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Direct and Spillover Impacts of a Community-Level HIV/AIDS Program: Evidence from a Randomized Controlled Trial in Mozambique

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Abstract

We study a prominent effort to help families cope with HIV/AIDS: a U.S. government-funded program in Mozambique implementing an interrelated set of health and education interventions. We report the results of a randomized evaluation of this program. We hypothesize that the program raises HIV testing rates by reducing imperfect information (related to HIV) and by reducing HIV-related stigma concerns. Primary outcome variables are survey-reported and directly-observed rates of HIV testing. We also examine a range of secondary outcomes related to information and stigma mechanisms, and secondary outcome variables such as school attendance and overall household well-being. Causal identification exploits multilevel random assignment (of communities to program receipt, and of treatment-community households to strong encouragement for program participation), allowing separate identification of impacts on households of being in a program community (with take-up at the community-average rate), versus having been strongly encouraged to participate the program (with very high take-up). We also examine spillovers (based on geographic proximity or social network connections) from strongly encouraged to other households. Additional treatments randomly assigned by our research team allow insight into complementarities with interventions providing targeted information and financial incentives for HIV testing.

1. Introduction

Out of an estimated 36.9 million people living with HIV worldwide in 2017, 25.7 million are in Sub-Saharan Africa. The region also accounts for a dominant share of new HIV infections: 1.17 million out of a global 1.8 million in that year. In Mozambique in 2017, 2.1 million people out of a population of 29.7 million were living with HIV (7.1% of the population), out of which 170,000 were children (aged 14 or below). The country has an estimated 130,000 new HIV infections annually, of which 13.8% are children. Mozambique recorded 70,000 AIDS-related deaths in 2017, likely because only slightly more than half of HIV-infected patients have access to anti-retroviral therapy (ART). Poor access and adherence to ART contributes to AIDS-related morbidity and mortality, as well as HIV transmission (to other adults as well as from mothers to children) (UNAIDS 2018).

National governments and the global development community have been pursuing a wide variety of programs to combat the HIV/AIDS pandemic. Across the broad scope of such programs, efforts to facilitate and promote HIV testing play a central role. HIV testing is a central focus because, first of all, testing is the prerequisite for being diagnosed as HIV positive and thereafter initiating life-saving anti-retroviral therapy. Furthermore, HIV is typically asymptomatic for years before the disease progresses to AIDS and symptoms become apparent. During the asymptomatic phase of the disease, HIV testing leading to treatment via ART at this early stage has substantial benefits on two key dimensions. First, treatment leads to lower viral loads and thus a much lower likelihood of transmission to sexual partners (Cohen et al 2013). Second, an HIV-positive individual's longer-run adherence to treatment and health outcomes are better when treatment occurs at an earlier stage of the disease (Ford et al 2018).

It is important to understand the extent to which various programs raise rates of HIV testing, but it is just as important to understand the mechanisms through which they do so. An understanding of mechanisms can shed light on the underlying market failures, and thereby help guide future policy in the HIV/AIDS realm (and potentially other related policy areas). Of particular relevance in this context, we are interested in whether improvements in information in beneficiary households are an important mechanism through which direct and spillover effects of such programs operate. If so, this finding can motivate future policies to address informational market failures (and future research to delve more deeply into them). Another mechanism through which program effects may operate is via reductions in HIV-related stigmatizing attitudes in the social network. If such programs reduce general stigmatizing attitudes, there may be increases in willingness to be tested for HIV and to initiate and adhere to HIV treatment regimens. The role of stigma in inhibiting health care utilization has not been widely studied in economics, in the HIV/AIDS context or elsewhere.

Among programs that have promotion of HIV testing as a primary aim, a common theory of change runs as follows. Imperfect information on HIV and on HIV treatment, and concerns about HIV-related stigma, reduce rates of HIV testing. Reducing HIV-related imperfect information and concerns about HIV-related stigma concerns can thereby raise HIV testing rates. Programs therefore commonly aim to reduce informational imperfections related to HIV, and to ameliorate concerns about HIV-related stigma, hoping thereby to raise rates of HIV testing. The program we study in Mozambique, *Força à Comunidade e Crianças* (FCC, "Strengthening Communities and Children"), has such a theory of change, and therefore includes such information-provision and anti-stigma elements as central features, for the most part delivered via home visits to households in villages where the program operates. In our study population, while in recent years public health initiatives have led to substantial increases in HIV testing rates, nearly half of adults and almost 90% of children have never been tested for HIV; there thus remains a great deal of room for improvement in HIV testing rates.

Our study exploits random assignment of household exposure to the FCC program to: 1) estimate the impact of such exposure on household HIV testing rates, and 2) reveal whether any such improvements operate via improvements in HIV-related information and reductions in HIV-related stigma concerns. In addition, given that not all households in a community directly benefit from the program, we will seek to quantify the extent to which impacts spill over from directly-affected households to others that are geographically or socially proximate. Finally, we investigate the extent to which the FCC program complements or substitutes for more targeted public health programs (such as information dissemination or financial incentives for HIV testing).

Our approach involves a three-part randomized controlled trial methodology. First, communities were randomly assigned to treatment or control status (inclusion in or exclusion from the FCC program). Second, a subset of households within treatment communities were randomly assigned to a strong

encouragement to participate in FCC programs ("directly enrolled" households). These directly enrolled households receive a home visit by an FCC program community worker and are assessed for inclusion in various FCC subcomponent programs. This will lead them to have higher participation rates in the program than other households in treatment communities. Other households not randomly selected for direct enrollment may end up being treated as well, but at lower rates. These first two randomization components were carried out in 2017, and have led to varying household exposure to the FCC program throughout 2018. They allow us to shed light on direct impacts, and to quantify spillovers.

The third part of the randomized methodology is currently being implemented alongside the endline survey, and is aimed at shedding light on the extent to which the FCC program complements or substitutes for more targeted public health interventions. We are interested in complementarity with public health information interventions focused on improving *information about HIV*, improving *information about HIV treatment* (antiretroviral therapy, or ART), and reducing *concerns about HIV-related stigma*. We are also interested in complementarity with *financial incentives for HIV testing*. We randomly assign simple treatments at the household level that our project staff administer immediately after the ongoing endline survey. (These treatments are detailed in the next section.) Complementarities could be positive or negative (the FCC program could magnify or reduce the impact of such later more targeted interventions). If these treatments are found to have smaller effects on directly-enrolled households in treatment communities than on households in control communities, this would be evidence that the FCC program and these more targeted interventions are substitutes. It is also possible that these treatments could be complementary with the FCC program: they could have *larger* effects on directly-enrolled households in treatment communities than on households in control communities.

A baseline survey was previously administered that defines a sample to follow over time through the upcoming endline survey, and that measured geographic and social proximity between households.

Our primary analyses will test whether household assignment to strong encouragement for participation in the FCC program (which we refer to as directly-enrolled beneficiary or "DEB" status) leads to higher rates of HIV testing in the household. HIV testing is the outcome variable of primary focus because it is a prerequisite for benefiting from the FCC program in the health domain.[1] HIV testing opens the door to FCC interventions promoting ART treatment initiation and adherence. In addition, HIV testing is emphasized and encouraged in the context of major FCC program components (OVC home visits by community workers, and school-based programs). The more specialized and narrower program components (such as VSLAs and youth groups) also systematically reinforce the importance of HIV testing.

In addition to measures of HIV testing, we will also examine a variety of outcomes in secondary analyses. Outcomes include ones reported by households in an endline household survey, as well as directly-observed school attendance in attendance checks by our staff. These secondary analyses will shed light on participation in other program components such as village savings and loan associations (VSLAs) and youth groups of different kinds, school attendance, and summary measures of life satisfaction and household economic status.

Secondary analyses will also shed light on mechanisms through which the FCC program has its effects, by examining impacts on survey-reported knowledge (related to HIV and ART), HIV testing and treatment, HIV-related stigmatizing attitudes, and sexual behavior. The question of operative mechanisms is central in evaluation of bundled or multifaceted programs. Program cost-effectiveness can be improved by focusing on program subcomponents that are driving overall impacts, and eliminating elements that are relatively ineffective. It was not feasible in our study to randomly assign study localities to different subsets of FCC program components. Secondary analyses using endline survey data will also reveal the extent to which different households participated in or were exposed to different program subcomponents. Analysis of heterogeneity of treatment effects across households exposed to different program subcomponents suggest which program subcomponents were behind program impacts. While this latter

[1] Our primary outcome variables measure HIV testing for both adults and children. The health of adults (in particular, their HIV status) is an important determinant of the outcomes of children in their households; HIV testing can lead adults to learn they are HIV positive, leading them to initiate ART, with positive effects on children in their households. When it comes to children (those aged below 18), HIV testing is important as well, most importantly after puberty and sexual debut leads to non-trivial rates of new HIV infection. There are also much smaller but nonzero rates of HIV infection from mothers (or other household members) to younger children.

analysis will be observational and therefore cannot establish causality, it can provide guidance to future studies that could prospectively randomize different program subcomponents to establish their separate effects.

Secondary hypotheses also involve spillovers from strongly-encouraged (DEB) households to other households via mechanisms related to HIV information, ART information, and reductions in concerns about HIV-related stigma. Design of community-level programs must consider optimal penetration or density of program coverage within the population. It is therefore crucial to understand spillover or indirect benefits to those who are not directly included in the program, as higher spillovers can imply lower optimal enrollment of the population as direct program beneficiaries.

The secondary analyses also include our third dimension of randomization (treatments randomized at the household level and implemented after the endline survey), which will shed light on the extent to which the FCC program is complementary or substitutable for more targeted public health treatments (improved HIV-related information, information reducing concerns over HIV-related stigma, and financial incentives for HIV testing).

This research is connected to a number of research areas in economics. First, the FCC program is an important example of bundled or multifaceted development programs more generally (beyond the realm of public health interventions). While multifaceted development programs have been increasingly studied in economics in recent years,[2] programs such as FCC that are centered around combating the HIV/AIDS crisis have received limited attention from economists. Multifaceted development interventions are motivated by the possibility of mutual reinforcement or positive complementarities between interventions – in other words, that interventions in a particular realm may have greater impact when combined with one or more interventions in another realm.[3]

In the context of studies of community-level, multifaceted intervention to combat the HIV/AIDS crisis, use randomized controlled trial methodologies is rare. Prior studies of PEPFAR programs have not exploited prospectively randomized research designs, and instead have relied on retrospective analysis with control or comparison groups that were not randomly selected. Relatedly, past studies have not tracked defined groups of individuals over time (from before to after program implementation), raising additional concerns about sample selection biases (Bryant et al 2012). Bendavid et al (2012) examine the impact of PEPFAR funding at the country level using a difference-in-difference approach, finding substantial reductions in adult mortality in Africa. A number of past studies have used randomized controlled trials to examine the impact of narrower, more targeted interventions related to HIV/AIDS (such as Thornton (2008), McCoy et al (2017), Ssewamala et al (2009), Ivers et al (2014), Baird et al (2011), Kiene et al (2017), and Yotebieng (2017)).

Our work is also related to past research on spillovers within social networks. Research on spillovers of health interventions using randomized research designs in developing countries include Miguel and Kremer (2004), Dupas (2014), and Oster and Thornton (2012). Studies of spillovers of non-health interventions include Conley and Udry (2010), Ligon and Schechter (2012), Carter et al (2014), Beaman et al (2015), and Angelucci and De Giorgi (2009).

2. Research Design

A. Interventions

The U.S. Government's most important program responding to the HIV/AIDS crisis is the President's Emergency Plan for AIDS Relief (PEPFAR), initiated in 2003. Recognizing that children are among the most vulnerable populations in the context of the HIV/AIDS pandemic, PEPFAR mandates part of its funding be devoted to programs benefiting children orphaned or made vulnerable by HIV/AIDS ("orphans

[2] Randomized research designs have recently been used to study "ultrapoor" programs, which involve combinations of interventions such as resource transfers, formal financial services, and education and skill development (Banerjee et al (2015), Bandiera et al (2017), and Blattman et al (2016)), but none of these take as their central focus helping households cope with the HIV/AIDS crisis or directly promote HIV testing and treatment.

[3] The approach has a long history, however, for example in the popularity of "integrated rural development" programs in the 1960s through the 1980s (reviewed by De Janvry 2004). More recently, Sachs (2005) made the case for multifaceted development programs in an argument for major increases in aid to developing countries.

and vulnerable children," or OVCs).[4,5] PEPFAR's programs for OVCs take an integrated approach, with interventions at child, family, and community levels; that target child needs at different developmental stages; and that are connected to other development programs related to education, nutrition, and household economic development (PEPFAR 2006). In fiscal year 2016, PEPFAR OVC programs supported 6.2 million OVCs and their caregivers worldwide (PEPFAR 2017).[6]

The program we study, *Força à Comunidade e Crianças* (FCC, "Strengthening Communities and Children"), is a multi-year PEPFAR OVC program. Its high-level aim is to improve families' and communities' ability to support, protect, and care for orphans and vulnerable children, their caregivers, and their households more generally.

While the FCC program is multifaceted and can affect many possible outcomes, this study focuses its primary analyses and hypotheses on two central outcome variables, the central program component, and a subset of mechanisms (intermediate outcomes) through which effects may operate. From this perspective, we will measure the program's overall impacts, measure spillovers from program beneficiaries to other households, and provide suggestive evidence on mechanisms through which the program achieves its impacts. Other outcomes, program components, and mechanisms will be the subject of secondary analyses, which can provide guidance for the foci of future studies.

To be specific, our primary focus is on the following:

- **Outcome variables**: Having been tested for HIV in past 12 months (self-reported); take-up of a recommendation to get a new HIV test (directly observed)
- **Program component**: Home visits by LIP staff (Case Care Workers, or CCWs)
- **Mechanisms / intermediate outcomes**: Information on HIV; information on antiretroviral therapy (ART); concerns about HIV-related stigma

HIV testing is the outcome variable of primary focus because it is the first, prerequisite step in the chain that then leads to initiation of HIV treatment (antiretroviral therapy, ART) and ART adherence. The importance of HIV testing is strongly emphasized in the most central and widespread program components (OVC home visits by community workers, and school-based programs) via information provision and efforts at reducing HIV-related stigmatizing attitudes. The more specialized and narrower program components (such as VSLAs and youth groups) also systematically reinforce the importance of HIV testing.

We now describe the FCC program, highlighting in detail the outcome variables, program component, and mechanisms (intermediate outcomes) of primary interest. Other aspects of secondary interest will be described in less detail.

The FCC program is composed of a number of interrelated components, and is implemented in study districts by local implementing partner (LIP) organizations under contract to the international NGO World Education Inc./Bantwana. A number of FCC program components are school-based, and so programs are implemented in local communities surrounding a focal school. Some components are focused on children, others on adults. In each community, activities take place with the collaboration and advice of a Community Child Protection Committee (CCPC) whose membership includes community leaders, volunteers, and local government officials. The program is implemented in seven districts of three provinces of Mozambique.[7]

The most widespread FCC program component is **home visits** by LIP staff known as "Case Care Workers" (CCWs) to households in program communities. Roughly 700 CCWs work across the study communities. LIPs hire CCWs from the communities they serve, in part based on recommendations by the CCPC and community leaders. In common with the local populations they serve, they typically have

[4] The UN defines an "orphan" as a child who has lost one or both parents. An estimated 13.4 million children and adolescents (0-17 years of age) worldwide had lost one or both parents to AIDS as of 2015. More than 80% of these children (10.9 million) live in sub-Saharan Africa (UNICEF 2016).

[5] PEPFAR's 2008 reauthorization mandated it to spend 10% of funds on assistance to OVCs. PEPFAR defines children as those below 18 years of age. These funds amounted to more than $1 billion in 2006-09, and $672 million in 2010-11. (PEPFAR Operational Plans for fiscal years 2006-2011, available at http://www.pepfar.gov.) In the 2015 fiscal year, PEPFAR spent $218 million on OVC programming (PEPFAR 2017).

[6] Reviews of research on OVCs include Bryant and Beard (2016), Goldberg and Short (2016), Nyberg et al (2012), and Shann et al (2013). See also Evans and Miguel (2007), Case et al (2004), Larson et al (2013), and Whetten et al (2014).

[7] Program provinces and districts are: Manica province (Manica, Chimoio, and Gondola districts), Sofala province (Dondo and Nhamatanda districts), and Zambezia province (Namacurra and Nicoadala districts).

no more than a primary school education. Roughly 80% of CCWs are female. They range in age from 18 to 48 with most falling between 25 and 40 years of age. CCWs receive a stipend of MZN 3,100 per month (roughly US$50), as well as in-kind compensation in the form of a bicycle, a work uniform, and cellphone airtime.

CCWs conduct home visits of households thought likely to be OVC households, based on personal knowledge and recommendations of the CCPC. The home visit itself is a conduit for dissemination of information and advice by CCWs, whose impacts we seek to measure. All household members may then participate in other FCC components, based on the results of the home visit. In home visits, CCWs conduct systematic vulnerability assessments, and identified "OVC" households (and individuals therein) are then linked to appropriate programs and services in communities, schools, and health facilities. One of the most important results of these home visits is referrals of individuals for HIV testing at the nearest PEPFAR-funded health clinic. The expectation is that CCWs refer all FCC program beneficiaries (both adults and children of all ages) who do not know their HIV status for HIV testing, and that even upon a negative test result testing should be repeated every twelve months. The number of individuals referred to HIV testing is a key outcome indicator for the FCC program, monitored by PEPFAR in the context of achieving the UNAIDS 90-90-90 global goals (90% of those with HIV diagnosed, 90% of those on ART, and 90% of those virally suppressed by 2020 (PEPFAR 2017)). Those testing positive for HIV are then referred to receive antiretroviral therapy (ART) through the clinic. CCWs in the community then follow up with individuals initiating ART to promote ART adherence on an ongoing basis. (For further detail, see Appendix A for the home visit guide, "Steps for a Home Visit".) Because of the centrality of encouraging HIV testing in the FCC program, it is the primary outcome of interest in this study.

During initial and subsequent home visits, CCWs undertake activities to increase HIV testing rates via two mechanisms we will examine explicitly: improving information and reducing stigma concerns. CCWs seek to improve FCC beneficiaries' **information related to HIV/AIDS**, such as methods of disease transmission, progression of the disease, treatment, HIV testing, and locations of health clinics providing testing and treatment. Information is conveyed verbally and, at the LIP's discretion, on printed material given to the household. In addition, CCWs are expected to engage program beneficiaries in "sensitization" to address **stigma related to HIV** (both one's own stigmatizing attitudes, and fear of stigma from others). CCWs engage in discussions to reduce stigmatizing attitudes among program beneficiaries. CCWs provide psychosocial support (PSS) and gradually gain program beneficiaries' trust over time in repeated interactions, with the expectation that reductions in fear of stigma will encourage people to be open to HIV testing, voluntarily disclose HIV-positive status to CCWs, and be open to future CCW follow-up promoting ART initiation and adherence.

In home visits, CCWs are also expected to give caregivers advice and encouragement regarding **children's education**. Caregivers are encouraged to make sure children go to school daily, have appropriate materials and uniforms, and have a place to study at home without distractions. They are encouraged to be involved in their children's education, such as by establishing contact with a child's teachers, maintaining contacts with a child's friends, and helping with homework. Caregivers are also encouraged to discourage girls' early marriage, and to keep girls in school even after the age of 18. (Further details on educational messaging can be found in the Education section of the home visit guide, Appendix A.) Given the prominence of education advice and encouragement in the home visit, child school attendance is a secondary outcome variable in the study.

The FCC program has a number of other components. Households are connected to these other components after the home visits, based on needs assessments conducted by CCWs. Many components are school-based, so children can also be included in these components through their schools. We briefly describe these other components below.

These other components are expected to reach only a relatively small fraction of those reached by home visits. Existing data on their penetration into the population is sparse, and so the endline survey will provide valuable insight into their actual coverage. The endline survey will collect data on household and individual participation in the different program components, and we will test whether outcomes are correlated with inclusion in different program components. These analyses will be exploratory and will establish correlations with participation in other FCC program components (rather than causality), so they are of secondary interest in this study.

These other FCC program components are as follows. Our endline survey will measure how widespread these components are, based on household reports, and we will also collect data from the program implementer on financial outlays for the different components.

- Education subsidies for girls: The FCC program provides a limited amount of funding to support school participation among girls considered particularly at-risk of dropping out of school. LIPs will select the most at-risk girls in a particular community after consultation with school officials and the CCPC. Beneficiary secondary school-age girls will each receive up to US$75 in school fees, elementary school-age girls up to US$50 in school materials (books and uniforms). The exact amounts and funded items vary by community and LIP.
- Child Rights Clubs (CRCs): These are school-based clubs for both girls and boys aiming to equip children with knowledge and skills related to child protection, gender-based violence, and psychosocial support. Topics covered include early marriage, teen pregnancy, reproductive and sexual health, and HIV/AIDS. Participants learn how to report abuse and how to make healthy choices. Activities are child-facilitated with adult oversight. Girls concurrently also take part in associated "Girls' Empowerment Clubs" which provide additional mentoring and support tailored to girls.
- Health and Nutrition Assessments: Using an established protocol, LIP staff conduct nutritional screenings of OVCs aged 6 months to 14 years. Screenings occur in school and community settings. Children identified as malnourished may be provided with food supplements for a limited period, and the most severely malnourished are referred to health clinics.
- Youth Economic Strengthening (YES) clubs: YES clubs are a community-based financial education program for both girls and boys who are out-of-school OVCs aged 15-18. Separate clubs are established for girls and boys. The program provides livelihood and entrepreneurship training, aimed at small-scale commercial rather than subsistence agriculture.
- Village Savings and Loan (VSL) groups: VSL programs involve facilitating and training individuals to organize themselves into simple savings and credit groups, with the aim of improving access to savings and credit in populations that are poorly served by formal institutions. Members can take loans from the communal pool of savings, upon review and approval by the group. Loans are repaid with interest, at an interest rate decided upon by the group. Groups manage their own funds, which are all internally generated from savings and interest earnings from loans. LIP staff will form VSL groups with a mixture of OVC and non-OVC households, as well as youth participating in YES clubs.

Random assignment

This study uses a randomized controlled trial (RCT) methodology to estimate causal direct and spillover effects of the FCC program, and to shed light on some of the operative mechanisms through which it achieves its effects. The randomization was done in three parts or stages.

Table 1 presents the full cross-cutting set of treatments, indicating the number of households per cell.

Table 1: Sample Households by Treatment

	FCC Treatment		FCC Control	Total
	DEB	non-DEB		
Randomization Stage 3 Treatments				
Anti-Stigma	222	158	380	760
HIV/AIDS Information	222	158	380	760
ART Information	222	158	380	760
HIV/AIDS and ART Information Combined	222	158	380	760
High Testing Incentive	222	158	380	760
Control	222	158	380	760
Total	1330	950	2280	4560

Notes on Randomization Stages:

In Randomization Stage 1, 78 study communities assigned at community level to FCC treatment or FCC control, within matched community pairs. In Randomization Stage 2, households in FCC Treatment communities further randomized into Directly-Enrolled Beneficiary (DEB) status or not. In Randomization Stage 3, all households randomly assigned to one of six targeted treatments, with equal probability.

Randomization Stage 1

The FCC program is a community-level intervention, so the first stage was random selection of communities to receive or not receive the FCC program. FCC interventions are centered in primary and secondary schools, so geographic areas of interest are residential areas surrounding schools. (We refer to areas surrounding schools simply as "communities", each of which has a "focal school" where school-based program components are implemented.) World Education Inc./Bantwana consulted with local implementing partners (LIPs) and government officials in the three provinces and seven districts in which the FCC program was to be implemented to identify a set of 76 communities deemed to be "eligible" for the program. These communities were chosen on the basis of being geographically proximate to antiretroviral therapy (ART) sites (health clinics offering HIV testing and treatment), having sufficient OVC populations, and having no other active donor-funded HIV/AIDS programs. These 76 communities were then sorted into stratification cells of matched community pairs, sets of two communities that were very similar in terms of distance to ART sites, school type (secondary or primary), and student population size.

Within each matched pair, treatment status was randomly assigned to one community, with the other school assigned to control status. Randomization of treatment status within matched pairs helps ensure balance in baseline characteristics between treatment and control units, so that treatment-control comparisons can then be credibly interpreted as causal effects of the program. This random assignment was carried out on the computer of one of the co-authors, one-time, with no re-randomization.

The result of the randomization was communicated to World Education/Bantwana in November 2016. The FCC program was then implemented in treatment communities, and not in control communities. School-based components of the program were implemented in the focal school in each treatment community, and not in control communities.

Randomization Stage 2

The second stage of randomization, at the household level, was implemented only within treatment communities.

Of households originally contacted and consented by the study team, a subset were randomly assigned to be "directly enrolled beneficiaries" (DEBs) of the FCC program: their geographic coordinates and household head's name and contact information were provided to World Education/Bantwana and their local implementing partners (LIPs). LIP staff (CCWs) then conducted household and individual

assessments for FCC program subcomponents. Analyses facilitated by this random assignment to DEB status are outlined below.

Random assignment of households to direct FCC enrollment was carried out in November and December 2017 on the computer of one of the co-authors, one time, with no re-randomization. Out of the 40 OVC households administered the baseline survey in each treatment community, 15 were randomly assigned to DEB status (so 25 baseline households in each treatment community have non-DEB status). In addition, to enhance statistical power, we also randomly assigned DEB status to 20 households who received a shorter Vulnerability Assessment (VA) survey but not the full baseline survey in each treatment community.[8] Therefore, a total of 35 households in each treatment community received DEB status. (Please see Section 2.E. "Baseline Survey" below for details on the VA survey and the baseline survey.)

This stage of randomization had two motivations. First, it creates a subgroup of households in treatment communities with relatively high take-up or participation in the FCC program. Estimates of the impact of the FCC program comparing this group to households in control communities therefore have relatively high statistical power. This deals with the possibility of low statistical power for treatment effect estimates based on generally comparing households in treatment and control communities (the share of households receiving FCC services in treatment communities is not expected to exceed 10%.) The second motivation is to measure spillovers of impacts to other households. Because DEBs were randomly selected, non-DEB households have random geographic and social proximity to DEB households. This facilitates credible measurement of spillovers from DEB to non-DEB households.

Randomization Stage 3

To understand complementarities between the FCC program and more targeted interventions, our research team will provide additional treatments after the administration of the endline survey: HIV/AIDS information, HIV treatment (antiretroviral therapy, or ART) information, information to reduce stigma concerns, and higher financial incentives for HIV testing.

Households participating in the endline survey will be randomly assigned to one of the six groups described below, with equal probability. The treatments will be only be administered to consenting survey respondents.

1) **Anti-stigma**: This treatment provides individual-specific information aimed at reducing the respondent's concerns about HIV-related stigma in the community.
2) **HIV/AIDS Information**: This treatment provides factual information about HIV/AIDS.
3) **Antiretroviral Therapy (ART) Information**: improve respondent's information about ART (the treatment for HIV/AIDS).
4) **Both HIV/AIDS and ART Information**: the combination of items 2 and 3 above.
5) **High incentive for HIV testing**: each HIV testing coupon offered to the household provides a financial incentive of 100 meticais (instead of 50 meticais for everyone else).
6) **Control**: None of the above treatments.

Please see Appendix B for details about each of these treatments. These treatments are randomly assigned on the computer of one of the co-authors without stratification, one time, with no re-randomization. The Stage 3 treatments are independent of (orthogonal to) the Stage 1 and Stage 2 randomizations. Sample sizes for each of the treatments is shown in Table 1.

These treatments are included in the study to reveal the extent to which the FCC program is complementary with information about HIV/AIDS, information about ART, information to reduce stigma, and financial incentives for HIV testing. We provide further details about these analyses below.

Blinding

There is no blinding of treatment status. Participants are aware that the research project aims to evaluate the FCC program, but are not explicitly informed of trial hypotheses. While non-DEB households in communities that were randomized into treatment status (Randomization Stage 1) may not realize their community is receiving the FCC program, this information is not intentionally being withheld from them. Households randomized into directly-enrolled beneficiary (DEB) status (Randomization Stage 2) will

[8] This latter group of 20 households will also be surveyed at endline in treatment communities, as will a randomly-selected group of 20 (VA-only, no-baseline) households in control communities for comparison purposes.

receive home visits and will know they are being treated. Households randomly selected for informational or stigma-addressing treatments (Randomization Stage 3) will be aware of these treatments.

Treatment effect estimates related to Randomization Stages 1 and 2 may be subject to biases in self-reporting in the endline survey. For example, there may be experimenter demand effects if respondents realize that the FCC program is intended to raise rates of HIV testing and school enrollment, leading them to falsely overstate these outcomes. (Treatments administered in the course of Randomization Stage 3 will be administered only after administration of the endline survey and so cannot influence endline survey data.) The possibility of such biased self-reporting motivates the objective measures of HIV testing and school enrollment that we outline below.

Intervention fidelity

According to WEI/Bantwana, Randomization Stage 1 (assignment of treatment and control communities) was implemented correctly. All 38 treatment communities received the FCC program, and none of the 38 control communities received the program. This is reflected in internal documentation and reports to the USAID Mission in Mozambique by WEI/Bantwana.

Randomization Stage 2 (assignment of some households in treatment communities to be directly-enrolled beneficiaries, or DEBs) also appears to have been implemented with high fidelity. WEI/Bantwana reports (based on data collected from LIPs) that as of December 2018, 77% of households assigned to DEB status were successfully administered a home visit by a CCW. We expect that among non-DEB households in treatment communities, the share of households receiving such home visits will not exceed 10%.

Household responses in the soon-to-be-implemented endline survey will provide independent confirmation of the share of households receiving home visits and participating in other FCC program components. These data will shed light on the accuracy of WEI/Bantwana's reports about Randomization Stage 1 and Stage 2 intervention fidelity.

Randomization Stage 3 (assignment of households to information and anti-stigma treatments to assess mechanisms of FCC impact) will be carried out by our research staff (survey enumerators) immediately following administration of the endline survey. Randomization and administration of these treatments will be programmed into the tablet-based survey software (Survey CTO), so we expect high rates of intervention fidelity for these treatments. In addition, independent audits of a subset of study participants will confirm whether households received the treatment to which they were randomly assigned.

B. Hypotheses

Primary hypotheses

The primary question of interest in this study is: what are the direct effects of the program on beneficiary households?

We address this question by estimating the causal effect of a household being a directly-enrolled beneficiary (DEB) of the FCC program, all of whom are in treatment communities. In estimating this effect, all households in control communities will be the control group. (Non-DEB households in treatment communities will be the subject of secondary analyses.)

Among primary outcomes of interest, there are two types. First, there are outcomes measuring knowledge of, contact with, and services provided by the FCC local implementing partners (LIPs). These will be considered "first stage" outcomes, which we will test to confirm and measure the extent to which the FCC program reached the intended beneficiaries. Second, there are final outcomes of primary interest.

Knowledge of, contact with, and services provided by LIPs

These outcomes come from the endline survey, reported by the primary household respondent. Section M (Support) of the endline survey asks a series of questions on the household's knowledge of, contact with, and services provided by FCC local implementing partners (LIPs). In this context, we will examine three outcome variables:

- An indicator for a household having heard of the FCC-LIP (equal to 1 if answering "yes" to question M01, and 0 otherwise).
- An indicator for a household having been visited by a Case Care Worker (CCW) of the FCC-LIP (equal to 1 if answering "yes" to question M02, and 0 otherwise).
- An indicator for a household having been referred to or received any services from the FCC-LIP. This indicator will constructed from several questions in Section M, which asks about services received from non-government organizations (NGOs), and which organization provided these services. Specifically, this indicator will be equal to 1 if the respondent reports the LIP in response to *any* of the questions MA5, MA8, M09, M13, M20, M24, M28, M31, M34, M36, M39, M41, M42 (and 0 otherwise). Please see Appendix C for Section M of the endline survey for details on these questions.

Hypothesis P1: Assignment of a household to DEB status raises the knowledge of, contact with, and services provided by FCC local implementing partners (LIPs), compared to households in control communities.

Final outcome of primary interest

The outcome variable of primary interest is HIV testing at the household level. This will be a binary outcome indicating that the household either self-reports having had or is directly observed by our survey staff having an HIV test upon our recommendation. This outcome captures the combination of having already had an HIV test, as well as openness to recommendations for future testing, both of which may be influenced by exposure to the FCC program.

To be specific, the component variables of this outcome variable are:

- HIV testing (self-reported): an indicator that anyone in the household has been tested for HIV in the last 12 months. This is a household-level variable equal to 1 if at least one household member is reported to have had an HIV test in the last 12 months, and 0 otherwise. This variable will be created based on answers to endline survey questions household-level question MA4 (and sub-question MA6) and individual-level question K10 (and sub-question K11).
    - MA4: Have you any household member been referred to take an HIV test during the past 12 months?
        - MA6: If yes, did anyone in the household take up the recommendation to be tested for HIV in the last 12 months?
    - K10: To your knowledge, have you ever been tested for HIV?
        - K11: If yes, when was the most recent test? (1 = in the last 12 months, 2 = 12-23 months ago, 3 = more than 2 years ago)
- HIV testing (directly observed): an indicator that at least one of a household's HIV testing coupons has been redeemed. This is a household-level variable equal to 1 if at least one of a household's incentive coupons is presented at the local health clinic for the HIV testing incentive payment before the 14-day deadline, and 0 otherwise.[9]

Our composite HIV testing outcome is therefore equal to 1 if HIV testing (self-reported) is equal to 1 or HIV testing (directly observed) is equal to 1, and 0 otherwise.

Hypothesis P2: Assignment of a household to DEB status raises rates of HIV testing in households, compared to households in control communities.

Secondary hypotheses

[9] The directly observed variable will be coded as zero for households refusing any incentive coupons, which we expect to be rare. Another rare case will be households with no-one eligible for coupons because of everyone having been tested within the last three months; in this case the directly observed variable will again be set to zero.

A number of secondary hypotheses are of interest, related to comparison of impacts on non-DEB households, impacts on other outcomes, mechanisms of impacts on DEBs, and spillovers from DEB to non-DEB households.

Impacts on HIV testing, examining self-reported and directly-observed variables separately

We will also examine the two HIV testing variables separately, without combining them into one composite outcome:

1) the measure of HIV testing based survey self-reports, and
2) the directly-observed measure of HIV testing based on redemption of testing incentive coupons.

These two outcomes are worth examining separately, because they measure distinct things, and each has strengths and weaknesses. HIV testing in the last 12 months is of greater research and policy interest, because it is not financially incentivized and therefore is the "natural" context in which the HIV testing decision is made. But this outcome measure has the downside that it is self-reported, and may be subject to reporting biases; in particular, it is likely to be overstated by households in the survey, particularly in treatment locations and for DEB households. We therefore complement this measure with a directly-observed measure: redemption of the coupons incentivizing HIV testing. Because the take-up of the coupons is directly observed, it has an important strength: it is immune from survey-reporting biases. The downside of this measure is that the HIV tests are financially incentivized, which departs from the general context of HIV testing. We believe the financial incentive is necessary to ensure the respondents turn in the coupons to our research staff at clinics (without submission of the coupons, there would be no way to measure take-up of testing).

If results differ between the self-reported and directly-observed measures of HIV testing, we will base substantive conclusions and policy recommendations on the findings that use the directly-observed outcome.

Hypothesis S0: Assignment of a household to DEB status raises rates of HIV testing in households, compared to households in control communities, as measured separately by the self-reported and directly observed outcome variables.

Impacts on school attendance

School attendance is a secondary outcome of interest, because an endorsement to attend school and to prioritize education is a component of the home visits of Case Care Workers. School-based components of FCC could also raise school attendance. We will estimate the effect of a student's household being a directly-enrolled beneficiary (DEB) of the FCC program, with households of all students in control communities as the control group.

The outcomes are:

- School attendance (self-reported): an indicator for a child attending school. This is an individual-level variable equal to 1 if a child is reported to be attending school, and 0 otherwise. The value of the indicator will be determined based on the response to endline survey question A17. This variable will be created for all school-age children (aged 6-17) listed in the baseline survey.
- School attendance (directly-observed): an indicator for a child attending school. This is an individual-level variable equal to 1 if a child is directly observed to be attending school by our project staff in an unannounced school visit, and 0 otherwise.

The rationale for examining both a self-reported and a directly-observed measure of school attendance is the same as the rationale for having both self-reported and directly-observed measures of HIV testing (see discussion of *Hypothesis S0* above).

If results differ between the self-reported and directly-observed measures of school attendance, we will base substantive conclusions and policy recommendations on the findings that use the directly-observed outcome.

Hypothesis S1: Assignment of a household to DEB status raises rates of school attendance among children in the household.

Impacts on other outcomes

Other outcomes are also of secondary interest. We consider these secondary because they are less likely to be influenced by the FCC program. Some outcomes (such as adherence to antiretroviral medication) are relevant only for HIV positive individuals, and others (such as the asset index) would likely only be affected in households enrolled in certain FCC components (such as the village savings and loan or VSL program) that are expected to have relatively few participants. Life satisfaction is relevant for all households, but we consider it quite unlikely that the program will affect this outcome.

As in the primary analyses, we will estimate the effect of a household being a directly-enrolled beneficiary (DEB) of the FCC program, with all households in control communities as the control group.

The outcomes are:

- Life satisfaction: Question P1 from endline survey: "Please imagine a ladder with steps numbered from zero at the bottom to 10 at the top. The top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time?" This is defined at the individual level for all adult respondents.
- Household asset index, defined as the first principal component of a vector of indicator variables for ownership of 14 assets (car, motorcycle, bicycle, radio, television, sewing machine, refrigerator, freezer, iron, bed, table, mobile phone, clock, and solar panel). This will be defined for all households.
- Health care utilization for individuals who self-report being HIV positive. This will be defined at the individual level for any individuals reported to be HIV positive in the endline survey.
    a. Indicator for being on antiretroviral therapy (ART). This equal to 1 if the individual reported currently being on ART, and 0 otherwise, based on endline survey question K21 ("Are you currently taking antiretroviral medicines?").
    b. Indicator for having high ART adherence. This is equal to 1 if the individual is reported to have missed no doses in the last 30 days (perfect adherence), and 0 otherwise, based on endline survey question K23 ("How often did you miss doses over the last 30 days?"). This variable will be coded as zero for anyone not currently on ART.

Hypothesis S2: Assignment of a household to DEB status raises life satisfaction, household assets and ART adherence rates.

Impacts on secondary outcomes that are possible mechanisms

We will also measure impacts of the FCC program on outcomes in four groups or "families": 1) HIV-related knowledge, 2) HIV-related stigmatizing attitudes, 3) other positive HIV-related attitudes, and 4) risky sexual behavior. These intermediate outcomes are all measured in the endline survey.

These outcomes are of interest in their own right, and in addition they may be mechanisms through which the program achieves its effects.

As in the primary analyses, we will estimate the effect of a household being a directly-enrolled beneficiary (DEB) of the FCC program, with all households in control communities as the control group.

The outcomes are as follows, by family. As relevant, we indicate specific component question numbers from the endline survey.

- *HIV-related knowledge*. Questions are indicators and are coded as 1 if answered correctly, and 0 otherwise. (Correct answers are in parentheses below, with additional detail as needed.)
    - J03: Have you ever heard of an infection called HIV? (Yes)
    - J05: Can HIV be transmitted from one person to another through sex behaviors? (Yes)
    - JA9: Can HIV be transmitted from one person to another through blood contact? (Yes)
    - J06: Can people reduce their chance of getting HIV by having just one uninfected sexual partner who has had no other sexual partners? (Yes)

    - J06a: Can people reduce their chance of getting HIV by not having sexual intercourse at all? (Yes)
    - J07: Can people get HIV from mosquito bites? (No)
    - J07a: Can people get HIV from shaking hands with an infected person? (No)
    - J07b: Can people get HIV from kissing an infected person? (No)
    - J14: Can people get HIV from sharing food with a person who has HIV? (No)
    - J15: Can people get HIV via witchcraft or other supernatural means? (No)
    - J21: Can HIV be transmitted from a mother to her baby during pregnancy? (Yes)
    - J22: Can HIV be transmitted from a mother to her baby during delivery? (Yes)
    - J23: Can HIV be transmitted from a mother to her baby by breastfeeding? (Yes)
    - J16: Is it possible for a person who looks healthy to have HIV? (Yes)
    - J16a: Is it possible for a person who feels healthy to have HIV? (Yes)
    - J08: Have you ever heard of a condom? (Yes)
    - J09: Do you know where to buy condoms? (Yes)
    - J10: Do you know where to obtain free condoms? (Yes)
    - J11: Do you think people can reduce the risk of transmission of HIV if they use condoms whenever they have sex? (Yes)
    - Indicator for knowing where one can get tested for HIV. Coded from question J24: Do you know of a place where people can go to get tested for HIV? (and answering Yes), and J25: If yes, where can people get tested for HIV? (correctly naming a nearby ART site).
    - JA1: Do you know if there are any special medicines that a doctor or nurse can give a woman infected with HIV, to reduce the risk of mother-to-baby transmission? (Yes)
    - J26: Is there an effective treatment for HIV? (Yes)
    - J26a: If yes, do you know what the treatment is called? (Antiretroviral therapy, or ART)
    - J27: Do you know of a place where people can receive treatment for HIV? (Yes)
    - J29: Can HIV be cured? (No)
    - JA5: Do you think treatment for HIV will be expensive at the local health center? (No)
    - JA6: Do you think treatment for HIV at the local health center can help patients stay healthy? (Yes)
    - JA7: Do you think treatment for HIV at the local health center can help patients live for as long as uninfected people? (Yes)
    - JA8: Do you think treatment for HIV at the local health center can prevent HIV transmission? (Yes)
    - JA13: For people infected with HIV, should they take medication even if they don't feel sick? (Yes)
    - J28: If HIV is left untreated can it cause AIDS (deficiency of the immune system that can lead to severe infections and death)? (Yes)
    - JA11: If not treated, how long do you think it takes for an HIV infected person to develop AIDS (deficiency of the immune system that can lead to severe infections and death)? (Exact answer is 10 years. Coded as correct if absolute difference between respondent's answer and 10 is below sample median in endline survey.)
    - JA12: If not treated, how long can a person sick with AIDS survive? (Exact answer is 3 years. Coded as correct if absolute difference between respondent's answer and 3 is below sample median in endline survey.)
- *HIV-related stigmatizing attitudes*. Questions are indicators and are coded as 1 if answer reveals lack of HIV-related stigma, and 0 otherwise. (Answers revealing lack of stigma are in parentheses below, with additional detail as needed.)
    - J17: Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had HIV? (Yes)
    - J18: If a member of your family got infected with HIV, would you want it to remain a secret? (No)
    - J19: If a member of your family became sick with AIDS would you be willing to care for them in your own household? (Yes)
    - J20: In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school? (Yes)

- *Other positive HIV-related attitudes*. Questions are indicators and are coded as 1 if answer indicates a "positive" HIV-related attitude, and 0 otherwise. (Answers considered "positive" are in parentheses.)
    - J13: Should children age 12-14 be taught about using a condom to avoid getting HIV? (Yes)
    - JA2: If a woman knows that her husband has an illness that is sexually transmitted, is it justified for her to ask her husband to use a condom in their relationship? (Yes)
    - JA3: It is justified for a wife to refuse to have sexual relations with her husband if she knows that he has sex with other women? (Yes)
- *Sexual behavior*
    - L03: How many sexual partners have you had in your lifetime? (count)
    - L04: How many sexual partners have you had in the last 12 months? (count)
    - L05: Have any of your partners ever been tested for HIV? (1=yes, 0=no)
    - L06: Have you ever had sex with someone who you know to have HIV? (1=yes, 0=no)
    - L07: Do you currently own condoms? (1=yes, 0=no)
    - Indicator for "always uses a condom when having sex" (1=yes, 0=no). (Based on responding "all of the time" to question L08: How often do you or your partner use a condom when having sex? (1 = all of the time, 2 = most of the time, 3 = sometimes, 4 = never).)
    - Indicator for a man ever having had sex with a male partner (1=yes, 0=no). (Constructed for men only. Based on responding "yes" to question L09: I have to ask this of everyone. Do you have or have you ever had sex with a male partner? This includes your current partner (if you are married this is your spouse) as well as any past sexual partners.)
    - L11: Have you ever been paid in exchange for sex? (Payment can be in money or in other forms, such as goods.) (1=yes, 0=no)
    - L12: Have you ever paid someone in exchange for sex? (Payment can be in money or in other forms, such as goods.) (1=yes, 0=no)

Hypothesis S3: Assignment of a household to DEB status raises HIV-related knowledge, reduces HIV-related stigmatizing attitudes, increases other positive attitudes towards HIV, and reduces rates of risky sexual behavior, compared to households in control communities.

Impacts on not-directly-enrolled-beneficiary (non-DEB) households

For all primary and secondary hypotheses regarding the impact of a household having directly-enrolled beneficiary (DEB) status, we will also estimate the impact of being in an FCC treatment community but as a non-DEB households (not being assigned to being a directly-enrolled beneficiary or DEB). These will be impacts for households exposed to or participating in the FCC program at the "ambient" rate of program coverage in population.

In each case, the outcome variables will be identical to the outcome variables examined for the hypothesis for DEB status. The causal (right hand side) variable of interest is non-DEB indicator, and comparison group is all OVCs in control communities.

For each prior hypothesis number related to impacts of DEB status, we append the suffix "*-nonDEB*" to indicate the corresponding hypothesis for impacts of non-DEB status. The corresponding non-DEB hypotheses to be explored are:

Hypothesis P1-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher knowledge of, contact with, and services provided by FCC local implementing partners (LIPs), compared to households in control communities.

Hypothesis P2-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher rates of HIV testing in households, compared to households in control communities.

Hypothesis S0-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher rates of HIV testing in households, compared to

households in control communities, as measured by separately by the self-reported and directly observed outcome variables.

Hypothesis S1-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher rates of school attendance among children in the household.

Hypothesis S2-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher life satisfaction, household asset indices, and ART adherence rates.

Hypothesis S3-nonDEB: Households who are in FCC communities but not assigned to directly-enrolled beneficiary (DEB) status will have higher HIV-related knowledge, lower HIV-related stigmatizing attitudes, higher rates of other positive HIV-related attitudes, and lower rates of risky sexual behavior, compared to households in control communities.

Spillovers

Another key question of interest is: given that not all households in a community directly benefit from the program, to what extent do impacts spill over from directly-affected households to others that are geographically or socially proximate? We consider this a question of secondary interest because it is predicated on positive direct effects on directly-enrolled beneficiaries (DEBs) (*Hypotheses P1* and *P2*).

One key channel through which spillovers may occur is information: DEBs may share information with proximate non-DEBs. In addition, stigma may be a key mechanism, if reduced stigma by DEBs leads non-DEBs in proximity to them to be more willing to take up HIV testing. Other channels are possible, but we consider them less likely.[10]

The outcome of interest for this analysis is the measure of HIV testing (as in *Hypothesis P2*).

Right-hand-side variables of interest are measures of social and geographic proximity to DEBs. We define and discuss these proximity measures in Section 3 (Empirical Analysis) below.

Hypothesis S4: Geographic and social proximity to DEB households leads non-DEB households to have higher HIV testing rates.

Impacts of Randomization Stage 3 treatments

We are also interested in the extent to which other types of more focused interventions are complementary with the FCC program. We implement additional treatments immediately after the endline survey (the Randomization Stage 3 treatments). As described above, we will implement treatments providing information about HIV, information about ART, information to reduce concerns about HIV-related stigma, and high financial incentives to receive an HIV test. These treatments will be randomly assigned at the household level.

The outcome of interest will be the objective measure of HIV testing (incentive coupon redemption) at the household level, as described above. (This is the only outcome measure that will be available to the study after the endline survey.)

We will estimate the causal impacts of the Randomization Stage 3 treatments on HIV testing, and the extent to which their effects vary according to a household's treatment status (DEB, non-DEB, and control). If these treatments have less impact on HIV testing for treated than in control households, we will take this as evidence the FCC program and these more targeted treatments are substitutes. Complementarity, on the other hand, would be revealed if these targeted treatments have larger impact for treated than control households.

[10] Channels we consider unlikely include health channels, such as via contagion, or financial channels, if DEBs benefit financially from the program and transfer resources to non-DEBs.

Hypothesis S5: The Randomization Stage 3 treatments (information about HIV, information about ART, information to reduce concerns about HIV-related stigma, and high financial incentives for HIV testing) have positive effects on rates of HIV testing.

Hypothesis S6: The Randomization Stage 3 treatments (information about HIV, information about ART, information to reduce concerns about HIV-related stigma, and high financial incentives for HIV testing) have smaller effects (in absolute value) on rates of HIV testing among DEB households than among households in control communities.

We will also examine whether the effects of the Randomization Stage 3 treatments on HIV testing differ for non-DEB households in treatment communities, compared to households in control communities.

Hypothesis S6-nonDEB: The Randomization Stage 3 treatments (information about HIV, information about ART, information to reduce concerns about HIV-related stigma, and high financial incentives for HIV testing) have smaller effects (in absolute value) on rates of HIV testing among non-DEB households than among households in control communities.

C. Methodological Framework / Identification Strategy

This study aims to provide convincing estimates of causal and spillover effects of the FCC program using a three-part randomized controlled trial (RCT) methodology. Random assignment allows estimated relationships to be interpreted as causal effects, rather than simply correlations.

D. Data

Unit of analysis

The unit of analysis will be the household or individual, depending on the outcome variable.

Hypotheses related to HIV testing focus on household-level outcomes because the intervention aspects related to HIV testing are delivered at the household level (not at the individual level). In addition, there is correlation of attitudes and testing within the household, so we can reasonably think of "household-level" decision-making regarding HIV testing.

Sample size

The first key outcome will be the HIV testing rate, based on redemption of the incentive coupon. This will be measured at the household level (an indicator for anyone in the household being tested). We expect sample size of 3,610 households for this outcome (35 DEB households in each of 38 treatment communities, plus 60 households in each of 38 control communities).

The second key outcome will be the school attendance rate of children, based on unannounced in-person attendance checks at schools. We expect a sample size of 7,720 individuals for this outcome (35 DEB households in each of 38 treatment communities, plus 60 households in each of 38 control communities, and each household with 2 school-age children).

For some secondary analyses, we will compare non-DEB households in treatment communities (of which there are 25 in each treatment community) with households in control communities (60 in each control community). Sample size for these analyses will therefore be 3,230 households.

In practice, analyses for hypotheses related to impacts of DEB and non-DEB status will be conducted in one regression that includes DEB, non-DEB, and control households in the analysis sample. These regressions will have sample sizes of 4,560 households (60 households in each of 76 communities) or 9,120 individuals (120 individuals in each of 76 communities).

Statistical Power

The study has sufficient power to detect effects of reasonable size in the analyses for our primary hypotheses. For these power calculations, we consider the intent-to-treat (ITT) effect of the FCC project on the two primary outcomes of interest, HIV testing and school attendance (*Hypothesis P2*).

Outcome variable distributions, sample sizes, and intra-cluster correlations are calculated using our own baseline survey data. Power calculations were done using the Stata program. The analysis sample consists of the DEB households from the treatment communities and the OVC households from the control communities. For the purpose of these calculations, we assume the number of observations within each community equals to the average expected numbers in the endline survey. As a result, the total endline sample includes 60 OVC households in each of the 38 control communities, and 35 DEB households in each of the 38 treatment communities. The calculation sets a familywise error rate (FEW) at 0.050 level. To account for multiple hypothesis testing, we follow Bonferroni (1935) to conservatively set the hypothesis test regarding each one of the two outcome variables at the 0.025 significance level. Moreover, to calculate the required effect size on the households who have indeed received the FCC treatment, we assume the take-up rate (share of households participating in the program) to be 0 among the control households and 0.770 among the DEB households (consisent with available WEI-reported participation rates among DEB households).

The following power calculation is for detecting an increase in the HIV testing rate. HIV testing of a household is defined as an indicator that at least one of a household's HIV testing coupons has been redeemed. The HIV testing rate of the control group is assumed to be 0.500.[11] The calculation assumes the intraclass correlation coefficient of the baseline self-reported HIV testing measure, 0.060. To achieve statistical power of 80%, the ITT effect would have to be 0.100 (0.600 HIV testing rate in DEB households). Taking into account the treatment uptake rate of 0.770, the FCC treatment effect on the households that have taken up the treatment (treatment effect on the treated, or ToT) would have to be 0.130 (0.630 HIV testing rate in DEB households). This is not an unreasonable effect size to consider, and yields testing rates considerably below what would be needed to achieve the PEPFAR 3.0 objective of having 90% of individuals living with HIV knowing their HIV status (which the FCC program takes as its benchmark).

To detect the increase in child (aged 6-17, inclusive) school attendance rates, we use the endline sample of 120 children in this age range in each control community and 70 children in each treatment community (on average 2 children aged 6-7 per household). We assume the school attendance rate among the control households equals the baseline rate (0.795) and that the intraclass correlation coefficient equals its baseline level (0.048). 80% power would require the ITT effect to be at least 0.065 (the school attendance rate to reach 0.860 among the children from DEB households), and the ToT effect to be at least 0.084 (the school attendance rate to reach 0.879 among the children from treated households). These effect sizes are also reasonable, given the variation in school attendance rates across communities. A reasonable benchmark is the cross-community interquartile range (75th minus 25th percentile), which is 0.121 in the baseline. The ToT effect size as a share of the interquartile range is 69.4%. We consider this to be an achievable effect size.

E. Data Collection and Processing

Key data sources are a baseline survey, an endline survey, directly observed HIV testing and school attendance, and administrative records from schools. We describe each in turn.

Baseline survey

A baseline survey was administered to define a sample of households and individuals to follow over time through the upcoming endline survey, and to measure baseline geographic and social proximity between households (for analysis of spillovers).

The target population of this study is OVCs and the households in which they live. OVC households were identified via door-to-door enumeration of households with a predefined list of questions to identify orphans and vulnerable children. Due to the sensitive nature of such questions, the protocol for identifying

[11] Other things equal, a rate of 0.500 in the control group leads to the largest minimum-detectable-effect. Any required effect size obtained below is calculated under this conservative scenario for statistical inference, and thus should be considered as the upper bound of the minimum-detectable-effect.

OVCs and their households was designed in close consultation with FCC program local implementing partners and field-tested to ensure cultural acceptance and recognition of cultural sensitivities.

Within both treatment and control communities, the baseline household data collection proceeded in several stages. First, 120 households were selected for administration of a vulnerability assessment (VA), whose purpose was to identify OVC households (intended beneficiaries of the FCC program). In communities surrounding eligible schools, households were selected for VAs using random-route sampling.[12] The VA consisted of a short set of questions to determine the household's OVC status. Households were defined as OVC households if any of the following conditions were true: a grandparent was head of the household (with no parents present); the ratio of children to adults was greater than four; at least one school-aged child was not attending school; the household ate fewer than two meals per day; the household goes some days without food; the household has illegal income or no income; the household has a chronically ill member, an HIV infected member, or a member receiving anti-retroviral therapy (ART); there are orphans in the household (one or both parents deceased); or an adult died of a chronic illness in the last five years.[13,14]

The second step was to select households for administration of the household baseline survey. Within the set of OVC households in a community (typically numbering 80-90), 40 were randomly selected as baseline survey households.[15] The household survey asked a comprehensive set of questions at household and individual levels on demographics, health (morbidity, mortality, and child anthropometric measurements), schooling, assets, income, labor supply, migration, financial access (credit and savings), and financial decision-making. It also included a comprehensive set of questions on beliefs about HIV, HIV testing and treatment, and sexual behavior.

All surveys were conducted in the main local languages spoken in the study districts by a Mozambican survey research firm under the supervision of the co-authors. Survey respondents include a primary adult (aged above 18) respondent for household-level questions and other adult household members for sections (such as on health and sexual activity) with individual responses. For children (aged 0-18), their parent or guardian was asked to answer health questions on their behalf.

After the completion of baseline household surveys in study communities, a final stage of data collection was carried out by independent auditors, who revisited all households who had been administered baseline surveys. Auditors' first role was to check for fraud on the part of survey staff, confirming that households were actually surveyed and re-administering a randomly-selected subset of the baseline survey questions to check for accuracy.[16] The second role of auditors was to field a social network survey on households' links with other surveyed households in the community (questions on contacts with whom respondents share information on health, finances, and agriculture). These social network data will be used to understand spillovers from households randomly assigned to be directly enrolled in the FCC program (Randomization Stage 2).

The fieldwork protocols were consistent with the state of the art in survey administration, exploiting the decades of experience of the University of Michigan's Institute for Social Research. Survey responses in the field were recorded on digital tablets using SurveyCTO software with internal logic checks to reduce data entry error. Real-time digital data collection eliminated the need for separate data entry from paper

[12] From the focal school in each of 76 study communities, interviewers were given a starting point from which they followed routing instructions that defined travel directions and selection of households to interview. Directions were randomly assigned, as were distances between successive surveyed households. This procedure resulted in households distributed as far as two kilometers from the focal school in each community.

[13] We administered vulnerability assessment (VA) surveys to 10,056 households. Of these, we classified 71.7% as OVC households.

[14] This definition was agreed upon with USAID's Mozambique Mission and World Education/Bantwana, and captures households disadvantaged for reasons beyond the narrow definition of an OVC according to PEPFAR. PEPFAR's definition (PEPFAR 2006) of an OVC is a child, 0-17 years old, who is either orphaned or made more vulnerable because of HIV/AIDS. An "orphan" is a child who has lost one or both parents to HIV/AIDS. A child is considered "vulnerable" if he or she: (a) is HIV-positive; (b) lives without adequate adult support (e.g., in a household with chronically ill parents, a household that has experienced a recent death from chronic illness, a household headed by a grandparent, and/or a household headed by a child); (c) lives outside of family care (e.g., in residential care or on the streets); or (d) is marginalized, stigmatized, or discriminated against.

[15] Among the non-OVC households, 20 were also selected for inclusion in the baseline survey sample. Due to budget constraints, we do not plan to include these non-OVC households in the endline survey.

[16] Auditing yielded no evidence of fraud or substantial error on the part of baseline survey staff.

surveys, eliminating one important source of data entry error. Data collected in the field were uploaded to a secure cloud server at the end of each workday, for immediate review and processing. Exact geo-coordinates (latitude and longitude) of each household were recorded using the tablet's GPS functionality, greatly easing the process of locating households between the VA survey, baseline survey, auditing/social network surveys, and the upcoming endline survey.

Household survey data collection began in May 2017 and concluded in March 2018. FCC program activities began in treatment communities in the first quarter of 2017, so the household data collection is in principle not prior to treatment. That said, most FCC program activities in 2017 were related to program set-up and some pilot program enrollment, with most beneficiaries expected to be enrolled in 2018 and beyond. We can confirm in our baseline data (in statistics reported below) that there is no statistically significant difference in knowledge of the FCC program local implementing partner (LIP) between treatment and control communities. Other key outcome variables are also balanced between treatment and control groups at baseline. We therefore consider the baseline sample to be a sample of households who are not subject to selection biases. The baseline sample therefore constitutes a valid sample that we will follow over time to see if differences emerge as a result of the FCC program.

Endline survey

The endline household survey will be administered from February to May 2019. The endline survey will provide measurements of a rich set of intermediate and final outcomes for estimation of treatment effects of the FCC program. Communities will be surveyed in the same order they were administered the baseline, and staging will ensure that at least 12 months will have passed since the baseline survey.

All OVC households (40 from each community) originally surveyed at baseline will be surveyed again at endline. In addition, to improve statistical power to detect treatment effects, we will supplement this sample with 20 OVC households not originally surveyed at baseline, who were only administered the vulnerability assessment (VA) survey. These 20 additional households will be randomly selected from among the OVC households in the VA-only sample.[17]

There will thus be a total of 4,560 households surveyed at endline (60 households per community). Each endline household survey is anticipated to take 40 minutes to administer on average.

As in the baseline survey, at least 10% of surveys in each community will be audited (households will be re-visited within two weeks and a subset of questions asked again by a different surveyor) to check for data quality and minimize fraud. As in the baseline, survey staff will be fully aware that auditing will be taking place, but will have no direct contact with auditors, so there should be strong ex-ante incentives for data quality and disincentives for fraud.

Program cost data

We will also collect detailed program implementation cost data from World Education Inc./Bantwana and local implementing partners. These data are provided to USAID and audited, so we believe they will be of relatively high quality. We will separately examine costs for different program components such as Case Care Workers, Village Savings and Loan Associations, educational subsidies for girls, Youth Economic Strengthening groups, nutritional assessments, etc.

In combination with household survey data on their interactions with the FCC program, these data will provide important context by providing one cost-based measure of the intensity of implementation of different program components. Knowledge of the intensity of different program components can inform interpretations of the mechanisms behind any program impacts. They will also be important for any future replication or scale-up that might seek to implement a program as close as possible to the one studied here.

Replacement of Attriting Households

[17] In control communities, the random selection will be among the VA-only OVC households. In treatment communities, the 20 VA-only DEB households (themselves originally randomly selected in Randomization Stage 2) will be the ones additionally included in the endline survey.

In addition to the list of households targeted for the endline survey in each community, we will also predefine a back-up randomly-ordered list of households (among remaining OVC VA-only households) with which to replace any of the originally-targeted households who cannot be or refuse to be surveyed at follow up.

Direct measures of HIV testing and schooling outcomes

The household- and individual-level outcome variables in the endline survey are all self-reported. With self-reported measures, there are concerns about reporting bias. In particular, there is a worry that those in the treatment group will tend to falsely overstate outcomes in the direction of being more "compliant" with expectations for HIV testing or school attendance. We will therefore also measure two key outcomes (HIV testing and school attendance) via direct observation of behavior. These measures will supplement survey self-reported measures. If estimated treatment effects point in the same direction for the survey-reported and directly-observed outcomes, this can improve confidence that results based on the survey-reported outcome represents true changes in behavior, rather than reporting bias.

HIV Testing

We will supplement survey-reported HIV testing rates with directly-observed HIV testing at health clinics. At the time of the endline survey, our survey team will recommend that individuals in the household be tested for HIV (if they have not had a test performed within the past three months) at a specified local health clinic within the next 14 days. To allow tracking of those who follow through with testing, households will be given coupons redeemable for a small financial incentive at the health clinic after having the HIV test. Coupons would have a unique code for each household, allowing us to track redemption of the coupons.[18] This will be done in all households, whether in treatment or control communities, and irrespective of FCC enrollment status. Kranzer et al (2017) successfully implemented this approach to measuring HIV testing rates in neighboring Zimbabwe.

An indicator for at least one of a household's coupons being used (indicating at least one household member had an HIV test in the 14-day window) will be our directly-observed HIV testing outcome variable. Because this outcome represents a real, administratively recorded health behavior, it avoids potential reporting biases associated with survey-reported HIV testing. Conceptually, it captures a household's *receptiveness to a recommendation to be HIV tested*.

School Attendance

In addition to self-reported data on school participation by children, we will also measure school participation directly. During the baseline phase of the study, we collected full names of all children in study households. Households signed consent forms allowing the study team to monitor children's health and schooling outcomes at schools and health clinics. We will seek to measure current school attendance rates of all children enumerated in the baseline survey.

We will measure school attendance (physical presence of children in school) during unannounced school visits by our research project staff from March to July 2019. Project staff will visit schools in study communities to check attendance rates of specific school-aged children who were listed in the baseline survey in their community.[19] (A prominent study that takes this approach is the Kremer and Miguel (2004) study on schooling impacts of deworming in Kenya.) We will also measure school enrollment (presence of children in school registration records), but school enrollment will not be a primary outcome variable.

[18] We will give households as many coupons as needed, for however many individuals do not know their status or report being HIV negative but were tested more than three months in the past. The coupon would have to be presented at the health clinic to receive the incentive. In the informed consent process, households will be informed of this invitation to be tested for HIV, that the unique code on the coupon would allow the study to know if it was redeemed, and of controls in place to ensure the confidentiality of the coupon redemption data. We will allow study participants to opt-out of being offered coupons, and still remain in the study. In the rare case of a household in which all individuals are reported to be HIV positive in the survey, the recommendation to undergo HIV testing will not be extended and the coupons not offered.

[19] In cases where the child could potentially be attending one among multiple study schools, we will check the student's attendance at all possible study schools.

Administrative Data

We will also use administrative data from the focal school in each study community to provide additional insight into program impacts. In each school, we collect data on school finances (budget and expenditures), and on counts of enrolled students. Enrollment data will be based on principal reports, as well as our own counts of students recorded in physical (paper) classroom record books. These data will be used to calculate total student enrollment counts, to gauge the impact of FCC treatment on total school enrollment.

Baseline Survey Summary Statistics and Balance Tests

To give a high-level sense of the study population, we provide here a set of summary statistics from OVC households in the baseline survey as well as school administrative data from the 2016 school year. For each variable, we present the overall mean, means in the treatment and control communities separately, and the p-value of an F-test that the means in treatment and control communities are equal (to confirm balance on baseline characteristics). Variables are mostly indicator (dummy) variables, except number of household members and the school administrative variables.

Table 2: Summary statistics (means) and balance tests for baseline sample

	Number of Obs.	Full Sample	Treatment	Control	p-value of difference
Panel A: Household and individual survey data					
Head is female	3,147	0.390	0.399	0.380	0.473
Owns a TV	3,000	0.319	0.299	0.341	0.451
Owns a bicycle	3,022	0.264	0.271	0.256	0.652
Owns a motorcycle	2,993	0.062	0.064	0.059	0.691
Number of household members	3,149	5.884	5.887	5.881	0.975
Experienced a shock in the last 12 months	3,080	0.307	0.312	0.302	0.834
Has formal savings	3,046	0.197	0.189	0.205	0.600
Member of village savings/loan group	3,090	0.101	0.114	0.088	0.233
Had a formal loan in the last 12 months	3,148	0.040	0.039	0.041	0.905
Heard of FCC local implementing partner (LIP)	3,038	0.425	0.467	0.383	0.145
Attends school (female, age 5-17)	3,259	0.755	0.759	0.752	0.798
Attends school (male, age 5-17)	3,287	0.787	0.786	0.788	0.943
Chronically ill (age 12-17)	1,909	0.040	0.046	0.033	0.204
Tested for HIV (age 12-17)	1,850	0.119	0.131	0.106	0.342
Chronically ill (age 18+)	3,693	0.120	0.136	0.103	0.058
Tested for HIV (age 18+)	3,680	0.527	0.519	0.535	0.639
Panel B: School administrative data					
Number of students	76	2097.908	2,009.289	2,186.526	0.619
Number of classes	76	32.513	31.500	33.526	0.644
Annual public funding (Moz. meticais)	76	199,453	197,009	201,808	0.909

Notes: Data in Panel A are at the household or individual level, and are from household baseline surveys administered in May 2017-Mar 2018. Data from Panel B are at the school level, and are from school administrative records collected in Dec 2016 - Jan 2017. Roughly 60 Moz. Meticais per US dollar in 2016.

Variables in the first 10 rows of Panel A are at the household level. Households have 5.9 members on average, and are headed by females 39.0% of the time. Consistent with overall high rates of poverty in central Mozambique, households have low asset ownership. 31.9% own a TV, 26.4% own a bicycle, and only 6.2% own a motorcycle. 30.7% experienced some negative shock in the last 12 months. Households have low levels of financial services access: 19.7% have formal savings, 10.1% are members of village savings and loan groups, and only 4.0% had a formal loan in the last 12 months.

Given these data were collected after the start of FCC program rollout in treatment areas, it is important to gauge the extent to which households in treatment areas may have already been exposed to the FCC program. We gauge this by asking households if they have heard of the FCC local implementing partner (LIP). Statistics in the 10th row of the table reveal that the share having heard of the LIP is higher in the treatment group (46.7%) than in the control group (38.3%), but the difference is not statistically significant at conventional levels (p-value 0.145).

Turning to individual level variables (the remaining rows of Panel A), we first consider child outcomes. There is quite far from full school enrollment: 75.5% and 78.7% of girls and boys attend school, respectively. 4.0% of children are reported as chronically ill, and 11.9% have been tested for HIV. When it comes to adult outcomes, 12.0% report being chronically ill, and just 52.7% report having been tested for HIV.

Consistent with the FCC program still being in the process of scale-up during the baseline survey, comparisons of means of all the variables in the table between treatment and control groups indicate that the program has yet to show effects in treatment areas. School-level outcomes in Panel B also indicate balance across treatment and control areas in number of students, number of classes, and public funding per school. None of the p-values of the F-tests indicate significant differences in the mean values of key outcomes across treatment and control groups.

3. Empirical Analysis

Statistical methods

Hypotheses will be tested using regression analyses. Regression equations specified below will be estimated using ordinary-least-squares, with standard errors clustered at the level of 76 communities (Moulton 1986).

Respondents will be allowed to skip any survey question they choose, and can also opt out of receiving incentive coupons for HIV testing, so some outcomes will have missing values. For each primary and secondary hypothesis, we will test whether the rate of missing values is affected by the relevant right-hand-side causal variables of interest. In cases where the rate of missing values is affected by a causal variable of interest, to address potential selection bias we will calculate Lee (2009) bounds on the treatment effect.

Nearly all outcome variables of interest are indicator (dummy) variables, so outliers will typically not be an issue. There is only one continuous outcome variables, the asset index (associated with *Hypotheses S2* and *S2-nonDEB*). For this outcome, we will test robustness of conclusions to winsorising the outcome at the 99th percentile (replacing values above the 99th percentile with the 99th percentile value).

Statistical model

Impacts of DEB and non-DEB status

To estimate the impact of directly-enrolled beneficiary (DEB) and non-DEB status, the regression equation will be as follows:

(1) $Y_{ijs} = \alpha + \beta B_{ijs} + \lambda N_{ijs} + \gamma_s + \varepsilon_{ijs}$

Y_{ijs} is the post-treatment outcome for individual or household *i* in community *j* in stratification cell (matched pair) *s*. B_{ijs} is the indicator for a household being randomly assigned to directly-enrolled beneficiary (DEB) status (1 if DEB, and 0 if not), while N_{ijs} is the indicator for a household being randomly assigned to non-directly-enrolled beneficiary (non-DEB) status in a treatment community (1 if non-DEB, and 0 if not). (Both variables are equal to zero for anyone in a control community. In other words, B_{ijs} and N_{ijs} simply partition households in treatment communities into two mutually exclusive subgroups.) γ_s is a fixed effect for stratification cell *s*.[20] ε_{ijs} is a mean-zero error term.

The coefficient β is the intent to treat (ITT) effect of assignment to DEB status (high probability of home visit by a CCW), while the coefficient λ is the corresponding effect of assignment to non-DEB status (receiving a CCW home visit at the low ambient rate in the community). The latter effect, λ, would not necessarily be zero, for two reasons. First, some fraction of these individuals will be enrolled, due to general penetration of the program into communities. Second, there will be spillovers from individuals enrolled in the FCC program. Random assignment of DEB status allows interpretation of these coefficients as causal effects.

The sample for this regression will be all individuals or households in treatment communities (whether DEB or non-DEB), and all individuals or households in control communities.

This regression will be used to test hypotheses related to the impact of random assignment to DEB status and non-DEB status within treatment communities.

Hypothesis tests regarding the impact of DEB status (*Hypotheses P1, P2, S0, S1, S2, S3*) will refer to coefficient β in this regression for the relevant outcome variable.

Hypothesis tests regarding the impact of non-DEB status (*Hypotheses P1-nonDEB, P2-nonDEB, S0-nonDEB, S1-nonDEB, S2-nonDEB, S3-nonDEB*) will refer to coefficient λ in this regression for the relevant outcome variable.

Spillovers from directly-enrolled beneficiary (DEB) households

We are also interested in spillovers from direct beneficiary households (DEBs) to non-direct beneficiaries (non-DEBs). One key channel through which spillovers may occur is information: DEBs may share information with proximate non-DEBs. In addition, stigma may be a key mechanism, if reduced stigma by DEBs leads non-DEBs in proximity to them to be more willing to take up HIV testing. This analysis seeks evidence of spillovers via geographic proximity and social network ties.

Building on equation (1), we will use the following equations to estimate spillovers, separately for social and geographic proximity:

(2a – social spillovers)

$$Y_{ijs} = \alpha + \delta B_{ijs} + \sigma N_{ijs} + \nu EnrollS_{ijs} + \omega S_{ijs} + \gamma_s + \varepsilon_{ijs}.$$

(2b – geographic spillovers)

$$Y_{ijs} = \alpha + \delta B_{ijs} + \sigma N_{ijs} + \mu EnrollDist1_{ijs} + \zeta EnrollDist2_{ijs} + \kappa Dist1_{ijs} + \lambda Dist2_{ijs} + \gamma_s + \varepsilon_{ijs}.$$

Compared to regression equation (1), regression equations (2a) and (2b) add estimates of spillover impacts on households of being socially and geographically proximate to other households that were

[20] Inclusion of the stratification cell fixed effects reduces standard errors by absorbing residual variation. Stratification is at the level of 38 matched pairs of communities within which treatment status was randomly assigned (so stratification cell fixed effects are equivalent to matched pair fixed effects).

directly enrolled in the FCC program. $EnrollS_{ijs}$ is a measure of the extent to which members of one's social network were randomly assigned to direct program enrollment.[21] $EnrollDist1_{ijs}$ is the number of directly-enrolled beneficiaries within a "close" radius of household *i*, while $EnrollDist2_{ijs}$ is similar but for direct beneficiaries in an "intermediate" distance.[22]

In each of these regression specifications, it is also important to control for variables representing the household's general social connectedness and geographic proximity to other surveyed households, because we would expect that households with larger social networks or in more densely-populated neighborhoods to have more directly-enrolled individuals in their social networks or in geographic proximity. Failing to control for such variables would lead to biased estimates of the coefficients on $EnrollS_{ijs}$ in equation (2a), and on $EnrollDist1_{ijs}$, and $EnrollDist2_{ijs}$ in equation (2b). Therefore, in equation (2a), we control for S_{ijs}, a measure of the extent to which members of one's social network are included in the survey sample. In equation (2b), we control for $Dist1_{ijs}$ (the number of surveyed households within a "close" radius of household *i*) and $Dist2_{ijs}$ (similar but for surveyed households in an "intermediate" distance).

The sample for this regression will be all individuals or households in treatment communities (whether DEB or non-DEB), and all individuals or households in control communities.

In equations (2a) and (2b), the coefficients on $EnrollS_{ijs}$, $EnrollDist1_{ijs}$, and $EnrollDist2_{ijs}$ quantify particular types of spillover effects. The coefficient ν on $EnrollS_{ijs}$ isolates spillovers that operate through social network connections. It represents the impact of having additional social network members randomly assigned as DEBs.

Spillovers operating via geographic proximity are revealed in the coefficients on the interaction terms with the $EnrollDist1_{ijs}$ and $EnrollDist2_{ijs}$ variables.[23] The coefficient on μ on $EnrollDist1_{ijs}$ is the impact of having more geographically close individuals randomly assigned as DEBs. We would expect this coefficient to be larger in magnitude than the coefficients ζ on the term corresponding to "intermediate" distance. These spillover coefficients are all credibly interpreted as causal effects. Because direct enrollment in FCC is randomly assigned, the extent to which households have directly-enrolled households in their social network or geographically proximate is also random.[24]

Hypothesis tests regarding spillovers from DEB to non-DEB households (*Hypothesis S4*) will refer to coefficients ν, μ, and ζ in these regressions for the relevant outcome variable.

Mechanisms of FCC Program Impacts

It was infeasible to randomly assign different communities to differently-composed packages or bundles of the FCC program subcomponents. Such random assignment would be required to credibly measure the causal effect of different program elements, revealing the subcomponent mechanisms through which the program has its effects. In this study, in the absence of such random assignment of program subcomponents, our aim is to provide suggestive or observational (non-causal) evidence on program subcomponents through which the FCC program operates. The patterns we observe in the data,

[21] Our social network data indicates that the number of social network members enrolled as direct beneficiaries will typically be in the single digits. We therefore expect to specify this variable simply as the count (number) of social network members enrolled as direct beneficiaries. The number of social network members who are DEBs has mean 0.158 and standard deviation 0.590.

[22] The definition of "close" and "intermediate" distances are as follows, with mean and standard deviation of the number of DEBs: close 0-200 meters (mean 1.93, std.dev. 3.03), intermediate 200-500 meters (mean 6.03, std. dev. 7.14). "Far" distance would be the excluded or reference category.

[23] Measuring geographic spillovers in this manner corresponds to the widely-emulated method used in Miguel and Kremer (2004) to capture health spillovers of deworming in Kenya.

[24] It is reasonable to presume that spillover effects differ between households who themselves were and were not randomly assigned to direct FCC enrollment. In particular, we might expect spillover impacts to be larger for households not directly enrolled. We will also investigate such heterogeneity in the magnitude of spillovers. In exploratory analyses, we would estimate regression specifications that add interaction terms with the $EnrollS_{ijs}$, $EnrollDist1_{ijs}$ and $EnrollDist2_{ijs}$ variables, on the one hand, with the indicators B_{ijs} and N_{ijs} on the other. Comparison of corresponding coefficients on the B_{ijs} and N_{ijs} interaction terms would reveal whether spillovers had greater impact among the directly-enrolled compared to the non-directly-enrolled.

even though not causally well-identified, can provide guidance and suggest foci for future studies that do randomly assign different program subcomponents.

Our approach on this front proceeds in two steps. First, we will measure household and individual participation in FCC subcomponents. Subcomponents for which we will measure participation include: education subsidies for girls; health and nutritional assessments for children; membership in village savings and loan (VSL) groups; contact with community health workers and volunteers for referrals and follow up to health clinics; and child participation in programs such as Child Rights Clubs and Youth Economic Strengthening clubs. Those subcomponents that achieve the highest levels of penetration into the study population will be the strongest candidates for driving the program's overall effects. Regressions in the form of equation (1) will be used to estimate the impact of DEB and non-DEB status on participation in FCC subcomponents.

Second, we will conduct exploratory subgroup analyses suggested by the previous two steps. For example, if treatment status has a substantial effect on child participation in Child Rights Clubs (CRCs), regression analyses based on equation (1) can be run, where the dependent variable is a final outcome of interest (say, child school attendance), and where the treatment indicator is interacted with an indicator variable for participation in a CRC. Such an analysis can reveal whether higher treatment impacts on child schooling are associated with participation in a CRC. If this turns out to be the case, it can provide justification for a future study prospectively randomizing CRC participation, to more credibly estimate the effect of CRC participation.

<u>*Randomization Stage 3 treatments*</u>

Main effect of treatments

The purpose of the Randomization Stage 3 treatments is to understand the complementarity between the FCC program, on the one hand, and future public health information interventions and HIV testing incentives, on the other. That said, the main effect of the Randomization Stage 3 treatments themselves is of interest.

The main effect of these treatments is estimated using the following modification of equation (1):

$$(3)\quad Y_{ijs} = \alpha + \beta B_{ijs} + \lambda N_{ijs} + \rho\, InfoHIV_{ijs} + \tau\, InfoART_{ijs} + \theta\, InfoHIV/ART_{ijs} + \pi\, Anti\text{-}Stigma_{ijs} + \psi\, HighTestPayment_{ijs} + \gamma_s + \varepsilon_{ijs}$$

Y_{ijs} is the post-treatment outcome for household i in community j in stratification cell (matched pair) s. The outcome variable for this analysis is the objective (coupon-redemption-based) measure of household HIV testing. B_{ijs}, N_{ijs}, γ_s, and ε_{ijs} are as in previous regressions.

$InfoHIV_{ijs}$ is an indicator equal to one if a household was randomly assigned to receiving the treatment providing information on HIV/AIDS, and zero otherwise. $InfoART_{ijs}$ and $AntiStigma_{js}$ are defined similarly, but for the randomly-assigned ART information and anti-stigma treatments, respectively. $InfoHIV/ART_{ijs}$ is the indicator for receiving both the HIV and ART information treatments. $HighTestPayment_{ijs}$ is an indicator for being offered the higher-value coupon for receiving an HIV test.

The sample for this analysis will be all households in treatment communities (whether DEBs or non-DEBs) and all households in control communities.

The coefficients ρ, τ, θ, π, and ψ are the intent to treat (ITT) effects of household assignment to the corresponding treatment. These can be interpreted as causal effects because each is randomly assigned.

The hypothesis tests regarding the impact of the Randomization Stage 3 treatments (*Hypothesis S5*) will refer to coefficients ρ, τ, θ, π, and ψ in this regression.

Complementarity between FCC program and more targeted interventions

Analyses of complementarity between the FCC program and the more targeted Randomization Stage 3 treatments will be conducted using the following regression equation, which is a modification of equation (3):

$$
\begin{aligned}
(4)\quad Y_{ijs} = {} & \alpha + \beta B_{ijs} + \lambda N_{ijs} \\
& + \rho\, InfoHIV_{ijs} + \tau\, InfoART_{ijs} + \theta\, InfoHIV/ART_{ijs} + \pi\, Anti\text{-}Stigma_{ijs} + \psi\, HighTestPayment_{ijs} \\
& + \delta B_{ijs}\, InfoHIV_{ijs} + \varpi B_{ijs}\, InfoART_{ijs} + \xi\ B_{ijs}\, InfoHIV/ART_{ijs} + \omega B_{ijs}\, AntiStigma_{ijs} \\
& + \mu\ B_{ijs}\, HighTestPayment_{ijs} \\
& + \sigma N_{ijs}\, InfoHIV_{ijs} + \phi N_{ijs}\, InfoART_{ijs} + \upsilon\ N_{ijs}\, InfoHIV/ART_{ijs} + \eta N_{ijs}\, AntiStigma_{ijs} \\
& + \nu\ N_{ijs}\, HighTestPayment_{ijs} \\
& + \gamma_s + \varepsilon_{ijs}
\end{aligned}
$$

This regression is similar to equation (3), but adds interaction terms between B_{ijs} and each of the Randomization Stage 3 treatments, as well as interaction terms between N_{ijs} and each of the Randomization Stage 3 treatments. These interaction terms reveal whether the effects of the Randomization Stage 3 treatments differ for DEB and non-DEB households, compared to the effect in control communities. Because of the inclusion of these interaction terms, the coefficients ρ, τ, θ, π, and ψ now represent the ITT effects of assignment to the treatments *for households in control communities*.

The coefficients δ, ϖ, ξ, ω, and μ represent the difference in the ITT effect of the Randomization Stage 3 treatments *for DEB households*, compared to the effect for households in control communities. The hypothesis tests regarding how impacts of the Randomization Stage 3 treatments differ for DEB households (*Hypothesis S6*) will refer to these coefficients.

The coefficients σ, ϕ, υ, η, and ν represent the difference in the ITT effect of the Randomization Stage 3 treatments *for non-DEB households*, compared to the effect for households in control communities. The hypothesis tests regarding how impacts of the Randomization Stage 3 treatments differ for non-DEB households (*Hypothesis S6-nonDEB*) will refer to these coefficients.

Multiple outcome and multiple hypothesis testing

In all cases where we adjust p-values to control the false discovery rate, we will use the method of List, Shaikh and Xu (2016).

Primary hypotheses and primary outcomes

Only one treatment is of primary interest: household random assignment to being a directly-enrolled beneficiary (DEB) of the FCC program.

Outcomes related to services provided via the FCC local implementing partners (LIPs) will be considered "first stage" outcomes, which we will test to confirm and measure the extent to which the FCC program reached the intended beneficiaries. When we assess *Hypothesis P1*, we will apply a multiple hypothesis test correction to the three "first stage" variables (indicators for knowledge of, contact with, and services provided by FCC local implementing partner).

No multiple hypothesis test correction is necessary for *Hypothesis P2* because that hypothesis only has one outcome variable.

Secondary hypotheses and outcomes

We have a number of secondary hypotheses and associated outcomes. These secondary hypotheses have to do with FCC program impacts in households that are not directly-enrolled beneficiaries (non-DEBs), mechanisms through which the FCC program achieves its effects, and spillovers from DEB to non-DEB households. Because these are secondary and exploratory analyses, the multiple hypothesis test corrections will be conducted within families of outcomes associated with particular secondary hypotheses, and will not be integrated with the multiple hypothesis test corrections for the primary hypotheses.

Aside from the multiple hypothesis test corrections listed below, any other analyses conducted will be considered exploratory and therefore will not be subject to multiple hypothesis test corrections.

For the secondary hypotheses, we will apply multiple hypothesis test corrections in the following groups of coefficients:

Hypothesis S0: coefficients on DEB status across regressions for the self-reported and directly-observed measures of HIV testing.

Hypothesis S1: coefficients on DEB status across regressions for the self-reported and directly-observed measures of school attendance.

Hypothesis S2: coefficients on DEB status across regressions for survey-reported individual life satisfaction, household asset index, and two measures of individual ART adherence.

Hypothesis S3: coefficients on DEB status across regressions within each of the following families (but not across families): a) HIV-related knowledge, b) HIV-related stigmatizing attitudes, and c) sexual behavior.

Hypothesis S4: the coefficient on social proximity to DEB households and the coefficient on the indicator for the closest geographic proximity to DEB households.

Hypothesis S5: the three coefficients on the HIV information, ART information, and anti-stigma treatment indicators.

Hypothesis S6: the three coefficients on the HIV information, ART information, and anti-stigma treatment indicators, plus the three coefficients on each of these interacted with DEB status.

Hypothesis P1-nonDEB: coefficients on non-DEB status across the three regressions for outcomes related to the knowledge of, contact with, and services provided by FCC local implementing partner.

Hypothesis P2-nonDEB: coefficients on non-DEB status across regressions for survey-reported and directly-observed HIV testing.

Hypothesis S1-nonDEB: coefficients on non-DEB status across regressions for survey-reported and directly-observed school attendance.

Hypothesis S2-nonDEB: coefficients on non-DEB status across regressions for survey-reported asset index and two measures of ART adherence.

Hypothesis S3-nonDEB: coefficients on non-DEB status across regressions within each of the following families (but not across families): a) HIV-related knowledge, b) HIV-related stigmatizing attitudes, and c) sexual behavior.

Hypothesis S6-nonDEB: the three coefficients on the HIV information, ART information, and anti-stigma treatment indicators, plus the three coefficients on each of these interacted with non-DEB status.

Other Secondary Analyses

We will also conduct some secondary analyses as robustness checks.

1) For testing Hypothesis P2 (impact on HIV testing) using equation (1), we will test the robustness of estimates to including control variables for the number of household members and the number for whom HIV tests were recommended.
2) For testing Hypothesis P2 (impact on HIV testing) using equation (1), we will test the robustness of estimates to conducting the analysis using individuals (rather than households) as the unit of observation.
3) For testing Hypothesis S5 (impacts of Stage 3 treatments) using equation 3 and Hypothesis S6 (heterogeneity in impacts of Stage 3 treatments by DEB status and non-DEB status) using equation 4, we will also estimate a specification where we pool the information treatments. In equations 3 and 4, we will replace the separate indicator variables for HIV information, ART information, and HIV+ART information with one indicator variable for receiving an “information” treatment.

Heterogeneous Effects

Aside from heterogeneity in the effects of the HIV information, ART information, anti-stigma, and high-testing-incentive treatments (*Hypothesis S6*), we do not anticipate any other estimation of treatment effect heterogeneity.

Limitations and Challenges

Perhaps the most important risk to the study design is that the FCC program might have been conducted with low implementation quality. The FCC program was implemented by local implementing partners (LIPs) under contract to World Education Inc./Bantwana. Based on administrative data we have seen, it is difficult to assess the quality of program implementation by LIPs. It is possible that LIPs are reaching fewer households with home visits than they have reported to WEI/Bantwana, CCWs are exerting low effort in home visits, fewer than expected participants have been enrolled in different FCC components, or implementation has had other types of shortfalls. The endline survey will provide important measures of implementation quality by LIPs. The risk is that low implementation quality could lead to a finding of zero impacts for the primary and most secondary hypotheses.

The one aspect of study design that would withstand low FCC program implementation quality is our Randomization Stage 3 treatments (the HIV information, ART information, and anti-stigma treatments), in combination with our separate implementation of HIV testing coupons for all study participant households. Because we are implementing these ourselves with our own funding and study personnel, we have high confidence in the implementation quality of these study design elements. *Hypothesis S5* tests the main effect of these treatments, and so would withstand low implementation quality of the FCC program.

Another possible risk is a re-emergence of civil disorder in the study region, related to conflict between the national ruling party FRELIMO and the opposition party RENAMO. In the year prior to the study's initiation, 2016, RENAMO forces took control of parts of Sofala and Manica provinces (two of our three study provinces), leading to travel blockages and safety concerns that halted research activities for international and domestic research teams doing work in the affected areas. A ceasefire was called in December 2016, and conditions have been peaceful since then while negotiations proceeded between the two parties. The death in May 2018 of RENAMO's long-time leader, Afonso Dhlakama, appears to have shifted the power balance in favor of the ruling party FRELIMO, probably making a re-emergence of conflict less likely. We are continuously monitoring this situation to ensure the safety of our staff, but on the whole consider it unlikely that renewed conflict between the two parties will interfere with our research.

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5. Appendices

Appendix A: Home Visit Guide

Appendix B: Details of Randomization Stage 3 Treatments

Appendix C: Section M of Endline Survey

Appendix D: Index of Abbreviations

Appendix E: Map of Study Areas

6. Administrative information

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Institutional Review Board (ethics approval): This study's protocols have been reviewed and approved by Institutional Review Boards (IRBs) in both Mozambique (by the national government's Ministry of Health, approval number 2233/GMS/002/016) and at the University of Michigan (Health Sciences and Social and Behavioral Sciences IRB, approval number HUM00115541).

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PROJECTO
FORÇA À COMUNIDADE
E ÀS CRIANÇAS

PASSOS DE UMA VISITA DOMICILIÁRIA

CARTÕES PARA VISITAS DOMICILIÁRIA

Adaptado & Reproduzido Por:

1. SAÚDE a família, APRESENTE-SE e EXPLIQUE o objectivo da visita (caso seja primeira visita).
2. SAÚDE a família. Convide a todos presentes a juntar-se à visita.
3. PERGUNTE se a família tem alguma preocupação urgente por exemplo Será que há um membro da família que precisa uma atenção medica **○ Todos membros da família estão seguras** **○ Todas pessoas da família comeram? Tem comida na família?** **○ Será que há crianças que sofre de abuso ou negligencia?** **○ Será que as crianças tem roupa ou mantas?**
4. FAÇA O SEGUIMENTO da visita passada. Partilhe o OBJECTIVO da visita do dia.
5. OBSERVE E PERGUNTE sobre as práticas relevantes. Procure saber o que DIFICULTA a responder às necessidades da criança.
6. ELOGIE o cuidador pelas boas práticas e acções realizadas.
7. ESCOLHE e partilhe 1-2 mensagens, usando os desenhos relevantes.
8. AJUDE AO CUIDADOR A PRATICAR una actividade relevante. ELOGIE pelo esforço.
9. EXPLIQUE a importância da actividade. Ajude a incluir essa actividade na ROTINA DIÁRIA da família.
10. REFIRA E/OU ACOMPANHE AOS SERVIÇOS se for preciso.
11. AVALIE o plano de acção junto com a família em cada segunda visita.
12. AGRADEÇA, e concordem sobre a próxima visita. Faça o REGISTO dos serviços prestados.

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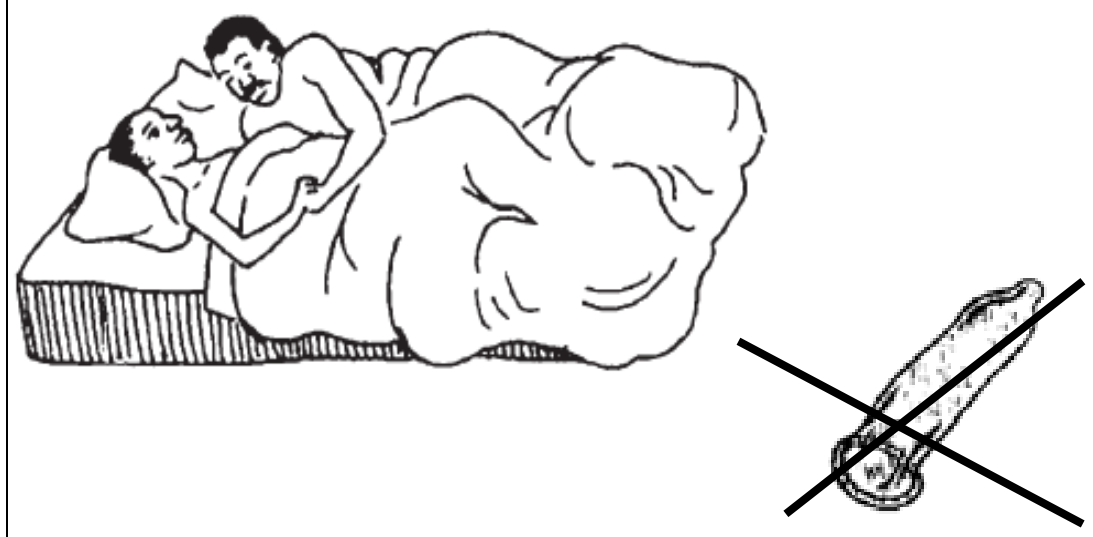
Bantwana WORLD EDUCATION INITIATIVE

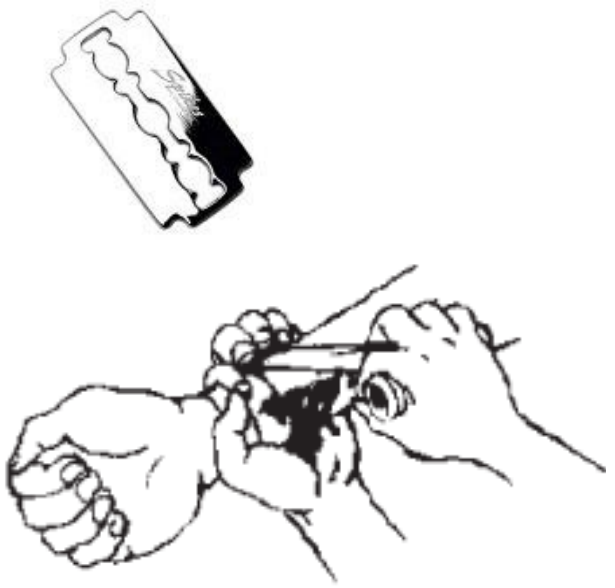
SAÚDE (1) - PREVENIR HIV

COMO SE APANHA HIV :



SEXO SEM PROTECÇÃO
COM UMA PESSOA INFECTADA

DA MÃE PARA O BEBÉ, DURANTE A GRAVIDEZ, PARTO, OU AO DAR PEITO

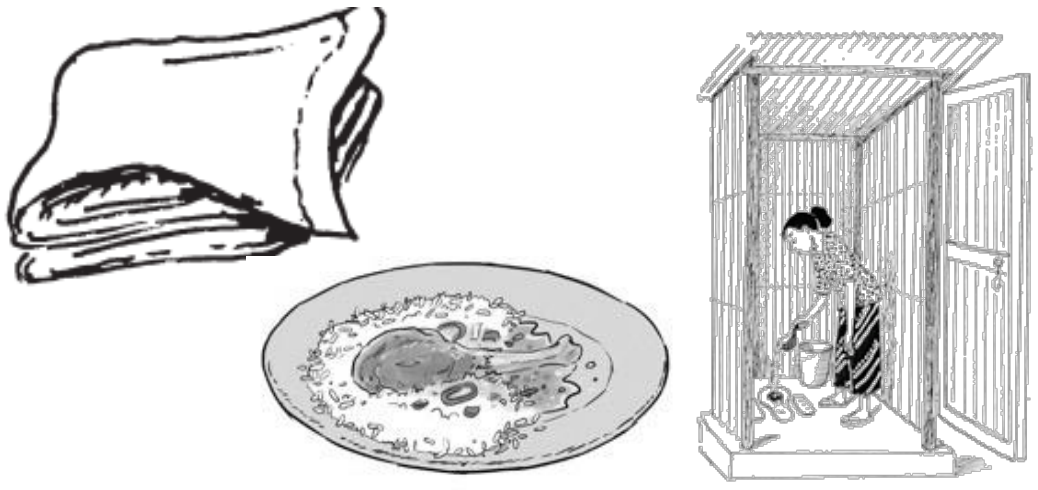


CONTACTO COM SANGUE DUMA PESSOA INFECTADA

COMO NÃO SE APANHA HIV :

ABRAÇAR, BEIJAR OU TOCAR

DORMIR NA MESMA CAMA



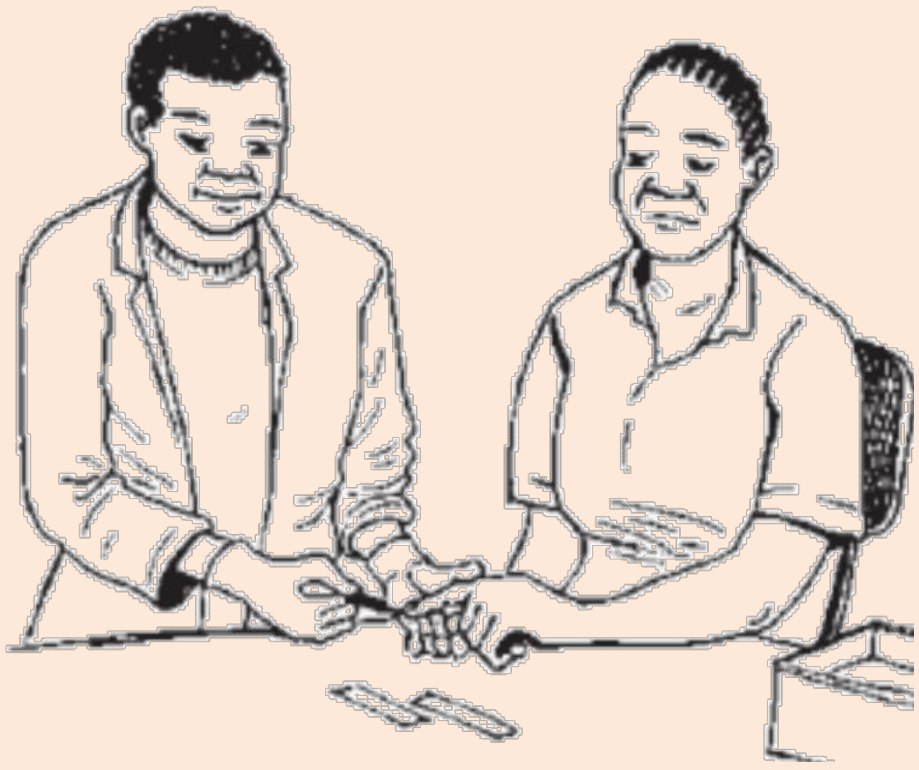
PARTILHAR ROUPA, PRATOS, E COMIDA. USAR A MESMA LATRINA.

CUSPIR OU TOSSIR

Adaptado & Reproduzido Por:

QUALQUER PESSOA PODE APANHAR HIV, SE NÃO SE PREVENIR.

SAÚDE (2) – FAZER TESTE DE HIV E REVELAR O SEU ESTADO



FAÇA O TESTE, PARA SABER O SEU ESTADO.
REPITA DAQUI AS 3 MESES PARA CONFIRMAR

SE A MÃE FOR SEROPOSITIVA, LEVE O BEBÉ DE 1 MÊS PARA FAZER O TESTE DE HIV.

REVELE O SEU ESTADO NA FAMÍLIA E DISCUTAM O CAMINHO A SEGUIR.
AJUDE A CRIANÇA A PARTIR DE 6 ANOS A SABER O ESTADO DELA.
Evitar estigmatizar e discriminar pessoas que estão vivendo com HIV

Fonte: MISAU/UNESCO, 2011; Where there is no artist

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Kantwana WORLD EDUCATION INITIATIVE

SAUDE (3) – FAZER O TRATAMENTO DE HIV E TER APOIO EMOCIONAL

**FAÇA CONSULTAS MENSAIS DE TARV,
E TOME COMPRIMIDOS CADA DIA A MESMA HORA, DEPOIS DUMA REFEIÇÃO.**

LEVE A CRIANÇA A CONSULTA TARV OU A CONSULTA DA CRIAN ÇA EM RISCO.

AJUDE A CRIANÇA A TOMAR MEDICAÇÃO A MESMA HORA CADA DIA, DEPOIS DUMA REFEIÇÃO.

**SE FOR SEROPOSITIVA, VOCÊ PODE FICAR DEPRIMIDA, OU SEM VONTADE.
PODE SER MAIS DIFICIL PARA SI,**

PROCURE TER PELO MENOS UMA BOA AMIGA OU AMIGO QUEM LHE DÁ FORÇA E CORAGEM.

Fontes de desenhos: Where there is no artist, PATH

Adaptado & Reproduzido Por:

Chantwana WORLD EDUCATION INITIATIVE

CUIDAR DA CRIANÇA.

SAÚDE (4) - PREVENIR AS DOENÇAS COMUNS

ASSEGURE QUE A SUA CRIANÇA RECEBA VACINAS, VITAMINA A E DESPARASITANTE

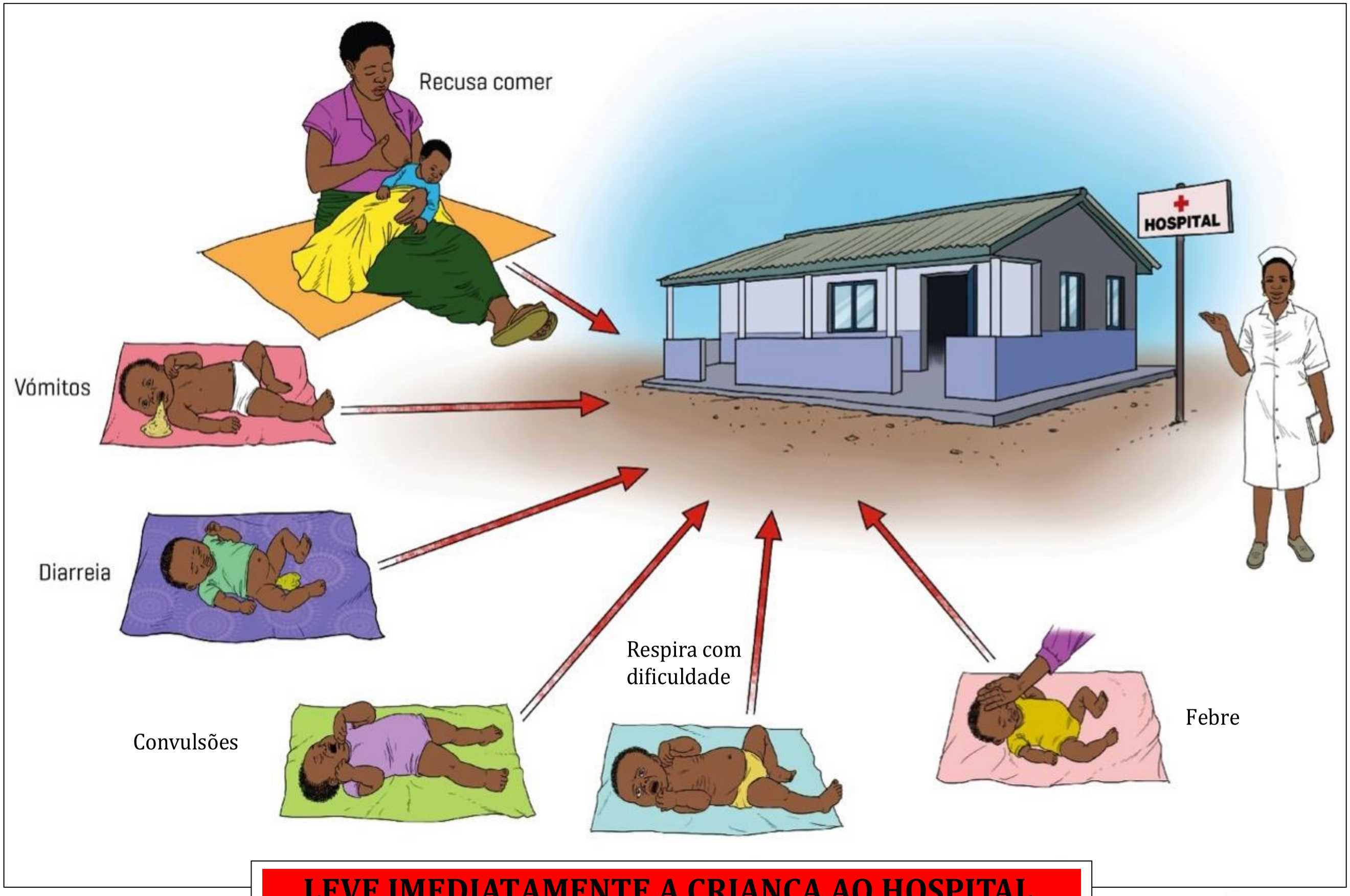
PREVINA A MALÁRIA USANDO A REDE MOSQUITEIRA

USE SOMENTE A ÁGUA FERVIDA OU LIMPA PARA BEBER. TAPE O BIDÃO E USE A CANECA.

LAVE AS MÃOS DA CRIANÇA COM FREQUÊNCIA

Adaptado & Reproduzido Por:
Chantwana WORLD EDUCATION INITIATIVE

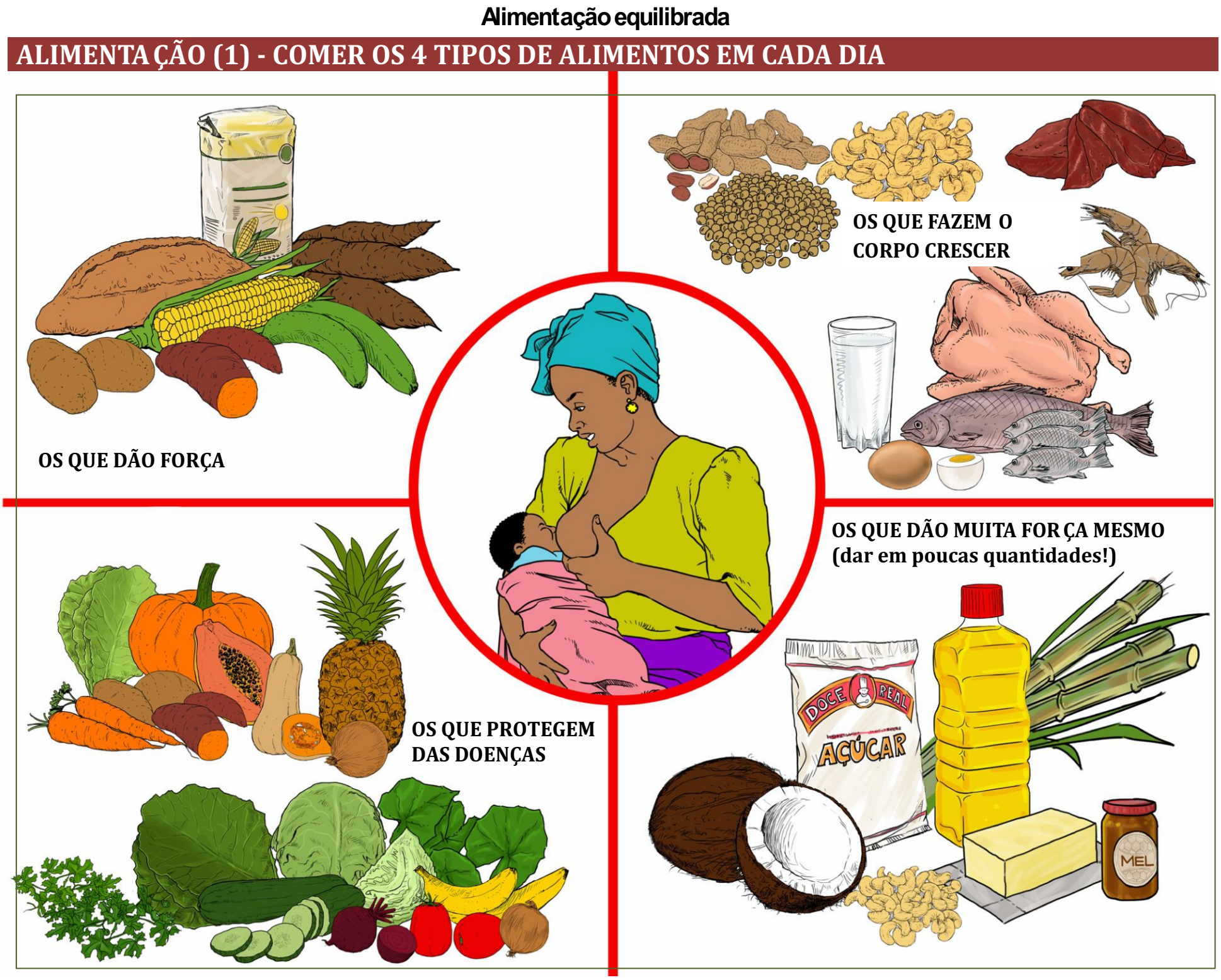
SAÚDE (5) – AGIR EM CASO DE SINAIS DE PERIGO NA CRIANÇA DE 0 A 5 ANOS



LEVE IMEDIATAMENTE A CRIANÇA AO HOSPITAL

Adaptado & Reproduzido Por:

Alimentação equilibrada



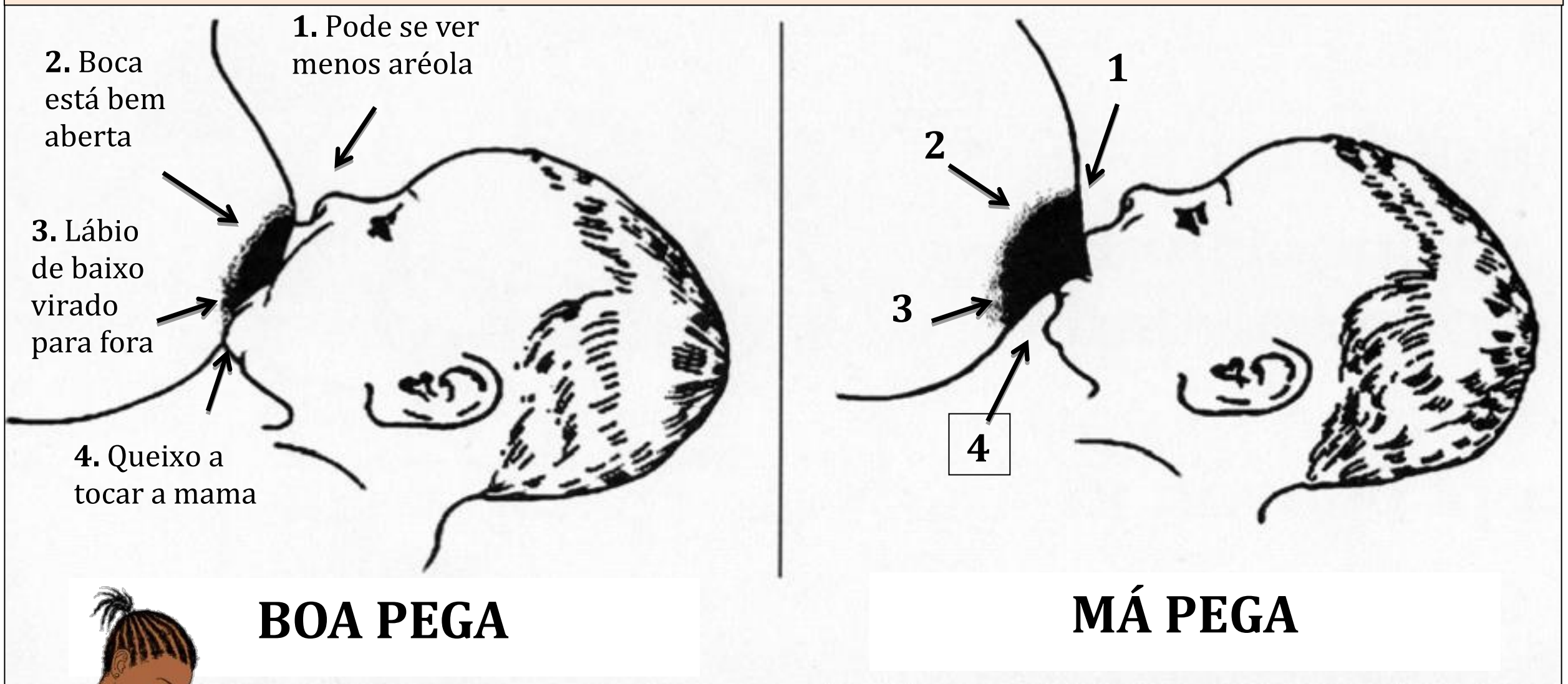
PATH

Adaptado & Reproduzido Por:

Chantwana WORLD EDUCATION INITIATIVE

ALIMENTAÇÃO (2) – DAR Só O LEITE MATERNO À CRIANÇA DE O AOS 6 MESES

Dê somente leite materno à criança entre 0 aos 6 meses, pois tem tudo o que a criança precisa, e protege à criança das doenças.



Sempre há leite suficiente: quanto mais amamenta, mais leite produz. Esvazie um peito, antes de passar para o segundo.

O leite mais gordo está mais para dentro!

Fontes de desenhos: WHO/UNICEF; PATH

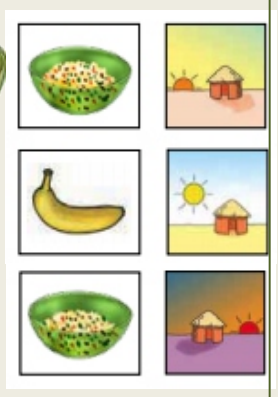
Adaptado & Reproduzido Por:

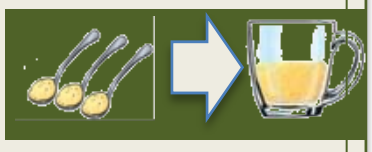
Chantwana WORLD EDUCATION INITIATIVE

PATH

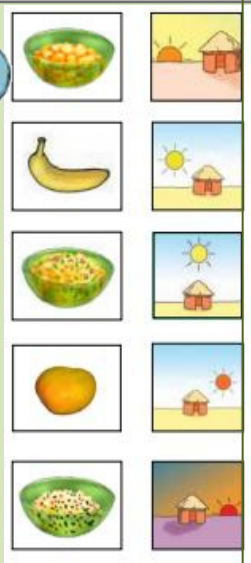
ALIMENTAÇÃO (3) DAR DE COMER A CRIANÇA A PARTIR DE 6 MESES

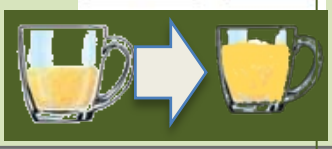
DE 6 AOS 8 MESES





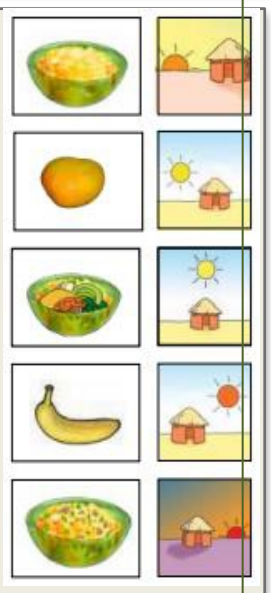
DE 9 AOS 11 MESES



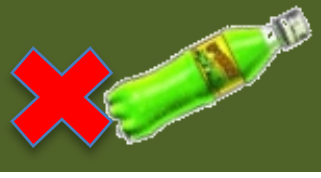


DE 1 AOS 5 ANOS

DAR O PEITO ATÉ AOS 2 ANOS



NÃO DÊ A CRIANÇA REFRESCOS, CHÁ, DOCES E NIK NAK, PARA NÃO ESTRAGAR O APETITE E OS DENTES.



PATH

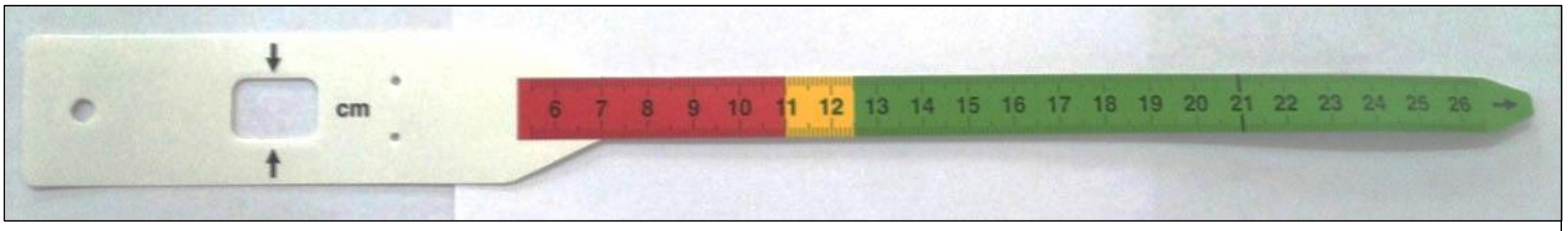
Adaptado & Reproduzido Por:

Bantwana WORLD EDUCATION INITIATIVE

ALIMENTAÇÃO (4) – DETECTAR DESNUTRIÇÃO NA CRIANÇA

A CRIANÇA PODE ESTIVER BAIXA PARA IDADE, TER PÉS ENCHADAS, CABELO FERRUJADO, E NÃO TER FORÇA.

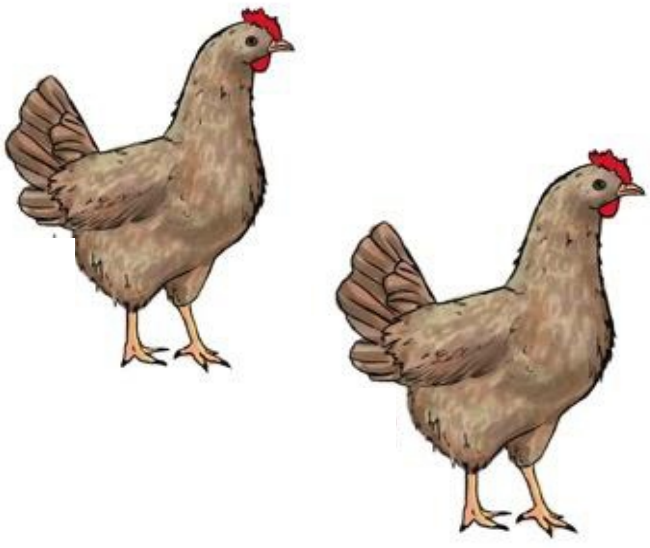
ISSO ACONTECE QUANDO ELA TEM FALTA DE ALIMENTOS QUE AJUDAM A CRESCER E PROTEGEM DAS DOENÇAS.



VERMELHO – Refira imediatamente para a US

AMARELO – Refira para a US se tem outras doenças caso não faz rehabilitação nutricional.

VERDE – Elogie e aconselhe a continuar com boas práticas de alimentação



CRIANÇA. ORGANIZE UMA HORTA CASEIRA . CRIE GALINHAS PARA TER OVOS EM CASA.

Fontes de desenhos: PATH, FHI360

Adaptado & Reproduzido Por:
Bantwana WORLD EDUCATION INITIATIVE

EDUCAÇÃO (1) - COMUNICAR COM ESCOLA E AJUDAR A CRIANÇA

CONVERSE COM OS PROFESSORES SOBRE A SUA CRIANÇA

PROCURE SABER COM AMIGAS, AMIGOS OU COLEGAS COMO A CRIANÇA ESTA A SER TRATADAS NA ESCOLA

ASSEGURE QUE A CRIANÇA VAI A ESCOLA CADA DIA

ASSEGURE QUE A CRIANÇA TENHA O MATERIAL E A UNIFORME PARA ESCOLA

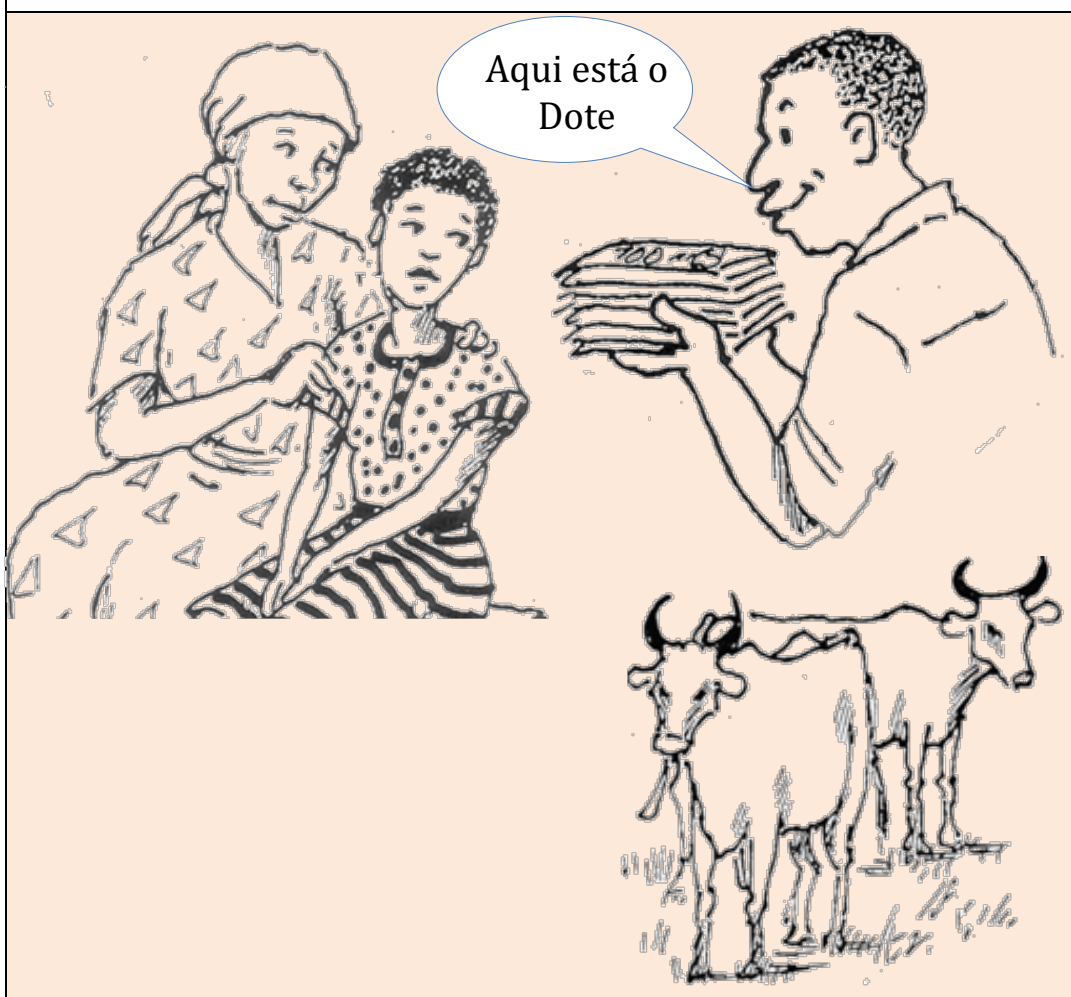
Adaptado & Reproduzido Por:

Tshantwana WORLD EDUCATION INITIATIVE

EDUCAÇÃO (2) - ENCORAJAR A CRIANÇA A ESTUDAR

DÊ A CRIANÇA O TEMPO PARA FAZER TPC, NUM LOCAL SEM DISTRACÇÕES.

AJUDE A CRIANÇA A PERCEBER O TPC. VERIFIQUE O TPC NO FIM. IDENTIFIQUE AS PESSOAS QUE PODEM AJUDAR



NÃO ACEITE O CASAMENTO PREMATURO DA MENINA (ANTES DE 18 ANOS).

MOTIVE A MENINA A ESTUDAR MESMO DEPOIS DE 18 ANOS. CONVERSE SOBRE O QUE APRENDE NA ESCOLA.

Adaptado & Reproduzido Por:

Ubantwana WORLD EDUCATION INITIATIVE

OBSERVE COMO A SUA CRIANÇA ESTÁ A SE DESENVOLVER

3 Meses

6 Meses

9 Meses

12 Meses

18 Meses

2 Anos

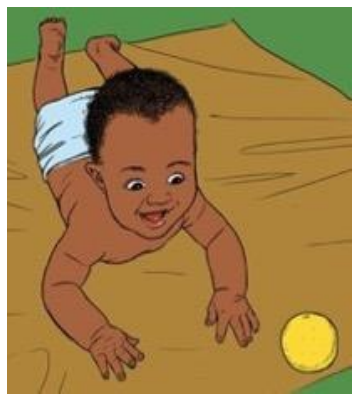
3 Anos

PATH

Adaptado & Reproduzido Por:

Chantwana WORLD EDUCATION INITIATIVE

DPI (2) - CONVERSAR E BRINCAR COM A CRIANÇA PARA ELA CRESCER SAUDÁVEL E ACTIVA

DESDE A GRAVIDEZ ATÉ AOS 3 MESES	DE 3 AOS 6 MESES	DE 6 AOS 12 MESES	DE 1 AOS 2 ANOS	DE 2 AOS 5 ANOS
				Dê coisas para contar e agrupar
				

Adaptado & Reproduzido Por:

Ganhwana WORLD EDUCATION INITIATIVE

PATH

DPI (3) – PREVENIR ACIDENTES NA CRIANÇA

VERIFIQUE SE HÁ ALGO DENTRO DA CASA OU NO QUINTAL, QUE POSSA SER PERIGOSO PARA A CRIANÇA. TOME AS MEDIDAS PARA PREVENIR OS ACIDENTES.

Adaptado & Reproduzido Por:

Fontes de desenhos: PATH

PROTECÇÃO E APOIO LEGAL (1)

TER A VOZ NA FAMÍLIA E SER TRATADO DE FORMA IGUAL

É BOM QUANDO AS CRIANÇAS PARTICIP AM COM SUAS IDEIAS E OPINIÕES, NA FAMÍLIA. ASSIM, TAMB ÉM SERÃO ACTIVAS NA SUA COMUNIDADE, QUANDO ADULTAS.

TRATE TODAS AS CRIANÇAS DE FORMA IGUAL, NA MANEIRA COMO DÁ DE COMER E NAS TAREFAS QUE ELES TENHAM

VIGIE PARA QUE AS CRIAN ÇAS NÃO EXCLUAM OU PROVOCAM OS MAIS VULNERÁVEIS.

ASSEGURE QUE A CRIANÇA TENHA OS DOCUMENTOS, TAIS COMO A CEDULA, O B.I., E O ATESTADO DE POBREZA

Fontes de desenhos: PATH, Where there is no artist

Adaptado & Reproduzido Por:
Kantwana WORLD EDUCATION INITIATIVE

PROTECÇÃO E APOIO LEGAL (2) - PREVENIR E DENUNCIAR A VIOLÊNCIA

NÃO BATA OU GRITA NA MULHER, DURANTE A GRAVIDEZ OU EM QUALQUER OUTRA ALTURA. A VIOLÊNCA NA GRAVIDEZ PODE LEVAR AO PARTO PREMATURO OU AT É A MORTE DO BEBÉ.

NÃO GRITE, CHAME NOMES OU BATE NA CRIANÇA

PROTEJA A CRIANÇA E DENUNCIE O VIOLADOR

CONFORTE A CRIANÇA VIOLADA E LEVE PARA HOSPITAL DENTRO DE 72 HORAS PARA PREVENIR HIV E OUTRAS DOENÇAS.

SINAIS DE VIOLÊNCIA: Arranhões, feridas nos órgãos genitais ou manchas no corpo; tristeza, isolamento, perda de interesse, medo, pesadelos, dificuldades na escola, esquecimento, agressividade.

Fontes de desenhos: PATH, PACTO

Adaptado & Reproduzido Por:
Chantwana WORLD EDUCATION INITIATIVE

HABITAÇÃO: UMA CASA CONDIGNA E BEM ORGANIZADA

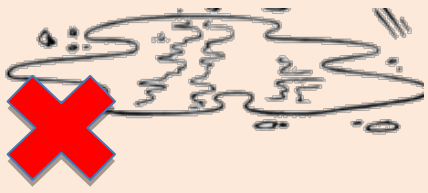
TENHA UMA COVA ONDE SE QUEIMA OU ENTERRA O LIXO DA CASA.

LIMPE A LATRINA COM CINZA E TAPE COM UMA TAMPA.

MANTENHA A CASA LIMPA E BEM AREJADA.

RECOLHE A ÁGUA DA CHUVA.

EVITE ÁGUAS PARADAS NO QUINTAL, PARA PREVENIR A MALÁRIA.



COZINHE NUM LUGAR ONDE ENTRA AR, PARA PREVENIR A PNEUMONIA.

USE A ÁGUA DE LOIÇA PARA A HORTA CASEIRA.

A CASA E O QUINTAL LIMPO EVITAM MUITAS DOENÇAS NAS CRIANÇAS. PEÇA APOIO AOS LIDERES LOCAIS PARA AJUDAR LHE MELHORAR A SUA CASA, OU CONSTRUIR UMA LATRINA.

Fontes de desenhos: MGCAS; Where there is no artista; UNESCO (Educação Parental)

Adaptado & Reproduzido Por:
Chantwana WORLD EDUCATION INITIATIVE

FORTALESCIMENTO ECONóMICO (1) - GRUPO DE POUPANÇA

JUNTE-SE AO GRUPO DE POUPANÇA, PARA MELHOR GERIR E POUPAR O SEU DINHEIRO.

INICIE UM NEGóCIO DE ACORDO COM SUAS CAPACIDADES E INTERESSES MESMOS SE NÃO ESTAS DENTRO DE GRUPO DE POUPANÇA .

Adaptado & Reproduzido Por:

Cantwana WORLD EDUCATION INITIATIVE

FORTALESCIMENTO ECONóMICO (2) - BOM USO DE DINHEIRO

MAU USO

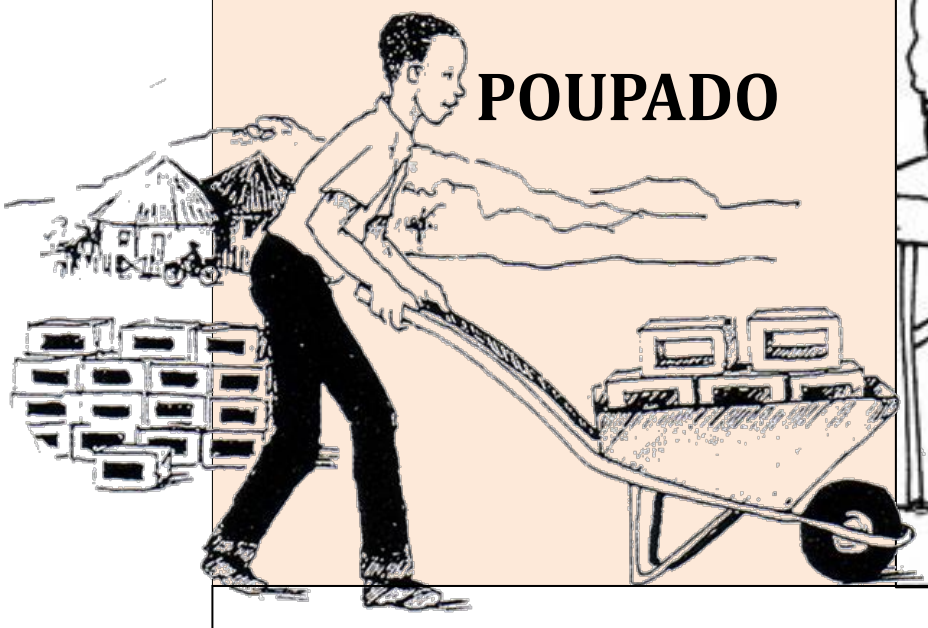
DO DINHEIRO

POUPADO

BOM USO

DO DINHEIRO

POUPADO



USE O DINHEIRO POUPADO PARA O BEM DAS CRIANÇAS E DA FAMÍLIA.

Fontes de desenhos: Where there is no artist

Adaptado & Reproduzido Por:

Tswana WORLD EDUCATION INITIATIVE

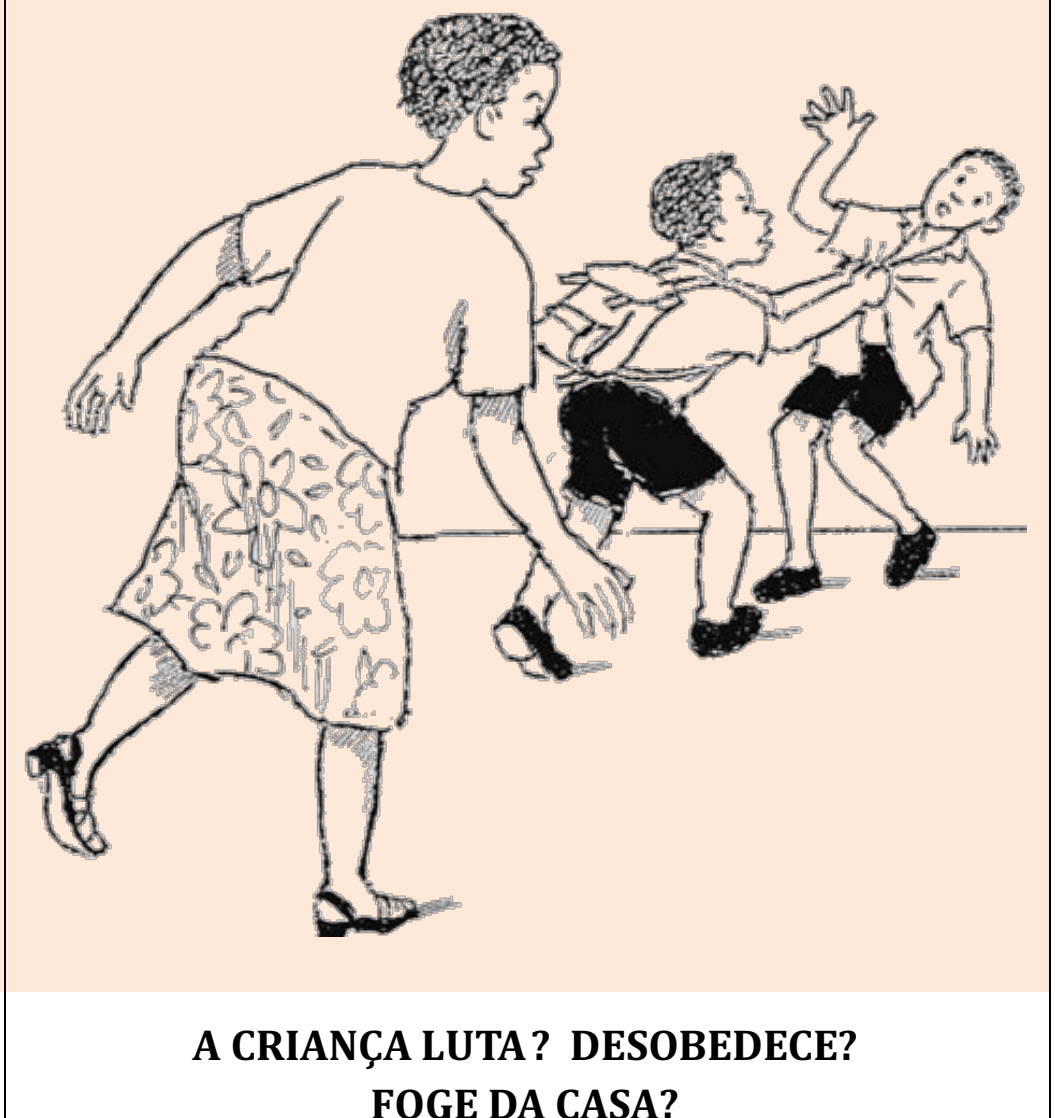
APOIO PSICO-SOCIAL (1)

OBSERVAR AS EMOÇÕES E O COMPORTAMENTO DA CRIAN ÇA

A CRIANÇA TEM AMIGOS?

A CRIANÇA ESTÁ ISOLADA?

A CRIANÇA TEM BOM COMPORTAMENTO?
APOIA COM TRABALHOS EM CASA?



A CRIANÇA LUTA? DESOBEDECE?
FOGE DA CASA?

Fontes de desenhos: Where there is no artista; MGCAS

Adaptado & Reproduzido Por:
Chantwana WORLD EDUCATION INITIATIVE

APOIO PSICO-SOCIAL (2) – CONVERSAR E ENCORAJAR A CRIANÇA

PREPARE A CAIXA DE MEMó RIA. CONVERSE COM A CRIANÇA ORFÃ SOBRE A FAMÍLIA DELA.

CONFORTE E ENCORAJE A CRIANÇA QUANDO ELA ESTIVER TRISTE

EM CADA DIA, PASSE ALGUM TEMPO COM SUA CRIANÇA

MOSTRE INTERESSE NAQUILO QUE INTERESSA A CRIANÇA. AJUDA ELA A APRENDER MAIS.

ELOGIE A CRIANÇA PELO BOM QUE FAZ

Fontes de desenhos: PATH; Where there is no artista; Hesperian

Adaptado & Reproduzido Por:
Chantwana WORLD EDUCATION INITIATIVE

Appendix B: Details of Randomization Stage 3 Treatments

In this section, we provide details of the Randomization Stage 3 treatments: 1) the Anti-Stigma Treatment, 2) the HIV/AIDS Information Treatment, 3) the ART Information Treatment, 4) the combined HIV/AIDS and ART information Treatment, and 5) The high incentive for HIV testing treatment.

1. Anti-Stigma Treatment

In this treatment, the household is given information intended to reduce their concerns about HIV-related stigma in their community. In sum, households are asked in the endline survey about the fraction of households in their community they think hold specific stigmatizing attitudes related to HIV. For any particular attitude for which they are overestimating the fraction of households with stigmatizing attitudes, they are then told the true (lower) rate in their community.

We provide below the implementation details for this treatment.

The following three questions on HIV-related stigma were asked in the baseline survey.

- J17: Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had HIV?
- J19: If a member of your family became sick with AIDS would you be willing to care for them in your own household?
- J20: In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school?

We summarized the answers to these three questions of the baseline respondents. Let x17, x19, and x20 be the shares of respondents answered "yes" to question J17, J18, and J19, respectively. **Appendix Table B** below shows the values of x17, x18, and x19 in each study community. Note that very high shares of respondents answered "yes," indicating relatively low rates of HIV-related stigma. The anti-stigma treatment will reveal these very low rates of stigmatizing attitudes to respondents, potentially reducing stigma concerns and thereby raising HIV testing rates.

In the endline survey, the respondent will be asked to guess the share of people in their neighborhood answering "yes" to each of the three questions above (i.e., guess the values of x17, x19, and x20). Specifically, in the endline survey, the respondent will be asked the following three questions:

- J17a: If I ask the question, "Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had HIV?", to 10 people in your neighborhood, how many of them, would you expect, to say "Yes"? (guess the value of x17)
- J19a: If I ask the question, "If a member of your family became sick with AIDS would you be willing to care for them in your own household?", to 10 people in your neighborhood, how many of them, would you expect, to say "Yes"? (guess the value of x19)
- J20a: If I ask the question, "In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school?", to 10 people in your neighborhood, how many of them, would you expect, to say "Yes"? (guess the value of x20)

Let the answers of the respondent to question J17a, J19a, and J20a in the endline survey be y17, y19, and y20[1], respectively. If y17 < x17, then, it suggests that the respondent has overestimated the HIV-related stigma in their community. In this case, we will reveal to him or her the true value of x17 in the respondent's community. If y17>= x17, we will not reveal x17. The same rule applies to the pairs of (y19, x19) and (y20, x20) as well.

[1] y17 = 100% if, when answering question J17a, the respondent guesses that "10 out of 10" people will say "yes"; y17 = 90% if, when answering question J17a, the respondent guesses that "9 out of 10" people will say "yes"; and so on. The same rules apply for y19 and y20.

If a respondent answered in the survey in such a way that y17>=x17, y19>=x19, and y20>=x20, then, the anti-stigma treatment will not apply to this respondent.

Enumerators will say the following to respondents, according to the following rules. Survey software will automatically implement these rules, and insert the bold bracketed items. ("Rounded value" means the relevant value from Appendix Table B, rounded to the nearest 10 percentage points, and expressed as an integer value out of 10.)

In the baseline survey, we asked people in your neighborhood questions about their attitudes towards HIV/AIDS. We would like to share with you how people responded to these questions.

If y17>=x17 for this respondent, the next two paragraphs are skipped. Otherwise, the enumerator says:

In the survey we just finished, you guessed that ***[insert respondent's answer to question J17a]*** *out of 10 people in your community would answer "yes" to the question, "Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had HIV?".*

We did ask this question to people in your community in the baseline survey. They answered "yes" to this question more often than you think they would. Our data show that ***[insert rounded value of x17 for the respondent's community from table below]*** *out of 10 of the people answered "yes", indicating that the vast majority of respondents are supportive of people living with HIV.*

If y19>=x19 for this respondent, the next two paragraphs are skipped. Otherwise, the enumerator says:

In the survey we just finished, you guessed that ***[insert respondent's answer to question J19a]*** *out of 10 people in your community would answer "yes" to the question, "If a member of your family became sick with AIDS would you be willing to care for them in your own household?".*

We did ask this question to people in your community in the baseline survey. They answered "yes" to this question more often than you think they would. Our data show that ***[insert rounded value of x19 for the respondent's community from table below]*** *of the people answered "yes", indicating that the majority of respondents are supportive of people living with HIV.*

If y20>=x20 for this respondent, the next two paragraphs are skipped. Otherwise, the enumerator says:

In the survey we just finished, you guessed that ***[insert respondent's answer to question J20a]*** *out of 10 people in your community would answer "yes" to the question, "In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school?".*

We did ask this question to people in your community in the baseline survey. They answered "yes" to this question more often than you think they would. Our data show that ***[insert rounded value of x20 for the respondent's community from table below]*** *of the people answered "yes", indicating that the majority of the respondents are supportive of people living with HIV.*

Appendix Table B: Rates of Non-Stigmatizing Attitudes Related to HIV

Community Name	Share of respondents answering "yes" to question J17 (i.e. x17)	Share of respondents answering "yes" to question J19 (i.e. x19)	Share of respondents answering "yes" to question J20 (i.e. x20)
EPC de Chipinde	92.5%	96.8%	92.3%
EPC de Munhonha	93.6%	99.1%	96.3%
ES do Dondo	86.8%	96.2%	91.4%
ES de Macharote	75.0%	94.3%	85.4%
EPC 25 de Setembro CFM	81.3%	98.9%	95.4%
EPC 7 de Abril - Matadouro	87.5%	99.0%	93.8%
EPC de Centro de Acomodação - Mach	75.0%	91.8%	87.5%
EPC C.A.de Cheringoma - Dondo	68.4%	85.4%	75.0%
EPC Samora M. Machel	72.6%	84.3%	76.3%
EPC de Mussassa	67.5%	85.7%	71.4%
EPC de Monte Siluvo	87.1%	100.0%	87.5%
EPC de Nharuchonga	79.2%	98.6%	89.7%
EPC de 3 de Fevereiro	88.9%	95.5%	85.7%
EPC 25 de Setembro	91.5%	96.7%	94.2%
EPC 12 de Outubro	89.3%	98.2%	91.1%
EPC Acordos de Lusaka	81.8%	100.0%	89.5%
ES de Tica	93.3%	96.8%	93.7%
ES de Metuchira	79.0%	98.5%	84.9%
EPC de Muda - Gondola	71.6%	89.7%	87.0%
EPC 1 de Maio - Gondola	81.1%	96.1%	94.7%
EPC de Eduardo Mondlane - Mucorodzi	72.6%	82.8%	88.9%
EPC de Cafumpe	75.0%	95.9%	90.4%
ES Josina Machel	88.1%	95.3%	93.8%
ES de Macombe	72.8%	94.1%	91.7%
EPC de Mussiquir	64.5%	87.5%	87.1%
EPC de Bela Vista - Gondola	72.4%	93.2%	86.0%
EPC de Cabeça do Velho	83.6%	80.9%	86.8%
EPC da Fepom	89.2%	97.3%	93.2%
EPC de Nhamaonha	79.7%	89.4%	87.9%
EPC 25 de Setembro	86.9%	100.0%	96.6%
EPC do Centro Hípico	78.7%	95.2%	91.9%
EPC de Nhamadjessa	74.3%	90.3%	91.5%
EPC 7 de Setembro	81.2%	97.1%	88.4%

EPC 7 de Abril	89.2%	95.5%	98.4%
EPC 1 de Junho	80.3%	92.4%	88.3%
EPC de Mudzingadzi	92.8%	94.5%	93.2%
ES da Soalpo	73.1%	94.2%	86.8%
ES Eduardo Mondlane	83.3%	84.6%	83.3%
ES da Vila Nova	72.2%	91.3%	86.1%
ES 7 de Abril	73.5%	95.7%	95.4%
ES de Messica	62.9%	88.2%	79.4%
EPC Eduardo Mondlane	77.3%	80.3%	81.5%
EPC de Vumba	82.0%	86.0%	84.0%
EPC Messica Aldeia	78.9%	89.5%	84.2%
EPC de Manhate	72.2%	79.2%	83.3%
EPC Estevao Dimaka	75.0%	72.9%	71.4%
EPC de Manica	71.7%	87.0%	92.5%

2. **HIV/AIDS Information Treatment**

Enumerators will show a video about HIV/AIDS after the conclusion of the endline survey. The transcript of the HIV/AIDS video is as follows:

HIV stands for Human Immunodeficiency Virus. When this virus infects someone, it attacks and eventually destroys the immune system over several years. The immune system is the part of your body that protects you from diseases. Most people with HIV look and feel normal at first until their immune system is destroyed and they develop severe infections and cancers that may be fatal.

HIV is not caused by witchcraft or supernatural power. HIV is a viral infection transmitted from one person to another through semen, vaginal fluid or, blood. It can also be transmitted from a mother to a baby during pregnancy, delivery or breastfeeding. HIV is not transmitted through mosquito bites, kissing, shaking hands or sharing dishes.

If a person with HIV does not receive treatment, HIV will multiply in the body very quickly. As the viruses multiply, they can damage the body's defenses against infections and cancers and eventually cause AIDS. Without treatment, an infected person develops AIDS in ten years on average. Common symptoms of AIDS include rapid weight loss; recurring fever; extreme tiredness; long-lasting diarrhea; swelling of the lymph glands; blotches on or under the skin or inside the mouth, nose, or eyelids; and memory loss. Without treatment, someone with AIDS typically survives about three years before they die.

3. **ART Information Treatment**

Enumerators will show a video about ART after the conclusion of the endline survey. The transcript of the ART video is as follows:

HIV is no longer considered to be a death sentence. We now have a free and very effective treatment for HIV.

Antiretroviral therapy (also known as ART) is medication that stops HIV dead in its tracks. It keeps an infected person healthy by preventing HIV from destroying their immune system. It does not eliminate the virus from the body but prevents it from harming the infected person and making it less likely for them to transmit the virus to others.

Starting medication at an earlier stage of infection will greatly improve an infected person's survival rate. So it is important for people who are at risk to take an HIV test even if they still feel healthy, and start ART treatment immediately if the test result is positive. People who are diagnosed with HIV early and who start medication quickly have a better chance of staying healthy and can live as long as uninfected people. Also, the earlier someone is diagnosed and starts treatment, the less likely they are to spread HIV to loved ones.

4. **Combined HIV/AIDS and ART Information Treatment**

Enumerators will show two videos after the conclusion of the endline survey. The two videos are the HIV/AIDS video as in the "HIV/AIDS Information Treatment" group, and the ART video as in the "ART Information Treatment" group.

5. **High Incentive for HIV Testing**

Each HIV testing coupon offered to the household provides a financial incentive of 100 meticais instead of 50 meticais for everyone else.

Appendix C: Section M of Endline Survey

Section M – Support

M01 Have you heard of [local FCC-LIP partner organization]?

MA1 What other social service organizations in your community have you heard of? (Selection from community-specific NGO list, allowing for free text entry of other response.)

Repeat questions M02-M05 for each organization that has been heard of, including the FCC-LIP

M02 If yes, have you or any members of your household been contacted by a case care worker (community health worker) from the organization?

M03 If yes, were you referred to any services by the case care workers from this organization?

M03 If yes, what types of services were you referred to? (Education Health Psycho Social Support (PSS) Housing Early Childhood Development (ECD) Child Protection Legal Support (CPL) Food & Nutrition Economic Strengthening Post Exposure Prophylaxis (PPE) Other (Specify))

M05 If yes, how many times have they contacted you related to their work as a case care worker?

MA2 When it comes to the services provided to you by the FCC local partner organization, have you been "graduated" from receiving these services? ("Graduated" means that you are no longer receiving any specific direct FCC services, and are no longer being visited regularly by the LIP case care workers.)

MA3 If so, when did you "graduate"?

MA4 Have you any household member been referred to take an HIV test during the past 12 months?

MA5 If yes, by whom or by what organization? (Review list of choices; can choose more than one)

LIP case care worker
[List other local NGOs]
Worker at another NGO (specify: _____)
Community leader
Government worker (specify agency:_____)
Neighbor
Family member
Other, specify: _____

MA6 If yes, did anyone in the household take up the recommendation to be tested for HIV in the last 12 months? If so, how many people did so in the last 12 months?

MA7 Has anyone in the household received support from a local organization to help them adhere to their antiretroviral therapy (ART) treatment for HIV/AIDS in the last 12 months?

MA8 If yes, by what organization? [provide list of local NGOs; can choose more than one]

MA9 If yes, how many people received such support in the last 12 months?

M06 Have you heard of your local Community Child Protection Committee?

M08 To your knowledge, have any of the children in your household undergone a nutrition assessment? (For example, measuring height/weight at a service site in the community.)

M09 If yes, which organization provided the services?
M10 Have you received any information about gender-based violence?
M11_1 If yes, do you know where to go to if you have any problem related to gender-based violence? (Centro de saúde/hospital Accao Social Policia, esquadra ONGs local ou INGOs local/chefe do quarteirão/líder comunitário)
M12 If yes, in which context did you get information on gender-based violence? (Child Rights Club School Council Girls's Empowerment Clubs Other School-based program (specify) Broadcast media (radio, TV) VSL Group Community meeting Other group meeting (specify) Other (specify))
M13 If yes, which organization provided this information?
M14 Have you heard about a child's rights club (a group where children can learn about and discuss child rights) at the school the children in your household attend?
M15 If yes, are any children in your household members of the child's rights club?
M16 Have you heard about a girls' empowerment club at the school or in the community that children in your household attend?
M17 If yes, are any children in your household members of the girls' empowerment club?
M18 In the past 12 months, has your household received any education subsidies or support for the children who belong to your household?
M19 If yes, what types of support did you receive? (Uniforms Books School materials Referral for Life Skills Services Psychosocial Support HIV Education GBV Prevention and Response Other (Specify))
M20 If yes, which organization provided the support?
M21 In the past 12 months, have you taken part in any teacher-parents meeting at the school the children in your household attend?
M22 In the past 12 months, has your household received any training, services or support to improve your income?
M23 If yes, what types of support did you receive? (Cash grant through social cash transfer Business Credit/loan scheme Group/Village Savings Entrepreneurship training Agricultural support Provision of land Income Generating Actives Vocational Training Village Saving and Loan Group Other (Specify))
M24 If yes, which organization provided the support?
M25 How would you rate the effect or impact of these services on your ability to support your family compared to the time before the services? (1 = not much change, 2 = worse off, 3 = improved, 4 = greatly improved)
M26 Have you heard about a youth economic strengthening (YES) club?
M27 If yes, are any children in your household members of the YES club?
M28 If yes, which organization organized the YES club?
M29 In the past 12 months, has your household received household supplies from a community group or other organization for which you did not have to pay?
M30 If yes, did you receive the following items? (Blankets Bed nets Clothing or Shoes Pots or Utensils for cooking or eating Other (Specify))
M31 If yes, which organization provided the support?
M32 In the past 12 months, has your household received assistance with home maintenance from a community group or other organization for which you did not have to pay?
M33 If yes, what were the type(s) or repairs? (Roof Door or Window Walls Floor Toilet or other plumbing Other (Specify))
M34 If yes, which organization provided the support?

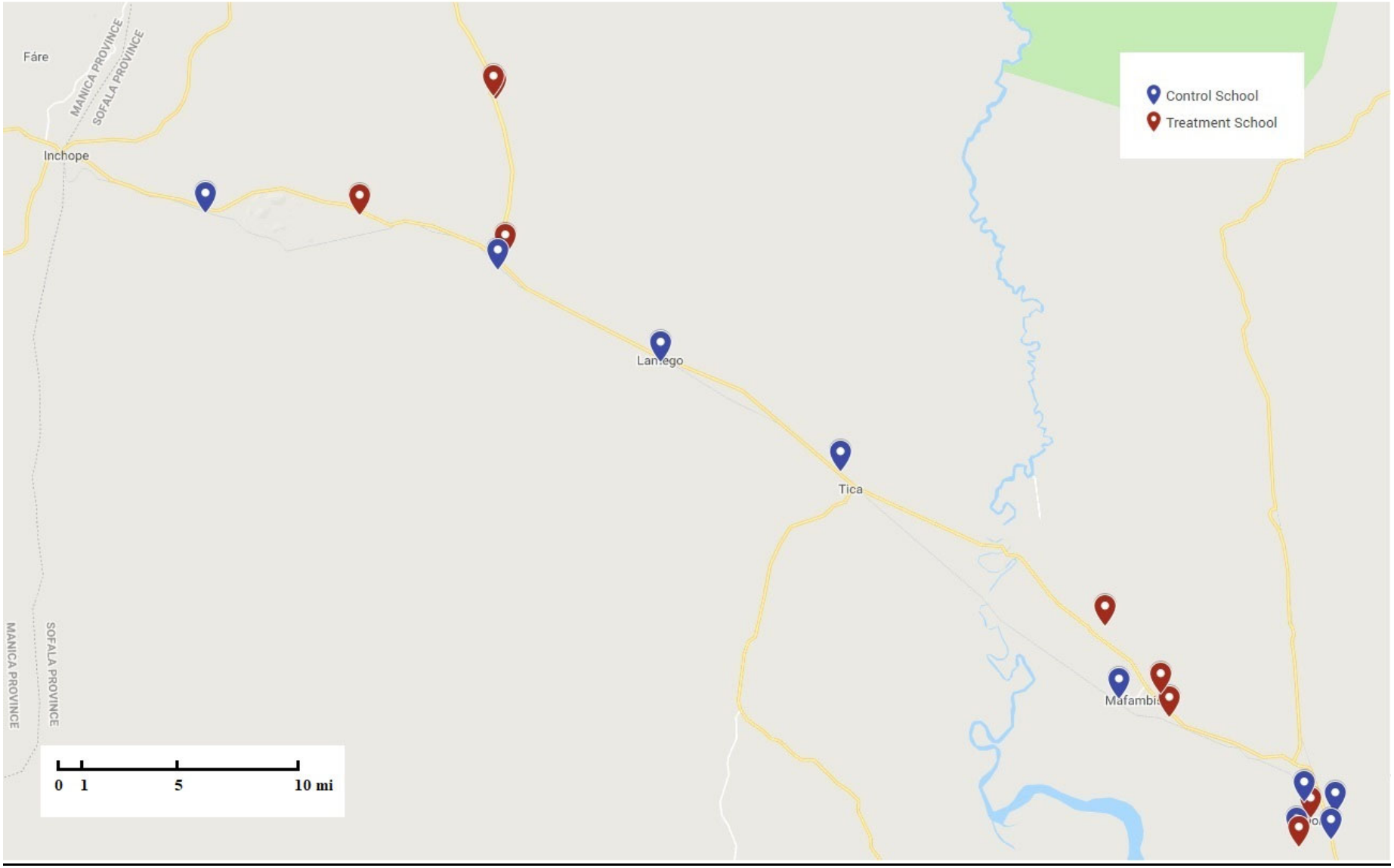
M35 In the past 12 months, have you or anyone in your household received information from a community group or other organization about preparing healthy foods for children under your care?
M36 If yes, which organization provided the support?
M37 In the past 12 months, has your household received a food package from a community group or other organization for which you did not have to pay?
M38 If yes, how many times during the last 12 months did you receive a food package (s)?
M39 If yes, which organization provided the support?
M40 In the past 12 months, has anyone from a community group or other organization provided you with information on children's rights?
M41 If yes, which group provided the information?
M42 In the past 12 months, has anyone from a community group or other organization provided you with information on the need to protect children from abuse?
M43 If yes, which group provided the information?

Appendix D: Index of Abbreviations

Abbreviation	Description
AIDS	Acquired Immune Deficiency Syndrome
ART	Antiretroviral Therapy
BASIS	The Feed the Future Innovation Lab for Assets and Market Access at UC Davis (AMA Innovation Lab, formerly BASIS)
CCPC	Community Child Protection Committee
CCW	Case Care Worker
CRC	Child Rights Clubs
DEB	Directly Enrolled Beneficiaries
FCC	Força à Comunidade e Crianças (Portuguese), Strengthening Communities and Children (English)
FEW	Familywise Error Rate
FRELIMO	The Mozambique Liberation Front
GPS	Global Positioning System
HIV	Human Immunodeficiency Virus
IRB	The Institutional Review Board
ITT	Intent-to-Treat
LIP	Local Implementing Partner
NGO	Non-Government Organization
OVC	Orphans and Vulnerable Children
PEPFAR	The U.S. President's Emergency Plan for AIDS Relief
PSS	Psychosocial Support
RCT	Randomized Controlled Trial
RENAMO	The Mozambican National Resistance
Tot	Treatment Effect on the Treated
TV	Television
UN	United Nations
UNAIDS	The Joint United Nations Programme on HIV/AIDS
UNICEF	The United Nations Children's Fund
USAID	The United States Agency for International Development
VA	Vulnerability Assessment
VSL	Village Savings and Loan
WEI	World Education, Inc
YES	Youth Economic Strengthening

Appendix E: Maps of Treatment Areas

Sofala



Manica

Control School
Treatment School
Manica
Moçambique
Zimbabwe
Bandula
Vumba
Cafumpe
Fetera
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Zambezia