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From protection to production: productive impacts of the Malawi Social Cash Transfer scheme

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The Malawi Social Cash Transfer (SCT) scheme is part of a wave of social protection programmes providing cash to poor households in order to reduce poverty and hunger and promote child education and health. This paper looks beyond the protective function of such programmes, analysing their productive impacts. Taking advantage of an experimental impact evaluation design, we find the SCT generates agricultural asset investments, reduces adult participation in low skilled labour, and limits child labour outside the home while increasing child involvement in household farm activities. The paper dispels the notion that cash support to ultra poor households in Malawi is charity or welfare, and provides evidence of its economic development impacts.

Keywords: cash transfers; impact evaluation; income-generating activities; Malawi

1. From protection to production

Social protection programmes that target poor households and provide cash to those households with the objective of increasing their consumption and inducing investment in child education and health to break the intergenerational transmission of poverty (Barrientos and DeJong 2004, Fiszbein *et al.* 2009) are part of an increasingly popular wave of development initiatives. However, the primary focus of cash transfer programmes on human capital accumulation of the young has led to some criticism that these programmes miss opportunities to be part of broader rural development programmes designed to alleviate poverty (Handa and Davis 2006). By providing cash, transfer programmes provide not only social protection but an incentive and opportunity to alter productive activities. Taking advantage of these opportunities could enhance the value of cash transfer programmes in poverty alleviation, particularly since cash transfers both increase recipient household income and provide a secure source of income.

In 2006 the Government of Malawi joined the wave of African nations setting up cash transfer programmes, initiating its own Social Cash Transfer (SCT) programme as part of a poverty reduction strategy that targeted ultra-poor, labour-constrained households. The SCT programme is an unconditional cash transfer programme designed to reduce poverty, hunger and starvation, and improve school enrolment and attendance and the health and

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nutrition of children among the poorest 10 per cent of households in Malawi (Miller *et al*. 2008c). Funded by the Global Fund through the National AIDS Commission and overseen by the Ministry of Gender, Children and Community Development, the programme now reaches 28,000 households and is expected to serve 300,000 households with 910,000 children by 2015. At present, of the approximately 106,000 individual beneficiaries, nearly two-thirds are children, and nearly one-half are orphaned children (UNICEF 2010).

The Malawi SCT value ranges from US$4 per month for a household with one eligible member to US$13 per month for households with four or more eligible members. In addition, the programme offers a schooling attendance bonus ranging from US$1.30 per month for primary school age children to US$2.60 per month for secondary school age children. Given estimates of average monthly per-capita income in the initial target district of US$7.80, this is a significant and reliable income source for beneficiary households. The transfer is likely to influence not only household consumption choices, but also production decisions, particularly if households face market constraints, and production and consumption decisions are not separable (Singh *et al.* 1986). Liquidity and credit constraints, in particular, are often cited as the main factors limiting productive spending and investments and the choice of income-generating activities in poor rural households (Rosenzweig and Wolpin 1993, Fenwick and Lyne 1999, Lopez and Romano 2000, Barrett *et al*. 2001, Winter-Nelson and Temu 2005). Cash transfers can help overcome such constraints and allow spending and investment that alters production and the allocation of resources, including labour.

Along with credit constraints, insurance market imperfections also limit the ability to smooth consumption across time and space. Faced with uncertainty and insurance market failure, households may take a variety of actions to hedge against risk, including managing risk ex ante by diversifying crops or income and through ex post actions to cope with bad outcomes such as selling off productive assets or taking kids out of school to work at home or elsewhere (Dercon 2002). Transfers provide income that is uncorrelated with other sources of income potentially altering risk management and coping strategies, and creating an additional influence on production choices.

The objective of this paper is to look beyond the social protection function of the Malawian SCT programme and analyse the impact of the programme on productive activities. Three productive areas are considered: investment in assets, particularly agricultural assets and livestock; changes in labour allocation primarily as seen through changes in income-generating activities; and alterations in risk-coping strategies including the use of child labour. Previous research has examined cash transfers and productive choices, although primarily in Latin America. For the Mexican Oportunidades programme, Todd *et al.* (2010) find that the programme increases the value and variety of food consumed from own production and increases land use, livestock ownership and crop spending, while Gertler *et al.* (2006) note that the programme is associated with increased land use and animal ownership as well as a greater likelihood of operating a microenterprise. Veras Soares *et al.* (2010) show that the Paraguayan Tekopora programme increases agricultural expenditures, particularly for extremely poor households. In Nicaragua, however, Maluccio (2010) finds that nearly all the transfer from the Red de Protección Social is used on consumption and education with little spending linked to agricultural or non-agricultural activities.

In this paper, we seek to add to this body of knowledge by examining the issue in an African context and to look more broadly at the productive choices available to beneficiary households and their risk-coping behaviour. Malawi presents a particularly interesting case because of the widely discussed fertiliser and seed subsidy programme that the country

has used to promote agricultural production. This large-scale programme has been found to increase national maize production and productivity, contributing to increased food availability, higher real wages and wider economic growth and poverty reduction (Dorward and Chirwa 2011). Of course, the Malawian SCT programme targets extreme poverty, and SCT recipients tend not to benefit directly from agricultural subsidy programmes. Such extremely poor and vulnerable households are often considered unable to participate in productive activities, or more broadly to contribute to rural economic development. It is in this context, and focusing on this population, that we examine the SCT programme and productive choices.

The remainder of the paper is organised as follows. The next section provides background on the SCT programme, describes the data used for the analysis, and presents descriptive statistics for that data. Section 3 explains the empirical approach taken to analyse the data. Section 4 presents the results of the analysis while Section 5 explores the heterogeneity of those results across a few key household characteristics. The final section draws conclusions from the analysis of the data.

2. Data sources and programme description

An important element of the Malawian SCT programme is that it included a rigorous evaluation with assignment of communities to treatment and control groups and pre-treatment and post-treatments rounds of data collection. That evaluation, which was overseen by a team of researchers at Boston University's School of Public Health in collaboration with the Centre for Social Research of the University of Malawi, yielded the data upon which this study is based.

2.1. Programme design, randomisation and targeting

For the evaluation of the Malawi SCT, a one-year pilot of the programme was designed and implemented in the Mchinji District in central Malawi. Specifically, four control and four treatment Village Development Groups (VDCs) forming part of the original programme rollout were randomly selected to be part of the evaluation.[1] These eight VDCs correspond to 23 villages. Within each village, Community Social Protection Committees identified eligible households according to the national eligibility criteria, and then ranked them in order to select the bottom 10 per cent for inclusion to the programme. Although the assignment of the programme to similar treatment and control groups facilitated the creation of a valid counterfactual, allowing a straightforward analysis to identify impact, the sample design and randomisation suffer from two weaknesses that contribute to some differences between treatment and control households at the baseline.

First, randomisation into treatment and control was at the level of the VDC, with eight total units: four treatment and four control. Thus while the overall number of households in the sample may be sufficient for a rigorous impact evaluation, the number of randomised VDCs is relatively small, which increases the likelihood of finding differences between treatment and control households at the baseline.

The implication of a small number of randomised units is complicated by a second factor. The Malawi SCT targeting mechanism allows a strong role for community participation. While eligibility rules are consistent across VDCs and communities, since Community Social Protection Committees are given the responsibility of ranking all eligible households in their area, ranking was often done based on local criteria, leading to variation in how the targeting was finalised.[2] If a relatively large number of communities

are randomised into treatment and control, this variation is less likely to be an issue as similar sets of rules might be developed across communities. However, in a small sample of communities, variation in these rules may have implications for the creation of a counterfactual. While it is always important to check whether the randomisation process created a reasonable counterfactual, it is particularly important in this case, and we do so in the following section.

Baseline data collection began prior to treatment in March 2007, after which the treatment group received the first transfer. Follow-up data were collected in September 2007 and again in April 2008, at which point the control households also began receiving transfers since they were designated as a 'delayed entry' group. In total 819 households were originally interviewed but 33 treatment and seven control households (8.1% and 2.7% of the original treatment and control sample, respectively) were lost over the two follow-up rounds and are not included in this analysis. The loss of these 40 households was due in part to re-targeting between the baseline and first follow-up, which found 17 ineligible treatment households, and to the dissolution of 23 households due to deaths (Miller *et al*. 2011). We ran various tests to determine whether those households dropped out in any systematic way that would bias our estimates on the panel sample. Results from those tests indicate that attrition is not a concern and that any minor differences between treatment and control linked to the attrition are likely to be dealt with using the combined propensity score matching (PSM) double-difference approach presented later in the paper.[3]

After excluding the attrition households, linking the three rounds of data yields 365 treatment and 386 control households with complete questionnaires over the 2007–2008 period. These 751 households contain 1876 children below 18 years of age, among which 1090 belonged to treatment households and 786 to control households. For this study, the children are analysed as an entire group, although the impacts for younger versus older age cohorts are also explored since work allocations may change considerably from childhood to adolescence.

The survey instrument was designed and implemented by field teams trained by the researchers at Boston University's School of Public Health and the Centre for Social Research of the University of Malawi. The instrument collected detailed information at the adult and child level on demographics, anthropometrics, employment, time use, health and healthcare, disabilities and migration. Child-level modules also focused on orphan status and schooling. Household-level modules focused on dwelling characteristics, assets, food and non-food expenditures, income, credit, literacy, shocks, and the access/use of other social safety nets and support programmes. The study was set up as a panel; therefore, the three survey instruments are broadly similar. There are important differences, however, in the level of detail in sections relevant to the analysis of productive impacts that influence the extent to which certain topics could be analysed.

2.2. Data issues and limitations

One notable constraint in our analysis stems from the fact that although the data used for this analysis constitute a panel following more than 750 households over a period of one year, the survey instruments were not identical over time, limiting the household level analysis to a certain extent. The most important differences were observed in the sections on household-level income and asset ownership and acquisition.

The income module in the survey included changes that affected the comparability of the income variable estimation over the three rounds of data collection. First, the reference period changed from Round 1 to Rounds 2 and 3. In Round 1, households report monthly

income; whereas in the follow-up, households define the income earned as weekly, monthly or yearly. Although the multiple frequencies provide better information, the change in methodology implies incomparable estimates of income levels will be obtained (Azzarri *et al*. 2010). Secondly, the frequency of income earned, which was not collected in the baseline, was collected in the follow-up rounds allowing for an accurate annualisation using the follow-up data but not for the baseline. Further, the list of income sources grew from 17 to 19 items over the three waves of the survey with the addition of ganyu[4] labour and household enterprise earnings in Round 2. The list of possible sources of income at times did not appear to be mutually exclusive, which could lead to erroneous trends if some households classify income under one type of income whereas other households classify the same activity under a different source. While these differences in the instrument over time limited the analysis of income levels, they do not present obstacles to the analysis of participation in income activities.

The asset ownership module experienced a similar evolution to that of income earned, with the number of items growing substantially from the baseline into the follow-up. The follow-up questionnaire did intelligently include questions that allow for ownership at the baseline to be reconstructed; however, the fact that the information for many items was not actually collected at the baseline indicates that some measurement error may be present in the backtracked data. Other information could not be backtracked, such as the levels of asset and livestock ownership at the baseline. The follow-up rounds probe for the number of items owned, whereas the baseline survey only captured whether or not households owned an item. Furthermore, the collection of land ownership data, a key productive asset, was different over the three rounds. Whereas at baseline the questionnaire asked only whether any land was owned, the first follow-up split the category into *Dambo* (irrigated) and Upper land. From Round 2 to Round 3 additional criteria specifying non-fallow land was incorporated. These changes are difficult to reconcile and led to the omission of any analysis of the land variables. Finally, none of the three questionnaires collected detailed agricultural production information, so we were not able to analyse the impact of the programme on input use or yields.

Even with these issues, the longitudinal dataset did permit indicators of productive impacts to be analysed in several ways, as is discussed in the following sections. Furthermore, the survey instrument changes were the same for all households regardless of their status in control and treatment so there is no reason to believe that they led to any systematic differences between the two groups.

2.3. Descriptive statistics

To explore household-level productive impacts we analyse indicators related to livelihoods, income-generating capacity and risk-coping. Following Zezza *et al*. (2010) these impacts will be explored in the context of three main areas: investments in productive assets and livestock; intrahousehold labour allocation and participation in income-generating activities; and risk management and mitigation strategies. Specific examples of anticipated changes in behaviour following the receipt of a cash transfer include investment in assets, the intensification of self-employment activities, the diversification of income sources, and favouring child schooling over child labour.

In order to set the context and establish the validity of the counterfactual for assessing impact, we describe the characteristics of households and children at the baseline. Table 1 reports the descriptive statistics for two sets of variables: those linked to programme eligibility criteria; and general household and child-level variables, which will be used later for the analysis. Descriptive statistics for the household and child-level indicator variables that

Table 1. Baseline characteristics.

	Total	Control (C)	Treatment (T)	Difference (C − T)	*P* value[a]
Eligibility criteria					
Consumed one or fewer meals per day	0.528	0.508	0.548	−0.040	0.271
Begging for food/money	0.384	0.329	0.438	−0.109	**0.002**
Poor	0.991	0.990	0.992	−0.002	0.760
Monthly per-capita expenditure (US$)[b]	1.374	1.381	1.368	0.013	0.938
Household owns 0−1 assets	0.503	0.523	0.482	0.041	0.261
Dependency ratio over three	0.721	0.723	0.718	0.005	0.879
Number of orphans	0.556	0.383	0.729	−0.345	**0.000**
Number of children	2.477	1.979	2.975	−0.996	**0.000**
Household characteristics					
Age of household head	61.508	63.051	59.965	3.086	**0.016**
Female household head	0.649	0.668	0.630	0.038	0.273
Single household head	0.725	0.732	0.717	0.015	0.647
Head has HIV/AIDS	0.014	0.023	0.005	0.018	**0.042**
Head is chronically ill or disabled	0.210	0.215	0.205	0.010	0.749
Years of schooling household head	1.599	1.205	1.992	−0.787	**0.000**
Head is not Catholic	0.523	0.475	0.571	−0.095	**0.009**
Household size	4.106	3.541	4.671	−1.130	**0.000**
Dependency ratio	3.131	2.849	3.412	−0.563	**0.004**
Households without able-bodied members (dependency ratio not calculable)	0.498	0.547	0.449	0.097	**0.008**
Number of household members					
Under 5 years	0.427	0.376	0.477	−0.101	**0.062**
Under 15 years	2.188	1.764	2.611	−0.847	**0.000**
5−10 years	0.806	0.630	0.981	−0.351	**0.000**
11−15 years	0.956	0.759	1.153	−0.394	**0.000**
15−59 years	1.142	0.933	1.351	−0.418	**0.000**
Over 60 years	0.778	0.845	0.710	0.135	**0.005**
Child characteristics					
Age in years	9.589	9.533	9.644	−0.112	0.591
Female	0.511	0.537	0.484	0.052	**0.025**
Orphan	0.572	0.571	0.573	−0.002	0.926
Number of households (for household-level variables)	751	386	365	751	
Number of children (for child-level variables)	1876	786	1090	1876	

[a]The *P* value pertains to the test of significance in the difference of means between the treatment and control groups for each variable. Significant differences are indicated in bold.
[b]Malawi Kwacha were converted to US dollars using the official annual exchange rate from the World Development Indicators database. For 2007, US$1 was equivalent to 139.96 Kwacha.

we hypothesise are influenced by the programme are presented in Table 2. In both cases, differences between control and treatment and tests of significance are presented.

The data from the eligibility criteria confirm that virtually all households interviewed in the control and treatment groups are very poor. Average monthly per-capita expenditure is reported to be approximately US$1.37, indicating the extremely limited resources available to these households. Around one-half of the households consumed one or fewer meals in the previous day and around one-half reported owning none or only one durable item. The sampled households have approximately four members, of which more than two

Table 2. Impact indicators.

	Baseline means					With controls		Using PSM	
	Total (i)	Control (ii)	Treatment (iii)	Difference (iv)	P value[a] (v)	Coefficient[b] (vi)	P value[a] (vii)	Coefficient[c] (viii)	P value[a] (ix)
Household-dependent variables									
Asset and livestock ownership (%)									
Hoes	0.881	0.883	0.879	0.004	0.867	−0.017	0.496	0.024	0.591
Axes	0.306	0.282	0.329	−0.046	0.168	0.037	0.235	0.036	0.517
Sickles	0.223	0.174	0.271	−0.098	**0.001**	0.089	**0.004**	0.061	0.148
Beer drum	0.007	0.003	0.011	−0.008	0.159	0.009	0.213	0.006	0.568
Cattle	0.002	0.003	0.000	0.003	0.331	−0.004	0.302	−0.003	0.295
Goats	0.016	0.018	0.014	0.004	0.629	−0.010	0.285	−0.006	0.722
Chickens	0.117	0.101	0.132	−0.030	0.193	0.005	0.834	0.073	**0.054**
Participation in income activities (%)									
Agricultural wage labour	0.076	0.044	0.107	−0.063	**0.001**	0.051	**0.006**	0.070	**0.001**
On farm self-employment	0.321	0.313	0.329	−0.015	0.654	0.009	0.798	0.015	0.781
Rental income	0.031	0.026	0.036	−0.010	0.441	0.008	0.540	0.027	0.117
Non-farm self-employment	0.470	0.425	0.515	−0.090	**0.013**	0.014	0.707	0.064	0.309
Private transfer income	0.391	0.363	0.419	−0.056	0.113	0.082	0.027	0.073	0.216
Private transfers: remittances	0.076	0.080	0.071	0.009	0.639	0.006	0.779	0.015	0.572
Private transfers: gifts from family/friends	0.353	0.311	0.395	−0.084	**0.016**	0.102	**0.005**	0.085	0.167
Risk-coping behaviour									
Days of ganyu labour worked by household head	7.531	6.940	8.121	−1.180	0.174	−0.691	0.425	−0.650	0.714
Household members beg for food/money (%)	0.384	0.329	0.438	−0.109	**0.002**	0.119	**0.002**	0.043	0.425
Children pulled from school to work for food/money (%)	0.452	0.386	0.518	−0.132	**0.000**	−0.007	0.832	0.061	0.346
Child-dependent variables									
Attending school (%)	0.740	0.706	0.774	−0.068	**0.002**	0.044	**0.046**	0.082	**0.068**
Days of school missed in the previous month	2.913	3.113	2.713	0.401	**0.098**	−0.353	0.199	−0.255	0.672

Activity participation in the previous week (%)									
Household chores	0.556	0.575	0.537	0.038	**0.099**	−0.064	**0.008**	−0.014	0.765
Household childcare	0.616	0.584	0.647	−0.063	0.605	−0.022	0.290	−0.018	0.695
Household elderly care	0.170	0.177	0.163	0.014	0.440	−0.015	0.444	−0.031	0.426
Domestic work outside the household	0.079	0.087	0.070	0.017	0.178	−0.025	**0.057**	−0.003	0.911
Paid domestic work outside the household	0.054	0.057	0.050	0.008	0.461	−0.013	0.254	−0.012	0.649
Self-employed income work	0.038	0.032	0.044	−0.012	0.177	0.008	0.462	−0.015	0.587
Family farm/non-farm business work	0.219	0.218	0.219	−0.002	0.930	0.003	0.902	0.017	0.698
Leisure activities in previous week	0.679	0.663	0.694	−0.032	0.147	−0.013	0.574	−0.012	0.747
Number of households (for household-level analysis variables)	751	386	365	751		746		746	
Number of children (for child-level analysis variables)	1876	786	1090	1876		1860		1860	

[a]The *P* value pertains to the test of significance in the difference of means between the treatment and control group. Significant differences are indicated in bold.
[b]The coefficient values represent β_1 obtained by estimating Equation (1) with baseline data using OLS.
[c]The coefficient values represent β_1 obtained by estimating Equation (1) on the baseline after matching treatment and control households on observable characteristics.

are children under 15 years of age. Approximately 57 per cent of the children sampled are either single or double orphans. Household heads are notably old at 61 years of age; most are single or female with less than two years of formal schooling and one-fifth are chronically ill or disabled. Moreover, households have on average a dependency ratio greater than three, when calculating it using the same methodology as Miller *et al*. (2011);[5] and for one-half of the sample, a dependency ratio cannot be estimated due to a lack of any able-bodied adult household members.

In terms of the primary productive variables – assets, labour and risk – households own virtually no assets, as can be seen in Table 2. Although households report ownership of small farm tools, mostly concentrated in hoes, axes and sickles, ownership of livestock assets is limited to non-existent. Twelve per cent of households own chickens, 2 per cent own goats and nearly none own pigs or cattle. As such, although 32 per cent of the sample participates in on farm (crop/livestock) activities, household livelihoods depend on other activities equally or more. Nearly one-half are involved in non-farm enterprises, 39 per cent rely on the receipt of private transfers, and household heads dedicate 7.5 days per month to ganyu labour.

Involvement in ganyu work corroborates the level of vulnerability indicated by the asset and expenditures data. Ganyu labour is a low-wage informal activity utilised by many households as a coping strategy in response to shocks as well as during the hungry season in Malawi. Since the opportunity to perform ganyu is offered by neighbours and friends to needy members of their social network, it serves as a way for households to share risk with their communities and operates as an 'ex ante social insurance mechanism' (Dimova *et al*. 2010). Improvements in household poverty or vulnerability would be related to a reduction in ganyu participation. Other risk-coping mechanisms, of a more detrimental nature, utilised by households in the sample include begging (38%) and pulling children out of school to work for food or money (45%).

Children's role in household activities is important, as evidenced by participation rates in both domestic tasks and income-generating activities. Over 60 per cent of children were involved in childcare for household members in the week preceding the survey and close to one-fifth were responsible for caring for elderly household members. Further, more than 20 per cent were also involved in the household's farm or non-farm business work. Three-quarters were attending school as well, and the average number of days missed in the previous month approached three days.

Tables 1 and 2 also present the descriptive statistics by the control and treatment assignment given in the survey. The results of *t* tests on the difference of means across groups are indicated by the reported *P* values. Significant differences across the control and treatment groups are observed for a number of variables, indicating that control households may not satisfy the criteria of a perfect counterfactual to treatment households. As noted, the reason for the difference may be related to variation in the community targeting procedures. A community may emphasise certain characteristics in targeting and face tradeoffs between, for example, supporting those with more children who are extremely poor, or the elderly. The differences between the control and treatment groups appear to be linked to demographics: in particular, the treatment group has more children and the control group has older household heads. In certain cases, the treatment group appears to be better off than the comparison according to measures of school attendance and absenteeism, ownership of sickles, educational attainment of the household head, and greater participation in non-farm self-employment. By other measures, the comparison group is relatively better off, with a lower dependency ratio, a lower reported frequency of begging, and lower participation in agricultural wage labour.

At the same time, the control and treatment groups are statistically the same across groups for certain key variables. These include the variables on poverty and expenditures, asset and livestock ownership, several of the income activity variables, and nearly all the child labour participation variables. Many of these variables coincide with the dependent variables on productive impacts that will be studied in the next sections, indicating that much of this paper's analysis will not be confounded by baseline differences.

Given the concerns over the counterfactual, we explore what methods might be used to control for pre-existing differences in control and treatment at the baseline. Of course, one option is to apply a double-difference approach, which we do use and discuss in the next section. Alternative approaches include using a series of explanatory variables in the analysis to control for any pre-existing differences and using propensity scores to adjust the treatment and control groups to create a quasi-experiment where these two groups are similar in baseline characteristics. These options are also discussed more fully below. As an initial test of the value of these approaches, in Table 2 we explore these options using the baseline data. Since this is pre-treatment, the expectation is that there will be limited significant differences between treatment and control in key indicators of impact when using a particular approach. As noted, simple tests of difference show some significant differences, which can be observed from the *P* values in column (v), although the majority of variables are not significantly different. Table 2 then reports baseline tests of differences between treatment and control for two additional options: first, when additional explanatory variables are included; and second, using propensity scores[6] in a standard matching procedure.[7] The outcome of these tests are given by the *P* values in columns (vii) and (ix), which indicate that when controlling for other observable factors, and when incorporating a matching procedure to correct for baseline differences, the pre-existing differences between the treatment and control are nearly entirely removed. Details on the use of explanatory variables, on how the propensity scores are calculated, and how the matching procedure is employed are noted below.

3. Assessing productive impacts: analytical methods

The results in Table 2 suggest that while using controls helps remove some pre-existing differences, PSM performs better in creating a reasonable counterfactual removing nearly all pre-existing significant differences in key variables. Combined with a double-difference approach, this appears to be a reasonable option. To assess the impact of the programme on production, two approaches are thus used: differences in differences (DD) with conditioning variables; and DD combined with PSM.

The DD approach attempts to isolate the effect of treatment, in this case the receipt of the SCT, on the outcome indicator of interest taking advantage of two important characteristics in the data: that it is a panel dataset following the same households over one year, and that the sample was stratified in order to draw eligible treatment and control groups. By taking the difference in outcomes for the treatment group before and after receiving the SCT, and subtracting from that the difference in outcomes for the control group before and after the SCT was disbursed, this methodology is able to control for pre-treatment differences between the two groups, and in particular the time-invariant unobservable factors that cannot be accounted for otherwise. When differences between treatment and control groups are caused by random sampling, the DD estimator with conditioning variables has the advantage of reducing standard errors thereby allowing for tighter confidence intervals, provided the effects are unrelated to the treatment and are constant over time (Wooldridge 2002).

The treatment effect can be estimated using DD by estimating the equation:

$$Y_i = \beta_0 + \beta_1 SCT_i + \beta_2 Round + \beta_3 (Round^* SCT_i) + BX + \mu_i \quad (1)$$

where Y_i is the outcome indicator of interest, SCT_i is a dummy equal to one if household i received the intervention, *Round* is a time dummy equal zero for the baseline and to one for the follow-up round, *Round*SCT* is the interaction between the intervention and time dummies, and μ_i is an error term. To control for household and community characteristics that may influence the outcome of interest beyond the treatment effect alone, we add in BX, a vector of household characteristics to control for observable differences across households at the baseline that could have an effect on Y_i. We include household head characteristics such as age, gender, marital status, educational attainment and disability; a set of household composition characteristics to control for the number of household members by age cohort and the presence of orphans; as well as variables related to the eligibility criteria.[8] These factors are not only those for which we observed some differences across treatment and control at the baseline, but also ones that could have some explanatory role in the estimation of Y_i. As for the coefficients, β_0 is a constant term, β_1 controls for the time invariant differences between the treatment and control, β_2 represents the effect of going from the baseline to the follow-up period, and β_3 is the double-difference estimator, which captures the treatment effect.

Although the DD with conditioning variables should be sufficient to create an unbiased estimate of impact in a randomised setting when the treatment and counterfactual are similar, we also used the PSM approach given the previously noted concerns about the counterfactual. PSM uses observable characteristics to match treatment households with similar, eligible non-beneficiary households in the sample. From the sample of households, a maximum likelihood estimation (probit or logit) is run to estimate programme participation. The regression estimates are used to calculate propensity scores of programme eligibility, which are then used to match the treatment households to the non-treated households. Observations outside the area of common support – that is, the overlap of propensity scores between treatment and control – are excluded in this process since they do not represent a reasonable counterfactual. The approach is sometimes referred to as quasi-experimental since the matching procedure creates treatment and control groups that are alike in all ways except for in receiving the treatment. More details on the matching approach are provided in Appendix 1.

For the PSM approach, a nearest five neighbours matching procedure is used in which treatment households are matched with the control households having the five closest propensity scores. Using this approach instead of the standard nearest-neighbour approach reduces the variance in our control sample since more information is used to construct the counterfactual group.[9] While it can potentially increase bias if distant neighbours are used, the similarity of the treatment and control make this less of an issue. In order to avoid possible poor matches, a caliper width of 0.01 is specified so that matching takes place within 1 percentage point of each treatment household's propensity score. Finally, matching is performed with replacement, and common support is imposed to restrict matching to include only those observations for which there was overlap in the treatment and control propensity score distributions. As a result, 12 control households were excluded because their propensity scores were lower than the minimum value of the propensity score distribution of treated households. In the child-level matching procedure, all observations fell in the region of common support; therefore, no observations were dropped out of the analysis.

One potential drawback of using PSM is the loss of observations due to the imposition of common support. Some papers indicate that this loss of observations prevents the average treatment effect from being accurately estimated (Diaz and Handa 2006, Heinrich *et al*. 2010, Khandker *et al*. 2010, Gertler *et al*. 2011). The experimental nature of the data means that, while not perfect, households have a similar range of characteristics and nearly all observations are used, which helps minimise this concern.

4. The impact of the SCT on production

The results of the analysis of the impact of the Malawian SCT on productive activities are presented in Tables 3 and 4. In all cases, results of both the DD with conditioning variables and PSM combined with DD are presented. For the DD with conditioning variables, standard least-squares regressions are used in all cases.[10] Unless otherwise mentioned we focus on the PSM results, pointing out differences where relevant. The discussion is divided into the three productive areas considered: investment in assets; changes in labour allocation; and changes in risk-coping strategies.

4.1. Investments in productive assets and livestock

The results present considerable evidence that the receipt of the SCT is able to generate investments that can influence household productive capacity. Investments are analysed by looking at changes in the share of households with ownership of agricultural assets, such as hoes, axes, sickles and beer drums, and we find significant changes in reported ownership due to the transfer. Ownership of agricultural assets increased 16 per cent for hoes, 32 per cent for axes and 30 per cent for sickles. The more limited magnitude of impact for hoes is probably due to the high ownership level of this tool at the baseline. While close to 90 per cent of households owned hoes, 30 and 22 per cent of the sample owned axes and sickles, respectively.

Of even greater magnitude is the impact on livestock ownership, with goat and chicken ownership increasing by 52 and 59 per cent, respectively, due to the transfer. Cattle ownership also records a significant increase, but only of approximately 1.5 per cent. The absence of a greater impact in this case could be a result of the large expense of buying cattle, the relative rarity of this activity among Malawian smallholders, or the perception by beneficiaries that investing in larger livestock may compromise their eligibility for the transfer.

4.2. Changes in labour allocation

Due to issues with the collection of income data previously discussed in Section 2.2, we use labour outcomes rather than income levels to assess changes in economic activities. In particular, we analyse household participation in a series of wage and non-wage income activities, along with children's participation in labour and non-labour activities. Given the increase in investment in on-farm agricultural activities, we would expect corresponding changes in labour allocation, with more participation and/or time allocated to on-farm and other self-employment activities.

We indeed find that participation in low-skilled agricultural wage activities does drop significantly (61%) for recipients in the programme. At the same time, participation in independent activities, such as on-farm work or self-employment in household enterprises, does not appear to be influenced by the SCT. This surprising result may be due to our

Table 3. Household-level impacts.

	DD	PSM−DD	DD	PSM−DD	DD	PSM−DD	DD	PSM−DD	DD	PSM−DD	DD	PSM−DD
	Hoes		Axes		Sickles		Beer drum		Cattle		Goats	
SCT	**0.129**	**0.159**	**0.279**	**0.322**	**0.293**	**0.298**	0.008	0.010	**0.009**	**0.015**	**0.487**	**0.522**
	(0.001)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	(0.486)	(0.302)	**(0.078)**	**(0.057)**	**(0.000)**	**(0.000)**
N	746	743	746	743	746	743	746	743	746	743	746	743
R^2	0.116		0.252		0.164		0.056		0.058		0.377	
	Chickens		Agricultural wages		On farm		Self-employment		Private transfers		Rent	
SCT	**0.590**	**0.593**	**−0.534**	**−0.613**	0.010	0.070	−0.032	0.039	**−0.312**	**−0.320**	0.017	0.008
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	(0.843)	(0.223)	(0.500)	(0.472)	**(0.000)**	**(0.000)**	(0.341)	(0.747)
N	746	743	746	743	746	743	746	743	746	743	746	743
R^2	0.361		0.279		0.052		0.138		0.130		0.102	
	Days of ganyu labour		Begging for food/money		Children pulled from school to work for food/money							
SCT	**−3.317**	**−4.875**	**−0.179**	**−0.140**	**−0.345**	**−0.368**						
	(0.001)	**(0.001)**	**(0.000)**	**(0.002)**	**(0.000)**	**(0.000)**						
N	746	743	746	743	746	743						
R^2	0.130		0.666		0.198							

Note: DD, differences in differences with controls, robust standard errors; PSM−DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

Table 4. Child-level impacts.

	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD
	School attendance		Days of school missed		Household chores		Hours spent on household chores		Household childcare		Hours spent on household childcare	
SCT	**0.038**	–0.025	**–0.760**	**–0.721**	**0.137**	**0.077**	**0.496**	0.203	0.027	–0.007	0.148	–0.045
	(0.095)	(0.350)	**(0.046)**	**(0.057)**	**(0.000)**	**(0.023)**	**(0.001)**	(0.131)	(0.302)	(0.769)	(0.207)	(0.711)
N	1596	1612	1035	1048	1860	1876	1860	1876	1860	1876	1860	1876
R^2	0.219		0.070		0.135		0.056		0.059		0.034	
	Household adult care		Hours spent on household adult care		Non-household income work		Hours spent on non-household income work		Family farm/non-farm business		Hours spent on family farm/ non-farm business	
SCT	0.032	0.025	0.220	0.101	0.000	0.003	0.022	0.017	**0.067**	0.021	**0.294**	**0.161**
	(0.253)	(0.319)	(0.101)	(0.255)	(0.973)	(0.808)	(0.636)	(0.729)	**(0.024)**	(0.578)	**(0.002)**	**(0.050)**
N	1860	1876	1860	1876	1860	1876	1860	1876	1860	1876	1860	1876
R^2	0.064		0.043		0.042		0.039		0.086		0.044	
	Domestic work outside the household		Paid domestic work outside the household		Hours spent on domestic work outside the household		Leisure		Leisure hours			
SCT	**–0.070**	**–0.074**	**–0.072**	**–0.077**	**–0.241**	**–0.261**	–0.010	–0.040	0.284	–0.193		
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	(0.722)	(0.299)	(0.159)	(0.320)		
N	1860	1876	1860	1876	1860	1876	1860	1876	1860	1876		
R^2	0.048		0.040		0.049		0.203		0.064			

Note: DD, differences in differences with controls, robust standard errors; PSM–DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

definition of household participation in income-generating activities, which is having non-zero income in a given activity. Therefore, participation in agricultural activities reflects having cash sales of crop or livestock products, while participation in subsistence agriculture is not captured.

If households are in fact increasing their agricultural activities, as evidenced by the increase in ownership of agricultural inputs and livestock, the question arises of where the labour for such activities is obtained. Increased agricultural activities may imply more intense labour from already working household members (more hours), or new participation by other household members. Further, as programme eligibility is in part conditioned on labour constraints, labour may well be sourced from the non-working-age members of the household. This possibility is studied by estimating the impact of the programme on child time use across various activities.

The child-level regressions (Table 4) reveal important adjustments in child activities within and outside the household. Although the SCT programme has limited impact on school enrolment, the number of school days missed per month drops by approximately 0.7 days. These outcomes could be linked to the schooling bonus that forms part of the transfer for households with schooling age children. However, given the lack of impact on enrolment it would appear that the bonus is not large enough to pull new children into school, but only to encourage enrolled children from missing more school.

At the same time, the transfer generates important impacts in the reduction of child participation in non-household labour. Involvement in domestic work outside the household, paid and unpaid, falls by 7 per cent; the number of hours worked on non-household domestic labour falls by approximately a quarter of an hour.

Although there is evidence of reductions in child labour outside the household, the time freed seems to be replaced with greater involvement in within-household tasks. The SCT did not lead to increased leisure[11] time for children; instead, 7 per cent more children participated in household chores (although when using PSM there is no impact on number of hours worked). Moreover, the SCT leads to more child participation in family farm/non-farm business activities. Although this impact on increased participation in household business activities is not robust to PSM, the SCT does lead to an increase of an additional 0.16 hours per week spent on farm/non-farm business activities among children, which is robust across DD and PSM.

The finding that the SCT impacts child time use in some household activities can be explained in two ways. First, if we consider the changes in non-labour household activities, it is likely that some children are being pulled into domestic tasks that were previously performed by adult household members who shifted their own time over to household on-farm activities due to the new investments in tools and livestock. Alternatively, or jointly, the significant reduction in child participation in paid and unpaid domestic work in other households indicates that with the reduction in liquidity constraints provided by the transfer, some children are being pulled from working outside the household and into similar tasks within their own households, as well as into household agricultural work.

One additional factor to consider when interpreting the labour results is the importance of age in intrahousehold and extrahousehold work allocations. While we have used a legal definition of working age, placing the cut-off value at 18 years, we do recognise that children of different ages may be given differing roles and responsibilities within and outside their households. In order to distinguish the impact of the SCT on children of different age groups, we divide the child sample into children four to 12 years of age and adolescents 13–18 years old. The impacts for these subgroups, presented in Table 5, highlight the work responsibilities characterised at different ages. Although all subgroups decreased

Table 5. Child-level impacts for younger versus older age groups.

	Household chores		Family farm/non-farm business		Hours spent on family farm/ non-farm business		Paid non-household domestic labour		Leisure	
	DD	PSM	DD	PSM	DD	PSM	DD	PSM	DD	PSM
Child is 10–17 years of age										
SCT	**0.664**	0.293	**0.088**	0.020	**0.464**	0.286	**–0.116**	**–0.112**	0.034	0.025
	(0.011)	(0.280)	**(0.064)**	(0.719)	**(0.005)**	(0.124)	**(0.000)**	**(0.001)**	(0.376)	(0.438)
N	985	999	985	999	985	999	985	999	985	999
R^2	0.074		0.127		0.075		0.064		0.257	
Child is 13–17 years of age										
SCT	**0.194**	0.051	**0.081**	0.007	**0.238**	0.303	**–0.075**	**–0.108**	–0.061	0.071
	(0.000)	(0.442)	**(0.041)**	(0.930)	**(0.009)**	(0.222)	**(0.000)**	**(0.042)**	(0.120)	(0.153)
N	1042	579	1042	579	1042	579	1042	579	1042	579
R^2	0.154		0.106		0.086		0.067		0.268	
Child is 5–9 years of age										
SCT	**0.187**	**0.105**	0.049	0.014	**0.200**	0.040	**–0.047**	**–0.037**	**–0.093**	**–0.124**
	(0.004)	**(0.094)**	(0.349)	(0.755)	**(0.068)**	(0.700)	**(0.044)**	**(0.047)**	**(0.081)**	**(0.040)**
N	546	548	546	548	546	548	546	548	546	548
R^2	0.147		0.143		0.150		0.110		0.289	
Child is 4–12 years of age										
SCT	**0.194**	**0.144**	**0.081**	0.032	**0.238**	0.131	**–0.075**	**–0.066**	–0.061	**–0.080**
	(0.000)	**(0.003)**	**(0.041)**	(0.477)	**(0.009)**	(0.182)	**(0.000)**	**(0.001)**	(0.120)	**(0.026)**
N	1042	1049	1042	1049	1042	1049	1042	1049	1042	1049
R^2	0.154		0.106		0.086		0.067		0.268	

Note: DD, differences in differences with controls, robust standard errors; PSM–DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

involvement in paid non-household labour due to the transfer, the magnitude of the impact was the greatest for older children at –0.11, most probably due to their greater involvement ex ante in this sort of activity. The impact for younger children ranged from –0.04 to –0.07. For involvement in household chores, the impacts were positive and significant for the younger cohorts, while statistically equivalent to zero for the older ones. Similarly, the only impacts on leisure time were observed for the younger cohorts, with the impact of the transfer reducing participation in non-labour, non-schooling activities.

4.3. Risk-coping behaviour and shocks

The validity of the first explanation of adjustment in child time use hinges on whether adult household members are also making adjustments in their activities. Although adult-level time-use data are not analysed in this paper, we do study the impact of the transfer on the number of days worked in ganyu labour by the household head, an activity described above as associated with community reciprocity arrangements. The transfer is responsible for a reduction of nearly five days worked of ganyu labour per month. Considering household heads worked on average 7.5 days per month at the baseline, this is an important impact. A reduction in the intensity of ganyu work indicates an increased availability of the household for other activities, such as home-based agriculture.

Other household approaches for sharing and coping with risk are observed to change as a result of the SCT. We measure these effects through the share of households reporting having begged and/or pulled their children out of school to work with the objective of obtaining food or money, and by the share of households receiving private transfers. The first two are self-reported risk-coping strategies, whereas the third is a reflection of how the household shares its risk with members of its social network.

The estimates of impact for each of these variables are significant and negative, demonstrating changes in risk-coping strategies and the perception of improved income-generating capacity due to the transfer. Begging for food or money drops by 14 per cent while pulling children out of school in order to work for food or money falls by 37 per cent, an outcome consistent with the reduced absenteeism and reduced participation in non-household labour activities observed above. At the same time, the receipt of private transfers, which comprise remittances and in cash/in kind gifts, drops by 32 per cent. Breaking down private transfers into those two categories reveals that the impact on private transfers is entirely due to a reduction in money or food gifts from friends and family. The declines in private transfer gifts and days of ganyu labour worked suggests the SCT is serving as a substitute for these informal means of support and, given the negative perception of receiving such support mechanisms, this substitution effect may have additional positive implications for the beneficiary households – as well as for previously donating households.

4.4. Seasonality

Most of these results hold up when looking at the impact of the programme after the first follow-up survey. The first follow-up took place six months after baseline, and corresponds with the harvest season in Malawi. Seasonality may have important implications in demand and supply of household labour, as well as on the type and intensity of agricultural investment. Two factors thus come into play in the analysis of these results: the relative brevity of treatment, and the changed economic context of the harvest season.

Across the board, the impact on household-level productive indicators is as significant in the harvest season as in the lean season (Table 6). The impact on investment in

Table 6. Seasonality impacts from baseline to the harvest season (first follow-up).

	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD
	Hoes		Axes		Sickles		Goats		Chickens		Agricultural wages	
SCT	**0.086**	**0.121**	**0.252**	**0.268**	**0.316**	**0.314**	**0.407**	**0.444**	**0.534**	**0.522**	**–0.454**	**–0.485**
	(0.019)	**(0.002)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
N	746	743	746	743	746	743	746	743	746	743	746	743
R^2	0.080		0.250		0.208		0.311		0.291		0.207	
	Private transfers		Days of ganyu labour		Begging for food/money		Children pulled from school to work for food/money		Household chores		Household childcare	
SCT	**–0.242**	**–0.252**	**–3.731**	**–3.868**	**–0.127**	**–0.114**	**–0.276**	**–0.329**	**0.114**	**0.112**	**0.059**	**0.065**
	(0.000)	**(0.000)**	**(0.000)**	**(0.003)**	**(0.000)**	**(0.068)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.003)**	**(0.044)**	**(0.025)**
N	746	743	746	743	746	743	746	743	1,860	1,876	1,860	1,876
R^2	0.144		0.137		0.602		0.172		0.081		0.079	
	Household adult care		Paid domestic work outside the household		Hours spent on domestic work outside the household		Family farm/non-farm business		Hours spent on family farm/non-farm business			
SCT	**0.069**	**0.104**	**–0.055**	**–0.061**	–0.117	**–0.207**	**0.132**	**0.120**	**0.292**	**0.237**		
	(0.018)	**(0.002)**	**(0.000)**	**(0.000)**	(0.116)	**(0.032)**	**(0.000)**	**(0.001)**	**(0.002)**	**(0.013)**		
N	1,860	1,876	1,860	1,876	1,860	1,876	1,860	1,876	1,860	1,876		
R^2	0.085		0.059		0.043		0.087		0.049			

Note: DD, differences in differences with controls, robust standard errors; PSM–DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

agricultural tools and livestock is observed, although in most cases the magnitude is slightly smaller than the impact at the second follow-up. For example, the SCT impact on hoes was a positive 12 per cent at the first follow-up, compared with 16 per cent at the second. On axe ownership the SCT impact was 27 per cent, rather than the 32 per cent at the time of the second follow-up. As for livestock, the transfer impacted chicken ownership at the first follow up by 52 per cent, which is beneath the 59 per cent impact at the second follow-up. Similarly, the adaptation of social network and community support also evolves with time. Whereas the impact of the SCT on the receipt of private transfers was –25 per cent at the first follow-up, it became –32 per cent by the second follow-up survey. A similar trend is found for ganyu labour.

Although the role of seasonality would be expected to be stronger at the first follow-up in terms of labour participation, this impact is not observed. The drop in agricultural wage labour participation at the first follow-up (49%) is not far below the impact at the second follow-up (61%). The expectation would be that increased labour demand during the harvest season would delay the impact of the transfer on agricultural labour participation; however, the bulk of the impact appears early on, indicating the impacts of the transfer are, to some extent, robust to seasonal factors.

A strong effect of seasonality is observed, however, in children's involvement in family farm/non-farm activities. We noted earlier that the impact in this area was only in terms of time spent; no increase in participation was observed when using PSM and DD. However, the analysis at the harvest season demonstrates strong and significant increases in children's participation and time spent in these activities. The share of children spending any time in the family businesses rose by 12 per cent using the DD–PSM estimator, and the number of hours dedicated increased by 0.24 per week. These results reflect greater household needs in home-based work in certain times of the year. This need extends beyond the family economic activities; the SCT leads to significant increases in percentage of children contributing to household chores (11%), child care (7%) and adult care (10%).

5. Heterogeneity of results

The impacts presented in the previous section support the hypothesis that a cash transfer programme such as the SCT is capable of generating productive outcomes and encouraging household agricultural activities. However, given that SCT eligibility is conditional on labour constraints, the question persists regarding the origin of the labour supply required for developing productive activities, and how the impacts may vary for different household types. In order to inform these questions, additional results are presented in Tables 7 and 8 for households with and without access to labour and for male-headed and female-headed households. These regressions seek to isolate the impact of the SCT on the separate groups but do not represent tests of significance across groups; that outcome would be attained with a triple difference methodology, which we do not attempt in this paper.

5.1. Access to labour

The labour supply question can begin to be answered by estimating the programme impact for households according to their access to labour. We separate those with no able-bodied household members between the ages of 19 and 65 from those with at least one able-bodied household member, when able-bodied is synonymous with having no disabilities and being of legal working age. While the outcomes presented in Table 7 are consistent with the earlier overall results, households with no able-bodied members remain somewhat more vulnerable as compared with households with an able-bodied member.[12]

Table 7. Impacts according to household composition.

	Household has 1+ able-bodied adults								Household has no able-bodied adults							
	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD
	Hoes		Sickles		Goats		Chickens		Hoes		Sickles		Goats		Chickens	
SCT	**0.095**	**0.103**	**0.231**	**0.198**	**0.577**	**0.570**	**0.696**	**0.715**	**0.161**	**0.197**	**0.336**	**0.367**	**0.414**	**0.439**	**0.500**	**0.534**
	(0.044)	**(0.016)**	**(0.000)**	**(0.006)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.007)**	**(0.001)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
N	376	373	376	373	376	373	376	373	370	370	370	370	370	370	370	370
R^2	0.168		0.164		0.433		0.439		0.160		0.268		0.359		0.330	
	Private transfers		Days of ganyu worked by head in last month		Begging for food/money		Children pulled from school to work for food/money		Private transfers		Days of ganyu worked by head in last month		Begging for food/money		Children pulled from school to work for food/money	
SCT	**–0.241**	**–0.199**	**–6.011**	**–9.375**	**–0.172**	**–0.120**	**–0.520**	**–0.529**	**–0.368**	**–0.389**	–0.293	–0.490	**–0.157**	**–0.166**	**–0.176**	–0.124
	(0.001)	**(0.014)**	**(0.001)**	**(0.000)**	**(0.000)**	**(0.065)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	(0.753)	(0.534)	**(0.002)**	**(0.040)**	**(0.003)**	(0.116)
N	376	373	376	373	376	373	376	373	370	370	370	370	370	370	370	370
R^2	0.168		0.220		0.727		0.293		0.246		0.160		0.636		0.204	
	School days missed in previous month		Paid domestic work outside the household		Worked on family farm/non-farm business		Hours spent on family farm/ non-farm business		School days missed in previous month		Paid domestic work outside the household		Worked on family farm/non-farm business		Hours spent on family farm/ non-farm business	
SCT	**–1.118**	**–1.197**	**–0.067**	**–0.070**	**0.111**	**0.058**	**0.334**	0.175	**–0.444**	–0.276	**–0.066**	**–0.094**	0.027	**–0.126**	0.327	–0.144
	(0.012)	**(0.001)**	**(0.001)**	**(0.000)**	**(0.003)**	**(0.084)**	**(0.004)**	(0.112)	**(0.500)**	(0.810)	**(0.032)**	**(0.022)**	(0.633)	**(0.095)**	(0.104)	(0.467)
N	699	699	1251	1251	1251	1251	1251	1251	336	349	609	625	609	625	609	625
R^2	0.098		0.050		0.089		0.052		0.179		0.121		0.163		0.119	

Note: DD, differences in differences with controls, robust standard errors; PSM–DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

Table 8. Impacts according to household head gender.

	Male head								Female head							
	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD	DD	PSM–DD
	Hoes		Sickles		Goats		Chickens		Hoes		Sickles		Goats		Chickens	
SCT	0.018	0.072	**0.190**	0.137	**0.432**	**0.467**	**0.583**	**0.571**	**0.195**	**0.230**	**0.344**	**0.436**	**0.517**	**0.520**	**0.595**	**0.612**
	(0.755)	(0.250)	**(0.033)**	(0.103)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
N	260	258	260	258	260	258	260	258	486	485	486	485	486	485	486	485
R^2	0.160		0.182		0.392		0.347		0.161		0.237		0.424		0.423	
	Private transfers		Days of ganyu worked by head		Begging for food/money		Children pulled to work for food/money		Private transfers		Days of ganyu worked by head		Begging for food/money		Children pulled to work for food/money	
SCT	**–0.337**	**–0.304**	**–4.259**	**–5.372**	**–0.191**	**–0.169**	**–0.232**	**–0.355**	**–0.328**	**–0.337**	**–3.607**	**–5.494**	**–0.163**	**–0.156**	**–0.396**	**–0.415**
	(0.001)	**(0.001)**	**(0.045)**	**(0.017)**	**(0.000)**	**(0.073)**	**(0.006)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.001)**	**(0.000)**	**(0.000)**	**(0.040)**	**(0.000)**	**(0.000)**
N	260	258	260	258	260	258	260	258	486	485	486	485	486	485	486	485
R^2	0.232		0.203		0.707		0.224		0.182		0.145		0.672		0.231	
	School days missed in previous month		Paid domestic work outside the household		Household chores		Hours spent on household chores		School days missed in previous month		Paid domestic work outside the household		Household chores		Hours spent on household chores	
SCT	–0.744	**–0.738**	**–0.058**	–0.047	**0.120**	0.010	0.231	–0.011	–0.703	–0.743	**–0.081**	**–0.091**	**0.171**	**0.152**	**0.699**	**0.424**
	(0.161)	**(0.098)**	**(0.023)**	(0.198)	**(0.030)**	(0.842)	(0.287)	(0.963)	(0.164)	(0.245)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.010)**
N	341	347	636	644	636	644	636	644	694	701	1,224	1,232	1,224	1,232	1,224	1,232
R^2	0.218		0.101		0.250		0.084		0.078		0.064		0.128		0.070	

Note: DD, differences in differences with controls, robust standard errors; PSM–DD, differences in differences with propensity score matching, bootstrapped standard errors (50 repetitions). *P* values reported in parentheses below estimates of impact. Significant impacts are indicated in bold.

Large divergences emerge in terms of the risk-coping and informal insurance mechanisms. Among able-bodied households, ganyu labour falls by over nine days a month (from an average of 10 days a month) and the share of households pulling children from school decreases by 53 per cent, while no impact is observed on either indicator for households with no able-bodied members. Similarly, children from households with able-bodied members missed on average one school day less per month upon receipt of the programme, while there was no impact for households without able-bodied members. Finally, households without able-bodied members experience a reduction in receipt of private transfers of 39 per cent, compared with a reduction of 20 per cent for households with able-bodied members. All together, households without able-bodied members, who tend to be elderly and single headed, appear to remain considerably more vulnerable, while the impacts of the programme are considerably strong for households with able-bodied members.

5.2. Gender of household head

Given a broad literature that indicates different asset endowments, livelihoods approaches and risk-coping strategies across gender lines, women have been demonstrated to be disadvantaged in the rural space. They own fewer productive assets, access productive inputs with more difficulty, face wage discrimination, and confront other obstacles, while at the same time are identified as having a central role in efforts to reduce poverty and food insecurity.[13]

To analyse these issues in the context of a cash transfer programme, we break down our results according to the gender of the household head (Table 8). Although many of the effects observed overall are robust across gender lines, such as the reduction in private transfer receipt and the reduced share of households reporting begging for food or money, a number of impacts come through only, or most strongly, for female-headed households. Most notable are the investment impacts, through which agricultural tool and livestock ownership are greatly and positively impacted for female-headed households, which is consistent with the smaller initial agricultural asset base among this group. Male-headed households have a broader initial asset base, which would explain the lack of, or more limited, impact on these assets due to the SCT.

In addition to the investment impacts, the child-level outcomes for female-headed households are strongly pronounced in terms of involvement in labour activities. Children in female-headed households are impacted by the SCT through a reduction in the participation (–9%) in non-household labour activities and in terms of greater participation (15%) and time spent (0.42 hours) in household chores. Conversely, the transfer impacts children in male-headed households only in terms of reduced school absenteeism. Since female-headed households are often also single-headed households, these outcomes are a reflection of the important constraints facing those households in order to meet household responsibilities, both in terms of domestic tasks and livelihoods generating activities.

6. Conclusions

Analysing the economic impacts of the Malawi SCT scheme using longitudinal data taken from the Mchinji district pilot from 2007 to 2008, this paper dispels the notion that cash support to the poorest 10 per cent of households in Malawi, or to populations with similar characteristics in other countries, is charity or welfare, without an impact on economic development. Even the poorest of the poor in Malawi, facing severe labour

constraints, manage to invest a portion of the transfer in their family business, which for most Malawians is subsistence agriculture. Our analysis demonstrates that, in the context of missing markets, cash geared for household consumption plays an important role in building up the productive capacity of poor, subsistence-oriented households.

Specifically, we find strong and robust impacts generated by the receipt of the SCT that reveal both the productive and protective nature of the transfer. Agricultural investments resulting from the programme were observed in terms of increased ownership of agricultural tools and livestock. Households reduced participation in low-skilled activities outside the household, such as agricultural wage labour and ganyu work, generally associated with vulnerability in Malawi. Although due to data limitations an increased focus on household agricultural activities could not be observed by way of increased income from agriculture, the investment impacts indicate an increased focus towards household-oriented productive activities.

At the same time, given those widespread labour constraints, concern arose that households were relying on child labour to intensify their agricultural activities. This appears to be true, and the impact is stronger in the harvest season, where children increased participation in household tasks such as chores and caring for household members. This outcome, linked with the reduction of household involvement in agricultural wage and ganyu labour, supports the notion that adult household members increased their involvement in home-based productive work while seeking younger child household members to substitute them in chores and household member care.

Finally, the results on risk-coping behaviour highlight a valuable aspect of the transfer: its protective function of reducing household vulnerability from shocks. Households often rely on short-term solutions, including taking children out of school, or selling assets, which have long-term impacts in terms of household productive capacity. The protective function is particularly strong for households with at least one able-bodied member; for those households with no able-bodied members, programme impact was not as strong, although this reflects the demographic characteristics of these households, elderly with fewer children.

This paper underscores the importance of considering, in terms of design, implementation and evaluation of social cash transfer programmes, the economic activities of beneficiary households. In the context of missing and incomplete credit, labour and product markets, cash transfer programmes, even if focused on health, education and nutritional outcomes, have important implications for the productive activities of beneficiary households. Some of these indirect effects are positive, enabling households to overcome liquidity and credit constraints, and increase their productive capacity. However, other aspects may lead to possibly undesirable indirect effects, particularly regarding child labour, or may limit the impact on the objectives of the programme, including school attendance and enrolment. Future analysis will benefit from impact evaluation design and data collection that considers economic impacts as a research objective in order to better understand the impact of cash transfers on extremely poor and vulnerable households.

Notes

1. For additional information on the SCT scheme and its context, see: http://www.cpc.unc.edu/projects/transfer
2. Miller *et al*. (2008a, 2008b, 2010) provide detailed explanations of the evaluation design and targeting procedure.

3. We ran *t* tests to see whether the characteristics of attrition and panel households were statistically different. These tests revealed that nearly all factors were statistically the same across attrited and panel households. The same result was found when running a probit on attrition status. However, programme participation was statistically significant in both the *t* tests and the probit, indicating that treatment households were 5.5 per cent more likely to drop out than controls. It is probable that this result is driven by the 17 households found to be ineligible. Due to the small number of observations, we were unable to further statistically analyse the systematic nature of ineligibility or deaths in the original sample.
4. Ganyu labour is a type of low wage casual labour performed in Malawi. More detail on this type of labour is provided in Section 2.3 and in Dercon (2002) and Whiteside (2000).
5. Although we follow the methodology described by Miller *et al*. (2011), for the purposes of not losing observations in the empirical analysis we assign the arbitrary value of 0.1 in the denominator when calculating the dependency ratio for households with no able-bodied adults to assure that its value for those households is greater than three.
6. Propensity scores are simply the estimated probability of eligibility given a series of explanatory variables that are anticipated to be linked to participation.
7. We also considered applying propensity scores used as weights in an inverse propensity weighted system but found it was not as effective as a standard PSM approach.
8. Child level regressions also include the age, gender and orphan status of the child.
9. We also tested the robustness of our results with nearest-neighbour matching with replacement, using caliper widths of 0.1 and 0.5, and bootstrapping standard errors with 50 repetitions. With few exceptions we obtained similar outcomes.
10. Angrist and Pischke (2009) explain that a standard regression model can correctly estimate treatment effects across experimental groups, even for limited dependent variables.
11. The category 'Leisure' was not specifically defined in the survey instrument. We assume it refers to non-labour, non-schooling activities.
12. In order to check the robustness of our results with respect to household labour constraints, we also ran regressions dividing the sample according to the household dependency ratio, creating a labour-unconstrained group for households with a dependency ratio below three, and a labour-constrained group for those households with the dependency equal to or greater than three or for which the dependency ratio could not be estimated due to there being no able-bodied adults in the household. The findings for these regressions are, with few exceptions, consistent with the findings when households are divided according to having none or at least one able-bodied adult member.
13. Although we do not provide a full assessment of this literature in this paper, Deere and Doss (2006), Peterman *et al*. (2010) and Quisumbing *et al*. (2011) review the evidence on gender and asset ownership, Quisumbing *et al*. (2011) review the gender dimension of response to shocks, while FAO *et al*. (2010), FAO (2011), and World Bank (2011) present thorough reviews of gender in terms of agriculture and development.

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Appendix 1. Methodological notes on the propensity score matching approach

The propensity score matching approach requires as a first stage the estimation of a propensity score upon which the matching of treatment and control factors will take place. Table A1 reports the results from two estimations of propensity scores, one for matching at the household level and another for matching children, to correspond with the different levels at which the analysis takes place. Explanatory factors taken from the baseline survey include programme eligibility criteria, demographic variables and exposure to shocks. The results are largely as expected, with demographic variables playing an important role in determining the probability of participation. At the household level, begging also explained a large share of programme participation. Other significant covariates in the child-level specification included those describing the consumption and vulnerability aspects of the eligibility criteria, orphan status and shocks. The signs on the coefficients were mostly, but not always, in line with expectations. One exception, at the child level, was having no assets which carried a negative sign. Although having fewer assets should have been associated with increased participation, the overall low ownership level of any asset across all households is likely to have confounded this coefficient. In general, the fact that targeting was not necessarily implemented consistently across communities is likely to be an important contributing factor to the cases where the coefficients are not the expected sign.

Since the results on the probit regressions gave a first indication of the adequacy of the specification, we then divided the propensity scores into five blocks in order to run balancing tests which would further check whether the specification was satisfactory. In practice, the balancing tests are a series of *t* tests on the difference in means across the control and treatment groups for each covariate in each block and on the average propensity score in each block. The tests concluded that the average propensity scores and all covariates are statistically the same for the treatment and control groups in each of the blocks; thus passing the tests. Since the survey does have a relatively small sample size, the concern arose of whether the blocks contain enough observations against which to run balancing tests. Table A2 reports the number of observations in each block. Although there appears to be few observations in two of the household level blocks, the propensity score distributions within each block are balanced overall. A close look at the distribution of the propensity scores in each block revealed that their means across groups are statistically equal and their standard deviations of similar magnitudes, reducing concerns of small sample size problems in the matching procedure.

Table A1. Probit results for propensity score estimation.

Panel A: child-level estimation	Intervention	Panel B: household-level estimation	Intervention
Consumed one or fewer meals per day	0.139**	Consumed one or fewer meals per day	0.114
	(0.024)		(0.243)
Begging for food/money	0.111	Begging for food/money	0.323*
	(0.200)		(0.061)
Monthly per-capita expenditure	0.001***	Monthly per-capita expenditure	0.000
	(0.000)		(0.276)
Household owns 0−1 assets	−0.176***	Household owns 0−1 assets	−0.072
	(0.006)		(0.481)
Dependency ratio over 3	−0.115	Dependency ratio over 3	−0.118
	(0.177)		(0.510)
Number of orphans	0.078***	Number of orphans	0.064
	(0.001)		(0.157)
Number of children	0.138***	Number of children	0.047
	(0.006)		(0.721)
Age of household head	−0.006	Number of household members under 5	0.109
	(0.835)		(0.482)
Age of household head squared	0.000	Number of household members 5−10 years	0.321**
	(0.880)		(0.046)
Gender of child	−0.081	Number of household members 11−15 years	0.288*
	(0.185)		(0.074)
Child is single or double orphan	−0.042	Number of household members 15−59 years	0.337***
	(0.547)		(0.002)
Number of household members under 5	−0.116**	Number of household members 60+ years	0.216
	(0.015)		(0.321)
Number of household members 5−10 years	0.108***	Elderly head * number of household members 60+	−0.124
	(0.009)		(0.632)
Number of household members 11−15 years	0.067*	Log of household size	−0.695***
	(0.086)		(0.009)
Number of household members 15−59 years	0.109***	Log of dependency ratio	0.099
	(0.002)		(0.151)
Number of household members 60+ years	−0.063	Years education of household head	0.018
	(0.600)		(0.490)
Log of household size	0.090	Elderly head * years education of head	0.127**
	(0.626)		(0.010)
Log of dependency ratio	0.020	Elderly head	−0.093
	(0.486)		(0.708)
Years education of household head	0.036***	Natural shock * begging	−0.052
	(0.008)		(0.805)

(*Continued*)

Table A1. (*Continued*).

Panel A: child-level estimation	Intervention	Panel B: household-level estimation	Intervention
Elderly head * years education head	0.037	Household suffered natural shock	0.082
	(0.230)		(0.507)
Elderly head	0.297*	Constant	−0.762***
	(0.073)		(0.001)
Elderly head * number of household members 60+	−0.111	Observations	751
	(0.516)	Robust *P* values in parentheses	
Natural shock * begging	0.353***		
	(0.006)		
Household suffered natural shock	−0.158**		
	(0.044)		
Constant	−0.954***		
	(0.000)		
Observations	1876		
Robust *P* values in parentheses			

*Significance at 10%; **significance at 5%; ***significance at 1%.

Table A2. Distribution of treatment and control observations across blocks.

	Household-level blocks						Child-level blocks					
	1	2	3	4	5	Total	1	2	3	4	5	Total
Control	25	235	72	31	11	374	247	197	166	116	60	786
Treatment	12	134	96	72	51	365	129	178	209	259	315	1090