

PROTOCOL

PHR Project: 17/151/05

Whole-school interventions promoting student commitment to school to prevent substance use and violence, and improve educational attainment: a systematic review

Versions

Version	Amendment	Rationale	Date	Submitted to NIHR	Submitted to PROSPERO
1	Protocol amended from original proposal to indicate on page 8 that the quality of economic evaluations to be assessed using adapted version of Drummond et al rather than CHEERS checklist.	Discussion with Alec Miners the study economist concluded this is a more appropriate tool.	4/10/19	4/10/19	7/10/19

This protocol guides the conduct of independent research funded by the National Institute for Health Research (NIHR) in England under its Public Health Research Board (17/151/04). The views expressed in this protocol are those of the authors and do not necessarily reflect those of the National Health Service (NHS), the NIHR or the Department of Health for England.

Whole-school interventions promoting student commitment to school to prevent substance use and violence, and improve educational attainment: a systematic review

Background

Description of the problem

Interventions that promote student commitment to school to prevent inter-personal violence (physical, emotional and social) and substance (tobacco, alcohol and other drug) use are a category of intervention sharing a theory of change and addressing important, inter-correlated outcomes.[1-4] This review will categorise interventions into sub-types and examine implementation, effectiveness and cost-effectiveness overall and by intervention sub-type, and mediators and moderators of effects.

Alcohol has been suggested to be the most harmful substance in the United Kingdom (UK).[5] Treating alcohol-related diseases costs the NHS in England an estimated £3.5 billion annually.[6] The total annual societal costs of alcohol use in England are estimated at £21 billion.[7] Alcohol related harms are strongly stratified by socioeconomic status (SES).[8] Early initiation of alcohol use and excessive drinking are linked to later heavy drinking and alcohol-related harms [9, 10] and poor health.[11] Alcohol use among young people is associated with truancy, exclusion and poor attainment, as well as unsafe sexual behaviour, unintended pregnancies, youth offending, accidents/ injuries and violence.[12] Preventing young people from taking up smoking is another key public health objective with 80,000 deaths due to smoking each year.[13] Smoking has been estimated to cost the NHS £5.2 billion per year and wider societal costs amount to £96 billion.[14, 15] Of smokers, 40% start in secondary school [16] and early initiation is associated with heavier and more enduring smoking and greater mortality.[17, 18] Smoking among young people is a key driver of health inequalities.[16] Among UK 15–16-year-olds, 25% have used cannabis and 9% have used other illicit drugs.[17] Early initiation and frequent use of ‘soft’ drugs may be a potential pathway to more problematic drug use in later life.[19] Drugs such as cannabis and ecstasy are associated with increased risk of mental health problems, particularly among frequent users.[19-22] Young people’s drug use is also associated with accidental injury, self-harm, suicide [23-25] and other ‘problem’ behaviours.[26-29]

The review also focuses on physical and non-physical inter-personal violence. The prevalence, harms and costs of violence among young people mean that addressing this is a public health priority.[30, 31] One UK study found that 10% of young people aged 11–12 reported carrying a weapon and 8% admitted attacking someone with intent to hurt them seriously.[32] By age 15–16, 24% of students report that they have carried a weapon and 19% reported attacking someone with the intention to hurt them seriously.[32] There are also links between aggression and anti-social behaviours in youth and violent crime in adulthood.[33, 34] As well as leading to further health inequalities, the economic costs to society of youth aggression, bullying and violence are high. For example, the total cost of crime attributable to conduct problems in childhood has been estimated at about £60 billion a year in England and Wales.[35] The review will also examine the effects of such interventions on educational attainment since such effects are plausible and an important determinant of whether interventions are likely to be scaled up in the education sector.

Description of the intervention

There is increasing academic and policy interest in whole-school interventions that promote student commitment to school to achieve health and educational benefits. Such interventions aim to promote young people’s health by modifying the whole-school environment to engender student commitment to learning and to the school community (appendix 1: logic model).[36] Interest in such interventions reflects awareness that traditional health education lessons struggle to find a place in school timetables and have patchy results which tend to dissipate with time.[37-40] It also reflects interest in socio-ecological determinants of health, of which the school environment is one important element.[41] If effective, such interventions might represent a pragmatic and efficient

means of addressing multiple inter-correlated risk behaviours. Informed by the most specific and comprehensive theory of how schools can influence student commitment to school and through this students' health behaviours,[42, 43] we will focus on whole-school interventions that aim to reduce violence or substance use via:

- modifying teaching to increase student engagement in academic learning;
- enhancing student-staff relationships;
- revision of school policies which involves students and/or which goes beyond health or behaviour management policies;
- encouraging all students to volunteer in the community; or
- parental involvement in school life.

Rationale for the current study

We previously conducted a systematic review of the effects of schools and school-environment interventions on health.[43] The review for the first time synthesised existing theory to identify the theory of human functioning and school organisation as the most specific and comprehensive theory to date of how schools might engage in whole-school actions to promote health.[42, 43] This theory proposes that student commitment to school can engender health benefits by providing students with the autonomy, social support and reasoning ability to make informed decisions to avoid engaging in risk behaviours such as violence, smoking, drug use and drinking alcohol. The theory also suggests that promoting student commitment, particularly for more disadvantaged students, requires schools to: ensure teaching and other aspects of school life centre of student needs; involve students in decision-making; and build stronger relationships between staff and students and between schools and local communities.

The review also synthesised empirical evidence from intervention trials and other studies. This provided support for the effects of such factors on substance use and violence from five studies of interventions addressing these factors as well as four observational studies comparing different schools to assess whether such school-level factors were associated with better student health outcomes.[44, 45] The review strongly influenced and was cited by the 2016 Lancet commission on adolescent health, which recommended a major focus on the quality of the school environment as a determinant of adolescent health.[36] More locally, the review informed and was cited in guidance from Public Health England sent to head teachers on the importance of schools taking a synergistic approach to promoting students' health and academic development.[46] Furthermore, the review informed the theory of change of the 'Learning Together' whole-school intervention evaluated in the INCLUSIVE trial also funded by NIHR PHR.[47, 48] This trial has found multiple benefits arising from enhancing the school environment: reduced student bullying victimisation, smoking and alcohol use and drunkenness, and improved mental health and wellbeing, and quality of life.

However, the review is now almost a decade old and was exploratory in scope. Its inclusion criterion for interventions was not informed by a specific intervention theory of change and it only found a limited number of outcome studies (and no economic evaluations or assessment of educational outcomes). It was thus limited in its ability to test the theory of human functioning and school organisation, and to determine the best ways to promote student commitment and thereby reduce substance use and violence. Furthermore, the small number of included studies meant that the review could not assess how implementation and intervention effects might vary between populations and settings. The review excluded interventions that included health education components alongside school-environment components so that the review could assess whether school-environment action alone could affect health. With hindsight, it is clear to us that curriculum and school-environment components are potentially synergistic (for example by social and emotional skills curriculum preparing students to engage in whole-school change[49]) and the decision to exclude such interventions led to the exclusion of many otherwise relevant studies.

A Cochrane review conducted at about the same time, which also involved CB,[50] synthesised evidence on the effectiveness of 'health promoting schools' (HPS) interventions (defined

as those comprising school-environment, curricular and parent/community components). This reported significant effects of such interventions on bullying victimisation and tobacco use, as well as emerging evidence for effects on alcohol and drug use, bullying perpetration and other violence. While presenting promising evidence, the review was limited by the inclusion of interventions with a wide variety of school-environment components ranging from posters promoting healthy lifestyles to major changes in student participation in decisions. These lacked a common underlying theory of change and this limited the review's ability to test specific theories or recommend which specific whole-school interventions should be implemented.

Since the above reviews were published, there has been an upsurge in evaluations of whole-school interventions aiming to promote health by building student commitment to school (see below and appendix 2: search and studies found). In the light of this and the limitations with the earlier reviews, we propose to undertake a completely new review (rather than merely an update of our earlier review) focused on such interventions. Unlike the previous reviews, this review would focus on interventions sharing a similar, specific theory of change informed by the theory of human functioning and school organisation. This will enable the review to can draw more specific conclusions about which approaches to whole-school change are most likely to achieve health and educational benefits. The review would not exclude whole-school interventions that also include health curriculum components for the reasons discussed above. Given the expansion in the evidence base and the revised inclusion criteria, we are confident that the review will be able to examine a substantial body of evidence, which is confirmed by our scoping searches described below. This evidence will enable us to assess how the implementation and effectiveness of such interventions vary between populations and settings, and draw conclusions about cost-effectiveness and educational outcomes.

Research aim and questions

To search systematically for, appraise the quality of, and synthesise evidence to address the following research questions:

1. What whole-school interventions that promote student commitment to school to prevent student substance use and violence have been evaluated, what intervention sub-types are apparent and how closely do these align with the theory of human functioning and school organisation?
2. What factors relating to setting, population and intervention influence the implementation of such interventions?
3. Overall and by intervention sub-type, what are the effects of such interventions on student substance use, violence and educational attainment?
4. What is the cost-effectiveness of such interventions, overall and by intervention sub-type?
5. Are the effects of such interventions on student substance use and violence mediated by student commitment to school, or moderated by setting or population?

Research objectives

- To conduct electronic and other searches by 1/2/20.
- To screen references and reports for inclusion in the review by 1/4/20.
- To extract data from and assess the quality of included studies by 1/8/20.
- To synthesise intervention descriptions to describe sub-types and alignment with theory by 1/9/20.
- To synthesise process evaluations to explore factors influencing implementation by 1/11/20.
- To consult with policy/practice and community stakeholders on the results of these analyses by 1/12/20.

- To synthesise outcome evaluations to examine effects, mediators and moderators by 1/4/21.
- To consult with policy/practice and community stakeholders on the results of these analyses by 1/5/21.
- To draw on the above work to draft and submit to NIHR a report addressing our research questions by 1/8/21.

Research design overview

Our proposal is for a multi-method systematic review which will follow existing general criteria for review conduct and reporting (e.g. Centre for Reviews and Dissemination; Preferred Reporting Items for Systematic Reviews and Meta-Analyses). The protocol will be registered with PROSPERO International Prospective Register of Systematic Review (<http://www.crd.york.ac.uk/Prospero/>).

Size of available literature

We undertook an exploratory search of PubMed on 10/8/18 using free-text terms for intervention, setting, study type and outcomes (appendix 2: search and studies found). This retrieved 28,421 references that, after screening on title, abstract and full report, yielded 55 includable reports. Reference checking of these reports and personal communications with researchers yielded 25 other reports so that in total we identified 80 includable reports relating to 32 distinct studies and 25 distinct interventions of which only two studies were included in our previous review. This suggests a substantial but manageable body of evidence to synthesise. The full database search that will be undertaken if the review is funded would encompass more search terms and databases to increase sensitivity, and to minimise bias would not search by outcomes.

Inclusion criteria for this review

Types of participant

Children and young people aged 5-18 years attending school.

Types of intervention/environment

Whole-school interventions that aim to reduce student violence or substance use via:

- modifying teaching to increase student engagement in academic learning;
- enhancing student-staff relationships;
- revision of school policies which involves students and/or which goes beyond health or behaviour management policies;
- encouraging all students to volunteer in the community; or
- parental involvement in school life.

We will exclude studies of interventions that:

- involve health or social and emotional skills curricula without whole-school components;
- target selected students or parents rather than being universal, whole-school interventions;
- only address behaviour management in the classroom or school-wide without addressing engagement or commitment to school;
- involve students as peer educators or peer social marketers without students being involved in school policy- or decision-making; or
- revise policies or procedures relating purely to health or behaviour management without student input.

Types of control

Treatment as usual, no treatment or other active treatment.

Types of outcome

The review will focus on health behaviours and outcomes plausibly influenced by commitment to school according to the theory of human functioning and school organisation i.e., violence and substance use, as well as educational attainment. Violence might include inter-personal

physical, emotional or social abuse. Substance use might include use of tobacco, alcohol, or other legal or illegal drugs. Outcome measures will be quantitative and may be self- or teacher-reported via questionnaires or diaries, or rely on clinical or administrative data. Outcome measures may draw on dichotomous, categorical or continuous variables. Behavioural outcomes may focus on: behaviours over a specific period; frequency (monthly, weekly or daily); the number of episodes of a behaviour; or an index constructed from multiple measures. Violence measures may combine indicators of behaviour as well as the upset or injury caused by behaviour. Measures may examine particular or general forms of behaviours (including composite measures of overall substance use or violence) or convictions. Violence measures may combine inter-personal violence with forms of anti-social behaviour that are not inter-personal violence if the former constitute a majority of items. Educational attainment may be assessed via research-administered tests or routine data on academic progress or performance in tests or exams. Economic analyses may examine health-related quality of life.

Types of study

To address RQ1 we will draw on intervention descriptions from process evaluations and experimental or quasi-experimental evaluations, as defined below. To address RQ 2, we will include process evaluations that draw on quantitative and/or qualitative data to report on the planning, delivery or receipt of interventions, and which assess how factors relating to interventions, populations or settings influence implementation. To address RQ3, 4 and 5, we will include cluster randomised controlled trials and quasi-experimental evaluations where schools are allocated non-randomly to intervention and control groups. Studies addressing RQ4 will include economic evaluations that relate costs to outcomes or benefits.

Search methods for the identification of studies

Our search strategy will be informed by those used in our previous NIHR review of the health effects of school environments and school environment interventions but amended to reflect our more specific inclusion criteria. We will not restrict searches by date, since our review is not simply an update of the earlier review, or by language or publication type. In terms of searches of bibliographic databases, our searches will primarily use free-text terms because studies are unlikely to be reliably indexed in databases with controlled vocabularies. We will take the following concepts from our inclusion criteria to develop a search string linked by “AND”: school AND intervention AND study types. For each concept, we will use free-text and controlled-vocabulary terms linked by “OR”. We will search the following databases: ASSIA; Australian Educational Index; BiblioMap (Database of Health Promotion Research); British Educational Index; Cochrane Central Register of Controlled Trials; Cochrane Database of Systematic Reviews; Database of Abstracts of Reviews of Effects; Database of Promoting Health Effectiveness Reviews; Dissertation Abstracts (UK theses, all dates; global theses 2010-2015); Econlit; EResearch Index Citations; Health Technology Assessments; International Bibliography of the Social Sciences; MEDLINE; NHS Economic Evaluation Database; PsycINFO; Social Policy and Practice including Child Data & Social Care Online; Social Science Citation Index/Web of Knowledge; and Trials Register of Promoting Health Interventions.

We will also search the following websites: Cambridge Journals; Centers for Disease Control and Prevention: Smoking & Tobacco Use; Child and Adolescent Research Unit; Childhoods Today; Children in Scotland; Children in Wales; Community Research and Development Information Service; Database of Educational Research (Evaluation for Policy and Practice (EPPI)-Centre); Drug and Alcohol Findings Effectiveness Bank; Google; Google Scholar; Government of Wales; Government of Scotland; Joseph Rowntree Foundation; National Criminal Justice Reference Service; National Society of the Prevention of Cruelty to Children; National Youth Agency; Northern Ireland Executive; OpenGrey; Personal Social Services Research Unit; Project Cork; UCL-IOE Digital Education Resource Archive; UK Clinical Research Net Study Portfolio; University of Illinois at Urbana Champaign; US Centre for Substance Abuse Prevention; Social Issues Research Centre; The Campbell Library; The

Children's Society; Open Library; Schools and Students' Health Education Unit Archive; WHO International Clinical Trials Registry Platform; and Young Minds: Child & Adolescent Mental Health.

We will also search reference lists from all studies that meet our inclusion criteria. We will hand-search journals that published included studies which we found only via reference checking and which are not indexed on databases we have searched (initially for the last 5 years and if these elicit >1 new included studies, further back to 1990). We will contact subject experts to identify relevant ongoing or completed research.

Data management and screening

Results of comprehensive searching will be downloaded into EPPI-Reviewer 4.[51] Two reviewers will pilot the screening of successive batches of 50 titles/abstracts, meeting to discuss disagreements, calling on a third reviewer where necessary. After refinements and once we achieve a batch-level agreement rate of 90%, each reference will be screened on title and abstract for potential inclusion by one reviewer. Full reports will be obtained for references judged as meeting our criteria or where there is insufficient information from title/abstract to judge. Screening of full study reports will then commence in the same manner as above.

Data extraction

Two reviewers will independently extract data from, and assess the quality of, included studies using existing tools,[52-55] comparing decisions and calling on a third reviewer where necessary. For intervention descriptions we will extract on domains included in a standard descriptive framework.[56] For intervention theory of change, we will extract data on constructs, mechanisms and any contextual contingencies affecting these, as well as other theories cited.

For all studies where relevant, we will extract information on: basic study details (study location, timing and duration; individual and organizational participant characteristics); study design and methods (design, sampling and sample size, allocation, blinding, control of confounding, accounting for data clustering, data collection, attrition, analysis); process evaluation findings and interpretation; outcome measures (timing, reliability of measures, intra-class correlation coefficients, effect sizes); relevant mediation and moderation analyses; and economic data (inputs and outputs relating to costs, consequences/benefits, disaggregated by time period where appropriate). The two reviewers will independently enter data from the data extraction forms into EPPI-Reviewer 4. If included studies are reported in languages that cannot be translated by the review team, a review author will complete the data extraction form in conjunction with a translator.

Published reports may be incomplete in a wide range of ways. For example, they may not: present information on all the outcomes that were measured (possibly resulting in outcome reporting bias); provide sufficient information about the intervention for accurate characterisation; or report statistical information necessary for the calculation of effect sizes. In all cases where there is a danger of missing data affecting our analysis, we will contact authors of papers wherever possible to request additional information. If authors are not traceable or sought information is unavailable from the authors within two months of contacting them, we will record that the study information is missing on the data extraction form, and this will be reflected in our risk of bias assessment for the study.

Assessment of quality and risk of bias

Two reviewers will assess the quality of each empirical report. The two reviewers will then meet to compare their assessments, resolving any differences through discussion and, where necessary, by calling on a third reviewer. We will assess the quality of process evaluations using the standard EPPI-Centre tool.[57] This will address the rigour of: sampling; data collection; data analysis; the extent to which the study findings are grounded in the data; whether the study privileges the perspectives of participants; the breadth of findings; and depth of findings. These

assessments will then be used to assign studies to two categories of 'weight of evidence'. First, reviewers will assign a weight (low, medium or high) to rate the reliability or trustworthiness of the findings (the extent to which the methods employed were rigorous/could minimise bias and error in the findings). Second, reviewers will assign an additional weight (low, medium, high) to rate the usefulness of the findings for shedding light on factors relating to the research questions. Guidance will be given to reviewers to help them reach an assessment on each criterion and the final weight of evidence.

For randomised trial evaluations, we will assess risk of bias within each included study using the tool outlined in the Cochrane Handbook for Systematic Reviews of Interventions.[52] For each study, reviewers will judge the likelihood of bias for: sequence generation and allocation concealment; blinding (of participants, personnel, or outcome assessors); incomplete outcome data; selective outcome reporting; other sources of bias (e.g. recruitment bias in cluster-randomised studies); and intensity/type of comparator. Each study will subsequently be identified as 'high risk', 'low risk' or 'unclear risk' within each domain. For non-random evaluations, we will assess quality using the ROBINS-I tool.[55] We will assess the quality of the economic evaluations using an adapted version of the Drummond et al checklist.[58] This requires the analyst to answer 24 questions regarding each study, ranging from the type of economic evaluation (e.g. cost-utility analysis) to the time horizon and rationale for the choice of modelling approach. Although the questionnaire is detailed, we will expand a number of its questions to ensure that information that is particularly relevant to this review is extracted.

We will assess reporting bias according to Sterne's guidance.[59] We will reduce the effect of reporting bias by focusing synthesis on studies rather than publications and avoiding duplicated data. Following the Cho statement on redundant publications,[60] we will attempt to detect duplicate studies and, if multiple articles report on the same study, we will extract data only once. We will prevent location bias by searching across multiple databases. We will minimise language bias by not excluding any article based on language. LSHTM has an international staff and translation of articles is unproblematic.

Data analysis

We will use descriptions of interventions and corresponding theories of change to develop a categorisation of sub-types (RQ1) taking as a starting point the intervention definition above, and including intervention activities described alongside school sub-systems (individual student, classroom, school) targeted by intervention activities. Sub-types will be hierarchically organised to permit higher-level detail and finer-grained description, and will be generated using an inductive form of intervention components analysis to describe key aspects of difference between interventions.[61] Within each high-level sub-type, we will synthesise intervention theories using meta-ethnographic methods to build up theories of change for intervention sub-types using methods similar to those we have used in previous reviews.[61] To evaluate the fit of these intervention sub-type theories to the theory of human development and school functioning, we will use best fit framework synthesis, which is an appropriate synthesis method when seeking to understand the applicability of an existing theory as a frame for synthesising evidence. We will match our synthesis of intervention sub-type theories of change to different dimensions of the theory of human development and school functioning and consider where and how constructs do not fit the theory.

We will synthesise process evaluation findings and interpretations (RQ2) using meta-ethnographic synthesis methods. As with earlier reviews,[62] this will be applied to textual reports of not only qualitative but also quantitative research (since it is not possible to synthesise quantitative findings from process evaluations using statistical pooling because of the heterogeneity of foci, methods and measures). Meta-ethnography will examine themes across sources, identifying cases of 'reciprocal translation' where similar concepts are expressed in different ways in different sources, as well as cases of 'refutational synthesis' where concepts from different sources

contradict one another. We will then aim to develop a 'line of argument' synthesis which juxtaposes concepts from different sources to develop a multi-faceted overall explanation. Second-order constructs (authors' interpretations of qualitative data) will be distinguished from first-order constructs (directly quoted data). The synthesis will not be restricted to studies judged to be of high quality. Instead, conclusions drawing on poorer quality reports will be given less interpretive weight.

In terms of procedure, the synthesis of process evaluations will take the following steps. First, the reviewers will prepare detailed tables to describe: the quality of each report; its empirical focus; and study site/population. Second, the two reviewers will undertake pilot analysis of two reports. The reviewers will read and re-read the results from these reports, applying line-by-line codes to capture the content of the data. They will draft memos explaining these codes. Coding will begin with in-vivo codes which closely reflect the words used in theory/findings sections. The reviewers will then group and organise codes, applying axial codes reflecting higher-order themes. The two reviewers will meet to compare and contrast their coding of these first two high-quality studies for each synthesis, developing an overall set of codes. Third, the two reviewers will go on to code the remaining studies for each synthesis drawing on the agreed set of codes but developing new in-vivo and axial codes as these arise from the analytical process, and again writing memos to explain these codes. At the end of this process, the two reviewers will meet to compare their sets of codes and memos. They will identify commonalities, differences of emphasis and contradictions with the aim of developing each overall analysis which draws on the strengths of the two sets of codes and which resolves any contradictions or inconsistencies, drawing on a third reviewer if necessary to achieve this. Analysis will produce tables demonstrating how first, second and third order constructs relate to one another, enhancing transparency about these emergent themes.

To address RQ3, we will first produce a narrative account of the effectiveness of these types of interventions overall and by intervention sub-type. This narrative synthesis will be ordered by outcome then within this by age group, intervention sub-type and follow-up time. Outcomes will be categorised into violence, smoking tobacco, drinking alcohol, using other drugs and academic attainment. Age will be categorised by the key-stage age-ranges used in the English educational system. Categorisation by intervention subtype will be informed by our prior categorisation of intervention descriptions and theories of change (RQ1). We will describe study results in the 'characteristics of included studies' table. We will then produce forest plots for each of our review outcomes, with separate plots for different outcomes and age groups, intervention sub-types and follow-up times. Plots will include point estimates and standard errors for each study, such as risk ratios for dichotomous outcomes or standardised mean differences for continuous outcomes.

We will then examine the extent of heterogeneity among the studies (as determined both by a Cochran's Q test and inspection of the I^2). If an indication of substantial heterogeneity is determined (e.g. study-level I^2 value greater than 50%) that cannot be explained through meta-regressions, we will investigate this further using subgroup and sensitivity analyses. We will then undertake meta-analysis to generate pooled estimates of intervention effects. We will estimate separate models for substance use, violence and educational attainment outcomes, and for different age-ranges. We will examine substance use outcomes together in one analysis, as well as separated into smoking tobacco, drinking alcohol, other drug use and any 'omnibus' measures of substance use. We will regard follow-up times of less than three months, three months to one year and more than one year post-intervention as different outcomes. We will run these models for interventions overall and where sufficient studies are found we will run separate models for different intervention sub-types and comparators. This categorisation will be informed by our analysis of intervention descriptions and theories of change (RQ1).

When studies are found to be statistically heterogeneous, we will use a random-effects model; otherwise we will use a fixed-effects model. When using the random-effects model, we will conduct a sensitivity check by using the fixed-effect model to reveal differences in results. We will consider using a robust variance estimation meta-analysis model to synthesise effect sizes. This is because outcome evaluations are likely to include multiple measures of conceptually related

outcomes and robust variance estimation meta-analysis improves on previous strategies for dealing with multiple relevant effect sizes per study, such as meta-analysing within studies or choosing one effect size, by including all relevant effect sizes but adjusting for inter-dependencies within studies.[63] Unlike multivariate meta-analysis, it does not require the variance-covariance matrix of included effect sizes to be known. Where meta-analyses are performed, we will include pooled effect sizes in forest plots, with the individual study point estimates weighted by a function of their precision.

Prior to synthesis, we will check for correct analysis (where appropriate) by cluster and report values of: intra-cluster correlation coefficients, cluster size, data for all participants or effect estimates and standard errors. Where proper account has not been taken of data clustering, we will correct for this by inflating the standard error by the square root of the design effect.[63] Where intra-cluster correlation coefficients are not reported, we will contact authors to request this information or impute one, based on values reported in other studies. Where imputation is necessary, we will undertake sensitivity analyses to assess the impact of a range of possible values. In other instances of missing data (such as missing population information), it may not be possible to include a study in a particular analysis if, for example, it is impossible to classify the population using our equity tool.

We will use the GRADE approach as described in the Cochrane Handbook for Systematic Reviews of Interventions to present the quality of evidence and 'Summary of findings' tables. The downgrading of the quality of a body of evidence for a specific outcome will be based on five factors: limitations of study; indirectness of evidence; inconsistency of results; precision of results; and publication bias. The GRADE approach specifies four levels of quality (high, moderate, low and very low). If sufficient studies are found, we will draw funnel plots to assess the presence of possible publication bias (trial effect versus standard error). While funnel plot asymmetry may indicate publication bias, this can be misleading with a small number of studies. We will discuss possible explanations for any asymmetry in the review in light of our number of included studies. We will assess the impact of risk of bias in the included studies via restricting analyses to studies deemed to be at low risk of selection bias, performance bias and attrition bias.

Next, we will synthesise economic evidence on interventions (RQ4). Measures of costs and indirect resource use and cost-effectiveness will be summarised using tables. Where information is available, the tables will be presented by time horizon so that both the short and longer-term economic effects can be identified. If measures of resource use are judged sufficiently homogeneous across studies, these will be synthesised using statistical meta-analysis.[54] Measures of costs, indirect resource use and cost-effectiveness will be adjusted for currency and inflation to the current UK context. These data will be used to inform a narrative synthesis of economic analyses and applicability to the UK context. We do not intend to perform de novo economic modelling since the identified interventions and their outcomes are likely to be diverse.

Finally, we will undertake further work to examine mediation and moderation. Mediation analyses will involve a narrative synthesis reporting whether, within studies, measures of student commitment to school appear to mediate intervention effects on student violence, substance use or educational attainment outcomes. Moderation analyses will examine what factors relating to setting and population moderate intervention effects within and between studies. To examine within-study moderation, we will narratively synthesise evidence from relevant sub-group analyses conducted within primary studies to explore what subgroup characteristics explain heterogeneity of effects within studies, assessing whether interactions are significant. To examine between-study moderation, we will use meta-regression to examine what factors related to setting and population influence intervention effectiveness if at least ten studies are found.[64, 65] It may not be feasible to apply this method if we judge there are too many confounders or insufficient data, or if meta-regression is unable to account for interdependencies in complex interventions. Hence, if necessary we will complement meta-regression with qualitative comparative analysis, adapted for use in research synthesis[66, 67] to assess necessary and sufficient conditions related to setting and

population for intervention effectiveness. We should stress that meta-regression and qualitative comparative analysis will be exploratory, hypothesis-building analyses since these will draw on observational rather than experimental comparisons.

Socioeconomic position and inequalities

Socioeconomic status (SES) and health inequalities are central to the research. We will examine whether whole-school interventions promoting student commitment to school to prevent substance misuse and violence lead to reductions in health inequalities in these outcomes via our analyses of moderation by setting and population (RQ5). Where data allow, we will examine whether participant age, SES, sex and ethnicity, baseline risk behaviours and school-level deprivation moderate effects.

Patient and public involvement

We have consulted very widely with the UK and international health and education sectors in developing this proposal. Consultation with Professor George Patton and colleagues on the Lancet commission on adolescent health (which includes young people and global policy-makers such as the World Health Organisation) has informed our continuing interest in the quality of the school environment as a social determinant of health and our new focus on intervention that promote student commitment to school and on synthesising educational outcomes and economic evidence. This is in order to provide fuller information on multi-sectoral ‘best buys’ for this approach. Consultation with Public Health England confirmed the importance of further evidence synthesis, particularly in terms of understanding the precise types of intervention that are effective, as well as evidence on educational outcomes and cost-effectiveness. Consultation with the Department for Education and the Educational Endowment Foundation (EEF) indicated their interest in assessing whether whole-school intervention may bring a range of benefits including for educational attainment. It also encouraged our synthesis of evidence on moderation of intervention effects by socio-economic status given the EEF’s interest in this area. Consultation with the Healthy Schools London programme suggested an appetite on the part of those aiming to promote health on the ground to gain access to up to date evidence on the effectiveness of whole-school interventions as a strategy to promote health.

In the course of the project, we will institute a policy and practice stakeholder group (including representatives from Public Health England; Department of Health; Department for Education, Association for Young People’s Health, Healthy Schools London, Education Endowment Foundation; and the National Union of Teachers), as well as consulting with a separate group of young people under the auspices of the ALPHA DECIPHer group of young researchers. Both groups will meet twice during the review. First, each group will review results of our syntheses of intervention descriptions and process evaluations to inform any refinement of these and of our hypotheses prior to synthesis of outcome and economic evaluations. Second, each group will review the syntheses of outcome and economic evaluations to inform any refinements to the analysis and drafting of the report. At this stage, the groups will consider and advise us whether the evidence of effectiveness and cost-effectiveness overall and by subgroups suggested that it would be worth investing in the development of a new intervention to be evaluated in the UK. We anticipate that were the review to inform a further proposal for intervention development or optimisation these organisations would continue to collaborate, with some being involved in co-production and piloting. In addition, the knowledge transfer event organised in collaboration with the Association for Young People’s Health will involve policy-makers, practitioners and young people who will advise on our knowledge transfer strategy.

Knowledge exchange and translation

Our aim is to provide research outputs which provide rigorous evidence on how whole-school interventions to promote student commitment to school might be used to reduce student violence and substance use, and improve educational attainment. We will produce two reports: a

full technical report for NIHR and a briefing report for policy and practice audiences. The research will be launched at an event organised by the Association for Young People's Health, drawing on this organisation's broad networks of policy-maker, practitioner and youth participation to advise on knowledge transfer strategies. In addition, we will disseminate the research via open-access international scientific journals, and via academic and policy conferences. We will undertake seminars involving representatives of the UK and devolved national government departments of health and education to present the research to discuss policy implications and next steps. We will also use stakeholder and academic networks to support dissemination as well as existing web and social media platforms.

Research governance and ethics

The principal investigator is responsible for the conduct and delivery of the work. The sponsor of the research is Professor Kara Hanson, Dean of the Faculty of Public Health and Policy, London School of Hygiene and Tropical Medicine. The co-applicants will form an investigator committee which will meet monthly throughout the project, overseeing its conduct. These meetings will be minuted to keep a record of tasks, deadlines and responsibilities. The research involves no human participants and draws solely on evidence already in the public realm, so RAS approval is not required. Review and approval by the London School of Hygiene and Tropical Medicine research ethics committee will be sought. The team will follow relevant guidelines and best practice including the Social Research Association's (SRA) ethical guidelines[68] and refer also to guidance recommended by the National Coordinating Centre for Public Engagement.[69]

Expertise

Professor Chris Bonell (LSHTM) will direct the review overseeing all stages and components. He is experienced in leading systematic reviews including three previous reviews funded by the NIHR Public Health Research programme. He is also experienced in primary research on whole-school interventions to promote health. Dr G.J. Melendez-Torres (Cardiff University) is an experienced systematic reviewer and social epidemiologist with ongoing involvement in several evidence synthesis projects. He will lead quantitative analyses. Ruth Ponsford (LSHTM) is a research fellow with experience in mixed-method research on school health who will manage the research day-to-day. Dr Alec Miners (LSHTM) is a health economist who will examine the evidence and advise on appropriate methods of quality appraisal and synthesis for the economic evaluations.

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