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One-year follow-up effects of the informed health choices secondary school intervention on students' ability to think critically about health in Uganda: a cluster randomized trial

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Abstract

Introduction We assessed the effects of the Informed Health Choices (IHC) secondary school intervention on students' ability to think critically about choices 1 year after the intervention.

Methods We randomized eighty secondary schools to the intervention or control (usual curriculum). The schools were randomly selected from the central region of Uganda and included rural and urban, government, and privately-owned schools. One randomly selected class of year-2 students (ages 14–17) from each school participated in the trial. The intervention included a 2-day teacher training workshop, 10 lessons accessed online by teachers and delivered in classrooms during one school term (May–August 2022). The lessons addressed nine prioritized IHC concepts. We used two multiple-choice questions for each concept to evaluate the students' ability to think critically about choices at both the end of the school term and again after 1 year. The primary outcome was the proportion of students with a passing score (≥ 9 of 18 questions answered correctly) on the "Critical Thinking about Health" (CTH) test.

Results After 1-year, 71% (1749/2477) of the students in the intervention schools and 71% (1684/2376) of the students in the control schools completed the CTH test. In the intervention schools, 53% (934/1749) of students who completed the test had a passing score compared to 33% (557/1684) of students in the control schools (adjusted difference 22%, 95% CI 16–28).

Conclusions The effect of the IHC secondary school intervention on students' ability to assess health-related claims was largely sustained for at least 1 year.

Trial registration Pan African Clinical Trial Registry PACTR202204861458660. Registered on 4 April 2022.

Keywords Critical thinking, Randomized trial, Critical health literacy, Secondary school, Teaching and learning resources

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Introduction

Claims about what might improve or harm our health are everywhere. For example, "drinking warm water with lemon fruit reduces body fat.", from a blog [1]. These claims can be made by friends, family, politicians, charlatans, and health professionals, not to mention the increased access to health information through the Internet and social media [2–4]. The Internet creates opportunities for people to use information when making choices but also increases the risk of being misled by unreliable information.

People often do not know how to tell the difference between reliable and unreliable claims about the effects of interventions. Making health choices based on unreliable claims can have serious consequences such as overuse of ineffective or harmful interventions (e.g., unnecessary antibiotic use) and underuse of effective interventions (e.g., vaccination hesitancy) [5, 6].

Potential solutions to this problem include improving access to reliable information, myth-busting or fact-checking, and educational interventions [7]. Educational interventions, like the one evaluated in this article, present an opportunity to "inoculate" people against misinformation by teaching them how to carefully assess health claims and how to make informed decisions about health. This has the potential to provide lasting protection [8, 9].

Careful assessment of health claims depends on understanding and using principles (concepts) to guide the assessment of claims about health actions and health choices [8, 9].

Teaching critical thinking is widely advocated [10] and is a key feature in competence-based curricula. However, Uganda is making slow progress towards implementing the recently rolled-out lower-secondary school competence-based curriculum. Key hindrances to implementing the new curriculum include limited learning resources, inadequate teacher training and preparation, time constraints, and lack of assessment tools [11]. Consequently, teachers continue to focus on rote learning and "teaching to the exam" rather than teaching critical thinking [12].

Randomized trials in Kenya, Rwanda, and Uganda have shown that the Informed Health Choices (IHC) secondary school intervention improved students' ability to apply nine key concepts for thinking critically about health choices, immediately after the intervention [13–16]. In this article, we report the effects of the IHC secondary school intervention in Uganda, 1 year later. One-year follow-up results of the parallel trials conducted in Kenya and Rwanda are also being reported.

Methods

Complete details about the methods can be found in the trial protocol [17] and the report of initial results [14]. The only deviation that was made from the protocol and the report of the initial results was the sensitivity analyses to explore the risk of bias due to attrition, as described below.

Study design and participants inclusion

We conducted a cluster-randomized trial in six administrative districts randomly selected from the 24 districts in the central region of Uganda. Education officers in each of the six selected districts provided a list of all secondary schools in their district. We drew a random sample of 20 schools from each of the six districts, stratified by school ICT access (blackboard only versus blackboard and a projector) and school ownership (private versus public). Within each participating school, we randomly selected one class of year-two students. The head teachers at the participating schools identified a year-two biology teacher for those classes. We obtained consent from the headteachers on behalf of their schools and from the teachers. We did not obtain assent from students or consent from their parents.

Random allocation and masking

An independent statistician used Sealed Envelope [18] to generate a block stratified random sequence and randomly allocated schools 1:1 to the intervention or control group. The stratification ensured a fair distribution of schools based on school ownership (public versus private) and ICT resources (blackboard only versus blackboard and a projector).

It was not possible to blind students, teachers, or the research team. We used printed copies of the Critical Thinking about Heath (CTH) test to measure outcomes. We informed the teachers and students in both arms of the trial of the purpose of the CTH test. We did not show them the test until the end of the school term when they were asked to complete it to prevent teaching to the test. After the initial assessment, we retrieved and kept all copies of the test, including blank tests, to prevent participants from practicing for the 1-year follow-up.

We used unique identifiers for all the students and teachers. The registration form included the name of the student so that the test scores from the 1-year follow-up could be shared with the teachers and students. The unique IDs were used to de-identify the data.

Table 1 Prioritized IHC key concepts in the secondary school resources

1. Treatments can cause harms as well as benefits.
2. Large, dramatic effects are rare.
3. Personal experiences or anecdotes alone are an unreliable basis for most claims.
4. Treatments that are new or technologically impressive may not be better than available alternatives.
5. Widely used treatments or those that have been used for decades are not necessarily beneficial or safe.
6. Identifying the effects of treatments depends on making comparisons.
7. Small studies may be misleading.
8. Comparison groups should be as similar as possible.
9. Weigh the benefits and savings against the harms and costs of acting or not.

Intervention

The involved providing teachers with digital resources and a 2-day training workshop. It focused on nine prioritized key concepts (Table 1) [19].

We developed educational resources, which are freely available online [20], using human-centered design [21]. The resources include dual versions of 10 lesson plans, one version for classrooms without a projector and one for classrooms with a projector. The resources also included background information, a teacher's guide, an overview of each lesson, and extra resources for the teachers, including materials for the 2-day training workshop. The intervention is described using the guideline for reporting evidence-based practice educational interventions and teaching (GREET) checklist in Additional file 1.

The intervention is described using the guideline for reporting evidence-based practice educational interventions and teaching (GREET) checklist in the Supplementary material.

We retained the schools using two WhatsApp groups: one for teachers in the intervention group and the other for teachers in the control group. No study materials were posted on the WhatsApp groups and we repeatedly reminded teachers that all study materials would be shared after the 1-year follow-up. In both groups, the interactions focused on general school activities and new curriculum implementation issues like teaching strategies used in delivering biology lessons and challenges faced in implementing the new lower-secondary school curriculum. We also made individual phone calls to headteachers and teachers to extend national holiday greetings to keep them engaged and interested in the research participation.

The lessons were designed to each be taught in a single 40-min school period. However, the lessons took an average of 69 min to complete [22]. All 10 lessons were taught in all but one of the intervention schools. Only five of the 10 lessons were taught in one school because of a change in the school program that did not allow the teacher to complete the lessons. The lessons were otherwise delivered as intended. Attendance by students varied.

For the 1-year follow-up, students and teachers completed the CTH test in their classrooms, like they did immediately after the intervention [14]. Research assistants administered and invigilated the test, and collected the tests immediately after they were completed. The participating teachers completed the test on the same date as their students. After teachers submitted their own answer sheets, the teachers and research assistants reviewed the answer sheets to ensure that students had marked their answers appropriately and answered all the questions. We told both students and teachers that the resources which we called "Be smart about your health" together with the CTH test would be shared upon study completion and all introduction sessions would be provided to the teachers in the control schools [21]. We used Zip grade scanner software to scan and score the answer sheets [23]. We gave the teachers the results for their class within 3 weeks of administering the test.

Outcomes

The CTH test (Supplementary material) was used to measure the student's ability to apply the nine key concepts addressed by the intervention. The test included two multiple-choice questions (MCQs) for each of the concepts with three response options for each question. The test was developed using 27 multiple-choice questions (MCQs) from the Claim Evaluation Tools item bank [24]. There are three MCQs for each of the nine key concepts. We conducted cognitive interviews and piloted the questions with students in Uganda, Kenya, and Rwanda [25]. Findings coming out of the interviews and pilot testing led to only minor changes, such as changing some of the names and other terminology used in the MCQs to improve familiarity. We used Rasch analysis to ensure construct validity [25]. The MCQs were well-targeted (mean person location − 0.218) and reliable (Conbach's alpha 0.72). Based on the results, two MCQs were selected for each key concept. All the 18 included MCQs had a good fit to the item characteristics curve.

The primary outcome was the proportion of students with a passing score (≥9 out of 18 questions answered correctly). The criterion for passing was determined by an independent group of judges [26]. Secondary outcomes included the proportion of teachers with a passing score, the proportions of students and teachers with mastery score (≥14 out of 18), mean scores for students and teachers, the proportions of students that answered both questions correctly for each of the nine concepts, and measures of students' intended behaviors and self-efficacy.

Additional outcomes measured in the 1-year follow-up included: self-reported behaviors potential adverse effects and the exploration of how students used what they learned in their daily lives. Those results are reported in a meta-analysis of the results from the three trials.

Statistical analysis

We powered this trial for the primary outcome using the University of Aberdeen Health Services Research Unit's Cluster Sample Size Calculator [27], as described in our previous report [14]. We estimated odds ratios and differences in means for binomial and continuous outcomes, respectively. We estimated odds ratios using mixed effects logistic regression. Differences in means were estimated using mixed effects linear regression. For outcomes measured at the level of students, we accounted for the cluster-randomized design using random intercepts at the level of school (the unit of randomization). Except where noted, all analyses were adjusted for the variables used in the stratified randomization (public versus private schools, and schools that had a projector versus those that did not). To aid interpretation, we re-expressed odds ratios as adjusted differences, accounting for the uncertainty of the control odds as well as the odds ratios. Missing test answers were counted as wrong answers. We followed the intention-to-treat principle throughout: all students and teachers who completed the test were included and analyzed in the group to which they were randomized. We report 95% confidence intervals, two-sided p-values, and model-based intraclass correlation coefficients (ICCs), where appropriate, throughout. Upper confidence interval limits on exact estimates of odds ratios greater than 1000 are reported as infinitely large. All statistical analyses were performed using Stata 16 (Stata Corp LLC, College Station, TX, USA).

We conducted sensitivity analyses to explore the risk of bias due to attrition: inverse probability weighting (IPW), Manski-type and Lee bounds (Supplementary file) for dichotomous and continuous outcomes, respectively, which provide sharp bounds on treatment effect under conditions that maximally favor or disfavor the intervention [28, 29]. These were prespecified in a protocol amendment [30]. In the IPW analysis, outcomes for students were weighted using inverse probabilities of non-attrition [31].

We estimated 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 odds ratios comparing dichotomized responses (e.g., very unlikely or unlikely, versus very likely or likely).

Role of the funding source

The funder had no role in study design, data collection, data analysis, data interpretation, or writing of the report. RS as the principal investigator had full access to all the data in the study and had final responsibility for the decision to submit for publication.

Results

To include the 80 schools, we assessed a total of 1049 lower secondary schools for eligibility between February 24 and April 22, 2022. We found 1007 schools to be eligible, and we randomly sampled 20 schools from each district stratified by ICT resources and school ownership. The schools were approached to obtain consent from their head teachers. Eighty-two schools consented to participate in the trial. On reaching out to the proposed teachers for consent, the researchers found that two schools had identified the same biology teacher for participation. We excluded both of those schools. The remaining 80 schools were randomly assigned to either the intervention ($n=40$) or control group ($n=40$). All 80 schools enrolled in the trial were retained for the 1-year follow-up assessment. The flow of the schools, teachers, and students through recruitment to the 1-year follow-up is shown in Fig. 1.

Overall, 71% (3433/4853) of the students who completed the CTH after the intervention completed the test again after 1 year. This proportion was the same (71%) in the 40 intervention schools ($n=1749/2477$ students) and the 40 control schools ($n=1684/2376$ students) (Table 2). The proportion of students lost to follow-up was 29% in each arm. However, loss to follow-up was not equally likely between trial arms with respect to the school funding model or student age.

Loss to follow-up was more likely for students who did better on the original test. Outcome data were missing for 5 (12.5%) of the control teachers. The

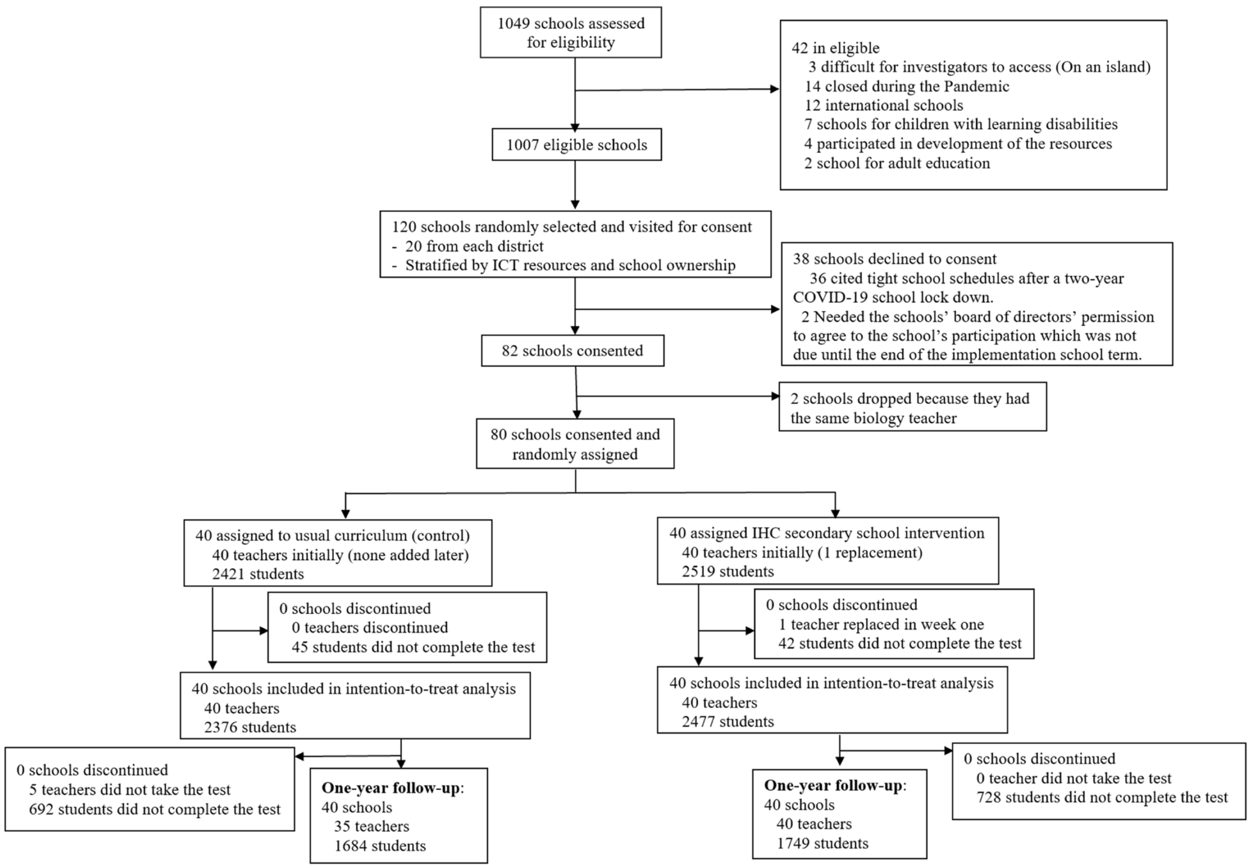

Fig. 1 Informed Health Choices trial profile with the 1-year follow-up

teachers lost to follow-up scored higher on the original test compared to control teachers not lost to follow-up (mean difference 16.0%, 95% CI 5.2% to 26.5%, $p = 0.0078$, non-prespecified).

In the intervention schools, 53.4% ($n = 934/1749$) of students that completed the test had a passing score compared with 33% ($n = 557/1684$) of students in the control schools (adjusted odds ratio 2.6, 95% CI 2.0 to 3.5, $p < 0.0001$). This corresponds to 22% (95% CI 16–28%) more students achieving passing scores in the intervention schools (Table 3). The size of this effect was less than the effect at the end of the school term when the intervention was delivered (adjusted difference 32.6%, 95% CI 26.0–39.2). At the end of the term, 55.1% of the students in the intervention schools had a passing score and 24.7% of the students in the control schools had a passing score. The results of the IPW sensitivity analysis did not differ substantially from the analysis without IPW (OR 2.6, 95% CI 2.0–3.5; adjusted difference 21.7%, 95% CI 15.5–27.9) (Supplementary material). After 1 year, students in the intervention schools were more likely than students in control schools to answer both questions correctly for six of the nine key concepts (adjusted differences from 6.3% (95% CI 1.9–10.7) to 19.9% (95% CI 13.4–26.5)) (Fig. 2).

In the intervention schools, 15.8% ($n = 277/1749$) of the students had a mastery score compared to 2.8% (47/1684) in the control schools (adjusted odds ratio 6.7, 95% CI 3.9 to 11.3, $p < 0.0001$). This corresponds to 12.2% (95% CI 8.3–16.1) more students achieving a mastery score in the intervention schools (Table 3). This difference was also smaller than what was observed at the end of the term when the intervention was delivered (16.4%, 95% CI 11.4–21.3) and the proportion of students in the intervention schools with a mastery score decreased from 18.0 to 15.8%. The results of the IPW sensitivity analysis again did not differ substantially from the analysis without IPW (OR 6.6, 95% CI 3.8–11.5; adjusted difference 11.7%, 95% CI 7.4–16.0).

The mean score was 51.4% in intervention schools compared to 40.7% in control schools (adjusted mean difference 11.2%, 95% CI 8.1–14.2, $p < 0.0001$). The mean difference was 17.0% (95% CI 13.5 − 20.5) at the end of the term when the intervention was delivered.

Students in the intervention schools were more likely to it is easy or very easy to know whether a claim is based

Table 2 Characteristics of the participants

			Control schools	Intervention schools
Schools				
Selected		*N*	40	40
	Kampala	*N* (%)	5 (12.5%)	5 (12.5%)
	Kayunga	*N* (%)	7 (17.5%)	8 (20.0%)
	Luwero	*N* (%)	8 (20.0%)	9 (22.5%)
	Mpigi	*N* (%)	9 (22.5%)	7 (17.5%)
	Mukono	*N* (%)	6 (15.0%)	4 (10.0%)
	Wakiso	*N* (%)	5 (12.5%)	7 (17.5%)
Ownership	Public	*N* (%)	19 (47.5%)	19 (47.5%)
	Private	*N* (%)	21 (52.5%)	21 (52.5%)
Performance on the 2021 National exam	Low	*N* (%)	16 (40.0%)	19 (47.5%)
	Moderate	*N* (%)	8 (20.0%)	5 (12.5%)
	High	*N* (%)	16 (40.0%)	16 (40.0%)
Teachers				
Teachers (identified by head teachers)		*N*	40	40
Completed tests		*N* (%)	35 (87.5%)	40 (100.0%)
Education[a]	Certificate	*N* (%)	0 (0.0%)	1 (2.5%)
	Diploma	*N* (%)	9 (25.7%)	13 (32.5%)
	Degree	*N* (%)	26 (74.3%)	26 (65.0%)
Years of experience[a]		Median (IQR)	10 (4 to 19)	8 (4.5 to 16.5)
Sex[a]	Female	*N* (%)	6 (17.1%)	12 (30.0%)
	Male	*N* (%)	29 (82.9%)	28 (70.0%)
Students				
Enrolled at the start of the term		*N*	2376	2477
Completed tests		*N* (%)	1684 (70.9%)	1749 (70.6%)
Completed tests per class		Median (IQR)	49 (41 to 59)	51 (38 to 67)
Class size		Median (IQR)	66 (50 to 87)	69 (52 to 87)
Sex[a]	Female	*N* (%)	973 (57.8%)	933 (53.3%)
	Male	*N* (%)	711 (42.2%)	816 (46.7%)
Age[a]		Mean (SD)	16.0 (1.1)	15.8 (1.0)

[a] Data are for participants who took the test

on a research study comparing treatments compared with students in the control schools (adjusted difference 13.7%, 95% CI 9.7–17.6) (Supplementary material). Compared to students in the control schools, students in the intervention schools were 5.8% (95% CI 1.9–9.6) more likely to consider it easy or very easy to assess the trustworthiness of the results of a research study comparing treatments. There was little difference between students in the intervention and control schools in how easy they said it was for them to find information about treatments that are based on research studies comparing treatments.

Students in the intervention schools were more likely to say they would find out what a claim was based on (adjusted difference 8.2%, 95% CI 4.9–11.6) and to find out if the claim was based on a research study comparing the treatment to no treatment (adjusted difference 10.9%, 95% CI 7.3–14.6).

In the intervention schools, 95% ($n = 38/40$) of teachers who completed the test had a passing score compared with 63% ($n = 22/40$) of teachers in the control schools (adjusted odds ratio 12.4, 95% CI 2.5 to 61.6, $p < 0.0021$) (Table 3). The adjusted difference was 33.0% (95% CI 15.7–50.4, $p = 0.0021$). Teachers in the intervention schools were also more likely to have a mastery score (72.5% vs 14.3%; adjusted difference 58.3%, 95% CI 40.2–76.3, $p < 0.0001$). The adjusted difference for passing scores was about the same as what was found at the end of the term when the intervention was delivered (32.1%, 95% CI 15.7–48.6). The adjusted difference for mastery scores was less than what was found at the end of the term (82.6% 95% CI 64.3 – 97.4). The mean difference (27.8%, 95% CI 20.2–35.4) also was less than at the end of the term (35%, 95% CI 27.4–42.6). The Lee bounds for the mean difference were 25.1% to 32.4% (95% CI 16.9–40.1).

Table 3 Main results—student results

	Control Schools	Intervention Schools	Adjusted difference	Odds ratio	*p*	ICC
	40 schools 1684 students 35 teachers	40 schools 1749 students 40 teachers				
Primary outcome[a]						
Students with a passing score (≥ 9/18)[b]	557 (33.1%)	934 (53.4%)	21.9% (16.0% to 27.8%)	2.6 (2.0 to 3.4)	< 0.0001	0.07
Secondary outcomes[a]						
Students with a mastery score (≥ 14/18)[b]	47 (2.8%)	277 (15.8%)	12.2% (8.3% to 16.1%)	6.7 (3.9 to 11.3)	< 0.0001	0.17
Mean score for students[c]	40.7% (17.0%)	51.4% (21.5%)	11.2% (8.1% to 14.2%)		< 0.0001	0.10
Teachers[d]						
Teachers with a passing score (≥ 9/18)[b]	22 (62.9%)	38 (95.0%)	33.0% (15.7% to 50.4%)	12.4 (2.5 to 61.6)	0.0021	
Teachers with a mastery score (≥ 14/18)[b]	5 (14.3%)	29 (72.5%)	58.3% (40.2% to 76.3%)	16.7 (5.0 to 55.9)	< 0.0001	
Mean score for teachers[c]	54.3% (17.9%)	81.9% (16.2%)	27.8% (20.2% to 35.4%)		< 0.0001	

Data are % (SD), % (95% CI), or *n* (%). *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 randomized units (schools), so these models did not include random intercepts. The stratification variables were modeled as fixed effects in all analyses. Wald-type confidence intervals and two-sided normal *p*-values were computed for all analyses

The difference between intervention and control schools in loss to follow-up was less than 0.3% and it is not possible to adjust for the stratification variables used in the randomization when calculating Lee bounds.

Discussion

Effects of the IHC secondary school intervention on students' ability to think critically about health choices were largely sustained for at least 1 year, in Uganda. This is similar to what we found in an earlier trial of the IHC primary school intervention, in Uganda, and in the parallel trials of the IHC secondary school intervention, in Kenya and Rwanda [13–15, 32]. Decay in what was learned in school is common [33]. Overall, across the three trials, there was some delay in what was learned [16]. A possible explanation for decay in what was learned from the IHC secondary school intervention is the limited time allocated to the lessons because the lessons were an add-on to the curriculum and content was not examined as national assessments [22].

The proportion of students that achieved a passing score was just over half in the intervention group both after 1 year (53.4%) like at the end of the term when the intervention was delivered (55.1%). In addition to having to fit the IHC secondary school intervention into an already packed school schedule, the intervention followed 2 years of school closure due to the COVID-19 pandemic. Nonetheless, it is likely that the large proportion of students who did not achieve a passing score reflects the limitation of a one-off intervention, at least to some extent. Ideally, the IHC key concepts should be taught in a spiral curriculum with iterative revision of the

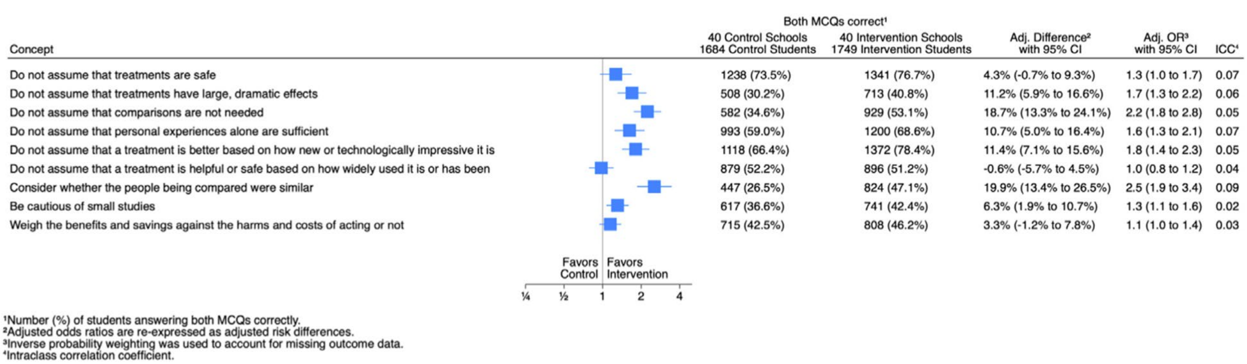

Fig. 2 Passing results for each Informed Health Concept 1 year after the intervention

key concepts over several school years, reinforcing and deepening learning, while introducing additional concepts [19].

The educational resources can be accessed with a smartphone and are automatically downloaded. Thus, the intervention is not dependent on schools having computers or other ICT infrastructure. We may also attribute the effectiveness of the IHC secondary school intervention after 1 year to the methods used and the stakeholders involved in designing the intervention. We used an iterative human-centered design process to develop the *Be smart about your health* resources, together with curriculum developers, educational policy makers, teachers, and students to ensure that the educational resources are well suited for secondary schools in East Africa. The resources are accessible online and responsive (suitable for different screen sizes, including smartphones).

The positive intervention effects were also sustained among teachers. This too is consistent with findings of the randomized trial of the IHC primary school intervention in Uganda [32] and the trials in Rwanda and Kenya [13, 15]. Our findings are consistent with other studies, which indicate that many adults do not understand and apply key concepts for assessing treatment claims and making informed health choices [34, 35].

A systematic review found that educational interventions that address one or more of the IHC key concepts can improve people's understanding of evidence in evaluating claims about health interventions, but the evidence is limited [36]. The effect of the IHC secondary school intervention in Uganda (an adjusted difference of 32.6%, 95% CI 26.0–39.0 for the primary outcome) is similar to what was found in the randomized trials of this intervention in Kenya (adjusted difference of 27.3%, 95% CI 20.0–35.0) and Rwanda (adjusted difference 37.2%, 95% CI 29.0–45.0). It is also similar to that of the IHC primary school intervention in Uganda [32]. In that trial, there was no decay in learning among children in the intervention schools. Possible reasons for that include that the intervention utilized printed resources, including a textbook for the children and a teachers' guide, and more time was allocated to the lessons (12 h altogether vs 7 h for the secondary school intervention). Also, that intervention did not follow the pandemic.

Strengths of this study include the recruitment of a representative sample of schools, which may ensure the applicability of the findings and random allocation. Two limitations of this 1-year follow-up study are that both the intervention and the outcome measure were developed by our group, and outcome data were missing entirely for 29% of the randomized students (loss to follow-up). While the proportion of students lost to follow-up was almost identical in each arm, loss to follow-up was not equally likely between trial arms with respect to the school funding model or student age. Also, loss to follow-up was also more likely for students who scored higher on the original test. This may have reduced the apparent effect of the intervention after 1 year in schools allocated to the intervention group, where students who did better on the original test were lost to follow-up. However, the sensitivity analyses that we conducted suggest that the effect estimates are robust.

Although it would be desirable to measure retention beyond 1 year, there are substantial barriers to following up with students. These include challenges to obtaining long-term research funding as well as increasing loss to follow-up, particularly in low-income settings. Loss to follow-up after 1 year in this study was similar to what was found in the trial of the IHC primary school intervention in Uganda and in parallel trials of the IHC secondary school intervention in Kenya and Rwanda [13–15, 32].

We were unable to find another outcome measure suitable for our study [37]. For the 1-year follow-up, we used the same MCQs used in the CTH test at the end of the school term (Supplementary material) [14]. These questions were adapted from a database of questions that independent research methodologists judged to have face validity [38]. We conducted cognitive interviews to ensure the questions were understandable and acceptable, and we used Rasch analysis, which showed that the questions fit the Rasch model and were reliable [38]. The scoring was done by trained research assistants using Zip grade [23], and the cut-offs for passing and mastery scores were determined by an independent group of curriculum specialists, teachers, and researchers [26]. Apart from taking the CTH test immediately after implementation of the intervention, the teachers and students were not shown the test or similar multiple-choice questions before taking the test again after 1 year.

The outcome measure also was treatment inherent, i.e., it measured content taught in the intervention group and not in the control group. In education research, treatment-inherent measures are associated with larger effect sizes than treatment-independent measures, which measure content taught in both comparison groups [39].

Another potential limitation of the intervention is that it did not include digital or printed resources that students could access directly. This minimized the cost of the intervention but may limit its effectiveness as it may reduce student interaction with the IHC educational resources.

We report more on retention of what was learned across the three trials, as well as the use of what was learned in daily life, and potential adverse effects in the meta-analysis of the three trials [16], process evaluations linked to the three trials [40–42], and a qualitative evidence synthesis [43].

Health is important to everyone. Using health to teach critical thinking skills may improve critical thinking about many other types of interventions [44]. The IHC secondary school intervention equips learners with critical thinking skills they can use now and in the future as patients, citizens, health professionals, and policy makers. We have shown that well-designed education interventions such as the IHC secondary school intervention can effectively teach students to think critically about health claims and choices.

Implications

This study shows that teaching adolescents in Ugandan secondary schools to think critically about claims about the effects of health interventions is feasible. The skills that the IHC secondary school intervention fosters are applicable and usable in students' daily lives. However, a spiral curriculum that provides opportunities to re-visit the key concepts, while introducing new concepts, may improve long-term effects compared to a one-off add-on to the curriculum during a single school term [22].

Supplementary Information

The online version contains supplementary material available at https://doi.org/10.1186/s13063-024-08607-7.

Additional file 1. GREET checklist. Supplementary table 1: Sensitivity analyses. Supplementary table 2: Self-efficacy. Supplementary table 3: Intended behaviours. Supplementary figure 1: Distribution of studnets's scores by trial arm. The Critical Thinking about Health Test.

Additional file 1.

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Authors' contributions

RS was the principal investigator. He drafted the protocol with help from the other investigators and was responsible for the day-to-day management of the trial. NKS and ADO had primary responsibility for overseeing the trial. All the investigators reviewed the manuscript, provided input, and agreed on this version. SR, JM, AO, and MO had primary responsibility for developing the secondary school resources. All the investigators contributed to the development of the resources and to the protocol. Astrid Austvoll-Dahlgren led the development and validation of the outcome measure. RS had primary responsibility for data collection. CJR did the statistical analyses. This study was funded by the Research Council of Norway through the Centre for Epidemic Interventions Research (CEIR), at the Norwegian Institute of Public Health. The steering group for the trial included RS, NKS, LN, MK, SR, ADO, MM, and FC and was responsible for final decisions about the protocol and reporting of the results.

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Data availability

Deidentified data from this trial and the data dictionary will be available on Zenodo with publication of this report, together with the protocol, informed consent forms, Critical Thinking About Health Test (Additional file 1).

Declarations

Ethics approval and consent to participate

Ethical approval for the trial was obtained from the relevant authorities in Uganda, from the School of Medicine Research and Ethics Committee at the Makerere University College of Health Sciences, and the Uganda National Council for Science and Technology (HS91ES).

Competing interests

We both developed and evaluated the intervention.

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