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Do school closures and school reopenings affect community transmission of COVID-19? A systematic review of observational studies

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ABSTRACT

Objectives To systematically reivew the observational evidence of the effect of school closures and school reopenings on SARS-CoV-2 community transmission.
Setting Schools (including early years settings, primary schools and secondary schools).
Intervention School closures and reopenings.
Outcome measure Community transmission of SARS-CoV-2 (including any measure of community infections rate, hospital admissions or mortality attributed to COVID-19).
Methods On 7 January 2021, we searched PubMed, Web of Science, Scopus, CINAHL, the WHO Global COVID-19 Research Database, ERIC, the British Education Index, the Australian Education Index and Google, searching title and abstracts for terms related to SARS-CoV-2 AND terms related to schools or non-pharmaceutical interventions (NPIs). We used the Cochrane Risk of Bias In Non-randomised Studies of Interventions tool to evaluate bias.
Results We identified 7474 articles, of which 40 were included, with data from 150 countries. Of these, 32 studies assessed school closures and 11 examined reopenings. There was substantial heterogeneity between school closure studies, with half of the studies at lower risk of bias reporting reduced community transmission by up to 60% and half reporting null findings. The majority (n=3 out of 4) of school reopening studies at lower risk of bias reported no associated increases in transmission.
Conclusions School closure studies were at risk of confounding and collinearity from other non-pharmacological interventions implemented around the same time as school closures, and the effectiveness of closures remains uncertain. School reopenings, in areas of low transmission and with appropriate mitigation measures, were generally not accompanied by increasing community transmission. With such varied evidence on effectiveness, and the harmful effects, policymakers should take a measured approach before implementing school closures; and should look to reopen schools in times of low transmission, with appropriate mitigation measures.

Strengths and limitations of this study

- ► While the role of non-pharmaceutical interventions as a whole in limiting community spread of SARS-CoV-2 is beyond doubt, the specific role of school closures is less clear because of the smaller role that children play in transmission of the disease.
- ► This is the first systematic review of the empirical evidence from the COVID-19 pandemic of the effectiveness of school closures and reopenings on community transmission of SARS-CoV-2.
- ► We include data from 150 countries, investigating both school closures and school reopenings.
- ► We were unable to meta-analyse due to data heterogeneity.

INTRODUCTION

School closures have been a common strategy to control the spread of SARS-CoV-2 during the COVID-19 pandemic. By 2 April 2020, 172 nations had enacted full closures or partial 'dismissals', affecting nearly 1·5 billion children.[1] As cases of COVID-19 started to fall, many countries looked to reopen schools, often with significant mitigation measures in place.[2] Over the northern hemisphere winter of 2020–21, many countries again closed schools with the aim of controlling a resurgence of cases. School closures have substantial negative consequences for children's well-being and education, which will impact on life chances and long-term health.[3,4] Closures exacerbate existing inequalities, with greater impacts on children from socioeconomically deprived backgrounds because those from higher income families have better opportunities for remote learning.

The role of non-pharmaceutical interventions (NPIs) collectively in limiting community spread is established. However, the specific contribution of school closures remains unclear. Observational studies suggest that school-aged children, particularly teenagers, play a role in transmission to peers and bringing infection into households,[5] although the relative importance compared with adults remains unclear.[6] Younger children appear less susceptible to infection and may play a smaller role in community transmission, compared with older children and adults.[7] Although some modelling studies have suggested that school closures can reduce SARS-CoV-2 community transmission,[8] others disagree.[9 10]

A rapid systematic review published in April 2020 found a small number of studies of the effectiveness of school closures in controlling the spread of COVID-19.[11] However, this review was undertaken very early in the pandemic and included no observational data on SARS-CoV-2. Since then many studies on the effects of closing or reopening schools on SARS-CoV-2 community transmission have been published, but there has been no systematic review of these studies. A clearer understanding of the impact of school closures and reopenings on community transmission is essential to aid policymakers in deciding if and when to implement school closures in response to rising virus prevalence, and when it is prudent to reopen schools. Here, we synthesise the observational evidence of the impact of closing or reopening schools on community transmission of SARS-CoV-2.

METHODS

The study protocol for this systematic review is registered on PROSPERO (ID: CRD42020213699).

Inclusion and exclusion criteria

We included any empirical study which reported a quantitative estimate of the effect of school closure or reopening on community transmission of SARS-CoV-2. We considered 'school' to include early years settings (eg, nurseries or kindergartens), primary schools and secondary schools, but excluded further or higher education (eg, universities). Community transmission was defined as any measure of community infection rate, hospital admissions or mortality attributed to COVID-19. We included studies published in 2020 or 2021 only. We included preprints, peer-reviewed and grey literature. We did not apply any restriction on language, but all searches were undertaken in English. We excluded prospective modelling studies and studies in which the assessed outcome was exclusively transmission within the school environment rather than the wider community.

Search strategy

We searched PubMed, Web of Science, Scopus, CINAHL, the WHO Global COVID-19 Research Database (including medRxiv and SSRN), ERIC, the British Education Index and the Australian Education Index, searching title and abstracts for terms related to SARS-CoV-2 AND terms related to schools or NPIs. To search the grey literature, we searched Google. We also included papers identified through professional networks. Full details of the search strategy are included in online supplemental appendix A. Searches were undertaken first on 12 October 2020 and updated on 7 January 2021.

Data extraction and risk of bias assessment

Article titles and abstracts were imported into the Rayyan QCRI webtool.[12] Two reviewers independently screened titles and abstracts, retrieved full texts of potentially relevant articles and assessed eligibility for inclusion.

Two reviewers independently extracted data and assessed risk of bias. Data extraction was performed using a pre-agreed extraction template which collected information on publication type (peer-reviewed or preprint), country, study design, exposure type (school closure or reopening), setting type (primary or secondary), study period, unit of observation, confounders adjusted for, other NPIs in place, analysis method, outcome measure and findings. We used the Cochrane Risk of Bias In Non-randomised Studies of Interventions tool[13] to evaluate bias.

Discrepancies were resolved by discussion in the first instance and by a third reviewer where necessary.

Data synthesis

Given the heterogeneous nature of the studies, prohibiting meta-analysis, a narrative synthesis was conducted. Schools often reopened with significant COVID-19 infection prevention and control measures in place, meaning that the effect of lifting restrictions may have been different from the effect of imposing them. We therefore considered the studies of school closures and school reopenings separately. We also aimed to evaluate differential effects for primary and secondary schools if data allowed.

Patient and public involvement

There was no patient or public involvement in this study.

RESULTS

We identified 7474 studies (figure 1). After removing 2339 duplicates, 5135 unique records were screened for inclusion. We excluded 4842 records at the title or abstract stage, leaving 293 records for full-text review. Of these, 40[14–53] met the inclusion criteria.

Description of studies

Included studies are described in table 1, grouped by exposure type and study design. Of these, 32 studies[14 15 18–21 23 24 26 29–40 42–44 46–53] reported the effect of school closures on community transmission of SARS-CoV-2, 11[16 22–25 27 28 35 43–45] examined school reopening and 3[16 17 41] investigated the effect of school holidays. Some studies considered more than one exposure. All studies used data from national government sources or international data repositories. A total of 15

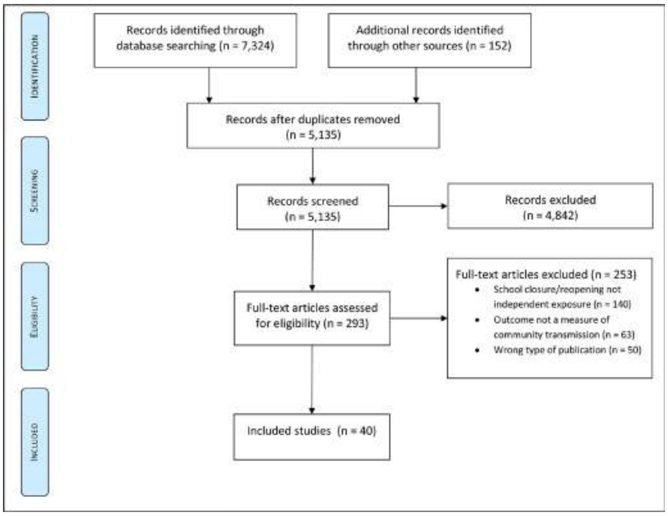

Figure 1 Preferred Reporting Items for Systematic Reviews and Meta-Analyses flow diagram.

studies were from peer-reviewed journals, while 24 studies were from preprint servers and 1 study was a conference abstract.

All studies were ecological in nature, that is, the unit of analysis was national or regional. Of the school closure studies, 13 reported data from a single country or region (the USA (n=10),[14 19–21 33 37 42 47–49] Italy (n=1),[23] Japan (n=1)[29] and Switzerland (n=1)[43]); 4 reported discrete estimates for several countries[26 38 44 53] and 15 studies pooled data from multiple countries (globally (n=8),[31 34–36 39 46 50 51] Europe only (n=2),[24 30] Europe and other high-income countries (n=5)[15 18 32 40 52]). The studies on school reopening generally reported on single countries (Germany (n=2),[22 28] USA (n=1),[25] Switzerland (n=1),[43] Belgium (n=1),[27] Israel (n=1),[45] Italy (n=1)[23]), but one reported discrete estimates for three countries (Denmark, Germany and Norway),[44] two pooled data from multiple countries globally[16 35] and one pooled data from multiple European countries.[24] Of the three school holiday studies, one reported on Germany,[41] one pooled data from 24 countries globally[16] and one pooled data from multiple European countries.[17]

The majority of studies (n=24) did not specify the type of school setting being studied. However, eight studies specified that they were reporting on primary and secondary schools only,[14 16 18 19 27 29 37 49] and six additionally include early years settings.[22–24 44 45 48] The two remaining studies used the date of primary school (n=1)[15] or secondary school (n=1)[43] closure as their exposure date, but did not indicate this was temporally distinct from closure of the other setting. Very few studies reported independent effect sizes for different setting types: two closure studies[24 48] and four reopening studies.[16 22 24 44]

Studies that specifically sought to estimate an effect of school closure policy on SARS-CoV-2 transmission included eight school closure studies,[14 23 29 32 37 38 42 44] six school reopening studies[22 23 25 28 44 45] and three school holiday studies. The remaining studies primarily sought to estimate the effect of NPIs (but reported an independent estimate for schools, alongside estimates for other NPIs within their analysis).

The studies used different analytic approaches: regression models (n=24),[14 17 19–21 25 26 28 30 31 33 35 36 39–42 44 46 48 49 51–53] Bayesian modelling (n=3),[15 18 47] comparison to a synthetic control group (n=4),[24 34 38 44] machine learning approaches (n=2),[43 50] time series analysis (n=1)[29] and visual representation of changes in transmission over time compared against the timing of school policy interventions, with or without formal statistical analysis (n=4).[16 22 37 45] We identified three study designs used to estimate the effect of school closures: pooled multiple-area before-after comparisons (n=22),[14 15 18–21 24 26 30 32–36 39 40 42 46–50] within-area before-after comparisons (n=7)[23 29 37 38 43 44 53] and pooled multiple-area comparisons of interventions in place at a fixed time point (n=3).[31 51 52]

In most instances of school closures, particularly in European countries, other NPIs were introduced at or around the same time. Some studies dealt with this at the design stage, choosing to study places where school closures were done in (relative) isolation[37] and some at the analytical stage (typically by undertaking regression and having multiple comparator countries). Some studies did not appear to have a mechanism in place to deal with this potential confounding.[32 40 44 52] Studies which pooled data from multiple areas also adjusted for other potential confounders, such as population factors (eg, proportion of population aged ≥65 years, population density) and SARS-CoV-2 testing regimes.

Among school closure studies, 18[14 15 19 20 24 26 29 31–34 37 39 42–44 50 51] reported effects on incidence, 11[14 19 21 30 38–40 42 46 52 53] on mortality, 1[37] on hospital admissions and mortality and 8[18 21 23 35 36 43 47 48] on an estimate of the effective Reproductive number (R) (derived from incidence and/or mortality data). Of the school reopening studies, six reported effects on incidence,[16 22 24 28 44 45] two on hospitalisations[25 44] and four on R.[23 27 35 43] Two school holiday studies reported an effect on incidence,[16 41] while the other reported on mortality.[17] The assumed lag period from school policy changes to changes in incidence rate varied between 7 and 20 days, with longer time periods of 26–28 days generally assumed for mortality.

Risk of bias is summarised in table 2. Of the school closure studies, 14 were found to be at moderate risk of bias,[14 15 18–20 24 26 30 35–37 46–48] 14 at serious risk[21 23 29 31 33 34 38 39 42 43 49–51 53] and 4 at critical risk of bias.[32 40 44 52] Of the school reopening studies, four were found to be at moderate risk,[24 25 28 35] four at serious risk[23 27 43 44] and three at critical risk of bias.[16 22 45] The school holiday studies were found to be at moderate (n=1),[41] serious (n=1)[17] or critical (n=1)[16] risk of bias.

There was significant heterogeneity in the study findings (table 3): 17 studies[14 24 31 32 34–38 40 42–44 48–51] reported that closing schools was associated with a reduction in transmission rates; 9[15 18 20 23 26 29 30 39 47] found no association

Table 1 Characteristics of included studies, stratified by study design

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
School closures—pooled multiple-area before-after comparison studies (n=22)							
Auger *et al* [14]	USA	Study period: 13 March 2020 to 23 March 2020 Exposure period: 1 January 2020 to 29 April 2020 Lag period: 16 days (incidence), 26 days (mortality)	Primary and secondary schools	US state	*Incidence*: NPIs preschool closure (restaurant closure, stay-at-home orders). NPIs postschool closure (stay-at-home orders). Testing rate preschool and post school closure *Mortality*: NPIs preschool closure (restaurant closure, mass gathering ban, stay-at-home orders). NPIs postschool closure (restaurant closures, stay-at-home orders) *Both*: cumulative COVID-19 cases preschool closure. % of population under 15, % of population over 65, % of nursing home residents, social vulnerability index and population density	Variable	Negative binomial regression to estimate effect of school closures on the changes in incidence and mortality rates, as calculated by interrupted time series analysis.
Banholzer *et al* [15]	USA, Canada, Australia, Norway, Switzerland and EU-15 countries	Study period: n=100 cases until 15 April 2020 Exposure date: variable Lag period: 7 days	Primary school closure data used to determine exposure date	Country	Border closure, event ban, gathering ban, venue closure, lockdown, work ban, day-of-the-week effects	Variable	Bayesian hierarchical model assuming negative binomial distribution of new cases.

Continued

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Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Brauner *et al* [18]	34 European and 7 non-European countries	Study period: 22 January 2020 to 30 May 2020 Exposure period: variable Incubation period: 6 days Infection to death: 22 days	Primary and secondary schools	Regional data where available, otherwise country	Mass gathering bans, business closures, university closures, stay-at-home orders	Variable	Bayesian hierarchical model to estimate effectiveness of individual NPIs on the reproduction number
Chernozhukov *et al* [19]	USA	Study period: 7 March 2020 to 3 June 2020 Exposure period: variable, but 80% of states closed within 2 days of 15 March 2020 Lag period: 14 days (incidence), 21 days (mortality)	Primary and secondary schools	US state	Business closures, stay-at-home orders, hospitality closures, mask mandates, mobility data, national case/mortality trends	Variable	Regression model with autoregressive strucutres to allow for dynamic effects of other NPIs and mobility data.
Courtemanche *et al* [20]	USA	Study period: 1 March 2020 to 27 April 2020 Exposure period: variable, generally mid-March Lag period: 10 and 20 days	Not specified	US counties, or county equivalents	Other NPIs (stay-at-home orders, hospitality closure, limiting gathering size), total daily tests done in that state	Variable	Fixed effects regression to estimate the effect of school closure on the growth rate of cases (% change).

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Dreher *et al*[21]	USA	Study period: 500th case until 30 April 2020 Exposure period: variable	Not specified	US state	Data collected on: demography (population density, population size, GDP, state-wide health and healthcare capacity) and on NPIs (stay-at-home orders, mass gathering bans and business closures). However, covariables with a p>0.1 in univariate analysis and collinear variables were excluded. Full details are not available of which covariables were included	Variable	1. Univariate linear regression of NPI implementation and average Rt after the 500th case. 2. Cox proportional hazards regression of the association between NPI implementation and time for cases to double from 500th to 1000th case. 3. Cox proportional hazards regression of the association between NPI implementation and time for deaths to double from 50 to 100.
Garchitorena *et al*[24]	32 European countries	Study period: 1 February 2020 to 16 September 2020 Exposure period: variable Lag period: no lag applied	Early years settings, primary schools and secondary schools	Country	Stay-at-home orders, university closures, mass gathering bans, mask mandates, work-from-home orders, public space closures, business and retail closures	Variable	Used incidence data, supplemented by a capture-recapture method using mortality data to infer undiagnosed cases. Compared this with a counterfactual age-structured Susceptible-Exposed-Infectious-Removed (SEIR) model coupled with Monte Carlo Markov Chain to estimate effectiveness of NPI combinations—then estimated their disentangled effects (considering each individual NPI over the duration of their implementation).

Continued

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Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Hsiang *et al*[26]	Italy, France, USA	Study period: 25 February 2020 to 6 April 2020 Exposure date: varied by country Lag period: no lag applied	Not specified	Provincial/Regional level (Italy and France), state level (USA)	Other NPIs (travel ban and quarantine, work-from-home order, no social gatherings, social distancing rules, business and religious closures, home isolation), test regimes	Variable	Reduced-form econometric (regression) analysis to estimate the effect of school closures on the continuous growth rate (log scale).
Jamison *et al*[30]	13 European countries	Study period: until 16 May 2020 Exposure period: variable Lag period: 18 days	Not specified	Country	Workplace closures, public event cancellations, restricting gathering sizes, closing public transport, stay-at-home orders, internal movement restrictions and international travel, mobility data, population >65 years, population density, number of acute care beds per population, starting date of epidemic, day of the epidemic	Variable	Linear regression model reporting the percentage point reduction in the daily change of deaths measured as a 5-day rolling average.
Kilmek-Tulwin and Tulwin[32]	15 European countries; Argentina, Brazil and Japan	Study period: not specified Exposure period: variable	Not specified	Country	None	Not specified	Wilcoxon signed rank test to determinethe significance of differences between pairs of incidence rates from different time points. Time points considered: 16th day, 30th day, 60th day since 100th case. Cases/ million population compared following implementation of school closures.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Krishnamachari *et al*[33]	USA	Study period: not specified Exposure period: variable	Not specified	US state US city	State analysis: days for preparation, population density, % urban, % black, % aged >65 years, % female City analysis: use of public transport for work, use of carpool for work, population density and % black Both analyses: days from state-level emergency declaration to gathering size restrictions, non-essential business closures, stay-at-home orders, gathering restrictions, restaurant closures	Variable	Negative binomial regression comparing states/cities above and below median value for days to implement school closures, on rate ratio of cumulative incidence on days 14, 21, 28, 35 and 42 following the area's 50th case. All variables in analysis classified a 1 if above median value for dataset, and 0 if below.
Li *et al*[34]	Worldwide (167 geopolitical areas)	Study period: 1 January 2020 to 19 May 2020 Exposure period: variable	Not specified	Country, province or state	None specified	School closures only considered in the context of travel and work restrictions, and mass gathering bans already being in place	Validate a novel SEIR model ('DELPHI') in the 167 countries between 28 April 2020 and 12 May 2020. Then elicit the effect of each day an NPI was in place on the DELPHI-derived changes to the infection rate at each time point.

Continued

Table 1 Continued							
Study	**Country**	**Study period**	**Setting type**	**Unit of exposure**	**Confounders/ Co-interventions adjusted for**	**Other NPI measures**	**Analysis type**
Li *et al*[3]	Worldwide (131 countries)	Study period: 1 January 2020 to 20 July 2020 Exposure period: variable	Not specified	Country	Other NPIs (international travel bans, internal travel bans, stay-at-home requirements, public transport closures, mass gathering bans, public event bans, workplace closures)	Variable	Defined a time period as a period in which the NPIs in a given country were the same. Calculated the R ratio as the ratio between the daily R of each period and the R from the last day of the previous period. Pooled countries using log-linear regression with the introduction and relaxation of each NPI as independent variables for the first 28 days after introduction/relaxation of the NPI.
Liu *et al*[36]	Worldwide (130 countries)	Study period: 1 January 2020 to 22 June 2020 Exposure period: variable Lag periods: 1, 5 and 10 days	Not specified	Mostly country, although lags were examined at the World Region level	Various parsimonious models. Variables considered: workplace closure, cancellation of public events, gathering size restrictions, public transport closures, stay-at-home requirements, internal movement restrictions, international travel restrictions, income support for households, public information campaigns, testing policy and contact tracing policy	Variable	Parsimonious linear fixed effects panel regression, using stepwise backwards variable selection. Accounted for collinearity of interventions by conducting hierarchical cluster analysis with multiscale bootstrapping to test the statistical significance of identified clusters.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Papadopoulos *et al* [39]	Worldwide (150 countries)	Study period: 1 January 2020 to 29 April 2020 Exposure period: variable Lag period: no lag applied	Not specified	Country	NPIs (workplace closure, public event cancellations, gathering size restrictions, public transport closures, stay-at-home restrictions, internal travel restrictions, international travel restrictions, public information campaigns, testing systems and contact tracing systems), timing of each NPI in days since first case, overall stringency index and sociodemographics (population, life expectancy, purchasing power, longitude, date of first death, average household size)	Variable	Univariate regression model for effect of school closures on total log cases and total log deaths. Multivariate regression model for effect of timing of school closures (relative to first case) on log total cases and log total deaths.
Piovani *et al*,[40]	37 OECD Member Countries	Study period: 1 January 2020 to 30 June 2020 Exposure period: variable Lag period: 26 days	Not specified	Country	Timing of mass gathering bans, time from first death to peak mortality, cumulative incidence at first death, log population size, hospital beds per population, % population aged 15–64 years, % urban, annual air passengers and population density	Variable	Multivariable negative binomial regression with panel data.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Rauscher [42]	USA	Study period: until 27 April 2020 Exposure period: state's 100th death until time of school closures Lag period: not specified	Not specified	US state	Population density, number of schools, public school enrolment, stay-at-home order date, whether school closures were mandated or recommended	Variable	Regression analyses of time between the state's 100th cases and day of school closures and the daily cumulative cases and deaths, measured on the log scale per 100 000 residents.
Stokes *et al* [46]	Worldwide (130 countries)	Exposure: time before first death; and first 14 days after first death Lag period: up to 24 days	Not specified	Country	An overall average strictness and timeliness of NPI measures (as a whole) derived from data on school closures, workplace closures, public event bans, gathering bans, public transport closures, stay-at-home orders, internal movement restrictions, international travel restrictions and public information campaigns. Also adjusted for days since NPI implementation, population density, % over 65, % male, life expectancy, hospital beds, GDP, health expenditure, international tourism, governance, region, testing policy, contact tracing policy	Variable	Multivariable linear regression to estimate the effect of NPIs (including school closures) as lagged variables on the daily mortality rate per 1 million 0–24 days after the first death, 14–38 days after the first death.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Wu *et al* [47]	USA	Study period: until 28 May 2020 Exposure period: variable	Not specified	US counties	Stay-at-home orders, mass gathering bans, restaurant closures, hospitality and gym closures, federal guidelines, foreign travel ban	Variable	Grouped together demographically and socioeconomically similar counties into five clusters, then developed a model of R for each cluster applying a Bayesian mechanistic model to excess mortality data.
Yang *et al* [48]	USA	Study period: 21 January 2020 to 5 June 2020 Exposure period: variable	Early years, and 'schools' (presumed primary and secondary)	US counties	County-level demographic characteristics, NPIs (school closures, leisure activity closure, stay-at-home orders, face mask mandates, daycare closures, nursing home visiting bans, medical service suspension) and previous week log R	Variable, but school closures generally implemented before other measures	Mechanistic transmission models fitted to lab-confirmed cases, applying lag times from the literature. Used generalised estimating equations with autoregression of confounders.
Yehya *et al* [49]	USA	Study period: 21 January 2020 to 29 April 2020 Exposure measure: time (days) between 10th COVID-19 death and school closure Lag (exposure to mortality): up to 28 days	Primary and secondary schools	US state	Population size, population density, % aged <18 years, % aged >65 years, % black, % Hispanic, % in poverty, geographical region	Variable	Multivariable negative binomial regression to estimate mortality rate ratios associated with each day of delaying school closure.

Continued

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Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Zeilinger *et al* [50]	Worldwide (176 countries)	Study period: until 17 August 2020 Exposure period: variable	Not specified	Country	NPIs (mass gathering bans, social distancing rules, business closures, curfews, declaration of emergencies, border restrictions, lockdown); % population >65, % population urban, GDP, % exposed to high PM2.5 air pollution; day of the year, and days since 25th cumulative case	Variable	Non-parametric machine learning model applied to each country, before pooling the estimated NPI effects across countries. Including only the 90 days after the 25th cumulative case.
School closures—within-area before-after comparison studies (n=7)							
Gandini *et al*[23] 2021 No evidence of association between schools and SARS-CoV-2 second wave in Italy	Italy	Study period: 7 August 2020 to 2 December 2020 Exposure period: variable. School reopenings during September. Closures in October and Nobermber Lag: under investigation	Early years, primary and secondary schools	Italian province	None specified	Variable	Created a model of R from data on new cases, parameters estimated using data from the first wave in Italy (serial interval 6.6) and Bayesian methodology to account for the epidemiological uncertainty. Reported as the median for the 7-day posterior moment. Compared neighbouring provinces that reopened or reclosed schools at different times.
Iwata *et al* [29]	Japan	Study period: 27 January 2020 to 31 March 2020 Exposure date: 29 February 2020 Lag period: 9 days	Primary and secondary schools	Country	None specified	Not specified	Time series analysis using Bayesian inference to estimate effect of school closures on the incidence rate of COVID-19.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Matzinger and Skinner [37]	USA	Study period: 6 March 2020 to 1 May 2020 Exposure date: 14 March 2020 (Georgia, Tennessee), 6 March 2020 (Mississippi) Lag period: under investigation	Primary and secondary schools	US state	None specified	Not specified	Calculated changes to the doubling time of new cases, hospitalisations and deaths by plotting $\log_2$ of cases, hospitalisations and deaths against time, and using segmented regression to analyse changes in the trends in response to NPI implementation.
Neidhofer and Neidhofer [38]	Argentina, Italy, South Korea	Study period: not specified Exposure date: Italy 4 March 2020 Argentina 16 March 2020 South Korea not specified Lag period: analysis up to 18 days postschool closure	Not specified	Country	Indirectly adjusted for in derivation of counterfactual, based on most comparable countries for: population size and density, median age, % aged >65 years, GDP per capita, hospital beds per 100 000 inhabitants, public health expenditures, average number of reported COVID-19 deaths before day zero, growth rate of reported COVID-19 cases with respect to the day before and mobility patterns retrieved from Google Mobility Reports	All three countries: banning of public events, restriction of international flights, contact tracing, public information campaign. Other unspecified interventions in place in each country	Difference-in-differences comparison to a synthetic control unit (derived from the weighted average of the epidemic curves from comparable countries that closed schools later), to estimate the % reduction in deaths in the 18 days postschool closure.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Shah *et al* [53]	Australia, Belgium, Italy, UK, USA	Study period: 1 February 2020 to 30 June 2020 Exposure period: variable Lag period: 6 weeks	Not specified	Country	Other NPIs (workplace closures, public event cancellations, restrictions on mass gatherings, public transport closure, stay-at-home orders, internal movement restrictions) and mobility data from Apple	Not specified	Poisson regression to estimate the effect of NPIs on mortality (outcome measure not fully explained).
Sruthi *et al* [43]	Switzerland	Study period: 9 March 2020 to 13 September 2020	Secondary schools used as exposure date	Swiss Canton (region)	Closures of hairdressers, bars, nightclubs, restaurants and retail. Travel restrictions. Mask mandates. Number of hotel rooms within the Canton. Results stratified by Cantons with and without mask mandates in place within secondary schools	Variable	Artificial intelligence model to disentangle the effect of individual NPIs on Rt. R estimated exclusively from incidence data.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Stage *et al* [44]	Denmark, Germany, Norway	Study period: March–June 2020 Closure dates: Around 16 March 2020 Reopening dates: staggered, from late April to mid-May Lag period: under study	Early years, primary and secondary schools	Country	None specified but timing of other NPIs, and changes to testing capacity outlined within analysis	Variable	Closures: observed data compared against counterfactual unmitigated simulation using an epidemic model fitted by Approximate Bayesian Computation, with a Poisson Gaussian process regression model. Response dates measured as a change in growth rate occurring at least 5 days after the intervention, exceeding the 75th centile of the modelled data, and where the deviation persists for at least 5 days. Reopening: growth rate change for each loosening of restrictions, estimating an instantaneous growth rate via a General Additive Model using a quasi-Poisson family with canonical link and default thin plate regression splines.
School closures—pooled multiple-area comparisons of interventions in place at a fixed time point (n=3)							

Continued

Walsh S, *et al*. *BMJ Open* 2021;**11**:e053371. doi:10.1136/bmjopen-2021-053371

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Juni *et al* [31]	Worldwide (144 countries)	Study period: Until 28 March 2020 Exposure date: 11 March 2020 Lag period: 10 days	Not specified	Country	Country-specific factors (GDP per capita, health expenditure as % of GDP, life expectancy, % aged ≥65 years, Infectious Disease Vulnerability Index, urban population density), geography factors (flight passengers per capita, closest distance to a geopolitical area with an already established epidemic, geogrpahical region) and climatic factors (temperature, humidity)	Variable	Weighted random-effects regression analysis to estimate the effect of school closures on the changes to the incidence rate (measured as the ratio of rate ratios, dividing cumulative cases up to 28 March 2020, by cumulative cases until 21 March 2020, for each area).
Walach and Hockertz [52]	34 European countries, Brazil, Canada, China, India, Iran, Japan and USA	Study period: until 15 May 2020 Exposure period: cut-off 15 May 2020 Lag period: no lag applied	Not specified	Country	Days of pandemic, life expectancy, smoking prevalence	Variable	First examined correlations between multiple individual variables and cases/deaths in non-parametric analysis. Then incorporated those with an r>0.3 into generalised linear models, starting with the best correlated variables and adding in only those that improved model fit.

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Wong *et al* [51]	Worldwide (139 countries)	Analysis period: 15 April 2020 to 30 April 2020 Exposure cut-off date: 31 March 2020 Lag period: 14 days	Not specified	Country	Stringency index (workplace closure, public event cancellation, restrictions on gathering size, public transport closure, stay-at-home orders, restrictions on internal movement and international travel, public information campaigns), GDP, population density	Variable	Multivariable linear regression to estimate the effect of school closures on the rate of increase in cumulative incidence of COVID-19.
School reopening studies (n=11)							
Beesley [16]	Worldwide (24 countries)	Study period: until 1 September 2020 Exposure date: variable Lag period: under investigation	Mostly all schools, but in the Netherlands noted that primary schools were reopened first	Country	None	Not specified	Naked eye analysis of 7-day rolling average of new cases.
Ehrhardt *et al* [22]	Germany	Study period: 25 February 2020 to 4 August 20202 Exposure period: school closures 17 March 2020 Staggered school reopening 4 May 2020 to 29 June 2020	Early years settings, primary and secondary schools	Baden-Wurttemberg (region of Germany)	None specified	Not specified	Presentation of an epidemic curve showing daily new cases in Baden-Wurttemberg from 25 February 2020 to 7 August 2020 with key school dates labelled.
Gandini *et al*[23]	See description in school closure section above						
Garchitorena *et al*[24]	See description in school closure section above						

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Harris *et al*[25]	USA	Study period: January–October 2020 Exposure period: variable Lag period: 1–2 weeks	Not specified	US counties	Adjusted for NPIs (stay-at-home orders, non-essential business closures, non-essential business reopening, restaurant closures, restaurant reopenings, mask mandates and resumption of religious gatherings), with state, county and calendar week fixed effects	Variable	Difference-in-differences event study model with propensity score matching comparing exposure data (codified as: virtual only 0, hybrid model 0.5, in-person teaching only 1) with inpatient hospitalisations with diagnoses of COVID-19 or COVID-19-related symptoms from insurance data.
Ingelbeen *et al*[27]	Belgium	Study period: 1 August 2020 to 30 November 2020 Exposure date: 1 September 2020 Lag period: no lag applied	Primary and secondary schools	Brussels, Belgium	None specified	Cafes, restaurants and sports facilities had already been reopened in a limited way from June, and five close contacts were permitted from July	Plotted R using data from the national contact tracing system. Also used the contact tracing data to examine age-specific trends in cases/ contacts following school reopenings.
Isphording *et al*[28]	Germany	Study period: 1 July 2020 to 5 October 2020 Exposure period: variable	Not specified	German counties	Adjusted for mobility data from a private company which have data on one-third of German mobile phone users, and Google mobility reports. Fixed effects used to control for demographic differences	Not specified	Regression model comparing changes in new cases between counties that reopen schools after the summer holidays, with counties that have not yet reopened schools. Considered data from 2 weeks before reopening to 3 weeks after.
Li *et al*[35]	See description in school closure section above						
Sruthi *et al*[43]	See description in school closure section above						

Continued

Table 1 Continued

Study	Country	Study period	Setting type	Unit of exposure	Confounders/ Co-interventions adjusted for	Other NPI measures	Analysis type
Stein-Zamir *et al*[45]	Germany	Study period: 1 July 2020 to 5 October 2020 Exposure period: variable	Not specified	German counties	Adjusted for mobility data from a private company which have data on one-third of German mobile phone users, and Google mobility reports. Fixed effects used to control for demographic differences	Not specified	Regression model comparing changes in new cases between counties that reopen schools after the summer holidays, with counties that have not yet reopened schools. Considered data from 2 weeks before reopening to 3 weeks after.
Stage *et al*[44]	See description in school closure section above						
School holiday studies (n=3)							
Beesley[16]	See description in school reopening section above						
Bjork *et al*[17]	11 European countries	Study period: 30 March 2020 to 7 June 2020 Exposure period: 10 February 2020 to 8 March 2020 Lag period: n/a	Not specified	Region	Population density, age distribution, country	Variable	Variance-weighted least squares linear regression comparing timing of February/ March half-term with excess mortality (compared with 2015–2019 data for each region).
Pluemper and Neumayer[41]	Germany	Study period: 10 June 2020 to 23 September 2020 Exposure period: variable	Not specified	School holiday timing: state (n=16) Outcome data: district (n=401)	Average taxable income and proportion of residents who are foreigners	Not specified	Multivariable regression model comparing incident growth rate 2 weeks before summer holidays up to 2 weeks afterwards, with fixed effects to account for for interdistrict differences, and a lagged dependent variable to account for background natioinal trends in the data.

n/a, not available; NPI, non-pharmaceutical intervention; OECD, Organisation for Economic Co-operation and Development.

Walsh S, *et al*. *BMJ Open* 2021;**11**:e053371. doi:10.1136/bmjopen-2021-053371

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Table 2 Findings from the risk of bias assessment using the ROBINS-I tool, stratified by study design

Study	Confounding or co-intervention bias	Selection bias	Misclassification bias	Deviation bias	Missing data bias	Outcome measurement bias	Outcome reporting bias	Overall judgement	Likely direction
School closures—pooled multiple-area before-after comparison studies									
Auger *et al*[14]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Favours experimental**
Banholzer *et al*[15]	Moderate	Low	Low	Low	Low	Moderate	Low	**Moderate**	**Unpredictable**
Brauner *et al*[18]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Chernozhukov *et al*[19]	Moderate	Low	Moderate	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Courtemanche *et al*[20]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Garchitorena *et al*[24]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Hsiang *et al*[26]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Jamison *et al*[30]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Li *et al*[35]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Liu *et al*[36]	Moderate	Low	Low	Low	Low	Low	Moderate	**Moderate**	**Unpredictable**
Stokes *et al*[46]	Moderate	Low	Low	Low	Low	Low	Moderate	**Moderate**	**Unpredictable**
Wu *et al*[47]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Yang *et al*[48]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Krishnamachari *et al*[33]	Moderate	Low	Serious	Low	Low	Low	Low	**Serious**	**Unpredictable**
Dreher *et al*[21]	Serious	Low	Moderate	Low	Low	Moderate	Low	**Serious**	**Favours experimental**
Li *et al*[34]	Moderate	Low	Serious	Low	Low	Low	Low	**Serious**	**Unpredictable**
Papadopoulos *et al*[39]	Moderate	Low	Moderate	Low	Low	Serious	Low	**Serious**	**Unpredictable**
Rauscher[42]	Serious	Low	Low	Low	Low	Low	Low	**Serious**	**Favours experimental**
Yehya *et al*[49]	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Favours experimental**
Zeilinger *et al*[50]	Moderate	Low	Low	Low	Low	Serious	Low	**Serious**	**Favours experimental**
Kilmek-Tulwin and Tulwin[32]	Critical	Moderate	Low	Low	Low	Moderate	Low	**Critical**	**Favours experimental**

Continued

Table 2 Continued

Study	Confounding or co-intervention bias	Selection bias	Misclassification bias	Deviation bias	Missing data bias	Outcome measurement bias	Outcome reporting bias	Overall judgement	Likely direction
Piovani *et al*[40]	Critical	Low	Low	Low	Low	Serious	Low	**Critical**	**Favours experimental**
School closures—within-area before-after comparison studies									
Matzinger and Skinner[37]	Moderate	Low	Low	Low	Low	Moderate	Low	**Moderate**	**Unpredictable**
Gandini *et al*[23]	Serious	Moderate	Low	Moderate	Low	Moderate	Low	**Serious**	**Unpredictable**
Iwata *et al*[29]	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Neidhofer and Neidhofer[38]	Serious	Serious	Low	Low	Low	Low	Moderate	**Serious**	**Favours experimental**
Shah *et al*[53]	Serious	Low	Moderate	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Sruthi *et al*[43]	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Stage—closures	Critical	Low	Low	Low	Low	Moderate	Low	**Critical**	**Favours experimental**
School closures—pooled multiple-area comparisons of interventions in place at a fixed time point									
Juni *et al*[31]	Serious	Low	Low	Low	Low	Low	Low	**Serious**	**Favours experimental**
Wong *et al*[51]	Serious	Low	Low	Low	Low	Low	Low	**Serious**	**Unpredictable**
Walach and Hockertz[52]	Critical	Low	Serious	Low	Low	Serious	Low	**Critical**	**Unpredictable**
School reopening studies									
Garchitorena *et al*[24]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Harris *et al*[25]	Moderate	Moderate	Low	Moderate	Low	Low	Moderate	**Moderate**	**Unpredictable**
Isphording *et al*[28]	Moderate	Low	Low	Low	Low	Moderate	Low	**Moderate**	**Unpredictable**
Li *et al*[35]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Gandini *et al*[23]	Serious	Moderate	Low	Moderate	Low	Moderate	Low	**Serious**	**Unpredictable**
Ingelbeen *et al*[27]	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Sruthi *et al*[43]	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Stage—opening	Serious	Low	Low	Low	Low	Moderate	Low	**Serious**	**Unpredictable**
Beesley[16]	Critical	Low	Moderate	Moderate	Low	Serious	Low	**Critical**	**Favours experimental**
Ehrhardt *et al*[22]	Critical	Low	Low	Moderate	Low	Low	Low	**Critical**	**Favours experimental**

Continued

Table 2 Continued

Study	Confounding or co-intervention bias	Selection bias	Misclassification bias	Deviation bias	Missing data bias	Outcome measurement bias	Outcome reporting bias	Overall judgement	Likely direction
Stein-Zamir *et al*[45]	Critical	Low	Low	Low	Low	Serious	Low	**Critical**	**Unpredictable**
School holiday studies									
Pluemper and Neumayer[41]	Moderate	Low	Low	Low	Low	Low	Low	**Moderate**	**Unpredictable**
Bjork *et al*[17]	Low	Low	Low	Serious	Low	Low	Low	**Serious**	**Favours comparator**
Beesley[16]	Critical	Low	Moderate	Moderate	Low	Serious	Low	**Critical**	**Favours experimental**

Scale applied: low, moderate, serious or critical.
'Favours experimental' indicates that the bias likely resulted in an exaggeration of the reduction in community transmission associated with school closures.
ROBINS-I, Cochrane Risk of Bias In Non-randomised Studies of Interventions.

between school closures and transmission; 5[19 21 33 46 53] reported mixed findings with evidence of a reduction in transmission in some analyses but not others and 1 study[52] reported that school closures were associated with an increase in mortality. The reported effect size of closing schools ranged from precise estimates of no effect[26] to approximately halving the incidence[14]; and from approximately doubling mortality[52] to approximately halving mortality.[14] The studies at the highest risk of bias generally reported large reductions in transmission associated with school closures, while studies at lower levels of bias reported more variable findings (figure 2). Of the school reopening studies, six[22–25 28 44] reported no increase in transmission associated with reopening of schools, while two[16 43] reported mixed findings and three[27 35 45] reported increases in transmission. Of the four school reopening studies at lowest risk of bias,[24 25 28 35] three[24 25 28] reported no association between school reopenings and transmission.

Narrative synthesis of findings

School closures

Pooled multiple-area before-after comparisons

We identified 22 studies[14 15 18–21 24 26 30 32–36 39 40 42 46–50] that analysed before-after data on multiple geographical units, and then pooled the results into one unified estimate of effect (generally by using regression analysis). These studies relied on different timings of NPI implementation in different areas to establish their independent effects, and were therefore at risk of collinearity if compared areas implemented the same NPIs at similar times. These studies were also at risk of bias from sociocultural differences between compared areas.

Of these studies, 11[14 24 32 34–36 40 42 48–50] reported that school closures were associated with significantly reduced community transmission of SARS-CoV-2, 7[15 18 20 26 30 39 47] reported no association and 4[19 21 33 46] reported mixed findings. Those studies found to be at higher risk of bias, generally because they were judged not to have adjusted appropriately for NPIs, testing or sociodemographic data, tended to report reductions in transmission; whereas those studies at lower risk of bias were as likely to report null effects as they were reductions (see figure 2).

Of the three studies[20] using this approach which were considered to be at the lowest risk of confounding, two reported no association and one reported that school closures reduced transmission. Courtemanche *et al*[20] used a fixed effects model (to account for interarea sociodemographic differences) in an event study design to estimate the effect of NPIs (including school closures) on SARS-CoV-2 incidence in US counties between March and April 2020. They adjusted for relevant NPIs, testing regime confounders and underlying trends in each counties' growth rates, and reported a null effect of school closures on growth rate, applying a lag of either 10 or 20 days. Hsiang *et al*[26] used a reduced form of econometric regression to compare changes in incidence in French regions, Italian regions and US states (in three

Table 3 Findings from included studies, stratified by study design

Study	Main finding	Outcome measure	Detailed results	Other comments
School closures—pooled multiple-area before-after comparison studies (n=22)				
Auger *et al*[14]	*School closures associated with reduced transmission*: school closures were associated with decreases in the rate of growth of COVID-19 incidence and mortality	Regression coefficient estimating effect of school closures on changes to weekly incidence and mortality rates	Adjusted model: incidence: 62% (95% CI 49% to 71%) relative reduction Mortality: 58% (95% CI 46% to 67%) relative reduction	Sensitivity analysis of shorter and longer lag periods did not significantly alter the findings. Early school closure associated with greater relative reduction in COVID-19 incidence and mortality.
Banholzer *et al*,[15]	*School closures not associated with a change in transmission*: school closures not statistically significantly associated with a reduction in the incidence rate	Relative reduction in new cases compared with cumulative incidence rate prior to NPI implementation	8% (95% CrI 0% to 23%)	Sensitivity analyses for altering n=100 cases start point, and 7-day lag, did not significantly change the findings. Concede that close temporal proximity of interventions precludes precise estimates, but that NPIs were sufficiently staggered within countries, and sufficiently heterogeneous across countries to have confidence that school closures were less effective than other NPIs.
Brauner *et al*[18]	*School closures not associated with a change in transmission*: school closures not statistically significantly associated with a reduction in Rt	% reduction in Rt with 95% Bayesian CrI	8.6% (95% CrI −13.3% to 30.5%)	Authors report close collinearity with university closures making independent estimates difficult. Findings robust to variety of sensitivity analyses.
Chernozhukov *et al*[19]	*School closures associated with a mixed effect on transmission*: school closures not associated with a change in incidence rate, but statistically significantly associated with a reduction in mortality rate	Regression coefficient estimating the change in weekly incidence rate and weekly mortality rate, measured on the log scale	Incidence rate: 0.019 (SE 0.101) Mortality rate: −0.234 (SE 0.112)	The authors report more precise estimates for other NPIs due to considerable variation in their timing between states, whereas there was very little variation in the timing of school closures across the country, with 80% of states closing schools within a couple of days of 15 March 2020. School closures significantly associated with reductions in mobility.
Courtemanche *et al*[20]	*School closures not associated with a change in transmission*: school closures not statistically associated with the growth rate of confirmed cases	Regression coefficient estimating effect of school closures on the growth rate of cases (% change)	Applying a 10-day lag: 1.71% (95% CI −0.38% to 3.79%) Applying a 20-day lag: 0.17% (95% CI −1.60% to 1.94%)	
Dreher *et al*[21]	*School closures associated with a mixed effect on transmission*: school closures associated with a statistically significant reduction in Rt, but no association with doubling time of cases or deaths	Regression coefficients from the linear and cox proportional hazards regressions. The first analysis is stratified into the first 7 days after iimplementation, and the second 7 days	1. First week: −0.17 (95% CI −0.30 to −0.05). Second week: −0.12 (-0.21 to −0.04) 2. 0.63 (0.25 to 1.63) 3. Null effect but numbers not reported	In adjusted models using Google mobility data, a 10% increase in time spent at home was reported in the week following school closures.

Continued

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Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Garchitorena *et al* [24]	*School closures associated with reduced transmission*: school closures statistically significantly associated with a reduction in COVID-19 transmission	Ratio of transmission rates with and without implementation of the NPI (assessed over the duration of the NPI being in place) Presented as a forest plot so the reported results here are estimated	EY settings: 9% reduction (95% CI 1% to 16%) Primary schools: 10% reduction (95% CI 2% to 18%) Secondary schools: 11% reduction (95% CI 3% to 19%)	
Hsiang *et al* [26]	*School closures not associated with a change in transmission*: school closures not statistically associated with the growth rate of confirmed cases	Regression coefficient estimating effect of school closures on the continuous growth rate (log scale)	Italy: −0.11 (95% CI −0.25 to 0.03) France: −0.01 (95% CI −0.09 to 0.07) USA: 0.03 (95% CI −0.03 to 0.09)	Sensitivity analysis applying a lag to NPI measures on data from China did not significantly alter the findings.
Jamison *et al* [30]	*School closures not associated with transmission*: school closures not statistically significantly associated with relative changes in the 5-day rolling average of COVID-19 mortality	Percentage point change to the 5-day rolling average of COVID-19 mortality	−2.8 (95% CI −6.7 to 1.0), p=0.150	
Kilmek-Tulwin and Tulwin[32]	*School closures associated with reduced transmission*: earlier school closures associated with lower incidence rates in the follow-up period	Change in incidence rate on the 16th, 30th and 60th day post 100th cases between countries ranked by the cases/million population at school closure	16th day: r=0.647, p=0.004 30th day: r=0.657, p=0.002 60th day: r=0.510, p=0.031	
Krishnamachari *et al* [33]	*School closures associated with a mixed effect on transmission*: school closures not statistically significantly associated with cumulative incidence rate in most analyses, but associated with a significant reduction in some analyses	Rate ratio of cumulative incidence between areas that below the median time from state-of-emergency declaration to closure and those above the median time, at days 14, 21, 28, 35 and 42 following the area's 50th case	US states: 14 days: 2.27 (95% CI 0.80, 1.70) p=0.42 21 days: 1.38 (95% CI 0.91, 2.10) p=0.13 28 days: 1.52 (95% CI 0.98 to 2.33), p=0.06 35 days: 1.59 (95% CI 1.03 to 2.44), p=0.04 42 days: 1.64 (95% CI 1.07 to 2.52), p=0.02 US 25 most populous cities: 14 days: 1.08 (95% CI 0.75 to 1.55), p=0.68 21 days: 1.22 (95% CI 0.81 to 1.83), p=0.34 28 days: 1.24 (95% CI 0.78 to 1.98), p=0.35 35 days: 1.24 (95% CI 0.75 to 2.05), p=0.40 42 days: 1.16 (95% CI 0.67 to 2.02), p=0.59	Secondary analysis comparing results in cities of low and high population density at 35 days post-50th case in the state. In low-density cities, they report a non-significant trend towards early school closures reducing cumulative incidence rate, in high-density cities they report the opposite—a non-significant trend towards late school closures reducing cumulative incidence rate.
Li *et al*[34]	*School closures associated with reduced transmission*: school closures were associated with a reduction in the COVID-19 incidence rate	Reported the additional benefit of every day that school closures were added to travel and work restrictions, and mass gathering bans	17.3 (SD 6.6) percentage point reduction in infection rate Travel and work restriction and mass gathering bans alone: 59.0 (SD 5.2) residual infection rate ovserved compared with DELPHI predicted no intervention Travel and work restriction and mass gatherings bans with school closures: 41.7 (SD 4.3)	

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Li *et al*[35]	*School closures associated with reduced transmission*: school closures associated with a reduction in Rt across the 28 days following closures	Ratio between R while NPI in place, and R on the last day of the previous time period. Reported at 7, 14 and 28 days (as well as visual representation of each individual day to demonstrate trend)	Day 7: 0.89 (95% CI 0.82 to 0.97) Day 14: 0.86 (95% CI 0.72 to 1.02) Day 28: 0.85 (95% CI 0.66 to 1.10)	
Liu *et al*[36]	*School closures associated with reduced transmission*: school closures associated with a statistically significant reduction in Rt across analyses	'Strong' evidence for NPI effectiveness if statistically significant across multiple parsimonious models varying the follow-up period, the lag time and the classification of the NPI. 'Moderate' evidence if significant in some models; 'weak' if not Effect sizes from individual models are a regression coefficient on change in R	'Strong' evidence of effectiveness for school closures. Effect sizes in individual models between 0.0 and −0.1	
Papadopoulos *et al*[39]	*School closures not associated with a change in transmission*: school closures not statistically significantly associated with a reduction in the total number of log cases or deaths	Regression coefficient estimating the effect of school closures, and timing of school closures relative to first death, on log total cases and log total deaths	Univariate analysis of school closure policy showed no statistically significant association with log total cases (−0.03 (95% CI −0.256 to 0.218) or log total deaths (−0.025 (95% CI −0.246 to 0.211), p=0.776) Univariate analysis of timing of school closure was significantly associated with reductions in outcomes, so was considered in multivariate analysis. Multivariate analysis showed found no statistically significant association with log total cases (coefficient −0.006, CIs not reported) or deaths (−0.012 (95% CI −0.024 to 0.00), p=0.050)	
Piovani *et al*[40]	*School closures associated with reduced transmission*: earlier school closures associated with lower cumulative COVID-19 mortality	Regression coefficient estimating % change in cumulative mortality for every day school closures delayed	Every 1 day delay in school closures was associated with an increase of 4.37% (95% CI 1.58 to 7.17), p=0.002 in cumulative COVID-19 mortality over the study period	
Rauscher[42]	*School closures associated with reduced transmission*: school closures were associated with fewer cases and fewer deaths	Percentage point increase in the number of new cases and deaths for every day school closures were delayed (not clear over what period the outcome measure represents, assumed until end of study period on 27 April 2020	Each day a state delayed school closures was associated with 0.3% higher cases (p<0.01) and 1.3% higher mortality (p<0.01)	Sensitivity analysis removing the seven states that only recommended school closures, but did not mandate them, did not significantly alter the findings.
Stokes *et al*[46]	*School closures associated with mixed effect on transmission*: school closures not statistically significantly associated with a reduction in mortality from 0 to 24 days after the first death, but associated with a reduction in the 14–38 days after	Regression coefficient estimating effect of school closure timeliness and stringency on the daily mortality rate per 1 000 000 population	0–24 days: −0.119 (95% CI −1.744 to 0.398) 14–38 days: −1.238 (95% CI −2.203 to −0.273) No observable trend by stringency of school closure measure (recommended vs partial closure vs full closure)	Sensitivity analyses for lab-confirmed COVID-19 versus clinical diagnosis; and for using negative binomial regression analayses did not alter the findings.

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Wu *et al* [47]	*School closures not associated with transmission*: school closures not statistically significantly associated with R	Output from Bayesian mechanistic model in the format: learnt weight (95% CI) Estimating effect of school closures on R	School closures not statistically significantly associated with Rt in any of the clusters, or when data are aggregated without clustering No clusters: 0.047 (95% CI −0.118 to 0.212) Cluster 1: 0.081 (95% CI −0.246 to 0.408) Cluster 2: 0.060 (95% CI −0.209 to 0.329) Cluster 3: 0.112 (95% CI −0.292 to 0.516) Cluster 4: 0.098 (95% CI −0.194 to 0.390) Cluster 5: 0.038 (95% CI −0.134 to 0.210)	
Yang *et al* [48]	*School closures associated with reduced transmission*: school closures and early years settings closures statistically significantly associated with reductions in R	% reduction in R	School closure associated with 37% reduction in R (95% CI 33% to 40%) Daycare closures associated with 31% reduction (26%–35%)	Sensitivity analysis using mortality data to derive Reff did not significantly alter findings Secondary analysis using data from google found that 32% (95% CI 28% to 34%) of the effect of school closures was explained by changes in workplace mobility.
Yehya *et al* [49]	*School closures associated with reduced transmission*: earlier school closures were associated with reductions in COVID-19 mortality at 28 days	Regression coefficient estimating increase in mortality at 28 days associated with each day school closures were delayed	5% (Mortality Rate Ratio 1.05, 95% CI 1.01 to 1.09)	Sensitivity analyses for starting exposure from first COVID-19 death, or for excluding New York/New Jersey from analysis, did not significantly change the findings.
Zeilinger *et al* [50]	*School closures associated with reduced transmission*: school closures associated with a reduction in growth rate of COVID-19 cases	Growth rate calculated as the ratio of cumulative cases from 1 day to the next, applying a 7-day moving mean to smooth out weekday effects	School closures associated with drop in predicted growth rate between 10 and 40 days after implementation, median drop 0.010 (not clear what this value equates to but relatively large compared with other NPIs)	
School closures—within-area before-after comparison studies (n=7)				
Gandini *et al* [23]	***School (re-)closures not associated with a change in transmission***: reclosing schools not associated with a change in the rate of decline of R	Plotting Rt over time with school reclosure timings noted Analysed the effect of reclosing schools on Rt, which was done proactively before national lockdown in two large provinces	Lombardy and Campania closed schools before the national school closures in November. In both cases, they find that Rt started to decline around 2 weeks before school closures, and the rate of decline did not change after school closures	Mitigation measures in place in reopened schools included: temperature checks, hand hygiene, increased cleaning and ventilation, one-way systems, mask mandates, social distancing and bans on school sports/music.
Iwata *et al* [29]	*School closures not associated with a change in transmission*: school closures not statistically associated with the incidence rate of new cases	Time series analysis coefficient estimating effect of school closures on the change in daily incidence rate	0.08 (95% CI −0.36 to 0.65)	Sensitivity analysis for different lag times did not change the general finding of null effect.

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Matzinger and Skinner [37]	*School closures associated with reduced transmission*: school closures were associated with reductions in the doubling time of new COVID-19 cases, hospitalisations and deaths	Changes to the doubling time of the epidemic in each state, following school closures	Georgia: 7 days after school closures the doubling time slowed from 2.1 to 3.4 days Tennessee: 8 days after school closures the doubling time slowed from 2 to 4.2 days Mississippi: 10–14 days after school closures the doubling time slowed from 1.4 to 3.5 days	Only included Georgia, Tennessee and Mississippi in their explicit analysis of school closure effect because these were the only states where the authors felt there was a long enough gap between implementation of school closures and other NPI measures. However, they show several figures of other states that initiated school closures at the same time as other lockdown measures. In these states (Arizona, Florida, Ilinois, Maryland, Massachussetts, New Jersey, New York and Texas), a similar pattern is observed for doubling time of cases, with time lags varying between 1 and 2 weeks. Patterns appeared to be similar for hospitalisations and deaths, although these data were not always reported, and more difficult to interpret.
Neidhofer and Neidhofer [38]	*School closures associated with reduced transmission*: school closures were associated with reductions in COVID-19 mortality	% Reduction in deaths in the 18 days postschool closure, compared with synthetic control unit	Argentina: 63%–90% reduction, Italy: 21%–35% reduction, South Korea: 72%–96% reduction in daily average COVID-19 deaths over the 18 days following school closures, compared with the counterfactual	Sensitivity analysis using only excess mortality in Italy reached similar conclusion Selected Argentina, Italy and South Korea because they closed schools at a different time to enacting national lockdown. Supplementary analysis of: Switzerland, Germany, the Netherlands, Indonesia, Canada, Brazil, France, UK, Spain, where school closure was implemented relatively later, and alongside other NPIs: ▶ large (protective) effect in Switzerland, the Netherlands, Indonesia and Canada; ▶ no effect of closures in Germany, Brazil, France and Spain; ▶ large (harmful) effect in the UK.

Continued

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Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Shah *et al*[53]	*School closures associated with mixed effect on transmission*: in Italy, school closures were associate with a reduction in mortality. In the other four countries no aassociation was found between school closures and mortality	Regression coefficient for effect of school closures on mortality (not explained in any greater detail)	Italy 0.81 (95% CI 0.68 to 0.97) Reported only as 'no association' for other countries	
Sruthi *et al*[43]	*School closures associated with reduced transmission*: secondary school closure was associated with a reduction in Rt	Changes to time-varying reproductive number R, estimated from data on new cases. Assumed to be in an infectious state for 14 days from diagnosis	Secondary school closures associated with an average reduction of Rt around 1.0	
Stage *et al*[44]	*School closures associated with reduced transmission*: school closures associated with reductions in the growth rate of new cases	% reduction in growth rate of new cases (Germany only—in Denmark and Norway the graph is drawn without formal statistical analysis)	26%–65% reduction in growth rate of cases across the different states of Germany. No quantitative estimate for Norway or Denmark but authors report a 'clear drop' in new cases after school closures	
School closures—pooled multiple-area comparisons of interventions in place at a fixed time point (n=3)				
Juni *et al*[31]	*School closures associated with reduced transmission*: school closures were statistically significantly associated with a relative reduction in the incidence rate of COVID-19	Regression coefficient estimating effect of school closures on changes to the incidence rate	Adjusted model: 0.77 (95% CI 0.63 to 0.93), p=0.009	Sensitivity analyses of seperating out high income countries did not significantly effect the results.
Walach and Hockertz[52]	*School closures associated with increased transmission*: school closures associated with an increase in COVID-19 mortality	Regression coefficient estimating effect of school closures on the COVID-19 mortality rate	Cases: school closures not associated with cases in univariate analysis so not considered for modelling Mortality: 2.54 (95% 1.24 to 3.85), p<0.0001	
Wong *et al*[51]	*School closures associated with reduced transmission*: school closures were associated with a smaller rate of increase in cumulative incidence of COVID-19	Regression coefficient estimating effect of school closures on the rate of increase in cumulative incidence	−0.53 (95% CI −1.00 to −0.06), p=0.027	Report no collinearity or interactions between different covariables in the model.
School reopening studies (n=11)				
Beesley[16]	*School reopenings associated with a mixed effect on transmission*: school reopening was associated with increases in the 7-day rolling average of new cases in most countries, but not all	Change in 7-day rolling average of new cases	China saw no change. Austria, Canada, France, Germany, Israel, Japan, the Netherlands, Singapore, Spain, Switzerland and the UK saw increases after 24–47 days; with longer lag times attributed to these countries opening schools in a limited to staggered way	Primary versus secondary: in the Netherlands, it was noted that the rise in cases 24 days after primary schools opened was much smaller than the rise 40 days after secondary schools reopened.

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Ehrhardt *et al*[22]	*School reopenings not associated with a change in transmission*: school reopenings not associated with any change in the rate of new cases	Presentation of an epidemic curve showing daily confirmed new cases, with school reopening date labelled	Daily new cases peaked at 1400/day and dropped to around 100/day at the time of staggered school reopening. Daily new cases remained at, or generally below, this level throughout the following 3 months until after schools broke up for summer holidays	Range of comprehensive infection prevention and control measures were in place in schools at the time of school reopening.
Gandini *et al*[23]	*School reopenings not associated with a change in transmission*: timing of school reopenings not consistently associated with onset of increases in R	Plotting R over time with school reopening timings noted. Pairing geographically neighbouring and socioeconomically similar provinces who reopened schools at different times. Comparing time between school reopening and subsequent increases in R—measured as the start of 3 consecutive weeks of increasing R	Bolzano opened schools a week earlier than Trento, but Trento saw a sustained rise in R 1 week ealier than Bolzano. In Abruzzo and Marche; Sicily and Calabria; and Veneto and Apulia; one province reopened schools a week before the other, but Rt increases occured at the same time	Mitigation measures in place in reopened schools included: temperature checks, hand hygiene, increased cleaning and ventilation, one-way systems, mask mandates, social distancing and bans on school sports/music.
Garchitorena *et al*[24]	*School reopenings not associated with a change in transmission*: partial relaxations of school closure measures associated with a null effect on COVID-19 transmission	Ratio of transmission rates with and without implementation of the NPI (assessed over the duration of the NPI being in place) Presented as a forest plot so the reported results here are estimated	EY settings: 0% (95% CI −8% to 8%) Primary schools: 2% (95% CI −7% to 10%) Secondary schools: 1% (95% CI −7% to 9%)	
Harris *et al*[25]	*School reopenings not associated with a change in transmission*: school reopenings not statistically significantly associated with an increase in COVID-19 hospitalisation rate	Regression coefficient reported for both hospitalisations per 100 000 population, and log total hospitalisations	Hospitalisations per 100 000 population: 0.295 (95% CI −0.072 to 0.662) Log total hospitalisations: −0.019 (95% CI −0.074 to 0.036)	Post hoc stratified analysis showed a statistically significant increase in hospitalisations for those counties in the top 25% of hospitalisation preschool reopenings, but no effects for those <75th centile.
Ingelbeen *et al*[27]	*School reopenings associated with increased transmission*: R increased after schools were reopened	Plotted R compared against the changes to the NPIs in place during the study period	R started to increase from approximately 1 week before schools reopened (from 0.9 to 1 at reopening), and then increase more sharply to 1.5 over the next fortnight	Also used the national contact tracing data to examine age-specific trends in number of contacts per case, and number of transmission events between age groups. The increase in Rt after school reopening did not appear to be driven by school-aged children, but by general increases in social mixing across all age groups.
Isphording *et al*[28]	*School reopenings not associated with a change in transmission*: school reopenings not statistically significantly associated with a change in rate of new COVID-19 cases	Regression coefficient estimating change in number of new cases per 100 000 in the 3 weeks postschool reopenings	Reduction of 0.55 cases per 100 000 associated with first 3 weeks of reopening schools. CIs reported only graphically, but upper estimate just crosses 0 (ie, reopening schools led to non-sginificant reduction in transmission of COVID-19)	Sensitivity analysis showed this to be true for all age groups. West German counties drove the non-significant reduction in transmission associated with reopening of schools, while in East Germany the rate of new cases remained constant.

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Li *et al* [35]	*School reopenings associated with increased transmission*: school reopenings associated with an increase in Rt across the 28 days following reopening	Ratio between R while NPI in place, and R on the last day of the previous time period. Reported at 7, 14 and 28 days (as well as visual representation of each individual day to demonstrate trend)	Day 7: 1.05 (95% CI 0.96 to 1.14) Day 14: 1.18 (95% CI 1.02 to 1.36) Day 28: 1.24 (95% CI 1.00 to 1.52)	
Sruthi *et al* [43]	*School reopenings associated with mixed effect on transmission*: secondary school reopening not associated with increase in Rt if mask mandates in place within schools	Changes to time-varying reproductive number R, estimated from data on new cases. Assumed to be in an infectious state for 14 days from diagnosis	Secondary schools reopened with mask mandates in place associated with no change in the R, compared with secondary schools being closed Secondary schools reopened without mask mandates in place associated with an approximate 1.0 increase in R	
Stein-Zamir *et al* [45]	*School reopenings associated with increased transmission*: school reopenings were associated with an increase in new cases of COVID-19	Presentation of an age-stratified epidemic curve showing confirmed cases of COVID-19 in Jerusalem, by date, and comparing to dates of school closure/reopening	Difficult to elicit exact effect sizes from the epidemic curve, but approximately 2 weeks after schools started to reopen, the number of new cases started to increase	Increases in cases after school reopening was more pronounced in younger age groups,[10–19] but were also seen across all ages to a lesser extent.
Stage *et al* [44]	*School reopenings not associated with transmission*: school reopening not associated with increases in the growth rate of hospitalisations or cases	Changes to the incidence rate and changes to instantaneous growth rate in hospitalisations (Denmark) and cases (Denmark, Germany and Norway)	In Germany, the growth rate of cases remained stable throughout and after the staggered reopening of schools. In Denmark and Norway, the growth rate of cases (and hospitalisations for Denmark) remained stable and negative, meaning that incidence continued to reduce despite school reopening	
School holiday studies (n=3)				
Beesley [16]	*School holidays associated with a mixed effect on transmission*: school holidays were associated with increases in the 7-day rolling average of new cases in most countries, but not all	Change in 7-day rolling average of new cases	In Austria, France, Germany and Switzerland, it was noted that school holidays 'exacerbated' the resurgence in incidence rate (not commented on for other countries) Sweden saw a reduction in the rolling average 23 days after they closed for summer holidays (the rolling average peaked within that 23-day period)	
Bjork *et al* [17]	*School holidays associated with increased transmission*: timing of a school winter holiday during the exposure period was positively associated with all-cause excess mortality	All-cause weekly excess mortality per million residents, between 30 March 2020 and 7 June 2020 compared with 2015–2019 mortality rates, compared with regions with no winter holiday or a holiday in the week before the exposure period	Winter holiday in weeks 7, 8, 9 and 10 associated with weekly excess mortality of 13.4 (95% CI 9.7 to 17.0), 5.9 (95% CI 2.3 to 9.5), 13.1 (95% CI 9.7 to 16.5) and 6.2 (95% CI 1.0 to 11.4) per million residents, respectively	The comparator group included those holidaying in week 6 or not at all, and was itself associated with excess mortality of 8.6 (95% CI 6.9 to 10.3).

Continued

Table 3 Continued

Study	Main finding	Outcome measure	Detailed results	Other comments
Pluemper and Neumayer [41]	*School holidays associated with increased transmission*: school holidays associated with increases in the incident growth rate	Percentage point increase in the incident growth rate associated with each week of the summer holiday	Each week of summer school holidays increased the incident growth rate by an average of 0.72 percentage points (95% 0.41 to 1.03). The effect of individual weeks increased during the holidays, such that the first 3 weeks were not indpendently statistically significant, but the sixth week of holidays was associated with an average 1.91 (95% CI 1.47 to 2.42) percentage points increase, which accounts for 49% of the national average growth rate that week	Larger effect sizes for richer regions, and regions with more foreigners, suggesting these regions had a higher proportion of travellers going abroad (the baseline rate in Germany was low at the start of the summer holidays).

CrI, credible interval; NPI, non-pharmaceutical intervention.

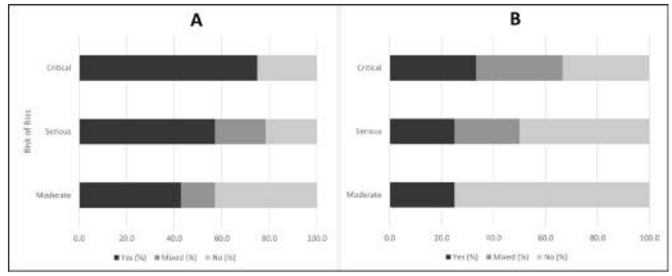

Figure 2 Main findings, stratified by risk of bias. (A) The studies' response to the question: Did school closures reduced community transmission? (Yes, No, Mixed). (B) The studies' response to the question: Did school reopenings increase community transmission? (Yes, No, Mixed).

separate analyses) before and after NPI implementation (including school closures) until early April 2020. Other key NPIs and testing regimes were adjusted for. The authors report a null effect of school closures on growth rate of SARS-CoV-2 incidence, with narrow CIs for France and the USA, but a regression coefficient suggestive of a non-significant preventative effect in Italy (−0.11 (95% CI −0.25 to 0.03)). Li *et al*[54] used the'EpiForecast' model of R[54] to estimate the effectiveness of different NPIs (including school closures) over time in 131 countries between January and June 2020. They identified time periods in which the NPIs in a given country were static, and calculated the 'R ratio' by dividing the average daily R of each period by the R from the last day of the previous period. They reported pooled estimates, regressed across all countries, for the first 28 days after introduction/relaxation of each NPI. Although the CIs for each daily effect size included 1, the trend was clearly towards a reduction in transmission following school closure implementation.

Within-area before-after comparisons

We identified seven studies[23 29 37 38 43 44 53] that compared community transmission of SARS-CoV-2 before and after school closure for single geographical units, and did not pool the results with those of other areas. This approach controls for confounding from population sociodemographic factors, but remains vulnerable to confounding from other NPIs and temporal changes to testing regimes. As with the pooled before-after comparison studies, those studies at higher risk of bias from confounding were more likely to report reductions in transmission associated with school closures.

One study using this approach was found to be at moderate risk of bias. Matzinger *et al*[37] identified the three US states which introduced school closures first, and with a sufficient lag before implementing other measures to assess their specific impact. They plotted incidence rates on a $\log_2$ scale and identified points of inflexion in the period after school closure. This assumes exponential growth in the absence of interventions, which may not have occurred given changes to testing regimes. The doubling time of new cases in Georgia slowed from 2.1 to 3.4 days 1 week after closing schools. Similar results were observed in Tennessee (2.0 to 4.2 days after 1 week) and Mississippi (1.4 to 3.4 days after 2 weeks). The authors also noted inflexion points for hospitalisations and mortality at later time points, although numerical changes were not reported. Tennessee showed a slowing in hospitalisations 1 week after cases, and mortality 1 week after hospitalisation. Mississippi showed a slowing in hospitalisations and mortality at the same time, 1 week after cases—the authors do not comment on this discrepancy. Georgia lacked early hospitalisation data to make such a comparison.

Pooled multiple-area comparisons of interventions in place at a fixed time point

Three studies[31 51 52] considered countries from around the world using a design in which NPIs were considered as binary variables on a specific date (ie, in place or not in place), and the cumulative incidence or mortality to that point was compared with the number of new cases of COVID-19 over a subsequent follow-up period; countries were then compared using regression analysis to elicit independent effect sizes for individual policies including school closures. This approach reduces bias from different testing regimes over time and between countries. However, the use of a single cut-off date for whether school closure was in place means that the effects of long-standing and recent school closures were pooled, introducing misclassification bias. Two of these studies[31 51] were at serious risk of bias and reported that school closures were associated with lower incidence; and one study[52] was at critical risk of bias and reported that closing schools was not associated with incidence but was associated with increased mortality. Each of these studies was at high risk of confounding from other NPIs, in addition to the risk of misclassification bias described above.

School reopening studies

Eleven studies[16 22–25 27 28 35 43–45] considered the effect of school reopening on subsequent SARS-CoV-2 community transmission.[24] Of these, five were pooled multiple-area before-after comparison studies,[24 25 28 35 43] and six were within-area multiple-area before-after comparison studies.[16 22 23 27 44 45] These studies benefited from more staggered lifting of restrictions (compared with their implementation), and more stable testing regimes.

Of the four studies at a lower risk of bias,[24 25 28 35] three[24 25 28] reported that schools were reopened without associated increases in transmission, while one[35] reported increased transmission. Garchitorena *et al*[24] compared incidence data, with adjustment for underdetection, from 32 European countries, using multivariate linear regression models with adjustment for other NPIs and fixed effects to account for intercountry sociodemographic differences. They reported no association with incidence rates up to 16 September 2020 of reopening early years settings (0% mean change in incidence rate (95% CI −8% to 8%)), primary schools (2% (95% CI −7% to 10%)), or secondary schools (−1% (95% CI −7% to 9%)). Harris *et al*[25] estimated the effect of school reopenings on COVID-19 hospitalisation in the USA using an event study model, with analysis at the county-level. They adjusted for other NPIs, and used fixed effects to account for calendar week effects and intercounty differences. They applied a 1-week lag period, and compared data from 10 weeks before to 6 weeks after school reopenings. They initially report null effects when pooling the effects across all counties, however, post hoc sensitivity analyses suggested that there were increases in hospitalisations for counties that were in the top 25% of baseline hospitalisation rate at school reopening (compared with null effects for the bottom 75%). Isphording *et al*[28] compared changes to the COVID-19 incidence rate in German counties that were first to reopen schools after the summer holidays, with those yet to reopen (noting that the timing of such decisions was set years in advance, and not changed due to the pandemic). They considered data from 2 weeks before to 3 weeks after school reopenings, and adjusted for mobility data, and used fixed effects to account for intercounty sociodemographic differences. They reported no association between school reopenings and incidence. One study Li *et al*,[35] is described above as it reports on the effect of both school closures and school reopenings around the world. As for school closures, their effect sizes for each individual day in the 28-day period postschool reopenings were not always statistically significant, but the data trend is clearly that of an increase in transmission associated with school reopenings.

The seven studies[16 22 23 27 43–45] at serious and critical risk of confounding are more difficult to interpret, again predominantly due to the high risk of confounding. Three[16 23 44] reported no association between school reopening and transmission, two[22 43] reported mixed findings and two[27 45] reported increased transmission following reopening of schools.

School holiday studies

Three studies[16 17 41] reported changes in SARS-CoV-2 community transmission associated with school holidays. These holidays occurred according to predetermined timetables and are therefore unlikely to be influenced by background trends in infections. Two studies examined associations between timing of summer holidays on incidence rates in Germany[41] and in multiple European countries,[16] respectively. The other study[17] reported on the timing of the February/March 2020 half-term break timing in countries that neighbour the Alps. Of these, one reported mixed findings on the effect of summer holidays,[16] and two reported that school holidays were associated with increased transmission.[17 41] The authors of these studies considered the primary exposure to be increased social contact from international travel, rather than decreases from the temporary closure or schools.

Different school setting types

One school closure study,[48] three school reopening studies[16 22 44] and one study looking at closures and reopenings[24] considered evidence of independent effects for different types of school closures.

Two studies reported independent effect sizes for different settings, but found considerable overlap between the effect sizes, and noted high temporal correlation between the policy timings meaning that collinearity limits the interpretability of the findings. Garchitorena *et al*[24] (moderate risk of bias) reported the effect of both school closures and school reopenings on changes to R in 32 European countries, with almost completely overlapping estimates of transmission reductions associated with closures in early years settings, primary schools and secondary schools; and equally null effects for each setting associated with reopenings. Yang *et al*[48] (moderate risk of bias) reported that school closures in US counties (presumed primary and secondary combined) were associated with 37% (95% CI 33% to 40%) reductions in R, compared with 31% reductions for early years settings (95% CI 26% to 35%).

Two studies reported staggered reopenings of different school settings, generally with younger children students returning first, and a week or two between each reopenings. Both studies found null effects on transmission overall, and therefore did not report any differential effect by setting type. Stage *et al*[44] (serious risk of bias) noted staggered reopenings in Norway, Denmark and Germany. Ehrhardt *et al*[22] (critical risk of bias) noted staggered reopenings of schools in Baden-Wuttemberg (a region of Germany).

Beesley[16] (critical risk of bias) noted that increases in the 7-day rolling average of new cases were greater in the 40 days after secondary school reopening than they were in the 24 days following primary schools reopening. However, this study is at high risk of confounding from other NPIs, and it is not clear why the chosen (and different) lag periods were applied.

DISCUSSION

We identified 40 studies that provided a quantitative estimate of the impact of school closures or reopening on community transmission of SARS-CoV-2. The studies included a range of countries and were heterogenous in design. Among higher quality, less confounded studies of school closures, 6 out of 14 reported that school closures had no effect on transmission, 6 reported that school closures were associated with reductions in transmission, and 2 reported mixed findings (figure 2); with findings ranging from no association to a 60% relative reduction in incidence and mortality rate.[14] Most studies of school reopening reported that school reopening, with extensive infection prevention and control measures in place and when the community infection levels were low, did not increase community transmission of SARS-CoV-2.

The strength of this study is that it draws on empirical data from actual school closures and reopenings during the COVID-19 pandemic and includes data from 150 countries. By necessity, we include observational rather than randomised controlled studies, as understandably no jurisdictions have undertaken such trials. We were unable to meta-analyse due to study heterogeneity. We were unable to meaningfully examine differences between primary and secondary schools as very few studies distinguished between them, despite the different transmission patterns for younger and older children. Data are also lacking from low-income countries, where sociocultural factors may produce different effects of school closures on transmission to high-income settings, leaving a substantial gap in the evidence base. Data in these studies come exclusively from 2020, and many studies report only up to the summer months, it is therefore unclear whether our findings are robust to the effects of new SARS-CoV-2 variants and vaccines.

A major challenge with estimating the 'independent' effect of school closures, acknowledged by many of the studies, is disentangling their effect from other NPIs occurring at the same time. While most studies tried to account for this, it is unclear how effective these methods were. Even where adjustment occurred there is a risk of residual confounding, which likely overestimated preventative associations; and collinearity (highly correlated independent variables meaning that is impossible to estimate specific effects for each) which could bias results towards or away from the null. One exception was a paper by Matzinger and Skinner,[37] which focused on three US states that implemented school closures first and without co-interventions, and reported a twofold increase in the time for cases to double 1 week after school closures. However, it is possible that the benefits observed here may be attributable, at least in part, to a 'signalling effect' with other changes to social mobility (eg, working from home) being prompted by school closures. Another approach, although ineligible for inclusion in our study, is to examine transmission data for breakpoints, and then work backwards to see what NPIs were in place at the time. Two studies that did this found that transmission started to drop following other NPIs, before school closures were implemented, and found no change in the gradient of decline after school closures in Switzerland[55] and Germany.[56] This may suggest school closures have different effects when implemented first, or on top of other restrictions, perhaps due to a broader signalling effect that the first implemented NPI has on societal mobility patterns. The true independent effect of school closures from the first wave around the world may simply be unknowable.

In contrast, lifting of NPIs in the summer of 2020 (including school reopenings) generally occurred in a more staggered way, and on a background of stable testing regimes and outcome ascertainment. Good-quality observational studies considering data from across 32 European countries,[24] Germany alone[28] and the USA[25] all demonstrated that school reopenings can be successfully implemented without increasing community transmission of SARS-CoV-2, where baseline incidence is low and robust infection prevention and control measures are in place.

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This finding is in keeping with several studies showing little or no effect of school reopening on intraschool transmission rates.[6 57 58] However, the US-based study did comment that those counties with the highest 25% of baseline hospitalisations at the time of reopenings (above 40 admissions per 100 000 population per week) did see an increase in transmission following school reopenings, although the bottom 75% of counties did not see any effect. This may explain why the other school reopening study at lower risk of bias[35] reported a clear, although non-significant, trend towards school reopenings being associated with increases in transmission rates across 131 countries worldwide, with the authors noting "we were unable to account for different precautions regarding school reopening that were adopted by some countries" before citing Israel as an example where an uptick in transmission occurred following reopening, and where 'students were in crowded classrooms and were not instructed to wear face masks'.

The variability in findings from our included studies are likely to reflect issues with study design. However, this may also suggest that there is no single effect of school closures and reopenings on community transmission and that contextual factors modify the impact of closures in different countries and over time. If the purpose of school closures is reduction in social contacts among children, the level of social mixing between children that occurs outside school once schools are closed is likely to be a key determinant of their effect at reducing community transmission. This will be influenced by other NPIs, and other key contextual factors including background prevalence of infection, use of preventive measures in schools prior to closures, age of children affected as well as sociodemographic and cultural factors.

Different countries have adopted different approaches to controlling COVID-19. Early in the pandemic school closures were common, and in some places were one of the first major social distancing measures introduced. The effectiveness of the overall bundle of lockdown measures implemented is proven, but the incremental benefit of school closures remains unclear. In contrast, only one of the four studies of school reopenings assessed at a lower risk of bias reported an increase in community transmission. Collectively, the evidence around school reopenings, while more limited in size, tends to suggest that school reopenings, when implemented during periods of low incidence and accompanied by robust preventive measures, are unlikely to have a measurable impact on community transmission. Further research is needed to validate these findings and their generalisability, including with respect to new variants. These findings are highly important given the harmful effects of school closures.[3 4] Policymakers and governments need to take a measured approach before implementing school closures in response to rising infection rates, and look to reopen schools, with appropriate mitigation measures in place, where other lockdown measures have successfully brought community transmission of SARS-CoV-2 under control.

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