The Impact of Emergency Food Aid on Children's Schooling and Work Decisions

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Abstract

In this paper, we empirically test the effect that food aid had on children's educational attainment and work decisions following the 2002 drought. Difference-in-differences and inverse-propensity score weighting regression results suggest that participation in Ethiopia's food-for-work program following the 2002 drought decreased the number of years of schooling for younger children and increased the likelihood that older children were not enrolled in school because of work responsibilities. Participation in Ethiopia's free distribution program following the 2002 drought increased the number of years of schooling for younger boys. This paper contributes to our understanding of determinants of schooling decisions and child labor. It provides valuable lessons on how unconditional transfers can impact schooling decisions and child labor.

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1 Introduction

Negative shocks can have devastating consequences for poor households. Imperfections in credit and insurance markets lead farm households to adopt non-market coping strategies to mitigate the adverse effects of negative shocks. These coping strategies include selling assets and relying on informal insurance arrangements (Ligon et al., 2002). However, asset-based coping strategies and informal risk-sharing mechanisms provide only partial insurance against adverse shocks (Morduch, 1995; Townsend, 1995; Fafchamps et al., 1998; Fafchamps and Lund, 2003; Kazianga and Udry, 2006; Mogues, 2011). Hence, households are forced to adopt other coping strategies. One such strategy that poor households use has been to decrease human capital investments. Decreases in child nutrition and health expenditure can have a detrimental impact on children's future human capital accumulation and earning ability.[1] Similarly, the withdrawal of children from school or the decision not to enroll children can adversely affect their future earnings, confining future generations to poverty (Jacoby and Skoufias, 1997; Thomas et al., 2004; Beegle et al., 2006; Asadullah et al., 2006; Duryea et al., 2007; Alcaraz et al., 2012; Cogneau and Jedwab, 2012; Björkman-Nyqvist, 2013).[2] Hence, the inability of poor households to protect themselves from adverse income shocks may result in households adopting short-term coping strategies that may have long-term consequences (Dercon, 2004).

Governments and donors in several countries have undertaken various social protection programs to insulate vulnerable groups of society from risks and consequences of shocks. A conditional cash transfer program is one example that aims to encourage households to send their children to school and to visit health centers.[3] With the expansion of conditional cash transfer programs in many developing countries, several studies have investigated the role of conditional cash transfer programs in preventing households that have been exposed to shocks from using child labor as a coping strategy (de Janvry et al., 2006; Gitter and Barham, 2009). This paper investigates the role of Ethiopia's food aid program following the 2002 drought. Unlike conditional cash transfer programs, Ethiopia's food aid program did not come with child enrollment conditions. Food aid programs are implemented to protect against ex-post downside consumption risk and, in essence, can mitigate

[1] See, for example, Hoddinott and Kinsey (2001); Yamano et al. (2005); Alderman et al. (2006); Hoddinott et al. (2008); Alderman et al. (2009).

[2] For a review of the literature on the effect of natural disasters on human capital, refer to Baez et al. (2010).

[3] Several studies have found a positive role of conditional cash transfer programs on enrollment and health. See Rawlings and Rubio (2005) for examples of the positive effect of conditional cash transfer programs on enrollment and health in Colombia, Mexico, and Nicaragua.

the adverse effects of shocks, thereby preventing households from relying on detrimental coping strategies. This paper explores whether aid recipient households were less likely to depend on child labor to protect their households from the adverse effects of the drought.

The impact that shocks have on human capital development has been extensively researched. For example, Björkman-Nyqvist (2013) shows that negative rainfall shocks negatively affected female enrollment and test scores in Uganda. Using panel data from Tanzania, Beegle et al. (2006) found that negative agricultural income shocks increased child labor and decreased school enrollment. Cogneau and Jedwab (2012) shows that the cocoa crisis of 1990 in Côte d'Ivoire delayed or canceled school enrollment of girls aged 7-11 years old, delayed or canceled school enrollment of older boys aged 12-15, and increased the labor of older boys aged 12-15. Asadullah et al. (2006) shows that, in Ethiopia, weather-induced crop shocks decrease the probability of primary school enrollment and completion of primary school for girls.[4]

This paper builds on the literature on the relationship between shocks and educational attainment and investigates the role that safety nets play in combatting households' decision to impact children's schooling in response to a shock. In this paper, we empirically test the effect that food aid had on children's educational attainment and work decisions following the 2002 drought. We separately investigate the effect of food-for-work and free distribution. The reason for separating the two programs is due to the differing selection criteria as well as the differences in work requirements. For example, labor requirements with the food-for-work program may leave parents with less time available for other productive activities (e.g. home production or own farm activities) and require additional help from children. This paper contributes to our understanding of determinants of schooling decisions and child labor. It provides valuable lessons on how unconditional transfers can impact schooling decisions and child labor.

A key limitation of previous studies investigating the impact of safety nets on schooling decisions is the inability to control for selection into the program based on both observable and unobservable characteristics. Identifying the effect of food aid on household behavior is hindered due to selection into the program if assignment into the program is not random. In this paper, we address the non-random assignment of aid allocations by using inverse-propensity score weighting and a difference-

[4]Negative aggregate shocks can have a negative or positive effect on schooling depending on whether the substitution effect or the income effect of a shock dominates. However, the empirical literature out of Africa has shown that the income effect dominates, and households respond to negative shocks by decreasing investments in schooling.

in-differences estimator. In doing so, we build on previous studies and add to the literature on the effects that food aid has on household behavior by exploring how it affects schooling decisions.

Difference-in-differences and inverse-propensity score weighting regression results suggest that participation in Ethiopia's food-for-work program following the 2002 drought decreased the number of years of schooling for younger children and increased the likelihood that older children were not enrolled in school because of work responsibilities. Participation in Ethiopia's free distribution program following the 2002 drought increased the number of years of schooling for younger boys. The paper highlights important policy implications related to safety nets and schooling decisions. Furthermore, the results reveal the importance of safety net programs and their role in educational outcomes.

This paper is most closely related to Hoddinott et al. (2010), which finds that Ethiopia's current public works component of the Productive Safety Net Program decreased time spent working for boys and increased their school attendance. Additionally, they present evidence that the public works adversely affected younger girls but benefited older girls. Hoddinott et al. (2010) use a similar approach employed in this paper, namely a matching estimator. However, a limitation of their paper is that the analysis relies on retrospective data for pre-program characteristics. A benefit of this paper is that we have detailed data on household and individual characteristics before and after the 2002 drought. Additionally, we not only investigate the relationship of participation in a public works program, but also participation in an unconditional food aid program.

The rest of the paper is structured as follows. Section 2 discusses the 2002 drought and its impact on the sampled villages. It also describes the data. Section 3 presents the identification strategy and the econometric model. Section 4 presents mean treatment effects. Section 5 discusses some limitations to the data and the implications for the interpretation of our results. Section 6 concludes.

2 Context and Data

The 2002 drought affected parts of Afar, Amhara, Oromia, and SNNP regions, decreased national 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. With the distribution of 1.5 million metric ton of

food, it was the largest food aid operation in Ethiopia's history by the government and the World Food Program (Simkin et al., 2004). The government's response to the drought had been hailed, for the most part, as a success. The success had been attributable to the monitoring and appeals for aid by the government's Disaster Prevention and Preparedness Commission (DPPC), the official body responsible for overseeing the aid disbursements in Ethiopia, and the timely response by the international donor community.[5]

To investigate the role of the Ethiopian Government's food aid safety net program on schooling decisions, we exploit the Government's response to the 2002 drought, using longitudinal data from the Ethiopian Rural Household Survey (ERHS). The ERHS was conducted in 15 Peasant Associations across rural Ethiopia.[6] 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 ERHS interviewed 1,477 households in seven rounds between 1994 and 2009. We make use of the 1999 and the 2004 rounds of the ERHS. Between 1999 and 2004, sample attrition was low, only 5.2 percent of the sampled households were lost (Dercon and Hoddinott, 2011).[7] The ERHS was administered before and after the drought which allows us to identify the effect of food aid on schooling and work decisions.[8] In essence, we are able to identify the effect of the Ethiopian Government's response to the 2002 drought.[9]

The 2004 round of the ERHS included a module containing detailed questions about the 2002 drought, its impact on household wellbeing, consumption, food security, coping mechanisms, the government's response to the drought, and participation in the safety net program. Table 1 provides information on households' experience with the 2002 drought for the fifteen villages surveyed in the ERHS. The sample consists of households that reported having school-age children in the household (between the ages of 3-15 in 1999).[10] In 9 of the 15 villages, over forty percent of the households

[5] A report by the Steering Committee for the Evaluation of the Joint Government and Humanitarian Partners Response to the 2002-03 Emergency in Ethiopia concludes that the response by the government of Ethiopia, local and international donor community managed by the Disaster Prevention and Preparedness Commission (DPPC) and NGOs effectively prevented famine. The report attributes the success to donor and government commitments and effective Early Warning Systems (EWS) coordinated by the DPPC (Simkin et al., 2004).

[6] The Peasant Associations are the lowest administrative unit in rural Ethiopia and consist of several villages. Throughout the paper we will refer to a village and a Peasant Association interchangeably.

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

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

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

[10] A similar table is presented in Gilligan and Hoddinott (2007) for the full sample of households.

report experiencing a drought during 2002. Household self-reported experiences with the drought also correspond to the share of households reporting receiving food aid. In the six villages not reporting a drought in 2002, less than eight percent of the households report receiving food aid compared to over forty percent in the villages that report experiencing a drought.

The surveyed households reported consuming on average two meals a day during the worst period of the drought. Approximately 40 percent of the households reported needing to sell livestock to pay for food during the drought; this varied between a low of 27 percent in Doma and Adele Keke to a high of 71 percent in Korodegaga. Approximately 24 percent of households felt like the community received enough food assistance during the drought. In 6 of the villages, over 40 percent of the households felt that the community received sufficient food assistance, however, in 3 of the villages less than 25 percent of households felt the community received enough food aid. On average, fewer households felt that their household received enough food aid, compared to their community receiving enough food aid. Only 20 percent of households reported receiving adequate food assistance. Korodegaga and Haresaw had the most satisfied households with 37 and 25 percent of households respectively feeling like they received enough food assistance. Of the households that received food assistance, approximately 56 percent felt that the food aid arrived on time. This share varied from a low of 36 percent of households in Aze Doboa to a high of 70 percent of households in Haresaw and Korodegaga.

The analysis is restricted to school-age children during the 2002 drought. Therefore, the sample consists of children that were between the ages of 3 and 15 in 1999. The final sample size consists of 1,399 children in 551 households across the 9 drought-impacted peasant associations. The ERHS ask about the enrollment status of each household member between the ages of 4 and 21 years of age within the 12 months prior to the survey. A child is considered enrolled in school if they report attending school during the previous 12 months. For children that were not attending school, the survey asks why the child was not attending school. We record a child as working if the child was not attending school because the child was required for farm activities, for household activities, to care for a family member, or to work for a wage.

2.1 Impact of the 2002 Drought on Schooling Decisions

We first explore whether the 2002 drought impacted the schooling decisions of school age children. We do this by comparing years of schooling completed in the ERHS villages that did not report suffering from the drought to the ERHS villages that did report suffering from the drought. Schooling decisions may be different across the two set of villages so we employ difference-in-differences, by comparing the change in years of schooling between the drought impacted villages to the non-drought impacted villages.[11] The difference-in-differences estimator is estimated from the following regression:

$$\Delta Y_{ij} = \beta_0 + \beta_1 D_j + \beta_2 D_j * Boy + \epsilon_{ij} \quad (1)$$

where ΔY_{ij} is the change in years of schooling between 1999 and 2004 for child i in village j. D is an indicator variable equal to 1 if the child resides in a village that suffered from the drought and 0 otherwise. Boy is an indicator variable for whether the child is a boy. The estimate of β_1 captures how much more years of schooling changed after the drought in drought impacted areas compared to non-drought impacted areas. β_2 captures whether the differential difference in years of schooling following the drought varied by gender.

Table 2 presents the difference-in-differences estimates of the impact of the 2002 drought on years of schooling. Column 1 presents the results for all children. Column 2 presents the results for children that were between the ages of 3 and 12 years of age in 1999 and Column 3 presents the results for children that were between the ages of 13 and 15 years of age in 1999. The age of 12 as the cutoff is chosen to represent the age when many children should be completing primary school. For all three specifications, children in villages that suffered from the drought had completed fewer years of schooling than children in villages that were not impacted by the drought. For younger children, girls in villages that suffered from the drought had .44 fewer years of schooling in 2004 compared to girls in the non-drought villages. For younger boys, boys in villages that suffered from the drought had .20 (-0.44 + 0.24) fewer years of schooling in 2004 compared to boys in the non-drought villages. For older children, children in villages that suffered from the drought had completed .73 fewer years of schooling two years after the drought.

[11] Changes in education might be different across different PAs as a result of other differences other than the drought. Recall the drought-impacted PAs also received emergency relief. Although the timing of the emergency relief was quick and averted a disaster, households still had time to respond to the drought in case emergency relief did not arrive or was delayed. Therefore, by comparing changes in years of schooling, we are still able to observe if households responded to the shock by withdrawing kids from school (or not enrolling kids in school).

The results in Table 2 rely on the assumption that villages that suffered from the drought were on the same preshock trend as the villages that did not suffer from the drought. Differential preshock trends could account for the differences observed between the drought hit areas and the non-drought hit areas. To address this concern, we take advantage of the three rounds of the ERHS prior to the 2002 drought (1995, 1997, and 1999) to compare the trend in years of schooling between drought-hit villages and the villages not affected by the drought before 2002. We run the following regression:

$$S_{ijt} = \beta_0 + \beta_1 D_j + \beta_2 trend_t + \beta_3 D_j * trend_t + \epsilon_{ij} \tag{2}$$

where S_{ijt} is school enrollment, $trend_t$ is the preshock time trend, and D is an indicator variable equal to 1 if the child resides in a village that suffered from the drought and 0 otherwise. β_1 gives the average difference in enrollment rates for children in the drought-impacted villages compared to the non-drought impacted villages. β_2 captures how school enrollment had been trending between 1995 and 1999 for all villages. β_3 captures whether there exist a preshock differential time trend in enrollment rates between drought hit areas and non-drought hit areas. Because the 1995 survey round only asked about current enrollment for children between the ages of 4 and 15 we run two set of regressions. The first set of regressions uses all three rounds and restricts the sample to children between the ages of 6 and 15. The second set of regressions uses the 1997 and 1999 survey rounds and includes all children between the ages of 6 and 18. The data is stacked so that the regression picks up the trend in enrollment rates of children between the ages of 6 and 15 or 18, depending on the sample restriction.

Table 3 presents the results from regression 2. Panel A presents the results using the 1995, 1997 and 1999 survey rounds of the ERHS and restricts the sample to children between the ages of 6 and 15. Panel B presents the results using the 1997 and 1999 survey rounds of the ERHS and includes all children between the ages of 6 and 18. The regressions are run on the full sample of children and separately for boys and girls. In all of the regressions, the coefficient on the indicator variable for the drought impacted villages is negative, drought-impacted villages had lower enrollment rates, on average, prior to the 2002 drought than non drought-impacted villages. In most of the regressions the coefficient is insignificant. School enrollment had been trending up between 1995 and 1999. This is positive and significant for all the regressions in both Panel A and Panel B. For all but one of the regressions, β_3 is not statistically different from zero suggesting that there was no preshock differential trend between drought hit areas and non-drought hit areas. β_3 is statistically significant

at the 10 percent level in Panel B for the full sample of children between the ages of 6 and 12.

The results presented in Tables 2 and 3 provide suggestive evidence that the drought lowered the years of schooling for school aged children. Next we investigate if the government's response to the drought mitigated the disruption to schooling.

3 Identification Strategy and Econometric Model

To identify the effect of the Ethiopian Government's response to the drought on schooling and work decisions we employ difference-in-differences and inverse propensity score weighting.[12] The estimator compares differences in pre-drought and post-drought schooling and work decisions of children in aid recipient households to weighted differences in pre-drought and post-drought schooling and work decisions of children in households that did not receive aid under the relevant food aid program.

Our identification strategy uses the propensity score, where the propensity score is $Pr(D = 1|x)$, as weights 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. (2014) 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 inverse-propensity score weighting with the difference-in-differences method to estimate the average treatment effect on the treated. The identification strategy accounts for observed and (time-invariant) unobserved factors that may influence children's schooling and work decisions.

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 is 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

[12] A similar method has been used by Gilligan and Hoddinott (2007) and Alem and Broussard (2013). Gilligan and Hoddinott (2007) investigate the impact of Ethiopia's food aid program following the 2002 drought on future consumption of farm households in rural Ethiopia. Alem and Broussard (2013) explore the impact that emergency relief following the 2002 drought had on fertilizer adoption.

treated and untreated households.[13] We calculate the ATT by applying weights to comparison households such that the outcomes for the comparison households represent the counterfactual outcomes of children in aid recipient households for their respective food aid programs.

Denoting the estimated propensity score for person i as $\hat{p_i}$, the estimated inverse-propensity score weight for person i is:

$$\hat{w_i} = D_i + (1 - D_i)\frac{\hat{p_i}}{1 - \hat{p_i}} \tag{3}$$

and the estimated average treatment effect on the treated is:

$$\hat{ATT} = \frac{1}{N_T}\sum_{i \in T} \hat{w_i} y_i - \frac{1}{N_C}\sum_{i \in C} \hat{w_i} y_i \tag{4}$$

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.[14]

The difference-in-differences reweighting estimator is obtained via the following regression:

$$\Delta Y_i = \beta_0 + \beta_1 D_i + \beta_2 D_i * Boy + \epsilon_i \tag{5}$$

where ΔY_i is the outcome of interest for individual i, D is an indicator variable equal to 1 if the household received food aid and 0 otherwise, and Boy is an indicator variable equal 1 if the child is a boy.[15] We weight the regression by the inverse propensity score described above. The estimate of β_1 captures the mean difference in the relevant outcome variable between children in aid recipient households and children in non-recipient households of the relevant program. β_2 identifies whether the treatment effect varies by gender. Our measure of aid comes from self-reported measures of aid received from the government or a non-Government Organization.[16] We estimate standard errors for the impact estimates by a bootstrap using 1000 replications of the sample.[17] Because

[13] This approach has been used recently in the economics literature to estimate the average treatment effects of economic development programs (Busso et al., 2013) 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.

[14] Note that the weight for the treated observations is 1.

[15] Given that work status is a binary variable, this implies running a regression on a dependent variable that takes on only three values: -1, 0 , 1.

[16] 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 used by Gilligan and Hoddinott (2007).

[17] Abadie and Imbens (2008) show that applying the bootstrap to calculate standard errors with matching estimators is not valid. This is not the case with inverse propensity score weighting.

propensities near one violate the condition required for reweighting that the probability of treatment be bounded away from one, to ensure common support we remove observations whose propensity score were greater than .90.[18] The FFW sample consists of 636 children in aid-receiving households and 471 comparison children. The FD sample consists of 747 children in aid-receiving households and 627 comparison children.

The controls we include for the participation regressions include household characteristics from 1999, variables that capture the households social networks, political connections, death and illness shocks, and past aid receipts. 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 selection of eligible households to the FD program is done by the PA administration with the participation of community leaders (Conning and Kevane, 2002).[19] Groups explicitly targeted for assistance are the old, disabled, lactating and pregnant women, and those attending to young children. Selection to the FFW involves self-targeting. In principle, the benefits of the program should only induce the targeted beneficiary group to self-select into participation. The FFW program offers a below-market wage rate, paid in-kind, to able-bodied individuals.

4 Results

We use a logit model to estimate the propensity scores for both the FFW and FD programs. The ERHS contains a large set of covariates that determine participation in Ethiopia's food aid program. Additionally, the richness of the ERHS allows us to ensure that: the variables used to construct the propensity scores are related to program participation and outcomes, program participants and non-program participants have access to the same markets, and the dependent variable is measured the same way for participants and nonparticipants (Heckman et al., 1997). We adopt many of the same set of control variables believed to be associated with the probability of participating in each food aid program used by Gilligan and Hoddinott (2007). Additionally, we include variables that are correlated with schooling decisions, specifically whether the household head and the spouse have any education. We include variables to capture the wealth of the household: log real consumption per adult equivalent in 1999, pre-drought (1999) land area owned and its square, and pre-drought

[18] To ensure that our results are not sensitive to this cutoff, we replicate the regressions using a cutoff of .95 and .98. The estimates are similar.

[19] Jayne et al. (2001) outline this two-stage process.

value of livestock holdings. We include pre-drought household demographic variables: household size, the household's dependency ratio, whether the household is headed by a female, and the log age of the household head. Variables that capture the health of the household include: 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, and whether the household experienced a death between 1999-2002. A household's political and social connections in the village have been shown to influence aid decisions (Broussard et al., 2014), so we include: whether the household head was born in the village, whether a parent of the respondent held a local official position (interacted with regional dummies), whether the parents of the household head were important in the village, number of iddirs[20] the household belonged to prior to the drought, and the number of people that would help the household in time of need. Additional control variables include the age of the child in 1999 and age squared, the gender of the child, whether the household head's primary job was farming, 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 FFW sample include the FFW wage interacted with household size. Refer to Table A.1 in the appendix for a detailed description of the variables used.

Table 4 presents the marginal effects from the logit used to create the propensity scores for participation in the Free Distribution program and the Food-For-Work program. Column 1 presents the means for each of the variables used in the participation model. The probability of receiving FD decreased with real consumption per capita and increased if the household had household members that were too weak, sick, young, or old to work for FFW and if a household member had died between 1999 and 2002. The probability of participating in the FFW program decreased with the value of livestock holdings if the household had household members that were too weak, sick, young, or old to work for FFW, and the larger the number of iddirs the household belonged to. The probability of participating in the FFW program increased if the household head's parent was important in the village. Households with more adults and more children are less likely to take part in the FFW program the larger the public works wage.

Table 5 provides the means and difference in means for the samples used in the analysis. Columns 1 and 2 provide the means for the treated and comparison samples for the FD sample. Column

[20] An iddir is a type of insurance program run by a community or group to meet emergency situations, primarily funerals.

3 provides the differences in means between the treated and comparison samples. The summary statistics are similar to the findings from the logit. FD recipient households had lower per capita consumption and livestock values in 1999 than non-recipient households, they were more likely to have a sick household member or a household member that died, and had a smaller social network.

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. FFW recipient households had slightly higher consumption than non-participant households, children in FFW receiving households were more likely to have a mother that had some formal education but were less likely to come from a female-headed household.

Inverse propensity score weighting balances the covariates between the treated and comparison samples. By reweighting the comparison sample with the inverse of the propensity score, the mean of each covariate should be equal across the treated and comparison samples. Columns 4 and 8 of Table 5 report estimated differences between the treated and comparison 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.

Tables 6 and 7 present the average treatment effect from the reweighted difference-in-differences analysis for the FFW and FD samples respectively. The outcome variables are the change in years of schooling and the change in work status. The changes in the outcome variables are between 1999 and 2004 (two years before and after the drought). Bootstrapped standard errors are presented in parentheses. Columns 1 and 2 present the results for the full sample, Columns 3 and 4 present the results for the sample of children that were between the ages of 3 and 12 in 1999, and Columns 5 and 6 present the results for the sample of children that were between the ages of 13 and 15 in 1999.

For the FFW sample (Table 6), regression results for the full sample show that children in households that received FFW had .49 fewer years of schooling in 2004 than children in households that did not receive FFW. The result is significant at the 5 percent level. The coefficient on the FFW dummy and boy interaction term is positive but insignificant. No significant effect of receiving FFW is obtained on the work decision. For younger children (aged 3-12), the coefficient on the FFW treatment dummy variable is negative and insignificant for the work decision. The coefficient on

the FFW treatment dummy variable is negative and significant for the years of schooling regression and the coefficient on the FFW treatment dummy interacted with the boy dummy is positive and significant. Results suggest that young girls in households that received FFW following the 2002 drought had .54 fewer years of schooling than young girls in households that did not receive FFW. For young boys, the difference was only .27 fewer years of schooling ($\beta_1 + \beta_2$). For older boys the pattern in the years of schooling regression is the same (negative coefficient on the treatment dummy and positive coefficient on the interaction term), however, neither of the coefficients are significant. For the work decision regression, the coefficient on the FFW participation dummy is positive and significant at the 5 percent level. The FFW participation dummy interacted with the boy dummy is negative and significant. Results suggest that older girls in households that received FFW following the 2002 drought were 19 percent more likely to not be enrolled in school because of work related responsibilities than children in households that did not receive FFW. Older boys were 3.3 percent more likely to work and less likely to be enrolled in school.

Contrary to the results found in Hoddinott et al. (2010), we find that FFW negatively impacted schooling and work decisions for both boys and girls. Our results for the FFW sample suggest that the work requirements of the FFW program resulted in households increasing the use of child labor for older children. Our results also suggest that girls suffered the most. Although we find that younger children in FFW participating households completed fewer years of schooling than comparison children in households that did not participate in FFW, we can not conclude that the fewer years of schooling was due to work related reasons.

Results for the FD sample are presented in Table 7. For the full sample of children for the work decision regression, the coefficient on the treatment dummy for whether the child resides in a household that received FD is insignificant. The interaction term of the treatment dummy with a dummy for whether the child is a boy is positive but insignificant. For the years of schooling regression, the coefficient on the treatment dummy is negative but insignificant. The coefficient on the interaction term is positive and significant at the 1 percent level. Boys in households that received FD had between .27 ($\beta_1 + \beta_2$) and .38 more years of schooling than comparison boys in households that did not receive FD. For younger children, a similar pattern exists. We find no significant effects in the work decision regressions, while for the years of schooling regression, the treatment coefficient is negative but insignificant. The coefficient on the interaction term is positive and significant at the five percent level. We find no significant effects for the older sample

of children.

Although the FD program did not come with school enrollment requirements, similar to previous findings on conditional cash transfer programs, FD also increased years of schooling. However, we fail to find evidence that girls and older children benefit from FD in the form of increased years of schooling. The only beneficiaries were young boys. If the 2002 drought induced households to withdraw their children from school, then older children would suffer those consequences the most; state dependence in school attendance implies that older children would be less likely to go back to school (de Janvry et al., 2006). Our results for older children are consistent with state dependence in school enrollment.

5 Robustness Checks

This section addresses potential concerns with the data and empirical analysis that may bias the main findings of the paper. First, we check to see if our results could be due to attrition bias. Attrition due only to time-invariant child characteristics does not cause a problem as the difference-in-differences estimator removes all time-invariant factors that may influence the estimation results. However, if attrition is systematically related to selection into treatment and comparison status and is time varying, then there is a possibility that our results are driven by attrition bias. For example, if the 2002 drought induced attrition (for instance by sending children away), and food aid reduced attrition, then our coefficient estimates could be biased. Attrition rates between 1999 and 2004 are slightly lower for children in aid recipient households, regardless of the aid program (refer to the last row in Table 5). For children in FFW and FD households the difference in attrition rates is statistically insignificant after adjustment using the inverse propensity scores as weights, suggesting that attrition bias is not a serious concern. Additionally, there is no indication that the children in aid-receiving households were more or less likely to work or be enrolled in school. That is, differences in the likelihood of working and differences in enrollment rates were statistically insignificant in 1999 between treated and comparison children that were not surveyed in 2004.

Another potential concern is due to the fact that children in the comparison group for the particular aid program could be from recipient households of the other aid program. Including children that were recipients of the other program could bias estimates. Alternatively, excluding children that

are from households receiving the other program from both the treatment and comparison group can also bias results.[21] To deal with this concern, we include an indicator for whether the child's household received the other aid program as an additional control variable in the logit regression. The results were unchanged when including this variable.[22]

We next restrict the samples to exclude children residing in households that received the other aid program. Over 40 percent of aid recipient households received both FFW and FD. Excluding recipients of the other program reduces the sample substantially. Dropping children that received FD from the sample for the FFW analysis decreased the sample from 1,107 children to 462 children. Dropping children that received FFW from the sample for the FD analysis decreased the sample from 1,374 to 393 children. Table 8 presents the results for the FFW restricted sample, excluding all children residing in households that received FD. Because of the small sample size the standard errors are larger. The estimated impact of FFW on the probability of working is of the same sign estimated for the non-restricted sample. We still find a significant and positive effect on the likelihood of older girls working. The effect is larger in magnitude. The estimated impacts of FFW on years of schooling are of the opposite sign for most of the age groups, but we fail to find any significant effects on schooling.

Table 9 presents the results for the FD restricted sample, excluding all children residing in households that participated in the FFW program. The estimated impact of FD on years of schooling are of the same sign and of similar magnitude using the restricted sample as those estimated using the full sample of children, however due to the larger standard errors we fail to find any significant effects. The estimated impact of FD on the probability of working is of the same sign for the main effect using the restricted sample as those estimated using the full sample; however the sign on the interaction term is of the opposite sign for children between the ages of 3 and 12 in 1999. We find a positive and significant effect of the impact of FD on the likelihood of working for older boys.

The results from the analysis where we restrict the sample should be interpreted with caution given that a large share of the sample was removed and, therefore, the restricted sample may not be representative of the children residing in the sample villages. However, the main results, not excluding children from households receiving the other program, should also be interpreted with

[21] The technical appendix to Gilligan and Hoddinott (2007) discusses the potential concerns associated with including or not including recipients of the other program in the sample of treated and comparison groups.

[22] Results not shown.

caution as the estimates identify the effect of a particular aid program when another aid program is also operating in the same village. We are unable to identify the independent impact of each aid program separately.

6 Conclusion

In Ethiopia, more than 80 percent of the population makes their living from agriculture. For the majority of poor agriculture households, they are unable to protect themselves from aggregate shocks such as droughts. The inability to protect themselves from such shocks can result in households choosing coping strategies that may have detrimental consequences for the human capital development of child household members. Safety nets have the potential to impact how households respond to negative shocks. The results presented in this paper suggest that programs that insure against adverse shocks may prevent households from disrupting children's educational attainment. For children from households receiving FD, the program was found to play an important role in mitigating the 2002 drought's adverse effects in terms of schooling, particular for male child household members. In line with the literature showing that households in rural communities perceive boys' education to be more valuable, we find that the program's effect on insuring households is limited to mitigating the drought's adverse effects regarding educational attainment to young male children.

However, for children in households that received FFW, results suggested that they had fewer years of schooling and that older children were less likely to be enrolled in school due to work related decisions. Given that children aged 12-15 in 1999 were 15-18 years old in 2002 (during the drought), the work requirement of the FFW program could have had the unintentional consequence of requiring additional help from older children. Additionally, older children were also eligible to work for the FFW program, which may have resulted in households choosing FFW participation over school participation.

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

	Number of Hhs.	Share of Hhs. Experiencing Drought in 2002	Average Number of Meals Each Day	Sold Livestock to Pay for Food	Share of Hhs. Receiving FFW	Share of Hhs. Receiving FD	Enough Food Aid Provided to This Community	Enough Food Aid Provided to This Household	Food Aid Came on Time
Haresaw	58	81.0	1.8	62.1	56.9	53.4	44.6	25.0	69.1
Geblen	43	90.7	1.9	58.1	74.4	67.4	44.2	11.6	62.8
Dinki	54	75.9	2.3	39.6	59.3	55.6	40.4	17.4	41.3
Yetemen	44	0.0	2.0		4.5	0.0			
Shumsha	82	46.3	1.6	50.6	90.2	78.0	41.6	18.9	66.7
Sirbana Godeti	59	5.1	2.5		1.7	1.7			
Adele Keke	76	92.1	2.0	27.6	36.8	42.1	40.0	14.3	54.5
Korodegaga	83	92.8	2.2	71.1	91.6	79.5	43.1	37.0	70.4
Trirufe Ketchema	73	21.9	2.5		8.2	2.7			
Imdibir	47	0.0	1.7		0.0	0.0			
Aze Deboa	58	72.4	2.2	62.1	79.3	62.1	8.6	7.5	35.8
Adado	62	8.1	2.6		3.2	0.0			
Gara Godo	80	86.3	1.8	50.0	52.5	56.3	16.7	16.4	42.9
Doma	44	72.7	1.8	27.3	52.3	11.4	21.4	15.2	50.0
Debre Berhan	129	14.7	2.8		4.7	0.0			
Total	992	50.2	2.2	42.2	40.6	34.4	24.3	19.8	56.1

Source: Ethiopian Rural Household Survey

Table 2: Difference-in-Differences Estimates of the Impact of the 2002 Drought on Years of Schooling

	Age in 1999		
	(3-15 Years Old)	(3-12 Years Old)	(13-15 Years Old)
Drought	-0.507**	-0.438*	-0.730**
	(0.239)	(0.235)	(0.288)
Drought*Boy	0.269**	0.240**	0.334
	(0.100)	(0.091)	(0.276)
N	2285	1742	543

Significance levels : $*$: 10% $**$: 5% $***$: 1%
Notes: Standard errors in parentheses.

Table 3: Preshock Trends in School Enrollment Rates

Panel A: Preshock Trends, 1995, 1997, and 1999									
	(6-15 Years Old)			(6-12 Years Old)			(13-15 Years Old)		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Drought	-0.051	-0.042	-0.074	-0.045	-0.023	-0.077	-0.066	-0.098	-0.057
	(0.067)	(0.082)	(0.075)	(0.070)	(0.082)	(0.078)	(0.076)	(0.124)	(0.083)
Trend	0.151***	0.154***	0.142***	0.142***	0.156***	0.125***	0.167***	0.138***	0.188***
	(0.015)	(0.017)	(0.017)	(0.016)	(0.019)	(0.019)	(0.018)	(0.027)	(0.024)
Drought*Trend	-0.012	-0.013	-0.006	-0.010	-0.021	0.005	-0.018	0.013	-0.042
	(0.023)	(0.025)	(0.028)	(0.025)	(0.028)	(0.033)	(0.028)	(0.042)	(0.033)
Panel B: Preshock Trends, 1997 and 1999									
	(6-18 Years Old)			(6-12 Years Old)			(13-18 Years Old)		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Drought	-0.107*	-0.103	-0.108	-0.131**	-0.149**	-0.113*	-0.075	-0.044	-0.099
	(0.057)	(0.063)	(0.064)	(0.049)	(0.064)	(0.058)	(0.086)	(0.092)	(0.086)
Trend	0.238***	0.242***	0.234***	0.119**	0.111*	0.127***	0.403***	0.403***	0.400***
	(0.026)	(0.030)	(0.027)	(0.044)	(0.061)	(0.037)	(0.020)	(0.029)	(0.024)
Drought*Trend	0.047	0.058	0.028	0.104*	0.119	0.084	-0.032	-0.013	-0.067
	(0.034)	(0.039)	(0.038)	(0.058)	(0.074)	(0.060)	(0.041)	(0.053)	(0.050)

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

Notes: Panel A restricts the sample to children between the ages of 6 and 15. The sample size is 7,479 children. Panel B includes all children between the ages of 6 and 18. The sample size is 6,446 children. Standard errors in parentheses.

Table 4: Logit Estimates for Participation in FFW or FD

	Mean	FD	FFW		Mean	FD	FFW
Ln real consumption, 1999	4.05	-0.102**	0.015	Household member died, 1999-2002	0.23	0.119*	-0.010
		(0.046)	-0.039			(0.067)	(0.054)
Real value of livestock holdings, 1999	1.38	-0.033	-0.055**	Male household member had serious illness	0.10	0.027	-0.070
		(0.028)	-0.025			(0.101)	(0.094)
Age, 1999	9.67	0.002	-0.022	Female household member had serious illness	0.11	0.028	0.038
		(0.018)	-0.014			(0.108)	(0.073)
Age squared	107.54	0.000	0.001	Household head born in this PA	0.74	-0.016	-0.016
		(0.001)	-0.001			(0.077)	(0.062)
Boy	0.52	-0.047	0.036	Parent important in PA social life	0.69	0.040	0.111**
		(0.029)	-0.023			(0.063)	(0.054)
Household head has any formal education	0.18	-0.063	-0.013	Parent holds official position in Kebele, Tigray	0.02	-0.077	0.105
		(0.086)	-0.072			(0.209)	(0.121)
Mother has any formal education	0.07	0.024	-0.005	Parent holds official position in Kebele, Amhara	0.03	-0.107	-0.094
		(0.121)	-0.095			(0.157)	(0.212)
Ln of household head age	3.83	-0.021	-0.065	Parent holds official position in Kebele, Oromia	0.03	0.180	0.044
		(0.119)	-0.087			(0.110)	(0.123)
Household head is female	0.24	-0.041	-0.153	Parent holds official position in Kebele, SNNPR	0.05	0.216**	-0.007
		(0.090)	-0.096			(0.099)	(0.085)
Household head primary job is farmer	0.81	-0.024	-0.042	Number of iddir household belonged to	0.84	0.002	-0.079*
		(0.091)	-0.077			(0.055)	(0.041)
Household members weak/sick/young/old	0.04	0.198*	-0.383***	Number of people that will help in time of need	9.00	-0.005	-0.002
		(0.118)	-0.117			(0.004)	(0.001)
Ln household size	1.87	-0.036	0.062	Household met at least one targeting criterion, FD	0.85	-0.066	
		(0.076)	-0.063			(0.078)	
Dependency ratio	1.52	0.014	-0.022	Household met at least one targeting criterion, FFW	-2.33		0.115
		(0.026)	-0.02				(0.074)
Land area owned (hectares)	1.24	-0.047	-0.022	Public works wage*Number of adults	-1.54		-0.008*
		(0.107)	-0.087				(0.005)
Land area owned squared	2.93	0.004	0.02	Public works wage*Number of children	-0.01		-0.014***
		(0.018)	-0.019				(0.004)
Household experienced drought	0.78	-0.018	0.048	Public works wage*Number of elderly	0.42		0.014*
		(0.070)	-0.06				(0.007)
Observations						1,478	1,478

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 5: Characteristics of Sampled Households: Selection Variables

	Free Distribution Sample				Food-For-Work Sample			
	Levels		Differences		Levels		Differences	
Selection Variables	Treated	Comparison	Unadjusted	Adjusted	Treated	Comparison	Unadjusted	Adjusted
Ln real consumption, 1999	4.00	4.08	-0.08**	-0.06	4.06	3.98	0.08*	-0.07
Real value of livestock holdings, 1999	1.28	1.51	-0.23***	-0.07	1.40	1.36	0.04	0.03
Age, 1999	9.74	9.58	0.16	-0.20	9.72	9.57	0.15	0.05
Age squared	108.86	106.23	2.64	-3.83	108.77	105.58	3.19	0.48
Boy	0.51	0.53	-0.01	-0.02	0.53	0.49	0.04	-0.01
Household head has any formal education	0.17	0.20	-0.03	0.01	0.20	0.17	0.03	0.02
Mother has any formal education	0.08	0.07	0.00	0.00	0.09	0.06	0.03**	0.01
Ln of household head age	3.83	3.82	0.01	0.00	3.83	3.83	-0.00	0.02
Household head is female	0.26	0.23	0.03	0.00	0.23	0.27	-0.04*	-0.02
Household head primary job is farmer	0.80	0.83	-0.03	0.00	0.82	0.79	0.03	0.03
Household members weak/sick/young/old	0.05	0.03	0.02**	0.00	0.02	0.08	-0.06***	0.00
Ln household size	1.85	1.91	-0.06***	0.01	1.89	1.86	0.03	0.02
Dependency ratio	1.53	1.49	0.03	0.04	1.45	1.63	-0.18***	0.12
Land area owned (hectares)	1.35	1.14	0.21***	-0.13	1.43	0.91	0.52***	0.07
Land area owned squared	3.49	2.30	1.19***	-0.74	3.86	1.23	2.63***	0.30
Household experienced drought	0.80	0.79	0.01	0.00	0.80	0.79	0.02	0.00
Household member died, 1999-2002	0.26	0.21	0.05**	-0.02	0.23	0.25	-0.02	0.01
Male household member had serious illness	0.11	0.09	0.02	0.01	0.09	0.13	-0.04**	-0.03
Female household member had serious illness	0.12	0.12	-0.00	-0.01	0.11	0.12	-0.01	-0.01
Household head born in this PA	0.75	0.73	0.02	0.03	0.73	0.77	-0.04*	0.02
Parent important in PA social life	0.70	0.70	0.00	0.00	0.73	0.63	0.10***	0.00
Parent holds official position in Kebele, Tigray	0.01	0.02	-0.00	0.00	0.02	0.01	0.01	-0.01
Parent holds official position in Kebele, Amhara	0.03	0.02	0.01	0.01	0.03	0.02	0.01	-0.04
Parent holds official position in Kebele, Oromia	0.05	0.02	0.03***	0.02	0.04	0.02	0.02***	0.00
Parent holds official position in Kebele, SNNPR	0.06	0.04	0.01	0.00	0.05	0.05	-0.00	0.03
Number of iddir household belonged to	0.89	0.79	0.09**	0.02	0.85	0.85	-0.00	0.00
Number of people that will help in time of need	7.10	11.39	-4.29***	0.31	8.40	10.24	-1.84	0.66
Household met at least one targeting criterion, FD	0.42	0.45	-0.04	0.03				
Household met at least one targeting criterion, FFW					0.89	0.79	0.10***	0.01
Public works wage*Number of adults					-4.33	0.98	-5.31***	-0.57
Public works wage*Number of children					-5.16	4.65	-9.82***	-0.58
Public works wage*Number of elderly					-0.05	0.06	-0.11	0.02
Attrition rate	0.04	0.09	-0.05***	-0.05	0.06	0.08	-0.03*	-0.00

Notes: Columns 4 and 8 report estimated differences between the treated and control samples after adjusting using inverse propensity score weighting.

Table 6: Estimates of the Impact of Food For Work on Schooling and Work Decisions

	Children Age 3-15 in 1999		**Children Age 3-12 in 1999**		**Children Age 13-15 in 1999**	
	Outcome Variables					
	Work	Yrs of Schooling	Work	Yrs of Schooling	Work	Yrs of Schooling
Difference in average outcomes, ATT	0.014	-0.489**	-0.044	-0.535**	0.190*	-0.330
	(0.056)	(0.217)	(0.062)	(0.227)	(0.105)	(0.477)
Interaction Term, Boy	-0.009	0.244	0.036	0.262*	-0.157*	0.203
	(0.041)	(0.162)	(0.044)	(0.156)	(0.093)	(0.370)

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

Notes: The sample consists of 636 children in aid-receiving households and 471 comparison children. Bootstrapped standard errors in parentheses using 1000 replications of the sample.

Table 7: Estimates of the Impact of Free Distribution on Schooling and Work Decisions

	Children Age 3-15 in 1999		**Children Age 3-12 in 1999**		**Children Age 13-15 in 1999**	
	Outcome Variables					
	Work	Yrs of Schooling	Work	Yrs of Schooling	Work	Yrs of Schooling
Difference in average outcomes, ATT	0.016	-0.108	0.017	-0.169	-0.012	0.132
	(0.053)	(0.139)	(0.057)	(0.149)	(0.089)	(0.359)
Interaction Term, Boy	0.025	0.375***	0.026	0.357**	0.048	0.351
	(0.037)	(0.141)	(0.041)	(0.147)	(0.088)	(0.316)

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

Notes: The sample consists of 747 children in aid-receiving households and 627 comparison children. Bootstrapped standard errors in parentheses using 1000 replications of the sample.

Table 8: Estimates of the Impact of Food For Work on Schooling and Work Decisions - Restricted Sample

	Children Age 3-15 in 1999		**Children Age 3-12 in 1999**		**Children Age 13-15 in 1999**	
	Outcome Variables					
	Work	Yrs of Schooling	Work	Yrs of Schooling	Work	Yrs of Schooling
Difference in average outcomes, ATT	0.108	0.052	-0.055	0.021	0.555***	0.262
	(0.102)	(0.400)	(0.115)	(0.518)	(0.176)	(0.695)
Interaction Term, Boy	-0.069	-0.212	-0.036	-0.125	-0.115	-0.373
	(0.097)	(0.271)	(0.100)	(0.264)	(0.186)	(0.657)

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

Notes: The Sample excludes children living households that received free distribution aid. The sample consists of 213 children in aid receiving households and 249 comparison children. Bootstrapped standard errors in parentheses using 1000 replications of the sample.

Table 9: Estimates of the Impact of Free Distribution on Schooling and Work Decisions - Restricted Sample

	Children Age 3-15 in 1999		**Children Age 3-12 in 1999**		**Children Age 13-15 in 1999**	
	Outcome Variables					
	Work	Yrs of Schooling	Work	Yrs of Schooling	Work	Yrs of Schooling
Difference in average outcomes, ATT	0.052	-0.021	0.081	-0.166	-0.315	0.391
	(0.138)	(0.511)	(0.147)	(0.552)	(0.229)	(1.057)
Interaction Term, Boy	-0.007	0.500	-0.084	0.316	0.446*	0.866
	(0.108)	(0.322)	(0.137)	(0.421)	(0.251)	(0.730)

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

Notes: The Sample excludes children living households that participated in the food-for-work program. The sample consists of 148 children in aid receiving households and 245 comparison children. Bootstrapped standard errors in parentheses using 1000 replications of the sample.

A Appendix

Table A.1: Variables Used in the Analysis

ln real consumption, 1999
Real value of livestock holdings in thousands of Ethiopian Birr, 1999
Land area owned (hectares), 1999
Land area owned squared, 1999
Age, 1999
Age Squared
Boy
Household head has any formal education, 1999
Mother has any formal education, 1999
Ln Age of household head, 1999
Household head is female, 1999
Household Head's Primary Job is a Farmer
All Household Members Were Too Weak/Sick/Young/Old to Work on a Public Works Project
Ln Household Size, 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
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
Household Head Born in This Peasant Association
Parent important in PA Social Life
Parent Holds Official Position in Kebele, 1999
Number of Iddir Household Belonged To, 1999
Number of People That Will Help in Time of Need, 1999
Household Met At Least One Community Targeting Criterion for FD
Household Met At Least One Community Targeting Criterion for FFW
Public Works Wage

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