March 2004 and 0 otherwise. We use the same measure of aid receipts as used by Gilligan and Hoddinott (2007).

between aid recipient households and non-recipient households of the relevant program.

Comparing simple differences between recipient and non-recipient households could lead to biased estimates of the true impact of food aid due to the fact that food aid was not randomly assigned and pre-intervention characteristics may have determined selection into each food aid program. Additionally, the difference-in-differences estimator relies on the strong assumption that, in the absence of the treatment, average outcomes for the treated and comparison households would have followed parallel paths over time. Let X be a vector of observable control variables which determine selection into the food aid program and are correlated with the outcome variable. Regressing ΔY on D and X would allow us to identify the effect of treatment on the outcome variable. This is the unconfoundedness assumption or the selection-on-observables assumption, which states that treatment is independent of potential outcomes conditional on the observed covariates. This means that, conditional on covariates, treated and non-treated households would, on average, be expected to experience the same changes in outcomes following the drought in the absence of treatment. Rosenbaum and Rubin (1983) show that conditioning on the propensity score, where the propensity score is $Pr(D = 1|x)$, also achieves identification.

Our identification strategy uses the propensity score for weighting to estimate the average treatment effect on the treated (ATT). Hirano et al. (2003) has shown that inverse propensity score weighting produces an efficient estimate of the ATT. Busso et al. (2013a) has shown that inverse-propensity score weighting exhibits small bias when the propensity score model is correctly specified and performs just as well as most matching estimators when overlap is good. Abadie (2005) extends the use of inverse-propensity score weighting to panel data and demonstrates the use of a semiparametric difference-in-differences estimator.

Inverse-propensity score weighting constructs two counterfactual means and takes their difference to obtain the average treatment effect (DiNardo, 2002). The treatment mean and the control mean for the population are obtained by a weighted mean of outcomes in the treated and control group, respectively. This approach reweights the data to balance the distribution of covariates across treated and untreated households.[23] We calculate the ATT by applying weights to comparison households such that the outcomes for the comparison households represent the counterfactual

[23]This approach has been used recently in the economics literature to estimate the average treatment effects of economic development programs (Busso et al., 2013b) and welfare reforms (Bitler et al., 2006), just to name a few. Refer to DiNardo (2002) and Hirano et al. (2003) for a discussion of the use of propensity score reweighting to estimate the average treatment effect on the treated.

outcomes of aid recipient households for their respective food aid programs.

Denoting the estimated propensity score for household i as $\hat{p}_i$, the estimated inverse-propensity score weight for household i is:

$$\hat{w}_i = D_i + (1 - D_i)\frac{\hat{p}_i}{1 - \hat{p}_i} \tag{2}$$

and the estimated average treatment effect on the treated is:

$$A\hat{T}T = \frac{1}{N_T}\sum_{i \in T} \hat{w}_i y_i - \frac{1}{N_C}\sum_{i \in C} \hat{w}_i y_i \tag{3}$$

where N_T is the number of treated observations and N_C is the number of comparison observations. The ATT is calculated by comparing the treatment mean to the reweighted comparison group mean.

Given the many restrictions in the land market in rural Ethiopia, livestock plays an important role in farm productivity.[24] We investigate whether participation in the food aid program has a differential impact on fertilizer use by allowing the impact of receiving aid to vary with the value of the household's livestock holdings. The difference-in-difference reweighted estimator is obtained via the following regression:

$$\Delta Y_i = \beta_0 + \beta_1 D_i + \beta_2 x_i + \beta_3 D_i(x_i - \bar{x}) + \epsilon_i \tag{4}$$

where ΔY_i is the outcome of interest for household i, D is the indicator for treatment, x_i is the value of the household's livestock holdings in 1999, and $\bar{x}$ is the mean value of livestock holdings for the sample, so that $(x_i - \bar{x})$ is the demeaned value of livestock holdings.[25] We weight the regression by the inverse propensity score described above. β_1 identifies the ATT, while $\beta_1 + \beta_3$ identifies how the treatment effect varies with different values of livestock holdings. If β_3 is significantly different from zero, there is evidence of heterogeneity of treatment effects by asset holdings. We estimate standard errors for the impact estimates by a bootstrap using 100 replications of the sample.

Due to the different selection criteria and work requirements across the two forms of food aid programs, we estimate separate treatment effects for participation in FFW and in FD. The richness

[24]Livestock owned by farm households can serve as collateral for credit or complement chemical fertilizer, as organic fertilizer with abundant carbon complements inorganic fertilizers.

[25]The mean value of livestock holdings for the sample in 1999 was 1,280 Ethiopian Birr (ETB). In 1999, 1 ETB equaled approximately 0.17 USD.

of the ERHS allows us to ensure that the variables used to construct the propensity scores are related to program participation and outcomes, that program participants and non-program participants have access to the same markets, and that the dependent variable is measured the same way for participants and nonparticipants (Heckman et al., 1997). We adopt many of the control variables, believed to be associated with the probability of participating in each food aid program, used by Gilligan and Hoddinott (2007).[26] Additionally, we include variables that are correlated with fertilizer usage, specifically lagged values of the outcome variables.

The control variables used to estimate the propensity scores include a dummy for whether the household used fertilizer in 1999; the log value of fertilizer per hectare used in 1999; log real consumption per adult equivalent in 1999; pre-drought (1999) land area owned and its square; pre-drought household demographic variables (number of male household members between the ages of 15 and 64, number of female household members between the ages of 15 and 64, number of household members younger than 15 years of age, number of household members older than 64 years of age, the household's dependency ratio, whether the household is headed by a female, and the log age of the household head); whether the household head's primary job was farming; whether the household head had any formal education; whether the household reported experiencing a drought between 1999-2002; whether a male or female household member experienced a serious illness between 1999-2002; whether all household members were too weak, sick, young or old to work; measures of the household's political and social connections in the village (whether the parents of the household head were important in the village, number of iddirs the household belonged to prior to the drought,[27] and the number of people that would help the household in time of need); an indicator for whether the household received any food aid in 1999; an indicator for whether the household received any food aid during the 1994/95 drought; and an indicator for whether the household met any targeting criteria for the respective program in its village.

Additional variables used to estimate the propensity score for the FD sample include whether a parent of the respondent held a local official position (interacted with regional dummies) and whether the household experienced a death between 1999 and 2002. Additional variables used to estimate the propensity score for the FFW sample include a dummy for whether the household reported

[26] The differences in the variables used to estimate the propensity score are due to ensuring that the "balancing property" is met. Because we use only a subset of the villages used in Gilligan and Hoddinott (2007) we had to exclude some of the conditioning variables used in their paper in order to ensure that the treatment sample and the sample of comparison observations had similar mean propensity scores and observables at various levels of the propensity scores.

[27] Iddirs are informal risk-sharing institutions in Ethiopia.

being visited by an extension agent between 1994 and 1999; the value of livestock holdings in 1999; whether the household's social network had grown in the five years prior to 1999; an indicator variable for whether the household head was born in the village; and whether the household experienced a death within the three years prior to 1999.[28] Refer to Table A.1 in the appendix for a detailed description of the variables used.

5 Results

We use a logit model in order to estimate the propensity scores for both the FFW and FD programs. Table 3 presents the marginal effects from the logit used to create the propensity scores for participation in the free distribution program and for the food-for-work program. Column 1 presents the means for each of the variables used in the participation model. Columns 2 and 3 provide the marginal effects from the logit for the FD and FFW samples respectively. The probability of receiving FD decreased with real consumption per capita and the number of children in the household and increased if the household received aid in previous years and had household members who were too weak, sick, young, or old to work for FFW. The probability of participating in the FFW program decreased with the age of the household head and increased with whether the head had formal education and whether the household head's parent was important in the village.

Table 4 provides the means and difference in means for the samples used in the analysis. Because propensities near zero or one violate the condition required for reweighting - that the probability of treatment be bounded away from zero and one - we remove observations with propensity scores close to zero or one in order to ensure common support. The FFW sample consists of 168 comparison households and 288 participant households. The FD sample consists of 179 comparison households and 285 participant households. Columns 1 and 2 provide the means for the treated and comparison samples for the FD sample. Column 3 provides the differences in means between the treated and comparison samples. The summary statistics are similar to the findings from the logit. Relative to the non-FD recipient households, FD recipient households were more likely to have received aid in the past, tended to have more land, had fewer household members under the age of 15, and had more household members who were too sick to work.

[28] The differences in the variables used to construct the propensity score across the FFW and FD sample are due to ensuring that the "balancing property" is met.

Columns 5 and 6 provide the means for the treated and comparison samples for the FFW sample. Column 7 provides the differences in means between the treated and comparison samples for the FFW sample. Relative to the non-FFW recipient households, FFW recipient households were more likely to have received aid during the 1994 drought; tended to have more land and more male adult household members; tended to be younger and slightly better educated; had fewer household members who were too sick to work; tended to have parents who were important in the village; were more likely to have had a household member die; and were less likely to have been born in the village.

Food aid programs are generally targeted to the poorest sections of the community, implying that poverty status of households would be the most important variable used to select program participants. Table 4 also reports the log of real consumption at the baseline (1999) for both program participants and non-participants. We do not find any statistically significant difference in the log of real consumption between program participants (in both FD and FFW households) and the respective control groups. This implies that selection into the programs based on real consumption was not that important.

Columns 4 and 8 of Table 4 report estimated differences between the treated and control samples after adjusting using inverse propensity score weighting. The differences in means between the FD recipient households and non-FD recipient households are no longer statistically different. The findings are the same for the FFW sample.

Table 5 presents the main results of the paper. Panel A presents the results for food-for-work and Panel B presents the results for free distribution. Columns 1 and 2 present the estimates from the naive difference-in-difference analysis without covariate adjustment. Columns 3 and 4 present the average treatment effect from the reweighted difference-in-difference analysis. The outcome variables are the change in fertilizer usage and the change in the log quantity of fertilizer used in kilograms per hectare. The change in the outcome variables is the difference between 1999 and 2004 (two years before and after the drought). Bootstrapped standard errors are presented in parentheses.

The naive estimator (Columns 1 and 2) shows that food aid recipient households had lower adoption rates and used less fertilizer per hectare than did non-aid recipient households; however, these results are not significantly different from zero. Reweighting the difference-in-difference estimator for covariate imbalance changes the sign of the point estimates for the food-for-work sample but does not change the sign for the free distribution sample (Columns 3 and 4). For the FFW sample, the estimated mean effect on fertilizer intensity is positive but insignificant while the estimated mean effect on fertilizer adoption is statistically significant at the 5 percent level. Because fertilizer usage decreased between 1999 and 2004, the results show that FFW participant households decreased their fertilizer usage by 17.5 percentage points less than non-FFW participant households. The differences between the naive and the reweighted difference-in-difference estimates demonstrate the selection bias associated with the targeting of the food aid programs.

5.1 Heterogeneous Impacts of Food Aid Participation by Livestock Holdings

It is plausible to argue that households with more assets will have easier access to credit and therefore will have the ability to invest in income-improving technologies such as fertilizer. The results presented so far provide the average treatment effect of receiving food aid under the respective food aid program; however, the effect of receiving food aid on fertilizer adoption and intensity may vary with the household's holdings of assets.

Columns 3 and 4 of Table 5 also present the coefficient on the interaction term of aid participation with demeaned livestock holdings in 1999. For FFW, the interaction term is positive and significant at the ten percent level for fertilizer intensity; FFW increases the amount of fertilizer used per hectare for households with higher livestock holdings. The average treatment effect increased by 3.2 percent for each additional 10 percent increase in a household's livestock value above the village mean of livestock holdings. These findings suggest that the government's response to the 2002 drought helped asset-rich farm households more than asset-poor households. We do not find heterogeneous effects of participation in free distribution on fertilizer usage.

5.2 Long-Run Effects

Columns 3 and 4 of Table 5 present the effect of receiving food aid on fertilizer adoption 18 months following the drought: households that participated in the Ethiopian government's FFW program had higher fertilizer adoption rates than did similar households that did not participate in the government's FFW program. These results can be characterized as short-run impacts of Ethiopia's emergency relief program. The ERHS was also administered in 2009, which allow us to test whether there were any long-run effects of the government's response to the 2002 drought.

Columns 5 and 6 of Table 5 present the long-run impact of Ethiopia's food aid program. The change in the outcome variables are between 1999 and 2009, seven years after the drought. The estimates are the average treatment effect from the reweighted difference-in-difference analysis. Bootstrapped standard errors are presented in parentheses. We fail to find any significant long-run impacts of receiving food aid in response to the 2002 drought.

5.3 Falsification Test

There may still be concerns that the significant differences reported in Columns 3 and 4 of Table 5 for FFW households are driven by unobservable characteristics. If unobservable characteristics determine selection into the food aid program and these characteristics are correlated with future fertilizer usage behavior, then the reported estimates will be biased. Additionally, food aid distribution is not a one-off event and receipt in one year is positively correlated with receipt in other years. If aid recipients in 2002 believed they were more likely to be protected by the food aid program, then *ex-ante* risk behavior would have been different for aid recipients and non-aid recipients and therefore the impact estimates would be biased.

To test this concern, we perform the following falsification test. We test the effect that the government's response to the 2002 drought had on fertilizer behavior in 1999, before food aid was administered (in response to the 2002 drought). If unobserved characteristics or differences in *ex-ante* risk taking behavior are driving our results, we would observe differences between the treated and comparison groups in years prior to the actual treatment.

Columns 1 and 2 of Table 6 present results from this falsification test. The dependent variable is the change in fertilizer usage between 1997 and 1999. The sample is restricted to the same sample used in Table 5. The treatment is whether the household received food aid in 2002. The reweighted difference-in-differences coefficients for the FFW sample are negative and not significantly different from zero, suggesting that our results are not driven by unobserved characteristics of the households.

6 Remarks

The results show that food aid in the form of food-for-work leads to households taking on more risk in agricultural production, while food aid as free distribution does not affect risk-taking in agricultural production. This difference between the effects of the two programs could be driven by differences in the variables used to construct the propensity scores for the two programs. Columns 3 and 4 of Table 6 present inverse propensity score estimates using a common set of variables to construct the propensity scores for the FFW sample and the FD sample. For the FFW sample, the point estimates for the ATT are smaller in magnitude and are no longer significant for adoption rates. The point estimates on the interaction term are very similar and the standard errors are smaller, leading to statistical significance at the 5 and 1 percent level of significance. Results for the FD sample are very similar to the point estimates in Table 5, Columns 3 and 4.

A more likely explanation for the difference in findings between the two programs has to do with the eligibility criteria for the two programs. FD is reserved for the poorest households and specifically for households that do not have an able-bodied worker. Additionally, fertilizer application is a labor-intensive activity, with labor and fertilizer application being complements in agricultural production; labor demands for application, weeding and harvest all increase with fertilizer application. We checked the data to see whether FD households grew different crops than FFW households, potentially growing crops that were less responsive to fertilizer. The 1999 survey round collected data on input use by crop and plot. Teff, wheat and maize cultivation accounted for the majority of fertilizer use. The data did not suggest that the FD sample was less likely to grow these crops. However, FD households are poorer on average than are FFW households and own less land on average, suggesting that the differences may be driven by the sample selection of households that are eligible for FFW versus FD.

Another concern may have to do with the fact that we separately estimate impacts of the FFW and FD programs, while in practice receipt of these two forms of food aid may be correlated. In fact, 40% of households in villages with food aid received both FFW and FD. Such a positive correlation between the two treatments would imply that the estimates do not in fact identify the treatment effect of either program, taken by itself. This correlation provides a limitation to the conclusions we can draw from our results, as we do not know the impact that FFW may have on fertilizer adoption in a village that does not receive FD, and vice versa. There are a few ways to deal with this issue:

1.) controlling for FD receipt when estimating impacts of FFW, and vice versa; 2.) excluding FD recipients when estimating impacts of FFW, and vice-versa; 3.) pooling FD and FFW programs into a single measure of food aid receipt.[29]

Removing beneficiaries of the other program from both the treated and comparison samples shrank the sample substantially, which made it difficult to obtain a matched comparison group. As explained in Heckman et al. (1997), dropping a large number of treatment observations due to a lack of common support leads to biased estimates of the average impact of the program. Given that the point estimates for FFW and FD differed in sign and magnitude, we decided that pooling the two programs into a single measure of food aid receipt would not address this concern.[30] Alternatively, for each program, we included an indicator for whether the household received the other program as a control variable in the propensity score matching and found that adding this control did not change the results.

7 Conclusion

Poor farm households that operate in settings where there are insurance and credit market imperfections and who are unable to insure themselves against exogenous shocks through informal mechanisms are known to engage in less risky, less profitable activities. Low adoption of productivity-enhancing modern agricultural inputs, such as fertilizer and improved seeds, is an important example of a risk mitigation strategy that induces what is known in the development economics literature as risk-induced poverty trap. In this paper, we investigated the role safety nets, in the form of food aid, play in promoting fertilizer adoption, using rich household panel data collected before and after an exogenous shock. The Ethiopian government's response to the 2002 large-scale drought provided us an important opportunity to identify the impact of food aid on adoption of fertilizer long after the intervention using a difference-in-differences inverse propensity score weighting estimator. The contribution of the paper is therefore in providing a clear causal relationship between safety nets and technology adoption, controlling for both observable and unobservable characteristics that influence selection to the food aid program.

[29] In a supplementary appendix, Gilligan and Hoddinott (2007) discuss each of these approaches and conclude that the impact estimates from the full sample (without throwing out observations that received the other program) were the most reliable.

[30] The results from pooling the two programs resulted in insignificant estimated coefficients.

Results suggest that households that participated in the food-for-work program following the 2002 drought were more likely to adopt fertilizer 18 months following the drought than were non-FFW participant households. This result is robust to a falsification test, which tests for the possible role of additional unobserved differences between food-for-work recipient households and non-FFW recipient households. We also found that the intensity of fertilizer usage increased with livestock holdings for food-for-work participant households. This provides some evidence that the program benefited asset-rich households more than asset-poor households. We found no significant effects of free distribution on fertilizer usage. We failed to find any long-run effects of the food aid program.

Given the strong need for improving agricultural productivity and food security in poor countries, understanding what promotes adoption of risky but productivity-enhancing modern agricultural inputs is important. We have presented evidence that safety nets can be viewed as a mechanism that allows households to take on more risk to pursue higher profits. Gilligan and Hoddinott (2007) also documented that such interventions improve consumption of households long after the intervention. Our results add to this finding by revealing the larger role that safety nets play in improving risk taking behavior of households, and hence in enhancing agricultural yield and promoting long-term welfare of poor households. This strongly suggests that the provision of formal risk mitigation and shock-coping mechanisms such as the one we considered in this paper could have larger, long-term positive effects on the welfare of poor households.

Table 1: Reported Impact of the 2002 Drought and the Government's Response

	Average Number of Meals Each Day (1)	Sold Livestock to Pay for Food? (2)	Share of Hhs. Receiving FFW (3)	Share of Hhs. Receiving FD (4)	Enough Food Aid Provided to This Community? (5)	Enough Food Aid Provided to Your Household? (6)	Did Food Aid Come on Time? (7)
Haresaw	1.8	55.6	56.8	58.0	45.6	19.2	72.7
Dinki	2.2	38.5	55.7	58.2	46.8	16.9	47.9
Adele	2.0	25.0	35.2	42.0	41.4	17.3	52.8
Korodegaga	2.2	68.4	92.9	81.6	43.7	33.3	64.6
Aze Deboa	2.1	61.4	77.1	64.3	8.6	6.3	35.9
Gara Godo	1.8	48.4	50.5	59.1	14.4	17.5	42.2
Total	2.0	49.6	61.5	60.9	33.7	19.5	53.6

Source: Ethiopian Rural Household Survey
Notes: FFW represents food for work, FD represents free food distribution

Table 2: Fertilizer Usage By Village By Round

		Share of Households Using Fertilizer		Application Rate Per Hectare (KG)	
Village	Obs. (1)	1999 (2)	2004 (3)	1999 (4)	2004 (5)
Haresaw	81	28.4	13.6	29	31
Dinki	79	25.3	17.7	45	39
Adele Keke	88	46.6	45.5	36	67
Korodegaga	98	59.2	52.0	53	58
Aze Deboa	70	85.7	45.7	32	54
Gara Godo	93	88.2	84.9	75	44
Total	509	55.8	44.6	50	52

Notes: Application rates per hectare are for households which report using fertilizer.

Table 3: Logit Estimates for Participation in FFW or FD - Marginal Effects

	Mean (1)	FD (2)	FFW (3)		Mean (1)	FD (2)	FFW (3)
Fertilizer Usage, 1999	0.558	0.139	-0.042	Male household member had serious illness	0.092	-0.019	0.035
		(0.125)	(0.135)			(0.082)	(0.077)
Ln Fertlizer per Hectare, 1999	2.143	-0.024	0.009	Female household member had serious illness	0.086	0.079	-0.029
		(0.031)	(0.035)			(0.083)	(0.078)
ln real consumption, 1999	4.119	-0.095	-0.013	Household members weak/sick/young/old	0.061	0.244	-0.157
		(0.036)***	(0.038)			(0.118)**	(0.116)
Aid Recipient, 1994	0.505	0.102	0.031	Parent important in PA social life	0.676	0.001	0.114
		(0.060)*	(0.059)			(0.048)	(0.046)**
Aid Recipient, 1999	0.307	0.107	-0.047	Number of iddir household belonged to	0.874	0.029	-0.029
		(0.054)**	(0.053)			(0.035)	(0.033)
Household head primary job is farmer	0.768	0.000	-0.037	Number of people that will help in time of need	7.189	-0.004	0.000
		(0.075)	(0.076)			(0.004)	(0.003)
Land area owned (hectares)	1.269	0.051	-0.053	Household met at least one targeting criterion, FD	0.448	-0.065	
		(0.070)	(0.105)			(0.054)	
Land area owned squared	3.115	-0.004	0.048	Household Member Died, 1999-2002	0.222	0.015	
		(0.012)	(0.028)*			(0.055)	
Number of Adult Men	1.503	-0.031	0.010	Parent holds official position in Kebele, Tigray	0.018	-0.126	
		(0.026)	(0.026)			(0.183)	
Number of Children	2.717	-0.046	-0.002	Parent holds official position in Kebele, Amhara	0.016	-0.178	
		(0.016)***	(0.016)			(0.173)	
Number of Elderly Adults	0.230	-0.058	0.070	Parent holds official position in Kebele, Oromia	0.055	0.038	
		(0.059)	(0.055)			(0.109)	
Number of Adult Female	1.597	0.008	0.034	Parent holds official position in Kebele, SNNPR	0.069	0.179	
		(0.026)	(0.025)			(0.097)*	
Dependency ratio	1.243	0.011	0.002	Household Visited By Ext. Agent, 1994-1999	0.192		-0.101
		(0.030)	(0.028)				(0.067)
Ln of household head age	3.824	-0.017	-0.217	Household met at least one targeting criterion, FFW	0.815		0.026
		(0.096)	(0.089)**				(0.060)
Household head has any formal education	0.189	0.056	0.111	Network size has grown in last 5 years	0.299		-0.073
		(0.065)	(0.063)*				(0.046)
Household head is female	0.287	-0.032	-0.056	Household head born in this PA	0.743		-0.069
		(0.073)	(0.072)				(0.059)
Household experienced drought	0.833	0.024	0.052	Household member died, 1996-1999	0.458		0.066
		(0.059)	(0.057)				(0.045)
				Real value of livestock holdings, 1999	1.18		-0.012
							(0.025)
Number of Observations						464	456

Notes: Dependent variable equals one if the household reports receiving aid from the relevant food aid program (FFW or FD) between September 2002 and March 2004 and 0 otherwise. Coefficients represent marginal effects

Table 4: Characteristics of Sampled Households: Selection Variables

	Free Distribution Sample				Food-For-Work Sample			
	Levels		Differences		Levels		Differences	
Selection Variables	Treated (1)	Comparison (2)	Unadjusted (3)	Adjusted (4)	Treated (5)	Comparison (6)	Unadjusted (7)	Adjusted (8)
Fertilizer Usage, 1999	0.579	0.587	-0.008	-0.026	0.590	0.595	-0.005	-0.071
Ln Fertlizer per Hectare, 1999	2.120	2.270	-0.15	-0.100	2.123	2.381	-0.259	-0.155
ln real consumption, 1999	4.083	4.139	-0.056	-0.006	4.098	4.084	0.014	0.149
Aid Recipient, 1994	0.572	0.464	0.108**	0.011	0.580	0.440	0.139***	0.024
Aid Recipient, 1999	0.340	0.251	0.089**	0.043	0.292	0.292	0	0.075
Household head primary job is farmer	0.800	0.788	0.012	0.014	0.799	0.786	0.013	-0.013
Land area owned (hectares)	1.420	1.134	0.286***	-0.022	1.578	0.884	0.695***	0.111
Land area owned squared	3.723	2.396	1.327**	-0.176	4.508	1.123	3.385***	0.877
Number of Adult Men	1.512	1.547	-0.035	0.013	1.611	1.435	0.177*	0.061
Number of Children	2.611	3.078	-0.468***	-0.093	2.833	2.774	0.06	-0.045
Number of Elderly Adults	0.204	0.246	-0.042	-0.018	0.215	0.238	-0.023	0.007
Number of Adult Female	1.628	1.654	-0.026	-0.158	1.681	1.571	0.109	-0.079
Dependency ratio	1.176	1.320	-0.144	-0.080	1.184	1.296	-0.112	0.041
Ln of household head age	3.817	3.825	-0.008	-0.020	3.793	3.854	-0.06**	-0.031
Household head has any formal education	0.214	0.173	0.041	0.010	0.240	0.137	0.103***	0.042
Household head is female	0.256	0.274	-0.018	0.001	0.240	0.298	-0.058	0.028
Households experienced drought	0.839	0.821	0.017	-0.005	0.858	0.815	0.042	-0.015
Male household member had serious illness	0.091	0.101	-0.009	-0.028	0.090	0.101	-0.011	0.001
Female household member had serious illness	0.098	0.089	0.009	-0.002	0.090	0.107	-0.017	-0.002
Household members weak/sick/young/old	0.070	0.028	0.042**	0.006	0.024	0.048	-0.023	0.000
Parent important in PA social life	0.674	0.676	-0.002	0.006	0.719	0.625	0.094**	0.055
Number of iddir household belonged to	0.919	0.849	0.07	-0.004	0.924	0.869	0.055	0.014
Number of people that will help in time of need	6.835	7.223	-0.388	-0.247	7.517	6.810	0.708	0.268
Household met at least one targeting criterion, FD	0.421	0.469	-0.048	-0.012				
Household Member Died, 1999-2002	0.228	0.207	0.021	-0.016				
Parent holds official position in Kebele, Tigray	0.011	0.022	-0.012	0.000				
Parent holds official position in Kebele, Amhara	0.014	0.022	-0.008	0.005				
Parent holds official position in Kebele, Oromia	0.060	0.039	0.021	0.011				
Parent holds official position in Kebele, SNNPR	0.088	0.045	0.043*	-0.003				
Household Visited By Ext. Agent, 1994-1999					0.163	0.268	-0.105***	-0.018
Real Value of Livestock Holdings, 1999					1.320	1.016	0.305	-0.111
Household met at least one targeting criterion, FFW					0.854	0.750	0.104***	0.009
Network size has grown in last 5 years					0.281	0.357	-0.076*	0.030
Household head born in this PA					0.701	0.815	-0.114***	0.011
Household member died, 1996-1999					0.507	0.429	0.078	-0.070

Notes: The FFW sample consists of 168 comparison households and 288 treated households. The FD sample consists of 179 comparison households and 285 treated households. Columns 4 and 8 report estimated differences between the treated and control samples after adjusting using inverse propensity score weighting.

Table 5: Estimates of the Impact of Food Aid

	Naive Estimate		Short-Run Estimate		Long-Run Estimate	
Panel A: Food For Work						
	Adoption (1)	Intensity (2)	Adoption (3)	Intensity (4)	Adoption (5)	Intensity (6)
Difference in average outcomes, ATT	-0.022	-0.204	0.175**	0.302	0.069	0.196
	(0.053)	(0.197)	(0.086)	(0.233)	(0.076)	(0.258)
Impact by Livestock Holdings, 1999:						
Interaction Term			0.130	0.321*	0.063	0.148
			(0.102)	(0.176)	(0.040)	(0.189)
Panel B: Free Distribution						
	Naive Estimate		Short-Run Estimate		Long-Run Estimate	
	Adoption (1)	Intensity (2)	Adoption (3)	Intensity (4)	Adoption (5)	Intensity (6)
Difference in average outcomes, ATT	-0.036	-0.235	-0.009	-0.119	0.029	0.149
	(0.049)	(0.214)	(0.050)	(0.227)	(0.060)	(0.295)
Impact by Livestock Holdings, 1999:						
Interaction Term			0.001	-0.048	-0.060	-0.151
			(0.054)	(0.182)	(0.037)	(0.156)

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Notes: The naive estimate gives the difference-in-differences estimate without covariate adjustment. The short-run and long-run estimates give the reweighted difference-in-differences estimates. The dependent variables are the change in the outcomes of interest between 1999 and 2004 (naive and short-run estimate) or between 1999 and 2009 (long-run estimate). The FFW sample consists of 168 comparison households and 288 treated households. The FD sample consists of 179 comparison households and 285 treated households. Bootstrapped standard errors (in parentheses) use 100 replications of the sample. Fertilizer adoption is an indicator for whether the household used fertilizer in 2004 relative to 1999, where -1 = stopped using fertilizer, 0 = no change in usage, 1 = started using fertilizer. Fertilizer intensification is the change in the log quantity of fertilizer used in kilograms per hectare, 1999-2004. Livestock holdings is the real value of livestock in thousands of Ethiopian Birr in 1999.

Table 6: Robustness Checks

	Falsification Test		Common Covariates	
Panel A: Food For Work				
	Adoption (1)	Intensity (2)	Adoption (3)	Intensity (4)
Difference in average outcomes, ATT	-0.087	-0.171	0.069	-0.106
	(0.055)	(0.275)	(0.072)	(0.238)
Impact by Livestock Holdings, 1999:				
Interaction Term	-0.077	-0.306	0.165**	0.439***
	(0.047)	(0.296)	(0.073)	(0.157)
Panel B: Free Distribution				
	Falsification Test		Common Covariates	
	Adoption (1)	Intensity (2)	Adoption (3)	Intensity (4)
Difference in average outcomes, ATT	0.016	0.164	-0.020	-0.146
	(0.049)	(0.194)	(0.050)	(0.207)
Impact by Livestock Holdings, 1999:				
Interaction Term	0.049	0.191	-0.000	-0.050
	(0.044)	(0.151)	(0.056)	(0.179)

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Notes: The dependent variable for the falsification test is the change in the outcomes of interest between 1997 and 1999. Columns 3 and 4 give the reweighted difference-in-differences estimates using a common set of variables to construct the propensity scores for the FFW sample and the FD sample. The FFW sample consists of 168 comparison households and 288 treated households. The FD sample consists of 179 comparison households and 285 treated households. Bootstrapped standard errors (in parentheses) use 100 replications of the sample. Fertilizer adoption is an indicator for whether the household used fertilizer in 2004 relative to 1999, where -1 = stopped using fertilizer, 0 = no change in usage, 1 = started using fertilizer. Fertilizer intensification is the change in the log quantity of fertilizer used in kilograms per hectare, 1999-2004. Livestock holdings is the real value of livestock in thousands of Ethiopian Birr in 1999.

A Appendix

Table A.1: Variables Used in the Analysis

Household Used Fertilizer, 1999
Ln Fertlizer per Hectare, 1999
ln real consumption, 1999
Household Received Food Aid in 1994/1995
Household Received Food Aid in 1999
Household Head's Primary Job is Farming
Land area owned (hectares), 1999
Land area owned squared, 1999
Number of Adult Male Household Members, 1999
Number of Household Members Between the Ages of 0-14, 1999
Number of Elderly Adult Household Members, 1999
Number of Adult Female Household Members, 1999
Dependency ratio - Number of HH members between the ages of 0-14 or 65 and older divided by the number of HH members aged 15-65, 1999
Ln Age of household head, 1999
Household head has any formal education, 1999
Household head is female, 1999
Household experienced drought Between 2000 and 2002
Male Household Member Had Serious Illness Between 1999 and 2002
Female Household Member Had Serious Illness Between 1999 and 2002
All Household Members Were Too Weak/Sick/Young/Old to Work on a Public Works Project
Parent important in PA Social Life
Number of Iddir Household Belonged To, 1999
Number of People Who Will Help in Time of Need, 1999

<u>Additional FD Variables</u>
Household Met At Least One Community Targeting Criterion for FD
Household Member Died Between 1999 and 2002
Parent Holds Official Position in Kebele, 1999

<u>Additional FFW Variables</u>
Household Participated in Extension Services between 1994-1999
Real value of livestock holdings in thousands of Ethiopian Birr, 1999
Household Met At Least One Community Targeting Criterion for FFW
Network Size Has Grown in Last 5 years, 1999
Household Head Born in This Peasant Association
Household Experienced a Death of a Household Member in the Last 3 years, 1999

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