SYSTEMATIC REVIEW AND META-ANALYSIS

Wildfire and Landscape Fire Smoke Exposure and the Risk of Acute Respiratory Infection in Children: A Systematic Review and Meta-Analysis

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ARTICLE INFO

ARTICLE TYPE
Systematic Review and Meta-Analysis

ARTICLE HISTORY
Received: June 20, 2026
Accepted: July 29, 2026
Published: August 14, 2026

KEYWORDS
Acute respiratory infection; Children; Meta-analysis; Particulate matter; Wildfire smoke

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DOI
https://doi.org/10.59345/sjped.v4i1.316

ABSTRACT

Background: Landscape fires are increasing and children are physiologically more vulnerable to smoke than adults. Acute respiratory infection remains a leading cause of death under five years. One previous meta-analysis pooled upper respiratory infection alone; the most recent systematic review pooled no data.

Objective: To quantify the association between exposure to smoke from wildfire and other landscape fires and the risk of acute respiratory infection in children and adolescents aged 0-19 years, and to assess differences by age, exposure window, income setting, and outcome definition.

Methods: Six Boolean strings were run in PubMed and MEDLINE (1,016 unique records). Observational studies reporting an effect estimate with 95% confidence interval for acute respiratory infection at ages 0-19 years exposed to landscape fire smoke were eligible. As no study reported means and standard deviations, pooling used the log-ratio scale with a random-effects DerSimonian-Laird model. Continuous and categorical contrasts were pooled separately. Risk of bias used ROBINS-E and the Newcastle-Ottawa Scale.

Results: Eighteen studies were included and seventeen pooled. In the continuous pool ($k = 12$), each 10 $\mu g/m^3$ increment of fire-related particulate matter gave a relative risk of 1.104 (95% CI 1.070-1.139; Hartung-Knapp 1.042-1.171), I^2 92%, prediction interval 1.015-1.201. Restricted to infection-specific outcomes ($k = 8$) it was 1.148 (1.067-1.234). Risk was higher under five years (1.202, 1.104-1.309) than in unstratified 0-19 year populations (1.018, 1.001-1.035; $p < 0.001$) and after prenatal than postnatal exposure (1.239 versus 1.077; $p < 0.001$). Egger's test indicated small-study effects ($p = 0.0006$); trim-and-fill gave 1.067 (1.036-1.099). A categorical pool ($k = 5$) gave an unstable 1.940 (1.269-2.967).

Conclusion: Fire-related particulate exposure was associated with a modest but consistent increase in the risk of acute respiratory infection in children, concentrated in those under five years and after prenatal exposure.

1. Introduction

Acute respiratory infection remained one of the largest single contributors to childhood morbidity and mortality worldwide throughout the period covered by this review. The Global Burden of Disease Study 2021 estimated that lower respiratory infections alone accounted for hundreds of thousands of deaths in children younger than five years each year, with the burden concentrated overwhelmingly in low- and middle-income countries.[1] The determinants of that burden are multiple, but ambient air pollution has consistently been identified among the most modifiable of them, and within ambient air pollution the contribution of smoke from landscape-scale fires has grown steadily more prominent.

Landscape fires — a category that encompasses forest and bush wildfires, peatland fires, and the deliberate burning of sugar cane and crop residues — were estimated to expose the majority of the world's population to at least one day of substantial fire-derived particulate matter each year between 2000 and 2019, and the population-weighted burden increased across that period.[2] That trajectory is being driven by climate: anthropogenic climate change accounted for between a third and four-fifths of burned area by ecoregion in the western United States and for roughly half of smoke $PM_{2.5}$ concentrations there,[3] and the global increase in extreme fire-weather days now carries an externally forced signal detectable above natural variability.[4] Children born today will therefore accumulate more lifetime smoke exposure than any recent generation.

The health significance of this trend is amplified by evidence that particulate matter of fire origin is not simply equivalent to an equal mass of particulate matter from traffic or industry. Wildfire-specific fine particulate matter was associated with substantially larger increases in respiratory hospital admissions than the same mass concentration from other sources in southern California,[5] and in a five-state analysis of more than six million respiratory emergency visits the per-microgram effect of smoke $PM_{2.5}$ on asthma was approximately eightfold that of non-smoke $PM_{2.5}$.[6] Applying a single undifferentiated concentration-response function underestimates the respiratory burden attributable to particulate matter by roughly a tenth, with the largest underestimation in the most socially vulnerable areas.[7]

Controlled and experimental work supplies a plausible mechanism. Short-term inhalation of wood smoke reduced bronchoalveolar macrophage phagocytosis and exerted cytotoxic effects in healthy adults,[8] and dosimetry-matched exposure of lung macrophages to particulate matter collected during a major wildfire episode halved phagocytic capacity in vitro.[9] Exposure of human nasal epithelial cells to wood-smoke particles altered antiviral gene expression before viral

challenge,[10] and mice exposed to biomass smoke before influenza infection showed higher lung viral load, suppressed innate antiviral mediators and failure to resolve inflammation.[11] Impairment of the innate immune defences of the airway is precisely the mechanism by which an inhaled pollutant would be expected to increase susceptibility to infection rather than merely to exacerbate pre-existing airway disease. It must be emphasised, however, that this mechanistic evidence derives from adult volunteers, adult-derived cell lines and animal models. No equivalent controlled exposure evidence exists in children, and extension of these mechanisms to the paediatric airway is an extrapolation rather than a demonstrated finding.

Children are nonetheless expected to be more susceptible than adults for reasons well established in paediatric physiology: minute ventilation per kilogram of body weight is higher, alveolarisation continues through the first years of life, and the immune repertoire is immature. Consistent with this, ambient particulate matter unrelated to fire has been repeatedly associated with paediatric respiratory infection — with acute lower respiratory infection and pneumonia admissions in Southwest China,[12] with bronchitis in Beijing,[13] with pneumonia in children under two years in Wuhan,[14] with infant bronchiolitis presentations in northern Italy,[15] and with acute respiratory infection across 34 sub-Saharan African countries.[16] Differential behavioural exposure, by contrast, is more often asserted than demonstrated. It is frequently claimed that children spend more time outdoors during the daylight hours when smoke concentrations peak, but the direction of this effect is setting-dependent: during severe haze episodes in Southeast Asia schools close and children are commonly kept indoors, which may reduce rather than increase their exposure relative to working adults. The physiological argument for paediatric susceptibility is therefore stronger than the behavioural one, and only the physiological argument is relied upon here.

Three recent syntheses illustrate the gap in the quantitative evidence. The most comprehensive synthesis of wildfire smoke and child health included 59 studies from 14 countries and pooled all-cause respiratory morbidity, asthma, bronchitis, upper respiratory infection and birth weight; upper respiratory infection was the only infectious outcome for which a pooled estimate was produced, and the authors explicitly called for more studies from low- and middle-income countries and for a focus on very young children.[17] A systematic review restricted to respiratory outcomes in children and adolescents included only five studies, all conducted in the United States or Canada, and performed no meta-analysis at all, the authors stating that pooling was not feasible.[18] A third review examined wildfire-related air pollution and infectious disease across all ages and reported that none of its included studies had performed a child-specific subgroup analysis.[19] Meanwhile, evidence that fire smoke exposure is associated with all-cause child mortality in low- and middle-income countries has strengthened considerably,[20] which makes the absence of a pooled infection-specific paediatric estimate all the more conspicuous, since acute respiratory infection is the most plausible pathway from smoke exposure to death in this age group.

The novelty of this study lies in its being the first meta-analysis to pool the association between landscape fire smoke exposure and acute respiratory infection specifically in children, to include low- and middle-income as well as high-income settings within a single quantitative framework, to separate continuous dose-response estimates from categorical fire-episode contrasts rather than combining quantities that are not commensurable, and to stratify the pooled estimate by age band and by prenatal versus postnatal exposure window. The aim of this study was to quantify the association between exposure to smoke from wildfire and other landscape fires and the risk of acute respiratory infection in children and adolescents aged 0-19 years, to determine whether that association differs by age, exposure window, income setting and outcome definition, and to assess the robustness of the pooled estimate to risk of bias, model specification and small-study effects.

2. Methods

This review was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement.[21] The review was not prospectively registered; this is acknowledged in the limitations.

Search strategy

Six Boolean search strings were executed in PubMed and MEDLINE, and are reproduced below verbatim as they were run, including the United States spellings that appear within them. No restriction was applied by language, publication year or geographical setting. The strings were designed to trade sensitivity against precision in a graded fashion, the broadest capturing any combination of fire or smoke terminology with paediatric and respiratory terminology, and the narrowest restricting all three concepts to the title and abstract fields.

The first and broadest string was (wildfire OR "landscape fire" OR "forest fire" OR haze OR "biomass burning" OR "peat fire" OR "smoke") AND (children OR pediatric OR paediatric OR infant) AND ("respiratory infection" OR pneumonia OR bronchiolitis OR "acute respiratory"), which retrieved 836 records. It was designed to over-retrieve, because the exposure vocabulary in this field is unusually diverse and a single term such as wildfire would have missed the peatland, sugar-cane and crop-residue literature entirely. The second string narrowed the field by requiring that a record also mention a quantitative effect measure or a healthcare-utilisation outcome, and read (wildfire smoke OR haze OR biomass burning OR vegetation fire OR peatland fire) AND (acute respiratory infection OR pneumonia OR upper respiratory OR lower respiratory) AND (children OR under-five OR infants) AND (odds ratio OR relative risk OR incidence rate ratio OR hospital admission OR emergency department); it retrieved 66 records.

The remaining four strings raised precision by confining every concept to the title and abstract fields. The third string, ("wildfire"[tiab] OR "wildfires"[tiab] OR "forest fire"[tiab] OR "haze"[tiab] OR "landscape fire"[tiab]) AND ("acute respiratory infection"[tiab] OR "ARI"[tiab] OR "pneumonia"[tiab] OR "respiratory infections"[tiab]) AND ("child"[tiab] OR "children"[tiab] OR "paediatric"[tiab] OR "pediatric"[tiab] OR "infant"[tiab] OR "under-five"[tiab]), retrieved 15 records. The fourth widened the outcome vocabulary to include non-specific respiratory disease and added bushfire to the exposure terms, reading (wildfire[tiab] OR wildfires[tiab] OR bushfire[tiab] OR "forest fire"[tiab] OR "landscape fire"[tiab]) AND (pneumonia[tiab] OR "respiratory infection"[tiab] OR "respiratory infections"[tiab] OR "respiratory disease"[tiab]) AND (child[tiab] OR children[tiab] OR paediatric[tiab] OR pediatric[tiab] OR infant[tiab]); it retrieved 23 records. The fifth substituted the agricultural and peatland vocabulary for the wildfire vocabulary, as (haze[tiab] OR "biomass burning"[tiab] OR "peatland fire"[tiab] OR "crop residue burning"[tiab]) AND (pneumonia[tiab] OR "respiratory infection"[tiab] OR "respiratory infections"[tiab] OR "respiratory disease"[tiab]) AND (child[tiab] OR children[tiab] OR paediatric[tiab] OR pediatric[tiab] OR

infant[tiab]), and retrieved 21 records. The sixth and last approached the question from the healthcare-utilisation side rather than the diagnostic side, reading ("wildfire smoke"[tiab] OR "smoke exposure"[tiab] OR "fire smoke"[tiab]) AND ("emergency department"[tiab] OR hospitalization[tiab] OR hospitalisation[tiab] OR "hospital admission"[tiab] OR "hospital admissions"[tiab]) AND (child[tiab] OR children[tiab] OR pediatric[tiab] OR paediatric[tiab]), and retrieved 172 records; it was included because several eligible studies report their outcome as an emergency presentation or admission rather than as a named infection.

The six strings together retrieved 1,133 records, of which 117 were retrieved by more than one string and removed, leaving 1,016 unique records for screening. The reference lists of the three previous syntheses[17-19] and of every included study were hand-searched; no additional unique eligible record was identified by this route. No other bibliographic database was searched, and no grey literature source, trial registry or preprint server was consulted. The consequences of this restriction are considered in the limitations, since a search confined to a single indexing service will preferentially retrieve the more visible literature and therefore bears directly on the interpretation of the small-study-effect analysis.

Eligibility criteria

Studies were eligible if they satisfied all of the following. The population comprised children or adolescents aged 0-19 years, or reported a separately extractable stratum within that range; studies of prenatal exposure were eligible where the outcome was ascertained in infancy. The exposure was smoke or particulate matter attributable to a landscape-scale fire, including forest and bush wildfire, peatland fire, transboundary haze, sugar-cane burning and agricultural crop-residue burning, ascertained as a fire-attributable particulate concentration, a satellite-derived fire intensity metric, or a defined fire or haze episode. The comparator was a lower or background particulate concentration, a non-fire period, or an unexposed or minimally exposed population. The outcome was acute respiratory infection or a component of it — pneumonia, lower respiratory tract infection, upper respiratory tract infection, acute bronchitis or bronchiolitis, otitis media, influenza or influenza-like illness — or, where an infection-specific category was unavailable, a respiratory outcome ascertained during a fire episode. The design was an original observational epidemiological study reporting an effect estimate with a 95% confidence interval.

Studies were excluded if the only reported outcome was asthma, wheeze or another non-infectious airway condition; if the outcome was a medication-dispensing proxy rather than a diagnosis; if no paediatric stratum could be extracted; if the outcome was all-cause mortality; or if the report was a review, editorial or modelled burden estimate that applied externally derived concentration-response functions without generating a primary epidemiological association.

Review process, screening and data extraction

The review process is described here in operational detail so that its limitations can be judged. Titles and abstracts of the 1,016 unique records were screened by a single reviewer against the eligibility criteria; a second reviewer independently re-screened all records that the first reviewer marked as potentially eligible or uncertain, and all 44 full texts were read by both reviewers. Disagreements at full-text stage were resolved by discussion and no disagreement required arbitration by a third party. No formal measure of inter-rater agreement was calculated. Risk-of-bias assessments were completed by one reviewer and checked in full by the second, with disagreements resolved by discussion; they were not performed wholly independently in duplicate, which both ROBINS-E[22] and the Newcastle-Ottawa Scale assume. Data extraction was performed by one reviewer into a structured spreadsheet and every extracted numerical value was checked against the source by the second reviewer. This process is weaker than fully duplicated independent screening and assessment, and the possibility of selection and extraction error is acknowledged in the limitations.

For every included study the following were extracted: bibliographic identifiers including a verified digital object identifier; study design; country, setting and study period; population age band and sample size; the exposure definition, exposure assessment method, lag structure and reported increment; the outcome definition and diagnostic coding; and the effect estimate with its 95% confidence interval, transcribed verbatim from the source. Where a study reported several eligible estimates, one was designated as primary according to a pre-specified convention: the most inclusive infection outcome was preferred, and where no composite outcome existed the outcome with the larger case count was selected. Remaining estimates were retained for the outcome-category subgroup analysis only. Percentage changes were converted to ratio measures as RR = 1 + (percentage change / 100) before transformation to the logarithmic scale. Multi-country studies were entered as single data points and their internal country-specific estimates were not entered separately, in order to avoid double-weighting.

Adjudication of one internally impossible confidence interval

One published confidence interval required adjudication, and because reconstructing a published interval is an unusual step the full sequence is set out here in one place. The child-specific upper respiratory infection interval reported by Stowell and colleagues[23] is printed as an odds ratio of 1.018 with a 95% confidence interval of 1.004 to 1.003 — a lower bound that exceeds its upper bound. The full text was retrieved and the interval confirmed to be internally impossible as printed. The upper bound was reconstructed under the assumption of log-symmetry about the published point estimate, as exp[2 x ln(1.018) - ln(1.004)] = 1.0322; the point estimate and the lower bound are exactly as published and nothing beyond that single symmetric bound was imputed. Rescaled to a 10 ug/m^3 increment the study contributes an estimate of 1.195 (1.041 to 1.373), which is the value it contributes to the model. The study was rated High on the ROBINS-E domain for selection of the reported result. A leave-one-out analysis excluding it gave a pooled estimate of 1.100 (1.066 to 1.136), against 1.104 (1.070 to 1.139) with it included, so the reconstruction has no material influence on the primary result.

A second and independent concern applies to the same estimate. The child-specific figure derives from the study's lag-zero sensitivity model rather than from its primary three-day exposure window, for which no child-specific estimate is reported. Selecting an estimate that a study's own authors did not designate as primary is a form of selective reporting introduced by the present review rather than by the original investigators, and it is acknowledged as such.

Risk of bias assessment

Cochrane RoB 2.0 was considered as the review's risk-of-bias instrument but was not applicable: RoB 2.0 is defined for randomised trials and no eligible study was a trial, since randomisation of children to fire smoke exposure is neither feasible nor ethical. Two instruments designed for observational research were therefore used instead. ROBINS-E[22] was applied as the instrument of record across its seven domains — confounding, measurement of the exposure,

selection of participants, post-exposure interventions, missing data, measurement of the outcome, and selection of the reported result — using the Low, Some concerns and High grammar. The design-specific Newcastle-Ottawa Scale was applied in parallel across its selection, comparability and outcome or exposure domains, so that the assessment could be compared with earlier syntheses in this literature, which have generally used that instrument. The cohort, case-control, case-crossover, time-series, quasi-experimental and cross-sectional variants were each scored against a common maximum of nine stars, comprising four for selection, two for comparability and three for outcome or exposure ascertainment, so that scores are comparable across designs; eight or nine stars was classed as Good and seven as Fair. Where the two instruments disagreed, ROBINS-E was treated as decisive, because measurement of the outcome is the central methodological weakness of this evidence base and only ROBINS-E penalises it proportionately.

Statistical analysis

The protocol specified a pooled standardised mean difference expressed as Hedges' g. No included study reported group means and standard deviations; every study reported a ratio measure for an incident event, from which a standardised mean difference is not estimable. Pooling was therefore performed on the natural-logarithm ratio scale. So that the protocol's requested Cohen-metric quantity remains available to readers comparing this review with syntheses reporting standardised effects, a standardised equivalent was derived alongside every pooled estimate using the Chinn transformation, g = ln(RR) / 1.81. The Chinn equivalents are presented for comparability only; no analysis in this review is performed on the g scale.

Relative risks, odds ratios and incidence rate ratios were treated as a single ratio measure and pooled together. This is a standard approximation and is close to exact when the outcome is uncommon in the reference group, but it will cause an odds ratio to overstate the corresponding relative risk where the outcome is common, as upper respiratory infection is in young children. The assumption is stated here because it is not tested elsewhere in this review, and its consequence is considered in the limitations.

Standard errors were derived from published confidence intervals as SE[ln(RR)] = [ln(UCL) - ln(LCL)] / 3.9199, which assumes symmetry on the logarithmic scale; the same assumption underlies the reconstruction described in section 2.3.1. Estimates reported on other increments were harmonised to a common 10 ug/m³ increment by linear rescaling on the logarithmic scale, $\ln(RR_{10}) = \ln(RR_{reported}) \times (10 / \text{reported increment})$. Random-effects models were fitted using the DerSimonian-Laird estimator of the between-study variance in the metafor package for R. Heterogeneity was quantified by the I^2 statistic and the between-study variance tau^2, and a 95% prediction interval was calculated for every pooled estimate, on the grounds that a prediction interval rather than a confidence interval describes the range within which the effect in a new setting would be expected to fall. Because the DerSimonian-Laird interval is anticonservative when heterogeneity is high, the Hartung-Knapp-Sidik-Jonkman interval was computed as a co-primary specification and is reported alongside the DerSimonian-Laird interval for both pools.

Two exposure families were pooled separately and never combined. The continuous pool comprised studies reporting an effect per unit increment of fire-attributable particulate matter; the categorical pool comprised studies contrasting a fire episode or high-exposure area against a reference period or area, a quantity that has no dose-response interpretation. It should be stated plainly that the continuous pool is itself not fully homogeneous in its exposure metric: ten of its twelve studies used fire-attributable fine particulate matter, one used total fine particulate matter measured during a fire season as a proxy for fire smoke, and one used particulate matter below ten micrometres rather than below 2.5 micrometres on satellite-defined burning days. The consequences of this mixture are quantified in the structural sensitivity analyses reported below.

One study was excluded from the continuous pool on pre-specified grounds: its estimate was reported per 1 ug/m³ of total, rather than fire-specific, particulate matter as a cumulative distributed-lag quantity, and rescaling it to a 10 ug/m³ increment would yield a relative risk of approximately 23, which is not a credible dose-response value; it was reported narratively and an add-back sensitivity analysis was performed.

Nine pre-specified subgroup analyses were performed, examining income setting, age band, exposure window, outcome category, study design, geographical region, reported increment class, Newcastle-Ottawa quality and outcome infection-specificity, each tested by a between-group Q statistic. No correction for multiplicity was applied, on the grounds that all nine were pre-specified and the analysis is exploratory rather than confirmatory; readers should nonetheless note that with nine tests at a two-sided alpha of 0.05 the expected number of false-positive findings under the global null is approximately 0.45, and should weight the two significant results accordingly. Sensitivity analyses comprised leave-one-out removal of every study in turn, eight structural exclusions and six alternative estimators of the between-study variance. Small-study effects were assessed by contour-enhanced funnel plot, the Egger regression test and the Begg rank correlation, with the Duval and Tweedie trim-and-fill procedure used to estimate an adjusted pooled effect; formal testing was restricted to the continuous pool, in which the number of studies exceeded ten. Statistical significance was set at a two-sided alpha of 0.05.

3. Results

Study selection

The six search strings retrieved 1,133 records, from which 117 duplicates were removed, leaving 1,016 unique records screened on title and abstract, as Figure 1 shows. Of these, 972 were excluded as not concerning a fire or smoke exposure, as containing no paediatric data, as reporting no respiratory-infection outcome, or as not being primary epidemiological studies. Forty-four full texts were sought and all forty-four were obtained; none was unobtainable. Twenty-six reports were excluded with reasons, itemised in Figure 1: nine reported asthma or wheeze without any infection outcome, eight reported no usable effect estimate with a 95% confidence interval for children, three were systematic reviews or meta-analyses, three used a medication or modelled-burden proxy outcome, two reported no paediatric stratum, and one reported all-cause mortality. Eighteen studies were included in the review. Seventeen entered quantitative synthesis; the eighteenth, the Montana case-crossover analysis of Landguth and colleagues,[24] was carried narratively because its per-1 ug/m³ cumulative-lag estimate of total particulate matter could not be rescaled to a common increment without producing an implausible value, as set out in section 2.5.

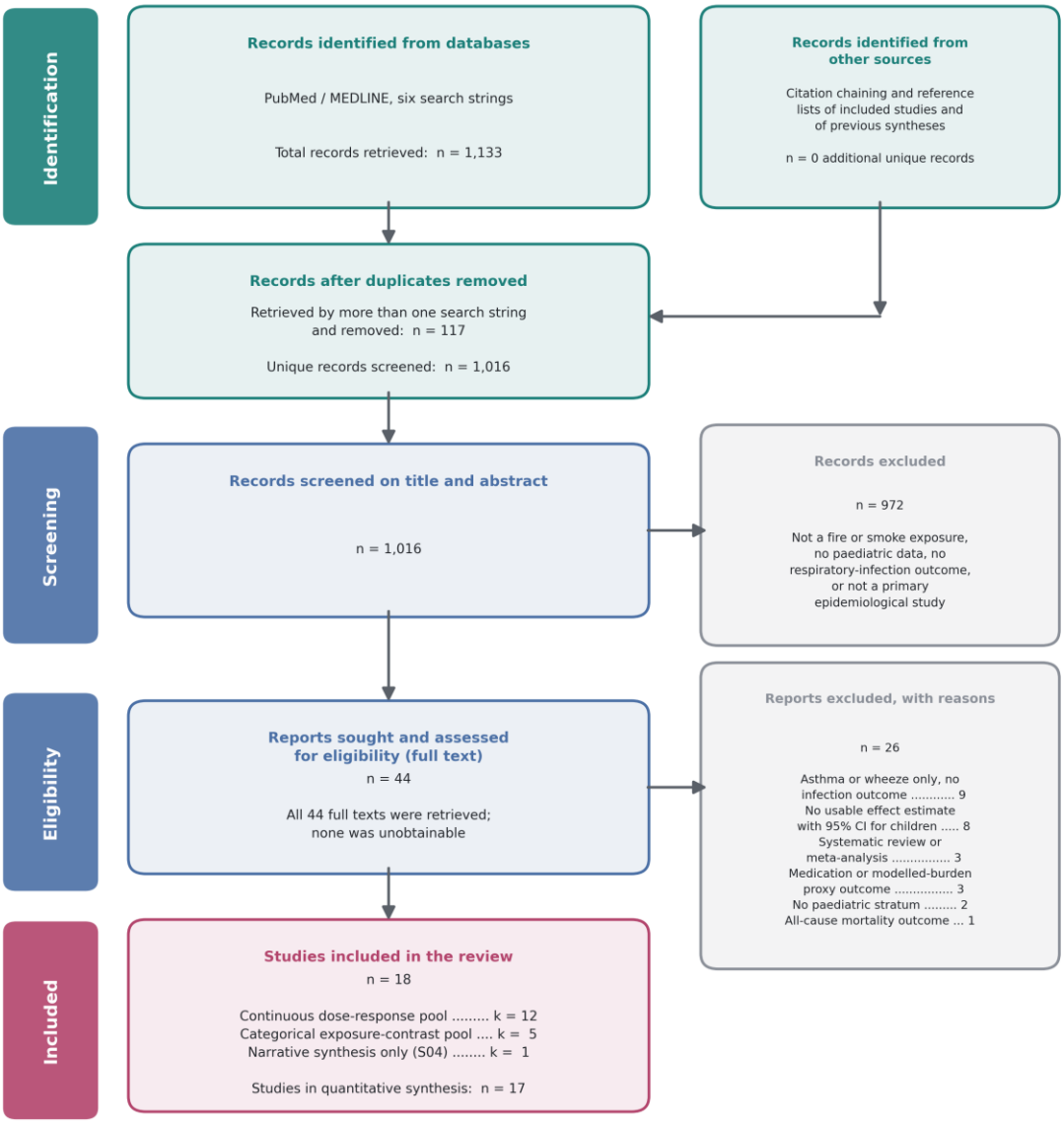

Figure 1. PRISMA 2020 flow diagram. Record counts are the actual yields of the six PubMed and MEDLINE strings reported in section 2.1. Of the eighteen included studies, twelve entered the continuous dose-response pool, five the categorical exposure-contrast pool, and one was carried narratively only.

Characteristics of included studies

The eighteen included studies are described in Table 1. By location of the study population, seven were conducted in North America, four in Asia, three in South America and two in Oceania, and two were multi-country analyses — one covering 48 low- and middle-income countries across Africa, Asia and Latin America, the other 1,012 communities in seven countries and territories. No included study was conducted in continental Europe. Seven studies were set wholly or partly in low- and middle-income countries.

Counting across all three synthesis groups of Table 1, study designs comprised seven case-crossover analyses, five time-series analyses, three cohort studies, one nested case-control study, one quasi-experimental analysis with a non-fire control year, and one cross-sectional analysis linked to satellite fire data, totalling eighteen. Sample sizes are not expressed in a common unit across studies: the smallest population was 289 individually phenotyped children in an Australian cohort and the largest dataset comprised more than 67 million hospital admissions in a seven-country time-series, so the range spans both participant counts and event counts. Exposure was ascertained as a modelled fire-specific particulate concentration in twelve studies, as total particulate matter during a fire season in two, and as a satellite-derived fire intensity or episode indicator in four. Outcomes were infection-specific in twelve studies and comprised a broader respiratory category in six. Studies are grouped in Table 1 by synthesis pool so that continuous per-increment estimates and categorical contrasts are not read as commensurable.

The effect estimate contributed by each study is set out separately in Table 2, which reports both the value exactly as published on the study's own increment or contrast and the value used in the model after harmonisation to a 10 ug/m^3 increment. Presenting both prevents the two quantities from being confused: for example, the Indian cross-sectional analysis contributes a relative risk of 3.65 for the top against the lowest quintile of crop-residue burning intensity,[40] which is a categorical contrast and is not comparable with the dose-response estimates in the upper block of the table. The harmonised values in the final column of Table 2 are those carried into every pooled analysis reported below.

Table 1. Characteristics of the eighteen included studies, grouped by synthesis pool.

ID	Study	Design	Setting	Period	Population	Exposure metric	Outcome	RoB	NOS
— CONTINUOUS DOSE-RESPONSE POOL (k = 12)									
S14	Uttajug et al.[25]	Case-crossover	Eight provinces, upper northern Thailand	2014-2018	0-14 y	PM_{10} on satellite-defined burning days	Respiratory hospital visit	High	7/9
S02	Zhou et al.[26]	Two-stage time-series	1,012 communities, seven countries and territories	2000-2019	0-19 y; 67.9 million admissions	Landscape-fire-sourced $PM_{2.5}$, gridded	Infectious-disease admission	Low	9/9
S17	Ignotti et al.[27]	Time-series	Alta Floresta, Amazonia, Brazil	2005	Children, dry (burning) season	Biomass-burning $PM_{2.5}$, modelled	Respiratory hospital admission	Some concerns	7/9
S06	Delfino et al.[28]	Time-series	Six counties, southern California, USA	2003	5-18 y stratum of 40,856 admissions	Wildfire $PM_{2.5}$, interpolation and MODIS	Pneumonia admission (ICD-9)	Some concerns	7/9
S08	Howard et al.[29]	Retrospective cohort	Northwest Territories, Canada	2012-2015	<5 y stratum	Ambient $PM_{2.5}$ during a fire season	Pneumonia, emergency visit (ICD-10-CA)	Some concerns	7/9
S03	Chen et al.[30]	Case-crossover	501 cities, China	2008-2019	4-24 y; 6,089,271 cases	Wildfire-related $PM_{2.5}$, machine learning	Notifiable respiratory transmitted disease	Some concerns	8/9
S07	Stowell et al.[23]	Case-crossover	Colorado, USA	2011-2014	0-18 y; 94,022 records	Smoke $PM_{2.5}$ separated from non-smoke	URTI (ICD-9 460-465, 466.0)	Some concerns	8/9
S16	Cancado et al.[31]	Time-series	Piracicaba, Sao Paulo, Brazil	1997-1998	<13 y	Sugar-cane burning $PM_{2.5}$, apportioned	Respiratory hospital admission	Some concerns	7/9
S12	Lan et al.[32]	Nested case-control	British Columbia, Canada	2016-2020	Infants to age 1 y; n = 98,791	Prenatal $PM_{2.5}$, CanOSSEM model	Lower respiratory infection (ICD-9)	Some concerns	9/9
S11	Willis et al.[33]	Prospective cohort	Latrobe Valley, Victoria, Australia	2014-2018	289 children (79 exposed in utero)	Individualised fire-attributable $PM_{2.5}$	Doctor-diagnosed URTI, cold or influenza	High	7/9
S01	Li et al.[34]	Case-crossover	48 low- and middle-income countries	2003-2014	<5 y; 36,432 children with ARI	Fire-sourced $PM_{2.5}$, chemical transport model	ARI (two-week symptom recall)	Some concerns	8/9
S15	Mahsin et al.[35]	Time-series (before-during-after)	Calgary, Alberta, Canada	2015	0-9 y stratum	Wildfire-season $PM_{2.5}$, city monitors	Respiratory physician visit	Some concerns	7/9
— CATEGORICAL EXPOSURE-CONTRAST POOL (k = 5)									
S18	Requia et al.[36]	Case-crossover	North (Amazon) region, Brazil	2008-2018	<=5 y stratum of 2,044,038 admissions	Wildfire waves (99th-percentile $PM_{2.5}$)	Respiratory hospital admission	Some concerns	8/9
S10	Ziou et al.[37]	Data-linkage cohort	Latrobe Valley, Victoria, Australia	2012-2015	Infants; 2,776 (861 exposed)	Modelled fire-related $PM_{2.5}$ at address	Respiratory infection, emergency dept.	Low	9/9
S05	Hutchinson et al.[38]	Case-crossover	San Diego County, California, USA	2007	0-4 y stratum of 176,851 beneficiaries	Modelled wildfire $PM_{2.5}$ by ZIP code	URTI, emergency department	Some concerns	8/9
S09	Lee et al.[39]	Quasi-experimental	Gangwon Province, South Korea	2017-2018	<9 y stratum, national insurance	Wildfire-exposed area and period	Pneumonia (ICD-10 J13, J15-J18)	Some concerns	8/9
S13	Chakrabarti et al.[40]	Cross-sectional	Northern India	2012-2014	<5 y subgroup of 252,539 persons	Crop-residue burning intensity (MODIS)	ARI, treatment sought within two weeks	High	7/9
— NARRATIVE SYNTHESIS ONLY (k = 1)									
S04	Landguth et al.[24]	Case-crossover	Western Montana, USA	2017-2020	0-17 y; 10,133 visits	Total $PM_{2.5}$ at address (smoke-dominated)	URTI and LRTI (ICD-10-CM)	High	7/9

ARI, acute respiratory infection; ICD, International Classification of Diseases; LRTI, lower respiratory tract infection; NOS, Newcastle-Ottawa Scale (stars out of a common maximum of nine for every design variant); URTI, upper respiratory tract infection.

Table 2. Effect estimates contributed by each included study, as published and after harmonisation.

ID	Study	Reported increment or contrast	Effect as published (95% CI)	Effect used in the model, per 10 ug/m³ (95% CI)
— CONTINUOUS DOSE-RESPONSE POOL (k = 12)				
S14	Uttajug et al.[25]	Per 10 ug/m³ PM_{10}, burning day	OR 1.01 (1.00-1.02)	1.010 (1.000-1.020)
S02	Zhou et al.[26]	Per 10 ug/m³	RR 1.015 (1.010-1.019)	1.015 (1.011-1.020)
S17	Ignotti et al.[27]	Per 10 ug/m³	+6.0% (1.4-10.8)	1.060 (1.014-1.108)
S06	Delfino et al.[28]	Per 10 ug/m³	+6.4% (-1.0 to 14.2)	1.064 (0.991-1.143)
S08	Howard et al.[29]	Per 10 ug/m³	IRR 1.13 (1.06-1.20)	1.130 (1.062-1.202)
S03	Chen et al.[30]	Per 5 ug/m³	+6.8% (5.0-8.7)	1.141 (1.102-1.181)
S07	Stowell et al.[23]	Per 1 ug/m³	OR 1.018 (1.004-1.032)*	1.195 (1.041-1.373)
S16	Cancado et al.[31]	Per 10.2 ug/m³	+21.4% (4.3-38.5)	1.209 (1.052-1.390)
S12	Lan et al.[32]	Per 10 ug/m³	OR 1.21 (1.15-1.28)	1.210 (1.147-1.277)
S11	Willis et al.[33]	Per 10 ug/m³	RR 1.35 (1.14-1.60)	1.350 (1.140-1.599)
S01	Li et al.[34]	Per 1 ug/m³	+3.2% (0.2-6.2)	1.370 (1.024-1.833)
S15	Mahsin et al.[35]	Per 10 ug/m³	RR 1.57 (1.21-2.02)	1.570 (1.215-2.029)
— CATEGORICAL EXPOSURE-CONTRAST POOL (k = 5)				
S18	Requia et al.[36]	Wildfire wave vs reference	OR 1.21 (1.07-1.34)	1.210 (1.081-1.354)
S10	Ziou et al.[37]	Exposed vs no or minimal exposure	OR 1.39 (1.05-1.83)	1.390 (1.053-1.835)
S05	Hutchinson et al.[38]	Peak fire period vs reference	RR 1.77 (1.28-2.45)	1.770 (1.279-2.449)
S09	Lee et al.[39]	Exposed area, fire period vs control	RR 2.48 (2.28-2.70)	2.480 (2.279-2.699)
S13	Chakrabarti et al.[40]	Top vs lowest burning quintile	RR 3.65 (3.06-4.34)	3.650 (3.065-4.347)
— NARRATIVE SYNTHESIS ONLY (k = 1)				
S04	Landguth et al.[24]	Per 1 ug/m³, cumulative lag	OR 1.37 (1.12-1.65)	1.370 (1.129-1.663)

*Effects reported as a percentage change were converted as RR = 1 + (percentage change / 100). For studies in the categorical block no rescaling applies and the final column reproduces the published contrast on the log-symmetric scale used in the model, which can differ from the published bounds in the third decimal place. * The published upper bound for this study is internally impossible and was reconstructed under log -symmetry; see section 2.3.1. CI, confidence interval; IRR, incidence rate ratio; OR, odds ratio; RR, relative risk.*

Risk of bias

Risk-of-bias judgements are displayed study by study in Figure 2 and summarised by domain in Table 3. On ROBINS-E, two studies were rated Low overall, twelve raised Some concerns and four were rated High. As Table 3 shows, the domains that drove the overall judgements were measurement of the outcome, in which three studies (17%) were High and seven (39%) raised some concerns, and measurement of the exposure, in which two studies (11%) were High and ten (56%) raised some concerns. Confounding was rated Low in ten studies (56%), reflecting the predominance of self-matched case-crossover and time-stratified designs in which time-invariant confounding is removed by design. The domain for post-exposure interventions was rated Low in all eighteen studies; this reflects the structure of environmental exposure research, in which no intervention is administered between exposure and outcome, rather than any particular methodological strength of the included studies, and it should not be read as a quality finding.

On the Newcastle-Ottawa Scale the same eighteen studies scored between seven and nine stars with a median of 7.5, giving nine studies of Good and nine of Fair quality and none of Poor quality, as the final column of Table 1 records. Mapped level for level, the two instruments agreed on seven of eighteen studies. The disagreement was systematic rather than contradictory: the Newcastle-Ottawa Scale rewards sampling frame and design and has no floor for outcome misclassification, so studies rated High on ROBINS-E precisely because their outcomes were not infection-specific or were parent-reported nonetheless retained seven stars. The two instruments were nevertheless directionally consistent, studies rated High on ROBINS-E averaging 7.0 of 9 stars against 7.9 for the remainder. This divergence is itself a finding of methodological interest: reviews in this literature that have relied on the Newcastle-Ottawa Scale alone will have systematically understated the risk arising from outcome misclassification, which is the dominant weakness of the evidence base.

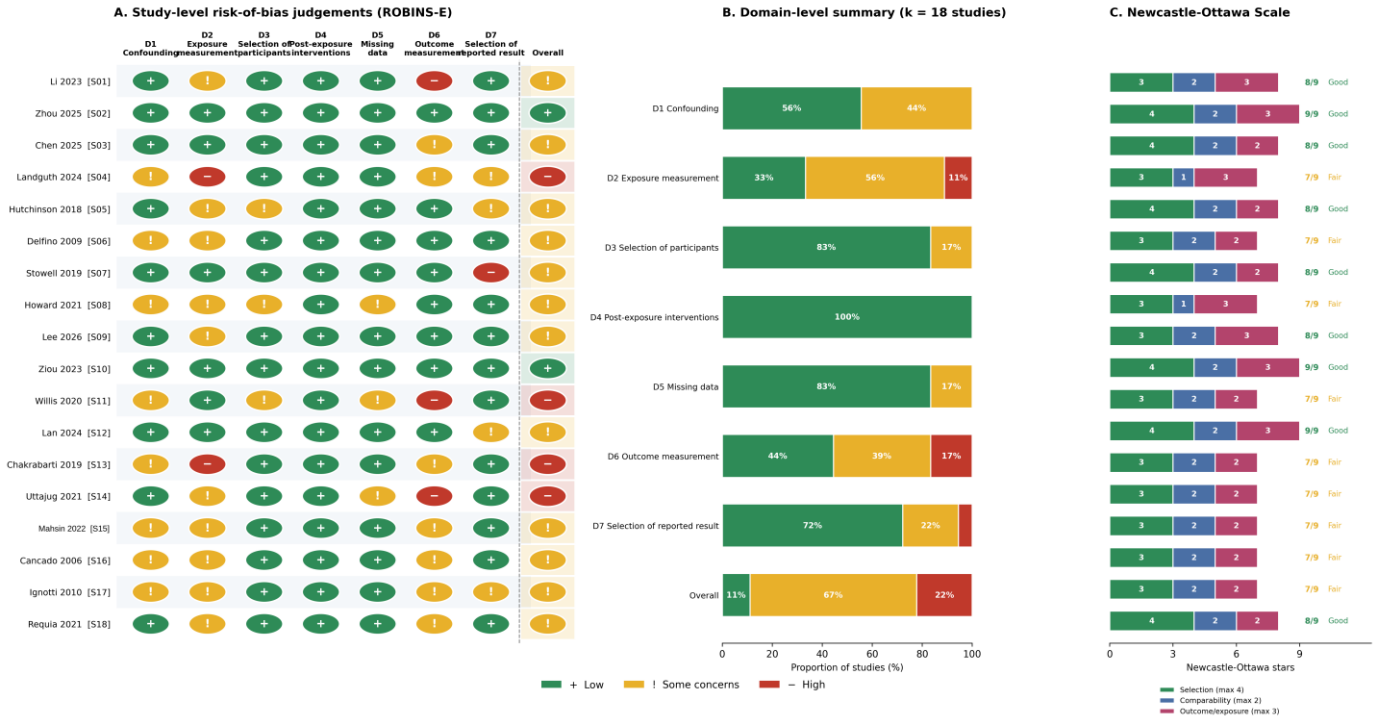

Figure 2. Risk of bias in the eighteen included observational studies. Panel A, study-level ROBINS-E judgements across the seven domains. Panel B, domain-level summary. Panel C, Newcastle-Ottawa Scale stars by domain. Studies appear in the same order as in Table 1.

Table 3. Domain-level distribution of ROBINS-E judgements across the eighteen included studies.

ROBINS-E domain	Low	Some concerns	High
D1 Confounding	10 (56%)	8 (44%)	0 (0%)
D2 Measurement of the exposure	6 (33%)	10 (56%)	2 (11%)
D3 Selection of participants	15 (83%)	3 (17%)	0 (0%)
D4 Post-exposure interventions	18 (100%)	0 (0%)	0 (0%)
D5 Missing data	15 (83%)	3 (17%)	0 (0%)
D6 Measurement of the outcome	8 (44%)	7 (39%)	3 (17%)
D7 Selection of the reported result	13 (72%)	4 (22%)	1 (6%)
Overall ROBINS-E judgement	2 (11%)	12 (67%)	4 (22%)

Values are numbers of studies with the percentage of the eighteen included studies in parentheses.

Primary meta-analysis

In the primary continuous dose-response pool of twelve studies, each 10 ug/m^3 increment in fire-related particulate matter was associated with a relative risk of acute respiratory infection in children of 1.104 (95% CI 1.070 to 1.139; z = 6.22; p < 0.001), as the upper panel of Figure 3 and the first row of Table 4 show. The equivalent standardised effect in the Cohen metric was a Hedges' g of 0.055 (95% CI 0.038 to 0.072), a small effect by conventional benchmarks. Heterogeneity was substantial (Q = 137.8 on 11 degrees of freedom; p < 0.001; I^2 92%; tau^2 0.0016). The 95% prediction interval was 1.015 to 1.201, excluding the null, which indicates that a positive association would be expected in a new setting drawn from the same distribution of studies; the caveat attaching to that interval is discussed in section 3.6. Under the co-primary Hartung-Knapp-Sidik-Jonkman specification the pooled estimate was unchanged at 1.104 with a wider interval of 1.042 to 1.171 (p = 0.003), so the conclusion did not depend on the anticonservative DerSimonian-Laird interval.

Restricting the pool to the eight studies whose outcome was infection-specific gave a larger estimate of 1.148 (95% CI 1.067 to 1.234), shown in the second row of Table 4. This is the more precise answer to the review's question as posed, and it is reported alongside the inclusive estimate throughout; the inclusive figure of 1.104 is retained as the primary result because it uses the whole eligible evidence base and because outcome specificity was not a statistically significant modifier, as Table 5 records.

Study-specific estimates in the continuous pool ranged from 1.010 (95% CI 1.000 to 1.020) in the Thai case-crossover analysis of burning-day particulate matter[25] to 1.570 (95% CI 1.215 to 2.029) in the Canadian before-during-after analysis of a remote wildfire smoke episode,[35] both visible at the extremes of the upper panel of Figure 3 and tabulated in Table 2. Only one continuous-pool estimate crossed the null, the pneumonia analysis of the 2003 southern California wildfires at 1.064 (0.991 to 1.143).[28] In the secondary categorical pool of five studies, shown in the lower panel of Figure 3 and the final row of Table 4, exposure to a fire episode or residence in a high-exposure area was associated with a relative risk of 1.940

(95% CI 1.269 to 2.967; Hartung-Knapp 1.108 to 3.399; p = 0.002; Hedges' g equivalent 0.366, 95% CI 0.132 to 0.601), with extreme heterogeneity (I^2 97%) and a 95% prediction interval of 0.701 to 5.371. That prediction interval includes the null, which means that although the average effect across these five studies was positive, the effect in a further study of the same kind could plausibly be null or even protective. The categorical result is therefore not a reliable guide to what would be observed in a new fire episode, and section 3.8 examines it further.

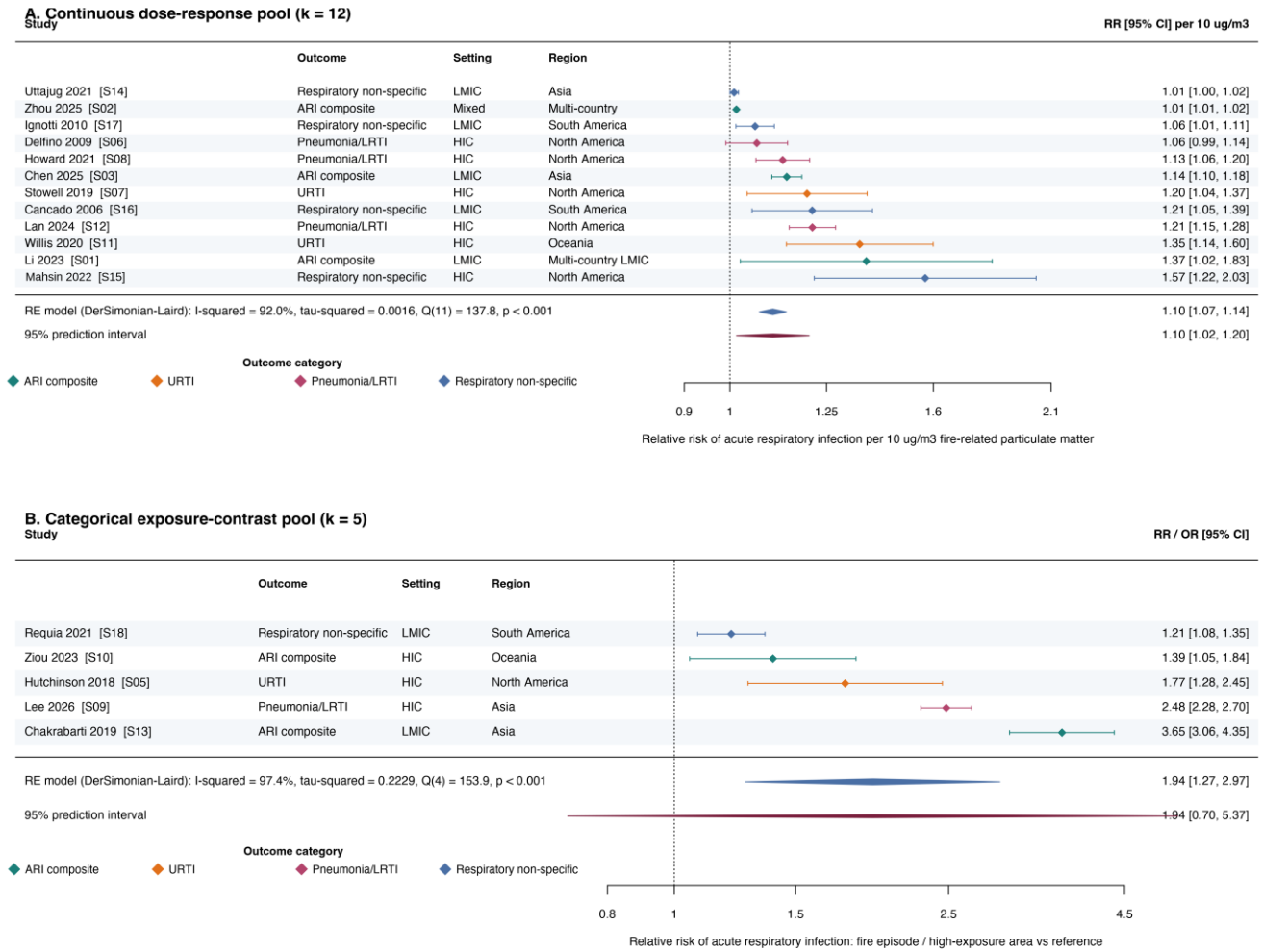

Figure 3. Forest plot of the association between fire-related particulate exposure and acute respiratory infection in children. Panel A, continuous dose-response pool expressed per 10 ug/m³ (k = 12). Panel B, categorical exposure-contrast pool (k = 5). Diamonds show random-effects pooled estimates and the lower bar the 95% prediction interval. Marker colour denotes outcome category. Plotted values are given to three decimal places in the final column of Table 2.

Table 4. Pooled estimates for the primary, restricted and secondary analyses.

Analysis	k	Pooled RR (95% CI)	Hartung-Knapp 95% CI	Hedges' g equivalent (95% CI)	I^2 (%)	tau^2	95% prediction interval	p
Primary: continuous dose-response pool, per 10 ug/m³	12	1.104 (1.070-1.139)	1.042-1.171	0.055 (0.038-0.072)	92.0	0.0016	1.015-1.201	<0.001
Restricted to infection-specific outcomes	8	1.148 (1.067-1.234)	Not fitted	Not fitted	93.9	0.0083	Not fitted	<0.001
Restricted to Newcastle-Ottawa Good quality	5	1.148 (1.042-1.264)	Not fitted	Not fitted	95.7	0.0094	Not fitted	0.005
Trim-and-fill adjusted (illustrative)	12 + 5 imputed	1.067 (1.036-1.099)	Not fitted	Not fitted	Not applicable	Not applicable	Not fitted	<0.001
Secondary: categorical exposure contrast	5	1.940 (1.269-2.967)	1.108-3.399	0.366 (0.132-0.601)	97.4	0.2229	0.701-5.371	0.002

Relative risks in the continuous analyses are expressed per 10 ug/m³ of fire-related particulate matter. The Hedges' g equivalent is the Chinn transformation ln(RR) / 1.81 and is presented for comparability only. CI, confidence interval; RR, relative risk.

Subgroup analyses

Nine subgroup variables were tested and two produced statistically significant between-group differences, as Table 5 sets out in full. Age band was the stronger of the two: the pooled relative risk was 1.202 (95% CI 1.104 to 1.309) in studies reporting an under-five stratum, 1.110 (1.038 to 1.186) in studies of children aged five to nineteen years, and 1.018 (1.001 to 1.035) in studies that reported an unstratified 0-19 year population, with a between-group Q of 23.60 on two

degrees of freedom ($p < 0.001$). Exposure window was the second: prenatal exposure was associated with a relative risk of 1.239 (1.134 to 1.354) against 1.077 (1.046 to 1.108) for postnatal exposure, with a between-group Q of 11.03 on one degree of freedom ($p < 0.001$). Heterogeneity fell markedly within the prenatal subgroup (I^2 31%) and within the upper respiratory tract infection subgroup (I^2 16%), suggesting that part of the overall heterogeneity is attributable to the mixing of exposure windows and outcome definitions.

Income setting did not modify the association to a statistically significant degree, the pooled estimate being 1.189 (1.106 to 1.278) in high-income and 1.102 (1.022 to 1.189) in low- and middle-income settings ($p = 0.151$). As Table 5 shows, outcome category, study design, geographical region, reported increment class, Newcastle-Ottawa quality and outcome infection-specificity were likewise not significant modifiers.

Table 5. Pre-specified subgroup analyses in the continuous dose-response pool.

Subgroup variable	Level	k	Pooled RR (95% CI)	I^2 (%)	p for interaction
Income setting	Low- and middle-income	5	1.102 (1.022-1.189)	92.9	0.151
	High-income	6	1.189 (1.106-1.278)	71.1	
Age band	0-19 years (not stratified)	4	1.018 (1.001-1.035)	75.7	<0.001
	Under 5 years	6	1.202 (1.104-1.309)	81.1	
	5-19 years	2	1.110 (1.038-1.186)	66.1	
Outcome category	Respiratory, not infection-specific	4	1.100 (1.014-1.194)	86.2	0.453
	Acute respiratory infection, composite	3	1.104 (0.988-1.233)	95.7	
	Pneumonia or lower respiratory infection	3	1.136 (1.057-1.222)	75.9	
	Upper respiratory tract infection	2	1.257 (1.118-1.414)	15.8	
Exposure window	Postnatal	10	1.077 (1.046-1.108)	89.7	<0.001
	Prenatal	2	1.239 (1.134-1.354)	31.4	
Study design	Case-crossover	4	1.123 (1.013-1.245)	94.3	0.526
	Time-series	5	1.085 (1.019-1.154)	82.2	
	Cohort	2	1.213 (1.023-1.438)	73.2	
Geographical region	Asia	2	1.072 (0.952-1.208)	97.7	0.428
	South America	2	1.113 (0.982-1.261)	68.1	
	North America	5	1.169 (1.086-1.258)	71.8	
Reported increment class	Per 5 ug/m³ or more	10	1.097 (1.063-1.132)	93.0	0.115
	Per 1 ug/m³	2	1.226 (1.082-1.389)	0.0	
Newcastle-Ottawa quality	Fair (7 stars)	7	1.122 (1.051-1.197)	86.8	0.850
	Good (8-9 stars)	5	1.148 (1.042-1.264)	95.7	
Outcome infection-specificity	Not infection-specific	4	1.100 (1.014-1.194)	86.2	0.681
	Infection-specific	8	1.148 (1.067-1.234)	93.9	

Relative risks are expressed per 10 ug/m³ of fire-related particulate matter. The p value for interaction is the between-group Q test and is reported once per subgroup variable. Nine variables were tested and no correction for multiplicity was applied. One study contributing a mixed income setting is not represented in the income stratification, and the two multi-country studies are not represented in the regional stratification. Newcastle-Ottawa quality is the two-level star-based rating defined in section 2.4 and is not the ROBINS-E overall judgement of Table 3.

The age gradient examined within studies

The subgroup comparison above is between studies rather than within them, and is therefore open to confounding by study-level characteristics such as setting, exposure metric and outcome definition. Three included studies reported more than one age stratum internally, allowing the age gradient to be inspected within a fixed design, population and outcome definition; their internal contrasts are set out in Table 6. The within-study evidence is not uniform. In the seven-country time-series the risk of an infectious-disease admission was slightly lower in children aged 0-4 years than in those aged 5-9 years,[26] the opposite of the between-study gradient. In the Canadian subarctic cohort the pneumonia estimate was higher in the under-fives than in the 5-19 year group,[29] and in the Indian cross-sectional analysis the under-five estimate exceeded the all-age estimate.[40] Two of three internal contrasts in Table 6 therefore support greater susceptibility in the youngest children and one contradicts it.

Table 6. Age contrasts reported within individual studies.

Study	Outcome	Younger stratum	Older or all-age stratum	Direction
Zhou et al.[26]	Infectious-disease hospital admission	0-4 y: RR 1.013 (1.008-1.017) per 10 ug/m³	5-9 y: RR 1.025 (1.017-1.033) per 10 ug/m³	Older stratum higher
Howard et al.[29]	Pneumonia, emergency visits	<5 y: IRR 1.13 (1.06-1.20) per 10 ug/m³	5-19 y: IRR 1.09 (0.99-1.19) per 10 ug/m³	Younger stratum higher
Chakrabarti et al.[40]	Acute respiratory infection	<5 y: RR 3.65 (3.06-4.34), top vs lowest quintile	All ages: RR 2.99 (2.77-3.23), top vs lowest quintile	Younger stratum higher

Estimates are as reported by each study on its own exposure scale and are not harmonised; they are presented to show the internal direction of the age gradient and are not pooled.

Taken together, the between-study contrast of Table 5 is large and highly significant, the within-study evidence of Table 6 is supportive but mixed, and the intermediate position of the 5-19 year subgroup argues that dilution of a susceptible infant population within an undifferentiated 0-19 year band contributes to, but does not fully explain, the gradient. The age finding is accordingly reported as strongly suggestive rather than as established, and the clinical implications drawn from it in section 4.6 are framed as prioritisation under uncertainty rather than as a demonstrated differential effect.

Sensitivity analyses

The primary estimate was robust, as Table 7 documents across seventeen specifications. Across twelve leave-one-out analyses the pooled relative risk ranged from 1.085 (1.054 to 1.117), on omission of the Canadian prenatal case-control study,[32] to 1.153 (1.086 to 1.226), on omission of the seven-country time-series;[26] every one of the twelve remained statistically significant. Eight structural exclusions were performed. Removing the two studies of the Hazelwood coal-mine fire, which is a landscape-scale smoke event but not a vegetation fire, gave 1.097 (1.063 to 1.131). Restricting to infection-specific outcomes gave 1.148 (1.067 to 1.234). Restricting to the five studies rated Good on the Newcastle-Ottawa Scale gave 1.148 (1.042 to 1.264), a point estimate higher than the primary result, indicating that the overall estimate is not inflated by methodologically weaker studies. Adding back the study excluded from the dose-response pool at its rescaled per-10 ug/m³ scale moved the estimate only to 1.111 (1.076 to 1.147).

Two of the structural exclusions in Table 7 bear directly on the coherence of the exposure metric. Removing the single study that used particulate matter below ten micrometres rather than below 2.5 micrometres raised the estimate from 1.104 to 1.153 (1.086 to 1.225), and removing the single study that used total rather than fire-specific particulate matter gave 1.100 (1.065 to 1.136). The first of these is a large movement for a single exclusion and shows that the pollutant metric matters materially; the second shows that fire-specificity of the exposure metric does not. A pool restricted simultaneously on both criteria was not fitted, and readers should not infer a combined value from the two separate exclusions.

Six alternative estimators of the between-study variance were fitted, in the final block of Table 7. Restricted maximum likelihood, Paule-Mandel, Sidik-Jonkman and empirical Bayes estimators all produced larger point estimates than DerSimonian-Laird, between 1.137 and 1.146, so the primary specification is the conservative choice for the pooled effect. It is, however, the least conservative choice for the prediction interval, because the prediction interval widens as the estimated between-study variance grows. The prediction interval reported in section 3.4 was computed under the DerSimonian-Laird variance of 0.0016, which was the smallest of the six estimates; under restricted maximum likelihood (tau² 0.0074) or Paule-Mandel (tau² 0.0099) it would be wider, and whether it would still exclude the null was not determined. The claim that a positive association would be expected in a new setting therefore rests on the DerSimonian-Laird specification and should be treated with corresponding caution.

Publication bias and small-study effects

In the continuous pool, which contained more than ten studies, Egger's regression test indicated marked asymmetry (intercept t = 4.93 on 10 degrees of freedom; p = 0.0006). This is not a marginal signal and should not be discounted. The Begg rank correlation was non-significant (Kendall tau = 0.212; p = 0.381), but the two tests are not equally informative at this number of studies: the rank correlation test has low power with twelve studies, and its failure to reject should not be read as evidence against asymmetry. Where the two disagree at this sample size, the regression test is the more reliable.

The contour-enhanced funnel plot in panel A of Figure 4 shows that several of the smaller, less precise studies fell outside the conventional significance contours on the right of the plot. One possible explanation is genuine heterogeneity rather than reporting bias: small studies in this literature are typically conducted in intensely affected populations where the exposure contrast is larger, and the subgroup analysis by increment class in Table 5 is weakly consistent with this, the two studies reporting per-1 ug/m³ increments giving 1.226 (1.082 to 1.389) against 1.097 (1.063 to 1.132) for studies reporting increments of 5 ug/m³ or more. That difference was not statistically significant (p = 0.115) and rests on two studies, so it is offered as a possibility rather than as an established alternative to reporting bias. The restriction of the search to a single indexing service, noted in section 2.1, is a further and more mundane explanation that cannot be excluded.

The Duval and Tweedie trim-and-fill procedure, shown in panel B of Figure 4, imputed five studies into a pool of twelve and produced an adjusted pooled relative risk of 1.067 (95% CI 1.036 to 1.099). Imputing more than a third of the pool is a large intervention and the adjusted figure should be read as an illustration of how far the estimate could move under a specific and strong assumption about the missingness mechanism, not as a corrected estimate. On that reading, the association remains positive and statistically significant after adjustment but its magnitude is attenuated by approximately one third on the logarithmic scale.

Table 7. Sensitivity analyses in the continuous dose-response pool.

Analysis type	Specification	k	Pooled RR (95% CI)	I^2 (%)
Reference model	All studies, DerSimonian-Laird	12	1.104 (1.070-1.139)	92.0
Leave-one-out (range)	Lowest: omitting Lan et al.[32]	11	1.085 (1.054-1.117)	89.8
	Highest: omitting Zhou et al.[26]	11	1.153 (1.086-1.226)	92.2
Structural exclusion	Excluding the non-vegetation (coal-mine) fire study	11	1.097 (1.063-1.131)	92.1
	Excluding outcomes that were not infection-specific	8	1.148 (1.067-1.234)	93.9
	Excluding exposure that was not fire-specific	11	1.100 (1.065-1.136)	92.1
	Excluding the prenatal exposure window	10	1.077 (1.046-1.108)	89.7
	Excluding the PM_{10} metric	11	1.153 (1.086-1.225)	92.6
	Excluding per-1 ug/m^3 reported increments	10	1.097 (1.063-1.132)	93.0
	Restricted to Newcastle-Ottawa Good quality	5	1.148 (1.042-1.264)	95.7
	Adding back the narrative study, rescaled to per 10 ug/m^3	13	1.111 (1.076-1.147)	91.8
Alternative model	Restricted maximum likelihood	12	1.137 (1.074-1.203)	98.2
	Paule-Mandel	12	1.143 (1.072-1.219)	98.6
	Sidik-Jonkman	12	1.146 (1.070-1.228)	98.8
	Empirical Bayes	12	1.143 (1.072-1.219)	98.6
	DerSimonian-Laird with Hartung-Knapp interval	12	1.104 (1.042-1.171)	92.0
	Fixed effect (inverse variance)	12	1.018 (1.014-1.022)	Not applicable

Relative risks are expressed per 10 ug/m^3 of fire-related particulate matter. Newcastle-Ottawa Good quality denotes eight or nine stars, as defined in section 2.4.

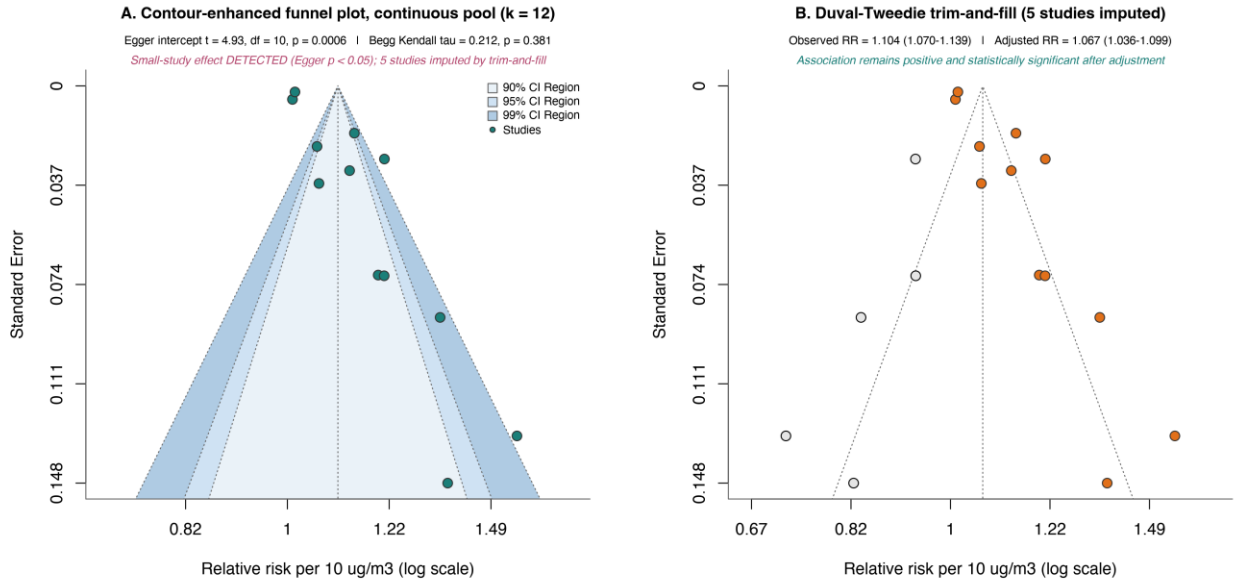

Figure 4. Small-study-effect diagnostics for the continuous dose-response pool. Panel A, contour-enhanced funnel plot with 90%, 95% and 99% significance contours. Panel B, Duval and Tweedie trim-and-fill, with the five imputed studies shown unfilled and the illustrative adjusted pooled estimate reported.

The categorical pool examined further

Because the categorical pool produced a large pooled estimate with a prediction interval that included the null, eleven alternative specifications were fitted; all eleven are listed in Table 8 and five are displayed in panel B of Figure 5. The pooled estimate was stable at 1.94 under every estimator of the between-study variance, and remained statistically significant under the Hartung-Knapp-Sidik-Jonkman interval (1.94, 95% CI 1.11 to 3.40). As the final column of Table 8 records, however, no specification produced a prediction interval that excluded the null, including the Riley interval appropriate to a small number of studies, and including every restriction by fire type, contrast type and outcome specificity.

Influence diagnostics are shown in panel A of Figure 5. The Indian cross-sectional analysis of crop-residue burning[40] made by far the largest contribution to heterogeneity, with a Baujat abscissa of 1.72 against 1.00 for the next study, and the Brazilian nationwide case-crossover[36] carried the largest single influence on the pooled estimate on the Baujat ordinate. On Cook's distance, which weights both quantities together, only the Indian analysis exceeded the conventional threshold, at 0.552 with a studentised residual of 1.556, against 0.380 for the Brazilian study. Omitting the Indian analysis reduced the pooled estimate to 1.652 (1.053 to 2.592) under DerSimonian-Laird and to 1.656 (0.985 to 2.785) under the preferred Paule-Mandel with Hartung-Knapp specification of Table 8, in which significance is lost; omitting the Korean quasi-experimental

study[39] gave 1.818 (0.999 to 3.307; p = 0.050). A post-hoc split by contrast type was significant, discrete fire episodes yielding 1.656 and the sustained seasonal burning-intensity gradient yielding 3.65 (between-group Q = 4.83 on one degree of freedom; p = 0.028), but this is hypothesis-generating only. The categorical pool is accordingly reported as a supporting direction-of-effect analysis and not as a standalone estimate of risk during fire episodes.

Table 8. Eleven alternative specifications of the categorical exposure-contrast pool.

Specification	k	Pooled RR (95% CI)	95% prediction interval	Prediction interval excludes the null
DerSimonian-Laird, z-based interval (primary model)	5	1.94 (1.27-2.97)	0.70-5.37	No
DerSimonian-Laird with Hartung-Knapp interval	5	1.94 (1.11-3.40)	0.47-8.07	No
Paule-Mandel, z-based interval	5	1.94 (1.31-2.89)	0.75-5.01	No
Paule-Mandel with Hartung-Knapp interval (preferred at k = 5)	5	1.94 (1.11-3.40)	0.51-7.43	No
Restricted maximum likelihood with Hartung-Knapp interval	5	1.94 (1.11-3.40)	0.50-7.51	No
Sidik-Jonkman with Hartung-Knapp interval	5	1.94 (1.11-3.40)	0.51-7.40	No
Paule-Mandel with Hartung-Knapp interval, Riley prediction interval	5	1.94 (1.11-3.40)	0.42-9.04	No
Discrete fire episodes only (excludes the crop-residue gradient study)	4	1.66 (0.98-2.78)	0.55-5.02	No
Vegetation fires only (excludes the coal-mine fire study)	4	2.10 (0.99-4.47)	0.40-11.06	No
Infection-specific outcomes only	4	2.20 (1.13-4.30)	0.52-9.35	No
Discrete vegetation-fire episodes only	3	1.75 (0.70-4.37)	0.29-10.38	No

HKSJ, Hartung-Knapp-Sidik-Jonkman. The pooled estimate is stable across estimators; no specification yields a prediction interval that excludes the null.

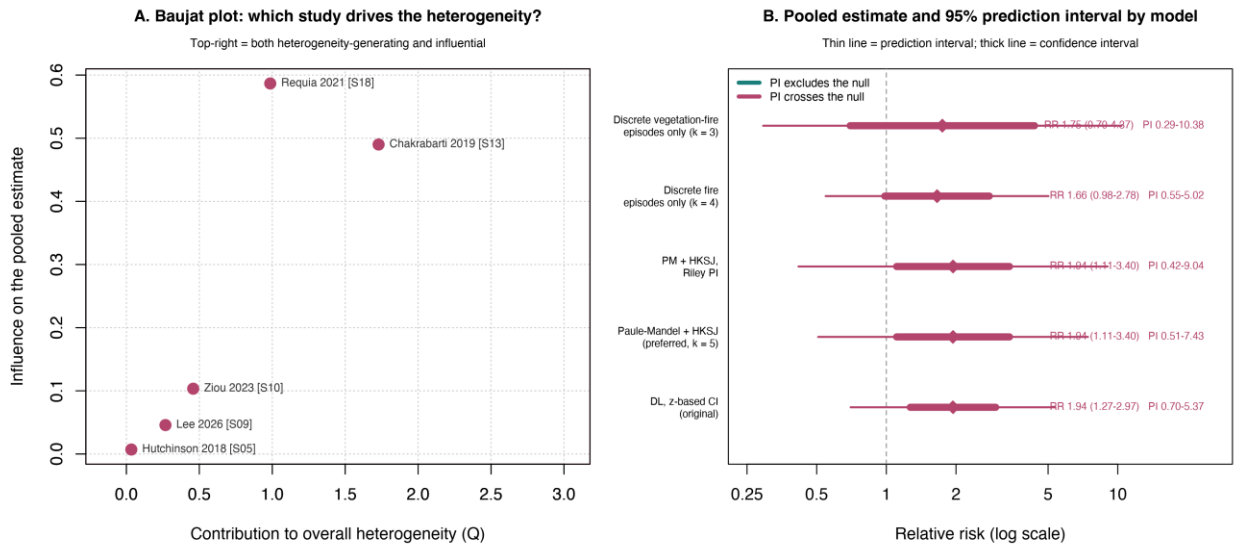

Figure 5. Diagnostics for the categorical exposure-contrast pool. Panel A, Baujat plot of each study's contribution to heterogeneity against its influence on the pooled estimate. Panel B, pooled estimate and 95% prediction interval under five of the eleven specifications listed in Table 8; the prediction interval crosses the null in all of them.

4. Discussion

Principal findings

This meta-analysis found that each 10 ug/m³ increment in fire-related particulate matter was associated with a 10.4% increase in the risk of acute respiratory infection in children, rising to 14.8% when the analysis was restricted to studies with infection-specific outcomes, as Table 4 records, and that the increase was concentrated in the youngest children and after prenatal exposure. The magnitude of the pooled effect is small in standardised terms — a Hedges' g of 0.055 — but the exposure to which it applies is not rare. Fire-attributable concentrations during the episodes captured by the included studies frequently exceeded the 10 ug/m³ unit of the pooled estimate several times over: the San Diego analysis reported a mean 24-hour concentration of 89.1 ug/m³ across its peak five-day fire period, with a maximum above 800 ug/m³,[38] and the Canadian subarctic cohort reported a median of 30.8 ug/m³ across an entire smoke season against approximately fivefold lower values in control years.[29] A small relative risk applied across an entire paediatric population, at concentrations several multiples of the unit increment and sustained for weeks, translates into a substantial absolute excess of clinic attendances, emergency presentations and hospital admissions.

The finding that the association was strongest in children under five years, with a pooled relative risk of 1.202 against 1.018 in unstratified paediatric populations, is the single most policy-relevant result of this review. It is also the finding that the previous quantitative synthesis explicitly identified as missing when it called for a focus on extremely young children.[17] Two interpretations are compatible with the data. The first is genuine biological susceptibility: alveolarisation continues through the first years of life, the immune repertoire is immature, and the ratio of minute ventilation to body mass is at its lifetime maximum. The second is a statistical artefact of dilution, since a study reporting an undifferentiated 0-19 year estimate necessarily averages a susceptible infant population with a much less susceptible adolescent one. As Table 6 showed, the within-study evidence is supportive but mixed, with two of three internal age contrasts favouring the younger stratum and one favouring the older. The honest summary is that the age gradient is likely to be real but is not established by these data alone.

The prenatal finding deserves particular emphasis, and its strength lies less in the between-group Q test of Table 5 — which compares two studies against ten and therefore has limited power — than in the qualitative convergence of the two contributing studies. They examined quite different populations and outcomes: a British Columbian nested case-control study of physician-billed lower respiratory infection in the first year of life, keyed to the canalicular stage of lung development,[32] and an Australian prospective cohort of parent-reported doctor-diagnosed upper respiratory infection assessed two to four years after exposure.[33] Their pooled estimate nonetheless showed low heterogeneity (I^2 31%). Convergence between two such dissimilar designs on a common effect size is more persuasive than either study alone or than the formal interaction test, and it points towards a developmental mechanism in which exposure in utero alters airway or immune development in a way that persists into childhood. That interpretation is supported by evidence outside this review: prenatal air pollution has been shown to alter the maternal proteome in a pattern that predicts higher risk of infection, colds, pneumonia and fever in the first three years of life,[41] to interact with climatic exposures in raising infant respiratory disease risk,[42] and to be associated, less consistently, with bronchiolitis admission in a birth cohort of more than 400,000 children;[43] early-gestation wildfire-specific particulate exposure has also been linked to altered infant lung function.[44]

Outcome measurement is the dominant source of heterogeneity

The most important methodological observation arising from this review is that the variability between studies is driven principally by how the outcome was measured rather than by how the exposure acted. The composite acute respiratory infection category combines maternal two-week recall of cough with rapid breathing, notifiable-disease surveillance, physician billing codes, emergency department diagnoses coded to two different revisions of the International Classification of Diseases, and all-cause respiratory hospital admission, as the outcome column of Table 1 makes plain. These are not interchangeable measurements of the same event, and the data show it: within the upper respiratory tract infection subgroup of Table 5, where the outcome construct is comparatively uniform, I^2 was 16%, whereas within the composite acute respiratory infection category it was 96%. A sixfold difference in within-subgroup heterogeneity between a narrow and a broad outcome definition is difficult to attribute to anything other than measurement.

This has two consequences for how the present results should be read. First, the pooled estimate for the composite outcome is an average across measurement instruments as much as across populations, and its confidence interval understates the uncertainty that a clinician should attach to it in any single setting. Second, the infection-specific estimate of 1.148 is preferable on construct-validity grounds even though it rests on fewer studies, and it is for that reason reported in the abstract alongside the inclusive figure. The composite estimate is retained as primary because it uses the whole eligible evidence base and because outcome specificity was not a significant modifier in formal testing, but readers who wish to know the effect on acute respiratory infection as a clinician would define it should take 1.148 (1.067 to 1.234) as the answer.

Comparison with previous syntheses and with the wider literature

The pooled estimate reported here is directly comparable with only one previous figure. The most recent meta-analysis of wildfire smoke and child health reported a pooled relative risk of 1.13 (95% CI 1.05 to 1.23) for upper respiratory infection.[17] In the present analysis the upper respiratory tract infection subgroup of Table 5 gave 1.257 (95% CI 1.118 to 1.414), somewhat higher but with overlapping intervals, and the overall composite estimate of 1.104 sits slightly below it. That earlier synthesis reported a null pooled estimate for all-cause respiratory morbidity (1.04, 0.96 to 1.12) and for bronchitis (0.93, 0.85 to 1.03), which contrasts with the significant positive estimates found here for respiratory outcomes that were not infection-specific (1.100, 1.014 to 1.194) and for pneumonia and lower respiratory infection (1.136, 1.057 to 1.222). The most likely explanation for the divergence is the eight studies published since that synthesis was assembled, several of them very large, together with the present review's deliberate separation of continuous from categorical exposure contrasts.

The systematic review restricted to respiratory outcomes in children and adolescents[18] included five studies, all from the United States and Canada, and reported by vote counting that all five showed increases in respiratory symptoms, healthcare visits and medication use during smoke days. That directional conclusion is confirmed and quantified here, and the geographical restriction that the authors named as a limitation is addressed by the inclusion of studies from India, China, Thailand, Brazil, Korea and a 48-country low- and middle-income country pool, as Table 1 shows. The review of wildfire-related air pollution and infectious disease across all ages[19] reported that none of its thirty included studies had performed a child-specific subgroup analysis; the present review is, to our knowledge, the first to supply one.

The wider literature on fire smoke and infection in adults is consistent with the direction reported here, and it addresses the most important rival explanation for the whole analysis — that what is being detected is misclassified exacerbation of pre-existing airway disease rather than genuine infection. Wildfire-season particulate matter has been associated with increased influenza and influenza-like illness risk across the western United States,[45] and a nationwide Brazilian case-crossover of 374,110 influenza cases found a 13.2% increase in influenza associated with a 3.4 ug/m^3 rise in wildfire-related $PM_{2.5}$ over a seven-day lag.[46] Wildfire smoke has likewise been associated with SARS-CoV-2 test positivity,[47] with coronavirus disease 2019 case and death counts,[48] and with post-wildfire coronavirus disease 2019 hospitalisation risk.[49] That these associations are observed for specific, laboratory-confirmable infections, with biologically plausible lags, strengthens the case that the association found here reflects increased

susceptibility to infection. Asthma remains the better-documented respiratory consequence of wildfire smoke, with recent paediatric[50] and population-wide[51] analyses reporting substantial increases in asthma presentations during smoke episodes, and with multi-pollutant analyses separating the contributions of $PM_{2.5}$, PM_{10} and ozone;[52] the present review indicates that infection should be regarded as a parallel rather than a subordinate concern.

Heterogeneity and what did not explain it

Heterogeneity was substantial in the continuous pool (I^2 92%) and extreme in the categorical pool (I^2 97%). Three sources appear to account for most of it: age composition, which was a statistically significant modifier in Table 5; exposure window, which reduced heterogeneity to 31% within the prenatal stratum; and outcome definition, discussed in section 4.2. A fourth, the pollutant metric, was not formally tested as a subgroup but was shown in the structural sensitivity analyses of Table 7 to move the pooled estimate by a substantial margin when the single study using particulate matter below ten micrometres was removed.

What did not explain heterogeneity is equally notable. Income setting, geographical region, study design and Newcastle-Ottawa quality were all non-significant modifiers, as Table 5 records. The absence of an income-setting effect argues against the intuition that children in low- and middle-income countries would show larger relative risks; the point estimate was in fact marginally higher in high-income settings, which most likely reflects better exposure resolution and more specific outcome ascertainment there rather than lesser biological vulnerability in low- and middle-income settings. Because the prediction interval of the continuous pool excluded the null under the primary specification, the heterogeneity encountered here is heterogeneity of magnitude rather than of direction, subject to the caveat in section 3.6 regarding the choice of variance estimator.

Limitations

Several limitations qualify these findings. They fall into five groups: one concerning a single reconstructed statistic, one concerning the exposure, one concerning the outcome, one concerning the analytical model, and one concerning the conduct and reach of the review itself.

The first is specific and has already been described in full. One published confidence interval was internally impossible and its upper bound was reconstructed under log-symmetry, and the child-specific estimate concerned derives from a sensitivity model rather than from that study's primary model; both issues are set out in section 2.3.1. The pooled estimate is essentially unchanged when the study is removed, so the consequence for the result is negligible, but the episode illustrates how fragile a synthesis becomes when it must rely on published summary statistics without access to the underlying data.

The exposure is the second and larger concern. Misclassification is likely to be pervasive and, in most of the designs used here, non-differential, which would bias estimates towards the null; the true dose-response relationship may therefore be steeper than the pooled estimate suggests. As Table 1 records, twelve studies used modelled fire-attributable particulate concentrations assigned at the community, postal-area or district level rather than measured personal exposure, two used total particulate matter during a fire season as a proxy for fire smoke, and four used a categorical episode or satellite fire-intensity indicator carrying no concentration information at all. Compounding this, the exposure metric is not uniform even within the continuous pool, which mixes fine particulate matter with particulate matter below ten micrometres, and fire-attributable with total concentrations. The pooled quantity is consequently an average over physically distinct exposures rather than a single well-defined one, and the size of that problem is visible in Table 7: removing the one study that used the coarser metric moved the estimate from 1.104 to 1.153.

The outcome is the third concern, and measurement of the outcome was the ROBINS-E domain that most often drove a High rating, as Table 3 shows. Six of the eighteen studies reported a respiratory outcome that was not infection-specific, two relied on caregiver recall of symptoms rather than a clinical diagnosis, and one relied on parent-reported doctor diagnoses recorded in monthly diaries. One study's outcome comprised notifiable respiratory transmitted diseases including measles and rubella,[30] which are systemic viral illnesses with respiratory transmission rather than acute respiratory infections in the sense used elsewhere in this review; it was retained because the category is dominated by seasonal influenza, and it was pre-specified as a leave-one-out candidate, its removal moving the pooled estimate to 1.091 (1.059 to 1.125). A related difficulty arises on the effect scale rather than the diagnostic one. Relative risks, odds ratios and incidence rate ratios were pooled together as a single ratio measure, and where the outcome is common in the reference group, as upper respiratory infection is in young children, an odds ratio will overstate the corresponding relative risk. The pooled estimate may therefore be slightly upward-biased, and that assumption was not tested.

The fourth group concerns the model. Small-study effects were formally detected by Egger's test, and although the trim-and-fill adjusted estimate remained significant it was attenuated by roughly a third, more than a third of the pool had to be imputed to produce it, and the restriction of the search to a single bibliographic database is an additional and unquantified contributor to any such asymmetry. Separately, harmonisation to a common 10 ug/m^3 increment assumes a log-linear exposure-response relationship across the observed exposure range. That assumption is unlikely to hold exactly. It was the reason one study reporting a large per-1 ug/m^3 effect had to be excluded from the dose-response pool altogether, and the increment-class subgroup of Table 5, in which studies reporting per-1 ug/m^3 increments gave 1.226 against 1.097 for studies reporting larger increments, points in the direction that a failure of log-linearity would predict, although that difference was not statistically significant.

The fifth group concerns confounding and independence across the included studies. Confounding by season was not systematically addressed. Fire seasons coincide with the dry season in tropical settings and with late summer in temperate ones, and respiratory infection incidence varies strongly by season for reasons that have nothing to do with smoke. The time-stratified case-crossover and time-series designs that constitute the majority of the pool control for season by matching or by smooth functions of time, but the cross-sectional and before-during-after designs do so less completely, and residual seasonal confounding cannot be excluded in those studies. Nor was population overlap between studies formally assessed: two included studies concern the same Hazelwood cohort[33,37] and two Brazilian time-series analyses cover overlapping regions and periods,[27,31] and pooling them as though they were independent will have overstated precision to an unknown degree.

Finally, the conduct and reach of the review impose their own limits. It was not prospectively registered; screening and risk-of-bias assessment were not performed wholly independently in duplicate, as section 2.3 sets out; and no measure of inter-rater agreement was calculated. No eligible paediatric study was identified for continental Europe, Israel or Japan, so the

findings may not generalise to European populations or to fire regimes unlike those represented here.

Clinical and public health implications for paediatrics

Three implications follow for paediatric practice. First, the age gradient identified here provides an empirical, if not yet conclusive, basis for prioritisation during smoke episodes. Children under five years, and pregnant women whose fetuses are in the canalicular stage of lung development, are the groups in whom protective measures have the strongest supporting evidence. It must be stated plainly that none of the protective measures customarily recommended was evaluated in any included study, and the wider trial evidence is mixed: a cluster-randomised, sham-controlled trial of classroom air purifiers did not reduce the odds of high respiratory-viral exposure although it modestly reduced viral diversity,[53] a randomised intervention trial in children with asthma reduced bedroom PM_{10} by 46% without a corresponding change in settled-dust endotoxin,[54] and a sham-controlled trial in adults with chronic obstructive pulmonary disease lowered exacerbation rates without changing the primary quality-of-life endpoint.[55] Recommending indoor filtration, restriction of outdoor activity or relocation during smoke episodes is therefore an extrapolation from an observational exposure-response association to an intervention that has not been tested against this outcome in this population. What the present data support is the targeting of whatever protective measures a health system chooses to deploy, not the efficacy of any particular measure.

Second, paediatric services in fire-prone regions should anticipate an infection-driven, not merely an asthma-driven, surge. Much existing clinical guidance on wildfire smoke is framed around asthma exacerbation. The present analysis indicates that pneumonia, lower respiratory tract infection and upper respiratory tract infection also rise, with pooled relative risks of similar or greater magnitude, and that the rise extends into the weeks following an episode: one included study reported a persisting pneumonia relative risk of 1.39 in the extended post-fire period.[39] Staffing, antimicrobial stewardship planning and oxygen provision should be scaled accordingly, and clinicians should be alert to the diagnostic overlap between smoke-induced airway irritation and early lower respiratory infection in a young child.

Third, for Indonesia and the wider Southeast Asian region the implications are immediate but the local evidence base is thin. Indonesia is represented in the primary pool only through the 48-country low- and middle-income country analysis,[34] in which Indonesian children are a small fraction of the 36,432 contributing cases. Two Indonesian studies of high contextual relevance could not enter the primary pool. The first is a quasi-experimental analysis of the 2015 forest fires across 203 districts of Sumatra and Kalimantan, which found a significant post-fire increase in respiratory primary-care visits among children under five but did not report an acute-respiratory-infection-specific coefficient in its main text.[56] The second is a case-crossover analysis of peatland-fire haze in Central Kalimantan, which reported a 74.4% increase in respiratory clinic visits during fire haze but was conducted across all ages with no paediatric stratum.[57] The regional burden is not in doubt: the extreme 2019 Indonesian peatland fires were associated with more than 87,000 excess deaths nationally,[58] and eliminating vegetation fires across Southeast Asia has been estimated to avert some 59,000 premature deaths each year, with the benefit falling disproportionately to the poor.[59] Thailand offers a partial model of what quantification can achieve: fire-originated PM_{10} was estimated to account for approximately 130,000 respiratory hospital visits over five years in the upper north, a share that fell from 1.8% to 0.5% of all respiratory visits after a regulatory burning ban,[60] and a large case-crossover study in the south has quantified the effect of transboundary haze on asthma exacerbation by age band.[61]

The single most useful contribution that Indonesian paediatric research could make to this literature would therefore be to publish age-stratified, infection-specific effect estimates from the routine Puskesmas and hospital record systems that already capture these episodes. Concretely, a poolable Indonesian study would need to report: age bands of under one year, one to four years and five to nineteen years separately; a diagnostic category restricted to acute respiratory infection, pneumonia or lower respiratory tract infection rather than all respiratory disease; a quantitative exposure metric expressed as a fire-attributable particulate concentration in micrograms per cubic metre, with the increment to which the effect estimate corresponds stated explicitly; and an effect estimate with a 95% confidence interval for each age band. None of these four requirements is technically demanding, and all are satisfiable from data that Indonesian health systems already hold. Their absence, rather than any absence of exposure, is why the country with among the most severe recurrent paediatric smoke exposure in the world contributes almost nothing to the global quantitative evidence base.

Updating this review

Eight of the eighteen studies included here post-date the most recent previous meta-analysis in this field, an accrual of roughly two eligible paediatric studies per year and rising. At that rate the present synthesis will be materially incomplete within approximately three years. A structured update should be planned on that horizon, and would be strengthened by extending the search beyond a single bibliographic database, by pre-specifying the outcome hierarchy that section 4.2 argues for, and by fitting the dose-response pool restricted to studies whose reported increment is already 10 ug/m^3, which would remove the log-linearity assumption entirely for that subset.

5. Conclusion

This systematic review and meta-analysis of eighteen observational studies found that exposure to smoke from wildfires and other landscape fires was associated with an increased risk of acute respiratory infection in children. In the primary dose-response synthesis of twelve studies, each 10 ug/m^3 increment in fire-related particulate matter was associated with a relative risk of 1.104 (95% CI 1.070 to 1.139; $p < 0.001$), corresponding to a Hedges' g equivalent of 0.055 (95% CI 0.038 to 0.072); the estimate was unchanged under the Hartung-Knapp-Sidik-Jonkman specification, with a wider interval of 1.042 to 1.171. Restricted to the eight studies whose outcome was infection-specific, which is the more construct-valid answer to the question as posed, the estimate was 1.148 (1.067 to 1.234). The 95% prediction interval of 1.015 to 1.201 excluded the null under the primary variance estimator, and the estimate withstood twelve leave-one-out analyses, eight structural exclusions and six alternative variance estimators without losing significance. Egger's test detected small-study effects and trim-and-fill imputation of five studies attenuated the estimate to an illustrative 1.067 (1.036 to 1.099), which remained positive and significant.

The association was not uniform across the paediatric age range. Children under five years showed a pooled relative risk of 1.202 (1.104 to 1.309), compared with 1.018 (1.001 to 1.035) in studies reporting an undifferentiated 0-19 year population. This contrast is between studies rather than within them, and the three studies reporting internal age

strata gave mixed evidence, two favouring greater susceptibility in the youngest children and one not; the gradient is therefore reported as strongly suggestive rather than as established. Prenatal exposure was associated with a larger effect than postnatal exposure (1.239 versus 1.077), and the persuasive element of that finding is the convergence of two studies differing markedly in design, population and outcome. Income setting, geographical region, study design and Newcastle-Ottawa quality did not modify the association significantly, indicating that the finding is not confined to any one health system or research tradition.

A secondary synthesis of five studies contrasting fire episodes against reference periods produced a larger pooled estimate of 1.940 (1.269 to 2.967), but its prediction interval included the null under every one of eleven specifications and its significance depended materially on a single influential study. It is therefore reported as supporting the direction of the primary finding rather than as an independent quantification of risk during fire episodes.

For paediatric practice the operational message is that landscape fire smoke should be regarded as a risk factor for acute respiratory infection, and not solely for asthma exacerbation, and that whatever protective and clinical resources a health system deploys during smoke episodes should be directed first towards children under five years and towards pregnancies in the second trimester, while recognising that no protective intervention has been evaluated against this outcome in this population. For research, the most valuable next contributions would be age-stratified, infection-specific estimates with explicit quantitative exposure metrics from the peatland-fire settings of Southeast Asia, where exposure is intense and recurrent but paediatric evidence remains almost entirely absent.

Declarations

Funding: this research received no specific grant from any funding agency in the public, commercial or not-for-profit sectors.

Conflicts of interest: the authors declare that they have no competing interests.

Data availability: the extracted dataset, the analysis scripts and the full risk-of-bias assessments are available from the corresponding author on reasonable request, and the authors intend to deposit them in a public repository on acceptance.

Ethical approval: not required. This study analysed previously published aggregate data; no individual participant data were accessed and no human participants were involved.

Author contributions: both authors contributed to the conception and design of the review, screening and data extraction, interpretation of the results, and drafting and critical revision of the manuscript. Both authors approved the final version.

Use of artificial intelligence: Generative artificial intelligence was not used to generate data, conduct the statistical analyses, or determine the study conclusions. The authors reviewed and approved the final manuscript and take full responsibility for its content.

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