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A Cash Plus Program Reduces Youth Exposure to Physical Violence in Zimbabwe

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Abstract

Violence against children and adolescents, a highly prevalent problem, is a clear violation of child rights and has detrimental effects on later life outcomes. Programs that alleviate poverty address a structural determinant of child vulnerability and can thereby reduce child abuse. This paper investigates whether the Government of Zimbabwe's Harmonized Social Cash Transfer (HSCT) Program, which combines cash transfers with complementary services, affects youth exposure to physical violence. The analysis uses data from a non-experimental impact evaluation and a difference-in-differences approach. Results show a 19-percentage point decline in the incidence of physical violence among youth four years into the program. HSCT-induced enhancements in beneficiary households' purchasing capacity and food security, improvements in caregiver subjective well-being, and reductions in youth participation in economic work for pay could be mediating the program's effects on youth abuse. This paper adds to the relatively scarce evidence on the impacts of anti-poverty policies on young people's susceptibility to physical violence in developing countries.

Keywords

cash transfers; violence against children; child protection; social protection; children and youth; Zimbabwe

I. Introduction

Violence against children can take the form of "physical or mental violence, injury or abuse, neglect or negligent treatment, maltreatment or exploitation, including sexual abuse" (United

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Nations (UN) General Assembly, 1989; UN Committee on the Rights of the Child, 2011). More than one billion children (individuals aged two to 17 years), or approximately half of all children around the world, are likely being subjected to violence every year (Hillis et al., 2016). Often, authority figures such as caregivers and teachers are the perpetrators of physical violence against children, although peers are also common offenders. When it comes to the emotional abuse of children, parents and caretakers are again most likely to be responsible. Young children are particularly vulnerable to sexual abuse by individuals they know such as caregivers, while adolescents tend to be victimized outside their home. Intimate partners are the likely perpetrators of sexual violence against adolescent girls (United Nations International Children's Fund (UNICEF), 2014).

Child abuse is a clear violation of child rights and has detrimental effects on later life outcomes. Experiences with violence in childhood aggravate mental health issues such as depression (Paolucci et al., 2001; Gershoff, 2002); hinder cognitive, social and emotional development (World Health Organization (WHO) & International Society for Prevention of Child Abuse and Neglect (ISPCAN), 2006); affect academic achievement (Paolucci et al., 2001; Ogando Portela & Pells, 2015); and increase the likelihood of being a victim to and/or perpetrator of violence in the future (Paolucci et al., 2001; Gershoff, 2002; Abramsky et al., 2011).

Poverty and other economic factors such as income inequality increase the risk for child neglect and abuse (WHO & ISPCAN, 2006; Hussey et al., 2006; Gilbert et al., 2009; Akmatov, 2011; Cancian et al., 2013; Meinck et al., 2015). Financial deprivation-child maltreatment linkages might emerge, for example, if caregiver stress from economic hardships leads to the neglect or maltreatment of children. Poor families might also use child or early marriage to relieve strains on household resources, which might expose children to abusive situations. Alternatively, youth in impoverished households might resort to risky practices, such as transactional sex, thus increasing their vulnerability to violence (Peterman et al., 2017).

Given that economic deprivation increases children's susceptibility to abuse and maltreatment, child protection programs have often incorporated financial strengthening strategies such as cash and in-kind transfers, skills training, microfinance and support for job searches (Marcus & Page, 2014). In this analysis, we explore whether one such program is able to protect young people from abuse. Specifically, we identify the impacts of a government program providing unconditional cash transfers (CT) to households on youth exposure to physical violence.¹ This paper adds to the relatively scarce evidence on the impacts of anti-poverty policies on young people's susceptibility to physical violence in developing countries. Focusing on physical violence is important since acts such as physical discipline or corporal punishment might constitute one of "the most common form of violence against children" (UNICEF, 2014).² By youth, we refer to individuals aged 13 to 24 years. We are thus able to look at the crucial period of adolescence—a time of major neuro-

¹Children's experiences with physical violence could include "corporal punishment and all other forms of torture, cruel, inhuman or degrading treatment or punishment as well as physical bullying and hazing by adults or by other children". Corporal punishment is punishment with physical force and might involve smacking, slapping, spanking, kicking, shaking, pinching, biting, burning and other similar activities (UNICEF, 2014).

behavioral changes (Dahl et al., 2018) during which negative events such as exposure to violence could substantially shape an individual's developmental trajectory. At the extreme, physical violence could lead to serious injury and in fact, unintentional and intentional injuries are a major cause of mortality among adolescents—accounting for 28 and 44 percent of deaths globally among those aged 10 to 14 years and 15 to 19 years respectively.

Our investigation adds to the literature on the relationship between CTs and violence, the bulk of which has looked specifically at domestic violence against women (Angelucci, 2008; Bobonis et al., 2013; Hidrobo and Fernald, 2013; Hidrobo et al., 2016, Buller et al., 2018; Heath et al., 2018). There are several studies that have examined the effects of social safety nets (SSNs) on youth or child exposure to violence, but while the collated evidence suggest that such programs could protect against sexual violence, it is not clear whether similar benefits exist for physically violent acts such as violent discipline by parents (Peterman et al., 2017).³ The Government of Zimbabwe's Harmonized Social Cash Transfer Program (HSCT) delivers unconditional CTs to food-poor households with high dependency ratios in order to reduce food insecurity and protect orphans and vulnerable children (OVC). Apart from providing transfers, the HSCT addresses child protection by disseminating supplemental information on child wellbeing at payment distribution sites—this information aims to increase sensitivity to violence against children and awareness of social welfare services available for children—as well as by providing support in accessing required services. Moving forward, we refer to the non-cash component of this cash plus program as services or as complementary services.

Soon after the HSCT was launched in 2012, a non-experimental impact evaluation was set up to study the program's effects on household and child well-being. Treatment households started receiving transfer payments after a baseline survey in May 2013 and a 12-month follow-up survey was conducted in July 2014. The first complementary services for children in beneficiary households began just before the 12-month follow-up survey so that the treatment at this follow-up survey essentially only consisted of cash. The 48-month follow-up survey took place in July and August 2017, four years after the cash transfers began and three years after the full suite of complementary services were initiated in treatment sites.

In this analysis, we use data from the three surveys of the HSCT impact evaluation to investigate whether the provision of cash and services changes the exposure of young individuals to physical abuse. We find no significant effects of the HSCT on youth exposure to violence 12-months into the program, but the impact estimates identified are positively signed suggesting increases in violence among treatment youth at the time. Perhaps the recently introduced HSCT complementary services had started sensitizing youth in CT households to issues related to violence and prompted greater reporting of abuse, but the program had not been operating long enough to impact actual levels of violence. In contrast, we detect a 19-percentage point decline in the incidence of violence among HSCT youth at

²The UNICEF (2014) report also summarizes data from the Demographic and Health Surveys (DHS) of multiple countries that highlight that adolescent girls have a higher likelihood of experiencing physical abuse than sexual abuse.

³Promising results on the linkages between SSNs and child or youth exposure to sexual violence tend to come from studies that focus on adolescent girls in Africa (Peterman et al., 2017).

the 48-month follow-up. An investigation into violence perpetrators shows that the overall reductions in abuse are not being driven by specific types of perpetrators.

To confirm that our results do represent the effect of the HSCT on violence reduction, we examine whether the program affects potential mechanisms. We find that beneficiary households increase the purchase of all consumption items as well as food, and experience higher levels of food security than households in the comparison group. The main survey respondents from the treatment group also report higher levels of subjective well-being and appear to be more optimistic about their future. This suggests that the alleviation of financial insecurity and the associated reduction in stress enables caregivers in HSCT households to provide a more protective environment for their youth. The influx of cash also appears to allow households to withdraw their youth from potentially dangerous casual and part-time work settings.

The rest of the paper is structured as follows. Section 2 discusses the linkages between poverty and child abuse. We describe the HSCT program, data, and the study sample and methods in Sections 3, 4 and 5 respectively. Section 6 contains the results and Section 7 covers robustness checks. We conclude with a discussion in Section 8.

II. Background

CTs are currently being used around the world as a major anti-poverty tool, and while their main objective is to mitigate the negative effects of extreme poverty, by specifically targeting a key structural risk factor for child abuse and maltreatment, these programs could also reduce youth exposure to violence. However, while much research has been conducted on the effects of these programs on child nutrition and health (Lagarde et al., 2007; Fiszbein et al., 2009; Leroy et al., 2009; Manley et al., 2013; Owusu-Addo & Cross, 2014; Bastagli et al., 2016; de Groot et al., 2017), and schooling (Fiszbein et al., 2009; Baird et al., 2013; Bastagli et al., 2016), impacts on childhood violence remain relatively unexplored (Peterman et al., 2017). Peterman et al. (2017) develop a framework to understand the pathways through which SSNs might impact emotional, physical and sexual violence against children.⁴ Given that SSNs are usually targeted to households, the authors postulate that they are likely to first influence household-level mechanisms and subsequently trigger caregiver/interpersonal-level and/or child-level pathways.

The household-level pathways are hypothesized to include economic security, labor force participation, time use, intra-household power dynamics and acute/chronic stress levels. To illustrate one way in which these factors could shape the risk of abuse—the alleviation of budget constraints has the potential to reduce stress levels of adults in the household and thereby deter them from physically disciplining young individuals. Alternatively, poverty alleviation within households could prevent youth from resorting to risky practices, such as transactional sex, that might expose them to violence.

⁴The authors classify the following types of schemes as social safety net programs—conditional CTs, unconditional CTs, unconditional in-kind transfers, public work or cash for work programs, and vouchers or fee waivers.

At the caregiver and interpersonal level, SSNs could impact substance use, psychosocial wellbeing, caregiving practices, and conflict/violence within the household. For instance, improved caregiver wellbeing due to the income support could facilitate the adoption of better parenting practices and thereby reduce children's problem behaviors. Child-level pathways encompass time in school, psychosocial wellbeing, time spent in risky settings, child marriage, and problem and risk behaviors. As an example, SSN-induced increases in school participation might cause children to withdraw from hazardous work environments, but might also put them at risk of violence perpetrated by authority figures and peers in school, as well as individuals encountered during commutes to school.

Peterman and colleagues emphasize that the violence effects of the different potential mechanisms are likely to be moderated by contextual factors (such as the established policy, institutional and legal framework) and by household, caregiver and child vulnerability characteristics (examples of which are social isolation and discrimination based on HIV status). SSN program features (like targeting and the specified gender of the recipient) are also bound to determine the extent to which programs are able to affect childhood violence.

The authors go on to review the evidence from 14 studies that explore child experiences of violence in households benefitting from SSNs. They conclude that while such programs have been able to protect adolescent girls in Africa from sexual violence, they have not had consistent impacts in other contexts or on younger children. Where program-driven changes in physical violence have been detected, the populations and specific outcomes examined have differed from the population and measure we are interested in. Two separate investigations into the impacts of an unconditional CT in Ecuador on child development find effects on the use of harsh parenting practices, but these hold only for certain sub-groups. Paxson and Schady (2010) see a reduction in the violent disciplining of children aged three to seven years in the poorest households, and Fernald and Hidrobo (2011) show a marginally significant *increase* in the violent disciplining of children aged one to three years in urban households. Note that the populations of interest in these two studies are much younger than the youth population we focus on. The one study that does point to CT-induced reductions in physical violence among adolescents (within the context of a conditional CT in rural South Africa) focuses just on girls and on abuse by intimate partners (Pettifor et al., 2016). Since caregivers, teachers and peers are common perpetrators of physical violence against youth (UNICEF, 2014), our broader categorization of physical abuse can speak to the potential of CTs to reduce risks from various quarters.

It is worth pointing out here that even though the relationship between CTs and child violence remains fairly understudied (Peterman et al, 2017), multiple studies have examined whether transfer programs have consequences for domestic violence against women (or intimate partner violence (IPV)) and shown that CTs can be a promising tool in reducing the exposure of women to abuse, while highlighting that effects are likely to depend on program features, household characteristics or contextual factors (Angelucci, 2008; Bobonis et al., 2013; Hidrobo & Fernald, 2013; Hidrobo et al., 2016, Buller et al., 2018; Heath et al., 2018).⁵ Given existing evidence on the protective effects of CTs for women, it is important to understand whether such beneficial effects hold for the different types of violence faced by other vulnerable demographics such as children.

III. Program description & study design

Initiated in 2012, the HSCT in Zimbabwe is oriented towards alleviating chronic food insecurity and poverty, and protecting OVCs. The program, which is part of the National Action Plan (NAP) for OVC, intends to harmonize the delivery of child protection interventions and cash transfers to poor households (Carolina Population Center, 2018; de Arruda, 2018).

Jointly funded by the Government of Zimbabwe, donors and UNICEF, the HSCT program targets labor constrained and food poor households. Based on data collected through a census in program wards, the Ministry of Public Service, Labor and Social Welfare (MPSLSW), which implements the program, identifies beneficiary households that meet the eligibility criteria. Households are classified as labor constrained if they do not have an able-bodied member between the ages of 18 and 59 years who has been able to work for more than three months, have a dependency ratio of three or more, or have a severely disabled or chronically ill member needing intensive care. Food poor households are those that fall below the food poverty line and are incapable of meeting essential needs—they consume one or no meals a day; are unable to purchase basic non-food items such as clothing; depend on begging or piece work; do not own valuable assets; and receive no regular support from relatives, pension systems or existing welfare programs.⁶ Labor constrained and food poor households are expected to contain the most vulnerable individuals—the poorest, orphans, elderly, disabled, chronically ill and those living with or affected by HIV (Seidenfeld et al., 2016).

Under the HSCT program, transfers are made to beneficiary households once in two months at designated payment points. The monthly value of the transfer depends on household size—\$10, \$15, \$20 and \$25 for households with one, two, three, and four or more members respectively. Households that are targeted by the program tend to be large (the median household size in the study sample at baseline is five) and so the majority of beneficiary households receive \$25 per month. This is about 20 percent of pre-program monthly household expenditure (American Institutes for Research, 2013).

In order to enhance HSCT's protective effects for children, several non-cash components have been incorporated into the program.⁷ Government agencies and non-government organization (NGO) partners have staff available at HSCT payment points to discuss welfare and protection issues for all children (child abuse is one of the topics covered), but also those

⁵For example, the effects of CTs could vary depending on transfer size (Angelucci, 2008) and women's education (Hidrobo and Fernald, 2013). CT programs tend to provide transfers to women and since resource inequality within households is associated with higher levels of IPV (Cools and Kotsadam, 2017), the CTs could increase domestic abuse.

⁶A household is considered to be food poor if it cannot meet the minimum food energy needs of household members—at least 2,100 kilocalorie per adult equivalent.

⁷Cash plus programs combine CTs with complementary services or interventions that target specific domains of interest. The services that are part of the 'plus' components often differ across programs. For example, the Adolescent Girls Initiative in Kenya provides several interventions such as support for community-driven efforts oriented towards improving the status of girls, and health and financial education for adolescents (Austrian et al., 2016). Another scheme focused on adolescents—the Tanzania Productive Social Safety Net cash plus model for safe transitions to adulthood—implements an intervention that builds livelihood skills and employment opportunities for youth, delivers health and gender equity messaging, and strengthens links to existing government services (Tanzania Cash Plus Evaluation Team, 2018). Some cash plus programs focused on improving child wellbeing incorporate nutrition- and health-focused behavior change communication for mothers and/or other household members (Barry et al., 2017; Ahmed et al., 2019).

that are especially relevant for children with disabilities and/or HIV/AIDS.⁸ Evidence from different settings indicates that conveying information on gender-based or IPV (through entertaining videos screened to groups of individuals) can help change attitudes towards violence (Banerjee et al., 2019), the willingness to report incidents, or even actual levels of violence (Green et al., 2019). Thus, information provided by the HSCT on child wellbeing could play an important role in shaping youth experiences with violence. HSCT payment points also have help desks available to take reports on a variety of cases of children in need of services—for example, those related to disability, to neglect or violence, and to birth registration—and these cases are referred to the appropriate agencies. For example, children with disabilities are directed to the National Case Management system. Efforts are made to link households to the health, education, protection and legal services they require. Finally, community-based volunteers are trained to identify, assist with and monitor children and families that need support.

An impact evaluation study was set up soon after HSCT's launch to identify program impacts on food security, human development, child protection and other outcomes. The study design took advantage of the phased rollout of the HSCT across the country. Treatment households were from wards in early scale-up areas that entered the program in 2013 or Phase two (in the districts of Binga, Mwenzi and Mudzi) and comparison households were from wards in later scale-up areas that would enter the program in phase four, which would begin after the end of the study, and that were matched to the treatment wards (in the districts of UMP, Chiredzi and Hwange).⁹ The primary sampling unit in the study was the ward, the lowest administrative unit of the program. Sixty wards were sampled from intervention areas. These sixty wards were then matched to thirty wards in non-treatment areas on a range of agro-ecological characteristics (geography) and economic conditions (access to services, level of development), for a total of 90 study wards.

Regular program targeting was conducted by the MPSLSW according to HSCT operational guidelines in all the intervention wards as well as in the non-intervention wards. This resulted in a list of eligible beneficiary households in each of the 90 wards selected for the study. From these eligibility lists, the study team randomly selected 34 households per ward to obtain the desired sample size. A total of 3,063 household were ultimately surveyed at the baseline in 2013.

There are several key features of the study design worth highlighting. First, all study households were eligible for the HSCT, regardless of study arm. Second, households in Phase 2 regions entered the program in 2013 shortly after the baseline survey, while households in Phase 4 regions were scheduled to enter the program after the completion of the HSCT evaluation study. Third, delayed entry wards were matched to early entry wards on factors that might affect the evolution of outcomes, such as agro-ecological and economic conditions. Fourth, the targeting of households was supply-driven, and done through a census that listed all households in a ward and captured their information to estimate poverty

⁸The NGO partners are J.F. Kapnek Trust, AfricAid and Childline Zimbabwe.

⁹In fact, due to the subsequent financial crisis in Zimbabwe, the HSCT did not end up being expanded.

scores and demographic composition. Take-up of the program by eligible households was universal (no household refused the transfer) and there were no behavioral conditions.

The implementation of the precise program targeting procedure in all wards (both early- and late-entry) led to an observationally equivalent comparison group—this is demonstrated by the household-level baseline balance tests we perform for the study sample in Table A1 in the Appendix (columns 1 and 2). Given our focus on youth in the current analysis, we also compare treatment and control households with youth members at baseline (columns 3 and 4), and the treatment and control households whose youth members actually participated in the HSCT impact evaluation’s youth module at any survey round, the module that provides the main data for our analysis as described below (columns 5 and 6). These statistics indicate that the HSCT evaluation study design produced a very strong non-experimental counterfactual for the treatment households in the study sample. In the absence of multiple rounds of pre-HSCT data, we cannot, however, formally test whether there were parallel trends across treatment status for our outcome of interest in the years before the program began. The assumption is that since treatment wards were matched to control wards on a wide range of agro-ecological and economic characteristics, they would have progressed similarly in the absence of the HSCT. The baseline survey was conducted in May 2013, treatment households started receiving transfers shortly after, and a 12-month follow-up survey was conducted in July 2014. Some of the complementary services for the children in beneficiary households began just before the first follow-up survey, but the full suite of complementary services began in earnest only after the 12-month survey. The delays were due to the time it took to finalize memorandums of understanding with partner organizations and to develop the protocols for the services being provided. The 48-month follow-up survey took place in July and August 2017. Each of the surveys were conducted after the annual harvest in Zimbabwe and are thus likely to capture household conditions at a time when food stores and resources are at a peak (Carolina Population Center, 2018). Institutional Review Board (IRB) approvals were secured from the American Institutes for Research’s Institutional Review Board and the Medical Research Council of Zimbabwe.

The military coup that ousted longtime president Robert Mugabe in November 2017 and the subsequent general election in Zimbabwe in July 2018 took place after the HSCT study ended. While the years that preceded these major events witnessed substantial political violence, it is important to point out that the study was purposefully designed to include areas dominated by the two major parties in the country, the Zimbabwe African National Union – Patriotic Front (ZANU-PF), the governing party, and the Movement for Democratic Change (MDC), the main opposition party. Thus, any results using the HSCT impact evaluation data are unlikely to be systematically biased by political activities, events or violence occurring in certain regions of Zimbabwe during the study period.

Several studies have documented the impacts of the HSCT on different outcomes, some using the data we employ in the current analysis. Bhalla et al. (2018) document that the HSCT improves food security and diet diversity in beneficiary households. In addition, the authors find that the HSCT enables households to purchase more food from markets and rely less on gifts of food, thereby increasing households’ ability to exercise choices over their food basket. Handa et al. (2018) pull information from several large government

unconditional CTs in sub-Saharan Africa (SSA), including the Zimbabwe HSCT, to show that common negative perceptions regarding CTs do not hold for these programs—for example, they do not lead to higher consumption of temptation goods (alcohol and tobacco), they do not foster dependency, and neither do they create community-level price distortions and inflation. Finally, two multi-country qualitative analyses show that the HSCT, like other CTs in SSA, improves psychosocial wellbeing (Attah et al., 2016), and enhances livelihood choices and productive investments, though these effects are mediated by household vulnerabilities, labor constraints and the availability of economic opportunities (Fisher et al., 2017). Note that no other study systematically investigates how the HSCT has impacted youth wellbeing over time, which is what we do in the current analysis with regard to exposure to physical violence.

IV. Data and measures

At each wave of the HSCT evaluation study, a household survey collected data on a wide range of topics such as education, health, time use, household enterprises, credit, food security, social networks, expenditures and livestock. In addition, a separate youth survey was administered to understand how the program shaped outcomes experienced during the critical period when young individuals are transitioning to adulthood.¹⁰ The youth were asked about their sexual experiences, mental health, alcohol consumption, HIV and experiences with physical violence. These surveys were conducted in private after seeking consent from both parents and respondents (only the respondents) for those 17 years or younger (18 years or older).

The HSCT youth module asked respondents about their victimization to three categories of physical violence during the 12 months prior to the survey:

Has anyone ever slapped or pushed you?

Has anyone ever hit you with a fist, kicked you, or beat you with an object?

Has anyone ever used or threatened to use a knife or other weapon against you?¹¹

Youth who responded to a physical violence question in the affirmative were also asked to specify whether the perpetrator of the last violent act was a parent/adult relative, boyfriend/girlfriend/intimate partner, authority figure (teacher/religious/community leader), peer/classmate, or other actor (for example, stranger). This information was collected separately for each type of violence that was measured.

The primary violence outcome that we examine in this analysis is an indicator variable that captures exposure to any kind of physical violence. The second aggregated measure of violence that we use denotes severe physical violence and it accounts for whether youth were hit with a fist, kicked or beaten with an object, or whether they were attacked or threatened with a knife or other weapon. We also look at the violence categories separately.

¹⁰All HSCT evaluation surveys can be found here: <https://transfer.cpc.unc.edu/countries-2/zimbabwe/>.

¹¹The DHS domestic violence module uses similar questions (UNICEF, 2014), as does the World Health Organization's Violence Against Women Instrument (Ellsberg and Heise, 2005).

In order to understand whether there are any changes in the violence committed by different categories of actors, we use categorical variables for the perpetrator of each type of violence. For brevity, we refer to the five categories as relative, partner, authority figure, peer and other.

As discussed in the background section above, an intervention like the HSCT could shape youth exposure to violence through different channels. The data collected through the household and youth surveys of the HSCT evaluation study allows us to explore whether there were effects on several of these potential channels. Specifically, we investigate household purchases and food security; caregiver subjective well-being and optimism; and youth schooling, work and partnership status. Table A2 in the Appendix describes all the outcomes that we examine in our analysis.

V. Sample and methods

The HSCT study sample at baseline consisted of 3,063 households—2,029 in the treatment group and 1,034 in the comparison group. Of these households, 86 percent and 84 percent were resurveyed at the 12-month and 48-month follow-up surveys respectively. There is no evidence of differential attrition by treatment status during either of the follow-up surveys (see Table A3).

At each survey wave, up to three youth were selected from every household to be interviewed for the youth module; if more than three eligible youth were present, the youngest three were prioritized. At baseline, youth between the ages of 13 and 20 years were surveyed, and at the 12-month follow-up survey, the age range for eligibility for the youth module was adjusted to 14–21 years. At the last follow-up, the age range for the youth module was further adjusted to 13–24 years to capture youth who were originally targeted at baseline, and also to allow for younger children to age into the youth module in anticipation of future data collection. The response rates to the HSCT impact evaluation youth survey was particularly low during the first two surveys—917 of 2,825 eligible youth (or 32 per cent) and 807 of 2,319 eligible youth (or 35 per cent) were interviewed during the baseline and 12-month follow-up surveys respectively. Of the 3,452 youth who were to be interviewed at final follow-up, 2,319 (or 67 per cent) were surveyed. Given the low response rates to the first two rounds of the youth survey and the time gap between the first and last surveys (four years), there was a low likelihood of following the same youth across time—only 73 individuals were surveyed for the youth module at all three survey waves.

Table 1 shows that there were differences in characteristics between youth who were surveyed and eligible youth who were not surveyed at every survey wave—for example, those not interviewed were typically older (the age differences between non-respondents and respondents are statistically significant at baseline and 48-months), and from households with older heads (significant at all waves).¹² However, as demonstrated in Table 2, the

¹²The field work protocol explicitly stated that enumerators were to interview the three youngest members in the eligible age range if more than three were available; this explains why interviewed youth are younger than eligible non-interviewed youth. The individual who we refer to as the household head is the main respondent—the person interviewed for the household (or main) module of the HSCT surveys. The objective was to interview the head of the household.

treatment and comparison group respondents at each wave are well balanced on a range of pre-treatment measures.¹³ Given the comparability of surveyed youth in treatment and control, contrasting young person's outcomes across study arms allows us to approximate how the program shaped different outcomes, though the identified effects likely hold only for the surveyed youth who tend to be younger on average than all youth residing in the study households.

After stacking the youth data from the three survey waves, we estimate the following difference-in-differences (DiD) model:

$$Y_{iht} = \beta_0 + \beta_1 X_{iht} + \beta_2 Z_h + \beta_3 \delta_t + \beta_4 T_h + \beta_5 (\delta_t^* T_h) + \epsilon_{iht} \quad 1$$

where Y_{iht} represents an outcome for youth i living in household h at survey wave t ($t=1$ for the baseline survey, $=2$ for the 12-month follow-up and $=3$ for the 48-month follow-up), X_{iht} are youth characteristics (sex and age) at time t , and Z_h are baseline values of several household characteristics—log household size, the household heads' sex, age, marital status and education, household demographic composition (the number of individuals in different age groups) and indicators for the province of residence. The vector δ_t contains survey round fixed effects and $T_h=1$ if household h resides in a treatment site and $=0$ if it resides in a comparison site. The vector of terms representing the interaction between the treatment variable and each of the time fixed effects is $\delta_t^* T_h$ —its coefficients represent the DiD estimators for program impacts at the two follow-up survey rounds. ϵ_{iht} is the error term for youth i in household h at time t .

We use linear probability models and estimate (1) for all indicator variable outcomes, though later on in the analysis we check whether results are sensitive to estimation with a non-linear model. As discussed in the Data section, in order to examine whether there were changes in the abusive actions of specific types of actors, we use an outcome variable for each type of violence to designate who among the mutually exclusive perpetrator categories was responsible for the last such abusive act against the respondent. Since the perpetrator variables represent unordered categories, we probe these outcomes by estimating (1) with multinomial logit models and present marginal effects or program impacts on the probability of violence by a certain type of actor (Maddala, 1986; Cameron & Trivedi, 2010). When exploring the perpetrators of violence, we restrict the sample to only those victimized by that kind of abuse.

In order to enhance the external validity of our estimates, we account for the incomplete response rates to the youth survey and weight all models with the inverse of the estimated probability of youth response. We adjust the youth weights with household weights. At baseline, household weights were constructed to make the study sample representative of all eligible households in the study regions. These baseline weights were updated at follow-up surveys to account for household attrition using inverse probability weighting (Carolina Population Center, 2018). Finally, we cluster the standard errors in (1) at the level of the primary sampling unit (the ward).

¹³It is also worth emphasizing that data was collected in a standardized way from both treatment and control regions.

We discuss above that the response rate to the youth survey was fairly low at baseline and at 12-months, but that 67 percent of targeted youth were interviewed during the final follow-up. To understand whether the estimates identified by the main DiD specification are driven by low response in the first two surveys, we also compare treatment and control youth only at 48-months with a cross-sectional model. Essentially, this is a single-difference model with the treatment indicator and all the control variables.

VI. Results

Prevalence of violence, and community culture and norms at baseline

We first present summary statistics for youth reports of violence at baseline (Table 3, Panel A). Almost half of all youth report being subjected to some form of physical violence and 24 percent of youth say they met with severe physical violence. The most frequent type of abuse is being slapped or pushed—40 percent of youth face this kind of violence. The most serious abuse—being attacked or threatened with a knife or other weapon—affects four percent of respondents. Relatives and peers are the most common perpetrators of violence against youth (perpetrators for specific types of violence are examined only for those who report facing that kind of abuse). Virtually none of the baseline means of the violence measures are statistically different for treatment and comparison youth.

In 2011, a nationally representative survey in Zimbabwe (the National Baseline Survey on the Life Experiences of Adolescents or NBSLEA) collected data on childhood experiences of violence. Even though the HSCT baseline survey was conducted a few years after the NBSLEA (in 2013), it is informative to compare the prevalence of violence in the current study sample with the NBSLEA sample. When we restrict our focus to HSCT youth aged 13–17 years (since the physical abuse questions in the NBSLEA used the same reference period as the HSCT surveys only for this age group), we find that 44 percent of girls and 55 percent of boys experience some form of physical violence. Prevalence levels are similar among youth aged 13–17 years in the NBSLEA sample—47 percent of girls and 48 percent of boys report experiencing physical abuse (ZIMSTAT et al., 2011) but note that the NBSLEA survey only asked about the violence perpetrated by partners, parents, adult relatives, and authority figures (the HSCT surveys ask about peers and other actors as well).¹⁴

As part of the community-level questionnaire that was administered during the HSCT surveys, information was collected on community culture and norms. We present baseline statistics by treatment status on several of these measures in Panel B of Table 3.¹⁵ Note that none of the community-level differences are statistically significant.

Finally, at the end of Panel B, Table 3, we examine summary statistics on community-level alcohol consumption measures. Unfortunately, these data were not collected at baseline, but

¹⁴Consistent with evidence from multiple settings on the links between poverty and child abuse/maltreatment (WHO & ISPCAN, 2006; Hussey et al., 2006; Gilbert et al., 2009; Akmatov, 2011; Cancian et al., 2013; Meinck et al., 2015), analysis of the NBSLEA data points to low socio-economic status being a risk factor for physical violence among children and youth in Zimbabwe (Izumi & Baago Rasmussen, 2018).

¹⁵The community surveys were administered to a group of community leaders including teachers, village headmen and business owners.

at the first follow-up survey. Arguably these practices would not have changed substantially in one year, particularly since the HSCT did not provide services to deter alcohol consumption, and so we assume that the first follow-up responses on these variables reflect pre-HSCT conditions. Treatment and control wards are balanced on all alcohol consumption-related measures.

HSCT operations

Before estimating the impacts of the HSCT on youth exposure to physical abuse, it is important to understand whether the program was actually implemented as intended. The HSCT impact evaluation included an operational performance module in the household surveys administered at follow-up survey rounds and its data can help gauge household experiences with the cash and service components of the program over time.

About 92 percent of treatment households at each follow-up survey report receiving at least one payment from the HSCT.¹⁶ There is evidence of little to no treatment contamination—only 0.2 (zero) percent of control households at 48-months (12-months) say that they ever received transfers under the HSCT. Recall that the HSCT is supposed to make payments every two months. When asked about payment schedules, 96 (99) percent of currently benefitting households during the final follow-up (12-month follow-up) say they received their last HSCT payment in the previous two months and 78 (91) percent of these households say they expect their next installment within the following two months. These numbers suggest that the CT operations of the program were functioning relatively regularly.

The plus component of the HSCT is oriented towards increasing beneficiary households' understanding of child protection issues, and their awareness and adoption of available child services. If the HSCT did indeed implement its complementary services, we would expect treatment households to fare better than control households on the knowledge and use of child services. We probe whether this is the case in Table A4. The specifications used for these results are single-difference models and are estimated separately for each wave.¹⁷

At first follow-up, we do not detect any program impacts on awareness of services (columns 1 and 3). Given that only some of the HSCT services had started before this survey round, these null results are to be expected. In contrast, beneficiary households at final follow-up are significantly more likely to name at least one resource for children within their community (columns 2 and 4). Treatment households are also significantly more likely to say that they know the point person for child protection issues in their ward—the community childcare worker (columns 5 and 6).¹⁸ We do not, however, find any evidence that the HSCT impacted the use of child services—these estimates are presented in columns 7–10. Despite the lack of effects on this last measure, it is reasonable to assume that the higher awareness

¹⁶We speculate that the small section of treatment households that do not report benefitting from the HSCT could be misreporting this information. No treatment households were excluded from the program during the study period, nor is there evidence of eligible households explicitly refusing to enroll in the program.

¹⁷We look at effects separately for each follow-up round since data on awareness was collected differently across survey rounds—during the 12-month follow-up survey, only households that had a child referred to child protection and support services were asked whether they could name any service for children in their community, such as health services, food/nutrition related services or psychological support; at the 48-month survey, the question on awareness of child services was asked to all households.

¹⁸This question was only asked at final follow-up.

of child services among treatment households versus control households stems from information received at the HSCT payment points. This is supported by information collected only from current program beneficiaries at 48-months—71 percent of household respondents confirm that beneficiaries receive services or information apart from CTs at the HSCT payment points. The evidence presented in this sub-section indicates that HSCT recipient households did receive cash and were likely exposed to at least some complementary services. Any program effects we identify in our analysis should thus be interpreted to be the result of both cash and child support services. Since all treatment households receive cash plus services, it is not possible for us to disentangle the impacts of the different components of the HSCT.

Main results

In Table 4, Panel A, we present the main results of our analysis. All but one of the 12-month impact estimates are positively signed, suggesting that the HSCT might have *increased* youth exposure to physical abuse at this time, but none of these estimates are statistically significant. Recall that some of the HSCT complementary services aimed at enhancing child wellbeing had only just begun to operate before this survey was conducted. These activities could have started sensitizing treatment youth to issues related to violence, thereby prompting greater *reporting* of any abuse that was taking place. However, given that the program—at least the CTs—had been in operation for only a year (and some program components hadn't even been implemented yet), actual impacts on violence may not have been able to materialize by this time.

Turning to the 48-month coefficients, we find that treatment youth are 19 percentage points less likely to face any physical violence than comparison youth at this time, a difference that is statistically significant at the one percent level (column 1). The impact estimate for severe physical violence (column 2) indicates a decline, but the coefficient does not attain statistical significance. Upon examining the different types of violence separately, we find that the results for any physical violence are driven by the first category—being slapped or pushed (column 3), which is the most common and perhaps the least severe kind of violence that was measured. The coefficient on the variable for being attacked or threatened with a weapon (column 5) is marginally significant, but very few youth report facing this type of violence—only four percent at baseline.

As discussed earlier, the response rate to the HSCT youth survey was low during the first two survey rounds. Since a much higher proportion of targeted youth were surveyed at final follow-up (67 percent), contrasting treatment and control respondents at this stage alone would likely be best able to approximate how youth fared under the HSCT. We thus now focus on data from only this wave—in Table 4, Panel B, the 48-month cross-sectional program estimates continue to show that treatment youths experience lower levels of physical violence than control youth and that these benefits are driven largely by reductions in being slapped or pushed.

Next, we probe whether there is any heterogeneity in the physical violence results by gender. At baseline, a higher proportion of boys—52 percent—report experiencing any form of physical violence than girls—44 percent. Table A5 in the Appendix shows that while the

HSCT impact estimates for the different violence outcomes at final follow-up are negatively signed for both groups, the violence deterring effects of the HSCT are larger in magnitude for boys. At 48-months, boys in the treatment group are about 28 percentage points less likely to be victimized by any form of physical abuse; the same figure for girls is one-third the size. It should be noted though that the impact estimates observed for boys in this table are not statistically different from the corresponding estimates for girls despite the large differences in the point estimates.

A growing body of evidence from developing countries suggests that authority figures in school commonly use corporal punishment against students (for example, see Ogando Portela & Pells, 2015; Bashir et al., 2018). In the current study sample, it does seem like schooling is a risk factor for violence—at baseline, a greater share of youth in school (55 percent) report facing physical violence than youth not in school (37 percent). Given that schooling shapes exposure to violence, HSCT's impacts on the incidence of physical violence could very well vary for those in and out of school. We investigate whether there is any such heterogeneity in Table A6 in the Appendix.

In columns 1 to 5 in Table A6, we look for differential effects by contemporaneous schooling (whether or not an individual is currently attending an education institution). At final follow-up, we do not find differences in program impacts for youth in school and out of school with one exception—the HSCT's effect on youth vulnerability to severe physical violence is higher for the former, but this difference is only marginally significant. As we show later, the HSCT had no discernable impacts on educational outcomes, and so the lack of heterogeneity in final program effects on violence by contemporaneous schooling is not surprising. In the remaining columns of Table A6 in the Appendix, we probe whether there is any heterogeneity in the effects on violence by baseline schooling but fail to find evidence of such trends.¹⁹

During the 48-month follow-up, the youth module included several questions about the environment that the respondents are in and their relationship with different individuals. We examine these outcomes here to get a sense of youths' perceptions about their living conditions. Since these questions were not asked in the earlier survey rounds, we cannot use empirical specification (1) to examine these outcomes, but estimate a single-difference model with the treatment indicator and all the usual control variables. Table 5 contains these results. Young individuals in treatment households appear to be significantly less worried about relationships at home and outside, be less likely to witness a parent being subject to IPV and experience lower levels of humiliation by adults. Collectively, these estimates demonstrate that treatment youth are in more protective settings than control youth, and are consistent with our main finding that the HSCT program reduces youth exposure to violence.

Next, we examine whether there are any changes in the types of perpetrators committing violence against youth. We look only at perpetrators who slapped/pushed youth and

¹⁹Since individuals in marriages or relationships might be susceptible to systematically different patterns of violence (such as IPV), we also examine whether there is any heterogeneity in results by current or baseline partnership status (whether an individual is married or co-habiting). There is no evidence of such differential program effects—see estimates in Table A7 in the Appendix.

punched/kicked/beat youth with an object. As highlighted above, only a small proportion of individuals are threatened or attacked with a weapon and we do not have enough variation on the variable capturing perpetrators for this type of violence to achieve model convergence. We present the marginal effects of the HSCT on the probability of violence by different categories of actors in Table 6. There are almost no statistically significant effects.²⁰ This suggests that the declines in physical violence that we observe in Table 4 are not driven by specific types of actors.

Impacts on potential pathways

In the Background section above, we discuss Peterman and colleagues' 2017 framework which highlights the various pathways through which programs like HSCT could influence childhood experiences of violence. In this section, we explore the impacts of the Zimbabwe CT on several of these potential pathways to understand whether they could have played a role in bringing about the observed declines in youth abuse. We separately examine outcomes at the level of the household, the caregiver and the youth.

In order to capture the HSCT's effects on household economic security, we probe the value of per capita monthly household purchases of all items and of food items. For this estimation, we use model (1), but exclude the youth-level control variables of age and gender. Additionally, we restrict our analysis to only those households that had a young individual between the ages of 13 and 24 years at baseline. Table 7, which contains the results of this investigation, shows positive HSCT effects on both purchase measures.²¹ The enhanced purchasing power of transfer-receiving households is likely to improve their control over consumption choices and their sense of self-reliance, conditions that might reduce stress related to income and food insecurity.

During the baseline, the household survey was administered to an individual within each household who was designated as the main respondent. The person most knowledgeable about the surveyed topics (such as health, education and consumption) was selected to be interviewed, usually the household head. Another household member was surveyed only if the head was not available. At follow-up waves, attempts were made to re-interview the same individual as the main respondent. The HSCT evaluation surveys collected data on the main respondent's perceptions regarding household food security, individual wellbeing and future expectations. We probe whether there are program effects on these reports under the assumption that the main respondent is likely to be the caregiver of children and youth in the household, or to be an appropriate proxy for the caregiver. In examining caregiver-level outcomes, we use a model akin to equation (1), except that the outcome and individual-level controls for this part of the analysis are at the level of the main respondent. As in our analysis of household-level pathways, we focus only on households that had at least one member aged 13 to 24 years at baseline.

²⁰The signs on the coefficients suggest that the program might have brought about declines in violence by relatives and peers, but increases in abuse by partners and authority figures.

²¹As mentioned previously, an earlier study documented HSCT's effects on total household consumption from different sources—from purchases, own production and gifts (Bhalla et al., 2018). The analysis demonstrated that recipient households increase consumption from own purchases and reduce their reliance on gifts. This allows households to exercise greater choice in selecting their food basket and leads to improved diet diversity.

In Table 8 we report impacts on the following outcomes—whether main respondents consume three or more meals on a regular basis, the Household Food Insecurity Access Scale (Coates et al., 2007), a Subjective Well-being Scale (adapted from the Satisfaction with Life Scale developed in Diener et al., 1985), whether respondents consider their households to be currently better off than the previous year, and optimism about the future. Most of these measures are indicator variables with the exception of the two scales on which higher values indicate more of the construct being measured—greater food insecurity (higher is worse) and enhanced well-being (higher is better). We describe these variables in more detail in Table A2 in the Appendix.

The estimates presented in Table 8 demonstrate that the HSCT program brings about improvements on all potential caregiver-level mechanisms by final follow-up—caregivers in treatment households consume more meals in a day (column 1), face lower levels of food insecurity (column 2), experience enhanced levels of well-being (column 3), consider their households to be better off than before (column 4) and exhibit higher levels of optimism (column 5).

Do the CTs alter other family dynamics? Any need felt by unmarried caregivers to marry in order to alleviate financial constraints might be precluded once households start receiving CTs. New adult members might perpetrate violence against youth or protect them from other sources of violence, and so the entry of adults into the household could change young individual's susceptibility to abuse. Given the typical profile of HSCT beneficiary household caregivers—elderly women, we do not expect such family compositional changes, or the lack thereof, to play a major role within the HSCT context.²² To confirm this, we present program effects on the likelihood of a caregiver being married or co-habiting with a partner. We focus on the caregiver's partnership status rather than the mother's partnership status since 16 percent of the youth sample are orphans (with the mother or both parents being dead), 24 percent don't have mothers living in the household (marital status was collected only for individuals currently in the household) and 12 percent have aged out of the category for which mother's identity was collected (this group included children aged 18 years and below).²³ Table A8, column 1 looks at program impacts on an indicator variable for whether or not the caregiver has a partner—there are no HSCT effects on this measure.²⁴

Simply looking at marital or partnership status might not be very informative if households are polygamous—a caregiver who has a spouse at baseline and a second spouse at first follow-up would not experience a change in marital status despite there being a change to household composition. This could theoretically be a concern for the context we are studying since Table 3, Panel B indicates that the average community (or ward) has 26 percent of households practicing polygamy at baseline, though note that this information is

²²Table A1 shows that the average household head is a woman above the age of 50.

²³The HSCT specifically sought to protect orphans among other vulnerable children. Hence, it is not surprising that a non-trivial proportion of youth in our sample are orphans. Zimbabwe, like other countries in SSA, has a high number of orphans, largely due to the HIV/AIDS epidemic (Seidenfeld et al., 2016).

²⁴Here we look at the person who is the main respondent during the baseline survey, presumably the household head. While all efforts were made to interview the same individual as the main respondent during the follow-up rounds, this was not always possible.

available only for 67 of the 90 communities included in the study sample (as indicated in the footnotes to Table 3).

Unfortunately, the household surveys administered by the HSCT impact evaluation did not collect information on polygamy within the household. However, it is possible to impute whether the main respondents at each survey round are in polygamous relationships. Information was collected on all current household members' relationship to the main respondent. By counting the number of spouses of the caregiver, we can back out the polygamous status of the main respondent. At every wave, 99 percent of main respondents had zero or one spouse. It is, thus, reasonable to assume that polygamous practices are not shaping HSCT's impacts on youth violence in the study sample. We further bolster this conclusion, by presenting program impacts for several other outcomes capturing household composition in columns 2–4 of Table A8. When probing household size in column 2, we do find a marginally significant decrease at final follow-up, but the estimates in the subsequent columns show no change in the adult composition of households (individuals older than youths) over time.

The last category of potential HSCT-youth violence mediators we look at are youth-level factors. We examine whether the program influences youths' schooling outcomes (current enrolment and recent attendance), work activities (engagement in casual or part-time labor) and partnership status (currently married or co-habiting). See Table A2 in the Appendix for more details on these outcome variables.

Table 9 presents the impacts we identify for potential youth-level pathways using equation (1). The HSCT program does not appear to have impacted youth schooling (columns 1 and 2) and the formation of relationships/marriages (column 4). However, at the 48-month follow-up, young individuals in the treatment group are 12.5 percentage points less likely to engage in casual work than their counterparts in the control group (column 3).²⁵ In other words, the HSCT seems to have allowed households to withdraw their youth from economic work activity.

A concern that arises with viewing the outcomes in Tables 7–9 as plausible pathways through which the HSCT reduced physical violence among youth is that these outcomes might have been determined simultaneously with youth violence or they might even be shaped by violence—for example, parents whose children or youth experience a reduction in their exposure to abuse might have higher subjective well-being. Note though that the HSCT had detectable positive effects on at least the household- and caregiver-level mediators by the 12-month follow-up (save one—column 2 in Table 8 which looks at the Household Food Insecurity Access Scale), whereas effects on youth violence did not materialize until the final follow-up. It is thus reasonable to posit that HSCT shaped youth exposure to violent actions through the household- and caregiver-level measures examined in Tables 7 and 8.

²⁵ Respondents could also potentially have been employed in work for a wage, salary, commission or payment in kind. In practice, however, 97 percent or more of the youth did not engage in such work during any survey wave (the reference period being the week before the survey). There are no statistically significant program effects on this type of work by young individuals.

The results we provide on potential mechanisms corroborate that dynamics within the households are likely to lead to the HSCT-induced declines in youth violence observed in our main analysis. However, these findings cannot speak to whether factors outside the household—for example, improved community conditions as a result of the benefits experienced by multiple households—are also playing a role. In fact, as our investigation into violence by different types of perpetrators shows (Table 6), the overall results do not seem to be driven solely by relatives.

VII. Robustness checks

In this section, we examine whether the main results are sensitive to several changes to the study sample or estimation procedure.

At each survey wave of the HSCT impact evaluation study, data was collected on all members of the study households. In case there was an individual living in the household who had not been there during a previous survey, information was specifically collected about when and why the individual had joined the household. Within the sample that we use for our analysis, we have 442 youth who enter the study sample only at final follow-up (the ‘new’ youth). The inclusion of these individuals in our sample could be problematic because of two reasons. Firstly, youth who have recently joined treatment households might not have had time to individually benefit from the HSCT program.²⁶ We do know when 76 percent of the ‘new’ youth at final follow-up join their households—of these, 66 percent enter their households only the year before or the year of the final survey. Note though that the new individuals in the treatment group would experience the protective effects of the program channeled through household and caregiver pathways since their households would have been receiving transfers since the start of the program. Secondly, the entry of the new youth into the study households could undermine the validity of our results if there are systematically different factors responsible for changing household composition across the treatment and control groups. If the new youth in the control group are more likely to have experienced physical violence prior to joining the households, this could explain the positive program impacts we observe at the second follow-up.

To understand whether the ‘new’ youth in the sample at final follow-up are driving the main results that we identify, we drop these individuals and re-run the violence models for the remaining sample. The impact estimates presented in Table A9 in the Appendix show that this redefinition of the sample does little to change our results.

Recall that we use weights in all our models—essentially to enhance the external validity of our estimates. Are these weights affecting the results? Given that the youth who were surveyed at each wave were balanced across treatment assignment on baseline measures (as shown in Table 2), estimating equation (1) without the weights would be sufficient to identify results that are internally valid. We thus re-examine the violence categories with an

²⁶A subsection of the youth module respondents interviewed at follow-up waves had not been recorded as household members during the HSCT baseline household survey, but were included in the household rosters collected at first follow-up—93 and 92 of those interviewed for the youth module at the 12-month and 48-month surveys respectively. These youth would have been exposed to the HSCT program for at least the three years between the last two surveys.

unweighted model (see Table A10 in the Appendix). As in the main results, we detect a decrease in any youth abuse at 48-months and find that this change is driven largely by a reduction in being slapped or pushed.

While all the primary violence measures that we probe are indicator variables, we use linear probability models (LPM) in our main analysis for ease of interpretation. In Table A11 in the Appendix, we re-examine these outcomes with probit models and find that the results remain unchanged.²⁷

The results also hold if we include youth age fixed effects instead of controlling for age as a continuous variable as in the main specification—in other words, the results are not being driven by patterns of violence specific to certain age groups. Finally, all impact estimates are unchanged if we add province-specific linear time trends to the empirical model to account for potential province-wide linear trends in the patterns of violence. These results are available upon request.

VIII. Discussion

In this paper, we examine whether the Government of Zimbabwe's HSCT, a cash plus poverty alleviation and child protection program, influences the victimization of youth to physical violence. Physical abuse is highly prevalent among the youth population that we focus on—at baseline (2013), 48 per cent of the youth report having been slapped, pushed, punched, kicked, beaten with an object or threatened/attacked with a gun/other weapon in the previous year. The HSCT is not able to significantly shape violence by the 12-month follow-up in 2014, though impact estimates are positively signed suggesting greater youth exposure to physical abuse in CT-receiving households at the time. Perhaps the introduction of the initial HSCT complementary child protection services just before this survey round started sensitizing youth to issues related to abuse and prompted greater reporting of violence. However, given that the program—at least the CTs—had been in operation for only a year, there simply may not have been enough time for actual impacts on violence to emerge. In contrast, the HSCT is able to reduce youth exposure to physical abuse after four years of implementation, decreasing violence by a sizeable 19 percentage points in 2017. Further analyses indicate that enhancements in beneficiary household purchases of food and other items, food security, improvements in caregiver subjective well-being and reductions in youth participation in economic work for pay could be the channels through which HSCT is able to reduce self-reported physical violence among adolescents and young adults.

Cash plus programs, such as the one we study, combine income transfers with complementary interventions. While the interactive effects of the separate components of these programs have the potential to enhance overall impacts in domains of interest, it is worth keeping in mind that the monetary and non-monetary costs of these programs are also higher than those of cash-only programs. Implementing transfers along with different interventions requires higher government capacity, supply-side improvements, training for service providers, and constant coordination among and monitoring of multiple actors. The

²⁷Impact estimates are the same when we estimate logit models (results available upon request).

ultimate efficacy of cash plus programming is likely to reflect how successfully these aspects are maneuvered.

In early 2017, the High Court in Zimbabwe ruled that the corporal punishment of children in homes and schools is a violation of constitutional rights. While the ruling is yet to be approved by the Constitutional Court, the government has said that it is considering passing legislation to ban corporal punishment (Global Initiative to End all Corporal Punishment of Children, 2020). Given that the use of physical force as a means of disciplining children is widely considered to be acceptable in the country (Ndoma, 2017), a legal ban, should it go into effect in the near future, might not be able to reduce physical abuse and punishment of children and youth by itself. However, as the results of the current analysis of a national CT program in Zimbabwe suggest, using cash to alleviate poverty, a structural determinant of child abuse and maltreatment, and complementary services can preclude or limit child exposure to physical violence and the subsequent harms experienced by the victims of abuse.

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Appendix

Table A1:

Treatment-Control baseline balance of households

	All households		Households with youth at baseline		Households contributing youth respondents at various survey rounds	
	Control	Treatment	Control	Treatment	Control	Treatment
<i>Household head's characteristics</i>						
Female	0.66	0.69	0.64	0.69*	0.65	0.69
Age	59.25	56.43	56.24	52.57	53.04	52.81
Widow	0.37	0.37	0.30	0.32	0.30	0.31
Divorced/separated	0.07	0.10	0.07	0.09	0.08	0.09

	All households		Households with youth at baseline		Households contributing youth respondents at various survey rounds	
	Control	Treatment	Control	Treatment	Control	Treatment
Has some schooling	0.61	0.56 [*]	0.67	0.62 [*]	0.64	0.62
Highest grade	3.48	3.27	3.94	3.75	3.79	3.73
<i>Household characteristics</i>						
Household size	5.14	5.03	6.23	6.21	6.21	5.98
Number of individuals 0–5 years	0.81	0.78	0.98	0.90	0.98	0.91
Number of individuals 6–17 years	2.21	2.22	2.86	3.01	2.92	2.88
Number of individuals 18–59 years	1.30	1.20	1.66	1.57	1.57	1.46
Number of individuals 60 and above years	0.82	0.82	0.72	0.73	0.74	0.73
Cares for at least one orphan	0.16	0.16	0.21	0.22	0.20	0.20
Has a chronically ill member	0.37	0.37	0.38	0.37	0.39	0.37
Has a member with a disability	0.28	0.25	0.27	0.26	0.29	0.25 [*]
Per capita expenditure	31.82	30.85	24.71	24.21	25.85	25.57
Share of budget spent on food	0.63	0.63	0.62	0.62	0.62	0.62
Owens or cultivated land in last year	0.92	0.94	0.94	0.96	0.93	0.96
Total land operated (in hectares)	6.12	5.90	6.47	6.92	6.52	5.80
Planted crops during last rainy season	0.87	0.90	0.92	0.94	0.91	0.93
Uses crop production inputs	0.23	0.23	0.25	0.25	0.26	0.25
Owens livestock	0.79	0.77	0.85	0.83	0.85	0.83
Operates non-farm business	0.13	0.11	0.16	0.12	0.16	0.12 [*]
Someone was in wage employment at any time in last year	0.10	0.11	0.12	0.13	0.10	0.12
Receives cash/labor/in-kind aid	0.76	0.67 ^{**}	0.72	0.67	0.72	0.66
Provides cash/labor/in-kind aid	0.23	0.24	0.23	0.26	0.24	0.26
Observations	1,034	2,029	615	1,214	582	1,265

Statistically significant weighted mean differences across groups based on Wald tests are represented by:

p<0.01

**

p<0.05

*

p<0.1.

The standard errors used for these tests are clustered at the ward-level.

Table A2:

Definition of outcomes

Variable	Definition
<i>Main outcomes</i>	
Physical violence	=1 if youth experienced any type of physical violence (being slapped/pushed, hit with a fist/kicked/beaten with an object or attacked/threatened with a knife/other weapon) in the previous 12 months, =0 otherwise
Severe physical violence	=1 if youth experienced any severe physical violence (hit with a fist/kicked/beaten with an object or attacked/threatened with a knife/other weapon) in the previous 12 months, =0 otherwise
Slapped/pushed	=1 if youth was slapped or pushed in the previous 12 months, =0 otherwise
Hit with a fist/kicked/beaten with object	=1 if youth was hit with a fist, kicked or beaten with an object in the previous 12 months, =0 otherwise
Attacked/threatened with knife/other weapon	=1 if youth was attacked or threatened with a knife or other weapon in the previous 12 months, =0 otherwise
Perpetrator the last time youth was slapped/pushed	=1 if parent or adult relative, =2 if a boyfriend or girlfriend or intimate partner, =3 if an authority figure (teacher, religious or community leader), =4 if a peer or classmate, =5 if another individual (for example, a stranger)
Perpetrator the last time youth was hit with a fist/kicked/beaten with object	=1 if parent or adult relative, =2 if a boyfriend or girlfriend or intimate partner, =3 if an authority figure (teacher, religious or community leader), =4 if a peer or classmate, =5 if another individual (for example, a stranger)
Perpetrator the last time youth was attacked/threatened with knife/other weapon	=1 if parent or adult relative, =2 if a boyfriend or girlfriend or intimate partner, =3 if an authority figure (teacher, religious or community leader), =4 if a peer or classmate, =5 if another individual (for example, a stranger)
<i>Supplementary outcomes</i>	
Youth is worried about relationship with people at home	=1 if youth reports being worried about her relationship with people in her home, =0 if she is not worried at all
Youth is worried about relationship with people outside home	=1 if youth reports being worried about her relationship with people she does not live with (such as friends and neighbors), =0 if she is not worried at all
Youth has seen parent being subjected to intimate partner violence at any point in time	=1 if youth has ever seen or heard a parent being punched, kicked or beaten up by the other parent, or their boyfriend or girlfriend, =0 otherwise
Adult humiliated youth in front of others in last 12 months	=1 if an adult said or did something on purpose to humiliate youth in front of others in the last 12 months, =0 otherwise
Adult made youth feel unwanted in last 12 months	=1 if an adult made youth feel unwanted in the last 12 months, =0 otherwise
Adult threatened to abandon/told youth to leave home in last 12 months	=1 if an adult threatened to abandon youth or told her to leave home in the last 12 months, =0 otherwise
<i>Household-level mediators</i>	
Monthly household expenditure per capita - <u>all</u> items	Value of household total expenditure per capita (from purchases) in the four weeks before the survey (in 2017 dollars)
Monthly household expenditure per capita - <u>food</u> items	Value of household food expenditure per capita (from purchases) in the four weeks before the survey (in 2017 dollars)
<i>Caregiver-level mediators</i>	

Variable	Definition
Consumed three or more meals in a day	=1 if main respondent reports regularly consuming three or more meals in a day, =0 otherwise
Household Food Insecurity Access Scale	This scale was developed by the Food and Nutritional Technical Assistance project (FANTA) of the U.S. Agency for International Development (USAID) (Coates et al., 2007). Main respondents are asked whether their households had to do any of the following in the four weeks before the survey due to a lack of resources: worry that their household would not have enough food, not eat preferred foods, consume a limited variety of foods, eat foods they did not want to eat, consume smaller meals than needed, eat fewer meals, have no food to eat, go to sleep at night hungry, and go a whole day and night without eating. Respondents answer to each question with 'No' (a score of zero), 'Rarely' (one), 'Sometimes' (two), or 'Often' (three). The responses to the nine items are summed and the final score ranges from zero (complete food security) to 27 (extreme food insecurity).
Subjective Well-being Scale	This scale is based on responses to seven questions and captures overall life satisfaction (the measure we use is similar to the Satisfaction with Life Scale developed by Diener et al., 1985). Main respondents were presented with the following statements during the interview - 'In most ways my life is close to ideal', 'The conditions of my life are excellent', 'I am satisfied with my life', 'So far I have gotten the important things I want in life', 'If I could live my life over, I would change almost nothing', 'I feel positive about my future', and 'I generally feel happy'. Answer options are on a five-point Likert scale, ranging from strongly disagree (a score of one) to strongly agree (five). The final score lies between seven and 35, with higher values representing more subjective well-being.
Household is better off compared to last year	=1 if main respondent considers her household to be doing better off than 12 months ago, =0 otherwise
Belief that household will be better off in one year	=1 if main respondent thinks that life will be better one year from now, =0 otherwise
<i>Other caregiver/household-level mediators</i>	
Caregiver is married or co-habiting	=1 if main respondent at baseline is currently married or co-habiting =0 otherwise
Household size	Number of household members
Number of female adults (>=25)	Number of female household members aged 25 years or older
Number of male adults (>=25)	Number of male household members aged 25 years or older
<i>Youth-level mediators</i>	
Currently enrolled in an educational institute	=1 if youth is currently attending an educational institution, =0 otherwise
Number of days attended school in the week before the interview	Number of days the youth attended school in the week before the survey
Did any work as a casual/part-time/ <i>maricho</i> laborer in the last 12 months	=1 if youth did this kind of work for anyone who is not a member of the household in the past 12 months, =0 otherwise
Currently married/co-habiting/has a partner	=1 if youth is currently married, co-habiting or has a boyfriend/girlfriend, =0 otherwise

Table A3:

Treatment-Control households remaining at follow-up waves – No evidence of differential attrition

	12-month follow-up		48-month follow-up	
	Control	Treatment	Control	Treatment

Household head's characteristics

	12-month follow-up		48-month follow-up	
	Control	Treatment	Control	Treatment
Female	0.66	0.69	0.66	0.69
Age	59.69	56.23	61.58	56.14
Widow	0.37	0.37	0.37	0.37
Divorced/separated	0.08	0.10	0.07	0.10
Has some schooling	0.61	0.56	0.61	0.56*
Highest grade	3.46	3.27	3.52	3.20*
<i>Household characteristics</i>				
Household size	5.14	5.02	5.15	5.04
Number of individuals 0–5 years	0.80	0.77	0.81	0.78
Number of individuals 6–17 years	2.21	2.22	2.21	2.23
Number of individuals 18–59 years	1.29	1.20	1.29	1.21
Number of individuals 60 and above years	0.83	0.82	0.83	0.82
Cares for at least one orphan	0.16	0.16	0.15	0.16
Has a chronically ill member	0.37	0.37	0.38	0.36
Has a member with a disability	0.28	0.25	0.27	0.26
Per capita expenditure	33.37	30.95	32.36	30.56
Share of budget spent on food	0.62	0.63	0.63	0.62
Owens or cultivated land in last year	0.91	0.94	0.91	0.94
Total land operated (in hectares)	5.77	5.84	6.04	5.62
Planted crops during last rainy season	0.87	0.90	0.86	0.90
Uses crop production inputs	0.24	0.24	0.25	0.23
Owens livestock	0.78	0.78	0.78	0.77
Operates non-farm business	0.14	0.11*	0.13	0.11
Someone was in wage employment at any time in last year	0.11	0.10	0.10	0.10
Receives cash/labor/in-kind aid	0.77	0.66***	0.75	0.67**
Provides cash/labor/in-kind aid	0.23	0.24	0.23	0.24
Observations	882	1,748	842	1,725

Statistically significant weighted mean differences across groups based on Wald tests are represented by:

p<0.01

**

p<0.05

*

p<0.1.

The standard errors used for these tests are clustered at the ward-level. At baseline, 3,063 households were surveyed; 85.9 percent and 83.8 percent of these households were re-surveyed respectively during the 12-month and 48-month follow-up rounds.

Table A4:
Program impacts on awareness and use of child support and protection services

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	Awareness of services				Knows a community childcare worker in the ward of residence [†]		Use of services			
	All households		Households with youth at baseline		All households	Households with youth at baseline	All households		Households with youth at baseline	
Follow-up at:	12-months	48-months	12-months	48-months	48-months	48-months	12-months	48-months	12-months	48-months
Impact	0.005 (0.028)	0.085 ^{**} (0.042)	0.028 (0.037)	0.121 ^{***} (0.045)	0.264 ^{***} (0.032)	0.292 ^{***} (0.042)	0.007 (0.029)	0.024 (0.015)	0.029 (0.038)	0.027 (0.022)
Observations	2,630	2,566	1,602	1,609	2,566	1,609	2,630	2,566	1,602	1,609
R-squared	0.142	0.371	0.154	0.333	0.163	0.152	0.137	0.035	0.155	0.034
Control group mean of dependent variable	0.119	0.388	0.141	0.379	0.336	0.364	0.124	0.084	0.147	0.105

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01
**
p<0.05
*
p<0.1.

Controls include baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible households in the study regions.

[†]This information was collected only at final follow-up.

Table A5:
Impacts of the HSCT program on youth exposure to physical violence, Results by gender

Dependent variable:	GIRLS					BOYS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Physical violence	Severe physical violence	Slapped/pushed	Hit with fist/kicked/beaten with object	Attacked/threatened with knife/other weapon	Physical violence	Severe physical violence	Slapped/pushed	Hit with fist/kicked/beaten with object	Attacked/threatened with knife/other weapon
Treatment	0.065 (0.068)	0.062 (0.062)	0.067 (0.065)	0.057 (0.058)	0.006 (0.023)	0.047 (0.076)	0.009 (0.069)	-0.005 (0.069)	0.010 (0.071)	0.021 (0.020)
12-months	-0.156 ^{***} (0.055)	0.017 (0.069)	-0.231 ^{***} (0.067)	0.030 (0.068)	-0.028 (0.017)	-0.068 (0.070)	-0.051 (0.066)	-0.115 [*] (0.066)	-0.050 (0.065)	0.004 (0.030)
48-months	0.002 (0.050)	0.012 (0.059)	0.019 (0.054)	0.012 (0.056)	-0.008 (0.017)	0.108 (0.078)	0.089 (0.086)	0.006 (0.063)	0.087 (0.092)	0.049 ^{**} (0.025)
12-month treatment impact	0.107 (0.085)	-0.013 (0.087)	0.141 [*] (0.081)	-0.014 (0.083)	0.014 (0.026)	-0.079 (0.083)	0.011 (0.080)	-0.049 (0.081)	0.008 (0.079)	-0.017 (0.036)

Dependent variable:	GIRLS					BOYS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Physical violence	Severe physical violence	Slapped/ pushed	Hit with fist/ kicked/ beaten with object	Attacked/ threatened with knife/ other weapon	Physical violence	Severe physical violence	Slapped/ pushed	Hit with fist/ kicked/ beaten with object	Attacked/ threatened with knife/ other weapon
48-month treatment impact	-0.089 (0.072)	-0.028 (0.073)	-0.136** (0.064)	-0.010 (0.067)	-0.011 (0.028)	-0.279*** (0.092)	-0.179* (0.094)	-0.141* (0.077)	-0.186* (0.099)	-0.066** (0.030)
Observations	1,962	1,962	1,962	1,962	1,958	2,076	2,076	2,075	2,075	2,074
R-squared	0.046	0.044	0.044	0.055	0.017	0.082	0.057	0.057	0.072	0.027
Baseline mean of dependent variable	0.439	0.220	0.362	0.194	0.042	0.517	0.267	0.437	0.254	0.040

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**

p<0.05

*

p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions. All physical violence measures capture abuse in the previous 12 months.

Table A6:

Heterogeneity in HSCT's effects on youth violence by schooling

Dependent variable:	Panel A: Heterogeneous effects by contemporaneous schooling					Panel B: Heterogeneous effects by baseline schooling				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	
	Physical violence	Severe physical violence	Slapped/ pushed	Hit with a fist/ kicked/ beaten with object	Attacked/ threatened with knife/ other weapon	Physical violence	Severe physical violence	Slapped/ pushed	Hit with a fist/ kicked/ beaten with object	
12-month treatment impact	-0.100 (0.096)	-0.138 (0.083)	-0.052 (0.077)	-0.117 (0.080)	-0.032 (0.038)	12-month treatment impact	-0.112 (0.106)	-0.117 (0.094)	-0.033 (0.090)	
12-month treatment impact*Currently enrolled in an educational institute	0.246* (0.148)	0.235* (0.133)	0.252** (0.115)	0.204 (0.133)	0.046 (0.043)	12-month treatment impact*Enrolled in an educational institute at baseline	0.211 (0.147)	0.177 (0.119)	0.166 (0.137)	
48-month treatment impact	-0.244*** (0.076)	-0.219*** (0.072)	-0.165** (0.065)	-0.200*** (0.072)	-0.048 (0.033)	48-month treatment impact	-0.218** (0.091)	-0.169* (0.088)	-0.206** (0.084)	
48-month treatment impact*Currently enrolled in an	0.097 (0.093)	0.184* (0.094)	0.039 (0.093)	0.158 (0.098)	0.009 (0.033)	48-month treatment impact*Enrolled in an	0.100 (0.103)	0.145 (0.101)	0.124 (0.107)	

Panel A: Heterogeneous effects by contemporaneous schooling						Panel B: Heterogeneous effects by baseline schooling				
Dependent variable:	(1)	(2)	(3)	(4)	(5)	Dependent variable:	(1)	(2)	(3)	(4)
	Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object	Attacked/threatened with knife/other weapon		Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object
educational institute						educational institute at baseline				
Observations	4,029	4,029	4,028	4,028	4,023	Observations	3,405	3,405	3,405	3,405
R-squared	0.063	0.051	0.049	0.056	0.011	R-squared	0.052	0.039	0.042	0.042
Baseline mean of dependent variable	0.479	0.244	0.400	0.224	0.041	Baseline mean of dependent variable	0.479	0.244	0.400	0.224

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

*** p<0.01

** p<0.05

* p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Specifications also contain fixed effects for treatment status and survey rounds, and interaction terms between the contemporaneous/baseline school enrolment indicator and these variables. Weights are applied to approximate effects for all eligible youth in the study regions. All physical violence measures capture abuse in the previous 12 months.

Table A7:

Heterogeneity in HSCT's effects on youth violence by partnership status

Panel A: Heterogeneous effects by current partnership status						Panel B: Heterogeneous effects by baseline partnership status				
Dependent variable:	(1)	(2)	(3)	(4)	(5)	Dependent variable:	(1)	(2)	(3)	(4)
	Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object	Attacked/threatened with knife/other weapon		Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object
12-month treatment impact	0.028 (0.062)	-0.011 (0.059)	0.083 (0.064)	-0.008 (0.054)	-0.006 (0.023)	12-month treatment impact	0.007 (0.066)	-0.023 (0.063)	0.073 (0.070)	-0.017 (0.057)
12-month treatment impact*Current marital status	-0.175 (0.224)	-0.122 (0.225)	-0.376 (0.261)	-0.155 (0.214)	0.029 (0.034)	12-month treatment impact*Baseline marital status	-0.069 (0.325)	-0.049 (0.316)	-0.164 (0.357)	-0.116 (0.309)
48-month treatment impact	-0.192*** (0.058)	-0.110* (0.061)	-0.146*** (0.051)	-0.101 (0.062)	-0.046* (0.023)	48-month treatment impact	-0.176** (0.079)	-0.111 (0.081)	-0.154** (0.071)	-0.100 (0.080)
48-month treatment impact*Current marital status	-0.294 (0.218)	-0.306 (0.214)	-0.283 (0.235)	-0.321 (0.214)	0.053* (0.031)	48-month treatment impact*Baseline marital status	-0.415 (0.277)	-0.245 (0.237)	-0.484* (0.273)	-0.278 (0.238)
Observations	4,021	4,021	4,020	4,020	4,016	Observations	2,598	2,598	2,598	2,598

Dependent variable:	Panel A: Heterogeneous effects by current partnership status					Panel B: Heterogeneous effects by baseline partnership status			
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)
R-squared	0.057	0.038	0.051	0.044	0.011	0.060	0.048	0.061	0.056
Baseline mean of dependent variable	0.477	0.243	0.399	0.223	0.041	0.477	0.243	0.399	0.223

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01
**
p<0.05
*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Specifications also contain fixed effects for treatment status and survey rounds, and interaction terms between the current/baseline married indicator and these variables. Weights are applied to approximate effects for all eligible youth in the study regions. All physical violence measures capture abuse in the previous 12 months.

Table A8:

Impacts of the HSCT program on other caregiver/household-level mediators

Dependent variable:	(1)	(2)	(3)	(4)
	Caregiver is married or co-habiting	Household size	Number of female adults (>=25 years)	Number of male adults (>=25 years)
Treatment	-0.022 (0.027)	0.224 (0.176)	0.000 (0.040)	-0.047 (0.036)
12-months	0.014 (0.027)	-0.117 (0.074)	0.017 (0.015)	0.002 (0.018)
48-months	0.001 (0.026)	2.100*** (0.239)	0.048 (0.031)	-0.024 (0.034)
12-month treatment impact	0.002 (0.032)	-0.043 (0.096)	0.009 (0.022)	-0.033 (0.025)
48-month treatment impact	-0.005 (0.032)	-0.431* (0.255)	-0.038 (0.037)	0.041 (0.040)
Observations	4,747	5,048	5,048	5,048
R-squared	0.268	0.157	0.018	0.292
Baseline mean of dependent variable	0.560	6.215	1.144	0.647

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01
**
p<0.05
*
p<0.1.

Controls in column 1 include caregivers' gender and age, and baseline values of the following household characteristics - log household size, main respondent education, household demographic composition and indicators for the province of residence. Columns 2-4 control for baseline values of the following household characteristics - main respondent age, gender, education and marital status, and indicators for the province of residence (baseline demographic characteristics are omitted since the outcome variables are related to household composition). Weights are applied to approximate effects for all eligible households in the study regions. The sample is restricted to household that had youth members at baseline.

Table A9:

Robustness check - Dropping new youth in households

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object	Attacked/threatened with knife/other weapon
Treatment	0.049 (0.050)	0.035 (0.050)	0.026 (0.044)	0.030 (0.048)	0.017 (0.016)
12-months	-0.132 *** (0.044)	-0.014 (0.045)	-0.199 *** (0.052)	-0.011 (0.041)	-0.010 (0.016)
48-months	0.037 (0.040)	0.056 (0.050)	-0.010 (0.037)	0.054 (0.053)	0.025 (0.015)
12-month treatment impact	0.044 (0.062)	0.003 (0.056)	0.078 (0.063)	0.005 (0.051)	-0.005 (0.021)
48-month treatment impact	-0.175 *** (0.056)	-0.106 * (0.062)	-0.121 ** (0.049)	-0.100 (0.063)	-0.042 * (0.023)
Observations	3,596	3,596	3,596	3,596	3,592
R-squared	0.048	0.038	0.041	0.043	0.009
Baseline mean of dependent variable	0.477	0.243	0.399	0.223	0.041

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**

p<0.05

*

p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions. The new youth are those who joined the study households between the 12-month follow-up and the 48-month follow-up surveys.

Table A10:

Robustness check - Dropping youth weights

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object	Attacked/threatened with knife/other weapon
Treatment	-0.004 (0.041)	-0.010 (0.038)	-0.003 (0.038)	-0.012 (0.037)	0.010 (0.014)
12-months	-0.198 *** (0.042)	-0.072 * (0.041)	-0.202 *** (0.044)	-0.062 (0.040)	-0.019 (0.014)
48-months	-0.002 (0.035)	0.009 (0.036)	-0.008 (0.033)	-0.000 (0.037)	0.014 (0.013)
12-month treatment impact	0.100 * (0.059)	0.050 (0.049)	0.089 (0.057)	0.041 (0.047)	0.008 (0.019)
48-month treatment impact	-0.120 ** (0.047)	-0.043 (0.044)	-0.111 ** (0.043)	-0.034 (0.043)	-0.027 (0.017)
Observations	4,038	4,038	4,037	4,037	4,032
R-squared	0.053	0.024	0.048	0.027	0.008
Baseline mean of dependent variable	0.477	0.243	0.399	0.223	0.041

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. All physical violence measures capture abuse in the previous 12 months.

Table A11:

Robustness check - Estimating probit models

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Physical violence	Severe physical violence	Slapped/ pushed	Hit with a fist/ kicked/beaten with object	Attacked/ threatened with knife/other weapon
12-month treatment impact	0.045 (0.061)	0.009 (0.054)	0.082 (0.059)	0.009 (0.049)	-0.003 (0.022)
48-month treatment impact	-0.186 *** (0.060)	-0.106 * (0.061)	-0.135 *** (0.048)	-0.097 (0.061)	-0.041 * (0.022)
Observations	4,038	4,038	4,037	4,037	4,032
Baseline mean of dependent variable	0.477	0.243	0.399	0.223	0.041

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions. All physical violence measures capture abuse in the previous 12 months. We present marginal effects of the treatment at the different follow-up waves.

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Highlights

- A program providing cash transfers and child support services in Zimbabwe reduces youth exposure to physical abuse.
- Physical violence among youth (aged 13 to 24 years) declines by 19 percentage points four years into the program.
- Improvements in household food security and caregiver subjective well-being could be channeling the observed reductions in violence.

Table 1:

Comparison of youth not surveyed and youth surveyed across survey waves

Variable	Baseline		12-month follow-up		48-month follow-up	
	Youth Non-Respondents	Youth Respondents	Youth Non-Respondents	Youth Respondents	Youth Non-Respondents	Youth Respondents
<i>Youth characteristics</i>						
Female	0.50	0.49	0.47	0.51 [*]	0.45	0.47
Age	15.43	15.28 [*]	16.31	16.30	16.98	16.52 ^{***}
Married/cohabiting at baseline [‡]	0.06	0.01 ^{***}	0.04	0.03 [*]	0.02	0.04
Attending school at baseline [‡]	0.63	0.66	0.68	0.70	0.80	0.87 ^{***}
<i>Household head's characteristics (baseline values)</i>						
Female	0.69	0.69	0.67	0.71	0.67	0.68
Age	56.54	50.94 ^{***}	58.28	51.95 ^{**}	54.76	53.43 [*]
Widow	0.31	0.32	0.32	0.29	0.31	0.29
Divorced/separated	0.09	0.08	0.09	0.09	0.08	0.10
Has some schooling	0.63	0.61	0.59	0.65 ^{**}	0.63	0.63
Highest grade	3.71	3.92	3.47	3.85 ^{**}	3.75	3.74
<i>Household characteristics (baseline values)</i>						
Household size	6.53	6.65	6.46	6.42	6.01	6.22 [*]
Number of individuals 0–5 years	0.86	0.86	0.84	0.88	0.82	0.91 ^{**}
Number of individuals 6–17 years	3.25	3.42 [*]	3.21	3.23	2.92	3.06 [*]
Number of individuals 18–59 years	1.64	1.64	1.60	1.55	1.48	1.53
Number of individuals 60 years and above	0.78	0.72	0.81	0.75	0.79	0.72 ^{**}
Cares for at least one orphan	0.16	0.13 [*]	0.17	0.15	0.10	0.11
Has a chronically ill member	0.04	0.04	0.03	0.03	0.00	0.07 ^{***}
Has a member with a disability	0.03	0.02	0.04	0.02 ^{**}	0.18	0.04
Per capita expenditure	22.70	23.61	23.07	24.08	24.28	23.89
Share of budget spent on food	0.61	0.60	0.60	0.60	0.61	0.61
Owens or cultivated land in last year	0.95	0.96	0.97	0.96	0.95	0.95

Variable	Baseline		12-month follow-up		48-month follow-up	
	Youth Non-Respondents	Youth Respondents	Youth Non-Respondents	Youth Respondents	Youth Non-Respondents	Youth Respondents
Total land operated (in hectares)	6.05	8.40	7.13	4.99 ^{**}	6.10	6.60
Planted crops during last rainy season	0.93	0.93	0.95	0.94	0.93	0.92
Uses crop production inputs	0.27	0.28	0.26	0.29	0.30	0.25 ^{***}
Owns livestock	0.84	0.85	0.86	0.83	0.84	0.84
Operates non-farm business	0.14	0.15	0.13	0.14	0.12	0.13
Someone was in wage employment at any time in last year	0.13	0.15 [*]	0.12	0.14	0.11	0.11
Receives cash/labor/in-kind aid	0.65	0.64	0.65	0.67	0.64	0.66
Provides cash/labor/in-kind aid	0.24	0.25	0.24	0.23	0.23	0.25
Observations	1,908	917	1,512	807	1,133	2,319

Statistically significant mean differences across groups based on Wald tests are represented by:

^{***} p<0.01

^{**} p<0.05

^{*} p<0.1.

The standard errors used for these tests are clustered at the ward-level.

[#] These variables were measured at baseline only for those who were recorded as household members at the time. Baseline marital/cohabiting status is available for 75 percent of eligible youth across survey rounds (this information was collected only for those aged 12 years of older), and baseline school enrolment is available for 88 percent of eligible youth.

Table 2:

Comparison of surveyed youth in treatment and control across survey waves

	Baseline		12-month follow-up		48-month follow-up	
	Control	Treatment	Control	Treatment	Control	Treatment
<i>Youth characteristics</i>						
Female	0.48	0.52	0.60	0.62	0.49	0.47
Age	15.33	15.32	16.49	16.15*	17.14	16.74*
Married/cohabiting at baseline [‡]	0.06	0.01	0.06	0.03	0.07	0.04
Attending school at baseline [‡]	0.55	0.63*	0.66	0.69	0.79	0.85*
<i>Household head's characteristics (baseline values)</i>						
Female	0.62	0.67	0.66	0.68	0.66	0.69
Age	54.16	50.16*	49.98	51.52	53.33	52.55
Widow	0.33	0.30	0.29	0.28	0.29	0.28
Divorced/separated	0.05	0.10	0.07	0.09	0.08	0.11
Has some schooling	0.63	0.60	0.71	0.63	0.64	0.64
Highest grade	3.70	3.68	4.19	3.91	3.78	3.82
<i>Household characteristics (baseline values)</i>						
Household size	7.61	7.09	6.84	6.55	6.66	6.47
Number of individuals 0–5 years	1.14	0.86*	1.09	0.82*	1.05	0.98
Number of individuals 6–17 years	3.83	3.67	3.44	3.44	3.20	3.24
Number of individuals 18–59 years	1.87	1.81	1.59	1.56	1.66	1.55
Number of individuals 60 and above years	0.77	0.74	0.71	0.72	0.74	0.69
Cares for at least one orphan	0.15	0.16	0.18	0.13	0.09	0.14**
Has a chronically ill member	0.03	0.03	0.04	0.02	0.04	0.06
Has a member with a disability	0.03	0.04	0.01	0.02	0.04	0.02
Per capita expenditure	23.73	22.11	27.21	25.94	25.11	24.23
Share of budget spent on food	0.60	0.60	0.62	0.61	0.61	0.62
Owns or cultivated land in last year	0.98	0.98	0.93	0.97	0.93	0.96*
Total land operated (in hectares)	10.61	5.87	5.83	4.80	6.72	5.60

	Baseline		12-month follow-up		48-month follow-up	
	Control	Treatment	Control	Treatment	Control	Treatment
Planted crops during last rainy season	0.96	0.94	0.90	0.95	0.91	0.92
Uses crop production inputs	0.31	0.27	0.28	0.29	0.24	0.23
Owns livestock	0.88	0.87	0.78	0.85	0.87	0.84
Operates non-farm business	0.22	0.13*	0.19	0.09**	0.15	0.10
Someone was in wage employment at any time in last year	0.16	0.15	0.16	0.14	0.12	0.11
Receives cash/labor/in-kind aid	0.72	0.64	0.75	0.66	0.73	0.66
Provides cash/labor/in-kind aid	0.23	0.30	0.23	0.27	0.25	0.25
Observations	273	644	253	554	722	1,597

Statistically significant weighted mean differences across groups based on Wald tests are represented by:

p<0.01

**
p<0.05

*
p<0.1.

The standard errors used for these tests are clustered at the ward-level.

‡ These variables were measured at baseline only for those who were recorded as household members at the time. Baseline marital/cohabiting status is available for 64 percent of youth respondents across waves (this information was collected only for those aged 12 years of older), and baseline school enrolment is available for 84 percent of youth respondents.

Table 3:

Treatment-Control balance on youth violence measures, and community culture and norms at baseline

<i>Panel A: Youth violence</i>			
Variable	All youth	Control group	Treatment group
Physical violence	0.48	0.44	0.49
Severe physical violence	0.24	0.23	0.25
Slapped/pushed	0.40	0.37	0.41
Hit with fist/kicked/beaten with object	0.22	0.21	0.23
Attacked or threatened with knife/other weapon	0.04	0.03	0.05
<i>Perpetrator - Slapped/pushed</i>			
Relative	0.29	0.31	0.29
Partner	0.05	0.09	0.04
Authority figure	0.24	0.25	0.24
Peer	0.36	0.31	0.38
Other	0.05	0.04	0.05
<i>Perpetrator - Hit with fist/kicked/beaten with object</i>			
Relative	0.32	0.25	0.35
Partner	0.06	0.13	0.04
Authority figure	0.23	0.32	0.19*
Peer	0.33	0.26	0.36
Other	0.06	0.05	0.06
<i>Perpetrator - Attacked or threatened with knife/other weapon</i>			
Relative	0.44	0.45	0.43
Partner	0.04	0.00	0.06
Authority figure	0.05	0.00	0.06
Peer	0.33	0.25	0.36
Other	0.14	0.30	0.10
Observations	917	273	644
<i>Panel B: Community culture and norms</i>			
Variable	All wards	Control wards	Treatment wards
Percentage of households practicing different religions			
Traditional	26.37	23.65	27.68
Christianity	65.03	66.46	64.34
Islam	0.62	0.00	0.93
Combination of traditional and other	7.66	6.04	8.44
Percentage of households that are polygamous [†]	26.13	19.30	29.25
Possible for wives to inherit property from husbands	0.99	0.96	1.00
Possible for widows to be inherited by brother/other male relative of the deceased	0.26	0.15	0.31
More than half of secondary school aged children are enrolled in school			

Boys	0.63	0.60	0.64
Girls	0.60	0.60	0.59
Orphans	0.47	0.44	0.49
Alcohol is available within the community [‡]	0.81	0.79	0.82
More than half the men drink alcohol regularly [‡]	0.28	0.34	0.25
More than half the women drink alcohol regularly [‡]	0.01	0.00	0.02
Drinking is a problem in the community [‡]	2.12	1.97	2.20
Observations	80	26	54

Statistically significant mean differences across groups based on Wald tests are represented by:

p<0.01

**
p<0.05

*
p<0.1.

Youth-level statistics are calculated using weights. Standard errors used for these tests are clustered at the ward-level.

[‡]Information is available only for 21 control wards and 46 treatment wards.

[‡]Collected during the 12-month follow-up survey only.

Table 4:

Impacts of the HSCT program on youth exposure to physical violence

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Physical violence	Severe physical violence	Slapped/pushed	Hit with a fist/kicked/beaten with object	Attacked/threatened with knife/other weapon
<i>Panel A: Main specification - difference-in-differences using data from three survey rounds</i>					
Treatment	0.057 (0.051)	0.039 (0.050)	0.031 (0.044)	0.033 (0.048)	0.018 (0.016)
12-months	-0.131 *** (0.043)	-0.019 (0.045)	-0.194 *** (0.051)	-0.015 (0.042)	-0.010 (0.016)
48-months	0.052 (0.049)	0.050 (0.056)	0.011 (0.039)	0.048 (0.058)	0.023 (0.015)
12-month treatment impact	0.041 (0.061)	0.006 (0.056)	0.075 (0.062)	0.007 (0.052)	-0.005 (0.021)
48-month treatment impact	-0.189 *** (0.062)	-0.109 (0.067)	-0.141 *** (0.049)	-0.102 (0.068)	-0.042 * (0.022)
Observations	4,038	4,038	4,037	4,037	4,032
R-squared	0.053	0.035	0.046	0.042	0.009
Baseline mean of dependent variable	0.477	0.243	0.399	0.223	0.041
<i>Panel B: Cross-sectional specification using only 48-month data</i>					
48-month treatment impact	-0.131 *** (0.037)	-0.069 * (0.037)	-0.108 *** (0.028)	-0.067 (0.041)	-0.023 * (0.012)
Observations	2,317	2,317	2,316	2,316	2,312
R-squared	0.052	0.037	0.045	0.044	0.010
Control group mean of dependent variable	0.441	0.252	0.344	0.232	0.056

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions. All physical violence measures capture abuse in the previous 12-months.

Table 5:

Impacts of the HSCT program on youth environment at 48-month follow-up survey

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Youth is worried about relationship with people at home	Youth is worried about relationship with people outside home	Youth has seen parent being subjected to intimate partner violence at any point in time	Adult humiliated youth in front of others in last 12 months	Adult made youth feel unwanted in last 12 months	Adult threatened to abandon/told youth to leave home in last 12 months
48-month treatment impact	-0.067** (0.030)	-0.070** (0.034)	-0.038*** (0.014)	-0.067*** (0.024)	-0.030 (0.026)	-0.013 (0.014)
Observations	2,315	2,315	2,297	2,297	2,304	2,304
R-squared	0.060	0.052	0.028	0.028	0.028	0.034
Control group mean of dependent variable	0.476	0.525	0.095	0.209	0.166	0.098

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions.

Table 6:

Impacts of the HSCT program on violence perpetrated against youth by specific types of actors (multinomial logit models)

	(1)	(2)
	Slapped/pushed	Hit with a fist/kicked/beaten with object
12-month treatment impact		
Relative	-0.149 (0.115)	-0.198* (0.111)
Partner	0.148** (0.072)	0.150 (0.093)
Authority figure	0.060 (0.121)	0.050 (0.175)
Peer	-0.095 (0.084)	-0.131 (0.130)
Other actor	0.036 (0.052)	0.130** (0.060)
48-month treatment impact		
Relative	-0.010 (0.091)	-0.155 (0.107)
Partner	0.102* (0.061)	0.144 (0.091)
Authority figure	0.039 (0.087)	0.135 (0.105)
Peer	-0.081 (0.075)	-0.056 (0.095)
Other actor	-0.050 (0.045)	-0.068 (0.043)
Observations	1,201	711
Baseline mean - violence by relative	0.294	0.319
Baseline mean - violence by partner	0.053	0.066
Baseline mean - violence by authority figure	0.244	0.229
Baseline mean - violence by peer	0.363	0.331
Baseline mean - violence by other actor	0.046	0.055

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions. Data was collected on the perpetrator who committed the last act of a specific type of violence in the previous 12-months.

Table 7:

Impacts of the HSCT program on household-level mediators

Dependent variable:	(1)	(2)
	Monthly household purchases per capita - <u>all</u> items	Monthly household purchases per capita - <u>food</u> items
Treatment	-0.159 (0.918)	-0.134 (0.557)
12-months	1.711 ** (0.720)	0.610 (0.463)
48-months	5.863 *** (0.547)	2.433 *** (0.355)
12-month treatment impact	2.298 *** (0.854)	1.401 ** (0.553)
48-month treatment impact	2.828 * (1.526)	2.125 (1.298)
Observations	5,040	5,040
R-squared	0.028	0.011
Baseline mean of dependent variable	11.022	3.703

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible households in the study regions. The sample is restricted to households that had youth members at baseline.

Table 8:

Impacts of the HSCT program on caregiver-level mediators

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Consumed three or more meals in a day	Household Food Insecurity Access Scale	Subjective Well-being Scale	Household is better off compared to last year	Belief that household will be better off in one year
Treatment	-0.003 (0.019)	-0.007 (0.512)	-0.062 (0.323)	-0.016 (0.024)	-0.010 (0.028)
12-months	0.193 *** (0.023)	-3.488 *** (0.605)	1.244 *** (0.313)	0.088 *** (0.031)	0.174 *** (0.039)
48-months	0.287 *** (0.021)	-2.802 *** (0.542)	2.676 *** (0.405)	0.266 *** (0.040)	0.146 *** (0.025)
12-month treatment impact	0.083 ** (0.034)	0.042 (0.750)	0.802 ** (0.394)	0.394 *** (0.038)	0.170 *** (0.044)
48-month treatment impact	0.072 ** (0.034)	-1.839 ** (0.728)	0.865 * (0.464)	0.224 *** (0.046)	0.188 *** (0.035)
Observations	5,035	4,691	5,031	5,035	4,977
R-squared	0.115	0.084	0.091	0.144	0.070
Baseline mean of dependent variable	0.291	14.013	9.725	0.103	0.242

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include caregiver age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible households in the study regions. The sample is restricted to households that had youth members at baseline.

Table 9:

Impacts of the HSCT program on youth-level mediators

	(1)	(2)	(3)	(4)
Dependent variable:	Currently enrolled in an educational institute	Number of days attended school in the week before the interview[†]	Did any work as a casual/part-time/<i>maricho</i> laborer in the last 12 months	Currently married/co-habiting/has a partner
Treatment	0.061 (0.044)	0.130 (0.252)	0.045 (0.060)	-0.020 (0.041)
12-months	-0.013 (0.071)	0.480 (0.291)	-0.069 (0.051)	0.052 (0.062)
48-months	0.075 ** (0.034)	0.156 (0.259)	-0.011 (0.052)	-0.004 (0.046)
12-month treatment impact	0.018 (0.077)	-0.167 (0.329)	-0.029 (0.061)	-0.007 (0.066)
48-month treatment impact	-0.008 (0.044)	-0.227 (0.315)	-0.125 ** (0.063)	-0.012 (0.049)
Observations	4,034	1,930	4,025	4,041
R-squared	0.355	0.030	0.110	0.264
Baseline mean of dependent variable	0.606	4.250	0.245	0.147

Robust standard errors presented in parentheses are adjusted for clustering at the level of the ward of residence.

p<0.01

**
p<0.05

*
p<0.1.

Controls include youth age and gender, and baseline values of the following household characteristics - log household size, main respondent age, gender, education and marital status, household demographic composition and indicators for the province of residence. Weights are applied to approximate effects for all eligible youth in the study regions.

[†] In examining this outcome, the sample is restricted to those attending an educational institute that was not closed for holiday during the reference period.