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Addendum November 2021

An Addendum has been added to this protocol: Rapid evidence assessment on attendance interventions for school-aged pupils. The changes include:

1. A complete data extraction tool, which can be found in Appendix 1.
2. Appraisal of included studies - a detailed overview of the risk of bias assessment has now been added to the protocol, which can be found in Appendix 2.

Please note, these updates have been published prior to starting data extraction.

Background and review rationale

Poor school attendance is a significant problem in the UK and many other countries across the world. In 2019/20, it was reported as 4.9% overall, with special schools showing a higher rate equal to 10.5% and persistent absence at 13.1% in England (gov.uk 2020). Research has found that poor attendance is linked to poor academic attainment across all stages (Balfanz & Byrnes, 2012; London et al., 2016) as well as anti-social characteristics, delinquent activity and negative behavioural outcomes (Gottfried, 2014; Baker, Sigmon, & Nugent, 2001).

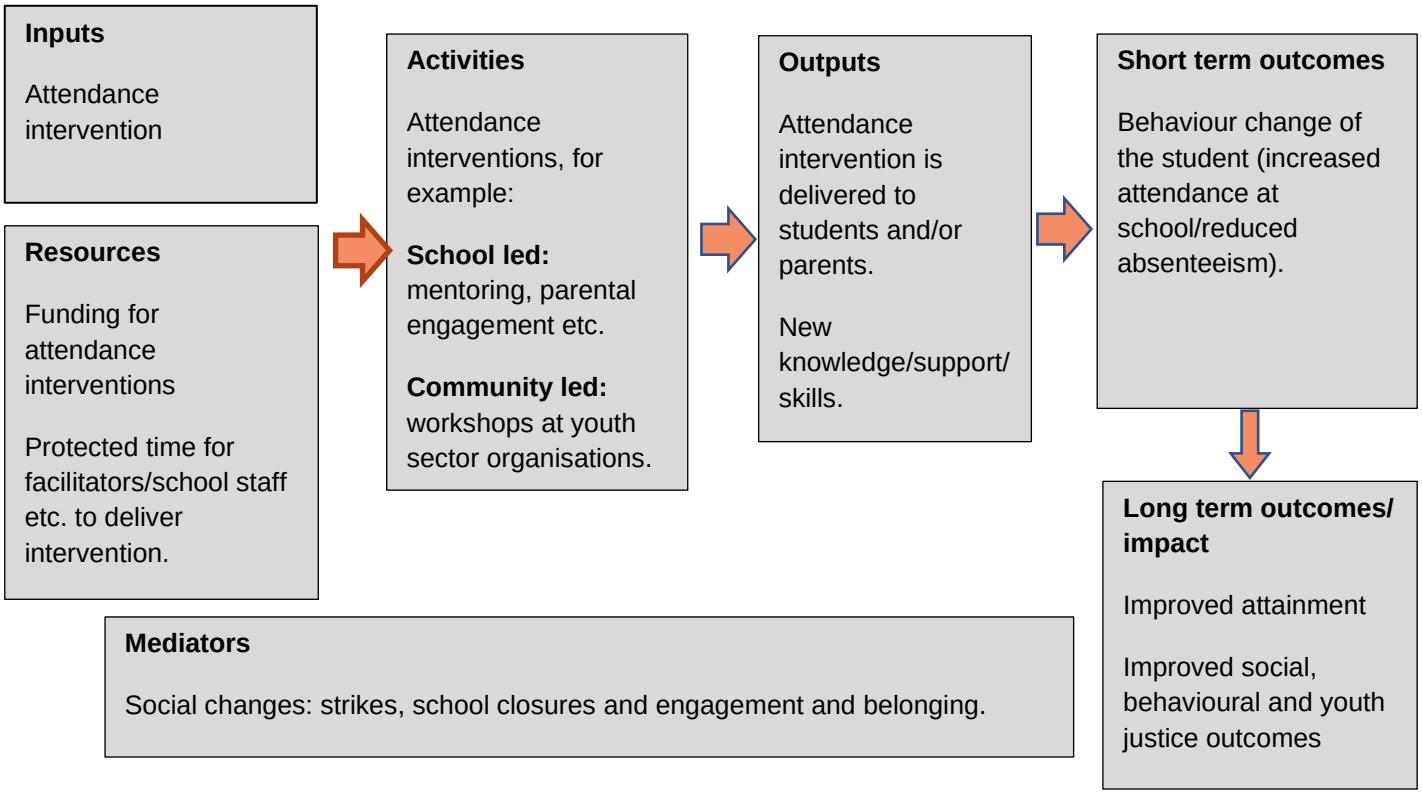
However, evidence suggests that small improvements in attendance can lead to meaningful impacts for these outcomes.

Several previous systematic reviews have addressed interventions for students who are chronically absent or truant and whole-school approaches. Maynard et al. (2012) examined empirical studies focused on improving attendance for chronically truant students. Sutphen et al. (2010) summarised promising truancy interventions and Freeman et al. (2019) summarised empirical research related to improving attendance or reducing tardiness in high schools.

While these reviews focused on specific areas of attendance and absence, this review aims to examine the evidence base to provide an overall picture of interventions that are being used to address attendance problems among school-aged children. This can include both overall absences as well as unauthorised absences. This review aims to be the basis for a report that provides an overview on the effectiveness of interventions on school attendance behaviours and the characteristics of these interventions.

We produced a simple theory of change (figure 1) in order to inform our inclusion criteria and extract the key elements of attendance interventions relevant for our purposes. While improvements in attendance can have long-term impacts on attainment and social and behavioural outcomes, we will be focusing primarily on activities, outputs and short-term outcomes, with scope to examine longer-term outcomes if these are explicitly included in our studies of interest.

Figure 1: Theory of Change



The aim of this review is to summarise the effects of interventions on school attendance behaviours, particularly the characteristics of these interventions and where evidence is available, examine which attendance intervention approaches are most likely to improve attendance among school-aged children. We will also look for evidence on characteristics of effective implementation of interventions that aim to improve school attendance. The findings will help inform wider thinking about attendance, exclusions and youth justice, particularly funding new programmes and interventions in this area in collaboration with the Youth Endowment Fund (YEF).

Research questions

We propose the following primary research question:

1. Do interventions that aim to increase pupil school attendance affect attendance behaviours of school-aged pupils?

In addition, we propose the following supplementary questions:

2. What are the common elements of interventions that improve primary and secondary student attendance?
3. Are certain types of interventions (e.g., school-based, community-based) more effective at improving primary and secondary student attendance?
4. What are the barriers and facilitators to effective implementation of attendance interventions?

5. Do studies examine the extent to which improvements in attendance act as a mediating variable for attainment and behavioural outcomes? If so, what are these outcomes? (e.g., substance misuse, bullying perpetration and victimisation, mental health and wellbeing)

Methodology

This review will be conducted in a short timescale and so we will undertake a rapid evidence assessment (REA) of the area. For this REA, we will draw on the Cochrane Rapid Reviews Methods Group's (RRMG) guidance for conducting rapid reviews (Garrity et al. 2021) and the Civil Service REA methodological guidance (Government Social Research Service, 2009). The scope of this review has been informed by the research questions, resources, and the timeframe. The following criteria will be used to determine whether a study will be included in the review.

Inclusion and exclusion criteria for the review

	Include	Exclude
Study design	This study will include RCTs and quasi-experimental evaluations of interventions that aim to increase school attendance. Meta-analyses or systematic reviews will be drawn upon to look for individual studies that fit the inclusion criteria.	Single group pre/post-test studies, qualitative studies and narrative, non-systematic reviews.
Population	School-aged children – Primary and secondary school settings including alternative provision and special schools. Pupils that have an identified attendance problem or are truant and those who do not but receive a whole-school intervention.	Children outside of primary or secondary school age. Early childcare settings, post-16 education, Higher Education.
Types of interventions	Interventions with a stated primary goal of increasing student attendance (or decreasing absenteeism) among primary or secondary school students. The intervention can take place in any format (e.g., face-to-face, online, one-off, or multiple sessions) and can be targeted for specific pupils or for whole-school and be community based. Some examples of included interventions are: • Mentoring sessions	Court-based interventions.

	• Parental engagement workshops • Additional staff support • Peer tutoring	
Comparison	No treatment, or business as usual or another treatment, e.g., comparison of two attendance interventions.	Studies that do not include a comparison group.
Outcome measures	The study must report on a measure of pupil attendance or absenteeism. If these studies collect other data on pupil attainment, engagement, and behavioural outcomes, we will extract and analyse this information.	Studies that do not include pupil attendance as an outcome.
Other criteria	Published since 2000 Published in English Studies conducted in the United States, Canada, the United Kingdom, and Australia Journals or grey literature	Published before 2000 Published in languages other than English

Study design

We will include any studies utilising randomised controlled trial (RCT) or quasi-experimental designs (QED) with a comparison group that received no treatment or treatment as usual. We will exclude any study outside of this methodological approach, for example single group pre-post-test design. A RCT is a study where participants are randomly assigned to be in one of 2 (or more) groups to test a specific intervention. One group (the experimental group) has the intervention being tested while the other (the comparison or control group) has an alternative intervention or no intervention. The groups are followed to monitor the effectiveness of the intervention. We will include RCTs with assignment at individual, household, community, or school level. A QED is a study used to estimate the impact of an intervention on a target population with non-random assignment of those groups. QED studies vary in approaches and designs and are often given different names. However, through the scoping period we don't anticipate large numbers of studies in this area and therefore, for the purpose of screeners we will include all the following designs:

- Non-randomized studies with selection on unobservables:
    - Regression discontinuity designs, where assignment was done on a threshold measured at pre-test, and the study used prospective or retrospective approaches of analysis to control for unobservable confounding.

    - Studies using design or methods to control for unobservable confounding, such as natural experiments with clearly defined intervention and comparison groups, which exploit natural randomness in implementation assignment by decision makers (e.g., public lottery) or random errors in implementation, and instrumental variables estimation.

- Non-randomized studies with pre-intervention and post-intervention outcomes data in intervention and comparisons groups, where data were individual level panel or pseudo-panels (repeated cross-sections), which used the following methods to control for confounding:

    - Studies controlling for time-invariant unobservable confounding, including difference-in-differences, or fixed- or random-effects models with an interaction term between time and intervention for pre-intervention and postintervention observations.

    - Studies assessing changes in trends in outcomes over a series of time points (interrupted time series, ITS), with or without contemporaneous comparison (controlled ITS), with sufficient observations to establish a trend and control for effects on outcomes due to factors other than the intervention.

- Non-randomized studies with control for observable confounding, including nonparametric approaches (e.g., statistical matching, covariate matching, coarsened exact matching, propensity score matching) and parametric approaches (e.g. propensity-weighted multiple regression analysis).

If we unexpectedly do identify a large number of QEDs, we will consider refining the definition. We will only include those RCTs and QEDs that address the effectiveness of attendance interventions and measure attendance as an outcome measure. We will also draw on information on implementation in the included RCTs and QEDs to answer question 5, rather than exploring broader literature.

Population

We will include only those RCTs and QEDs that use school-aged pupils in their population sample, that means pupil who attend primary and secondary schools (terminology may differ in those studies conducted in countries outside of England). Since, the review aims to capture both whole-school and targeted interventions, pupils who have significant attendance issues and those who may not but are involved in whole-school approaches will be included. The primary inclusion criteria will be interventions that aim to increase attendance among school-aged children.

Types of interventions

We will include any type of intervention that includes school-aged pupils and aims to increase attendance. These interventions can be both school-based such as mentoring and workshops or community-based such as programmes that take place in local youth sector organisations.

Outcomes

We will include studies that report on a measure of pupil attendance or decreasing absenteeism. If these studies also report on other outcomes such as attainment, engagement and behavioural outcomes, these will be extracted and analysed.

Other criteria

Adopting the approach of Maynard et el. (2012) and due to differences in educational systems, this review will only include those studies conducted in the United States, Canada, the United Kingdom, and Australia. They will be written in English and published post 2000. We will include studies published in journal articles or in grey literature.

We will include any follow-up duration, coding multiple outcomes where studies report multiple follow-ups.

Search strategy for identification of studies

Search systems and databases to be searched

Searches will be conducted using a combination of search systems and bibliographic databases, including ERIC, PsychInfo and Google Scholar, and hand searches of known sources of systematic reviews such as the Campbell Library. We will also screen studies for inclusion from known existing systematic reviews of attendance interventions (Maynard et al. 2012, Sutphen el al. 2010 and Freeman et al. 2019).

Search Systems and databases to be searched:

- ERIC
- PsychInfo
- Google Scholar[1]
- Web of Science

Other sources:

- Review of Education Research: https://journals.sagepub.com/home/rer
- Education Research Review: https://www.journals.elsevier.com/educational-research-review
- EEF Teaching and Learning Toolkit
- YEF Evidence and Gap Map: https://youthendowmentfund.org.uk/evidence-and-gap-map/
- EIF Guidebook: https://guidebook.eif.org.uk/

Both toolkit resources are based on systematic reviews as well as including grey literature, adding to the comprehensiveness of the search for this rapid evidence assessment.

Search terms

[1] Google scholar has a 256 character limit and does not automatically search for truncations. We will look at the first 200 results in Google Scholar, in line with the recommendation of Haddaway et al. 2015.

We have drawn on the search terms used in the Maynard et al. (2012), Sutphen et al. (2010) and Freeman et al. (2019) reviews, combined with new search terms to cover the wider scope of this current review. The terms will be used to search on titles and abstracts and adapted as necessary depending on the search functions of the search systems and databases.

Within the review timeframe we are working towards, we propose to limit the number of unique combinations of search terms to ensure adequate time to conduct the searches across peer reviewed and non-peer reviewed sources and review the search results to select material for inclusion. To this end, we propose the use of search strings that combine multiple terms and operators (e.g., AND, OR, and wildcards). Where it is possible to refine searches using filters such as categories on web of science, we will exclude categories that are not related to education and attendance. Where filters on sites correspond to inclusion criteria we will also filter during the search – for example, only searching studies published from 2000.

The table below lists the proposed search strings. Synonymous terms are grouped within parentheses and separated using 'OR'. Variants of words can be searched using wildcards, e.g., 'Evaluation*' will include 'evaluation', 'evaluate' and 'evaluations. If searches return too few or too many results, search parameters can be adjusted as necessary to reduce results to a sufficient or more manageable number, e.g., by adding exclusion terms such as NOT. These search strings and operators work in both academic databases and Google search. As per PRISMA recommendations, we will record the numbers of search results, the criteria applied for filtering, and the number of excluded articles. These will be reported as a flow diagram (see PRISMA example here: http://prisma-statement.org/PRISMAStatement/FlowDiagram).

Category	Search terms
Targeted population AND	"High School" OR "Secondary school" OR "Primary School" OR "Elementary School" OR "Students" OR "Pupils" OR "Schools"
Intervention AND	"Evaluation" OR "Intervention" OR "Program" OR "Policy" OR "support" OR "Treatment" OR "Outcome" OR "Mentoring" OR "Parental engagement"
Targeted behaviour/outcome	"Attendance" OR "Absence" OR "Truancy" OR "Absenteeism"

Selection of studies

The results of the search will be imported into EPPI reviewer and duplicates removed. Each search result will be screened twice, first on abstract and title only, then if needed, on the full text. After initial calibration, each screening stage will be completed by one reviewer only

due to the timeline for this project. However, we will take a "safety first" approach at both screening stages (Shemilt et al., 2016); that is, the reviewer will have the option of marking a search result as unclear for review by a second reviewer.

At the title and abstract and full-text stage, every reviewer will begin by screening the same 30 search results. The results of this screening will be compared to ensure that the inclusion and exclusion criteria are being interpreted and applied in the same way. The priority screening tool within EPPI-reviewer (Thomas et al., 2010) will be used for title and abstract screening to order results by probability of inclusion and stop screening once we reach a certain point when relevant studies are no longer being identified. The priority screening function orders the results based on the words in the title and abstract of the included and excluded papers from a training set of screening. It does this using machine learning text mining technology. We will screen a random set of 10 percent of the search results as the training set. Reviewers will stop screening after 100 studies are rejected in a row using the tool. As a check on this approach, we will randomly sample 30 of the unscreened titles to see if this approach has missed any relevant studies.

The results of this process will be documented using a PRISMA-style flow chart generated from EPPI-reviewer.

Data extraction and management

We will systematically extract data in EPPI-reviewer web[2] using a data extraction tool. The tool can be found in Appendix 1. We will follow a similar approach to the Maynard et al. (2012) review and code on: 1) study descriptors 2) sample descriptors 3) intervention descriptors using Tidier[3] 4) Risk of bias 5) Outcome and 6) effect size data[4]. We will extract descriptive data about the type of intervention, and the comparator (that is- whether the participants who were usually getting 'business as usual' may have been receiving some other form of help), duration, method of delivery, reach, attrition figures, outcomes measured by the study, description of the effect sizes and any information about implementation of the attendance intervention. A team will be responsible for extracting information from the included studies using the data extraction tool. The core team will do double data extraction on 20% of studies (randomly selected).

Update November 2021: Addendum has been added in Appendix 1.

Appraisal of included studies

Due to the quick turnaround of the rapid evidence assessment, we will not undertake a full risk of bias assessment. We will use an adapted approach of the Cochrane tool outlined in their handbook for systematic reviews of interventions[5], to assess how much confidence to place in the findings from the included RCTs or QEDS. An addendum will be added to the protocol prior to the start of data extraction.

[2] *Update November 2021 – in protocol version 1 data extraction was specified as taking place in Excel – this has been revised, as the team will be conducting data extraction in EPPI-reviewer.*
[3] Better reporting of interventions: template for intervention description and replication (TIDieR) checklist and guide; BMJ 2014;348:g1687
[4] The sections have been updated in November 2021
[5] https://methods.cochrane.org/bias/resources/cochrane-handbook

Update November 2021: Addendum has been added in Appendix 2.

Data synthesis

In the main, we will undertake a narrative synthesis of the included studies to answer the review questions, but where appropriate we will aim to do a meta-analysis where studies are sufficiently similar based on the theory of change, presenting effect sizes and associated measures of uncertainty where studies present the necessary information to calculate effect sizes. The intervention outcome of interest for this review is attendance, which is likely to be reported as a continuous variable. That is, reported in terms of mean number of days attended or absent, mean number of classes absent, or mean percentage of days attended or absent. Therefore, we will use this to calculate standardised effect sizes.

There might be studies with multiple treatment arms with only one control group and several attendance outcomes are presented for the same study. We will not extract all effect sizes, and only include one effect estimate per study in our meta-analysis. Where we have several publications reporting on the same study we will use effect sizes from the key publication. For studies with outcome measures at different time points, we will synthesis on the outcome measured closest upon completion of the intervention.

To answer question 2 and 4, we will extract qualitative and quantitative information where available in the study on barriers and facilitators to effective implementation of attendance intervention. We will use thematic analysis to synthesise the results. Thomas and Harden (2008) outline the three stages of thematic synthesis: the coding of text ‘line-by-line’; the development of descriptive themes and the generation of ‘analytical themes’. Due to the tight turnaround of this review, we will not code line-by-line but extract the factors that seem to be barriers or facilitators or appear to mediate the effects from studies into descriptive themes. While the aim of developing descriptive themes remains close to the primary studies, the analytical themes is a stage where we will interpret and synthesise findings from all the studies.

To answer question 3, about whether certain types of interventions are more effective at improving attendance, we will use regression analysis to compare intervention type for moderating effects.

To answer question 5, we will extract data on other outcomes, if presented, for example academic attainment and behavioural outcomes (e.g., substance misuse, bullying perpetration and mental health and wellbeing).

Overall, this analysis will be supported by the data synthesis process that will also cover descriptive analysis by summarising characteristics of interventions and included studies. This will entail synthesising sample size, attrition, types of interventions, duration of intervention, setting of intervention delivery, participant characteristics and effect size of attendance.

Reporting

The technical report will use the EEF review reporting template for evidence reviews.

A school facing publication will summarise the evidence for different interventions that aim to improve attendance among pupils, describing the impact and implementation challenges and specific characteristics of those that appear to be effective programmes.

Peer review

This REA protocol will be peer reviewed by one reviewer. The REA will be peer reviewed by two peer reviewers.

Personnel

Core team:

- Jonathon Kay
- Rupal Patel
- Hannah Blausten
- Harry Madgwick

Conflicts of interest

No conflict of interest

Timeline

	Task	Completion date
Protocol Development	Peer review protocol	10th September
	Finalise protocol	20th September
	Data extraction tool finalised	20th September
Search	Academic search	15th September
	Search grey literature and organisational websites	17th September
	Citation tracking (checking included studies in the relevant systematic reviews and meta-analyses	17th September
Screening	Screening at title (and abstract)	1st October
	Full text retrieval	6th October
	Full text screening	29th October
Data Extraction	Data extraction (descriptives, intervention, implementation, effect sizes), assuming 10 per day	19th November
	Check data extraction	23rd November
	Data extraction (critical appraisal of studies)	29th November

	Task	Completion date
Synthesis and write-up	Narrative synthesis	6th December
	Write up of new synthesis	13th December
	Review of findings by senior team	20th December
	Write up draft technical report using REA template	End of December
Peer Review	Select and approach two peer reviewers	20th December
	Send to peer reviewer	5th January
	Peer reviews complete	14th January
	Integrate peer review comments	21st January
	Publish report	February
Supportive resources for schools	Summary document	End of Jan
	Feedback from wider team	February
	Finalise school facing findings document	February

Appendix 1

Data extraction tool

Q	Question	Codes
Study descriptors		
	Type of publication	Journal article, Dissertation or thesis, Technical report, Book or book chapter, Conference paper, Other (please specify)
Sample descriptors		
	Sample population age	Tick box (in pupil ages rather than school years)
	Intervention sample size (at analysis)	Numerical value
	Comparison group sample size (at analysis)	Numerical value
	What is the proportion of low SES/FSM students in the sample?	FSM or low SES percentage, Further information about FSM or SES in the study sample, No SES/FSM information provided
Intervention descriptors		
	Country in which intervention was implemented	United States, Canada, England, Scotland, Wales, Northern Island and Australia
	Name of programme/intervention	Write the name of the programme/intervention
	Describe any rationale, theory, or goal of the elements essential to the intervention	Open response
	Description of intervention	Open response
	Year intervention started	State year
	Where was the intervention delivered?	Primary school, Secondary school, community centre, virtually, other (please specify)
	When did the intervention take place?	During regular school hours, before/after school, evenings and/or weekends, summer/holiday period, other (please specify), Unclear/not specified
	Was training for the intervention provided?	Yes (please specify), No, Unclear/Not specified

	What is the intervention delivery approach?	Whole school, Whole class, Large group (+6), Small group (3-5), One to one, Peer to Peer, Student alone (self-administered), Other (please specify)
	Length of individual intervention session	Less than an hour, one- hour, half a day, one-day, other (please specify)
	Frequency of intervention	One-off, fortnightly, weekly, two times a week, daily, other (please specify)
	Overall duration of intervention	One day, one week, two to three weeks, 1 month, 1 month to 3 months, 3 months to 6 months, 6 months to 1 year, more than 1 year
	Type of intervention (select as many as apply)	Mentoring, Parental engagement, Peer support, academic, cognitive skills training, behavioural interventions, extracurricular activities, Counselling, Social Work or other Therapeutic intervention (individual), Social Work or other Therapeutic intervention (group), social and emotional learning, breakfast clubs, Incentives/rewards schemes, other (please specify)
	Person(s) providing the intervention	Not stated/unclear, class teachers, external teachers, social worker, teaching assistants, other school staff, parents/carers, volunteers, peers, research staff, digital technology, other (describe)
	Educational setting	Primary/elementary, Middle school, Secondary/high school, Residential/boarding school, Independent/private school, Home, alternative provision, Other educational setting (please specify)
	Are the costs reported?	Yes (please specify), No
Risk of bias		
	Allocation bias - Type of allocation	Random, non-random studies with selection on unobservables, Non-random studies with pre/post intervention outcome data, Non-random with control for observable confounding, not assigned.
	Confounding: (Was the method of analysis executed adequately to ensure the comparability of groups throughout the study and prevent confounding?)	Yes, Probably Yes, Probably No, No, Unclear
	Overall allocation bias	High, some concerns, low
	Was an appropriate analysis used to estimate the effect of assignment to intervention?	Low risk, High risk, Unclear
	Attrition bias	Low risk, some concerns, high risk
	Overall RoB assessment	High risk of bias, some concerns, low risk of bias
Research methods		

	What is the level of assignment?	Individual, class, school - cluster, school - multi-site, region/district, not provided/not available
Outcomes		
	Outcome on attendance	
	What is the comparison?	No treatment, treatment as usual, another treatment, other (specify)
	Timing of post-intervention data collection	Number of days/weeks/months after intervention or NA if unknown
	Type of outcome (attendance)	Days absent, total attendance, persistent absence classifier
	Other outcomes?	Academic outcomes, school exclusions. Criminal justice outcomes, substance abuse, behaviour (other), other (please specify)
	What is the level of assignment?	Individual, class, school - cluster, school - multi-site, region/district, not provided/not available
Effect Size data		
	Attendance outcome measure (Outcome description in EPPI-reviewer)*	Open response (How the study has monitored/measured attendance rates)
	Is there more than one treatment group?	Yes (please specify), No, Not specified/N/A
	Standard error	Numerical value
	Standard deviation	Numerical value
	Confidence interval lower	Numerical value
	Confidence interval upper	Numerical value
	Effect Size measure	Numerical value
Qualitative information		
	Is there a process evaluation	Yes/No
	Facilitators to implementation	Open response, no detail
	Barriers to implementation	Open response, no detail
	Other notes	Open response

Appendix 2: Risk of bias

There are no consistent guidelines or standard methodologies for rapid reviews One of the common shortcuts applied to rapid reviews is to either undertake a light-touch or no risk of bias assessment (Haby et al. 2016). Risk of bias assessments are generally underutilised in education – a recently conducted review of systematic reviews found that fewer than 10% conducted full risk of bias assessments. Despite this, an understanding of study quality is an important factor in both practitioner facing recommendations and funding decisions.

In designing the risk of bias assessment for this review, the team have attempted to balance an approach that will identify key threats to validity in the underlying studies, with an assessment approach that is possible to deliver within the timeline of a rapid review.

The domains from the Cochrane Risk of Bias 2 tool (Higgins et. al. 2016) have been assessed and adapted. The domains are listed and discussed below. Domains have been omitted where RoB assessment was unlikely to differentiate between studies or where assessing the risk of bias was unfeasible in the timelines of a rapid review.

Cochrane Risk of Bias 2 domains:

Domain 1: Risk of bias arising from the randomization process

We capture risk of bias around allocation through extracting information on the method of assigning participants, and the comparability of groups after allocation.

Risk of bias questions:

- How were the participants assigned?
- Was the method of analysis executed adequately to ensure the comparability of groups throughout the study and prevent confounding?
- Risk of bias for allocation? [High/Some concerns/Low]

Domain 2: Risk of bias due to deviations from the intended interventions (effect of assignment to intervention/effect of adhering to intervention)

In the education studies participants cannot be blinded to the intervention. A question has been included that captures whether appropriate analysis has been used to capture deviations from intended intervention (i.e. intention to treat analysis).

Domain 3: Missing outcome data

We assess whether the method of analysis was adequately executed to ensure the comparability of groups and prevent confounding.

Domain 4: Risk of bias in measurement of the outcome

Given the outcome of interest is attendance, which is routinely collected through school administrative data, the rapid review has not included a separate risk of bias assessment on the basis of outcome measurement. This domain was therefore omitted.

No risk of bias questions included.

Domain 5: Risk of bias in selection of the reported result

While there is a risk of bias from selective reporting, the rapid nature of this review mean that it is not feasible to identify and review protocols to make a comprehensive risk of bias assessment. The data extraction tool does capture the independence of the evaluation team. It is a limitation in the overall assessment of risk of bias that will be highlighted in the final report. The

Light touch risk of bias tool for REA

Domain 1: Risk of bias arising from the allocation process		
How were the participants assigned?	Random, non-random studies with selection on unobservables, Non-random studies with pre/post intervention outcome data, Non-random with control for observable confounding, not assigned but matched, non-random not matched prior to treatment, unclear, not assigned.	How were the participants assigned or allocated to their group (i.e. treatment and control)? ***Random:*** Select this code where the report describes the participants' allocation to their group as random or pseudo-random (computer generated). Please highlight in the text or add information to the info box about the randomisation details. ***Non-random studies with selection on unobservables:*** - Regression discontinuity designs, where assignment was done on a threshold measured at pre-test, and the study used prospective or retrospective approaches of analysis to control for unobservable confounding. - Studies using design or methods to control for unobservable confounding, such as natural experiments with clearly defined intervention and comparison groups, which exploit natural randomness in implementation assignment by decision makers (e.g., public lottery) or random errors in implementation, and instrumental variables estimation. ***Non-random studies with pre/post intervention outcome data:*** - Studies controlling for time-invariant unobservable confounding, including difference-in-differences, or fixed- or random-effects models with an interaction term between time and intervention for pre-intervention and postintervention observations. - Studies assessing changes in trends in outcomes over a series of time points (interrupted time series, ITS), with or without contemporaneous comparison (controlled ITS), with sufficient observations to establish a trend and control for effects on outcomes due to factors other than the intervention. ***Non-random with control for observable confounding***: - including nonparametric approaches (e.g., statistical matching, covariate matching, coarsened exact matching, propensity score matching) and parametric approaches (e.g. propensity-weighted multiple regression analysis). **Unclear** [Selectable (show checkbox)]

		Please only select this code if there are no details about control and intervention allocation or if the information is so unclear as to prevent a reasonable inference. ***Not assigned - naturally occurring sample:*** This is where researchers take advantage of a situation where a comparison can be made between groups from changes that either are planned or have already happened which will give and estimate of the impact of the intervention or approach of interest.
Confounding: (Was the method of analysis executed adequately to ensure the comparability of groups throughout the study and prevent confounding?)	Yes, Probably Yes, Probably No, No, Unclear	Select appropriate category Notes: **a) Baseline characteristics are similar in magnitude;** **b) Unbalanced covariates at the individual and cluster level are controlled in adjusted analysis;** Score “Yes” if criterion a) and b) are satisfied; - Score "Probably yes" if a) is not satisfied but b) is satisfied and imbalances are small in magnitude OR if only a) is satisfied. -Score “Unclear” if no balance table is provided or if imbalances are controlled for but they are very large in magnitude and assignment mechanism is not coded as "Yes" or "Probably yes". -Score "Probably no" if a) and b) are not satisfied and the magnitude of imbalances are small. -Score “No” if a) and b) are not satisfied and the magnitude of imbalances are large and covariates are clear determinant of the outcomes.
Allocation risk of bias	Low risk of bias Some concerns High risk of bias	**Low risk of bias:** Allocation is random and Y or PY Non-random with selection on unobservables and Y Non-random with pre-post outcome data and Y **Some concerns:** Allocation is random and PN Non-random with selection on unobservables and PY or unclear Non-random with pre-post outcome data/control for observables and PY Naturally occurring sample and Y **High risk of bias:** Allocation is random and N Non-random with selection on unobservables and PN or N Non-random with pre-post outcome data/control for observables and unclear, PN or N Naturally occurring sample and PY, PN or N

Domain 2: Risk of bias due to deviations from the intended interventions (effect of assignment to intervention/effect of adhering to intervention)		
Was an appropriate analysis used to estimate the effect of assignment to the intervention?	High risk of bias Some concerns Low risk of bias	Score “high risk” if either naïve ‘per-protocol’ analyses (excluding trial participants who did not receive their assigned intervention) or ‘as treated’ analyses (in which trial participants are grouped according to the intervention that they received, rather than according to their assigned intervention) Score “low risk” if intention to treat analysis is used Score “unclear” if it is unclear what type of analysis is used.
Domain 3: Missing outcome data		
Selection bias	High risk of bias Some concerns Low risk of bias	Select appropriate category -Score "Low risk of bias" if there is less than 20 per cent attrition and the study establishes that attrition is randomly distributed -Score "Some concerns" if there is an attrition problem but no information provided on the relationship between attrition and treatment status or if there is attrition which is likely to be related to the intervention -Score "High risk of bias" if there is evidence of differential attrition affecting more than 20 per cent of the data.
Overall risk of bias judgement		
Overall judgement	High risk of bias Some concerns Low risk of bias	Select the category that corresponds with the highest risk in any of the domains (e.g. if any domains have high risk of bias this will be the overall rating).

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