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Labor intensive public works and children's activities: the case of Malawi

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As part of broader efforts towards durable solutions to child labour, the International Labour Organization (ILO), the United Nations Children's Fund (UNICEF), and the World Bank initiated the interagency Understanding Children's Work (UCW) Programme in December 2000. The Programme is guided by the Roadmap adopted at The Hague Global Child Labour Conference 2010, which lays out the priorities for the international community in the fight against child labour. Through a variety of data collection, research, and assessment activities, the UCW Programme is broadly directed toward improving understanding of the child labour and youth employment challenges, their causes and effects, how they can be measured, and effective policies for addressing it. For further information, see the project website at www.ucw-project.org.

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ABSTRACT

Public works programs are widely employed to provide temporary support to vulnerable households. Contrary to other social protection programs little is known about their impact on human capital accumulation. In this paper we study the impact of a public work program on education and child labour on the basis of a randomized trial. In particular we study the effects of a labour intensive public work program piloted in Malawi. The intervention was randomized across villages and households with several branches relative to the season of the intervention (lean or harvest) and to the timing of payment. The results indicate temporary positive effects on school attendance and no reduction in child labour. This broadly confirms less robust evidence already available and points to the need of amending public work programs to make them more effective in promoting human capital investments if they are meant to address child labour.

Labour intensive public works and children's activities: the case of Malawi

Working Paper

September 2016

CONTENTS

1. INTRODUCTION

1. Labour intensive public works (LIPW) programs provide temporary opportunities for the poor to work on labour-intensive projects, such as the development and maintenance of local infrastructure. By requiring beneficiaries to work, and to do so at relatively low wages, LIPW programs ensure that only those workers most in need self-select into the program (Besley and Coate, 1992). LIPW programs are used on a wide scale and, together with cash transfer programs, they constitute the core of many developing countries' social safety net (Camfield, 2014; Grosh et al., 2008).[1]

2. Yet, in strong contrast with cash transfer programs, there is limited evidence on the effects of LIPW programs on children's wellbeing.[2] The evidence gap is important, because the effects of LIPW programs on children are a priori undetermined. As we discuss in more detail below, on the one hand the additional income generated through participation in LIPW programs may encourage household investment in education and reduce households' reliance on children to cope with economic shocks. On the other hand, participation in LIPW programs may increase the shadow value of children's time in out of school activities as adults might have less time to work, for instance, on the family plot or caring for children. This paper aims to contribute to filling the evidence gap, by examining the effects of Malawi's LIPW program on children's school attendance and their engagement in economic activities and household chores.

3. Malawi's LIPW program, which had about half a million beneficiaries in 2012, provides a lump-sum payment in return for temporary work on local infrastructural projects. A special feature of this program, as discussed by Beegle et al. (2011), is that it primarily operates during the maize planting season (maize is Malawi's staple food and most rural households are heavily dependent on maize grown on their own small plot of land). Most LIPW programs in other countries, in contrast, operate during the agricultural lean season when the demand for (agricultural) labour is low. By operating during the maize planting season, Malawi's LIPW program provides farmers with a timely source of funding to purchase fertilizer from the country's subsidized fertilizer scheme.

4. To identify the effect of Malawi's LIPW program, we rely on a cluster-randomized trial designed by Beegle et al. (2011) as part of which villages were randomly allocated either to a control group or to one of four treatment groups. Sampled eligible households in the treatment groups were invited to participate in *two* waves of the LIPW program, which

[1] Exact figures on the use of LIPW programs are not available, but a recent report listed 167 LIPW projects implemented over the past decade in Sub-Saharan Africa alone (McCord and Slater, 2009). Many of these programs are large in scale and programs covering millions of households are not exceptional.

[2] For evidence on the effects of cash transfer programs on children's education see for instance Baird, Ferreira, and Özler (2014), Fiszbein and Schady (2009), and Saavedra and Garcia (2012). For evidence on the effects of cash transfer programs on child labor see for instance De Hoop and Rosati (2014), Edmonds (2008), and Fiszbein and Schady (2009).

varied across the four treatment groups both in terms of the timing of the second wave of the program (lean season versus post-harvest season) and payment schedule (multiple tranches versus lump-sum payment). Four waves of survey data were collected at relatively short intervals (a few months apart) to capture the differential effects of the four treatment arms. The setup of the trial is unique and particularly useful for our purposes, because the effect of the LIPW program on children's activities is likely to differ across agricultural seasons. In the lean season it may dampen the effect of limited resources available in credit-constrained households, allowing children to stay in school. In the planting and harvest seasons it may increase the shadow value of children's productive activities (for example on the household farm) if adult household members are labour constrained, thus possibly encouraging children to work instead of attending school.

5. Exploiting this same cluster-randomized trial, Beegle, Galasso, and Goldberg (2014) find that only about 50% of the households who are offered the program decided to participate, which the authors conclude "confirms that the low wage rate and the (work) conditionality requirement are binding". The authors further show that the effects of the program on beneficiary households are generally limited. They find no evidence that the program affects or displaces other productive activities carried out by adults, evidence of "significant slack in [the local] labour markets". The program does not increase fertilizer use as it intends to (although receipt of fertilizer coupons increases in beneficiary households). And finally, food security does not improve in beneficiary households but, surprisingly, deteriorates in non-beneficiary households living in treatment villages.

6. We find that the LIPW program encourages school attendance of children from households that were offered the program.[3] [4] This effect is particularly pronounced during the lean season, when children are especially likely to drop out of school.[5] The finding that the effect on school participation is strongest during the lean season suggests that households use the LIPW program as a buffer during periods of limited resources.[6] We also observe increased school attendance during the planting and (post-) harvest season. The latter suggests that household participation in the program does not increase the shadow value of children's productive activities. Finally, the program does not have a significant effect on children’s participation in household chore nor

[3] We have also examined spillover effects of the program on children from households that reside in treatment villages but that were not offered the program. However, as we shall explain in more detail below, we decided not to focus on those spillover effects in this paper.

[4] Interestingly, the order of magnitude of the increase in school participation is similar to that observed for adolescent girls in a cash transfer experiment in Malawi (Baird, Özler, and McIntosh, 2011) suggesting that the LIPW program encourages school participation through a direct income effect.

[5] High dropout rates during this period have been documented also by others (e.g. Burbano and Gelli, 2009; World Bank, 2004).

[6] Similarly, others have found that households use cash transfers as mechanisms to cope with economic shocks (De Janvry et al., 2006; Fitzsimons and Mesnard, Forthcoming)

economic activities, with a couple of exceptions that are not entirely consistent with the estimated effects on education and cannot be easily explained.

7. There are only two prior studies, to our knowledge, that provide rigorous (albeit non-experimental) estimates of the impact of an LIPW program on children's participation in school and work.[7] One of these studies, makes clear that potential detrimental effects on child labour are not merely of theoretical concern (Hoddinott, Gilligan, and Taffesse, 2009). The study looks at the effects of Ethiopia's Public Safety Net program, which implements both food for work and cash for work projects in "food-insecure" districts. The study is not based on a randomized controlled trial, but relies on propensity score matching to identify the effects of these projects. The results suggest that, in their most basic form, the food for work and cash for work projects tend to reduce child labour. However, when the projects are combined with interventions aimed at increasing the income generating capacity of the beneficiary households, it appears that they may increase children's (in particular girls') participation in work.

8. The other study looks at the effects of an LIPW program implemented in Argentina in response to the economic crisis experienced by the country in the early 2000s (Juras, 2014). Galasso and Ravallion (2004) found that, even though it was not precisely targeted, this LIPW program helped poor households to cope with the economic crisis. Following, the propensity score matching approach of Galasso and Ravallion (2004), Juras (2014) shows "that the program substantially increased children's school attendance in addition to reducing child labour".

9. The remainder of this paper proceeds as follows. We start by giving the necessary background in Section 2. In this section we first discuss the theoretical relationship between LIPW programs and children's participation in school and productive activities. We then proceed to describe Malawi's LIPW program, the setup of the cluster-randomized trial, and the data collected as part of the trial. Section 3 discusses our estimation strategy. Section 4 shows and discusses our main results and Section 5 concludes.

[7] Evidence on other aspects of LIPW programs, although typically not based on randomized evaluations, is more abundant. Most of the evidence comes from studies of India's Mahatma Gandhi National Rural Employment Guarantee Act. While a full review of this literature is beyond the scope of this paper, it is worth mentioning that there is evidence on the effects of LIPW programs on a range of outcomes, such as income gains of participating workers (Datt and Ravallion, 1994), poverty alleviation (Galasso and Ravallion, 2004; Klonner and Oldiges, 2012), displacement effects and crowding out of private work (Imbert and Papp, Forthcoming), consumption and asset accumulation (Deininger and Liu, 2013), social cohesion (Andrews and Kryeziu, 2013), and the ability of LIPW programs to guarantee employment (Dutta et al., 2012).

2. BACKGROUND[8]

2.1 Malawi's LIPW program

10. Malawi's LIPW program, which has been in operation for about two decades, covers all districts of the country. Within districts, the program is targeted to the poorest and most vulnerable villages, based partly on "poverty and vulnerability criteria" and partly on a participatory process involving local officials: so-called traditional authorities and group village headmen. Within these villages, the group village headmen and local committees jointly allocate the program to households deemed especially vulnerable through an informal process.

11. The LIPW program provides short-term work on labour intensive projects. The projects primarily consist of the construction and "upgrading" of roads, afforestation, and irrigation. In 2012, beneficiaries were paid a lump-sum of 3600 Malawi Kwacha (approximately US$11) one week after the completion of 12 days of fulltime work. The program is implemented in part during the lean season to provide beneficiary subsistence farmers with a timely source of funding to purchase fertilizer from the country's subsidized fertilizer scheme.

2.2 The experiment

12. An experiment was designed by Beegle et al. (2011) to capture the effect of the program in its current form, as well as the effect of altering the payment schedule and the season in which the program is implemented. As part of this experiment, a group of 183 villages was randomly allocated to a control group and four treatment groups. The villages selected into the control group did not receive the LIPW program in 2012/2013. Within the treatment villages a group of households was offered the opportunity to participate in two waves of the LIPW program.

13. As shown in Figure 1, the LIPW program offered to the selected households differed randomly across the four treatment groups both in terms of the timing of the *second* wave of the program and in terms of the schedule according to which beneficiaries were paid[9]. The first wave of the program took place in the 2012 maize-planting season (November and December). The timing of this first wave of the program was identical in all four groups of treatment villages. The second wave of the program took

[8] The discussion of the program, the experimental design, and the data collection is based on Beegle, Galasso, Goldberg, Mandala, and Suri (2011) and Beegle, Galasso, Goldberg (2014). The reader is referred to these references for additional detail.

[9] The distribution of the 183 villages across the control group and the four treatment groups is as follows: control (38), lean season & lump-sum payment (35), lean season & payment in tranches (36), post-harvest season & lump-sum payment (40), and post-harvest season & payment in tranches (34).

place either towards the end of the 2013 lean season (in March and April) or after the 2013 maize-harvesting period had ended (in June and July). We refer to these two groups respectively as the lean season and post-harvest season treatment villages. Depending on the treatment group to which the village had been assigned, beneficiaries were paid either at in one installment approximately a week after completing the work or in 5 installments of 720 Malawi Kwacha each at three-day intervals[10].

14. In the next two subsections we describe respectively (i) how the 183 villages were selected for the experiment and (ii) selection of households within these villages into the experimental sample.

2.2.1 Village selection

15. Villages located in a stratified sample of 12 districts[11] (out of Malawi's 28 districts) were randomly selected to be incorporated into the experiment if (i) they had been pre-screened as eligible to benefit from the 2012/2013 wave of the LIPW program[12] and (ii) they had been covered by the World Bank's 2010/2011 Integrated Household Survey (also known as the IHS-3) - a nationally representative survey covering 16 randomly selected households in each of 768 villages (or more precisely census enumeration areas).[13] A total of 183 villages was identified as meeting these criteria and incorporated in the sample. Importantly though, 24 of these villages had been identified incorrectly as they had actually not been covered by the IHS-3. The fact that IHS-3 baseline data is not available for these 24 villages has implications for our estimation strategy.

2.2.2 Household selection

16. A total of 16 households was followed over the period of the study in each of the 183 treatment and control villages. In principle, these 16 households were the same households that had also been covered by the IHS-3. In case households originally covered by the IHS-3 could not be found, they were replaced by households drawn from the IHS-3 listing. If the village had not been covered by the IHS-3, a new listing was carried out from which 16 households were randomly selected to be incorporated in the study.

17. In treatment villages, a subsample of these 16 households was offered the opportunity to participate in the LIPW program. Selection into the LIPW program took place in two steps. First, a relatively small group of

[10] 1 payment before the 12 day period and 4 payments during and after the 12 day period

[11] Two districts from the northern region (Karonga, Mzimba), 4 districts from the central region (Dowa, Lilongwe, Mchinji, Ntchisi), and 6 districts from the southern region (Blantyre, Chikwawa, Mangochi, Nsanje, Phalombe, Zomba).

[12] The village level experiment was possible because the number of eligible villages exceeded implementation capacity.

[13] For more information on the IHS-3 see National Statistical Office (2012).

households was selected from the universe of households in the treatment villages according to the standard selection procedures described above. The households selected according to this procedure may include both households that are part of the 16 households in the study sample and other households residing in the village.[14] Second, because the number of households selected according to the standard selection procedures was too small to achieve a sufficiently powered study design, an additional group of 10 households was randomly selected from the 16 households in the study sample in each of the treatment villages. The households selected according to the standard selection procedures would have participated in the program also in the absence of the study. The additional 10 households participated as a result of the study.

2.3 Data

18. The experiment was set up so that the IHS-3 data could be used as a first baseline. A second baseline was collected in October and November of 2012. Both sources of baseline data, however, have limitations. The IHS-3 data are not available for 24 of the 183 villages and for replacement households. And the collection of the second wave of baseline data took place after the LIPW program had been announced in some of the treatment villages and some households (about 6% according to self-reported information in the second baseline) had already started to participate in the first wave of the program.

19. Three follow-up surveys were administered *after* the completion of each of the three intervention waves. In the remainder of this paper, we shall refer to the five surveys (the 2 baseline surveys and the 3 follow-up surveys) respectively as survey rounds 0, 1, 2, 3, and 4. Table 1 summarizes the timeline of the data collection and how it compares to the agricultural seasons and academic calendar. Column (1) shows the relevant maize farming seasons, column (2) the academic calendar, column (3) the timing of the survey rounds, and column (4) the timing of the treatment waves.

20. The household questionnaire contains a roster section with questions about the demographic characteristics, education, health, and economic activities and household chores carried out by individual household members. It also contains modules on household consumption and expenditures, durable goods, farming equipment, transfers given out and received, credit given out and received, social safety nets, cognitive abilities of the household head and spouse, and a plot roster, use of fertilizer coupons, and planted seeds. The roster sections on education and economic activities and household chores are most relevant for our purposes. The education section covered all individuals in the household

[14] The number of households selected according to this procedure amounts to a maximum of a few households per village and on average only about 0.2 of the 16 households in the study sample.

aged 5 and older. However, the age range of the section on economic activities and household chores differs across survey rounds. In round 0 it covers individuals aged 5 and older, in round 1 it covers individuals aged 15 and older, and in the subsequent rounds it covered individuals aged 10 and older.

3. EMPIRICAL STRATEGY

21. We focus on the effect of the LIPW program on children from households that were offered the program as part of the experiment (i.e. the intent to treat effect). To do so, as we explain in more detail below, we use village level assignment to the program as an instrument for residing in a households that had been offered the program as part of the experiment. We examine in detail whether the timing of household incorporation into the LIPW program matters, by looking separately at treatment villages that received the second wave of the program in the lean season and those that received the second wave in the post-harvest season. Because the payment schedule does not appear to influence school participation and economic activities (as we show in the appendix) we do not distinguish between lump-sum payments and payment in tranches in the analysis presented here.

22. We use the round 1 data (i.e. the second baseline) as our main baseline measurement. The round 1 data are especially useful to select our analysis sample.[15] Moreover, although the round 1 measurement may be affected by anticipation effects, it nonetheless provides a useful source of information regarding children's activities before the program was implemented at scale. Our analysis focuses on children aged 5 to 16 at round 1. We set 16 as the maximum age to ensure that all children included in our sample are younger than 18 in the following survey rounds. To minimize the possibility that our results are affected by endogenous changes in household composition, we restrict our sample to children observed at round 1; new household entrants are not included in our estimation sample. For economic activities and household chores we limit the sample to children aged 10 to 16 at round 1, because information on these activities was not collected for younger children. The sample includes 5467 children aged 5 to 16 and 2889 children aged 10 to 16 at round 1.[16]

3.1 Outcome variables

23. School attendance is determined on the basis the following question: "Are you currently attending school or, if school is not now in session, did you attend school in the session just completed and plan to attend next

[15] The round 0 data cannot be used for this purpose, as they are missing for the 24 villages that were not part of the IHS-3 and for the replacement households in the IHS-3 villages.

[16] The children are spread roughly evenly across the treatment and the control group. Children aged 5 to 16: 1126 in control, 1131 in treatment group 1, 979 in treatment group 2, 1143 in treatment group 3, and 1088 in treatment group 4. Children aged 10 to 16: 603 in control, 586 in treatment group 1, 520 in treatment group 2, 623 in treatment group 3, and 557 in treatment group 4.

session?" For the first three rounds of data (i.e. rounds 1, 2, and 3), which were collected at short intervals, this question captures (changes in) attendance during the same school year. For the last round of data (round 4), this question captures re-enrollment and dropout in the following school year (see also Table 1 for a timeline). The other education variables we examine are *regular* school attendance (missing less than two weeks of school in the period since the previous survey[17]) and total expenditures on education in the month prior to the interview.

24. In addition to these education outcomes, we examine children's participation in economic activities in the 7 days prior to the interview[18] and household chores in the day prior to the interview[19]. Because the round 0 survey collected data on economic activities and household chores only for individuals aged 15 and older we cannot obtain a baseline measurement for these activities.

3.2 Attrition and balance

25. Appendix 1 discusses sample attrition and balance in detail. In the control villages attrition from the round 1 sample is about 9% in round 2, 11% in round 3, and 18% in round 4. By and large, the attrition rate among children from households that were offered the program is similar to that of children from control villages. Because the round 4 attrition rate is high the results for this survey round need to be interpreted with care.

26. Appendix 1 also presents balance checks, which make clear that anticipation effects probably did not substantively affect our baseline measurements. Children from households that were offered the program were generally somewhat less likely to attend school in round 1 than children from control villages, although this difference is not significant.[20] The other covariates we examine are generally not significantly different for children from households that were offered the program and children from control villages. One important exception is that, in round 1, children in control villages were significantly more likely to be female than children in treatment villages. We therefore examine the effects of the LIPW program also separately by gender.

[17] And, for the round 1 survey, missing less than two weeks of school in the past 12 months.

[18] We classify individuals as involved in economic activities if they spent time on household agricultural activities, running or helping to run a household non-agricultural business, informal agricultural work not on the household farm (so-called ganyu labour), or any other work for pay in the 7 days prior to the interview.

[19] We classify individuals as participating in household chores if they spent time collecting water or firewood in the day prior to the interview.

[20] Children from households that were offered the second wave of the program in the post-harvest season were significantly less likely to attend school regularly at baseline than children from control villages.

3.3 Estimation strategy

27. We identify the intent to treat effect of the LIPW program based on cross-section IV regressions specified as follows (estimated separately for each of the three follow-up survey rounds):

$$Y_{ijvf} = \beta_2 T_{jv}^{p-h} + \beta_1 T_{jv}^{lean} + \boldsymbol{\beta_3{}'X_{ijvb}} + \varepsilon_{ijvf} \quad (1)$$

Y_{ijvf} refers to the outcome variable (e.g. school attendance) for individual i from household j in village v at follow-up (f), T_{jv}^{p-h} is a binary variable taking the value 1 for households in treatment villages that received the second wave of the LIPW program in the post-harvest season and were offered the program as part of the experiment (henceforth candidate post-harvest season households), T_{jv}^{lean} is a binary variable taking the value 1 for households in villages that received the second wave of the LIPW program in the lean season and were offered the program as part of the experiment (henceforth candidate lean season households), $\boldsymbol{X_{ijvb}}$ is a vector of round 1 (b) covariates, and ε_{ijvf} is the error term. The covariates include variables that should not have been affected by anticipation effects: the age and sex of the child, district fixed effects, and (follow-up) survey week fixed effects.[21] We cluster standard errors at the village level.

28. To capture the intent to treat effect of the program, we estimate equation (1) with 2SLS, using assignment of villages to the lean and post-harvest season treatment (T_v^{lean} and T_v^{p-h}) as instruments for whether the households were *offered* the lean and post-harvest season treatment (T_{jv}^{lean} and T_{jv}^{p-h}).[22] [23]

29. We establish the robustness of our findings based on difference in differences regressions (again estimated separately for each of the three follow-up survey rounds) specified as follows:

$$Y_{ijvt} = \beta_2 T_{jvt}^{p-h} + \beta_1 T_{jvt}^{lean} + d_i + d_t + \varepsilon_{ivt} \quad (2)$$

30. Here the subscript t refers to the survey round (baseline or follow-up), d_i is an individual fixed effect, d_t is a fixed effect for the follow-up survey round. Because our baseline measurement may not be entirely clean (some

[21] We follow Beegle et al. (2014) in incorporating district and survey week fixed effects.

[22] We assume that the stable unit treatment value assumption (SUTVA) holds for children from households that were not offered the program (both in control and treatment villages). We believe that this assumption is reasonable, as it does not seem plausible that the low-intensity program we study would lead to spillover effects on school participation in such a short period of time.

[23] Beegle et al (2014) use a similar specification in part of their analysis. They also examine intent to treat effects by using both random village assignment (our instrument) and within village random household assignment (our treatment variable) as regressors.

households may have anticipated the program or perhaps even started to participate), the result of the difference in differences regressions should be interpreted as the effect of rolling out the program at scale. We estimate equation (2) with 2SLS, using the same instruments as above.[24] We do not estimate equation (2) for economic activities and household chores, as these outcome variables were not observed for children younger than 15 at baseline. As before, we cluster standard errors at the village level.

31. Because attrition rates are substantive, we also examine whether our results are sensitive to correcting for attrition. To correct for attrition, we reweight our regressions based on the probability that an individual is observed at follow-up. The attrition weights are generated based on the following probit regression (again estimated separately for each of the three follow-up survey rounds):

$$Obs_{ijvf} = \beta_1 T_v^{lean} + \beta_2 T_v^{p-h} + \beta_3 T_{jv}^{lean} + \beta_4 T_{jv}^{p-h} + \boldsymbol{\beta_5}'\boldsymbol{Z_{ijvb}} + \boldsymbol{\beta_6}'\boldsymbol{Z_{ijvb}^{missing}} + \varepsilon_{ijvt} \quad (3)$$

32. Here Obs_{ijvf} is a binary variable taking the value 1 if the individual is observed at follow-up (round 2, 3, or 4), the terms T_v^{lean} and T_v^{p-h} refer to village level assignment,[25] while the terms T_{jv}^{lean} and T_{jv}^{p-h} refer to household-level receipt of the treatment. The vector $\boldsymbol{Z_{ijvb}}$ contains baseline covariates, a more extensive number than incorporated in equation (1).[26] The weights equal the inverse of the estimated probability of being observed at follow-up.

[24] We use a Stata procedure developed by Schaffer (2010) called xtivreg2.

[25] I.e. these are the instruments we use in the estimation of equations (1) and (2).

[26] Including, in addition to the covariates included in the estimation of equation (1): number of household members, number of household members aged 0 to 17, head is female, head is literate in English, head is literate in Chichewa, head ever attended school, head completed primary school, the household possesses a cell-phone, the household uses firewood for fuel, the household has access to electricity, the first principal component of a group of 32 durable goods, (some) household members participate in agriculture, (some) household members run a non-agricultural business, child of household head, grandchild of household head, literate in English, literate in Chichewa, did not complete any schooling, completed primary school, ever attended school, currently attends school. In appendix 1 we examine whether these covariates are balanced at baseline.

4. RESULTS

4.1 Descriptive statistics

33. Table 2 describes the children from control villages in round 1. About 93% of these children attended school at some point in their life. The children who had never attended school were mostly among the youngest in our sample. Current school attendance and regular school attendance were somewhat lower at 88 and 82% respectively. The vast majority of children (98%) had not completed primary school at the time of the round 1 survey, likely reflecting both late entry into the school system and slow grade progression. Education expenditures in the month prior to the interview were Malawi Kwacha (MK) 435 (about US$1.30).

34. The other covariates clearly reflect the poor rural background of the children in our sample. Over 80% of the household heads attended school at some point in their life, but less than 20% completed primary school and only about 70% of household heads could read and write in Chichewa, Malawi's main language. Virtually all households relied on firewood for fuel. Although about two thirds of all households had a mobile phone, only about 2% of households had access to electricity in the dwelling. In the vast majority of households one or more household members were engaged in farming, reflecting the fact that most of Malawi's rural population cultivates maize (for subsistence) on the family plot. Over 20% of households had a female head. Households had about 6.5 members on average out of which about 4 are children aged 0 to 17.

35. Figure 2 examines how school attendance by Malawian children aged 5 to 16 fluctuates over the school year. It exploits the round 0 (IHS3) data, which were collected over the period from March 2010 to March 2011, for the villages incorporated in the study sample. The graph makes clear that the fraction of children reported to attend school peaks at about 82% in the first months of the school year (from September to December) and drops towards the end of the school year (from April to June). Figure 3 shows a similar pattern for the children from control communities over the period of the experiment. Their school participation, depicted by the solid line, dropped from round 1 (the beginning of the 2011/2012 school year) to round 3. It picked up again in round 4 (at the beginning of the 2012/2013 school year).

36. School attendance of children from both candidate lean season and post-harvest households (respectively the dotted and dashed lines) remained stable from survey round 1 to 2. The fact that these households had received the first wave of the LIPW program by round 2 suggests that that the program prevented children from dropping out during the school year. Similarly, the school participation of children from the candidate lean season households, which had by then received the second wave of the LIPW program, also remained stable from round 2 to 3. In contrast, the

school participation of children from candidate post-harvest households, which had not yet received the second wave of the LIPW program, dropped from round 2 to 3. In the next section we examine these apparent treatment effects more formally.

4.2 Impact on education outcomes

37. Panel A of Table 3 shows the intent to treat effect of the program on school attendance, estimated based on the cross-section IV regressions as specified in equation (1). Column (1) shows that the reported round 1 school attendance rates were somewhat lower for children from eligible households, especially those from post-harvest season villages, than for children from control villages. By round 2, when the first wave of the LIPW program had been implemented in all treatment villages, the school attendance rates of children from eligible households exceeded that of children from the control villages (Column (2)). While this difference is statistically significant only in lean season villages, an F-test does not reject the null-hypothesis that the impact on school attendance is identical in lean season and post-harvest season villages. In fact, the difference-in-differences estimates which correct for imbalances in round 1 school attendance rates, discussed in more detail below, suggest that the impact of the LIPW program was virtually identical in the lean and post-harvest season villages.

38. In round 3, the school attendance rates of children from lean season households, which had by then received the second wave of the LIPW program, significantly exceeded that of children in control villages *and* that of children from treated households in post-harvest season households that had not yet received the second wave of the LIPW program. This finding, suggests that participation in the LIPW program during the lean season reduces the probability that children drop out of school. In round 4, the estimated treatment effects are quite similar to those for round 3, although not statistically significant.

39. Panels B and C of Table 3 examine the effect of the program on the 2 other education outcome variables: *regular* school attendance and education expenditure in the month prior to the interview. For regular school attendance we observe an impact pattern that is qualitatively similar to that for school attendance discussed previously. However, the estimated treatment effects are less pronounced and mostly not statistically significant. The value of household expenditure on education tends to be small[27] but with high variance, as a result of which estimated effects for this outcome variable are mostly not statistically significant.

40. Table 4 examines the effect of the program separately for children who were in school in round 1 (Panel A) and children who were not (Panel B).

[27] In 2012, the exchange rate of the Malawi Kwache was about 400 to the US$. In other terms, at baseline households in control villages spent about a dollar a month on a child’s education.

The impact pattern for children who were in school in round (1) is similar to the impact pattern for the full sample. This is not surprising, given that the majority of children in our sample was in school in round 1. The impact for children who were not in school is more pronounced. In round 2 their school attendance is about 10 percentage points higher if their household had been offered the LIPW program. However, given that only about 12% of the children in our sample did not attend school in round 1, statistical power is low and (possibly as a result) the estimated effects are not statistically significant. In round 3 we observe strong and significant effects for children from candidate lean season households. The estimated round 4 treatment effects tend to be less pronounced and not statistically significant.

41. Panel A of Table 5 examines whether the estimated effect on school attendance is heterogeneous by gender. In the analysis, we interact our treatment variables with the indicators for male and female and we test for the equality of the coefficients by means of F-tests. The results indicate that the treatment effect is not significantly different for boys and girls. Similarly, Panel B of Table 5 examines whether the estimated effect on school attendance is heterogeneous by age. We focus on two age groups: children aged 5 to 9 in round 1 and children aged 10 to 16 in round 1. We split the age samples in these two groups because, for the latter age group, we can also estimate effects on economic activities and household chores, as we shall discuss below. The overall impact pattern is similar as before and the estimated effects for younger children are generally not significantly different from those estimated for older children.

4.3 Robustness and effects by payment method

42. Appendix Table 5 examines whether the estimated effect on children's school attendance is robust if we correct for attrition in the cross-section regressions (Panel A), if we estimate the effect of the program based on the difference-in-differences specification (2) (Panel B), and if we correct for attrition in the difference-in-differences regressions (Panel C). The results are by and large robust. Because attrition rates were initially quite low, correcting for attrition has little effect on the round 2 and 3 treatment effects as expected. The same holds for the round 4 estimates, even though attrition rates are markedly higher for this round.

43. When we use a differences-in-differences specification, the estimated round 2 treatment effect is more pronounced in post-harvest season villages (and almost identical to that estimated for lean season villages) than when we use the cross-section specification. The reason is that the difference-in-differences estimates correct for the slightly lower round 1 school attendance rate of children from candidate post-harvest season households.

44. Appendix Table 6 also re-estimates the results presented in Table 3, but now separately for villages receiving the lump-sum payment and the payment in tranches. We show both estimates based on the cross-section

specification (Panel A) and on the difference-in-differences specification (Panel B) both without correcting for attrition. We find some imbalance at baseline (column (1)), as the round 1 probability of being in school for children from households that were offered the program in post harvest villages with payment in tranches is markedly lower than the probability of being in school for children from control villages. However, the difference in differences estimates, which correct for this imbalance, show that the estimated effects of treatment with lump-sum payment is neither qualitatively nor statistically different from the estimated effect of treatment with payment in tranches.

4.4 Impact on economic activities and household chores

45. As mentioned, in round 1 information on economic activities and household chores was collected only for individuals aged 15 and older, while in rounds 2, 3, and 4, information on economic activities and household chores was collected for individuals aged 10 and older. Therefore, we carry out our analysis only for children aged 10 to 16 at baseline, a group for which we are not able to examine round 1 balance.[28] Because participation in economic activities and household chores is gender specific in Malawi and because we observed gender imbalances at baseline (see also Appendix 1), we examine impacts on economic activities and household chores following the gender heterogeneity specification used earlier.

46. Table 6 shows that in control villages, across the three follow-up survey waves, the likelihood of participating in economic activities is quite similar for boys and girls (about two-third of children in both age groups participate in economic activities). Girls, however, are twice as likely to participate in household chores as boys (about two-thirds of girls participate in household chores vs. about one third of boys). The intent to treat effect on economic activities is mostly non significant with the exception of a positive effects on boys in the post harvest season. The impact remain positive in the following wave, but it losses significativity. Similar considerations applies to the impact on household chores carried out by girls: also in this case we can only identify marginally significant positive effects. These results seems to indicate that the program, albeit able to increase school attendance, was not able to reduce participation of boys and girls in economic activities and household chores: if anything there are indications of an increase in participation in these activities.

[28] We did examine round 1 balance for children aged 15 and 16.

5. DISCUSSION AND CONCLUSION

47. Public works schemes are widely used as an instrument to address household vulnerability, supporting at least in theory not only consumption and/ or physical capital investments, but also investment in human capital. The debate on whether public works schemes should be preferred to other social protection interventions like cash transfers needs not to be addressed here. However, while there is abundant evidence about the impact of cash transfers on human capital accumulation, much less is known about the impact of public work schemes. The available evidence is scarce and mainly not based on experimental data. The results, as discussed in the introduction, are at least ambiguous. Public work schemes appear to increase at times school attendance, at least among some age groups. They also tend not to reduce and in some cases even to increase participation of children in economic activities and/or household chores.

48. In this paper we have presented the results of the first (to our knowledge) randomized trial evaluation of the impact of a public work scheme on education and child labour. The results indicate that overall the impact of the program on human capital is relatively small and with contradictory aspects. We observe an increase in school attendance of children belonging to households treated during the lean season, but not on that of households treated in the post harvest season. Moreover, the impact does not appear to carry over to the new academic year, but appears to consist mainly in a reduction of the drop out rate observed during the lean season. There is no evidence of a reduction of children participation on economic activities and/or household chores: on the contrary if anything we can identify an increase albeit limited in size and temporally.

49. The evidence gathered using a randomized trial, does not substantially contradict the less robust results available in the literature. While more evidence needs obviously to be gathered on the human capital effects of public works, we show that this is an issue that deserve careful consideration and, probably, a modification of the design of these programs to make them more effective in addressing (or less detrimental to) human capital accumulation.

APPENDIX 1: ATTRITION AND BALANCE

Appendix table 1 examines sample attrition from round 1 to rounds 2, 3, and 4 based on the cross-section regression specification (1). Column (1) shows that in round 2 the attrition rate in the control villages was about 9%. The attrition rate for children from households that were offered the program in both groups of treatment villages is not significantly different from the attrition rate for children in the control villages. Moreover, the attrition rate in the post-harvest and lean season treatment groups are not significantly different from each other. The attrition rate in the control group in round 3 was about 11%. It appears that the attrition rate is about 5 percentage points lower in the post-harvest treatment group than in the control group. In round 4 the attrition rate is higher, in the control villages about 18% percent, but largely balanced across the control and treatment groups.

We have carried out various checks to examine whether the baseline data are balanced across the control and treatment groups. Appendix table 2 examines the balance of our main education outcome variables as well as two key covariates: age and gender. For each of these variables we examine balance in round 1 using the cross-section regression specification (1) as well as balance in round 1 for the sub-sample of children observed in rounds 2, 3, and 4 respectively. In the balance checks the only controls we include are district fixed effects. School attendance by children from households that were offered the program is a bit lower than that of children from control villages, but not significantly so. *Regular* school attendance, however, *is* significantly lower for children from households that were offered the program in post-harvest treatment villages. We also observe an important imbalance in terms of gender. Children from households that were offered the program are generally less likely to be male than children from control villages.

Appendix table 3 examines the balance of all other covariates that we include in the estimation of the attrition weights. These covariates include 13 household level covariates, 7 child level covariates, and an indicator for any of the covariates missing from the round 1 data.[29] For brevity, we show only balance for the round 1 sample, not for the sub-samples of children observed in rounds 2, 3, and 4. We find no evidence of statistically significant imbalance for any of these covariates.

We have also examined the balance of our outcome variables and the covariates examined in Appendix tables 2 & 3 using the IHS-3 data (i.e. the first baseline). For brevity, we show only the balance of our outcome variables, focusing on children that were observed both in our main sample in round 1 *and* in the IHS-3 survey (Appendix table 4). We find that, in the IHS-3 data, children from lean season villages were initially about 4

[29] In the estimation of the attrition weights we do not include the indicator for "any missing covariates" but indicators for individual missing covariates.

percentage points more likely to attend school (not statistically significant) and 15 percentage points less likely to be engaged in work (significant) than children from control villages. These are substantive deviations. Given that no imbalance for school attendance is observed in the round 1 data, we conclude that the sample of villages that was not covered by the IHS-3 was not balanced.

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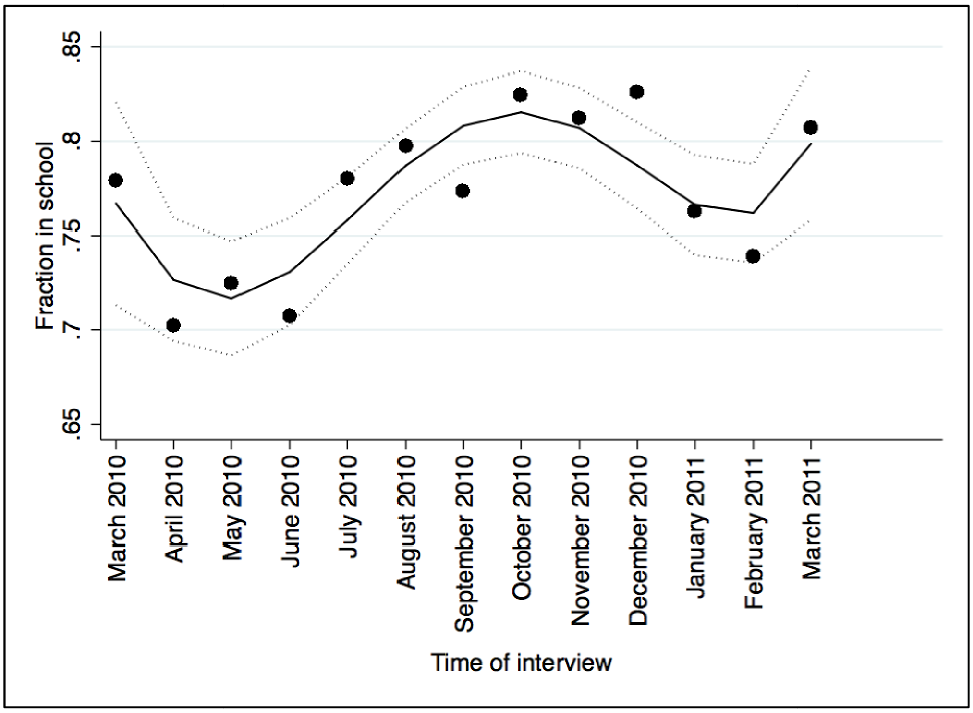
FIGURES

Figure 1. **Summary of the 4 treatment groups**

	Wave 1 treatment	Wave 2 treatment	
	Maize planting season Nov. & Dec. 2012	Lean season Mar. & Apr. 2013	Post-harvest season Jun. & Jul. 2013
Lump-sum payment	All 75 lump-sum villages	35 lean, lump-sum villages	40 post-harvest, lump-sum villages
Payment in tranches	All 70 tranches villages	36 lean, tranches villages	34 post-harvest, tranches villages

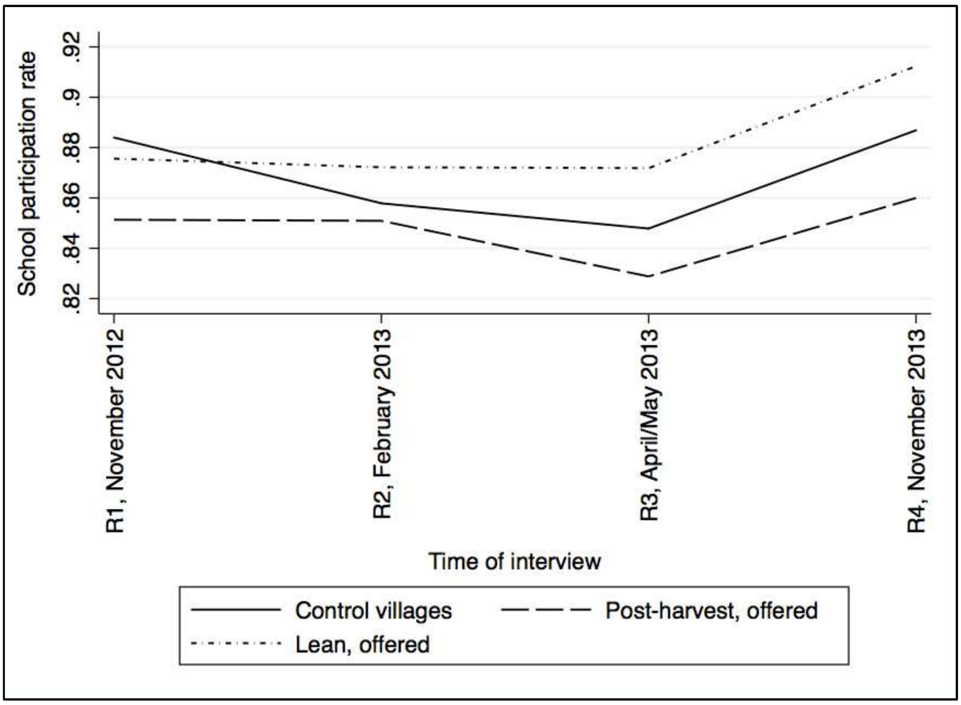
Note. The figure summarizes the timing of the 4 treatment combinations: lean season & lump-sum payment, lean season & payment in tranches, post-harvest season & lump-sum payment, post harvest season & payment in tranches.

Figure 2. **Fluctuations in school participation, IHS3 data**



Note. The figure shows how self-reported school participation fluctuates over the calendar year in the IHS3 data. Dots represent monthly averages, the continuous line represents a 4th degree fractional polynomial fitted regression, and the dotted lines depict the 95% confidence interval around this regression.

Figure 3. Fluctuations in school participation, by treatment group



Note. The figure shows the evolution of school participation for children from control villages (solid line), children from households that were offered the program in post harvest season treatment villages (dashed line), and children from households that were offered the program in lean season treatment villages (dotted line). Survey rounds are depicted on the horizontal axis, school participation rates are depicted on the vertical axis. No adjustments are made for attrition or otherwise.

TABLES

Table 1. Timeline of program implementation

	Maize farming seasons	Academic calendar	Survey rounds	Treatment waves
	(1)	(2)	(3)	(4)
November, 2012		Term 1	Round 1	Planting (wave 1)
December, 2012	Planting	Term 1		Planting (wave 1)
January, 2013	Lean	Term 2		
February, 2013	Lean	Term 2	Round 2	
March, 2013	Lean	Term 2		Lean (wave 2)
April, 2013	Harvest	Term 2	Round 3	Lean (wave 2)
May, 2013	Harvest	Term 3	Round 3	
June, 2013		Term 3		Post-harv. (wave 2)
July, 2013		Term 3		Post-harv. (wave 2)
August, 2013		Holiday		
September, 2013		Term 1		
October, 2013		Term 1		
November, 2013		Term 1	Round 4	

Table 2. Descriptive statistics

Education variables	
Ever attended school	0,927
Currently attends school	0,884
Attends school regularly	0,818
Had not yet completed primary school	0,979
Total education expenditure	435
Household-level covariates	
Head ever attended school	0,824
Head completed primary school	0,189
Head literate in Chichewa	0,680
Head literate in English	0,309
Head female	0,225
Household uses firewood for fuel	0,994
Electricity in dwelling	0,022
Owns a cellphone	0,632
One or more members participate in agricultural activities	0,870
One or more members run a non-agricultural business	0,361
Number of household members	6,4
Number of household members aged 0 to 17	4,0
Child-level covariates	
Male	0,528
Child of household head	0,822
Grandchild of household head	0,117
Literate in Chichewa	0,457
Literate in English	0,159
Age	10,1

Note. Averages for children from control villages aged 5 to 16 in round 1.

Table 3. Impact on school attendance, regular school attendance, and education expenditure

	Round 1 balance	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)	(4)
Panel A. School attendance				
Treatment post-harvest	-0,034	0,035	-0,006	-0,003
	(0,029)	(0,031)	(0,028)	(0,027)
Treatment lean	-0,011	0,060*	0,059**	0,033
	(0,030)	(0,033)	(0,027)	(0,024)
Number of observations	5.409	4.854	4.716	4.359
P(post-harvest=lean)	0,372	0,298	0,005	0,145
Mean control	0,884	0,858	0,848	0,887
Panel B. Regular school attendance				
Treatment post-harvest	-0,058*	0,013	0,002	0,021
	(0,033)	(0,040)	(0,037)	(0,048)
Treatment lean	0,010	0,040	0,052	0,058
	(0,034)	(0,041)	(0,037)	(0,049)
Number of observations	5.409	4.854	4.716	4.359
P(post-harvest=lean)	0,013	0,362	0,172	0,341
Mean control	0,818	0,782	0,783	0,775
Panel C. Education expenditure in last month				
Treatment post-harvest	452*	-230	198	-704
	(271)	495	124	385
Treatment lean	183	-324	171	-654
	(123)	422	99	343
Number of observations	5.409	4.854	4.716	4.359
P(post-harvest=lean)	0,285	0,829	0,854	0,778
Mean control	435	616	269	1.156

Note. Intent to treat effect on school attendance of children aged 5 to 16 in round 1, estimated based on the cross-section regression specified in equation (1) estimated using village assignment to the post-harvest and lean season treatment as instruments. The cross-section regression for round 1 includes only district fixed effects as controls. The cross section regressions for rounds 2, 3, and 4 include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Impact on school attendance by initial school status

	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)
Panel A. Initially in school			
Treatment post-harvest	0,032	0,004	0,020
	(0,025)	(0,024)	(0,022)
Treatment lean	0,042	0,059**	0,034
	(0,026)	(0,023)	(0,021)
Number of observations	4.235	4.128	3.845
P(post-harvest=lean)	0,501	0,001	0,492
Mean control	0,923	0,911	0,921
Panel B. Initially out of school			
Treatment post-harvest	0,102	0,084	-0,099
	(0,099)	(0,073)	(0,091)
Treatment lean	0,088	0,144*	-0,029
	(0,118)	(0,085)	(0,089)
Number of observations	571	547	471
P(post-harvest=lean)	0,868	0,429	0,382
Mean control	0,324	0,320	0,568

Note. Intent to treat effect on school attendance of children aged 5 to 16 in round 1, estimated based on the cross-section regression specified in equation (1) estimated using village assignment to the post-harvest and lean season treatment as instruments. The cross-section regression for round 1 includes only district fixed effects as controls. The cross section regressions for rounds 2, 3, and 4 include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Heteogeneity of impact on school attendance by gender and age

	Round 1 (baseline)	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)	(4)
Panel A. Heterogeneity by gender of the child				
Treatment post-harvest, male	-0,016	0,032	0,004	-0,008
	(0,033)	(0,038)	(0,040)	(0,038)
Treatment post-harvest, female	-0,008	0,039	-0,017	0,000
	(0,035)	(0,039)	(0,039)	(0,033)
Treatment lean, male	-0,011	0,046	0,072*	0,053
	(0,032)	(0,041)	(0,040)	(0,035)
Treatment lean, female	0,019	0,072*	0,046	0,013
	(0,033)	(0,040)	(0,037)	(0,029)
Number of observations	5.397	4.842	4.705	4.348
P(post-harvest male=lean male)	0,894	0,670	0,032	0,057
P(post-harvest female=lean female)	0,388	0,288	0,039	0,654
P(post-harvest male=post-harvest female)	0,825	0,878	0,714	0,870
P(lean male=lean female)	0,394	0,592	0,631	0,356
Mean control, male	0,881	0,857	0,837	0,879
Mean control, female	0,888	0,859	0,860	0,896
Panel B. Heterogeneity by age of the child				
Treatment post-harvest, young (5-9)	-0,006	0,030	-0,011	-0,029
	(0,041)	(0,042)	(0,042)	(0,034)
Treatment post-harvest, old (10-17)	-0,017	0,040	-0,001	0,019
	(0,032)	(0,036)	(0,033)	(0,035)
Treatment lean, young (5-9)	-0,014	0,048	0,032	0,013
	(0,040)	(0,040)	(0,041)	(0,030)
Treatment lean, old (10-17)	0,020	0,071*	0,084***	0,049
	(0,031)	(0,039)	(0,032)	(0,036)
Number of observations	5.409	4.851	4.713	4.356
P(post-harvest young=lean young)	0,814	0,621	0,212	0,154
P(post-harvest old=lean old)	0,175	0,244	0,002	0,361
P(post-harvest young=post-harvest old)	0,795	0,830	0,840	0,279
P(lean young=lean old)	0,459	0,605	0,282	0,442
Mean control, young	0,855	0,834	0,828	0,910
Mean control, old	0,908	0,878	0,864	0,868

Note. Heterogeneous intent to treat effect on school attendance of boys and girls aged 5 to 16 in round 1, estimated based on the cross-section regression specified in equation (1) augmented by interacting the treatment variables with the indicators for male and female, estimated using village assignment to the post-harvest and lean season treatment (also interacted with the gender indicators) as instruments. The cross-section regression for round 1 includes only district fixed effects as controls. The cross section regressions for rounds 2, 3, and 4 include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Heteogeneity of impact on economic activities and household chores by gender

	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)
Panel A. Work in the past 7 days			
Treatment post-harvest, male	0,016	0,154**	0,059
	(0,064)	(0,075)	(0,073)
Treatment post-harvest, female	0,039	0,074	0,080
	(0,065)	(0,063)	(0,078)
Treatment lean, male	-0,091	0,119	-0,061
	(0,068)	(0,083)	(0,074)
Treatment lean, female	-0,068	0,076	0,019
	(0,073)	(0,075)	(0,068)
Number of observations	2.573	2.551	2.954
P(post-harvest male=lean male)	0,088	0,591	0,037
P(post-harvest female=lean female)	0,074	0,977	0,360
P(post-harvest male=post-harvest female)	0,764	0,337	0,794
P(lean male=lean female)	0,784	0,634	0,257
Mean control, male	0,621	0,546	0,423
Mean control, female	0,574	0,559	0,394
Panel B. Household chores in the past day			
Treatment post-harvest, male	0,099	0,006	0,033
	(0,077)	(0,061)	(0,069)
Treatment post-harvest, female	0,029	0,106	0,089
	(0,077)	(0,071)	(0,068)
Treatment lean, male	0,069	-0,006	0,098
	(0,085)	(0,068)	(0,069)
Treatment lean, female	0,026	0,102	0,127**
	(0,072)	(0,074)	(0,059)
Number of observations	2.564	2.540	2.964
P(post-harvest male=lean male)	0,630	0,848	0,285
P(post-harvest female=lean female)	0,966	0,945	0,521
P(post-harvest male=post-harvest female)	0,524	0,282	0,504
P(lean male=lean female)	0,712	0,276	0,715
Mean control, male	0,328	0,361	0,286
Mean control, female	0,674	0,599	0,530

Note. Intent to treat effect on participation in economic activities and household chores by children aged 10 to 16 in round 1, estimated based on the cross-section regression specified in equation (1) augmented by interacting the treatment variables with the indicators for male and female, estimated using village assignment to the post-harvest and lean season treatment (also interacted with the gender indicators) as instruments. Regressions include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix table 1. Attrition

	Observed in:		
	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)
Treatment post-harvest	-0,028	-0,054*	-0,025
	(0,025)	(0,028)	(0,037)
Treatment lean	-0,000	0,001	-0,019
	(0,026)	(0,030)	(0,038)
Number of observations	5.467	5.467	5.467
P(post-harvest=lean)	0,152	0,036	0,831
Mean control	0,088	0,114	0,176

Note. Intent to treat effect on the probability that children aged 5 to 16 in round 1 are not observed at follow-up, estimated using the cross-section regression specified in equation (1). Village assignment to the post-harvest and lean season treatment are used as instruments for whether households were offered the post-harvest or lean-season treatment. The regressions include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix table 2. Balance of education outcome variables and key covariates

	Round 1 (baseline)	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)	Round 1 (baseline)	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)	Round 1 (baseline)	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	School attendance				Regular school attendance				Education expenditure in last month			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment post-harvest	-0,034	-0,034	-0,030	-0,033	-0,058*	-0,056*	-0,060*	-0,075**	452*	447	452	266,220*
	(0,029)	(0,029)	(0,030)	(0,028)	(0,033)	(0,033)	(0,034)	(0,034)	(271)	(291)	(293)	(152,287)
Treatment lean	-0,011	-0,005	-0,008	-0,003	0,010	0,013	0,006	0,012	183	203	177	216,536*
	(0,030)	(0,029)	(0,030)	(0,027)	(0,034)	(0,035)	(0,035)	(0,033)	(123)	(131)	(133)	(118,708)
Number of observations	5.409	4.973	4.853	4.517	5.409	4.973	4.853	4.517	5.409	4.973	4.853	4.517
P(post-harvest=lean)	0,372	0,262	0,392	0,209	0,013	0,011	0,015	0,001	0,285	0,351	0,303	0,747
Mean control	0,884	0,885	0,887	0,891	0,818	0,820	0,825	0,830	435	436	435	436,433

	Male				Age			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment post-harvest	-0,090***	-0,100***	-0,092***	-0,084***	-0,329	-0,323	-0,370*	-0,371
	(0,031)	(0,032)	(0,035)	(0,032)	(0,206)	(0,209)	(0,211)	(0,247)
Treatment lean	-0,058*	-0,065*	-0,061*	-0,063*	-0,158	-0,066	-0,172	-0,186
	(0,033)	(0,034)	(0,036)	(0,032)	(0,193)	(0,198)	(0,198)	(0,221)
Number of observations	5.455	5.016	4.893	4.559	5.467	5.025	4.902	4.567
P(post-harvest=lean)	0,259	0,206	0,265	0,461	0,302	0,118	0,240	0,290
Mean control	0,528	0,534	0,531	0,531	10,149	10,108	10,121	10,106

Note. Balance of education outcome variables (school attendance, regular school attendance, and education expenditure) and key covariates (age and gender) all measured in Round 1, estimated using the cross-section regression specified in equation (1). Village assignment to the post-harvest and lean season treatment are used as instruments for whether households were offered the post-harvest or lean-season treatment. The regressions include only district fixed effects as controls. For each of the variables we show balance for the full sample of children aged 5 to 16 observed in round 1 as well as for the sub-samples of children observed in survey rounds 2, 3, and 4. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix table 3. Balance of additional covariates

	Head ever attended school	Head completed primary school	Head literate in Chichewa	Head literate in English	Head female	Household uses firewood for fuel	Electricity in dwelling	Owns a cellphone	One or more members participate in agricultural activities	One or more members run a non-agricultural business	Number of household members	Number of household members aged 0 to 17	Durable goods index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Treatment post-harvest	-0,006	-0,016	0,007	-0,057	0,027	-0,000	0,012	-0,053	0,021	0,039	-0,397	-0,230	-0,130
	(0,044)	(0,056)	(0,063)	(0,054)	(0,052)	(0,010)	(0,022)	(0,154)	(0,048)	(0,057)	(0,297)	(0,231)	(0,338)
Treatment lean	-0,037	0,032	0,018	0,039	0,009	-0,002	0,003	0,023	-0,045	0,011	0,312	0,304	-0,027
	(0,049)	(0,057)	(0,066)	(0,059)	(0,051)	(0,008)	(0,022)	(0,142)	(0,053)	(0,057)	(0,295)	(0,227)	(0,346)
Number of observations	5.457	5.457	5.453	5.448	5.464	5.466	5.462	5.463	5.467	5.464	5.467	5.467	5.467
P(post-harvest=lean)	0,426	0,229	0,828	0,044	0,658	0,838	0,559	0,526	0,137	0,554	0,004	0,005	0,695
Mean control	0,824	0,189	0,680	0,309	0,225	0,994	0,022	0,632	0,870	0,361	6,436	4,026	0,119

	Ever attended school	Had not yet completed primary school	Had completed primary school, but not secondary school	Child of household head	Grandchild of household head	Literate in Chichewa	Literate in English	Any covariates missing in round 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment post-harvest	-0,036	-0,011	0,004	0,016	0,020	-0,037	-0,019	0,003
	(0,024)	(0,012)	(0,010)	(0,042)	(0,038)	(0,044)	(0,031)	(0,013)
Treatment lean	-0,022	-0,008	0,001	0,016	0,005	-0,005	0,010	-0,008
	(0,023)	(0,011)	(0,009)	(0,041)	(0,037)	(0,045)	(0,031)	(0,012)
Number of observations	5.409	5.409	5.409	5.461	5.461	5.422	5.418	5.467
P(post-harvest=lean)	0,547	0,781	0,697	0,987	0,585	0,418	0,243	0,391
Mean control	0,927	0,979	0,017	0,822	0,117	0,457	0,159	0,022

Note. Balance of additional household and child level covariates measured in Round 1, estimated using the cross-section regression specified in equation (1). Village assignment to the post-harvest and lean season treatment are used as instruments for whether households were offered the post-harvest or lean-season treatment. The regressions include only district fixed effects as controls. We show balance only for the full sample of children aged 5 to 16 observed in round 1. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix table 4. Balance of outcome variables in the IHS-3 data

	School attendance	Regular school attendance	Education expenditure in last month	Work in the past 7 days	Household chores in the past day	Work or chores
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment post-harvest	-0,019	-0,032	946	-0,131**	0,088*	-0,023
	(0,042)	(0,045)	(1.128)	(0,065)	(0,053)	(0,066)
Treatment lean	0,042	-0,004	49	-0,171***	0,061	-0,051
	(0,038)	(0,044)	(310)	(0,064)	(0,047)	(0,061)
Number of observations	2.925	2.925	2.925	2.922	2.919	2.917
P(post-harvest=lean)	0,096	0,471	0,387	0,377	0,542	0,555
Mean control	0,795	0,778	575	0,299	0,301	0,450

Note. Balance of outcome variables as measured in the IHS-3 data, estimated using the cross-section regression specified in equation (1). Village assignment to the post-harvest and lean season treatment are used as instruments for whether households were offered the post-harvest or lean-season treatment. The regressions include only district fixed effects as controls. We show balance only for the full sample of children aged 5 to 16 observed in round 1 *and* in the IHS-3 data. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix table 5. Robustness of estimated impact on school attendance

	Round 2 (after first treatment wave in lean *and* post-harvest season villages)	Round 3 (after second treatment wave in lean season villages)	Round 4 (after second treatment wave in post-harvest villages)
	(1)	(2)	(3)
Panel A. Cross section, corrected for attrition			
Treatment post-harvest	0,035	-0,006	0,011
	(0,031)	(0,029)	(0,028)
Treatment lean	0,050	0,058**	0,047*
	(0,032)	(0,028)	(0,026)
Number of observations	4.851	4.713	4.356
P(post-harvest=lean)	0,541	0,007	0,149
Mean control	0,858	0,845	0,877
Panel B. Difference in differences, not corrected for attrition			
Treatment post-harvest	0,051*	0,018	0,016
	(0,027)	(0,027)	(0,031)
Treatment lean	0,050*	0,071***	0,034
	(0,027)	(0,027)	(0,029)
Number of observations	9.606	9.344	8.626
P(post-harvest=lean)	0,950	0,020	0,505
Panel C. Difference in differences, corrected for attrition			
Treatment post-harvest	0,052*	0,029	0,031
	(0,028)	(0,028)	(0,032)
Treatment lean	0,044	0,078***	0,048
	(0,028)	(0,028)	(0,031)
Number of observations	9.606	9.344	8.626
P(post-harvest=lean)	0,703	0,036	0,557

Note. Intent to treat effect on school attendance of children aged 5 to 16 in round 1, estimated using the cross-section regression specified in equation (1) and the difference in differences regressions specified in equation (2). In Panels A and C we correct for attrition by reweighting individuals according to the inverse of the probability that they are observed at follow-up. The probability of being observed at follow-up is estimated based on the probit specified in equation (3). Both the cross-section and difference in differences specification are estimated using village assignment to the post-harvest and lean season treatment as instruments for whether households were offered the post-harvest or lean-season treatment. The cross section regressions for rounds 2, 3, and 4 include as controls: a gender dummy, age dummies, district fixed effects, and follow-up survey week fixed effects. The difference in differences regressions capture individual fixed effects and survey round fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.