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REVIEW ARTICLE

Effectiveness of Culturally Based HIV Education on Adolescents' Knowledge, Attitudes, and Intentions: A Systematic Review

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ABSTRACT

Introduction: Adolescents aged 10–19 years remain a priority population in the global HIV response. UNICEF reports an almost 50% reduction in new HIV infections among adolescents aged 15–19 years since 2010, while UNAIDS estimates a 62% decline among children aged 0–14 years between 2010 and 2024.Generic HIV education often fails to shift the social norms and cultural scripts shaping adolescent sexual behaviour, and no systematic review has focused specifically on culturally based HIV education for adolescents. This review evaluated its effectiveness on knowledge, attitudes, and preventive intentions or behaviours.

Methods: This systematic review is reported in accordance with PRISMA 2020; the protocol was prepared using PRISMA-P and prospectively registered in PROSPERO (CRD420261288038). PubMed/MEDLINE, ScienceDirect, EBSCOhost, the Cochrane Library, and Taylor & Francis were searched up to 1 February 2026. Eligible studies were controlled trials with random or quasi-random allocation comparing culturally tailored HIV education with a standard, non-adapted, or no-intervention control among adolescents aged 10–19 years. Two reviewers independently screened records in Rayyan, extracted data in duplicate, and appraised each trial at item level using the 13-item Joanna Briggs Institute checklist for randomised controlled trials. Because no two trials shared a common effect metric, outcome construct, and follow-up interval, meta-analysis was not defensible; findings were synthesised using Synthesis Without Meta-analysis (SWiM) principles, and certainty was appraised using GRADE domains.

Results: Of 543 records, 536 were screened after deduplication and 16 full texts assessed; six trials (1994–2015) met the criteria (3,777 participants allocated; mean age 12.6–14.6 years, range approximately 11–18 years). Four trials were conducted in the United States, one in the Philippines, and one in Spain. Only two trials directly contrasted a culturally congruent version with a culturally dissimilar version of the same programme; in the remaining four, cultural tailoring was embedded within a broader programme compared with attention-matched non-HIV education, no intervention, or a second active programme. Five trials reported greater overall knowledge gains in the culturally adapted condition, while one reported no overall main effect with benefit confined to a subgroup. Five trials reported attitude/belief outcomes, with four showing favourable changes in HIV-related attitude measures; effects on intentions and behaviours were mixed, with behavioural benefits persisting to six months in one trial. At item level, no trial satisfied all 13 JBI criteria; allocation concealment and blinding were unclear or absent in most trials, and two trials did not report a random allocation sequence. Certainty of evidence was low for knowledge and attitudes and very low for intentions and behaviours.

Conclusion: Across a small, geographically concentrated, and methodologically heterogeneous evidence base, culturally based HIV education was generally associated with greater gains in HIV knowledge than comparison conditions, although one direct-comparison trial reported no overall main effect, with less consistent benefits for attitudes and mixed effects on preventive intentions and behaviours. Because most included trials did not isolate cultural adaptation from concurrent differences in content, intensity, or comparator activity, these findings should be read as cautious evidence of benefit rather than as demonstrated superiority over conventional HIV education. Trials that hold content constant while varying cultural congruence, report effect estimates with confidence intervals, and measure long-term behavioural outcomes are warranted.

Keywords: HIV Prevention; Adolescents; Health Education; Cultural Adaptation; Sexual Health

INTRODUCTION

The epidemiology of HIV/AIDS in the third decade of the 21st century shows an alarming demographic shift, with adolescents requiring focused prevention efforts (1). According to UNAIDS estimates for 2024 (published in July 2025), approximately 40.8 million people were living with HIV globally; new HIV infections among children aged 0–14 years had declined by 62% since 2010, reaching approximately 120,000 in 2024 (2). UNICEF's 2024 annual results report states that, since 2010, new HIV infections among adolescents aged 15–19 years have fallen by almost 50% (1). A December 2024 UNICEF report also documented that half as many adolescent girls aged 15–19 years acquired HIV in 2023 as in 2010 (3).

Adolescents (10-19 years) account for a substantial proportion of new HIV infections globally, with a particularly high burden in sub-Saharan Africa and other low- and middle-income countries (4). In many settings, adolescents face structural, social, and cultural barriers to accessing HIV prevention information and services, including stigma, taboos around sexuality, and gender inequities (5).

Culture-based health education models offer solutions by integrating community values into intervention design (6). This approach can be divided into two levels of adaptation: surface structure and deep structure (7). Surface structure involves adjusting directly observable elements, such as language, music, clothing, and visual backgrounds in educational materials (8). Meanwhile, deep structure involves integrating the historical, social, and psychological forces that underlie certain groups' behaviour, such as the use of traditional belief systems or the role of community elders in conveying prevention messages (7).

Insufficient HIV knowledge, permissive attitudes toward risky sexual behaviours, and low levels of preventive intentions, such as condom use or HIV testing, remain common among adolescents in diverse cultural contexts (9). Generic, non-contextualized education Programmes often fail to shift social norms and cultural scripts that shape adolescent sexual and risk behaviours (7).

Culturally based HIV education was defined as an educational intervention in which at least one core programme element content, materials, language of delivery, illustrative narratives, role models, facilitators, or normative framing was deliberately modified to correspond to the language, symbols, values, beliefs, social roles, or normative structures of an identified cultural, ethnic, religious, or national group, and in which the authors described that modification as an intentional design decision rather than an incidental feature of setting (10). Two adaptation levels were coded following Resnicow's surface- and deep-structure taxonomy: surface structure denotes observable matching of language, dialect, music, dress, actors, imagery, and setting; deep structure denotes incorporation of the historical, religious, social, and psychological forces underlying the behaviour of the group, such as religiously grounded sexual norms, family or generational authority, gender-role expectations, or community conceptions of illness (7,8).

Randomised trials of culturally tailored HIV prevention interventions, such as the Cuídate Programme among Latino adolescents in the United States and church-based interventions for families in rural Kenya, have demonstrated improvements in HIV knowledge, condom-related attitudes, parent-adolescent communication, and some measures of safer sexual behaviour compared with standard interventions (11,12). Other HIV-prevention trials in distinct populations and settings have reported improvements in condom use, HIV knowledge, self-efficacy, and other risk-related outcomes (13,14). These trials include youths as well as adult women and therefore provide contextual evidence for HIV-prevention intervention design rather than adolescent-specific evidence of cultural adaptation.

Existing reviews on adolescent HIV prevention often pool a wide range of biomedical, structural, behavioural, and digital interventions and seldom distinguish clearly between culturally adapted and non-adapted educational approaches (9). Reviews of school-based peer education (15), adolescent behaviour change interventions in sub-Saharan Africa (16), school-based interventions in the WHO Western Pacific Region (17), and contextual or culturally adapted HIV interventions across the life course each cover part of this territory (7). Their units of analysis, however, are either the delivery platform (school, peer, digital), the geographic region, or the full age spectrum of people living with or at risk of HIV. Cultural adaptation frameworks have likewise been reviewed outside HIV, in psychosocial interventions generally (18). What is not available is a synthesis restricted simultaneously to adolescents aged 10–19 years, educational rather than biomedical or structural interventions, and controlled trials in which the cultural adaptation of the education is the described feature of the intervention. Narrowing the frame in this way permits one inference that broader reviews cannot support: whether the benefit reported for culturally adapted adolescent HIV education is attributable to cultural congruence itself, or whether it is confounded with programme content, dose, and comparator activity. That question is answerable only by identifying trials that hold content constant while varying cultural congruence, the organizing logic of the present review.

Therefore, this systematic review aimed to identify and synthesise evidence from controlled trials evaluating culturally based HIV prevention education among adolescents aged 10-19 years; characterise the type and depth of cultural adaptation and the nature of the comparator in each trial; and evaluate the effectiveness of culturally based HIV prevention education, stratified by contrast type, in improving HIV-related knowledge, attitudes, and preventive intentions or behaviours among adolescents.

METHODS

Reporting of the review findings

This systematic review is reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement, which is the applicable standard for a completed review report; the completed PRISMA

2020 checklist is provided as Supplementary File 1. The review protocol was prepared using the PRISMA-P extension, which applies to protocols rather than to final reports, and was prospectively registered in PROSPERO on 18 January 2026 under registration number CRD420261288038; the registered record is publicly available, and the full protocol is provided as Supplementary File 2. The review was conducted between January and March 2026.

Eligibility criteria

Eligibility criteria followed the PICOS framework. Population (P): Adolescents aged 10–19 years inclusive, in line with the WHO definition, in any setting (schools, communities, faith-based organisations, health facilities, or digital platforms). Studies were eligible if the reported mean age fell within 10–19 years and the sampling frame was an adolescent population. Where a study enrolled a mixed-age sample, it was eligible only if at least 80% of participants were aged 10–19 years or if outcomes for the 10–19-year age band were reported separately; studies whose sampling frame was young adults were excluded, and one full text was excluded because its participants fell outside the 10–19-year band. Age ranges as well as means are reported for every included study in Table 2. Intervention (I): A culturally based or culturally tailored HIV prevention health education intervention meeting the operational definition and minimum adaptation threshold specified above. Comparator (C): Any concurrent comparison condition, comprising: (a) the same educational content delivered without cultural congruence; (b) standard or usual HIV education without explicit cultural adaptation; (c) attention-matched general health education without an HIV component; (d) no intervention or usual curriculum; or (e) a second active HIV education programme. Because these comparators differ in what they hold constant, each included trial was classified into one of four contrast types (Table 3). This classification governs the strength of inference permitted about cultural adaptation specifically: Type A, culturally congruent versus culturally dissimilar delivery of matched content (a direct test of cultural congruence); Type B, culturally tailored HIV education versus attention-matched non-HIV health education that is itself culturally tailored (a test of HIV content, not of cultural adaptation); Type C, culturally developed HIV education versus no intervention (a test of the programme as a whole); and Type D, two active, culturally situated programmes compared head to head (a test of relative efficacy between adapted programmes). Outcomes (O). Primary outcomes were HIV-related knowledge scores; HIV-related attitudes (for example, attitudes toward condom use, HIV testing, and people living with HIV); and HIV preventive intentions and/or behaviours (for example, intention to use condoms, actual condom use, number of sexual partners, frequency of unprotected sex, HIV testing uptake). Secondary outcomes were related psychosocial constructs (self-efficacy, parent–adolescent communication, perceived norms, risk perception) when reported. Study design (S). Controlled trials with random or quasi-random allocation at the individual, class, or school level, including pilot, pragmatic, cluster, and stepped-wedge designs. Additional criteria required full-text, peer-reviewed articles in English, with no year or country restriction, and excluded non-research publications (editorials, commentaries, narrative reviews), conference abstracts without full data, and grey literature.

Search strategy

Five interfaces were searched, corresponding to the following databases and collections: PubMed/MEDLINE (via the PubMed interface); MEDLINE Complete and CINAHL Complete (via the EBSCOhost platform); the Cochrane Central Register of Controlled Trials, CENTRAL (via the Cochrane Library interface); the Elsevier ScienceDirect full-text collection; and the Taylor & Francis Online full-text collection. ScienceDirect and Taylor & Francis Online are publisher platforms rather than bibliographic databases and were searched to capture full-text records not indexed in the sources above; this is stated explicitly because their coverage is not equivalent to that of a bibliographic database. Each source was searched from inception with no date limit. The last search date for every source was 1 February 2026; no source was searched on a different date, and no source was updated subsequently. The complete line-by-line strategy for each interface, including the exact search strings, controlled vocabulary (MeSH and CINAHL headings), field tags, Boolean operators, any filters or limits applied, the date each search was run, and the number of records returned per line, is provided in Supplementary File 3 in a format permitting direct re-execution. No filters for language, publication date, or study design were applied within the search interfaces; language and design eligibility were applied at screening. Reference lists of included studies and of relevant reviews were hand-searched; this route identified no additional eligible records. Two authors (N and FM) ran the searches independently and reconciled the retrieved sets.

Selection of studies

Records were deduplicated and imported into Rayyan. Two reviewers (N and FM) independently screened all 536 titles and abstracts in blinded mode in Rayyan, and the same two reviewers independently assessed all 16 full texts against the eligibility criteria; screening was therefore independent and in duplicate at both stages. The two reviewers resolved disagreements at either stage through discussion; when they did not reach consensus, a third reviewer (EM) adjudicated, and the adjudicated decision was final. Three full-text records required adjudication, all concerning whether the described adaptation met the minimum surface-structure threshold. Inter-reviewer agreement at the full-text stage was recorded in Rayyan. The PRISMA 2020 flow diagram (Figure 1) reports the number of records at each stage and the reasons for full-text exclusion.

Data extraction and management

Two reviewers (N and FM) extracted data independently and in duplicate for every included study, using a standardised form in Microsoft Excel that was piloted on two studies and refined before full extraction. Extracted records were compared field by field; discrepancies were resolved by discussion, and unresolved discrepancies were referred to a third reviewer (MKS). All numerical entries were verified against the source article by the reviewer who had not performed the original entry for that

field. Study authors were not contacted, as all six trials were published between 1994 and 2015 and contact details were not current. The following were extracted: study characteristics (authors, year, country, journal, setting); participant characteristics (age mean and range, sex, cultural or ethnic group, educational level, sample size as allocated and as analysed); design characteristics (unit of allocation, allocation procedure as described by the authors, number of clusters, and whether the original analysis accounted for clustering); intervention details (theoretical framework, content, delivery method, the specific cultural elements adapted, the process by which adaptation was undertaken, and the surface- or deep-structure classification, duration, frequency, intensity); comparator characteristics (content, cultural status of the comparator, duration, intensity, and contrast type); and outcome measures. For every primary outcome, the following were extracted, in order of preference: (i) an adjusted between-group effect estimate with a 95% confidence interval; (ii) an adjusted between-group test statistic with degrees of freedom and an exact p-value; (iii) group-specific means with standard deviations or standard errors at each assessment point; and (iv) group-specific proportions with denominators. The analysed denominator and the exact follow-up interval were extracted for every effect. Where a study reported multiple follow-up points, all were extracted (immediate post-intervention, 2 weeks, 3 months, 6 months) so that outcomes could be grouped by comparable timing during synthesis.

Risk-of-bias assessment

Methodological quality was appraised using the Joanna Briggs Institute (JBI) Critical Appraisal Checklist for Randomized Controlled Trials. The JBI checklist comprises 13 appraisal questions answered "yes", "no", "unclear", or "not applicable", and yields an overall include, exclude, or seek-further-information decision. It is not a domain-based risk-of-bias instrument and does not incorporate a validated algorithm for classifying trials as low, moderate, or high risk of bias. The primary presentation in this review is therefore the item-level appraisal for all 13 questions for every trial (Table 1), and no overall risk category is assigned. To assist comparison between trials, the count of items answered "yes" out of 13 is reported alongside a narrative summary of which specific items were not met; this count is descriptive and is not interpreted as a risk-of-bias score. Two reviewers (N and FM) appraised each item independently; disagreements were resolved by discussion with a third reviewer (MKS). Appraisal of the randomisation and trial-design items was made after the unit of allocation, the allocation procedure as described by the trial authors, and the handling of clustering had been extracted, since these determine whether those items can be answered affirmatively.

Data synthesis and analysis

Outcomes were first grouped a priori into four domains: HIV knowledge, HIV-related attitudes, preventive intentions, and sexual behaviour, and within each domain into three timing bands: immediate to 2 weeks post-intervention, 1–3 months, and 6 months or longer. The effect metric available from each trial was then determined for each domain-by-timing group (Table 4). Synthesis followed the Synthesis Without Meta-analysis (SWiM) reporting guideline. Studies were grouped by outcome domain, timing band, and contrast type. Within each group, the standardised metric for synthesis was the direction of the adjusted between-group effect and its reported precision, and the synthesis method was vote-counting based on effect direction, supplemented by reported effect estimates. Vote-counting was applied to effect direction rather than statistical significance, and results are presented as the number of comparisons favouring the culturally adapted condition out of the total contributing comparisons, with corresponding effect estimates displayed alongside to show magnitude. Studies were ordered within groups by contrast type, because Type A contrasts carry evidential weight for cultural congruence that Types B, C, and D do not. Certainty of evidence was appraised for each primary outcome domain using the GRADE domains of risk of bias, inconsistency, indirectness, imprecision, and publication bias, adapted for a non-meta-analytic synthesis in which imprecision was judged from the reported confidence intervals and analysed denominators of contributing trials rather than from a pooled interval (Table 5). Formal statistical tests for small-study effects were not undertaken, as they are uninformative with six studies; the possibility of publication and selective-reporting bias is addressed narratively.

RESULTS

Study Selection

A systematic literature search across five sources (PubMed/MEDLINE, ScienceDirect, Cochrane CENTRAL, Taylor & Francis, EBSCOhost) identified 543 records; 7 duplicates were removed, yielding 536 unique records for title and abstract screening. Of these, 520 records were excluded at title and abstract screening because they did not meet the eligibility criteria. Exclusion labels in Rayyan were not mutually exclusive at this stage, so only the total number of excluded records is reported, consistent with the PRISMA 2020 flow diagram. Sixteen reports were sought for full-text retrieval, and one could not be retrieved. The remaining 15 full-text reports were assessed for eligibility, and nine were excluded. Some excluded reports met more than one exclusion criterion; the 11 reasons recorded across the nine reports were: intervention not explicitly culturally tailored (n=7), ineligible study design (n=1), no HIV knowledge, attitude, or intention outcome (n=1), outcome limited to biological STI endpoints (n=1), and participants outside the 10–19-year band (n=1). Ultimately, six controlled trials were included in the systematic review. All six reported HIV knowledge outcomes; five reported attitude/belief outcomes, three reported preventive intentions, and two reported sexual behaviour, as detailed in Table 4 and illustrated in the PRISMA 2020 flow diagram (Figure 1).

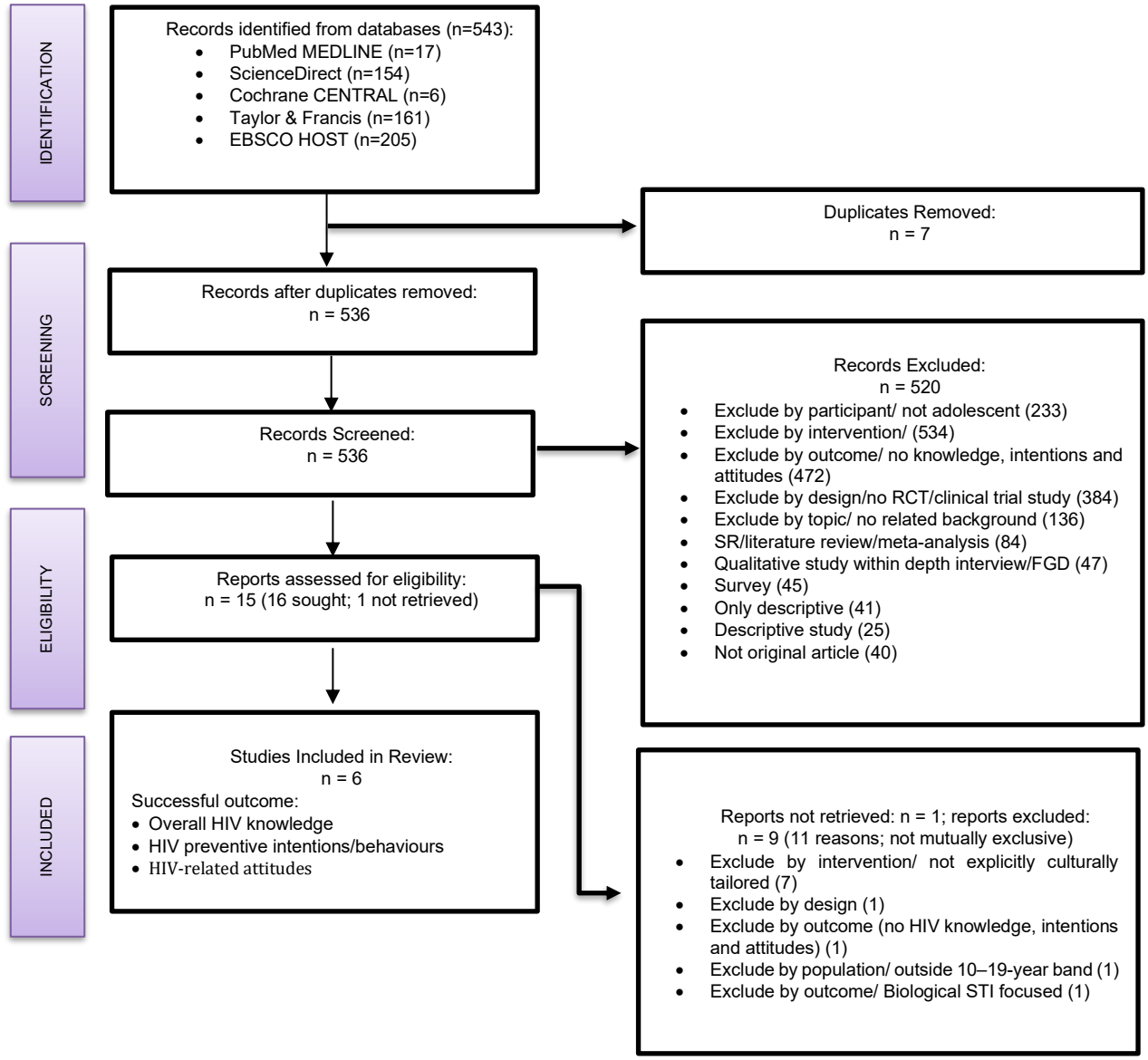


Figure 1. PRISMA 2020 Flow Diagram of the Study Selection Process

Methodological quality of the included trials

All six included trials were assessed against the 13 JBI criteria (Table 1), and none met all criteria. The most common concerns were unclear or inadequate allocation concealment, lack of participant and interventionist blinding, unclear outcome-assessor blinding, absence of intention-to-treat analysis, and incomplete follow-up. True randomisation was adequately described in three trials, unclear in two, and absent in one. Measurement reliability and statistical analysis were appropriate in all six trials. Five trials used class- or school-level allocation, but only two accounted for clustering in the analysis. The remaining three cluster-allocated trials analysed individual-level data without adjustment for clustering, potentially overstating precision. A sensitivity assessment restricted to the three trials with adequately described randomisation and design-appropriate analysis did not change the direction of the knowledge finding but reduced the evidence for attitudes and behaviour and eliminated both Type A contrasts. Thus, the trials providing the most direct evidence of cultural congruence were also among those with weaker allocation reporting.

Table 1. JBI quality assessment of the included studies

Author, year	Q1 True random.	Q2 Alloc. conceal.	Q3 Baseline equal	Q4 Blind participants.	Q5 Blind personnel	Q6 Blind assessors	Q7 Identical tx	Q8 Complete f/u	Q9 ITT analysis	Q10 Consistent meas.	Q11 Reliable meas.	Q12 Statistics	Q13 Design approp.	Total "Yes"
Jemmott et al. (1999)(19)	Y	U	Y	U	U	Y	Y	Y	U	Y	Y	Y	Y	9
Aplasca et al. (1995) (20)	Y	U	N	N	N	U	U	Y	N	Y	Y	Y	Y	6
Espada et al. (2015) (21)	Y	U	N	N	N	U	Y	N	N	Y	Y	Y	Y	6
Stevenson and Davis (1994) (22)	U	N	Y	N	N	U	Y	Y	N	Y	Y	Y	Y	7
Stevenson et al. (1995) (23)	U	N	Y	N	N	U	Y	Y	U	Y	Y	Y	Y	7
Siegel et al. (1995) (24)	N	N	Y	Y	Y	U	U	N	N	Y	Y	Y	Y	7

JBI, Joanna Briggs Institute; Y, yes; N, no; U, unclear.

Study characteristics

Table 2 summarises the six included trials, published between 1994 and 2015. In total, 3,777 participants were allocated, and 3,314 contributed to the analyses reported here. Mean ages ranged from 12.6 to 14.6 years, and reported age ranges spanned approximately 11 to 18 years, consistent with the 10–19 year eligibility criterion. Four trials were conducted in the United States (19,22–24), one in the Philippines (20), and one in Spain (21). All six were delivered in school or school-linked community education settings. The unit of allocation was the individual in one trial (19), the school with random allocation in two (20,21), the class in two (22,23), and the school without a described random sequence in one (24). Clustering was accounted for in the original analysis in two trials (20,21). It was not accounted for in the three trials allocating at class or school level, one of which analysed matched individual pre–post data; in the individually randomised trial, the small groups nested within condition were likewise not modelled (19,22–24). Clustering by allocation unit was therefore not applicable only to the individually randomised trial (19). This matters for interpretation, because effect estimates from cluster-allocated trials analysed at the individual level are likely to be reported with spuriously narrow precision. For Aplasca et al., 845 students completed the baseline survey, and 804 (420 intervention, 384 control) completed the post-intervention survey and constitute the analysed sample; randomised, followed-up, and analysed denominators are reported separately for all trials (20).

Table 3 shows two patterns. Adaptation was almost entirely surface-level language, imagery, actors, and setting, with only Aplasca et al. engaging deep structure through a Roman Catholic normative context (20), and Espada et al. removing the source programme's deep-structure elements without substituting a Spanish equivalent (21). More consequentially, the comparator determines what each trial can estimate: only two trials hold content constant while varying cultural congruence (22,23), whereas the remaining four compare against a culturally tailored programme (19), a second culturally situated programme (21), or no intervention (20,21,24). Results are therefore reported by contrast type.

Table 2. Characteristics of included studies

Author, year	Country/setting	Design, allocation unit & clustering	Sample (allocated/analysed)	Age, mean (range)	Intervention	Comparator	Dose/follow-up
Jemmott et al., 1999 (19)	United States; public schools, Trenton, NJ; Saturday small-group sessions	RCT; individual allocation stratified by gender and age using computer-generated random sequence; clustering not applicable (individual allocation), small groups nested within condition not modelled	496 allocated (HIV 269 / control 227); 480 analysed at 3 months (96.8%); 460 at 6 months (92.7%)	13.2 (SD 0.94); grades 7-8	5-hour culture sensitive small group HIV risk reduction programme (SCT, TRA/TPB); videos witl multiethnic casts, condon skills, role play games; 6–8 pe group; 4 trained facilitators	5-hour attention-matched general health promotion (CVD, cancer, nutrition, smoking), also delivered with culturally congruent materials and inner-city casts	Single 5-h session; post, 3 mo, 6 mo

Author, year	Country/setting	Design, allocation unit & clustering	Sample (allocated/analysed)	Age, mean (range)	Intervention	Comparator	Dose/follow-up
Aplasca et al., 1995 (20)	Philippines; 4 public high schools, semi-urban Metro Manila	Cluster RCT; school-level randomisation (2 intervention, 2 control), one classroom randomly selected per grade level; clustering addressed by classroom-level ANOVA correction and intraclass correlation estimate	845 completed baseline; 804 analysed (420 intervention/ 384 control)	14.7 intervention / 14.9 control; high school years 1–4 (approx. 13–16)	12 lessons of 40 min twice weekly over 6 weeks; Pilipino-language manual, flip charts, audio tapes, role play, games, dialogues, values clarification; PEHM teachers trained over 2 days	No AIDS prevention programme (usual curriculum); no attention control	6 weeks; surveys at 2 and 8 weeks post (8-week reported)
Espada et al., 2015 (21)	Spain; 18 public high schools across 5 provinces	Cluster RCT; school-level randomisation using a computerised random number generator, 6 schools per arm; clustering accounted for by generalized estimating equations with school controlled in all analyses	1,563 allocated (COMPAS 622 / ¡Cuídate! 442 / control 499); 1,268 analysed (519 / 334 / 415); 82.8% retention	14.61 (SD 0.77); range 14–16	COMPAS, five 50-min sessions (IMB model, Social Learning Theory). ¡Cuídate!, Spanish-adapted, six 45-min modules (SCT, TRA/TPB); interactive activities, role play, condom skills; psychologist facilitators	Two comparisons: (i) each active programme versus a no-intervention control group; (ii) COMPAS versus ¡Cuídate! head-to-head	5–6 weekly sessions; 1 week post
Stevenson & Davis, 1994 (22)	United States; community centres, summer academic remediation programme, northeastern city	Cluster-allocated trial; 8 classes randomly assigned to CSV or CDV; clustering not accounted for in analysis (ANCOVA at individual level)	121 allocated (60 per condition as analysed); 111 with sex reported (52 male, 59 female)	14.3; eligibility restricted to 14–15 years	Culturally similar video (CSV, "Don't Forget Sherrie"): Black English, urban setting, all African-American actors, contemporary music, culturally congruent life dilemmas; plus focus group discussion	Culturally dissimilar video (CDV, "All of Us and AIDS"): standard English, suburban setting, predominantly white actors; content matched by independent judges (82% agreement); no no-treatment arm	Single session (1.5 h); 2 weeks post
Stevenson et al., 1995 (23)	United States; community centres, summer academic remediation programme, northeastern city	Controlled trial with quasi-random allocation; 10 classes (18–25 students each) formed by geographic proximity and assigned to CSV or CDV; no random allocation sequence described; clustering not accounted for	194 allocated (74 male, 120 female); 165 analysed for the primary ANCOVA	14.6; adolescent summer programme (approx. 13–17)	CSV produced from the same script as the CDV but co-directed, rehearsed and filmed by 12 adolescents from the participant pool; adaptation on language, environment, race of actors, fashion, expressions, music; plus focus group	CDV: original professionally produced video from which the CSV script was derived; script held constant across arms; no no-treatment arm	Single session (1.5 h); 2 weeks post

Author, year	Country/setting	Design, allocation unit & clustering	Sample (allocated/analysed)	Age, mean (range)	Intervention	Comparator	Dose/follow-up
Siegel et al., 1995 (24)	United States; 3 junior high schools, inner-city district, northern California	Non-randomised controlled trial; allocation at school level (2 intervention schools, 1 demographically similar control school) without a random sequence; authors state individual randomisation was not possible; clustering not accounted for	1,078 completed pretest; 915 posttest; 557 matched pre–post analysed (434 intervention / 123 control)	12.6; grades 7–9 (approx. 12–15)	7-unit, 12-session HIV-prevention curriculum over 3 weeks; interactive exercises, videotapes, decision-making and refusal skills, response rehearsal; science teachers with 2-day training; curriculum reviewed by community stakeholders and pilot-tested for language and experiential fit	Demographically similar school receiving no HIV-prevention curriculum	3 weeks; 3 months post

CSV, culturally similar video; CDV, culturally dissimilar video; IMB, information–motivation–behavioural skills; PEHM, Physical Education, Health and Music; SCT, social cognitive theory; TRA/TPB, theory of reasoned action/theory of planned behaviour.

Table 3. Cultural adaptation characteristics and contrast type of the included trials

Author, year	Cultural elements adapted and adaptation process	Adaptation level	Cultural status of comparator	Contrast type and inference permitted
Jemmott et al., 1999 (19)	Content derived from focus groups with youth from the study population; videos with multiethnic casts; role play built around an African-American couple depicted in a magazine; culturally familiar activity formats; all materials pilot-tested with inner-city African-American adolescents; facilitator race and gender systematically varied by design	Surface structure (language, actors, imagery, activity formats). Limited deep structure; no explicit engagement with community belief systems or generational authority	Also culturally tailored: the general health promotion control used multiethnic-cast videos and an inner-city cast, and was matched for duration, group size, and facilitator training	Type B. Because both arms are culturally congruent and matched for attention and dose, this trial estimates the effect of HIV-specific content, not the effect of cultural adaptation. Its findings support the value of culturally tailored HIV education as a package but cannot attribute benefit to cultural congruence
Aplasca et al., 1995 (20)	Curriculum co-developed by Filipino public high school teachers with local AIDS experts, social scientists, health educators, and curriculum experts from the Department of Education; manual written in Pilipino; content negotiated against a predominantly Roman Catholic normative context, with emphasis on delaying sex until adulthood or marriage, condoms framed as HIV prevention rather than family planning, and condom demonstration excluded at the request of school authorities	Surface structure (language of delivery, locally produced materials) and deep structure (religious and normative framing of sexuality, marriage and family values, negotiated content boundaries)	No intervention; usual curriculum; no cultural or attention matching	Type C. Estimates the effect of a locally developed, culturally embedded programme against no programme. Cultural adaptation is confounded with the presence of any HIV education, so the contrast cannot isolate cultural congruence
Espada et al., 2015 (21)	¡Cuídate! adapted from a Latino-US curriculum to the Spanish context through three focus groups (topic experts, postgraduate psychology students, Spanish adolescents); Latino-specific content removed; vocabulary and expressions adapted; the original audiovisual material was withdrawn because setting, music, vocabulary and US inner-city actors were judged	Surface structure (vocabulary, expressions, examples). Deep-structure elements of the source programme (familismo, gender-role expectations) were partly	Two comparators: a no-intervention control (no cultural content), and a second active programme that is itself culturally situated for the same population	Types C and D. The comparison with the control group tests each programme as a whole; the head-to-head comparison tests relative efficacy between two culturally situated programmes and found no significant difference on any outcome. Neither comparison isolates cultural congruence

	inappropriate for Spanish adolescents. COMPAS was locally developed for Spanish adolescents	removed rather than replaced during adaptation, and no Spanish deep-structure equivalent was substituted		
Stevenson & Davis, 1994 (22)	Two pre-existing videos selected and matched for content by independent judges across seven AIDS knowledge domains and five belief domains (82% agreement). The CSV differed from the CDV on language (slang and Black English), environment (urban setting), race of actors, fashion, life dilemmas, and contemporary background music	Surface structure, explicitly and exclusively; the authors define cultural similarity along observable dimensions	Culturally dissimilar delivery of content-matched material (standard English, suburban setting, predominantly white actors)	Type A. Content is held approximately constant while cultural congruence is varied, so this trial provides a direct, if small, test of cultural congruence. It is one of only two such tests in the evidence base
Stevenson et al., 1995 (23)	Both arms used the same script; the CSV was produced by allowing twelve adolescents from the participant pool to co-direct, rehearse, modify, and film it, making the adaptation process participatory. Adaptation dimensions were language (appropriate slang and Black English), environment (urban, graffiti-walled community centre), race of actors, fashion, behavioural style and expressions, and contemporary music	Surface structure, with a participatory adaptation process; no deep-structure content specified	Culturally dissimilar delivery of the same script (upper-middle-class standard English, suburban high school, predominantly Caucasian actors)	Type A. Script held constant across arms, giving the cleanest available test of cultural congruence in this review. The trial reported no overall main effect of cultural congruence on knowledge; the benefit was confined to a self-appraisal subgroup
Siegel et al., 1995 (24)	Curriculum reviewed before implementation by school district representatives, classroom teachers, county health department educators, parent organisations and the local Red Cross; designed to be culturally appropriate for a predominantly non-white (African-American and Asian) middle-adolescent population; each segment pilot-tested in demographically similar schools and modified where students reported difficulty from language or experiential differences	Surface structure (language accessibility, locally reviewed content). Adaptation is described procedurally rather than specified in terms of adapted elements, which limits reproducibility	No HIV-prevention curriculum; cultural status of the control condition not described	Type C. Estimates the effect of the curriculum as a whole against no curriculum; cultural adaptation is not separable from programme content or dose

Contrast types: A, culturally congruent versus culturally dissimilar delivery of matched content; B, culturally tailored HIV education versus attention-matched, culturally tailored non-HIV health education; C, culturally developed HIV education versus no intervention; D, two active culturally situated programmes compared head to head.

Effects on knowledge, attitudes, intentions, and behaviour

Table 4 makes both the vote-count and its limits visible. At the trial level, five of six knowledge comparisons showed an overall effect favouring the culturally adapted condition; Stevenson et al. (1995) reported no overall main effect, with benefit confined to a subgroup. Five trials reported attitude/belief outcomes, four of which showed favourable changes in HIV-related attitude measures. Findings for preventive intentions and sexual behaviour were mixed. Magnitude, however, runs inversely to inferential strength. The only estimates accompanied by confidence intervals and standardised effect sizes come from Types C and D, where cultural adaptation is confounded with programme content and dose, and even these are small outside the knowledge domain (d 0.06-0.40 for knowledge, 0.09-0.15 for attitudes and intentions). The two Type A contrasts, which alone isolate cultural congruence, report F statistics without adjusted variances or intervals, and neither measured intentions or behaviour. Timing compounds the problem: four trials assessed outcomes within two weeks, and only one reported at six months, where it found behavioural effects absent at three months. No two trials in any domain share a convertible metric, follow-up interval, and comparator.

Table 5 shows certainty declining in step with distance from the exposure: low for knowledge and attitudes, very low for intentions and behaviour. Risk of bias is the common downgrade across all four domains, reflecting absent or unclear allocation concealment in every trial, two trials without a described random sequence, and unmodelled clustering in three. What separates the domains is indirectness. Knowledge is informed by two direct tests of cultural congruence, attitudes by

two that were null on belief outcomes, and intentions and behaviour by none at all, so for the two most policy-relevant domains the exposure of interest is untested rather than merely uncertain. Imprecision compounds this, with analysed denominators falling from 1,541 for knowledge to 116 for behaviour, including one control subsample of twenty participants. Inconsistency is the only domain not downgraded for knowledge: five trials showed an overall favourable effect, while the sixth reported no overall main effect but a favourable subgroup interaction. The ratings therefore reflect the structure of the evidence base as much as the quality of individual trials.

Table 4. Between-group effect estimates by outcome domain, follow-up interval, and contrast type

Author, year (contrast type)	Outcome domain	Time point	Comparison	Effect estimate (precision as reported)	Analysed n
Stevenson & Davis, 1994 (22) Type A	Knowledge	2 weeks	Culturally similar vs culturally dissimilar video (content matched)	ANCOVA on 52-item scale: F=7.01, p<.05 favouring CSV. Adjusted means CSV 36.96 (1.2)→39.64 (1.9); CDV 38.61 (1.1)→39.88 (2.9). No CI reported; clustering by class not modelled	120
Stevenson & Davis, 1994 (22) Type A	Beliefs/attitudes	2 weeks	CSV vs CDV	Prevention beliefs p=.59; safer alternatives p=.60; cultural beliefs p=.75 — all null. Video favourability 3.84 (.4) vs 2.99 (.5), F=6.725, p=.04 favouring CSV	120
Stevenson et al., 1995 (23) Type A	Knowledge	2 weeks	CSV vs CDV (identical script)	Main effect of cultural congruence F=.84, ns. Perceived-knowledge × congruence interaction F=3.36, p<.05: "Know a lot" subgroup CSV 41.6 (6.1)→43.9 (5.8) vs CDV 41.4 (7.1)→39.0 (5.1); "know some" and "know a little" subgroups showed no CSV advantage. No CI; clustering not modelled	165
Jemmott et al., 1999 (19) Type B	Knowledge	Post / 3 mo / 6 mo	Culturally tailored HIV programme vs attention-matched, culturally tailored general health promotion	F(1,493)=24.17, p=.0001; F(1,477)=20.50, p=.0001; F(1,457)=6.02, p=.014. Adjusted means plotted; SEs not tabulated; no CI	493 / 477 / 457
Jemmott et al., 1999 (19) Type B	Attitudes/beliefs	Post / 3 mo / 6 mo	As above	Prevention beliefs F(1,486)=9.23, p=.003; F(1,471)=9.06, p=.003; ns at 6 mo. Hedonistic beliefs F(1,487)=6.02, p=.014; F(1,471)=5.46, p=.02; F(1,449)=5.47, p=.02. Subjective norms null at all points (p=.339/.245/.483)	449–487
Jemmott et al., 1999 (19) Type B	Intentions/self-efficacy	Post / 3 mo / 6 mo	As above	Intentions F(1,488)=5.42, p=.02; F(1,472)=5.89, p=.016; F(1,452)=3.89, p=.049. Self-efficacy F(1,488)=4.08, p=.044; F(1,472)=3.88, p=.05; F(1,452)=5.00, p=.026	452–488
Jemmott et al., 1999 (19) Type B	Sexual behaviour	3 mo / 6 mo	As above	3 mo: no significant difference on any of the 7 measures. 6 mo: risk-behaviour index F(1,445)=7.36, p=.007; unprotected coitus F(1,399)=4.66, p=.031; anal intercourse prevalence F(1,403)=11.70, p=.0007, frequency F(1,409)=10.59, p=.001, partners F(1,409)=8.29, p=.004; abstinence F(1,380)=0.55, p=.46 (null)	380–445
Aplasca et al., 1995 (20) Type C	Knowledge	8 weeks	Locally developed curriculum vs no intervention	Difference in mean change: AIDS biology +0.45 (P<0.001); transmission +0.93 (P<0.01); prevention +0.41 (P<0.01). No CI reported; classroom-level correction applied	804 (420/384)
Aplasca et al., 1995 (20) Type C	Attitudes	8 weeks	As above	Avoidance of people with AIDS −0.45 (P<0.01, favourable direction); compassion +0.12 (P<0.001)	804
Aplasca et al., 1995 (20) Type C	Intentions	8 weeks	As above	Intended preventive behaviour +0.08, not significant (trend P=0.07). Agreement that students should delay sex until adulthood/marriage significantly increased (P<0.001)	804
Espada et al., 2015 (21) Types C and D	Knowledge	1 week	i) COMPAS vs control; ii) ¡Cuídate! vs. control; (iii) COMPAS vs. ¡Cuídate!	Total 25-item knowledge, GEE coefficient: i) 3.10 (95% CI 2.32 to 3.88), p<.0001, d=0.40; ii) 2.45 (95% CI 1.63 to 3.26), p<.0001, d=0.30; iii) 0.65 (95% CI −0.30 to 1.61), p=.18, d=0.06	1,541
Espada et al., 2015 (21) Types C and D	Attitudes	1 week	As above	Total HIV attitudes: (i) 1.89 (95% CI 0.70 to 3.09), p=.0018, d=0.15; (ii) 1.54 (95% CI −0.11 to 3.21), p=.069, d=0.09; (iii) 0.34, p=.68, d=0.02	1,536
Espada et al., 2015 (21) Types C and D	Intentions	1 week	As above	Safer-sex intention: (i) 0.59 (95% CI 0.18 to 1.00), p=.0042, d=0.14; (ii) 1.19 (95% CI 0.39 to 1.99), p=.0033, d=0.15; (iii) −0.59, p=.11, d=0.08	1,522

Author, year (contrast type)	Outcome domain	Time point	Comparison	Effect estimate (precision as reported)	Analysed n
Espada et al., 2015 (21) Types C and D	Risk perception/self-efficacy/norms	1 week	As above	Risk perception (i) 0.21 (95% CI 0.02 to 0.40), p=.025; (ii) 0.20 (95% CI −0.09 to 0.51), ns. Self-efficacy (i) 3.89 (95% CI 1.45 to 6.33), p=.0018; (ii) ns. Perceived norms null for both	1,503–1,523
Siegel et al., 1995 (24) Type C	Knowledge	3 months	Culturally reviewed curriculum vs no curriculum (school-level, non-randomised)	Mean change 11-item scale: intervention +1.2 (95% CI 1.0 to 1.4) vs control −0.03 (95% CI −0.3 to 0.3), p<.0001. Clustering not modelled	557 (434/123)
Siegel et al., 1995 (24) Type C	Attitudes	3 months	As above	Mean change 5-item scale: intervention +0.7 (95% CI 0.5 to 0.8) vs control +0.09 (95% CI −0.12 to 0.3), p<.0001; significant on 2 of 5 items	557
Siegel et al., 1995 (24) Type C	Sexual behaviour	3 months	As above	Sexual risk-taking change: intervention −0.6 (95% CI −1.1 to 0.1) vs control −0.5 (95% CI −1.5 to 0.4); no difference detected	116 (96/20)

Contrast types are defined in Table 3. CSV, culturally similar video; CDV, culturally dissimilar video; GEE, generalized estimating equations; d, standardised mean difference as reported by the trial authors. Where a trial reported no confidence interval, this is stated explicitly rather than imputed.

Table 5. Certainty of evidence by outcome domain, appraised using GRADE domains adapted for non-meta-analytic synthesis

Outcome domain	Trials (n)	Risk of bias	Inconsistency	Indirectness	Imprecision	Certainty and rationale
HIV knowledge	6 (2 Type A, 1 Type B, 3 Type C/D)	Serious: allocation concealment unclear or absent in all trials; 2 trials without a described random sequence; clustering unmodelled in 3	Not serious: five of six trial-level comparisons favoured the adapted condition overall; the remaining trial reported no overall main effect but a favourable subgroup interaction.	Serious: only 2 of 6 comparisons test cultural congruence directly, and both are the smallest trials; 4 of 6 trials from the United States; newest trial published 2015	Serious: CIs reported by only 2 trials; smallest analysed n=120; precision overstated where clustering was ignored	Low. Culturally based HIV education is probably associated with greater knowledge gain than comparison conditions, but the estimate cannot be attributed specifically to cultural adaptation.
HIV-related attitudes	5 (1 Type A, 1 Type B, 3 Type C/D)	Serious: as above, plus incomplete follow-up in 2 contributing trials	Serious: the culturally adapted arm of the one head-to-head trial did not reach significance; the Type A trial reporting attitude/belief outcomes was null on belief outcomes	Serious: no Type A trial demonstrated an attitude effect, so cultural congruence is untested for this domain	Serious: small effect sizes (d 0.09–0.15) where reported; item-level effects confined to 2 of 5 items in one trial	Low. Culturally based HIV education may improve attitudes toward people living with HIV and toward condom use, with small effect sizes and no direct evidence that cultural congruence is the active ingredient
Preventive intentions	3 (no Type A)	Serious: as above	Serious: one trial null overall with a trend only; one trial significant but small; direction consistent, magnitude not	Very serious: no trial with a Type A contrast measured intentions; all evidence comes from programme-versus-nothing or head-to-head comparisons	Serious: d 0.14-0.15 where reported; one trial with a null primary result	Very low. The effect of culturally based HIV education on preventive intentions is uncertain, and the effect of cultural congruence specifically is untested.
Sexual behaviour	2 (1 Type B, 1 Type C; no Type A)	Very serious: behavioural subsamples small and self-reported; one trial non-randomised with 51.7% pre–post matching	Serious: one trial positive at 6 months only and null at 3 months; one trial null	Very serious: no direct test of cultural congruence; behavioural denominators as small as 116; low baseline sexual activity in school samples	Very serious: 20 participants in one control subsample; wide or unreported intervals	Very Low. The evidence does not permit a conclusion about the effect of culturally based HIV education on adolescent sexual behaviour.

DISCUSSION

The main findings of this systematic review are that culturally based HIV education was associated with greater overall knowledge gain than comparison conditions in five of the six trials, while the remaining Type A trial reported no overall main effect but a favourable subgroup interaction; benefits for attitudes were more variable, and effects on preventive intentions and behaviour were mixed. This gradient should be read with two structural qualifications, both of which limit what can be attributed to cultural adaptation. First, direction of effect is not the same as magnitude or certainty. Where confidence intervals were reported at all, the effects on attitudes and intentions were small, and certainty was rated low for knowledge and attitudes and very low for intentions and behaviour. Second, and more consequentially, only two of the six trials compared culturally congruent with culturally dissimilar delivery of matched content; in the remaining four, cultural tailoring was bundled with the presence of HIV content itself, with additional programme dose, or with a comparator that was also culturally tailored. The larger and statistically stronger effects in this review come from precisely those trials that do not isolate cultural congruence. In contrast, the two trials that do isolate it produced one small differential-change effect and one null main effect. This pattern aligns with the global literature on peer education and school-based interventions, which report that the most responsive domains are knowledge gains and risk awareness, while changes in risky sexual behaviours tend to be smaller and inconsistent (15-17,25,26).

Interpreting the mechanism requires similar restraint. It is plausible, and consistent with theory, that cultural integration strengthens cognitive-affective pathways in behaviour change: framing HIV within community values such as family honour, religiosity, and peer solidarity may shape injunctive norms and reduce dissonance between health messages and social expectations surrounding adolescent sexuality (27). None of the six included trials, however, tested this pathway. No trial measured cultural identification, perceived cultural fit, or ethnic identity as a mediator, and no trial conducted a formal mediation analysis linking cultural congruence to outcome through a normative or affective mechanism. Espada et al. measured perceived norms as an outcome and found no effect for either programme (21). Jemmott et al. measured subjective norms and found no effect at any time point (19). Stevenson and Davis measured culturally motivated AIDS beliefs and found no between-condition difference (22). The only mechanism-adjacent signals in the evidence base are that adolescents rated the culturally congruent material more favourably than the culturally dissimilar material, and that the culturally congruent condition benefited a subgroup with exaggerated self-appraised knowledge (23). Both point to an attentional or receptivity mechanism rather than a normative one. The norm-mediated account is therefore best treated as a hypothesis that the present evidence does not test, with receptivity the pathway that has at least indirect empirical support.

The most important interpretive caveat concerns the outcome gradient itself. The knowledge > attitude > intention ordering observed across the six trials reproduces a pattern documented in the broader educational literature. In a meta-analysis of school-based sex education in low- and middle-income countries, Fonner et al. reported a large pooled effect on HIV knowledge (Hedges' g = 0.63; 95% CI 0.49-0.78) but markedly smaller effects on self-efficacy (g = 0.25; 95% CI 0.14-0.36) and on condom use (OR = 1.34; 95% CI 1.18-1.52) (28). A comparable gradient appears in the meta-analysis of comprehensive sexuality education by Kim et al. In this context, cognition (OR = 5.76) and knowledge (OR = 2.00) produced the largest pooled effects, followed by attitudes (OR = 1.76) and, most weakly, behavioural intention (OR = 1.29; 95% CI 1.00-1.66) (29). Because this same hierarchy emerges reliably in programmes with no cultural adaptation whatsoever, the gradient observed here cannot be treated as evidence of a culture-specific effect. It is a general property of the response of educational interventions across outcome domains: proximal cognitive outcomes move most readily, distal behavioural outcomes least. The convergence strengthens the internal plausibility of the present findings as a description of what educational interventions do, while removing it as a basis for inferring what cultural adaptation adds. Framed positively, the question this review can advance is narrower than whether cultural adaptation works: it is whether cultural congruence increases receptivity to, and engagement with, education whose content is already effective. The two Type A trials, the acceptability finding, and the "know-it-all" subgroup interaction all point in that direction, and that is the specific contribution claimed here.

Compared with prior literature, these findings align with two major patterns. First, systematic reviews of adolescent behaviour change interventions in sub-Saharan Africa indicate that approximately half of programmes, particularly those employing peer education and life skills approaches, successfully enhance knowledge and certain preventive behaviour indicators (16). Second, global systematic reviews of school-based peer education report evidence of effectiveness in about 52% of studies, with knowledge improvements far more prominent than behaviour change (15). Similar results appear in reviews of school interventions in low- and middle-income countries in the WHO Western Pacific Region (17). Against that background, this review does not demonstrate that culturally adapted programmes outperform generic ones, which the included evidence does not support, but rather that the field has largely not been designed to answer the question. Four of six trials compare a culturally adapted programme with no programme or with a non-HIV programme; the two trials that hold content constant are the two smallest, the two oldest, and the two with the weakest allocation reporting.

Generalisability is constrained in ways that bear directly on policy relevance. Four of the six trials were conducted in the United States, one in the Philippines, and one in Spain; no trial was conducted in sub-Saharan Africa, which carries the largest contemporary adolescent HIV burden, and none in South or Southeast Asia beyond the single Philippine trial. The newest included trial was published in 2015, and the oldest in 1994, so the entire evidence base predates the current HIV prevention landscape: none of these trials was designed in an era of oral pre-exposure prophylaxis, long-acting injectable prevention, HIV self-testing, undetectable-equals-untransmittable messaging, or social-media-mediated sexual health information. Cultural adaptation of HIV education today must contend with a message environment and a prevention toolkit that did not

exist when five of these six trials were conducted. Any extension of these findings to contemporary settings, and particularly to high-burden settings not represented here, is therefore an inference beyond the evidence rather than a direct application of it, and should be described as such in guidance documents.

This review also reveals limitations consistent with evidence and gap maps of social and behaviour change communication interventions for HIV prevention among adolescent girls and young women in low- and middle-income countries. Bose et al. demonstrate that current evidence is heavily concentrated on "awareness-intention" spectrum outcomes, with sparse data on "intention–action" and "action–habit" stages (27). A parallel pattern emerges in the trials identified here: definitions and measurements of cultural elements rely on authors' narrative descriptions and are rarely accompanied by indicators that permit systematic analysis of cultural mechanisms. This shortcoming is not confined to HIV prevention. In a systematic review of twelve cultural adaptation frameworks for psychosocial interventions, Day et al. found that most frameworks specify what may be modified intervention content and delivery context while offering little guidance on identifying which core components must be preserved to maintain fidelity to the original programme (18). Espada et al. illustrate the consequence precisely: adapting ¡Cuídate! for Spain involved removing the Latino-specific deep-structure content and withdrawing the audiovisual material without substituting a Spanish equivalent, and the adapted programme subsequently underperformed the locally developed programme on the attitudinal and risk-perception outcomes (21). Whether that reflects loss of active cultural components during adaptation or ordinary between-programme variation cannot be determined from the report. The absence of comparable adaptation-reporting standards across the six trials is why adaptation level could be classified only coarsely, and why the contribution of individual cultural components to the observed effects cannot be estimated.

From a practice and policy perspective, these findings are consistent with, rather than proof of, the proposition that adolescent HIV prevention programme design should move beyond generic education toward multi-component models integrating cultural sensitivity, peer education, and structural interventions. Reviews of "cash plus" or bundled interventions in Africa show that combining economic strengthening, reproductive health education, and gender equity training improves HIV knowledge, service utilisation, and several psychosocial outcomes, although impacts on HIV incidence and risky behaviour remain mixed (25). Within that framework, culturally based education is best positioned as a plausible and low-cost component of layered prevention packages whose overall effect rests on the other components as much as on cultural fit. In Indonesia, a survey of adolescents in Islamic boarding schools documented persistent misconceptions about HIV transmission alongside heavy reliance on social media as a primary information source, and identified structured health education within educational institutions as the principal corrective channel (30). Settings in which religious institutions shape both the delivery context and the normative content of sexual health education represent precisely the conditions under which deep-structure adaptation should matter most and precisely the conditions on which this review found no trial evidence. The single trial with a substantial deep-structure component was conducted in a Roman Catholic context in 1995 and compared its programme with no intervention (20). This is a specific, addressable gap.

Finally, this review's findings on single-session interventions emphasise that brief, structured formats yield gains in knowledge, HIV testing intentions, and stigma reduction (26). The behavioural mechanisms underlying such brief formats have been mapped by Mo et al., who found that digital HIV prevention programmes achieving improvements in knowledge and condom-use self-efficacy among adolescents consistently incorporated a specific subset of behaviour change techniques, notably prompts for intention formation and behavioural rehearsal (31). These techniques correspond closely to the skills-training and role-play components of the culturally tailored interventions reviewed here, suggesting that cultural adaptation and digital delivery may be complementary rather than competing design strategies—a hypothesis for future trial design rather than a finding of this review.

Limitations of the Study

Limitations of the included evidence. All six included studies used a concurrent comparison group, but two did not report a random allocation sequence. Five trials allocated at class or school level and only two accounted for clustering, so reported precision is likely overstated in three trials. Allocation concealment was unclear or absent in all six trials and no trial reported an intention-to-treat analysis. Outcome measurement relied on self-report throughout, including for sexual behaviour, and behavioural subsamples were very small (as few as 20 participants in one control subsample). Comparators were heterogeneous and, in four trials, did not isolate cultural adaptation. Cultural adaptation itself was described narratively rather than measured, so adaptation depth could be coded only coarsely. Finally, the evidence base is geographically concentrated (four of six trials from the United States) and temporally dated (1994–2015).

Limitations of the review process. Eligibility was restricted to English-language, peer-reviewed, full-text publications, and grey literature was excluded, so relevant programme evaluations published in other languages or outside the peer-reviewed literature plausibly including evaluations from high-burden settings will have been missed. Two of the five sources searched are publisher platforms rather than bibliographic databases, and no trial registry or regional database (for example, LILACS, African Index Medicus, or WHO ICTRP) was searched, which is a material omission given the geographic gap identified above. Study authors were not contacted for unreported variance data. The classification of adaptation level and contrast type was developed post hoc; although two reviewers coded independently, the categories were not pre-registered. Formal assessment of publication bias was not possible with six studies.

CONCLUSION

Across six controlled trials conducted between 1994 and 2015, culturally based HIV education was associated with greater overall HIV knowledge gains in five trials, while one direct comparison showed no overall main effect but a subgroup

interaction; improvements in HIV-related attitudes were smaller and less consistent, and effects on preventive intentions were mixed, with no trial providing direct evidence about sexual behaviour attributable to cultural congruence. Certainty in these findings is low for knowledge and attitudes and very low for intentions and behaviour. Only two trials held educational content constant while varying cultural congruence, and these produced one small differential-change effect and one null main effect qualified by a subgroup interaction; the larger effects in this evidence base come from trials in which cultural tailoring is inseparable from HIV content, programme dose, or comparator activity. The available evidence therefore supports the conclusion that culturally adapted HIV education is acceptable to adolescents and is delivered alongside knowledge gains in most trials, but it does not establish that culturally adapted education outperforms conventional education, and it does not identify cultural congruence as the active ingredient. Programme designers may reasonably continue to adapt HIV education to local language, symbols, and normative context on grounds of acceptability, engagement, and equity; they should not do so on the assumption that adaptation alone will change behaviour.

Priorities for future research follow from the structure of this evidence base. First, trials are needed that hold curriculum content, dose, and facilitator training constant while varying cultural congruence, so that the effect of adaptation can be estimated rather than assumed. Second, trials should be conducted in the settings that carry the greatest contemporary adolescent HIV burden and that were unrepresented here, particularly sub-Saharan Africa and religiously structured educational settings in Asia, where deep-structure adaptation is most likely to be consequential. Third, reporting should be standardised: the specific cultural elements adapted, the adaptation process, the core components preserved, and the cultural status of the comparator should be reported using an established adaptation framework, and effect estimates should be reported with confidence intervals and with analysis appropriate to the unit of allocation. Fourth, trials should measure cultural fit, cultural identification, or perceived congruence as candidate mediators, so that the mechanism can be tested rather than inferred. Finally, follow-up should extend beyond three months to include behavioural endpoints, since the only trial here reporting six-month behavioural outcomes found effects that were absent at three months.

DECLARATIONS

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