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Social Protection for all ages? Impacts of Ethiopia's Productive Safety Net Program on child nutrition.

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Social Protection for all ages? Impacts of Ethiopia's Productive Safety Net Program on child nutrition

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Abstract

We investigate the impact of a large-scale social protection scheme, the Productive Safety Net Program (PSNP) in Ethiopia, on child nutritional outcomes. Children living in households that receive cash transfers should experience improved child nutrition. However, in the case of the PSNP, which for the majority of participants is a public works program, there are several potential threats to finding effects: first, without conditionality on child inputs, increased household income may not be translated into improved child nutrition. Second, the work requirement may impact on parental time, child time use and calories burned. Third, if there is a critical period for child human capital investment that closes before the age of 5 then children above this age may not see any improvement in medium-term nutritional outcomes, measured here as height-for-age. Using a cohort study that collected data both pre-and post-program implementation in 2002, 2006 and 2009, we exploit several novel aspects of the survey design to find estimates that can deal with non-random program placement. We present both matching and difference-in-differences estimates for the index children, as well as sibling-differences. Our estimates show an important positive medium-term nutritional impact of the program for children aged 5-15 that are comparable in size to Conditional Cash Transfer program impacts for much younger children. We show indicative evidence that the program impact on improved nutrition is associated with improved food security and reduced child working hours. Our robustness checks restrict the comparison group, by including only households who were shortlisted, but never received PSNP, and also exclude those who never received aid, thus identifying impact based on timing alone. We cannot rule out that the nutritional impact of the program is the same for younger and older children.

Keywords: Ethiopia, nutrition, social protection, children.

Highlights:

1. An Ethiopian public works program improves child nutrition in the medium term
2. We do not find significant differences in impact between children aged 5, 8, and 15
3. Impacts associated with improved household food security and fall in child labor
4. Height-for-age captures medium-term program impact better than consumption

1 Introduction

Can social protection schemes with a work requirement act as a safety-net for children, and for what age groups? The rationale for investment in child health and nutrition on a national level hinges on its link with economic growth as well as equity. Evidence has been growing recently that child nutritional investments (and conversely, shocks to these investments) can have a significant impact on human capital attainments and achievements as adults (Almond and Currie, 2011). Although the consequences of malnutrition are well appreciated worldwide, attention is only beginning to be given to the extent to which social protection has the potential to impact child health and nutritional outcomes. Thus far, most studies quantifying impacts on children have been of Conditional Cash Transfer programs (CCTs) in Latin America (reviewed by Fiszbein et al. (2009)). CCTs explicitly incorporate requirements that participants invest in child human capital (preschool or nursery enrolment, vaccinations etc).

However, Ruel et al. (2013) review 77 CCTs and Unconditional Cash Transfers (UCTs) and overall, impacts are disappointing: "an effect size that is neither statistically significant nor biologically meaningful (p542). Alderman (2014) further notes "The potential of transfer programs to be nutrition sensitive remains largely untapped" (page v). Whether social protection is a positive influence on the health of children is an especially pertinent question in Ethiopia, where malnutrition is the underlying cause of about 57% of child deaths (Save the Children UK, 2009). In 2005, the Government of Ethiopia introduced the Productive Safety Net Program (PSNP), the second largest social protection scheme in Africa, a program comprising 80% public works (food-for-work or cash-for-work) and 20% unconditional transfers for those unable to work, covering almost eight million rural citizens. The annual donor-financed budget is approximately US$347 million (roughly 1.2% of Ethiopia's GDP).

The evidence on whether the PSNP has been effective at improving household level measures of food security and consumption status has been somewhat mixed - whilst Gilligan et al. (2009) find little evidence of improvements in consumption amongst targeted households, using a longer evaluation period Berhane et al. (2014) do find improvements in food security for households that received PSNP for more than four years.

A priori we expect participation in the PSNP to improve nutritional status of children due to the increase in household income (Glewwe and Miguel, 2007; Christiaensen and Alderman, 2004). Social protection programs with an adult work requirement, however, may be ineffective in reaching children within the household because of intrahousehold dynamics or other unintended consequences of programs such as child labor demands (Woldehanna, 2010). Therefore, whether PSNP improved child health given the work requirement and the multiple risks faced by households is an empirical question worthy of further study.

We focus on the effect of the PSNP on individual child nutritional status, as measured by anthropometric height-for-age z-scores, a commonly used indicator of the stock of child health. The PSNP was introduced in 2005 and is non-randomly targeted towards food-insecure households, making distinguishing a mere correlation from causal effect more difficult.

The paper offers several contributions: First, the results contribute to the impact evaluation of social protection through a new lens of child nutrition. Evaluation of PSNP impacts is of general interest since the program is implemented at scale in a very low income country in Africa. Second, our dataset, the Young Lives Ethiopia survey, offers rich child and household level panel information for two cohorts (born 1994 and 2001), including siblings, which allows us to evaluate program impacts of different age groups from 5-15. Third, we use a battery of robustness checks that are useful for others attempting program evaluation in a non-random setting. One of these is a sibling-difference specification, with siblings measured at the same age point pre- and post-treatment. Results show that the program provides a significant boost to child growth, even when the child is exposed to the program beyond the first 1000 days.

The article is structured as follows: the next section gives a brief outline of the program background and literature. We then outline the conceptual framework, estimation strategy and data. We present the empirical results, followed by discussion, and conclusion, that the program does have positive impacts on nutrition.

2 Background

2.1 PSNP and food aid in Ethiopia

Food security and nutrition are long-standing issues in Ethiopia, though there have been improvements overall in the past 15 years. According to a recent Demographic and Health Survey (DHS, 2012), stunting prevalence decreased from 58 percent to 51 percent between 2000 and 2005 and to 44 percent in 2011, still higher than average for Africa (38.2% in 2010), or developing countries overall (29.2%, De Onis et al. (2012)).

The PSNP was introduced with an objective 'to provide transfers to the food insecure population' as well as to 'bridge the food gap' (Ministry of Agriculture and Rural Development, 2004). It operates as a safety net, whereby the public works (also known as cash/food-for-work, or workfare) program operates seasonally, but predictably (and similarly for the direct support, or unconditional cash transfer). In 2009, the year of our study, the PSNP supported 7.6 million people (roughly 10% of the national population) in 8 of the country's 10 regions. The program had expanded from 4.5 million beneficiaries in 2005 and by 2014 was estimated at 10 million (Holmemo, 2014). The PSNP, centrally co-ordinated by Government, represents a departure from previous social protection schemes, which were mainly delivered as emergency food-for-work (FFW) programs on an ad-hoc basis by multiple actors. The PSNP was designed to provide predictable support for selected households over several years. Wiseman et al. (2010)

provide a comprehensive review of lessons learned 2005-2009, which we summarise. The program has a principle paying in cash rather than food, with the ratio of cash/food in 2008 at 60/40, and the daily wage rate was 8 birr in 2008 ($0.56). Average annual transfers for both direct support and the public works beneficiaries in 2009 were $137, which compared with a per capita income for Ethiopia of approx $550. Unlike other public works schemes (e.g. India, South Africa), targeting is not based on self selection. The PSNP combines geographic and community-based targeting to identify chronically food-insecure households. During the period our study covers, the PSNP was in an expansion phase. Whilst "graduation" of food-secure households was an aim of the program, between 2007 and 2009, only 280,000 individuals graduated. A further evaluation found that targeting in 2008 followed the Government's official guidelines and was well-targeted against an international comparison (Coll-Black et al., 2011).

Gilligan et al. (2009) examined the effect of PSNP on the food gap (number of months the household reports having difficulty meeting food needs), calorie intake, and number of meals consumed by children in the hungry season. The authors found some evidence of impact for public works participation on calorie acquisition. Berhane et al. (2011) showed statistically significant impacts of the PSNP on households' food security and consumption status, and Berhane et al. (2014) found a significant impact of PSNP on food security as years of participation increased.

Other evidence suggests that 62% of the households enrolled in the PSNP avoided selling assets in situations of food shortages, and 36% avoided using savings to buy food (Alderman and Yemtsov, 2012). Beneficiaries were significantly more likely to consume the required 1,800 calories per day than non-beneficiaries, and PSNP has likely also helped households protect their assets, avoid low pay labor and sacrifice school fees or health costs to deal with shocks (Save the Children UK, 2008).

However, studies suggest that PSNP has produced both intended and unintended outcomes for children, in particular with regard to their time use. The minimum age for public works participation is 18 years and according to Sharp et al. (2006) approximately 8% of workers were under 18. Tafere and Woldehanna (2012) investigate impact of PSNP on the older cohort of the Young Lives survey (15 year olds) and show that the program increased time spent on work, both paid and unpaid. The authors note that the public works requirement, including its timetable, leads households to supplement adult labor with child labor. Camfield (2014) finds considerable evidence of girls working directly in the PSNP program, or increasing their household chores in response to caregivers' participation. Hoddinott et al. (2010) find that boys aged 6-16 spend less time on agricultural labor, and younger boys aged 6-10 as well as girls aged 11-16 spend less time on tasks within the household. However, girls younger than 11 spend more time on tasks within the household as well as face a reduction in school enrolment. Outes (2015) shows that the time use effects are significant for children.

2.2 Literature review

We provide a necessarily brief review of the theoretical and empirical literature on the impact of nutritional inputs experienced at different stages of the child's development as pertinent to our findings. Almond and Currie (2011) review this in detail, and also summarise the empirical evidence to date. The seminal work of Heckman and co-authors (Cunha and Heckman, 2007) introduced the concept of "critical period programming", whereby one (earlier) period of growth may be more important than others, especially if later period investments build on earlier period investments. The authors consider "sensitive periods" as those which have a higher impact of inputs on human capital outcomes (the rate of return to a similar level of investment is higher), and "critical periods" as those where a deficit in inputs cannot be remediated at all.

The nutrition literature also has a clear focus on the importance of the first 1000 days of life (from conception to age 24 months) (Victora et al., 2010). In terms of catchup growth, the term has variously meant whether children who were stunted in an early childhood period can cross the stunting 'line' (achieve a height-for-age z-score (HAZ) of greater than -2) in later periods, or simply whether the coefficient in a regression of later nutrition (usually measured as height-for-age) on earlier nutrition has a coefficient that is relatively high (close to one). Outes and Porter (2013) show that the relationship between HAZ at age 1 and 5 in Ethiopia is modified by a wealth-gradient - children from better off backgrounds appear to catch up more quickly. However there is very strong persistence between HAZ at age 5 and HAZ at age 8. Lundeen et al. (2014) argue that there is substantial variation in growth over the age of one year, and that nutritional interventions may be worthwhile for those above the so-called critical period. Schott et al. (2013) document nutritional catchup between the ages of 1 and 8 and its correlates. Prentice et al. (2013) challenge the view that interventions cannot have an impact on nutritional status beyond the 1000 day cutoff, and cite evidence of nutritional catch-up during mid and later childhood (including puberty), even in the absence of interventions. Hirvonen (2014) critically reviews this literature and provides evidence of catch up for older children in Tanzania.

Several studies suggest that there exists biological potential for catch-up growth in response to interventions. Leroy et al. (2009) review the evidence on the impact of Conditional Cash Transfers (CCT) on child nutrition and do find significant improvements in child anthropometrics, though less evidence on micronutrient intake. They also note that women's time use in particular may be altered by programs. CCTs through other aspects of the program design may also impact nutrition through the channel of improved health. As noted above Alderman (2014) and Ruel et al. (2013) discuss the potential for social safety nets to be designed in such a way as to improve their nutritional impact.

A handful of studies have attempted to find heterogeneous program impacts for different age groups. Barham et al. (2013) find evidence of catchup growth in boys whose families received transfers in a CCT program in Nicaragua before the age of 2 compared to those receiving it afterwards (though still below the age of five). Behrman and Hoddinott (2005) study under 5's and find the greatest impact of Progresa CCT on nutritional outcomes of children aged 24-36 months, with no impact for those greater than 36 months. Singh et al. (2014) however find that the midday meal program in India did improve nutrition of school children between 5 and 8 years old.

3 Methods

3.1 Conceptual Framework

We build on the approach of Behrman and Hoddinott (2005) to derive a reduced form health demand function, with child health entering directly into the welfare function of the household, reflecting its intrinsic value. The household maximizes its utility subject to two constraints: a health production function and a budget constraint. Recent literature on human capital development (Cunha and Heckman, 2007; Almond and Currie, 2011) extends the model to include influence of prior nutritional investments, which may vary over time. They incorporate the concept of "critical periods" that have higher returns to investment than others, and "dynamic complementarity", whereby the return to current investments may be increasing in past investments (Cunha and Heckman, 2007). Our reduced form conditional nutritional demand function is thus specified as:

$$H_{ikt} = h_t \left(H_{ikt-1}, C_{ikt}, M_{kt}, W_{kt}, A_{kt}, P_{vt}, Z_{vt} \right) \tag{1}$$

Where H_{ikt}, the nutritional status of child i in household k at time t is a function of: C, is a vector of child characteristics such as sex, age, inherited healthiness/growth potential; M are caregiver characteristics; W is household wealth; A represent other characteristics of the household such as composition and livelihood; P are prices; Z includes community characteristics such as availability of social support programs, environmental characteristics.

PSNP enters equation 1 as an "exogenous" influence on nutrition. However, several econometric issues are pertinent. We expand on these empirical concerns and our proposed solutions below.

3.2 Empirical strategy

Our main empirical concern when estimating the impact of the PSNP on child nutrition is that of non-random program placement. Our starting point is a difference-in-differences estimator (DID) at the child level. DID estimates can lead to a substantial reduction in selection bias in estimated program impact and give us an effect of the "treatment on the treated"(ATT) (Angrist and Pischke, 2009), providing that we can offer both a convincing comparison group and allay the concern that growth in the treatment and comparison groups would have been different even in the absence of the program.

We pursue a two-pronged strategy to check the robustness of the results to such concerns: first, we add a vector of child controls to control for as much heterogeneity between children and their families as possible, including access to aid in previous rounds, and the lag of child nutrition. Second, we progressively restrict the sample to narrow the group of children to a more convincing comparison set, presenting results that are consistent across the use of five different comparison groups overall. In order of restrictiveness these are: 1) all rural households (all), 2) households with a similar probability of being treated (matched sample), 3) matched households with a parallel trend 2002-2006 (matched-parallel trend), 4) households shortlisted that did not receive (shortlisted), and 5) siblings within households who had ever received aid (ever aid).

Child difference in difference

The empirical analog to equation 1 is expressed in levels as:

$$H_{ikt} = \alpha + \beta_1 H_{ikt-1} + \beta_2 PSNP_{kt} + \beta_3 K_{kt} + \beta_4 Z_{vt} + \delta_1 \mu_i + \delta_2 \nu_k + \delta_3 \lambda_v + \epsilon_{it} \tag{2}$$

The lagged health input enters directly. K and Z are vectors of time-varying observable household and community characteristics. μ_i, ν_k and λ_v are child, household and community (unobservable) fixed effects respectively. All of these are by definition time invariant. The treatment variable is time-varying and binary, equal to one if the household has participated in PSNP at any point between 2006 and 2009 and zero otherwise. We identify the impact of the program on nutrition outcomes in time t, three years after the program was introduced relative to time $t-1$ which is defined as pre-program. Gilligan et al. (2009) showed that transfers were delayed during the first year of implementation of the PSNP (2005/6), and impact was not experienced in Round 2 (Woldehanna, 2010), motivating us to use 2006 as our baseline. Our working empirical specification is the first difference of equation 2:

$$\Delta H_{ikt} = \alpha + \beta_1 \Delta H_{ikt-1} + \beta_2 \Delta PSNP_{kt} + \beta_3 \Delta K_{kt} + \beta_4 \Delta Z_{vt} + \Delta \epsilon_{it} \tag{3}$$

In which, $\Delta H_{ikt-1} = H_{ikt-1} - H_{ikt-2}$. We test whether the lagged difference of the dependent variable is significant, to avoid dynamic misspecification (Bond, 2002). A key econometric concern however is that ΔH_{it-1} is endogenous, being determined possibly by unobservable child-level heterogeneity and parental preferences. This concern can be addressed by using H_{it-2} as instrumental variable (Bond, 2002). Comparing the results of both the contemporaneous and the dynamic model (table A7, Appendix) allows us to assess whether including lagged height growth changes our estimate of the program impact, but we find that the estimates of the program impact are not significantly different between specifications.

Sibling difference

We use information on younger siblings to further identify the program impact. The advantage of this, based on the survey design are twofold: first we can look at the impact on children younger than our youngest index child. Second, it allows us to control for age-specific factors that determine child nutrition, and also eliminate household (and community) unobservable fixed-effects.

The nutrition status of sibling s in the same household k can be represented by an analog to equation 2, with an s subscript. Note that the way the survey is designed, t for the younger sibling corresponds to $t-1$ for the index child, so we measure the sibling in 2009, and the index child in 2006 (pre-PSNP). We can remove the unobservable household and community (though not individual child) characteristics by differencing (Griliches, 1979):

$$\Delta H_{i-s,k} = \alpha + \beta_2 PSNP_{kt} + \beta_3 \Delta K_{kt} + \beta_4 \Delta Z_{vt} + \mu_i - \mu_s + \Delta\epsilon_{i-s,kt} \tag{4}$$

Note that the PSNP identification will depend here on changes within households over time (discussed below).

Restricting the comparison group

The issue of non-random program placement is particularly pertinent for the PSNP, since one of the targeting criteria of the new program was that households were a) food insecure and b) had been receiving food aid in the past. In the child difference-in-difference specifications, we already control for those who were receiving food aid or cash/food for work in the 2006 survey round (60% of 2009 recipients). We deal with this issue further by restricting the comparison group using matching, shortlisting and most restrictively, excluding those who never received aid.

Matched sample: We use propensity score matching to carefully construct a comparison group of non-participants that is as similar as possible to the program beneficiaries. The matching model included both indicators of need (household assets, size, gender of head, shocks) as well as social connection (a variable about whether the household had an influential friend or relative in the community in 2006). We match children on pre-program household characteristics, and within the common support, we construct the comparison group for whom the parallel trends assumption holds, by dropping the top and bottom 20% of households according to the propensity score.

Shortlisted sample: We exploit the shortlisting process of the PSNP. Many communities had public meetings to discuss the shortlist for potential beneficiaries of PSNP, which was then approved by the next level of administration. Due to budget constraints, some households were shortlisted, but did not receive the program. We might consider this group of (122) households to be the ideal comparison group for estimating program impact. The YL survey included a question "Were you shortlisted for PSNP participation", and thus we use this question to define a more restrictive, but arguably more comparable sample, since it is likely these households would receive the program at some point in the near future, considering the significant expansion of PSNP noted earlier from 2009 to present. We could not reject that this sample had the same level of wealth as the treatment group in 2006, nor parallel trends in height growth 2002-2006.

Ever-aid sample: For the sibling-difference model we can use our most conservative comparison group. We further restrict the sample to those who *ever* received aid in any round, which allays the concern that there are unobservable differences between households (children) that never received aid. Thus we estimate program impact based on timing of receipt only. We create dummy variables for "household received PSNP 2009 but not 2006", and similarly for those receiving aid in 2006 only. The sibling is measured in 2009, thus we can extract the program impact in 2009 from the coefficient on the "2009 only" dummy. By definition, the PSNP could not affect the index child age 5, measured before 2009, giving a clean identification, albeit with a smaller sample.

Thus, the approach taken employs several different specifications to correct for potential confounders to estimation, and several different samples to see if we can find an estimator-robust conclusion of impact of PSNP on child health, despite non-random program placement. Table 1 summarizes the full set of approaches and samples used.

Remaining concerns

We note several potential threats to finding an impact that remain. Increased income from PSNP may cause households to reduce efforts in other areas, leading to no net income gain. Households may decide to allocate food away from the child due to PSNP (such as adult work requirement means grown ups are hungrier). Increase in working time of parents could reduce their other inputs into child nutrition. PSNP could also cause an increase in demand for work in children (childcare, housework, substitution on the family enterprise, or directly in PSNP) and these substituted activities incur higher calorific inputs. We examine these channels further below.

4 Data

We use Young Lives Ethiopia, a longitudinal cohort study conducted over three waves (2002, 2007 and 2009-10). The younger cohort (1999 children) were aged 6 to 18 months in 2002, and the older cohort (1000 children) were aged 7-8 years. Overall attrition for the sample was just 5.7% over the eight-year period. As noted above, malnutrition in Ethiopia remains a widespread problem, and in our sample, just under half of the children were stunted in 2002. The proportion stunted fell to 25% in 2009, with just over 7% severely stunted.

The sample comprises data from 20 sentinel sites chosen in five regions. Households within sites were chosen randomly amongst those that had children born in the stipulated year. Importantly, PSNP was operating in 15 of these sites with 530 out of the 1886 households (29.1% of the sample) active beneficiaries of the program.

In all rounds we have information on child health, anthropometrics and individual characteristics; caregiver background, livelihood, household composition, socio-economic status, social capital, shocks; and community characteristics. Households were asked whether they were enrolled in PSNP and in which years, the value of payments received in the past 12 months, if they had been shortlisted or if they have graduated from the program.

Figure 1 shows the timing of the three survey rounds as well as the introduction of the PSNP and subsequent rollout.

The outcome variable in our analysis is 'Height-for-age' z-score (HAZ), a well established measure of individual health status especially among children (Martorell et al., 1994) which acts as proxy for child health status. Z-scores are derived by comparing the child's height with that of a reference group of well nourished children known as the WHO Reference 2006 (De Onis et al., 2006; World Health Organisation, 2006). We also report impact on the weight-for-age z-scores (WAZ), a commonly used indicator of wasting, which is a more short-term measure of nutritional status.

Table 2 reports descriptive statistics comparing children from households participating in the PSNP (treatment group) to non-beneficiaries (comparison group). The concern however is that households receiving transfers from PSNP are possibly different from those not enrolled in PSNP for reasons that also affect the health of children in these households, which we deal with below.

5 Results

We present the results from the difference in differences (DID) for the index child, and the siblings. Our results show consistent impacts on height for age z-scores (HAZ) across methods.

As can be seen from table 2, the difference in means of treated and untreated children's HAZ levels in 2009 (post-treatment) are not significantly different. The simple DID specification with basic controls for age and gender of child is presented in column (1) of table 3 showing a significant positive 0.13 s.d. impact of the PSNP on the treatment group. In columns 2-4 we add full controls of household and child characteristics, drought and other shocks, and prices as well as lagged aid receipts in 2006, lagged child growth between 2002 and 2006 (instrumented with round 1 HAZ) as the full specification of equation 3. Robustness checks building up this specification are presented in Appendix table A7. Column 3 restricts the sample to the common support, and column 4 the shortlisted sample (as outlined above).

Sibling-difference specification: Table A1 in the Supplementary Appendix gives descriptive information for 661 sibling pairs, and tests to reject sample selection bias (table A2). The estimating equation 4 was presented in the above section, testing the hypothesis that PSNP has zero impact on nutrition of younger siblings at age 5 in 2009 (post-treatment), compared with their older sibling in 2006 (pre-treatment).

Note here that since we exploit the differential timing, we specify the PSNP variable as 2009 only, or aid in 2006 only (since households receiving aid in both periods will show no within sibling variation).

6 Discussion

The analysis shown above provides evidence of improvements in nutritional outcomes due to PSNP, for children at different ages (from age 3 to 5; age 5 to 8; and age 12 to 15). In table 3, the coefficient on the PSNP variable is robust to the inclusion of progressively more rigorous controls, with a point estimate around .18 standard deviation impact of the PSNP, which is equivalent to approximately 2.4cm for 8 year old boys. This estimate is stable across the different specifications, and in particular does not change when the lagged value of height growth is added (see table A7, Appendix for full results). This finding motivated our contemporaneous sibling DID estimates, allowing us to more precisely control for unobservable household characteristics whilst exploiting the timing of the survey rounds and the PSNP introduction.

The sibling difference results show improvement in sibling HAZ in 2009 at approx age 5 *after* the household received PSNP transfers (in 2009), compared with outcome of the index children in 2006 (pre-PSNP) who received no aid prior to age 5. The comparison group is households who either never received any aid, or received aid in both rounds. Results in table 4 showed the estimates are significant and substantially larger (approx 0.7*** s.d.) than the child DID estimates. Note the lower standard deviation of height at this age translates to just under 3.5cm, not dissimilar in magnitude to the child results using shortlisted children as the comparison group. In column (2) we add a control for households who receive PSNP or aid in both rounds, which does not affect results.

Sibling difference estimates include control variables that capture changes in the household and communities that might have affected health investments across siblings. Sibling difference in nutritional outcomes also depend on birth order, fertility characteristics and maternal health during both the pregnancies. To the extent that these are uncorrelated with program placement, they should not affect the results, but a caveat is that parents might allocate subsequent investments differently across siblings based on unobserved differences, for instance, a weaker child may attract more attention and inputs from parents for survival, or the converse. Future rounds of the survey will be able to provide more child-specific details of the younger siblings.

Effect of restricting comparison group

Matched sample (n=956): The results from table 3 column (3), show a slightly higher point estimate (.19, significant at 5%). We also calculated treatment effects using nearest neighbor matching with an exact match specified for gender and cohort. Using propensity score matching, the treatment effect on the treated (ATT) was estimated at 0.12 (significant at 5%), which is slightly lower, but in line with our DID estimates. All matching results are in table A3, Appendix.

Shortlisted sample (n=783) Replicating the DID estimate showed a higher (.44 sd) impact of PSNP on HAZ scores (table 3, column 4). The matching estimate of ATT also suggests higher (0.288 s.d.) impact (both significant at 1%). Despite the smaller sample size we tentatively conclude that this higher impact with the very strict comparison group shows that the DID estimates on the full sample may be an underestimate.

Ever aid sample (n=406): Comparing siblings within household removes unobserved household and community factors that might bias the estimate of PSNP impact. However, we still may harbor a concern that these comparison and treatment groups are imperfect, if selection into the PSNP or any aid program is based on unobservable household characteristics that interact with nutritional investments/outcomes. We thus in column (3) of table 4 kept only households who ever received aid. Despite the sample size dropping to just over 400 sibling pairs, the treatment effect of 2009 PSNP is still significant, and the impact point estimate is similar in magnitude (0.7sd, or around 3.5cm). Results for the restricted samples in full are provided in the Supplemental Appendix.

Further issues

We ran further robustness checks including trimming the height-for-age z-scores for outliers by excluding HAZ outside of the 95th (upper and lower) percentiles which did not change the results. Overall, all of the estimates are consistent with the hypothesis that PSNP targets vulnerable/food-insecure households, reliable income or direct food supplies improve the food situation of the treatment group and benefits also accrue to children.

In our child-level analysis we cannot rule out the equality of the treatment effect for the two cohorts age 8, and 15, by including an interaction term for cohort, and whilst this does not provide comprehensive evidence of an equal treatment effect at all ages, it suggests that the window of opportunity for nutritional interventions having a positive impact on children may be significantly higher than the 1000 day window (Lundeen et al., 2014). The sibling comparison did show higher results for the younger age group, but due to the difference in method we cannot necessarily infer that impact is higher. Note we did not find any differential impacts by gender.

Drought affected many households in the sample during the study period, and a dummy for the household experiencing drought shocks is included in all specifications. We find that the impact of drought on PSNP participants is still high (not significantly lower than for non-participants), however this does not eliminate the program effects (a t-test of the joint impact of PSNP and drought being zero is rejected).

We considered differential impacts of the two modalities of the PSNP. Households enrolled in direct support should have experienced only an income effect, however, those enrolled in public works could have both income and substitution effects. We found no significant difference between the two modalities in terms of their impact, though this may be due to the quite small numbers of DS recipients.

Finally, other analysis of PSNP has shown variations in both amount of days worked across regions and payment received. In 2006, households employed in public works received 50 birr per month in Tigray whereas those in Amhara only 30 birr per month (Gilligan et al., 2009). In further robustness checks we included region dummies that did not affect the results. We also systematically dropped each cluster one-by-one to ensure that one cluster is not driving the results.

Association with potential causal factors

We provide some exploratory analysis of factors that may cause improvements in nutrition of the sample children and their correlation with PSNP participation. We expect the PSNP to increase household consumption and food consumption since it provides an income supplement. In addition, households with improved incomes may be able to improve the dietary diversity of their children. We also examine food security and time use.

Our results show some puzzling findings in terms of household food intake: Column (1) of table 5 shows in fact a negative correlation with real food consumption per adult equivalent (8% fall between 2006 and 2009). Dietary diversity also shows no significant impact (column 3). Food security however did significantly improve (column 2). The likely difference in these results is in the survey design. Both consumption and dietary diversity questions were asked with reference to the previous day or week to the survey. In the food security module however, the question was whether households "had worried that they would run out of food" *in the past 12 months*. Finally, we find a significant improvement in weight-for-age z-scores (WAZ, column 4), though the point estimate is smaller than that of HAZ. A full set of WAZ results for index and sibling specification are in the Appendix (table A5, A6) Arguably, WAZ results are a more short-term measure of nutritional status and consumption and dietary diversity are a snapshot of nutrition in the week before the survey, and the day of the survey respectively. In the

Appendix (Section 3; table A4; figure 1) we consider how the timings of the survey and PSNP payments may be driving the somewhat counter-intuitive results on consumption. Finally, in column (5) of table 5 we find a significant reduction in hours worked between 2006 and 2009 for children in the PSNP program. This suggests that the income effect dominates the substitution effect. We re-estimated the results of table 5 for the shortlisted households and found the results to be broadly similar, except that there was no significant difference in food consumption for the PSNP households relative to the shortlisted households.

7 Conclusion

Food insecurity remains a chronic issue in Ethiopia as much of the country's population depends on agriculture for their livelihood. With highly volatile annual rainfall, a rapidly increasing population, diminishing landholdings, and a lack of on-farm technological innovation, child malnutrition remains a pressing issue in the Ethiopian context. This paper examined evidence that the PSNP has had a positive nutritional impact on children of participant households. Due to the issue of non-random program placement (common to many programs), it is a challenge to measure impact reliably. Using several estimators and appropriate samples we found positive (and significant) evidence of the program acting as a safety net for children, cushioning them from nutritional vulnerabilities. Further, we do not see any significant difference in the impact between ages 8, and 12 and 15. The impact is higher for those exposed between the ages of 2 and 5, but this cannot be distinguished from the different estimation strategy used (sibling-differences). We acknowledge that our sample does not contain many children who received the program below the "critical" age of 1000 days from conception, but arguably this makes the results all the more striking.

Not only is the estimate of impact of transfers from the PSNP on health status of children statistically significant (between 0.2 and 0.7 s.d. of HAZ scores), but also economically significant. The difference between the 25th and 50th percentiles of z-scores in 2009 is 0.68 s.d. so we are looking at potentially large effects of up to 3.5cm in height. This is possible because children who have experienced large periods of food insecurity must have large potential for catch-up in response to such programs.

While the link between CCTs and child nutrition are well explored in the Latin America, there is much less evidence of impact of other safety-nets, particularly those with a work requirement, and especially in Africa. Our estimates show impacts that are quite similar in magnitude to effects of CCTs on the nutrition of younger children (Leroy et al., 2009), despite the PSNP being mainly a cash/food-for-work program. Unlike CCTs, it has no requirements that participants invest in child nutrition or other human capital. More child-sensitive social protection could likely increase the impact of PSNP further. Finally, we note from our results that using short-term outcome measures such as food consumption diaries or dietary diversity scores may mask the longer-term protective impact of programs that alleviate seasonal hunger.

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8 Tables and Figures

Figure 1: Timeline of PSNP introduction and survey implementation

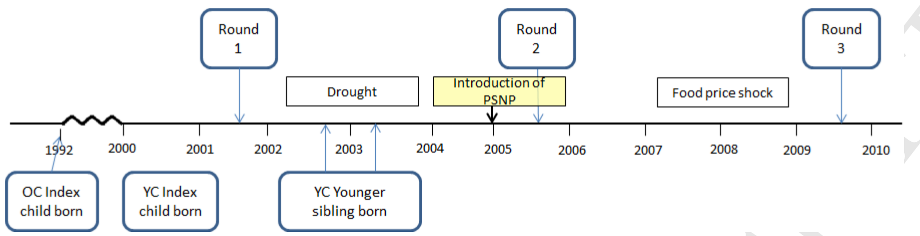

Table 1: Specifications and samples used

Sample/ Method	All rural	Matched common support	Matched parallel trend	Shortlisted	Ever Aid
Child DID	✓	✓	✓	✓	✗
Child DID + controls, lag aid, HAZ	✓	✓	✓	✓	✗
PS-Matching	✗	✓	✓	✓	✗
Sibling Diff	✓	✗	✗	✗	✓
N	1605	1542	956	783	406

Note: Sibling "All Rural" sample contains only children with a sibling, N=661.
DID=Difference-in-difference estimator. PS=Propensity score.

Table 2: Descriptive Statistics by Treatment Status

	Total	Treatment	Control	Difference
HAZ 2009	-1.43	-1.46	-1.40	-0.06
	(1.19)	(1.16)	(1.21)	
Change in HAZ 2006-2009	0.17	0.24	0.12	0.11**
	(0.88)	(0.85)	(0.90)	
Change in WAZ 2006-2009	-0.32	-0.31	-0.33	0.02
	(0.59)	(0.54)	(0.62)	
Received aid prior to PSNP	0.54	0.90	0.27	0.62***
	(0.50)	(0.31)	(0.45)	
Younger cohort	0.67	0.66	0.67	-0.00
	(0.47)	(0.47)	(0.47)	
Male	0.53	0.54	0.53	0.01
	(0.50)	(0.50)	(0.50)	
Change in total expenditure 2006-2009	7.41	2.82	10.80	-7.98*
	(86.02)	(71.69)	(95.13)	
Sex of household head	0.85	0.78	0.90	-0.12***
	(0.35)	(0.41)	(0.30)	
Change in price index 2006-2009	0.77	0.79	0.76	0.02***
	(0.06)	(0.06)	(0.06)	
Shock (drought) 2006-2009	0.52	0.58	0.48	0.10***
	(0.50)	(0.49)	(0.50)	
Shock (food prices) 2006-2009	0.81	0.81	0.82	-0.01
	(0.39)	(0.39)	(0.39)	
Observations		682	924	

Note: Height and Weight-for-age Z-scores calculated using http://www.who.int/childgrowth/en/

Table 3: Difference-in-Difference Child height Growth

	Diff-Diff Basic	Diff-Diff Full controls	Diff-Diff Common support	Diff-Diff Shortlisted
	(1)	(2)	(3)	(4)
PSNP	.127	.179	.190	.439
	(.066)*	(.068)***	(.085)**	(.106)***
Obs.	1605	1605	956	783
R^2	.071	.085	.112	.133

Dependent variable in all columns is change in child height for age z-score (HAZ) between 2006 and 2009. Standard errors are clustered by village and year of birth. Column (1) includes cohort age and sex as controls. Household controls in columns 2-4 include change in size, composition, lag wealth, drought and other shocks, prices, aid receipts in 2006, lagged change in HAZ (02-06)instrumented with lag (2) HAZ (2002).

Table 4: Sibling Difference in Child Height-for-Age

	Sib-Diff1	Sib-Diff2	Sib-Diff3 Ever-aid
	(1)	(2)	(3)
Household received PSNP 2009 only	.685	.707	.667
	(.234)***	(.237)***	(.214)***
Household received aid before 2006 only	-.021	-.001	-.052
	(.154)	(.148)	(.189)
Household received aid in all rounds		.045	
		(.136)	
Obs.	661	661	406
R^2	.085	.086	.1

Dependent variable for all models is the difference in height-for-age z-score (HAZ) between two siblings in the same household. Sample in columns 1-2 includes all households with a younger sibling. Column 3 restricts the sample to households that received aid in both 2006 and 2009. Standard errors are clustered by village and year of birth. Controls include gender and age difference, change in household size, composition, lag wealth, drought and other shocks, prices.

Table 5: Potential channels of impact

	Foodexp	Foodshrt	Dietdiv	WAZ	Hours
	(1)	(2)	(3)	(4)	(5)
PSNP 2009	-.083	-.098	-.043	.079	-.379
	(.049)*	(.039)**	(.102)	(.048)*	(.203)*
Obs.	1604	1601	1068	1068	1605
R^2	.062	.039	.03	.085	.197

Dependent variables all changes 2006-2009 in: (1) ln food consumption per adult equivalent. (2) Response to question "did you worry that the household would run out of food in the past 12 months?". (3) Dietary diversity index. (4) Weight-for-age z-score (WAZ) 2006-2009. (5) Hours worked inside and outside household. Standard errors clustered by village and year of birth. All columns include household level controls, columns (3), (4) and (5) child level controls as in table 3. Dietary diversity and WAZ were not computed for the older cohort in round 3 as they were 15 years old.

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Social Protection for all ages? Impacts of Ethiopia's Productive Safety Net Programme on child nutrition Supplementary Appendix Material

March 5, 2016

1 Sibling model descriptive statistics and robustness

Table A1 shows the descriptive statistics for the 661 siblings in the sample, by treatment status.

Table A1: Descriptive Statistics by Treatment Status - sibling sample

	Total	Treatment	Control	Difference
HAZ 2006	-1.64	-1.65	-1.64	-0.01
	(1.07)	(1.05)	(1.10)	
HAZ - younger sibling	-1.55	-1.56	-1.53	-0.03
	(1.38)	(1.31)	(1.43)	
Received aid prior to PSNP	0.56	0.88	0.29	0.59***
	(0.50)	(0.33)	(0.45)	
Male	0.57	0.55	0.58	-0.02
	(0.50)	(0.50)	(0.49)	
Observations		303	358	

Table A2 shows a number of robustness checks for the sibling sub-sample. The first two are checks for sample selection bias (Fitzgerald et al., 1998). In column (1) we ran the main regression presented in Table 2 of the main text, on the full sample, but included a dummy variable equal to one if the child subsequently appeared in the sibling sub-sample. The coefficient is non-significant. In column (2) we ran a probit model to see whether any of our explanatory variables are different for the sibling sample. None are, but the sibling sub-sample are more likely to be treated by PSNP. In column (3) we ran the main specification as in column (1) but only on the sibling sample. The coefficient on PSNP in 2009 is of similar magnitude, and significant at 10%. In the final column, the dependent variable is sibling-difference in HAZ at age 5, as in table 3 of the main text. The coefficient on PSNP is slightly lower, and is less precisely estimated, which is to be expected given that this variable does not capture intra-household differences in PNSP timing. Hence we motivate our decision to separate out the PSNP 2009 only and aid in 2006 only variables, as these allow us to appropriately estimate the program impact on siblings, using the untreated index child in 2006 as the comparison group.

Table A2: Sibling sample specification checks

	Check1	Check2	Check3	Check4
	(1)	(2)	(3)	(4)
PSNP	.152	.218	.196	.157
	(.070)**	(.130)*	(.108)*	(.143)
Aid 2006	-.076	-.065	-.111	
	(.081)	(.114)	(.091)	
Change in HAZ 2006-2009		-.062		
		(.053)		
Sibling sub-sample	-.066			
	(.055)			
Obs.	1068	1068	661	661
R^2	.107		.069	.075

Notes: Dependent variable for column 2 is a dummy equal to one if child is in sibling sample. Column 1,3, dependent variable is change in HAZ 2006-2009 of index child. Column 4 dependent variable is sibling difference HAZ (2009 of sibling, 2006 of index child). Standard errors are clustered by village and year of birth.

2 Robustness of main result and matching estimates

In table A3 we show our propensity score matching results compared with the difference-in-differences child results when we restrict the sample. In column (1) and (2) estimates are for the sample we created by estimating a propensity score for program participation (Rosenbaum and Rubin, 1983; Becker et al., 2002) and including the common support. When we drop the top and bottom 20% of the propensity scores, and test the parallel trends assumption we see no difference for this sub-sample (956 children). Columns (4) and (5) use the participant children, plus the children in households who were shortlisted for the sample, that we expect to receive PSNP at some point in the near future. The parallel trends assumption is also satisfied for this group of (783) children.

Column (1) shows matching estimates (nearest neighbour, average treatment effect on the treated) based on the full sample. Column (2) re-estimates the main specification from column (3) of table (2) in the main text using the matched sample of households within the common support of the propensity score and the point estimate increases from .12 to .18. Column 3 drops households in the top and bottom 20% of propensity scores, with similar results. Columns (4) and (5) restrict the sample to only those either participating, or shortlisted to participate. Column (4) repeats the matching exercise, and the impact is considerably higher for this subgroup. The program impact using DID as in table (2), column 4 of the main text using the shortlisted comparison group is shown in column (5), and is again considerably higher, suggesting that a simple difference-in-difference using the full sample is likely to be an underestimate.

3 Timing of PSNP introduction

In the sibling-difference analysis (table 4 in the main text), we found a significant and high impact of the PSNP on child nutrition, when the treatment variable was defined

Table A3: Child height Growth: Matching and restricted Samples

	All	CS	PT	SL	SL
	(1)	(2)	(3)	(4)	(5)
PSNP	.123	.186	.192	.267	.434
	(.062)**	(.069)***	(.087)***	(.040)**	(.108)***
Obs.	1541	1541	956	783	783
R^2	-	.093	.092	-	.141

Dependent variable in all columns is change in child height for age z-score (HAZ) between 2006 and 2009. Column 1 and 3 are matching estimates (using teffects nnmatch in stata14). Column 2 excludes the top and bottom 20% of the propensity score for program participation. Column 3 and 4 restricts the sample to only program participants plus those shortlisted to participate. Standard errors are clustered by village and year of birth.

as a dummy equal to one when households received PSNP in 2009 only. For a consist approach, we also re-estimated the program impact for all children using the definition PSNP-2009 only. We find a higher impact (0.4 s.d.) than for those who received the program in 2009, but also in the years prior to 2009. The omitted comparison group in this case (table A4 below, is thus those who received no aid. The test for equality of coefficients between the 2009 only, and 2006 and 2009 participants was rejected (p=.03), and between 2009 only and 2006 only is also rejected (p=.04). The results were robust to the inclusion of aid received in 2006, and 2009, and a dummy for those who received aid in 2006 but not 2009. Results are shown in table A4.

Table A4: Timing of PSNP introduction and child growth

	(1)	(2)	(3)	(4)
PSNP 2009 *only*	.395	.508	.286	.411
	(.149)***	(.157)***	(.141)**	(.169)**
Aid before 2006 only		.158		-.274
		(.071)**		(.202)
Aid in both rounds		.212		.182
		(.060)***		(.108)*
Obs.	1605	1605	783	783
R^2	.092	.101	.126	.141

Dependent variable for all models is the difference in height-for-age z-score (HAZ) between 2006 and 2009. Controls as in table ?? in the main text. Standard errors clustered by village and year of birth.

As outlined above, there is still concern that the comparison group may be qualitatively different from those who received aid at any point (likely better-off), so we re-estimate on our most conservative sample, those who were shortlisted but did not receive PSNP in column 3-4. The point estimate changes slightly, and the impact of aid in 2006 becomes insignificant, but the effect remains greatest for those who received PSNP only in 2009. What is the likely reason for this? One possibility is that the quality of program implementation improved since its introduction in 2005. Wiseman et al. (2010) raise several problems in the early years of PSNP including capacity constraints, co-ordination problems and delayed payments. Later reports have noted that between 2007 and 2009 the program expanded significantly and program design and implementation was improved through greater efficiency and predictability of transfers, strengthened governance, monitoring and evaluation systems as well as delivering more timely responses to shocks (Holmemo, 2014). Also, we speculate that the velocity of height growth could be higher in the first year or two of receiving the program, as the child "catches up" to its potential level of height-for-age. Subsequently, linear growth could be fairly similar to untreated children, though with the child staying on a higher nutritional path. In our data we see the "09only" children do not have significantly higher HAZ in 2009, rather, they had (significantly) lower HAZ in 2006, which is suggestive. Although this result suggests potential high benefits of further expansion of PSNP, a caveat is that the number of households receiving PSNP in 2009 only are few: just 76 households.

4 Weight-for-age results

Here we provide the main results using Weight-for-age (WAZ) as the dependent variable.

Table A5: Child Weight: Main Results

	DDif-waz	DDiflagaid-waz	DDiflagwaz	DDifivwaz
	(1)	(2)	(3)	(4)
PSNP 2009	.034	.079	.094	.106
	(.050)	(.048)*	(.048)*	(.052)**
Male	-.234	-.233	-.197	-.240
	(.034)***	(.035)***	(.044)***	(.043)***
Age (months)	.595	.588	.629	.555
	(.234)**	(.224)***	(.234)***	(.234)**
Received aid 2006		-.086	-.102	-.119
		(.050)*	(.049)**	(.051)**
Change in WAZ 2002-2006			-.073	.055
			(.022)***	(.024)**
Obs.	1068	1068	995	995
R^2	.082	.085	.112	.037

Dependent variable in all columns is change in child weight for age z-score (WAZ) between 2006 and 2009. Standard errors are clustered by village and year of birth.

Table A6: Child Weight: Sibling Results

	cdidwaz	sdidwaz	sdid2waz	sdid3waz	sdid4waz
	(1)	(2)	(3)	(4)	(5)
R3-psnp	.093	.005			
	(.056)*	(.115)			
Household received PSNP 2009 only			.203	.191	.227
			(.158)	(.156)	(.168)
Household received aid before 2006 only			.033	.021	.060
			(.141)	(.128)	(.191)
Household received aid in all rounds				-.026	
				(.102)	
Obs.	665	671	671	671	407
R^2	.13	.131	.133	.133	.127

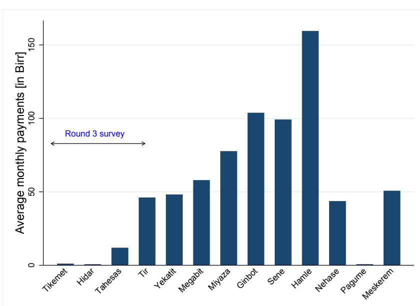
Dependent variable in all columns is change in child weight for age z-score (WAZ) between 2006 and 2009. Standard errors are clustered by village and year of birth.

5 Timing of PSNP Payments and Young Lives Survey

We discuss here the results in table (4) which show slightly weaker impact of PSNP on WAZ (see also the full tables in this Annex), and no (or negative) impact on monthly consumption expenditure, or dietary diversity. We note that consumption and dietary diversity variables are based on recall of food consumption over the past 2 weeks, and the past week respectively.

Hirvonen et al. (2015) document seasonality and dietary diversity in Ethiopia in some considerable detail and show how nutritional intake fluctuates throughout the year. We have data on household recall of PSNP payments by month in the year before the survey (2009). Figure 1 shows that the Young Lives survey was timed not to be during the hungry season in round 3 (2009) and correspondingly, that PSNP payments are very low during the period immediately before the survey, which we believe would explain the non-results for the food consumption-based variables. We tentatively conclude from this that the finding for HAZ (which has been comprehensively robustness-checked) and food shortage over a 12 month period are indicative of the longer-term impact of PSNP, in terms of smoothing out seasonality and adverse shocks. In using surveys to estimate program impacts, such issues should carefully be considered.

Figure 1: PSNP Timing



Notes: Payments (Ethiopian Birr) were collected using the Ethiopian calendar system, that begins on September 11th, and has 13 months. The timing of the 2009 Young Lives survey was during October 2009 to January 2010, which corresponds to *Tikamet* thro *Tir* of Ethiopian calendar 2002 (7 years behind the Gregorian Calendar). See Hirvonen et al. (2015) for more details.

Table A7: Child Height: Full Results

	DD	DDcont	DDiflagaid	DDiflaghaz	DDifiv
	(1)	(2)	(3)	(4)	(5)
PSNP	.127	.200	.178	.163	.179
	(.066)*	(.057)***	(.068)***	(.065)**	(.068)***
Child's age - in months	.022	.003	.003	.036	.0008
	(.008)***	(.023)	(.023)	(.025)	(.025)
Younger cohort	2.091	2.093	2.090	2.871	2.028
	(.662)***	(.592)***	(.595)***	(.548)***	(.590)***
Male		-.347	-.347	-.323	-.349
		(.073)***	(.073)***	(.075)***	(.073)***
Change in household size 2006-2009		-.005	-.004	-.002	-.005
		(.013)	(.013)	(.012)	(.013)
Change in wealth index 2002-2006		.278	.277	.256	.279
		(.297)	(.292)	(.266)	(.290)
Shock (death of livestock) 2006-2009		.004	.004	-.002	.005
		(.048)	(.048)	(.046)	(.048)
Shock (drought) 2006-2009		-.106	-.106	-.118	-.105
		(.047)**	(.047)**	(.045)***	(.047)**
Shock (flood) 2006-2009		.136	.135	.097	.138
		(.070)*	(.071)*	(.067)	(.072)*
Shock (crops failed) 2006-2009		.031	.029	.028	.029
		(.045)	(.047)	(.042)	(.047)
Shock (food prices) 2006-2009		-.151	-.150	-.121	-.152
		(.073)**	(.074)**	(.070)*	(.074)**
Shock (illness of father) 2006-2009		-.003	-.005	-.002	-.006
		(.062)	(.062)	(.061)	(.062)
Shock (illness of mother) 2006-2009		.053	.052	.011	.056
		(.058)	(.058)	(.059)	(.058)
Change CPI-food 06-09		-.622	-.652	-.657	-.652
		(.376)*	(.382)*	(.376)*	(.380)*
Change CPI-nonfood 06-09		-.022	.019	-.161	.033
		(.624)	(.603)	(.486)	(.609)
Received aid 2006			.042	.0006	.046
			(.063)	(.058)	(.062)
Change in HAZ 2002-2006				-.132	.010
				(.019)***	(.024)
Obs.	1606	1605	1605	1605	1605
R^2	.071	.093	.093	.142	.085
Kleibergen-Paap F					273.3
Hansen J					.00

Dependent variable in all columns is change in child height for age z-score (HAZ) between 2006 and 2009. Standard errors are clustered by village and year of birth. Column (5) instrumented lag height using ivreg2 (Baum et al., 2002).

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