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Effects of the Informed Health Choices secondary school intervention on the ability of students in Rwanda to think critically about health choices: A cluster-randomized trial

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Abstract

Aim: The aim of this trial was to evaluate the effects of the Informed Health Choices intervention on the ability of students in Rwandan to think critically and make Informed Health Choices.

Methods: We conducted a two-arm cluster-randomized trial in 84 lower secondary schools from 10 districts representing five provinces of Rwanda. We used stratified randomization to allocate schools to the intervention or control. One class in each intervention school had ten 40-min lessons taught by a trained teacher in addition to the usual curriculum. Control schools followed the usual curriculum. The primary outcome was a passing score (≥ 9 out of 18 questions answered correctly) for students on the Critical Thinking about Health Test completed within 2 weeks after the intervention. We conducted an intention-to-treat analysis using generalized linear mixed models, accounting for the cluster design using random intercepts.

Results: Between February 25 and March 29, 2022, we recruited 3,212 participants. We assigned 1,572 students and 42 teachers to the intervention arm and 1,556 students and 42 teachers to the control arm. The proportion of students who passed the test in the intervention arm was 915/1,572 (58.2%) compared to 302/1,556 (19.4%) in the control arm, adjusted odds ratio 10.6 (95% CI: 6.3–17.8), $p < 0.0001$, adjusted difference 37.2% (95% CI: 29.5%–45.0%).

Conclusions: The intervention is effective in helping students think critically about health choices. It was possible to improve students' ability to think critically about health in the context of a competence-based curriculum in Rwanda, despite challenging postpandemic conditions.

KEYWORDS
adolescents, critical health literacy, health literacy, Informed Health Choices, Rwanda

1 | INTRODUCTION

Adolescence is a critical stage in life where young people start to make choices on their own, including health choices. Most of the health choices that adolescents and adults make stem from claims that family members, peers, communities, and Internet, media, and social media sources believe to be true.[1–3] Young people and many adults are unable to assess the trustworthiness of health claims.[4,5] Failing to base decisions on reliable evidence when making choices can result in waste of resources and unnecessary suffering. The ability to make an informed health choice requires health literacy skills—the ability to obtain, process, and understand information needed to make informed decisions.[6,7] There is an opportunity to develop such skills among young people in school settings, particularly critical thinking skills—reasonable reflective thinking focused on deciding what to believe or do.[5,8–10]

There are several reasons for teaching critical thinking about health in secondary schools. First, young people are eager to learn and adapt easily.[11] Second, critical thinking is among the key competences that many countries, including Rwanda, have included in their primary and secondary school curricula.[5,12] Furthermore, young people are already exposed to health information but lack necessary skills to think critically about that information and make well-informed choices. Fourthly, health is important to everyone, and it is necessary to understand and apply key concepts to assess the reliability of claims about health. In addition, other none health interventions have largely same key concepts for assessing the reliability of claims.[13] Lastly, young people make up 16% of the world population, and 50% of Rwanda's population is less than 20 years old.[14] Investing in their health education, specifically improving their ability to think critically about health, may potentially improve health decision making in a large segment of the population.

A systematic review of the effectiveness of interventions to teach people key concepts required to assess claims about the effects of health interventions found that well designed educational interventions can improve people's ability to apply such concepts.[15] However, the included studies had important limitations, and the review found only three randomized trials that were conducted in schools. Another systematic review of the effects of school-based educational interventions that teach adolescents to critically appraise health claims found that school-based interventions may have an effect on knowledge and skills required for critical appraisal of health claims.[16] However, the certainty of the evidence for all comparisons and outcomes was very low. Most studies in the two reviews were conducted in high-income settings.

One of our studies included in the first review was a cluster-randomized trial of a primary school intervention in Uganda.[17] The intervention helped children to assess the reliability of claims about treatment effects. A follow-up study published after the review showed that children in the intervention arm of the Ugandan trial retained what they learned for at least 1 year.[18] Although the primary school resources were effective, it was hard to scale up use of the resources due to the cost of the intervention, which included a printed textbook that used a comic story.[17] In addition, the primary school lessons were an add-on to the curriculum, rather than being integrated into the curriculum.[19]

Prior to this trial, we conducted a context analysis in Rwanda to explore how we could overcome barriers to wide use of educational resources in secondary schools.[5] Most secondary schools in Rwanda now have "smart classrooms" with computers for students and an Internet connection. Making the resources digital rather than relying on printing could help ensure that they can be widely used at low cost. We therefore developed digital educational resources and planned a cluster-randomized trial to assess the effects of using the resources.

2 | METHODS

2.1 | Design

This was a two-arm cluster-randomized trial conducted in Rwanda. The study was approved by the Rwanda National Ethics Committee (Approval No. 1019/RNEC/2020 and subsequent amendments No. 41/RNEC/2022 and No. 236/RNEC/2022). The trial protocol can be found online.[20] We made no changes to the methods after commencement of the trial. We obtained permission to conduct the trial in schools from the Ministry of Education through Rwanda Basic Education Board. The trial was registered in the Pan African Clinical Trial Registry, trial identifier: PACTR202203880375077.

2.2 | Setting and participants

We conducted our study in lower secondary schools from 10 of the 30 districts in the country. In Rwanda, the basic education system is governed by districts with technical oversight from the Rwanda Basic Education Board (REB) and The National Examination and School Inspection Authority (NESA). Through REB and NESA, we obtained a list of schools with their characteristics and how they categorized them in terms of school performance. We included public, private, and government aided schools using the national curriculum, which had computers, Internet, over 100 students, and over 10 teachers. We excluded special needs schools, schools that were hard to reach for geographical reasons, and schools that participated in piloting the intervention.

We used multistage sampling to select schools. First, we randomly selected 10 districts, two from each of the five provinces in Rwanda. Then we randomly selected 84 schools from a list of eligible schools in those districts. The schools were stratified by their performance (low versus high performance as defined by NESA) and the sample was proportionate to the number of schools in each district. Participants were students and their science teachers from selected schools. The students were in year 2 of secondary education. We included year 2 students so that we could collect data for this trial and at 1 year follow-up. Students are usually placed in other schools after 3 years. We recruited schools through district authorities after presenting a letter of approval from the Ministry of Education, and school directors selected one senior-two class (normal starting age 14 years) and one teacher. We recruited all students in the class selected by the school director. Before randomization, we obtained consent from school directors and teachers who participated in the study and assent from students.

2.3 | Random allocation and masking

We used a computer-generated sequence to allocate schools in a 1:1 ratio to the intervention or control arm. We used block randomization to balance for school performance, with block sizes of six and four, and equal numbers in each arm. Allocation was conducted by a statistician who was not involved in the recruitment of schools or the analysis of data. We did not change the list after random allocation by the statistician. We did not mask the trial participants or investigators.

2.4 | Procedures

The schools allocated to the intervention arm received the IHC secondary school intervention in addition to the usual curriculum. Teachers in the intervention schools were given access to digital educational resources that included 10 lessons in two versions (blackboard and projector versions) and a teachers' guide.[21] We employed human-centered design with multiple iterations to design the intervention. The lessons focused on nine key concepts that were prioritized by curriculum developers, teachers, and members of the research team (Table 1).[22,23] A detailed description of the intervention is provided using the GREET 2015 checklist in Supplementary File S1.

Teachers in the intervention arm attended a 3-day teacher-training workshop before teaching the lessons. The teacher training was provided by teachers who participated in a pilot study of the IHC secondary school intervention. The teachers in the intervention arm delivered the lessons in a single school term. For each school, the administration planned the timetable based on the free time available. We intended each lesson to last for 40 min (one period). Teachers were free to extend the lesson time or modify the lesson plans.

Teachers in the control arm did not receive any educational resources or training. They were introduced to the trial and its objectives during recruitment meetings. Teachers in both the control and intervention arms of the trial continued with the standard competence-based curriculum. The curriculum includes nine subjects and key generic competences that are taught across subjects, including critical thinking.[24]

At the end of the term in which the intervention was delivered, students and teachers in the intervention and control arms completed the "Critical Thinking about Health" test, Supplementary File S2. We developed this test to measure the ability of students to understand and apply the key concepts covered in trial (Table 1). It includes two multiple-choice questions for each of the nine key concepts. Each question has a scenario including one of the nine concepts, a question about the scenario, and three response options. The questions were taken from the Claim Evaluation Tools item bank.[25]

The test also included questions about English reading proficiency, intended behaviors and self-efficacy, with Likert response options. Prior to the trial, we conducted cognitive interviews and pilot with secondary school students to ensure that the questions and that the format were clear and acceptable. Based on the findings, we modified the questions to clarify some of the terms and to improve formatting. We then conducted a Rasch analysis to assess the validity and reliability of the test.[26] We used a combination of the Nedelsky and Angoff methods to determine the cut off for passing and mastery scores.[27]

The test was administered by trained research assistants within 2 weeks after the intervention was delivered. The research assistants had a questionnaire and answer sheet for each student and teacher, and a unique code was assigned to each participant. The research assistant supervised the test and ensured that students answered the questions independently. After the test, the research assistant scanned the answer sheets.

2.5 | Outcomes

The primary outcome was the proportion of students with a passing score ($\geq$ 9 out of 18 questions answered correctly) on the Critical Thinking about Health Test. Secondary outcomes were the proportion of teachers with a passing score, the proportion of students and teachers with a mastery score ($\geq$14 out of 18), students' and teachers' mean scores (percent correct answers for the 18 multiple-choice questions), the proportion of students that answered both questions correctly for each of the nine concepts, intended behaviors, and self-efficacy.

We assessed the outcomes at the end of the term when the intervention was delivered. After 1 year, we will administer the test again to measure retention of what was learned. We also will compare how well students perform on their national examinations and assess use of what was learned by students in their daily lives and potential adverse effects.

TABLE 1 Learning goals and the prioritized key concepts for the 10 lessons covered in the trial.

	Title of the lesson	Lesson goals	Prioritized key concepts
	Thinking critically about claims		
1	Health actions	- Identify health actions - Explain why it is important to think critically about health actions	1. Health actions can have helpful effects, but they can also have harmful effects and be expensive. 2. The effects of most health actions are not obvious, especially changes that do not occur right after the health action. 3. Usually, personal experience (something that happened to someone after taking a health action) is a weak basis for claims about the effects of health actions. 4. Health actions that have not been evaluated in a reliable comparison but are commonly used or have been used for a long time are often assumed to work. However, they might not work and might be harmful or wasteful. 5. Health actions that have not been evaluated in a reliable comparison but are new, expensive, or technologically impressive are often assumed to work. However, they also might not work and might be harmful or wasteful. 6. Knowledge about the effects of health actions depends on comparisons.
2	Health claims	- Identify claims about the effects of health actions	
3	Unreliable claims	- Identify claims about the effects of health actions that are only based on personal experiences, how commonly used something is, or how new or expensive something is - Explain why most such claims are unreliable	
4	Reliable claims	- Explain why knowledge about the effects of health actions depends on comparisons - Explain why we need researchers to make the comparisons	
5	Using what we learned[1]	- Remember what they learned in Lessons 1 to 4 - Use what they learned in these lessons in their daily lives - Recognize limits to what they have learned	
	Thinking critically about comparisons		
6	Randomly created groups	- Explain why groups of people in a comparison should be similar at the start	7. In a comparison between health actions, important differences (other than the health actions) between comparison groups can be misleading. Randomly creating groups makes sure groups of people are as similar as possible at the start of a comparison and avoids unknown differences. 8. If a comparison between health actions is too small, we cannot be sure that the results reflect a true difference (or lack of difference) between the effects of the different health actions. The results could just be by chance.
7	Large-enough groups	- Explain what it means for comparisons between health actions to be large enough.	
	Making smart choices		
8	Personal choices	- Identify advantages and disadvantages of health actions, for individuals	9. People making a choice about whether to take a health action should consider the potential benefits and potential harms, costs, and other advantages and disadvantages. People making a community choice should also consider who will benefit, who will be harmed, who will achieve savings, and who will bear the costs.
9	Community choices	- Identify advantages and disadvantages of health actions, for communities	
10	Using what we learned[2]	- Remember what they learned in Lessons 1 to 9 - Use what they learned in these lessons in their daily lives - Recognize limits to what they have learned	

2.6 | Statistical analysis

We powered the trial for the primary outcome using the University of Aberdeen Health Services Research Unit's Cluster Sample Size Calculator.[28] We made the following assumptions: 39 students per cluster (one class in each school) based on education statistics,[29] an intraclass correlation at 0.19 and 30% of students achieving a passing score in the control arm based on a previous trial in primary schools,[17] a minimally important difference of 20% based on at least 50% of students in the intervention arm having a passing score, an alpha of 1%, power of 90%, and a maximum 10% loss to follow-up. Based on these assumptions, we calculated a sample size of 84 schools.

In the analysis, we estimated adjusted odds ratios and differences in means for binomial and continuous outcomes, respectively. We estimated adjusted odds ratios using mixed effects logistic regression. Adjusted differences in means were estimated using mixed effects linear regression. For outcomes measured at the level of student, we accounted for the cluster-randomized design using random intercepts at the level of school (the unit of randomization). Because there was a one-to-one relationship between teachers and schools, it was not necessary to account for clustering at the level of teachers. Except where noted below, all analyses were adjusted for the variable used in the stratified random allocation (low versus high school performance). To aid interpretation, we re-expressed odds ratios as adjusted differences, accounting for uncertainty of the odds in the control arm as well as the odds ratios. Missing test answers were counted as wrong answers. We followed the intention-to-treat principle throughout: all children and teachers who completed the test were included and analyzed in the arms to which they were allocated. We have reported 95% confidence intervals and two-sided p values, where appropriate, throughout. All statistical analyses were performed using Stata 16 (StataCorp LLC, College Station, Texas, USA).

Few data were missing so we did not perform the prespecified analyses to explore the risk of bias due to attrition. We estimated adjusted odds ratios comparing students' ability to correctly answer both multiple-choice questions for each of the nine concepts and present these results as a forest plot. For questions about intended behaviors and self-efficacy, we report numbers and percentages of students for each response option and estimates of adjusted odds ratios comparing dichotomized responses (e.g., very unlikely or unlikely, versus very likely or likely).

We performed two planned subgroup analyses as described in our trial protocol.[20] In the first, we estimated treatment effects for the primary outcome in schools with high and low performance as defined by NESA. In the second, we estimated treatment effects for the primary outcome in students whose English reading proficiency was assessed to be advanced, basic, or lacking. Students who correctly answered all four literacy questions in the Critical Thinking about Health Test were categorized as having advanced proficiency. Students who correctly answered both basic questions correctly and one or both of the advanced questions incorrectly were categorized as having basic proficiency. Students who did not correctly answer both basic questions were categorized as lacking basic reading proficiency. For each subgroup analysis, we estimated odds ratios for the interactions between treatment and the variable defining the subgroups and report these alongside p values testing hypotheses of no interaction.

Finally, we assessed whether the students who were randomized to the intervention liked the lessons, found them easy, and found them helpful. We report numbers and percentages of students for each response option as well as for dichotomized responses (e.g., liked the lessons a little or very much versus disliked the lessons a little or a lot).

3 | RESULTS

3.1 | Characteristics of trial participants

We recruited participants between February 25, 2022, and March 29, 2022. In total, we recruited 3,128 students in second year of lower secondary and 84 sciences teachers. We randomly assigned 42 schools (1,556 students and 42 teachers) to the control arm and 42 schools (1,572 students and 42 teachers) to the intervention arm. No schools or teachers were lost to follow-up. Thirty-eight students in the intervention arm and 33 in the control arm were absent on the day the test was administered. All participants who were recruited were analyzed for the primary and secondary outcomes. Figure 1 shows the flow of schools, teachers, and students through the study.

Most of the schools in both arms (26 (61.9%) in the control arm and 19 (45.2%) in the intervention arm) were government aided schools, that is, schools mostly owned by faith-based organizations or parents but receiving financial support from the government (Table 2). In both arms 24 schools (57.1%) were categorized as low performing and 18 (42.9%) were categorized as high performing. There were fewer teachers with a bachelor's degree in education in the control arm compared to the intervention arm (22 (52.4%) vs. 31 (73.8%)). The average number of years of teaching experience was similar in the control and intervention arms (9.5 vs. 9.3 years). The median number of students in each class was similar in the control and intervention arms (39 vs. 40). The proportions of female students (53.8% vs. 56.0% and the mean age (15.8 vs. 15.7) were similar in the control and intervention arms.

3.2 | Main findings of the trial

The proportion of students with a passing score in the intervention arm was 915/1572 (58.2%) compared to 302/1556 (19.4%) in the control arm (adjusted odds ratio 10.6 (95% CI: 6.3–17.8), $p < 0.0001$, adjusted difference 37.2% (95% CI: 29.5–45.0)) (Table 3). The proportion of students in the intervention arm with a mastery score was 370/1572 (23.5%) compared to 16/1556 (1.0%) in the control arm (adjusted odds ratio 102.5 (95% CI: 31.9–329.1), $p < 0.0001$, adjusted difference 22.3% (95% CI: 16.6—28.1)). The mean test score for students in the intervention arm was 55.4% (SD 23.1) compared to 33.8% (SD 15.9) in the control arm (adjusted mean difference 20.8% (95% CI: 16.6%—25.0%), $p < 0.0001$).

The proportion of teachers with a passing score in the intervention arm was 41/42 (97%) compared to 20/42 (47.6%) in the control arm (adjusted odds ratio 45.6 (95% CI: 5.7–363.9), $p < 0.0003$, adjusted difference 50.0% (95% CI: 34.2—65.8)). The proportion of teachers with a mastery score was 32/42 (76.2%) in the intervention arm compared to 2/42 (4.8%) in the control arm (odds ratio 64.4 (95% CI: 13.1–315.9), $p < 0.0001$, adjusted difference 71.4% (95% CI: 57.0—85.8)). The mean test score for teachers in the intervention arm was 83.9% (SD 15.2) compared to 47.0% (SD 16.3) in the control arm (adjusted mean difference 36.9% (95% CI: 30.3%—43.5%), $p < 0.0001$).

3.3 | Performance of students on each of the concepts covered in the trial

Students in the intervention arm performed better than those in the control arm on correctly answering both questions for each of the nine key concepts (Figure 2). The largest effect was for the concept "Do not assume that comparisons are not needed," for which 627/1572 (39.9%) students in the intervention arm answered both questions correctly compared to 70/1556 (4.5%) in the control arm (adjusted odds ratio 17.9 (95% CI: 10.9–29.4), $p < 0.0001$, adjusted difference 34.4% (95% CI: 28.3–40.5)). The smallest effect was for the concept "Do not assume that treatments are safe," for which 493/1572 (31.3%) students in the intervention arm answered both questions correctly compared to

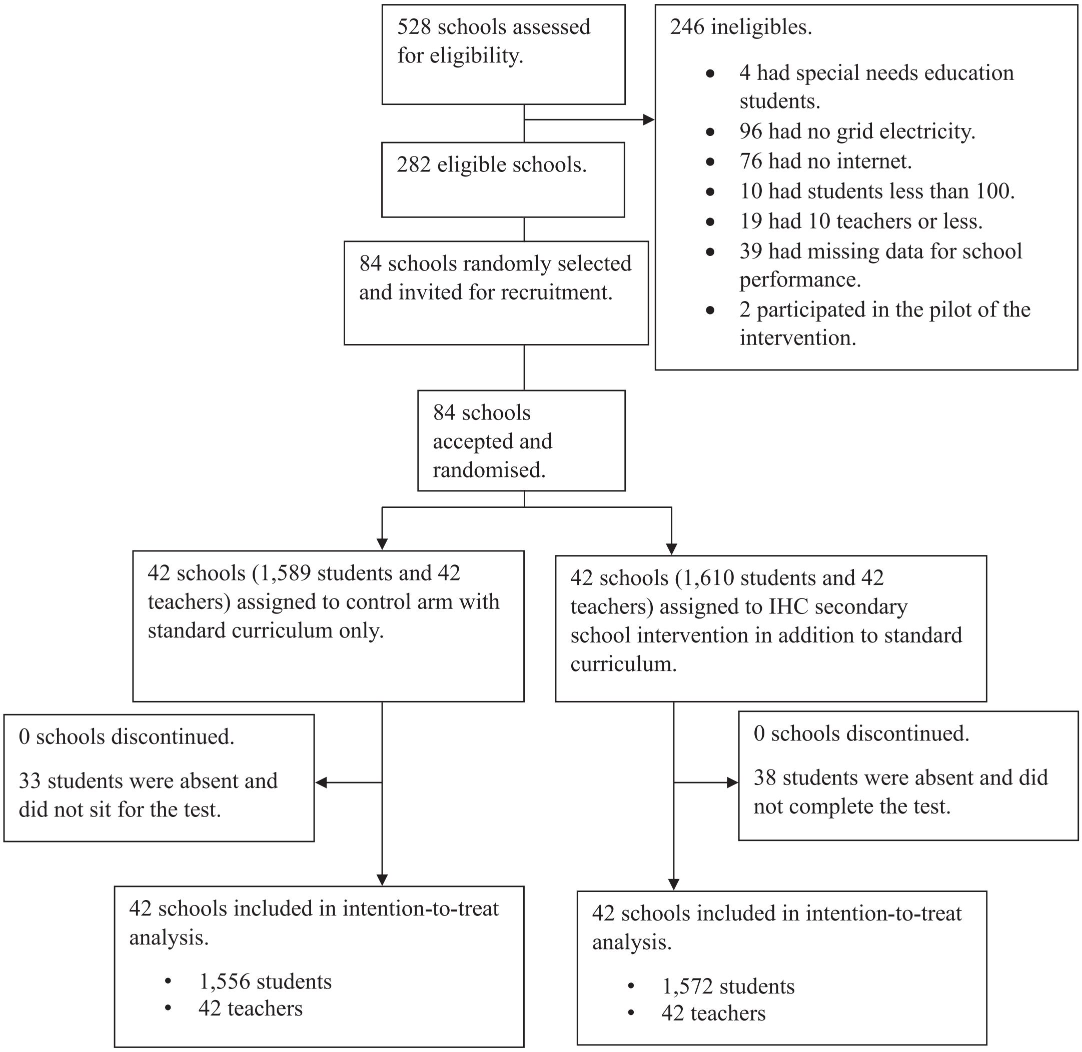

FIGURE 1 Flow diagram of study participants in the trial.

292/1556 (18.8%) in the control arm (adjusted odds ratio 2.2 (95% CI: 1.5–3.2), $p < 0.0001$, adjusted difference 11.8% (95% CI: 6.1–17.4)).

3.4 | Subgroup analysis on school performance and English proficiency of participants

The effect of the intervention was similar in high and low performing schools (adjusted odds ratio for an interaction between the intervention and school performance (low vs. high) 0.8 (95% CI: 0.3–2.3, $p = 0.72$) (Table 4). The effect also was similar for students with advanced and basic English reading proficiency (odds ratio for an interaction between the intervention and English reading proficiency (basic vs. advanced) 0.9 (95% CI: 0.5–1.4, $p = 0.57$). The intervention was effective for students lacking English reading proficiency (adjusted odds ratio 5.6 (95% CI: 3.2—9.9), $p < 0.0001$, adjusted difference 22.9% (15.4—30.4%)). However, the effect was less for students lacking English reading proficiency compared to students with advanced proficiency (adjusted odds ratio for an interaction between the intervention and English reading proficiency (lacking vs. advanced), 0.3 (95% CI: 0.2—0.6), $p < 0.0001$).

3.5 | Self-efficacy and intended behaviors of students participated in the trial

There was little difference in the proportions of students in the intervention arm compared to the control arm who found it easy or very easy to know if a claim about treatments is based on research studies comparing treatments (4.0% (95% CI: −2.3 to 10.2)), to find information about treatments that is based on research (0.5% (95% CI: −4.9 to 5.9)), to judge the trustworthiness of the results of a research study comparing treatments (4.0% (95% CI: −0.8 to 8.8)), or to judge the relevance of a research study comparing treatments (2.7% (95% CI: −1.8 to 7.2)) compared to students in the control arm (Table S5).

More students in the intervention schools compared to the control schools said they were likely or very likely to find out if a claim was based on a research study (adjusted odds ratio 1.4 (95% CI: 1.2–1.8),

TABLE 2 Characteristics of participants in the trial.

		Control schools	Intervention schools
School characteristics			
Schools	*N*	42	42
Province			
Eastern	*N* (%)	14 (33.3%)	12 (28.6%)
Kigali City	*N* (%)	5 (11.9%)	1 (2.4%)
Northern	*N* (%)	9 (21.4%)	10 (23.8%)
Southern	*N* (%)	9 (21.4%)	7 (16.7%)
Western	*N* (%)	5 (11.9%)	12 (28.6%)
School type			
Boarding	*N* (%)	15 (35.7%)	14 (33.3%)
Day schools	*N* (%)	27 (64.3%)	28 (66.7%)
School ownership			
Government aided	*N* (%)	19 (45.2%)	26 (61.9%)
Private	*N* (%)	8 (19.0%)	5 (11.9%)
Public	*N* (%)	15 (35.7%)	11 (26.2%)
School performance			
Low	*N* (%)	24 (57.1%)	24 (57.1%)
High	*N* (%)	18 (42.9%)	18 (42.9%)
Teacher characteristics			
Teachers	*N*	42	42
Completed test	*N* (%)	42 (100.0%)	42 (100.0%)
Education level			
Advanced diploma	*N* (%)	19 (45.2%)	11 (26.2%)
Bachelor's degree	*N* (%)	22 (52.4%)	31 (73.8%)
Masters	*N* (%)	1 (2.4%)	0 (0.0%)
Experience (years)	Mean (SD)	9.5 (6.0)	9.3 (6.4)
Students' characteristics			
Recruited in the study	*N*	1589	1610
Completed test	*N*	1556	1572
Completed test per class	Median (IQR)	39 (33 to 46)	40 (33 to 46)
Gender			
Female	*N* (%)	837 (53.8%)	881 (56.0%)
Male	*N* (%)	719 (46.2%)	691 (44.0%)
Age	Mean (SD)	15.8 (1.4)	15.7 (1.4)

adjusted difference 8.8% (95% CI: 3.5%–14.1%)). There was little difference in how likely they were to find out what a claim is based on (adjusted difference −1.5% (95% CI: −6.1 to 3.1)) or how likely they were to participate in a research study if asked (adjusted difference 3.3% (95% CI: −8.2 to 1.7)) (Table S6).

Most students in the intervention arm liked the lessons a little or very much (85.8%), found the lessons easy or very easy to understand (71.7%), and found what they learned helpful or very helpful (86.7%) (Table S7).

4 | DISCUSSION

The IHC secondary school intervention was effective in helping students to think critically about health claims and choices compared to the usual curriculum. More than half (58%) of the students in the intervention schools had a passing score on the Critical Thinking about Health Test compared to just under 20% of students in the control schools. About 23% of the students in intervention schools mastered the nine key concepts compared to 1% in the control schools. The intervention was effective in both low and high performing schools. The effect was less for students lacking English reading proficiency than for students with advanced proficiency. This may, in part, be because the test was written. The intervention itself required very little reading.

Teachers also benefitted from the intervention. All but one of the teachers in the intervention arm ($n = 42$) had a passing score compared to less than half (48%) of the control teachers ($n = 42$). About three quarters (76%) of them mastered the nine key concepts compared to 5% of the control teachers.

Randomized trials of the IHC secondary school intervention were conducted in Kenya and Uganda in parallel with this trial (unpublished work). The intervention had large effects in all three countries. The proportion of students with a passing score in Kenya was 61.7% (adjusted difference 27.3% (95% CI: 19.6–34.9), $p < 0.0001$). The proportion of students with a passing score in Uganda was 55.1% (adjusted difference 32.6% (95% CI: 26.0–39.2), $p < 0.0001$).

A previous randomized trial of the IHC primary school intervention in Uganda also found a large effect.[17] The proportion of students with a passing score, was 69% and the adjusted difference was 50% (95% CI: 44–55). In that trial, the intervention included a printed textbook that used a comic book story, a printed teachers' guide, and other printed materials. Twelve key concepts were taught, and the intervention included double periods (80 min) for each of the nine lessons. In contrast, our secondary school intervention utilized substantially less time (40 min for each of the ten lessons) and did not include printed materials for the students or the teachers, and only the teachers had access to the digital resources. In addition, the intervention was delivered in a time that was extra stressful for teachers and students. This was because the intervention took place in the last school term following prolonged school closures due to the COVID-19 pandemic.

Other studies have shown that educational interventions can improve people's ability to think critically about the effects of health interventions.[15,16] However, previously there were only two other small, randomized trials in schools and none using digital educational resources.

Critical thinking is among the key competences regarded as essential in the new Rwandan competence-based curriculum, which was implemented in 2016.[5] This trial shows that it is possible to teach such

TABLE 3 Main results of the primary and secondary outcomes of the trial.

	Control schools	Intervention schools	Adjusted difference	Adjusted odds ratio	*p* Value	ICC
Primary outcome[a]						
Students with a passing score (≥9/18)[b]	302 (19.4%)	915 (58.2%)	37.2% (29.5–45.0)	10.6 (6.3—17.8)	<0.0001	0.26
Secondary outcomes[a]						
Students with a mastery score (≥14/18)[b]	16 (1.0%)	370 (23.5%)	22.3% (16.6—28.1)	102.5 (31.9—329.1)	<0.0001	0.37
Mean score for students[c]	33.8% (15.9%)	55.4% (23.1%)	20.8% (16.6—25.0)		<0.0001	0.28
Teachers[d]						
Teachers with a passing score (≥9/18)[b]	20 (47.6%)	41 (97.6%)	50.0% (34.2—65.8)	45.6 (5.7—363.9)	0.0003	
Teachers with a mastery score (≥14/18)[b]	2 (4.8%)	32 (76.2%)	71.4% (57.0—85.8)	64.4 (13.1—315.9)	<0.0001	
Mean score for teachers[c]	47.0% (16.3%)	83.9% (15.2%)	36.9% (30.3—43.5)		<0.0001	

Data are % (SD), % (95% CI), or *n* (%).
Abbreviation: ICC = intraclass correlation coefficient.
[a]The cluster design was accounted for using random intercepts at the level of school.
[b]Logistic regression was used to estimate an adjusted odds ratio, which is re-expressed as an adjusted risk difference.
[c]Linear regression was used to estimate an adjusted difference in means.
[d]Teachers were treated as equivalent to the units of randomization (schools), so these models did not include random intercepts. The stratification variable was modeled as a fixed effect in all analyses. Wald-type confidence intervals and two-sided normal *p* values were computed in all analyses.

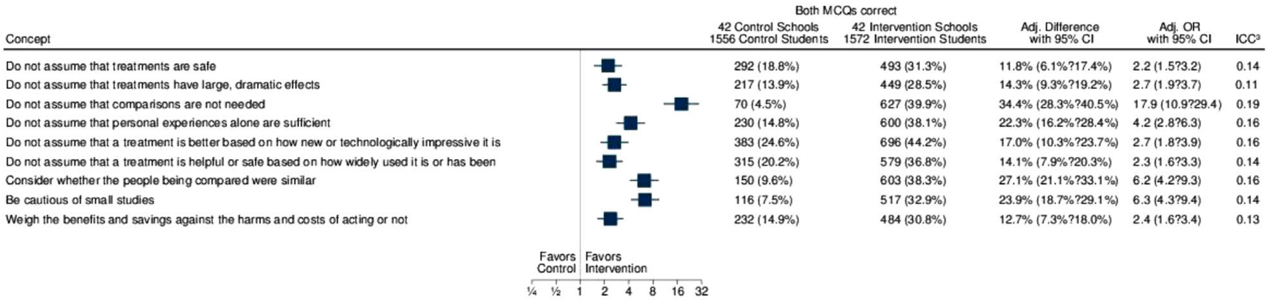

FIGURE 2 Results for each key concept covered in the trial. $p < 0.0001$ for all comparisons. *Number (%) of students answering both MCQs correctly. †Adjusted odds ratios are re-expressed as adjusted risk differences. ‡Intraclass correlation coefficient.

skills in a classroom setting with Internet access and a projector and few other resources. However, the teacher training we provided was probably essential. Findings of the pilot study we conducted prior to the trial, feedback from teachers, and findings of our context analysis all indicated that teachers lacked skills in teaching critical thinking generally and specifically critical thinking about health.[5]

The teacher training was consistent with usual practice in Rwanda, where teachers are trained prior to the introduction of new teaching methods or the implementation of new curricular changes. The workshop was taught by teachers using resources that we provided and could easily be scaled up. In addition to the workshop, the projector version of the lessons, which includes a presentation for each lesson, provided scaffolding for the teachers. An inclusion criterion for schools in this trial was that they had Internet access and projectors. Most public schools in Rwanda have computers, projectors, Internet access, and electricity. However, use of ICT by students is still limited, due in part to low computer-to-student ratios.[5]

School health education in Rwanda includes comprehensive sexuality education, prevention and control of sexually transmitted infections, neglected tropical diseases, hygiene and sanitation.[30] These programs differ from our intervention in two ways. First, they focus on teaching students what to do and not how to assess what to believe and do. Second, they are disease specific, whereas this intervention applies to any disease or health intervention. Teaching critical thinking about health could potentially improve the effectiveness of other school health programs and could potentially be integrated with those programs.

This study has several strengths. Importantly, it was a large, randomized trial carried out in a random sample of schools in Rwanda. In addition, there was very little loss to follow-up of study participants, most likely because the outcomes were assessed at the end of the school term when all students were ready to sit for exams. The Critical Thinking about Health Test was validated,[26] and neither the teachers nor students were exposed to the similar multiple-choice questions before it was administered.

However, the limitation is that responses to the questions about self-efficacy, intended behaviors, and students' perceptions of the lessons may have been biased to some extent by social desirability.

TABLE 4 Subgroup analyses on school performance and English proficiency.

	Control schools (*n* = 42)	Intervention schools (*n* = 42)	Adjusted difference	Adjusted odds ratio	*p* Value	ICC
Low and high performing schools						
Low performing schools	874 students	862 students				
	24 schools	24 schools				
Students with a passing score (≥9/18)	65 (7.4%)	362 (41.9%)	33.0% (23.8–42.3)	11.3 (5.8–21.9)	<0.0001	0.23
High Performing Schools	682 students 18 schools	710 students 18 schools				
Students with a passing score (≥9/18)	237 (34.8%)	553 (77.9%)	41.6% (28.1–55.2)	9.8 (4.3–22.0)	<0.0001	0.29
Interaction						
Intervention × High Performance				0.8 (0.3–2.3)	0.7242	
Students with advanced, basic, and lacking English reading proficiency						
Advanced Proficiency	416 students	481 students				
	37 schools	39 schools				
Students with a passing score (≥9/18)	143 (34.4%)	395 (82.1%)	45.8% (34.8–56.8)	15.2 (7.3–31.7)	<0.0001	0.30
Basic Proficiency	432 students	443 students				
	41 schools	41 schools				
Students with a passing score (≥9/18)	90 (20.8%)	306 (69.1%)	47.2% (38.0–56.3)	17.1 (8.8–33.2)	<0.0001	0.25
Lacking Proficiency	708 students	648 students				
	41 schools	42 schools				
Students with a passing score (≥9/18)	69 (9.7%)	214 (33.0%)	22.9% (15.4–30.4)	5.6 (3.2–9.9)	<0.0001	0.22
Interactions with Reading Proficiency						
Intervention × Basic Proficiency			0.9 (0.5–1.4)	0.5700		
Intervention × Lacking Proficiency			0.3 (0.2–0.6)	<0.0001		
Joint test of no interaction					<0.0001	

Data are *n* (%) and % (95% CI). Logistic regression was used to estimate adjusted odds ratios, which are re-expressed as adjusted risk differences. The cluster design was accounted for using random intercepts at the level of school. Wald-type confidence intervals and two-sided normal *p* values were computed in all analyses. Low school performance was used as the reference and Advanced English reading proficiency was used as the reference. ICC = intraclass correlation coefficient.

Other limitations of the study include uncertainty about retention of what was learned, the extent to which students use what they learned in their daily lives, and potential adverse effects. We will measure the extent to which students have retained what they learned after 1 year, using the same Critical Thinking about Health Test. We are exploring use of what was learned (transfer), other potential benefits, and potential adverse effects in a process evaluation and will explore these further in the 1-year follow-up study.

Inequities, both in the effects of the intervention and their sustainability over time, are an important concern for this intervention. Many of the students in the intervention schools did not achieve a passing score on the test. The subgroup analysis evaluating the impact of English reading proficiency on the effectiveness of the intervention suggests that students who otherwise do less well in school may also benefit less from the intervention.

The Critical Thinking about Health Test was a treatment-inherent outcome measure. That is, it measured content taught in the intervention schools and not in the control schools. Treatment-inherent outcome measures are associated with larger effect sizes than treatment-independent measures of content taught equally in intervention and control schools.[31] Thus, it is inappropriate to compare the effect of our intervention to treatment-independent outcome measures, such as reading or math tests.

Notwithstanding these limitations, the intervention has been more rigorously evaluated than most of what is taught in schools, and we have shown that it improves the ability of some students, as well as their teachers, to think critically about health claims and choices. Future research should focus on developing and evaluating ways of expanding the lessons across multiple school terms to reinforce the nine key concepts taught in the 10 lessons. They should also introduce additional concepts in a spiral curriculum that is integrated into secondary school curricula,[23] ensuring that students who did not achieve a passing score are provided additional support and implementing the intervention nationally.

In summary, this study shows that it is possible to teach critical thinking about health to secondary school students in a low-income setting without a costly intervention. This can potentially reduce the risk of being misled by claims about treatment effects, increase trust in evidence-based information, and help to improve the extent to which decisions about health interventions are well informed.

AUTHOR CONTRIBUTIONS

MM was the principal investigator for this trial. He conceptualized the study, planned data collection, managed trial process, and wrote the manuscript. LN and ADO were the supervisors and oversaw the trial implementation and were members of the trial steering committee together with NKS and MK. CMCS supported the coordination of field work activities. All authors contributed to the protocol development, review, and approval of this manuscript and had final responsibility for the decision to submit for publication. SER, ADO, JM, and MO led the development of the intervention. AD led the development and validation of the outcome measure. All authors except CJR and AD contributed to the development, review, and approval of the intervention. CJR provided statistical advice and conducted the statistical analyses. All authors had full access to all the data and final responsibility for the decision to submit for publication.

CONFLICT OF INTEREST STATEMENT

MM, LN, CMCS, FC, RS, MO, AN, DS, JM, MK, SL, NKS, SER, and ADO both developed and evaluated the intervention.

DATA AVAILABILITY STATEMENT

All de-identifiable individual-participant data and the data dictionary will be made available on Zenodo. The study protocol with the detailed analysis plan can be found online at https://doi.org/10.5281/zenodo.6562788.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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