Evaluation of a Comprehensive School Health Programme in Zambia

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Study Protocol

Provision of healthcare services at scale: the human capital effects of a school health program

Research Protocol

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1. Introduction

While much attention has been dedicated to the health and well-being of children under 5 years old, the needs of older children and adolescents have been historically overlooked. Although the period of life between 5 and 14 years of age is critical for physical, psychological, cognitive and social development (Bundy, de Silva et al. 2017), children in this age group face health-related challenges higher than previously realized (Hill, Zimmerman et al. 2017). In Zambia, the prevalence of malaria is highest in children aged 5-17, with 40% of children testing positive in endemic areas (Pinchoff, Chaponda et al. 2016). More generally, high levels of morbidity have been observed in primary school children (Wei, Brigell et al. 2019). These problems are caused or compounded by inadequate access to prevention and treatment for school-age children. Delayed treatment is often due to the high opportunity costs associated with long waiting times in over-crowded facilities, where children over five are no longer prioritized over adults (Coalson, Cohee et al. 2018). Long waiting times to access care are particularly acute when government health services are free, as is the case in Zambia.

Lack of access of school-aged children to prevention and treatment has detrimental effects on a range of health, education, and economic outcomes. Delayed access to care increases illness spells and may sometimes lead to avoidable deaths. For example, 40% of severe malaria cases could be averted if children sought treatment within the first day of symptom onset (Coalson, Cohee et al. 2018). Ill health has also been found to hinder growth in puberty (Torres, Peterson et al. 2000). In turn, health-related issues have a negative impact on education outcomes. They are a major cause of the 20-25% absenteeism rate observed amongst school children in Zambia (Banda 2017, Kabanga and Mulauzi 2020). Evidence also suggests that adolescent Zambian girls miss up to 36 school days per year due to menstrual-hygiene related challenges, primarily caused by lack of adequate facilities and unsupportive environment for menstrual hygiene management (Ministry of Education 2013). Ill health has been shown to lead to lower cognitive abilities (Clarke, Rouhani et al. 2017), which, together with absenteeism, increases the likelihood of dropout.

As investments in education have resulted in primary school enrolment rates reaching 90% in Zambia, schools are a unique platform to increase access to preventive and curative healthcare services for children older than five.

2. The Healthy Learners programme

2.1. Description of the programme

Since 2010, Healthy Learners (HL), an international NGO based in Lusaka, has partnered with the Zambian Government to develop a comprehensive school health program, which makes schools an entry point into the healthcare system by training and supporting teachers as community health

workers, tasked to engage with local communities, promote health behaviours, and refer sick learners to health facilities.
The main components of the programme are described in detail below.

School Health Workers. In each school, teachers are selected to become school health workers (SHWs). Following a two-week training, SHWs perform two main roles: 1) they deliver health talks in all classes and coordinate the delivery of preventative care with local clinics (e.g. deworming campaigns); 2) they take turns to be on duty in the 'school health room', a purposefully constructed building where sick students can receive care and rest if needed. With the help of a tablet-based clinical decision support system (CDSS) which does not follow a branching logic to minimize input errors and uses Bayesian logic to weigh reported symptoms (Finette, McLaughlin et al. 2019), SHWs follow a suggested course of action which builds upon recommended WHO guidelines. At the end of each screening, a SHW follows one of three courses of action:

- They provide reassurance that the symptoms are self-limiting and that no specific treatment is required beyond possible symptomatic relief medicines that they can dispense (e.g. paracetamol);
- They provide treatment for a limited number of conditions (malaria, diarrhoea, schistosomiasis, pneumonia, conjunctivitis), or
- They refer the child to the public clinic associated to the school to be further assessed by a healthcare professional. The referral can be normal, in which case the guardian of the child is contacted and asked to take their child to the clinic, or urgent for severe conditions (in which case the SHW may accompany the child to the clinic themselves).

This system ensures efficient health-seeking behaviours by school-age children, limiting unnecessary visits to the clinic (overuse) and lack of or delays in required referral (underuse).

Priority referral system. With the support from the Ministry of Health and engagement with the local clinics, children referred by SHWs are given priority by health care workers who see them within 30 minutes of arriving at the facility. This **"**fast-track" referral system is facilitated by the referral form containing information about symptoms and suggested diagnosis by the CDSS. The system ensures that parents do not have to waste precious time taking their children to facilities.

Monitoring of children's illness. a proactive system is set up to actively monitor the absenteeism of children due to illness. A 'buddy system' is introduced in each class, whereby children are allocated to a small group and encouraged to signal to SHWs when one of the members of their buddy group is absent due to illness. SHWs follow-up with parents of signalled children to discuss health-seeking options. After a sick child is referred to a facility, SHWs receive a feedback form filled by the clinic which help them follow up with the parents to discuss adherence to treatment and return to school.

Preventive programs. SHWs coordinate with local health authorities and clinics to ensure the timely and smooth delivery of preventive technologies, such as distribution of deworming drugs or vitamin A supplementation, which in theory should be provided routinely by health authorities. In malaria-endemic areas such as the Copperbelt Province, the programme also delivers free bednets.

2.2. Theory of change

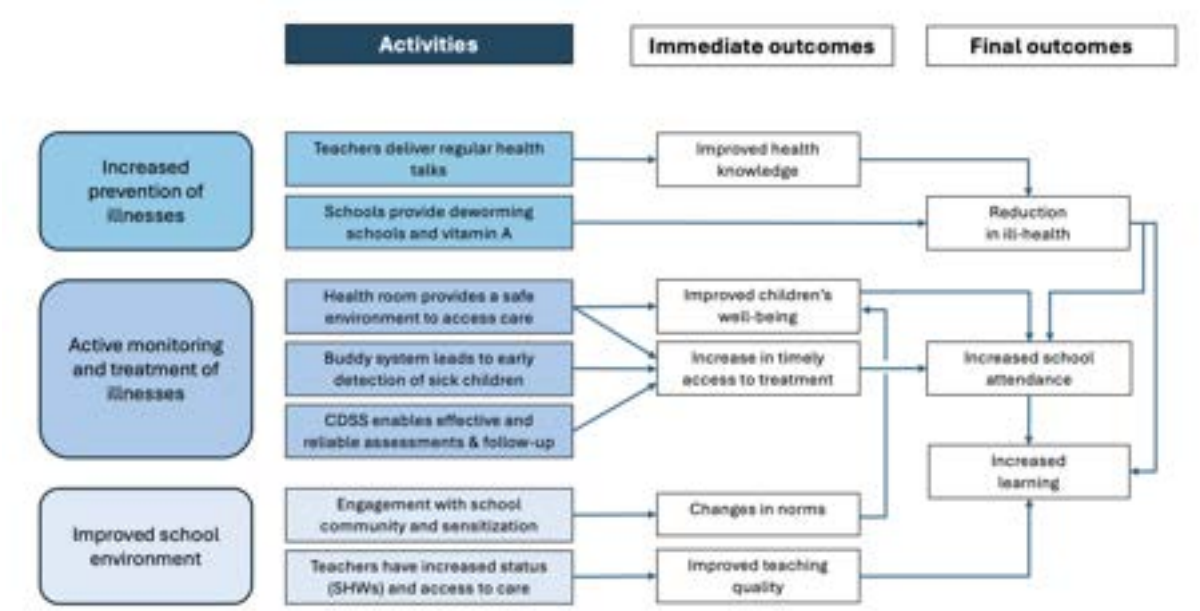
The programme builds upon three pillars which create pathways to improved health and education outcomes, as broadly indicated in the diagram below.

- **Improved prevention of illnesses**

The first pillar relates to prevention of illnesses through two pathways. On the one hand, SHWs deliver regular talks on basic health knowledge and promotion. The fact that messages are delivered by close, trusted members of the communities may enhance their effectiveness (Blair, Morse et al. 2017, Alsan

and Eichmeyer 2021). Results from the pilot evaluation showed increased knowledge and behaviour change in children (Wei, Brigell et al. 2019), which reduces incidence of illnesses.
On the other hand, the programme directly increases **uptake of preventive care** thanks to effective coordination with the local clinics and high levels of trust within the community ensuring high participation rates. Health talks and deworming have been shown to reduce anaemia and school absenteeism in the short run (Miguel and Kremer 2004), and improve health and economic outcomes in the long-run (Baird, Hicks et al. 2016), although these benefits remain unclear in less endemic areas (Taylor-Robinson, Maayan et al. 2019).

Diagram 1: Theory of change of the School Health Programme:



- **Active monitoring and treatment of illnesses**

The second set of activities seeks to create a system that enables the active monitoring and treatment of ill-health in children. The programme directly provides **access to curative care** directly and indirectly. The health room creates a safe space where unwell children (including menstruating girls) find support and care. The clinical decision support system used by SHWs enables reliable triage and assessment (Finette, McLaughlin et al. 2019) leading either to immediate access to treatment for a few common ailments or fast-track referral for more severe cases which shorten wait times – a known cause of delayed care-seeking (Coalson, Cohee et al. 2018), which can have dramatic consequences in acute cases (Mousa, Al-Taiar et al. 2020). Finally, a 'buddy system' is introduced in each class, whereby children are encouraged to signal to SHWs when one of the members of their buddy group is absent. SHWs follow up with parents of signalled children to discuss health-seeking options. Together, these factors contribute to quicker access to treatment, which should lead to shorter illness spells, improved health and reduced absenteeism.

- **Improving the school environment**

The programme creates a supportive environment that is promoting changes in social norms to enable behaviour change. To shape new norms in the school community, the programme engages with all members of local communities, including school administrators, teachers, parents, local clinic staff and community leaders. Early interactions contribute to the visibility and understanding of the program, strengthens community linkages, and create ownership and trust from local communities to support changes in social norms related to child welfare, school absenteeism, and health-seeking.

The programme endows teachers trained as SHWs with a new role, outside of their classroom duty hours (in line with concept of teacher specialization in Zambia). These teachers, who are supported by school administrators, also trained by the program, and benefit from active supervision and increases their status in the communities through their contacts with learners and their families. The programme also provides access to care for teachers and their children increasing their motivation and reducing a potential source of absenteeism.

3. Evaluation questions

Our study aims to address five research questions:

1. Does the HL programme improve health-seeking patterns and health outcomes of school learners compared to (i) the status quo in Zambia and (ii) a school-based deworming programme?
2. Does the HL programme improve school attendance compared to (i) the status quo in Zambia and (ii) a school-based deworming program?
3. Do any health and/or attendance benefits translate into improved well-being and human capital accumulation, measured by test-scores and by retention rates?
4. What are the indirect effects of the program: (i) on SHWs: does the role of SHW impact teaching effort or the quality of instruction? (ii) on clinics: does the programme have an effect on the quality of health service provision for non-programme beneficiaries?
5. Is the HL programme more cost-effective than mass distribution of deworming drugs?

4. Research design

4.1. Treatment arms

The intervention will be implemented by Healthy Learners, in close collaboration with the local education and health authorities. To obtain a sample of schools with a range of exposure to infectious diseases, schools have been chosen from three districts in the Copperbelt Province (Chingola, Luanshya and Masaiti) and three districts in the Luapula Province (Samfya, Mwense and Kawambwa).

Participating schools will be randomized to one of three groups:

- **Status quo** (75 schools): schools to operate as usual with no intervention other than the usual activities planned and organized by the government, until the end of the trial.
- **Deworming** (60 schools): Healthy Learners will strengthen the delivery of the national deworming programme to ensure that it occurs twice a year, during the same period as in the full treatment arm to ensure comparability of the effect.
- **School Health Programme** (90 schools): schools to operate as usual and Healthy Learners will deliver their full intervention, including the provision of deworming and other preventive drugs.

The deworming arm is included for several reasons. First, it will allow us to disentangle the effect of the HL intervention from the effect of a cheaper intervention that has shown to increase school attendance and long-term economic benefits (Miguel and Kremer 2004, Baird, Hicks et al. 2016, Hamory, Miguel et al. 2021). Second, this will allow us to benchmark the cost-effectiveness of the full model against an intervention that is widely supported in policy and funding circles (Evidence Action 2022). Lastly, this will allow us to attempt to replicate results from a seminal trial on deworming in Kenya, in a context where their results have not been replicated in other settings and are still met by scepticism in medical circles (Taylor-Robinson, Maayan et al. 2019).

4.2. Sampling and randomization

In preparation of the RCT, Healthy Learners collected census information about all of the schools in the study districts including location of the referral clinic, school location, enrolment, number of grades, number of teachers, presence and type of school feeding programme etc. We identified 283 primary or combined schools through the census exercise, of which 242 schools were assessed as eligible to take part in the HL programme (and therefore the study). Eligibility criteria were:

- Minimal functionality: HL will only roll out the programme in fully functional schools that have at least 5 classroom teachers (otherwise it is not possible to staff the health room). 39/283 schools did not meet this criterion.
- Reachability: the schools have to be reachable during the rainy season in order to ensure ongoing support for the SHWs and supply delivery to health rooms. 2/283 schools did not meet this criterion.

The trial is a cluster-RCT, which is most suitable for a group-based intervention such as the one evaluated here. Individual randomization is undesirable due to the spillover effects across children within schools, as well as the practical and ethical challenges that would arise from restricting access to some children in a given school.

Randomisation proceeded in two steps. First, we randomly selected 225 from 242 eligible schools (in November 2023); secondly, we randomly allocated the remaining 225 schools to the three treatment arms following baseline data collection (in June 2024). In both stages, randomisation was stratified by (1) district and (2) distance to the nearest health facility, to ensure a balanced mix of school remoteness and disease burden across the treatment arms.

Blinding of intervention units, i.e. schools, to their own treatment (or that of others) is not feasible. Even if some teachers or learners are aware of the Healthy Learners intervention in schools nearby, they would not have access to it.

4.3. Potential threats to randomization

Imperfect take-up of the programme is possible though unlikely and will not pose any threat to the identification of the programme impact. HL works in close collaboration with the district education authorities and organizes preliminary meetings with headteachers and local communities to create buy-in, oversees the SHW training, and monitors the implementation of the programme. However, implementation of the programme could be imperfect – e.g. slightly delayed (e.g., late construction of the school room), and some schools might implement the programme better than others (more health talks organized). Some of these aspects will be monitored and will help us understand the fidelity of the intervention. Regardless, this will not present any concern for the identification of the impact of the programme, as we only focus on ITT estimates. Finally, non-compliance of schools assigned to the control group is highly unlikely and could only be due to some implementation error – close coordination between the research team and HL will ensure that this problem does not occur.

A potential concern is that the programme could make schools more attractive, and parents could transfer their children from control to treatment schools. This could potentially bias downward the ITT effect of the program. This problem should be limited by the remoteness of our predominantly rural sample of schools, which should limit the appeal of transferring from one school to another and minimize the risk of transfers. Nevertheless, we will carefully record transfers of individual children in the school and household sample, and we will monitor enrolment at the school level.

Attrition is a problem for any study following up research subjects over time. To minimize attrition due to refusal to participate, we will make participation in the survey easy and attractive to participants – for example, families will be offered incentives for completing study tools. We also anticipate that there will be some loss to follow-up during the midline and endline surveys. We will introduce several strategies to minimize attrition and the risk of differential attrition rates across treatment arms. We will also introduce a routine tracking protocol to interview subjects moving during the study; in addition, we will have an intensive tracking protocol for a random sub-sample of those who could not be located during regular tracking.

5. Outcomes and data collection

5.1. Study outcomes

5.1.1. Primary outcomes

We will have two primary outcomes, aligned with the primary objectives of the programme:

- **Improving health outcomes**

Improved timely access to treatment as well as improved health knowledge should reduce the prevalence of illnesses. Because the programme does not focus on one specific disease, we will use a composite disease burden index. To construct the index, we will collect the following individual outcomes 18 months after the start of the programme:

- whether the child tests positive to malaria in a rapid diagnostic test result;
- whether the child has a moderate to high worm load (using a stool test[1]);

[1] Ahead of the collection, each school will be visited to explain the purpose of the stool sample collection and obtain parental consent. On the day of the collection, sterile Ziploc aluminium bags, labelled with a unique identity code, will be issued to each of the consented child. Advice will also be provided on how to handle the stool bag and specimen. Fresh stool samples will be transported to a laboratory in cool

- whether the child is anaemic (using a hemocue test)
- whether schistosomiasis is detected in the child's urine;
- whether the child reports having had diarrhoea in the past week.

The index will be constructed using inverse covariance-weighted average disease rates, following Anderson (2008). We use a summary index as the primary specification as we expect that all variables in the index are likely to move in the same direction with different magnitudes. Collapsing the outcomes into an index will therefore reduce the number of outcomes tested, avoiding the issue of spurious findings resulting from multiple hypothesis testing.

- **Increasing attendance of children at school:**

Our primary education outcome is **attendance**. We expect that children's attendance will increase due to the combined effect of reduction in morbidity, improved access to health services (at school and health centres), as well as improved health-seeking behaviours and shorter illness spells.[2]

We will measure attendance during unannounced attendance spot checks (one per term over a 2-year period). Visits will be planned to follow a specific schedule which will seek to capture seasonal variation. Schools will not know when the visits occur; visits will be carried out by the research firm, not the implementing NGO.

On each spot check visit, we will check attendance of a panel of 60 learners, randomly selected at baseline. We will calculate each child's attendance rate across all the spot checks and conduct analysis at the individual learner level.

boxes immediately after collection at the school. Collection of the stool samples will be done in close collaboration with the Ministry of Health to ensure a smooth implementation.

[2] Although the original pilot study did not find any improvement in school attendance, we believe that this null effect was partly due to problems with the self-reported measure use. Furthermore, significant changes have been made to the programme since which significantly increase the likelihood of reducing absenteeism (improved care-taking of children with the introduction of ThinkMD, introduction of the buddy system to track absenteeism efficiently, provision of phone to help SHWs follow-up with parents and implementation of sensitization campaigns to change social norms around school absenteeism).

5.1.2. Secondary outcomes

In addition to the impact on the primary outcomes, we will investigate the effect of the programme on a range of other outcomes for the children, but also consider the broader effects of the programme on teachers and clinics.

SECONDARY HEALTH OUTCOMES

- **Access to formal healthcare services**

The primary mechanism through which the intervention can improve health is by increasing utilization of healthcare services by school-aged children when they are ill. To assess the impact of the intervention on access to care when ill, we will measure the proportion of children who were taken to a clinic or received medicines at school when ill in the past 2 weeks.

In household surveys at 12-month and 24-month follow up, we ask parents about a set of 9 common symptoms experienced by school-aged children: diarrhoea, fever, coughing, difficulty breathing, a blocked or runny nose, convulsions, vomiting, skin rash, worms in their stool. Conditional on having had any symptom, we will ask if the guardian sought care for the child and record any access of formal healthcare services as child going to the clinic or the school health room, or child taking medication received in the clinic or school health room.

- **Care-seeking**

To obtain more granular data about timely use of care, we will use high-frequency symptom diaries that will allow us to measure (i) the prevalence of under-use of healthcare (i.e. a situation where a child does not seek care or delays seeking care, where symptoms indicate a need to seek medical advice); (ii) the number of days where care is needed but not sought, and (iii) the existence of over-use of care (i.e., cases where care was sought in healthcare facilities for minor symptoms) to evaluate potential unintended effects. We will collect the diaries over an 8-week period capturing daily health symptoms and care-seeking decisions in the 18-month follow-up.

- **Morbidity**

We will measure morbidity in two ways. First, through household surveys and symptoms diaries, we will obtain the proportion of school-age children reporting experiencing ill-health symptoms over a given period. Second, with symptom diaries, we will obtain a measure of the duration of illness episodes.

We capture the prevalence of common childhood illness symptoms in a recent recall period (2 weeks) through surveys administered to children's guardian/parent at baseline (before the start of the SHP), midline and endline (respectively 12 and 24 months later). We will combine the prevalence rates for all symptoms into an index, following Anderson (2008), to limit the number of hypotheses tested.

Through symptom diaries, we will obtain measures of the incidence and duration of illness for each child (average number of spells, duration of spells, and severity of spells).

Finally, we will measure height and weight[3] and report age-standardised ratios to measure nutritional outcomes (prevalence of stunting, underweight and overweight). Reduced worm loads and improved treatment of conditions such as malaria should reduce anaemia and fatigue levels. Together with

[3] Weight will be measured with a calibrated electronic scale and height with a stadiometer using a telescopic measuring rod according to acceptable standardized methods. Enumerators will be trained to ask children to remove shoes and hats when measuring height and to take off heavy outer clothing before stepping on the scale.

improved hygiene conditions, these could lead to reduction in malnourishment, as suggested by the pilot study (Wei, Brigell et al. 2019).

- **Mental health and well-being**

We expect the intervention to have indirect effects on the well-being and/or mental health of learners, due to the community building components of the programme (such as the buddy system for monitoring of absent learners) and increased focus on learner welfare among the teachers.

For mental health, we measure symptoms of anxiety and depression using the DSM-5 scale (administered to parents only for ages 6-10 and both parents and children for ages 11-17). For wellbeing, we will collect the Paediatric Quality of Life Inventory (PedsQL), with items on physical, emotional, social and school functioning, adapting items to the Zambian context through piloting and qualitative work. Finally, to evaluate any changes in collegiality stemming from the programme, we will collect information about the learners' social networks (average number of connections, strength of connections, network density, and segmentation) and the extent of bullying in the schools (through learner reports of 5 types of events in the last week in school, such as being pushed or kicked, having items snatched, or being called names). These outcomes are captured through learner surveys during the midline and endline (respectively 12 and 24 months after programme start).

- **Health knowledge**

As part of the programme, SHWs give regular lectures to students about health issues. Therefore, we would expect the knowledge of students to improve as a result to exposure to these lectures. Health knowledge will be measured at baseline, midline and endline. Health knowledge will be tested by a series of simple questions related to basic hygiene and health notions, including questions relating to menstrual health practices (for girls close to the age of menarche).
To capture any spillovers in knowledge from learners to their household, we also set these tests in surveys to parents/guardians and siblings.

- **Health behaviour**

Preventive behaviours will be measured through a combination of self-reported outcomes and surveyor observations (e.g. malaria bednet usage, handwashing, observed hygiene). School incidence of pregnancy in older grades, recorded in surveys with the headteacher, will indirectly capture unsafe sexual behaviours.

- **Menstrual hygiene**

As the school health room is providing a safe and private space for girls to seek advice and emergency provision to manage periods, we would expect menstrual hygiene to improve. In addition, the health lesson curriculum includes period management and attitudes. We will ask female learners over 11 about experiencing symptoms of UTIs at endline, as improvements in menstrual hygiene are associated with reductions in UTIs (Phillips-Howard, Nyothach et al. 2016). In addition, we will elicit menstrual hygiene management, knowledge, and attitudes through an 8 item scale (developed by the research team using a range of sources; we will conduct a separate validation sub-study for this tool).

SECONDARY EDUCATION OUTCOMES

All learners enrolled at baseline will have been exposed to the programme for two years at endline.[4] We believe that this initial time frame may be sufficient to observe changes in learning ability and general academic learning (mainly literacy and numeracy).

- **Learning ability**

[4] We do not rule out the possibility to follow these cohorts for longer if promising results emerge.

Changes in **learning ability** would be directly linked to improvement in health outcomes. Children's learning ability will be measured through standardized tests administered in learner surveys at endline, measuring working memory and attention (e.g. digit span tests).

- **Learning (test scores)**

For **general academic learning**, we believe the combined effects of (1) reduced children absenteeism, (2) improved parental engagement with the school (resulting from the fact that schools providing both health and education to their children will appear more valuable), (3) increased teacher motivation (and, possibly increased teacher attendance resulting from improved access to care and motivation) and (4) improved student health and well-being, may translate into changes in learning outcomes within the study's time frame. Even though evidence on these links is mixed, some studies have shown that improved health or teachers' reduced absenteeism improves learning outcomes (Miguel and Kremer 2004; Duflo, Hanna, and Ryan 2012), even over a 1-2 year window. Measuring academic learning will also allow us to document or rule out potential negative effects that could occur as a result of effort displacement by teachers serving as SHWs.

To measure improvement in learning, we will use a combination of administrative data (grade 7 exam results obtained from the school administrative records) and standardized tests administered individually to learners through age-appropriate tests, administered during the learner surveys at midline and endline, in the official language of instruction of the school.[5]

- **Attendance of upper year students**

We expect that attendance of upper year students will increase due to the health benefits of the programme and additionally due to improved menstrual health management of the female learners. To capture attendance in this group, we will conduct six unannounced spot checks (one in each term throughout the two-year study period), when we will randomly select two classes in grades 6 and 7 of upper primary; work with the class teacher and registers to calculate how many learners are registered in the class; and conduct a head count of learners to capture the attendance rate.

INDIRECT EFFECTS AND MECHANISMS

- **Teachers**

We will consider the effects of the programme on teachers by comparing a random sample of 10 at each school across the three treatment arms.[6] Outcomes of interest will include (i) teacher absenteeism from the school and the class room captured through spot checks; (ii) teaching quality using structured classroom observations using the Teach Primary standardised tool (Molina, Fatima et al. 2020); and (iii) teacher retention rates, captured through administrative records.

To test whether the programme has a negative impact on the quantity and quality or learning provided by teachers, we will collect information on all teachers who applied to the SHW positions but were not selected.[7] We will then compare the learning outcomes of children in our panel taught by a SHW

[5] We will build a set of learning assessments using items that have been tested and validated in Zambia, as well as additional items that from international assessments that will allow us to benchmark learning levels relative to global scales. In addition to the Zambia EGRA and EGMA assessments, we will use items from an on-going JPAL study on "Teaching at the Right Level" in Zambia that covers similar grade levels as our proposed study. Finally, the team will use items from international assessments such as TIMMS (4th grade) and SACMEQ (if available) to facilitate better comparisons to other studies.

[6] Although ideally we would randomize the role of SHWs to teachers, this was not deemed feasible by Healthy Learners as it would be difficult to get it accepted by schools. Furthermore, the Healthy Learners programme has a sophisticated system of selecting teachers into the SHW role. Randomly assigning teachers to this position would likely affect the fidelity of programme implementation. This prevents us from causally identifying the effect of the SHW role on teacher outcomes. Still, we plan to make comparisons about the effect of the programme on our sample of teachers across treatment arms and can describe differences in teacher outcomes between SHWs and other teachers.

[7] The selection process involves the headteacher, district and HL representatives. The positions always receive a lot more interest than available positions, so a 2-step selection process is introduced. First, a shortlist including roughly twice the number of available positions is established by the headteacher. Second, applicants are interviewed and selected based on a few criteria (suitability for the SHW post, necessity to have a variety of teacher grades and gender, proximity of the teacher etc).

or a teacher who unsuccessfully applied to be a SHW and use data on children's reports of teacher behaviour to provide additional suggestive evidence on this potential mechanism.

- **Clinics**

It is possible that the programme could overwhelm the local healthcare system by referring many children, resulting in increased waiting times for patients. Using referral data from the programme and data on outpatient visit volume extracted from facility registers, we will determine the increase in volume coming from children referred by the program. In addition, we will undertake clinic visits to measure average waiting times of patients, by recording the start and end time of visits conducted by up to 15 patients attending the clinic that day.

Even if students are referred to clinics, poor quality of clinics could thwart the intended benefits of referrals. For example, if qualified staff are not present, if drugs are missing or if staff are unable to diagnose correctly the referred children. We will evaluate the capacity of reference clinics to provide adequate treatment. We will conduct spot checks visits to record the presence of qualified staff and essential medicines required to treat the most common diseases. Through validated clinical vignettes of common childhood illnesses, we will assess the competence of staff to treat learners. We will experimentally vary whether providers are given the HL referral forms during as part of the clinical vignettes, to measure the effect of the referral form on the ability to identify and treat referred children adequately.

- **Norms and beliefs**

From anecdotal evidence and past reports of teachers and communities involved in the programme elsewhere in Zambia, it seems that the programme is likely to have an impact on parents, by shifting norms regarding child health and education. In addition, the programme may improve perceived benefits of schools and schooling, with impact on beliefs about long-term economic prospects. We expect these outcomes to be refined by qualitative work, but for now, we anticipate that we will look at the impact of the programme on:

- Norms around **corporal punishment**. We will capture incidence of corporal punishment by parents/household members and by teachers in school by asking learners (privately) if anyone in their household or at school has used a set of punishment techniques with them. The list of corporal punishment techniques is drawn from the DHS and verified with HL staff to ensure it is appropriate and culturally sensitive in this context. We expand the list with additional items of acceptable punishment forms to mask the sensitive items of interest. To capture social norms around corporal punishment, we will present vignettes about example teachers and parents responding to learners misbehaving, and capture perceptions about appropriate and inappropriate responses. We will also measure how participants believe other members of their community would respond, to differentiate between private beliefs and perceived social norms (Bursztyn, González et al. 2020).
- Norms around **menstruation**. We will evaluate both knowledge and stigma associated with menstruation through survey measures. In addition, we may draw on lab-in-the-field experiments to derive more reliable and direct measures of the willingness of students to discuss menstruation in public following Macours, Vera et al. (2024).
- **Economic expectations**. We will collect data on parents' and older children's educational and labour market expectations, drawing on questions from the economic literature (Giustinelli 2023). We will measure parents' perceived value of school investment and collect willingness to pay measures to estimate how much parents value the programme.

All measures of beliefs and social norms are highly context and participant specific. Therefore, we will use qualitative interviews and collect pilot data to validate and refine these tools extensively prior to data collection.

- **Process evaluation**

We will conduct a process evaluation using quantitative data from observations, the midline and endline surveys, and routine data from the HL monitoring system, as well as in-depth interviews with

school and HL staff, and focus group discussions. The aim will be to enable us to understand (i) fidelity of the programme implementation; (ii) programme reach; (iii) its acceptability; and (iv) the social, structural and logistical factors that impede or facilitate these.

To measure the fidelity of the SHW training, we will collect pre- and post-training data for trained teachers, using a broad set of questions which cover their knowledge of child health, specific diseases, and the body, as well as their understanding of their role as a SHW and the SHP more broadly.

Using monitoring data from HL, in all intervention schools, we will capture the number of teachers trained by the school; the number and share of learners per school assessed, treated and referred; the share of referred children seen the same day at the clinic; the share of children in a school receiving preventive services (e.g., deworming drugs), and the number and topics of health talks delivered. Learners' exposure to programme activities will be further triangulated in midline and endline surveys.

To measure programme acceptability, we will conduct semi-structured in-depth interviews (IDIs) with headteachers and teachers in schools purposively sampled to ensure overall variation across schools in location, size and health profiles of students. We will also interview staff in nearby clinics to understand their perceptions of the impact of the programme on health facilities and on the community. Finally, interviews will be organised with HL staff, purposefully sampled to reflect the different support functions of the organisation. More in-depth qualitative data will be collected from five purposively selected case study schools: group discussions with female and male students (separately) and school staff at midline and endline; and IDIs with staff and students at midline and endline and with caregivers at endline.

- **Cost-effectiveness analysis**

Our cost-effectiveness analysis (CEA) will assess the value of the interventions in our study in relation to each other, as well as in comparison to other interventions that target the same primary outcomes. To compute programme costs for the full SHP and the two alternative models, we will obtain detailed implementation costs from HL and the MOE. We will then compute the unit cost per child covered, and the incremental cost-effectiveness ratio of the full SHP per unit increase in policy-relevant outcomes, relative to the two other models. We will benchmark these estimates against other school-based health interventions in the region to inform scalability.

HL will use a structured approach to collect implementation costs. Before implementation, HL have catalogued all known expenses related to the intervention, including items directly provided to participants, staffing costs for programme implementation, and resources allocated for intervention development. HL will continuously monitor and update the costs throughout the roll-out of the intervention, helping guarantee any changes or new expenses are accounted for. At this stage, we plan to collect the following cost data:

CATEGORY	COST ITEM
SCHOOL HEALTH FIXED COSTS	Construction of health rooms
	Teacher training
	HL programme staff
	DEBS/MOH staff
	SHP supplies
	Occupancy costs
	Transport and logistics
	Electronics (tablets, phones)

	Community engagement activities
SCHOOL HEALTH VARIABLE COSTS	HL programme staff
	Occupancy costs
	Transport and logistics
	Software subscriptions
	Programme allowances
	Programme meetings and mentorship activities
	Supplies (for health rooms, preventative care rollouts)
	Communication (airtime and data)
	Electronics (replacements)
	Trainings (replacement of SHWs)

Surveys with school and health facility administrators will inform the collection of any additional expenses which may be borne by the schools and the communities in which the programme is taking place (e.g., additional staffing). Likewise, we will capture whether the intervention leads to significant avoided costs (e.g., communities need to organise fewer nutritional campaigns). The research team will continue to iterate with HL to refine the list so that all cost components are captured accurately.

To analyse cost-effectiveness, we will use results from the randomized evaluation to calculate effect-cost ratios for each intervention (i.e., Outcome in T1 / Cost of T1) and incremental ratios to compare interventions (i.e., (Outcome of T1 - Outcome of T2) / (Cost of T1 - Cost of T2)). Given the variety of outcomes in this study, based on continuous and categorical data, we will present standardised outcome to cost ratios, reporting the cost per %SD change in outcomes.

5.2. Data collection

The research activities will then draw on different population samples within each school, depending on the type of data collection approach.

Attendance spot checks. School attendance will be measured through 6 unannounced spot checks[8] spread over the duration of the study. Given that schools can be very large (more than 1,500 children in urban areas), it would be challenging to survey all children in all schools at each visit. Instead, during each school visit, the research team will record the presence of two groups:

- The 60 children enrolled in the study from baseline;
- All children enrolled in two classes of grades 6 and 7 (we will count the number of girls and boys present in the classroom and compare it to the total number of boys and girls in the class from the registry). The choice of these two grades will allow us to over-sample older children, where we expect that girls might be more likely to miss school due to menstruation management issues.

Household surveys. Before the start of the program, we will randomly select 60 children per school, equally sampled from three grades (grade 1, grade 3 and grade 5). Households will be interviewed three times in total: at baseline before the start of the program, 12 months after the start of the programme (midline) and 24 months after the start of the programme (endline). Interviews will be carried out with learners' parents or guardians to elicit health-seeking behaviours and its determinants, and school valuation. Short tests measuring learning abilities and academic skills will also be administered to school-aged children during these surveys.

[8] We envisage that two or three of the spot checks will occur at the same time as a school survey.

The **clinical outcomes** that will contribute to our morbidity index will be collected at endline in sub-sample of 30 children. These will include (i) haemoglobin level measured by a point of care hemoglobinometer; (ii) prevalence of malaria measured by a malaria Rapid Diagnostic Test; (iii) and prevalence of moderate to high worm load in the stools[9] and (iv) prevalence of urinary tract infection and Schistosoma haematobium in urine samples.

Symptom diaries. In each school, half of the households (n=30) taking part in the household surveys will be chosen to fill in pictorial symptom diaries and treatment choices for 8 weeks: once during the rainy season, and, funding permitting, once during the cold season. Pictorial diaries will be distributed to families to record daily a range of health symptoms of their children. Different alternatives for collecting diaries will be piloted to ensure quality and reliability of the data.

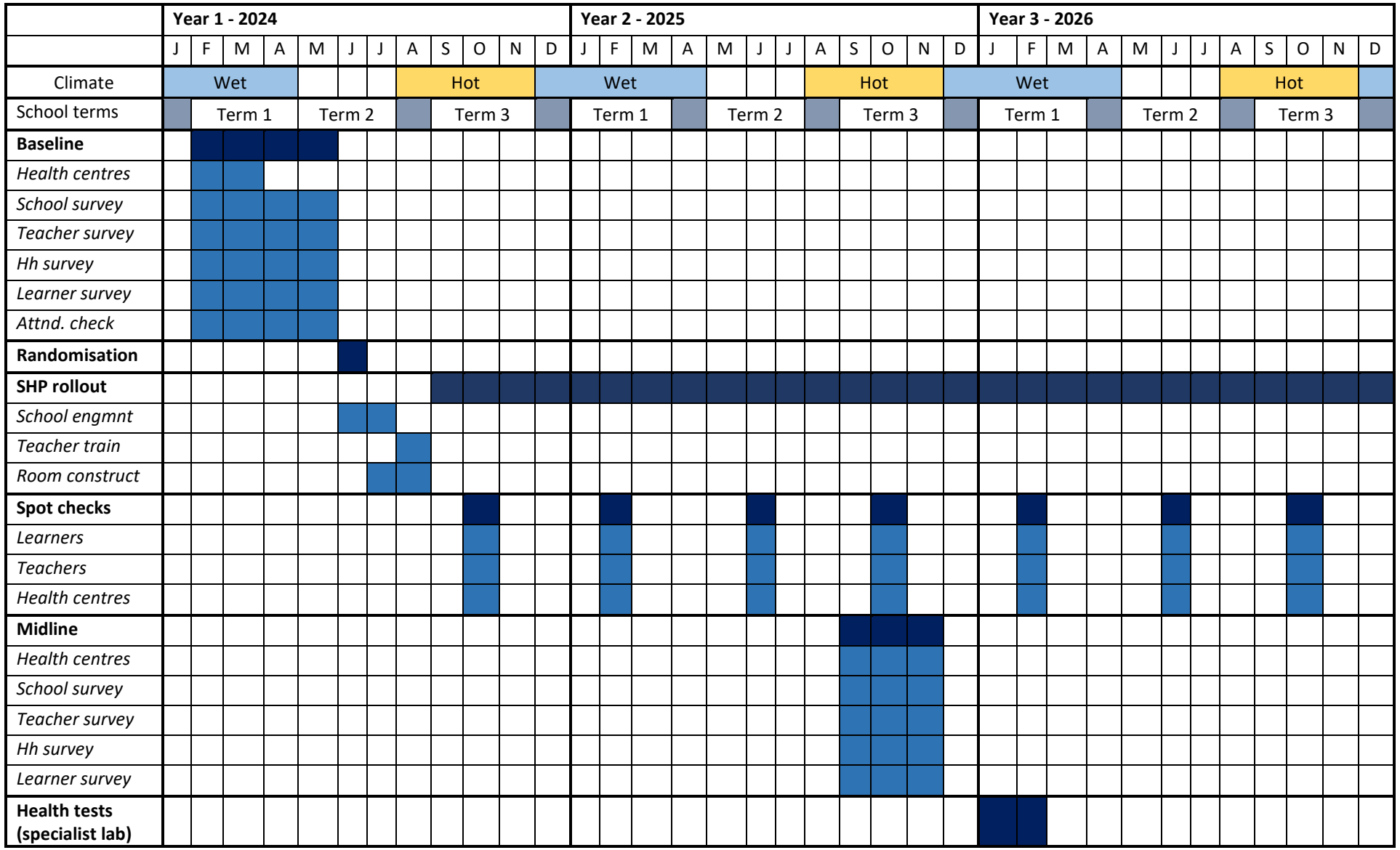
School surveys. Each study school will be visited three times (baseline, midline, endline – around the same time as household surveys). During school visits, the head of the school and ten teachers will be selected to be interviewed.[10] All teachers will be interviewed three times: before the start of the programme, then 12 and 24 months later. At each unannounced school visit, their presence and activity in school will be checked. In addition, two attendance spot checks will be used to conduct teaching observations in the classroom.

Health facility surveys. We will undertake unannounced visits to all health facilities attached to control and treated schools (because in urban areas more than one school may fall within the catchment of the same health facility, we envisage that we will survey a sample of n=160 facilities). During the visit, we will measure staff absenteeism, drug stocks of essential medicines and average patient waiting times. We will also interview all staff responsible for children consultations present on the day of the visit to evaluate their clinical skills using clinical vignettes based on actual school-aged children cases; we will also use the clinical vignettes to determine to what extent the ThinkMD-powered referral forms listing symptoms facilitate establishment of correct diagnoses.

The timeline of the main data collection activities is shown below. Activities in the treatment arms will start at the end of the first quarter of 2024. Before they start, we will conduct our baseline data collection activities with households and teachers. Once the programme starts, we will organise a series of attendance spot checks, a midline survey in the second year, with health diaries during the rainy season, and an endline survey in the 3rd year, with health diaries in the cold season.

[9] Ahead of the collection, each school will be visited to explain the purpose of the stool sample collection and obtain parental consent. On the day of the collection, sterile Ziploc aluminium bags, labelled with a unique identity code, will be issued to each of the consented child. Advice will also be provided on how to handle the stool bag and specimen. Fresh stool samples will be transported to a laboratory in cool boxes immediately after collection at the school. Collection of the stool samples will be done in close collaboration with the Ministry of Health to ensure a smooth implementation.

[10] This may not be possible in the smallest schools if most teachers are trained to be SHWs.


Year 1 - 2024
Year 2 - 2025
Year 3 - 2026
J F M A M J J A S O N D
Climate
Wet
Hot
School terms
Term 1
Term 2
Term 3
Baseline
Health centres
School survey
Teacher survey
Hh survey
Learner survey
Attnd. check
Randomisation
SHP rollout
School engmnt
Teacher train
Room construct
Spot checks
Learners
Teachers
Health centres
Midline
Health centres
School survey
Teacher survey
Hh survey
Learner survey
Health tests (specialist lab)

	Year 1 - 2024												Year 2 - 2025												Year 3 - 2026											
	J	F	M	A	M	J	J	A	S	O	N	D	J	F	M	A	M	J	J	A	S	O	N	D	J	F	M	A	M	J	J	A	S	O	N	D
Climate	Wet							Hot					Wet							Hot					Wet							Hot					
School terms		Term 1			Term 2				Term 3					Term 1				Term 2				Term 3				Term 1				Term 2				Term 3		
Health diaries																																				
Endline																																				
Health centres																																				
School survey																																				
Teacher survey																																				
Hh survey																																				
Learner survey																																				

5.3. Statistical power

The tables in Appendix below summarise the minimum detectable effects (MDE) for the primary outcomes above. For completeness, we provide the MDEs for the following the two comparisons that we will be interested in: (i) Full programme vs. deworming; (ii) Full programme vs. control.

Power calculations have been made assuming an alpha of 0.05 and 80% power. Specific assumptions about intra-cluster correlations and reference levels in the control group are indicated. These MDEs do not account for possible gains in precision coming from the inclusion of baseline outcome values or individual variables predictive of an outcome. We anticipate to use double LASSO estimators to choose control variables in final models (Belloni, Chernozhukov et al. 2014).

In our calculations, we use intra-cluster correlation coefficients (ICC) estimates that range from 0.05 and 0.10. A review of the values of ICCs in c-RCTs of school-based interventions aiming to improve health outcomes in learners found that two-thirds of school-level ICCs were no greater than 0.05 and three-quarters were under 0.08 (Parker, Nunns et al. 2023). For education outcomes, using data from the national surveys, a study found that ICC was 0.15 in mathematics in Zambia (Kelcey, Shen et al. 2016), while a recent impact evaluation of a programme found that the ICC for reading was also 0.15 (Ome and Menendez 2018). Results also indicated that covariance adjustment generally reduced clustering and accounted for up to 30% of the school variance (Kelcey, Shen et al. 2016).

Table 1: Minimal Detectable Effects for primary outcomes

Outcome description	Outcome levels		MDE		Assumptions
	Control group	Deworming group	Control v. HL	Deworming v. HL	
Proportion of sick children accessing care	50%	50%	8.86 pp (9.74pp)	9.43 pp (10.37 pp)	ICC=0.15; n=55 in each school (allowing for attrition)
Standardized index of morbidity	Level standardized to 0 and SD=1	-0.05	- 0.158 SD (-0.174 SD)	-0.168 SD (-0.185 SD)	ICC=0.10; n=30 in each school
School attendance	85%	90%	4.75 pp (5.35pp)	4.22 pp (4.58pp)	ICC=0.10

Note: The two MDEs presented are unadjusted and adjusted (in parenthesis) for the two comparisons. For access to care, we are assuming a conservative rate of 50% to calculate the smallest absolute change we would be able to detect.

Additional indicative power calculations for some of our secondary outcomes are indicated in Appendix.

6. Ethics

The project has already received ethics approval from the London School of Economics and ERES Converge IRB in Zambia. In addition, the study received approval from the National Research Health Authority, the Ministry of Education as well as the provincial directorates.

We highlight below the main ethical questions raised by the Trial.

Informed consent. All participants will receive concise and clear information explaining the study objectives and terms of consent in the vernacular. Participation will be entirely voluntary and eligible individuals will be informed that their decision to participate or not would not impact children's education. In addition to written parental consent, we will collect written child assent, appropriate to age and level of understanding. Due to likely low literacy levels for some in our sample, if participants are unable to read the information sheet or sign themselves, an impartial witness will be asked to sign on their behalf, confirming that: 1) the enumerator read out loud all information about the study as

per the information sheet; 2) the participant had opportunity to ask any questions they had and 3) that all effort was made to ensure the participant understands the terms of participation, as detailed on the information sheet.

Minimizing harm. Because the study involves school-age children, we will develop and train all research staff in child protection policies to safeguard children from any form of coercion or pressure during the study. For the collection of stool samples and skin prick tests, protocols will be developed to minimize discomfort or distress and ensure the safety of participants. We will consider and respect the local cultural norms and beliefs surrounding samples, and work in partnership with the MOH to engage with communities and explain the study objectives. Clinically trained staff will ensure that samples are handled and processed correctly. If lab results indicate a health problem, guidance will be given to parents about potential next steps, including referral to local clinics to receive care.

Sensitive topics. We have undertaken additional legal and contextual research to appropriately handle capturing data on sensitive research topics (as defined in the Economic and Social Research Council's guidance). In particular, we seek to test (as secondary outcomes) if the SHP leads to changes to norms around corporal punishment in the school and in the community more broadly. We drew on expertise of local policymakers and the senior legal team at Healthy Learners, as well as guidance on child protection in Zambia,[11] Child Ethics (partners to UNICEF),[12] the Healthy Learners Child Protection Policy for School Health Teachers, and UK Research Ethics Board guidance.[13]

Questions about acts of corporal punishment committed or witnessed will be asked of 1) teachers; 2) parents or guardians, and 3) learners. In order to collect this data, we need to appropriately handle two situations: 1) an adult study participant themselves admits to using corporal punishment; 2) a learner discloses incidence of abuse, or an enumerator becomes suspicious about possible abuse (which is both part of general safeguarding procedures, and may be more likely if we explicitly ask about corporal punishment).

Ethical issue 1: if an adult in the study admits using corporal punishment, researchers may have a duty to report this (thereby compromising participant confidentiality). We consulted the senior team members of Healthy Learners in the legal department and/or with experience in education policy. We identified that while the use of corporal punishment has been banned in schools, there is no norm of enforcing this (and likewise there is no enforcement in the household). Social norms and attitudes around corporal punishment in Zambia vary from those accepted in the UK/US/EU – in qualitative discussions, participants were more accepting of these practices as appropriate to use for discipline and generally open to discussing these topics. Jointly with Healthy Learners, we determined that 1) it would not be sensitive to local norms for us to report every incident we learn about and 2) the value of the research is very high, as reliable data about these practices is scarce, and if the SHP does shift norms, this would be a key finding of interest. We will report aggregated statistics obtained from baseline data to the District Education Boards (DEBS), but we will not report individuals or school-level statistics – unless we learn of practices which would be deemed abuse in this context.

Ethical issue 2: handling disclosure of abuse, or witnessing abuse of learners. Understanding that local best practices and the social services sectors vary widely between contexts, our protocol for responding to incidents of abuse is guided by our local partners, Healthy Learners, based on their extensive legal and policy experience within the school sector. We also received advice from the local ethics board (ERES) and the National Health Research Authority. The recommendation is that, if it is

[11] Parliament of Zambia, The Children's Code Bill, 2022.

[12] Privacy and Confidentiality Ethical Guidance, 2024 Child Ethics (UNICEF)

[13] Safeguarding Children in Research Contexts, Sheffield Hallam University Research Ethics Board and the references within

possible, resolution within the community would likely lead to the best outcomes for the child. Reporting any incidents to enforcement authorities might result in worse outcomes for the child.

Any witnessed or disclosed events would be reported by the enumerator to their field supervisor, who in turn would communicate with the PIs. The enumerators are trained extensively on how to handle any reports from the child within the school, including appropriate behaviour around the child and their obligations towards the child. They are also given contact details for the child help line and the GBV help line, to use in emergency situations.

The research team will respond to any reports on a case-by-case basis, with support from Healthy Learners' legal team. The first point of contact for resolution of the situation will be the headteacher (unless this is the alleged abuser, in which the case will be handled with the DEBS) and their advice sought for handling the matter within the community. We would oversee proceedings through local Healthy Learners officers stationed in each study district.

Given their vulnerability, it would not be appropriate to keep the child in the study (note: participation in the study does not itself benefit participants in any way – the child would still receive any treatment delivered at the school level). Enumerators are trained to speak to the child about the disclosure and their right to confidentiality: based on feedback from the research team, we may be obliged to report the incident and waive their right to secrecy. We would strive for the child's agreement to any case being reported up, but would need to act in accordance with our duty to prevent harm and protect the child's best interests if we do not have their agreement.

Ancillary care. Collecting biometric disease burden data leads to a moral duty to facilitate ancillary care to those participants who test positive to any diseases we test for. Given the availability of free care in the local health sector, our approach will focus on 1) informing the guardians of the learner who tests positive to any condition, educating them about the condition and its appropriate management, and referring them to their health facility. We will ensure that the information provided is clear and simple to understand for the guardians. 2) Using data from health facilities, we will also evaluate if there is sufficient capacity in the local healthcare system to manage the cases we refer (supplies of medication, staffing levels); if not, we will work with our partners in the Ministry of Health to strengthen these facilities in preparation for biometric testing.

Incentives. We plan to offer incentives to participants of Kw 50. For the symptom diaries, we plan to offer incentives to parents to bring back diaries to schools each week. We will carefully set these incentives at a reasonable level to avoid creating distortions or undue enticements but compensate fairly for the time taken to travel. We plan to offer a lumpsum of Kw 500 to the school to pay for snacks and refreshment to be provided to all staff supporting the activities taking part in the school over the course of the four-day visit. We will make sure that this is formally communicated to a group of individuals, to ensure that the lumpsum is used to.

Confidentiality and privacy. Careful protocols and training will be in place to collect data in a way that protects the identity and personal information of participants. This will include de-identifying data and storing separately personal information (names, contact, location) necessary to follow-up participants, storing data securely, and restricting access to researchers only.

Sharing of results and benefits. Results will be shared with all participating school communities through events organised by HL. If the programme is effective, HL already has plans to scale it up to all participating groups, so that all communities involved in the research will benefit.

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Appendix Table 1: Minimal Detectable Effects for health-related outcomes

	Outcome	Tool	Expected level in control schools	MDE		Assumptions/ Comment	
				Control v. HL model	Deworming v. HL model	Expected level in deworming schools	
1.	Prevalence of ill-health symptoms	HH survey	20%	-6.617 pp	-6.992 pp	Same as control	ICC=0.10 ; n=55 in each school (allowing for attrition)
2.	Average duration of illness spells	Diaries	5 days illness spells (sd=5)	-0.955 days	-1.018 days	Same as control	ICC=0.10 ; n=10 (assuming 1/3 have at least 1 illness spell)
3.	Prevalence of under-use of care	Diaries	50% [(d)]	-7.863 pp	-8.374 pp	Same as control	ICC=0.15 ; n=10 (assuming 1/3 have at least 1 illness spell)
4.	Av. Number of ill days where care not sought	Diaries	4 days illness spells (SD=4)	-0.764 days	-0.814 days	Same as control	ICC=0.10 ; n=10 (assuming 1/3 have at least 1 illness spell)
5.	Health knowledge (index)	HH survey	n/a	0.114 SD	0.121 SD	Same as control	ICC=0.05 ; n=55 in each school (allowing for attrition)
6.	Health knowledge (% correct responses)	HH survey	50%[(d)]	5.668 pp	6.039 pp	Same as control	ICC=0.05 ; n=55 in each school (allowing for attrition)

Notes: (a) 50% of children under 5 are anaemic according to DHS Zambia; data on school-age children are less common, though prevalence in the MENA region was 22% (Joulaei, Keshani et al. 2021) (b) 35% in under 5 according to DHS; (c) based on 20% in under 5 according to studies ; (d) unknown so assuming worst possible proportion to detect absolute level of MDE.

Appendix Table 2: Minimal Detectable Effects for education-related outcomes

	Outcome	Tool	Expected level in control schools	MDE		Assumptions	
				Control v. HL model	Deworming v. HL model	Expected level in deworming schools	Intra-cluster correlation, sample size
1.	Children's learning ability (working memory and attention)	HH survey	n/a	0.114 SD	0.121 SD	Same as control	ICC=0.05 ; n=55 in each school (allowing for attrition)
2.	Children academic learning: (age-specific standardized test)	HH survey	n/a	0.149 SD	0.159 SD	n/a	ICC=0.10 ; n=55 in each school (allowing for attrition)
3.	Children academic learning: Grade 7 exam score (standardized)	Administrative records	n/a	0.149 SD	0.158 SD	n/a	ICC=0.10 ; n=60 in each school (assumed average number of G7 children in school – likely conservative)

Notes: (a) Data from the Ministry of Education 2020 database.