Received: 27 May 2020 | Revised: 4 October 2020 | Accepted: 5 October 2020

DOI: 10.1111/obr.13160

SUPPLEMENT ARTICLE

OBESITY Reviews WILEY

Comparative effectiveness of school-based interventions targeting physical activity, physical fitness or sedentary behaviour on obesity prevention in 6- to 12-year-old children: A systematic review and meta-analysis

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Funding information
Croatian Science Foundation, Grant/Award Numbers: DOK-2018-01-2328, DOK-2018-09-8532; H2020 Societal Challenges, Grant/Award Number: 774548

Summary

A systematic search of the literature was performed to compare the effects of interventions that targeted sedentary behaviours or physical activity (PA) or physical fitness on primary prevention of obesity in 6- to 12-year-old children. The search identified 146 reports that provided relevant data for meta-analysis. Point estimates in % body fat were higher for fitness interventions compared with PA interventions (standardized mean difference = −0.11%; 95% CI = −0.26 to 0.04, and −0.04%; 95% CI = −0.15 to 0.06, respectively). Including sedentary behaviour to a PA- or fitness-oriented intervention was not accompanied by an increase in intervention effectiveness, as the point estimates were slightly smaller compared with those for PA- or fitness-only interventions. Overall, the effects tended to be larger in girls than in boys, especially for PA + sedentary behaviour interventions. There was some evidence for inequality, as the effects on body mass index were seen when interventions were delivered in the general population (standardized mean difference = −0.05, 95% CI = −0.07 to −0.02), but not in groups of disadvantaged children (standardized mean difference = −0.01, 95% CI = −0.29 to 0.19). In conclusion, school-based PA interventions appear to be an effective strategy in the primary prevention of childhood obesity among 6- to 12-year-old children, but targeting sedentary behaviour in addition to PA or fitness does not increase the effectiveness of the intervention.

KEYWORDS

BMI, body fat, overweight, screen time

GLOSSARY OF TERMS

%BF	percentage of body fat
BMI	body mass index
NCDs	noncommunicable diseases
PA	physical activity
PROSPERO	Prospective Register of Systematic Reviews
RCT	randomized controlled trial
SMD	standardized mean difference

1 | INTRODUCTION

Noncommunicable diseases remain the leading cause of death in most parts of the world, and a large part of this mortality is ascribed to insufficient physical activity (PA) and obesity.[1] Specifically, physical inactivity is the fourth, and obesity has been ranked as the fifth leading risk for global mortality.[1] At the same time, the prevalence of overweight and obesity is rising worldwide among all age groups, with the epidemic being especially marked among children and adolescents.[2] In this age group, obesity has increased dramatically during the last few decades of the 20th century, especially in the most developed countries.[2] Interestingly, it seems that this increase has been much larger in 5- to 19-year-old children as compared with younger children.[3]

Obesity in children has been linked to both short-[4] and long-term adverse health outcomes.[5] Furthermore, childhood obesity frequently persists in adulthood, which is accompanied by many well-known detrimental effects on health.[6] PA, alongside unhealthy dietary habits, is proposed as one of the major contributors to childhood obesity.[7] In addition, PA in childhood has been linked to many other favourable health outcomes as well as to improved academic performance.[8] Although there remains little doubt that PA is beneficial for health, many posit that physical fitness is an even more powerful marker of health.[9,10] On the other hand, sedentary time has also been associated with several adverse health outcomes, although evidence for a specific link with obesity is weak.[11] Currently, there is only little or no evidence that a relationship between sedentary time and adiposity in children and adolescents is causal.[12] Indeed, a recent study that collated data from 14 accelerometer investigations in children and used iso-temporal substitution to model the effects of reduced sedentary time on health estimated that replacing 1 h of persistent sedentary time with nonsedentary pursuits would lead to only a mild reduction of body mass index (BMI).[13] On the other hand, the same study found that replacing 1 h of sedentary time by moderate-to-vigorous PA increases the estimated decrease in BMI by more than seven times.[13] Hence, PA interventions might exhibit larger effects on obesity-related outcomes than interventions aimed at reducing sedentary behaviours. However, this remains to be confirmed in clinical trials.

Obesity, PA and sedentary pursuits are complex phenomena that require population-based solutions. For children, schools are frequently identified as an ideal setting for introducing lifestyle change and the prevention of weight gain. In most countries, school is obligatory, at least by midadolescence; hence, all children can be reached, which makes schools a perfect setting to reduce health inequalities. In addition, children spend a significant portion of the day in school. Because academic activities are mostly sedentary, ample opportunities for PA should be provided in order to increase energy expenditure and introduce the well-known benefits of PA on health and academic performance. Indeed, several previous systematic reviews that examined the effects of obesity prevention interventions have shown that school-based interventions are most effective when a PA component is included.[14–16] However, the characteristics of successful PA intervention are less understood.

We aimed to bridge this gap by assessing what types of PA interventions in schools are the most effective in improving obesity-related outcomes. To this end, we compared the effects of three groups of interventions: (1) programmes that aimed to reduce sedentary behaviour, (2) interventions that intended to increase PA and (3) interventions that were designed to improve physical fitness. We identified several systematic reviews published in the last 10 years that assessed the effectiveness of PA interventions on obesity prevention.[14–21] However, none of these studies attempted to document and analyse specific elements of PA programmes. Moreover, several of these analyses might have missed large studies as they were restricted to randomized designs.[18,21] Others were restricted to high-income countries[15] or to a single obesity-related outcome only.[19,20] Thus, in order to cover a complete spectrum of PA interventions, we included all school-based interventions that targeted energy expenditure, regardless of the type or duration of the intervention. The wide range of included interventions will serve to identify features that enhance the effectiveness of these programmes in primary prevention of obesity, with special focus on the type of energy expenditure component targeted.

2 | METHODS

The protocol for this review was registered with Prospective Register of Systematic Reviews (PROSPERO 2019 CRD42019129295), and the methods are briefly described in the following sections.

2.1 | Literature search and data extraction

We searched MEDLINE, The Cochrane Central Register of Controlled Trials (CENTRAL), Scopus, LILACS, OpenGrey, Open Access Thesis and Dissertations, Clinical Trials and the WHO International Clinical Trials Registry for peer-reviewed studies published in the last 25 years (between 1 January 1994 and 15 April 2019). We designed our research question by following population, intervention, comparison and outcome framework; used MeSH terms in MEDLINE plus keyword searches structured around four constructs (population–children; intervention–PA, fitness and sedentary behaviour; setting–school; outcome–adiposity); and adapted this strategy to individual databases (see List S1 for MEDLINE search strategy). We did not limit our search to any specific geographical region; however, we included only studies written in European languages. The search strategy was validated by conducting sensitivity analysis in MEDLINE with a test set of 10 key papers selected as exemplary papers answering our research question. Adjustments to the search strategy finished when all 10 key papers were identified by the search. All database search results were extracted and imported into the web-based reference manager: Rayyan. After duplicates were removed, results were screened initially by abstract and title. The first 500 results were screened independently by two reviewers (H.P. and J.K.). Given that >95% agreement between reviewers in included studies was recorded, each of the two reviewers screened half of the remaining results. Ambiguities on study eligibility were resolved through discussion with a third reviewer (M.So.). In addition to this, we checked reference lists of key systematic reviews in the same area for eligible studies.[14,16–18,20,22] Lastly, we searched reference lists of all included reports.

Inclusion criteria were as follows: (1) randomized or non-randomized control trial, controlled before and after study or natural experiment; (2) control group; and (3) participants aged 6 to 12 years (mean age at the start of the study = 5.5 to 12.49). Namely, school-going youth are typically divided into two age groups: children (6–12 years) and adolescents (13–18 years). Considering that the same strategies would probably not work for both children and adolescents and because of large heterogeneity in high school curricula that preclude one-size-fits-all policies, we decided to limit this review to children aged 6 to 12 years; (4) interventions of any duration that have aimed to either (a) increase PA and/or physical fitness or (b) reduce sedentary behaviour; (5) intervention that was performed primarily in school setting; (6) follow-up of at least 12 weeks from the start of the intervention; and (7) any obesity-related outcome was measured (e.g. BMI, BMI *z*-score, BMI percentile, prevalence or incidence of overweight or obesity, percentage of body fat [%BF], skinfold thicknesses, waist circumference, waist circumference percentile, and waist-to-height ratio). Studies were excluded if (1) no obesity-related outcome was reported or the data came from self-report; (2) they included exclusively children with overweight or obesity or only special populations (e.g. children with a specific illness, blind, and physically disabled); and (3) full text was not available (i.e. only conference abstract).

After study selection, an extraction template was created (M.So.), and study characteristics were extracted by two reviewers working independently (Ž.L.P. and P.J.). Papers reporting on the results of the same study were collated so each study is the unit of analysis rather than each paper. Values at the longest available follow-up were taken for quantitative analyses.

The details on the intervention content were extracted from the main papers, the intervention protocols and the related web resources. Two reviewers independently extracted half of the data (Ž.L.P. and P.J.), and about 10% of the extracted data were double checked by the third reviewer (M.So.). Extracted items included authors, year, period of the study, number of clusters and participants, demographic characteristics, details on intervention type and content, duration of intervention and follow-up, and adverse outcomes.

Obesity-related study outcomes were extracted by two reviewers (JK and P.J.), working independently on half of the data, and entered in a predesigned excel template. The third reviewer (M.So.) verified 10% of the extracted results, and any discrepancies were resolved through discussion.

2.2 | Risk of bias assessment

A single reviewer (M.So.) assessed the risk of bias of all studies that met our inclusion criteria using Cochrane 'risk of bias' assessment tool for randomized studies[23] and modified Newcastle–Ottawa scale for nonrandomized study designs.[24]

For individual randomized controlled trial (RCT), the assessment contained the following domains: (1) random sequence generation, (2) allocation sequence concealment, (3) blinding of outcome assessment, (4) incomplete outcome data, (5) selective outcome reporting and (6) other bias (in this domain, we assessed bias arising from [a] contamination and [b] low fidelity). We excluded blinding of participants and study personnel domain because this is generally not possible for PA interventions. For cluster-randomized trials, we included several more domains specific to this design: (1) recruitment bias (when participants are approached after the clusters have already been randomized), (2) baseline imbalance (often present when a small number of clusters are being randomized), (3) loss of clusters (when whole clusters are lost from the trial, either immediately after randomization or during follow-up) and (4) inappropriate data analysis (when clustering of observations is not taken into account in data analysis). We judged the risk of bias in each domain as having low, high or unclear risk. Studies judged as having low risk of bias in at least five domains for individual RCTs and eight domains for cluster RCTs were classified as having an overall low risk of bias.

For nonrandomized study design, risk of bias assessment was performed using modified Newcastle–Ottawa scale for cohort studies. This scale originally includes eight domains, but one domain (i.e. demonstration that the outcome of interest was not present at the start of the study) was deemed not to be applicable for studies included in this review; hence, it was omitted. The domains assessed included the following: (1) representativeness of the intervention cohort (were participants representative for the community?), (2) selection of the nonintervention cohort (were controls drawn from the same community as the participants of the intervention?), (3) ascertainment of intervention (was the intervention implemented according to the plan?), (4) comparability of cohorts on the basis of the design or analysis (were analyses adjusted for age, gender and other important features, such as clustering and baseline values for the outcome of interest?), (5) assessment of outcome (was the outcome measured with an objective method?), (6) follow-up longer than 6 months from the start of the intervention (was the follow-up long enough for outcomes to occur?) and (7) adequacy of follow-up of cohort (subjects lost to follow-up unlikely to introduce bias owing to low or balanced attrition). According to the standard scoring protocol,[24] we awarded one star for Domains 1, 2, 3, 5, 6, and 7 and a maximum of two stars for comparability of cohorts domain. Studies that totalled at least six stars were classified as having an overall low risk of bias.

2.3 | Data analysis

We combine, in all cases, mean differences, calculated as follows:

$$\text{Mean difference} = \text{Differences in the intervention group} - \text{Differences in the control group},$$

where the mean differences in the intervention and control groups denote the differences between values at follow-up and baseline in each of the groups. The units of measurement were kg m^{-2}, units of the standardized normal and % for BMI, BMI *z*-score and %BF, respectively. Next, when comparing five groups of interventions in both the main analyses and subgroup analysis (described in the next section), we standardized these raw differences using the standard deviations of the differences (with the exception of the BMI *z*-score, because it is already standardized).

We computed the uncertainty parameter (I^2) representing the percentage of total variance in the observed results explained by heterogeneity and assessed heterogeneity using the *Q* test.[25] We performed the meta-analysis using a random-effects model, which takes into account both within- and between-study heterogeneity.[26] Finally, we assessed publication bias for both overall results and main subgroup analyses with Egger's test for asymmetry.[27] The level of significance (alpha) was set at 5%.

2.3.1 | Subgroup analyses

To compare the effects of interventions targeting different elements of energy expenditure, we first categorized interventions into those affecting PA, physical fitness or sedentary behaviour. This was done by examining the content of the interventions (e.g. the intensity and volume of introduced PA, and the aims explicitly stated by the authors). Then we classified interventions into those aiming to change only one element of energy expenditure (i.e. decreasing sedentary behaviour or increasing PA/fitness) and those targeting two elements of energy expenditure (i.e. PA plus sedentary behaviour or physical fitness plus sedentary behaviour). Hence, five groups of interventions were created in total: (1) sedentary behaviour, (2) PA, (3) fitness, (4) PA + sedentary behaviour and (5) fitness + sedentary behaviour. Within each of these subgroups, we examined how the characteristics of the content (e.g. duration and intensity of PA introduced and diet component) and the intervention (e.g. duration, follow-up period and parent involvement) modify the effectiveness of the programme.

We further stratified all analyses by gender (both genders, i.e. studies that did not distinguish gender; boys and girls). Lastly, as a very limited number of studies that directly compared the effects in children of varying socio-economic status (SES) were available, for examining the equality aspect of interventions studied here, we compared the effects found in studies that focused on economically deprived children with interventions that included general population of children.

2.3.2 | Sensitivity analyses

For sensitivity analysis, we stratified the analyses, separately for PA, fitness and PA + sedentary and fitness + sedentary behaviour interventions, by study design (RCT vs. other designs), risk of bias (low risk of bias vs. moderate and high risk of bias), study period (<2009 vs. ≥2009) and mean age of participants (6–9 vs. 10–12 years).

3 | RESULTS

The search strategy retrieved 18 239 studies from eight databases. After duplicates were removed, 17 014 records were screened by title and abstract. In the next step, 1091 were selected for screening of the full-text paper, and 242 were found to conform to our inclusion criteria. Searching the reference list of seven systematic reviews led to the addition of 11 papers, and additional search of references of included studies yielded four more reports. All in all, the search retrieved 257 papers. A large majority of the papers were in English (246 or 97%), whereas other languages included Spanish (eight papers), German (two papers) and Dutch (one paper). Several of the included papers reported on outcomes of the same intervention study at different time points or in different subpopulations. Hence, results were extracted from 200 individual intervention studies (see List S2 for the list of included and excluded studies), and 146 of these provided data suitable for meta-analysis and were finally included in this review (Figure 1). After we excluded studies that did not provide standard errors, those that did not provide results at baseline (or preintervention) and those that did not provide information related to control group, we combined, by meta-analysis, studies that assessed the following outcomes: BMI (102 studies, 171 analyses), BMI *z*-score (56 studies, 119 analyses) and %BF (46 studies, 91 analyses). On the other hand, obesity prevalence/incidence and waist circumference were found to be unsuitable for meta-analyses because of large heterogeneity in reporting, and other outcome measures were not included frequently enough to deserve meta-analysis.

3.1 | Characteristics of the included studies

An overview of the characteristics of the included studies is given in Table 1, and the details on individual studies are presented in Table S1.

The largest part of the studies was performed in Europe (44%), although studies from Central and Eastern Europe were scarce. A slightly lower share of studies was situated in North America (31%), mostly in the United States (34 studies). On the other hand, only two studies were performed in Africa (both in South Africa). In addition, only one study was performed in multiple countries (i.e. in eight European countries). Randomized controlled design was applied in over 60% of the included studies, with over 90% of these studies being cluster RCTs, a type of experimental study in which groups of subjects and not individual subjects are randomized (e.g. randomization is performed by class or by school). A similar number of studies included younger and older children, and about 20% of the studies were focused on vulnerable groups of children. Finally,

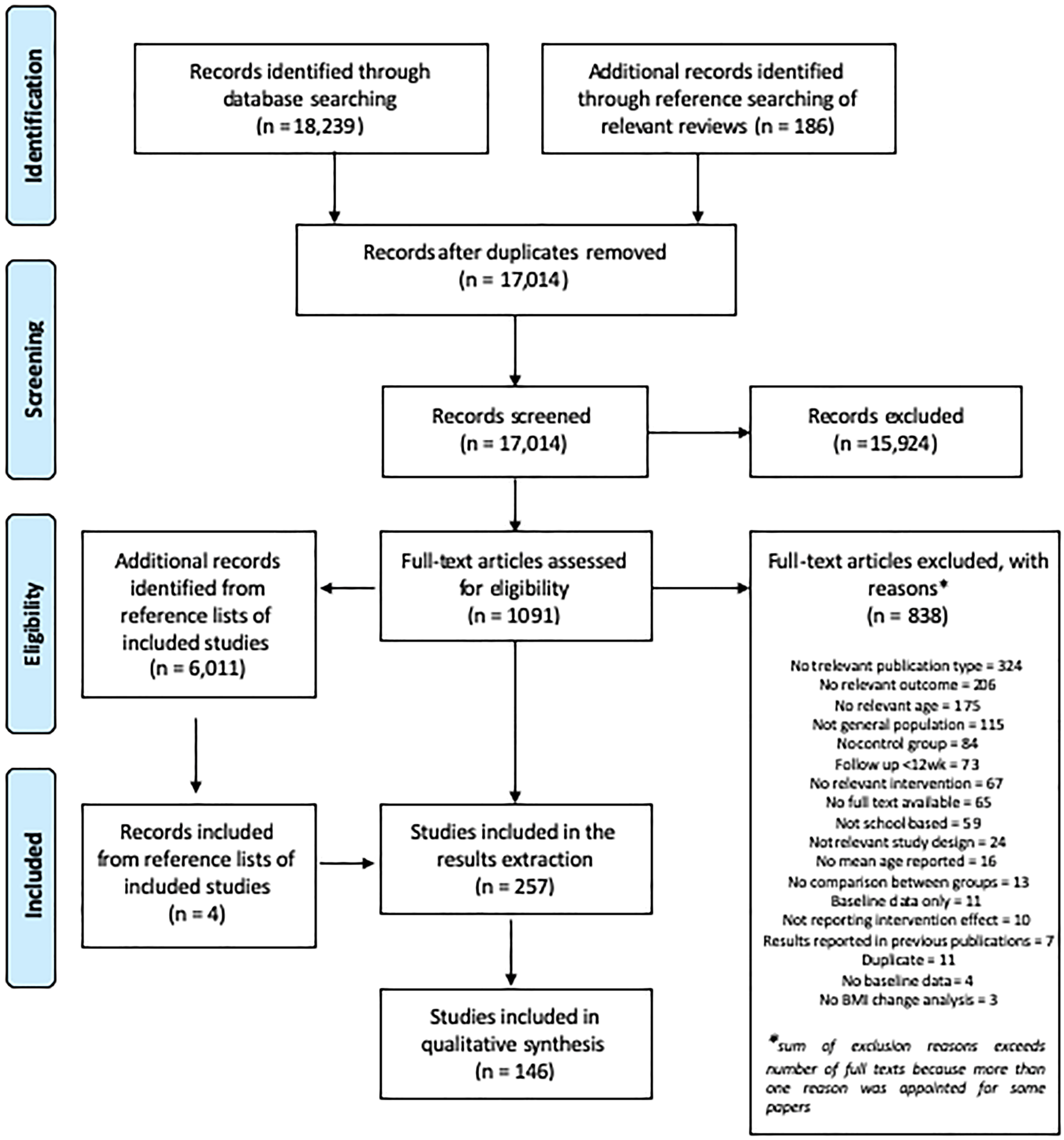

FIGURE 1 PRISMA flow chart showing the study selection process

only a small part of school-based interventions extended to the community and home settings (13/146 studies), and just over half of the school-only interventions attempted to involve parents and guardians (81/146 studies).

Majority of interventions included in this review were designed to affect only one component of energy expenditure (104/146 studies or 71%), with about half of these targeting PA and the other half aiming to improve physical fitness. On the other hand, only two interventions that focused exclusively on sedentary behaviour were included, one that restricted screen time and the other that introduced standing desks in the classrooms. Next, among 43 interventions that targeted multiple movement behaviours, only six aimed at reducing sedentary behaviour and improving fitness, and the other 37 strived to increase PA while reducing sedentary behaviour. Comparison of characteristics of interventions that included only PA or fitness versus interventions that also addressed sedentary behaviour is given in Table 2.

Studies that included sedentary behaviour alongside PA or fitness component introduced some form of PA less frequently, and when they did, they delivered a smaller dose of PA compared with PA- or fitness-only interventions. In addition, interventions that included sedentary behaviour involved parents more often (77% vs. 48%). Duration was similar in the two groups of interventions, as well as the share of interventions that included a diet component (mostly in the form of nutritional education or changes in food provision and environment). Around one third of interventions extended over several years, with a couple of programmes spanning over 4–6 years. Finally, the sustainability of intervention effects was analysed in roughly 30% of the studies, although only about half of these studies followed participants for at least 1 year after the end of intervention.

TABLE 1 Characteristics of the included studies

Study characteristics		N	%
Location	Europe	64	44
	North America	44	31
	South America	9	6
	Asia	16	11
	Oceania	9	6
	Africa	2	2
Study design	RCT	91	62
	Quasi-experimental	55	38
Study period	Before 2009	71	49
	2009–2019	48	33
	Not specified	27	18
Age of participants	6–9 years	74	51
	10–12 years	66	45
	Both age groups	6	4
Characteristics of participants	General population	116	79
	Low SES	26	18
	Specific ethnic group	4	3
Aims of intervention[a]	Sedentary time	2	2
	PA	55	38
	Physical fitness	47	32
	Combined	43	30
Diet component	Yes	95	65
	No	51	35
Duration of intervention	<6 months	38	26
	6–12 months	62	42
	>12 months	46	32
Follow-up[b]	Only postintervention	99	68
	<1 year	24	16
	≥1 year	23	16
Parent involvement	Yes	81	55
	No	64	45
Additional setting	Yes	13	10
	No	133	90
Risk of bias	Low	36	25
	Moderate	71	48
	High	39	27

Note: Quasi-experimental design includes nonrandomized controlled trials, controlled before and after studies and natural experiments.
Abbreviations: PA, physical activity; RCT, randomized controlled trial; SES, socio-economic status.
[a]One study had two experimental groups: one that included PA and the other exposed to a combined intervention that additionally included sedentary behaviour component.
[b]Follow-up period is given in months after the end of the intervention.

3.2 | Risk of bias

Risk of bias across domains for randomized and nonrandomized studies is shown in Figures 2 and 3, respectively, whereas risk of bias assessment across individual studies is presented in Tables S2 and S3. We considered almost all trials to have low bias in blinding of outcome assessors domain as the outcomes were objectively assessed and as only several outcome measures were subject to observer bias. In addition, no studies have been found to have a high likelihood of selective reporting. We judged most of the trials as having low risk of bias concerning lost to follow-up (72/82), incomplete outcome data (74/91) and baseline imbalance (61/82). Risk of bias was low for about half of the trials in terms of allocation concealment (48/91), random sequence generation (43/91) and adequate statistical analyses for clustered nature of the data (50/82). Conversely, there was unclear or high risk of other bias in almost 2/3 of trials, mostly relating to low intervention fidelity. Despite seemingly favourable results of risk of bias assessment, only 24/91 RCTs were judged as having an overall low risk of bias (according to criteria described in Section 2).

For nonrandomized studies, the least bias was noticed for the outcome domain, where about one third of the studies were awarded maximum points. On the other hand, in terms of selection domain, only one study was given maximum points. Lastly, for comparability, an equal part of studies was assigned with 0, 1 or 2 points. Generally, 12/55 studies totalled at least 6/8 points and were, hence, considered to have low overall risk of bias.

3.3 | Results by intervention type

All in all, pooled effect sizes indicated that school-based PA interventions favourably affected all three outcomes analysed (BMI = −0.16 kg m^{-2}, 95% CI = −0.25 to −0.07; BMI *z*-score = −0.07, 95% CI = −0.10 to −0.05; %BF −0.34%, 95% CI = −0.55 to −0.13). Yet it has to be noted that indices of heterogeneity were large for all outcomes and ranged from I^2 = 82% to I^2 = 92%.

When only studies that provided effects by gender are examined (*n* = 22G and 21B for BMI, *n* = 11G and 12B for BMI *z*-score, *n* = 13G and 13G for %BF), it becomes evident that gender is a significant moderator of the effectiveness of interventions analysed here. Specifically, interventions were effective or borderline effective in girls irrespective of the outcome assessed (BMI = −0.21 kg m^{-2}, −0.28 to −0.14; BMI *z*-score = −0.12, −0.27 to 0.03; %BF = −0.68%, −1.08 to −0.29), whereas in boys, null pooled effect was noted for BMI (−0.01 kg m^{-2}, −0.08 to 0.05) and BMI *z*-score (−0.01, −0.05 to 0.06), and only borderline pooled effect was seen for %BF (−0.49%, −1.12 to 0.15, *p* = 0.13).

Forest plots showing the standardized differences in mean (SMD) between the intervention group and the control group across individual interventions stratified by the content of the intervention are shown in Figures 4–6. All in all, the largest point estimates were found for interventions designed to improve physical fitness, with the differences being in favour of the intervention groups for all three outcomes assessed (BMI = −0.04, 95% CI = −0.07 to −0.00; BMI *z*-score = −0.10, −95% CI = −0.16 to −0.03; %BF = −0.11, 95% CI = −0.26 to 0.04). Interventions that aimed to increase PA showed similar effect on BMI *z*-score (SMD = −0.09; 95% CI = −0.12, −0.06)

TABLE 2 Characteristics of the studies stratified by movement behaviours targeted by the intervention

Study characteristics		SB/PA/fitness (*N* = 104)	PA + SB/fitness + SB (*N* = 43)
		N (%)	*N* (%)
Study design	RCT	59 (57)	33 (77)
	Quasi-experimental	45 (43)	10 (23)
Study period	Before 2009	51 (49)	21 (49)
	2009–2019	33 (32)	15 (35)
	Not specified	20 (19)	7 (16)
Age of participants	6–9 years	54 (52)	20 (47)
	10–12 years	45 (43)	22 (51)
	Both age groups	5 (5)	1 (2)
Intervention components	Sedentary behaviour	2 (2)	43 (100)
	Physical activity	55 (53)	37 (86)
	Physical fitness	47 (45)	6 (14)
Duration of intervention	<6 months	23 (22)	15 (34)
	6–12 months	49 (47)	14 (33)
	>12 months	32 (31)	14 (33)
Follow-up[a]	Only postintervention	72 (69)	27 (63)
	<1 year	17 (16)	7 (16)
	≥1 year	15 (15)	9 (21)
Duration of PA (min week^{-1})	0	7 (7)	16 (37)
	1–120	41 (40)	14 (32)
	≥120	42 (40)	5 (12)
	Not specified	14 (13)	8 (19)
Intensity of PA	Low to moderate	23 (22)	2 (5)
	Moderate to vigorous	47 (45)	9 (21)
	Not specified	34 (33)	32 (74)
Diet component	Yes	65 (63)	31 (72)
	No	39 (37)	12 (28)
Parent involvement	Yes	50 (48)	33 (77)
	No	54 (52)	10 (23)
Additional setting	Yes	8 (8)	5 (12)
	No	97 (92)	38 (88)
Risk of bias	Low	18 (17)	18 (42)
	Moderate	56 (54)	15 (35)
	High	29 (29)	10 (23)

Abbreviations: PA, physical activity; RCT, randomized controlled trial; SB, sedentary behaviour.
[a]Follow-up period is given in months after the end of the intervention.

and BMI (SMD = −0.04; 95% CI = −0.09, 0.02), but the effects on %BF (SMD = −0.04; 95% CI = −0.15, 0.06) were less pronounced than for fitness-oriented programmes. Lastly, as only two interventions that focused exclusively on reducing sedentary behaviours were included, the pooled effects for this type of intervention were not computed.

Interventions designed to improve physical fitness while also aiming to reduce sedentary time proved to be unsuccessful in favourably affecting BMI (SMD = −0.01; 95% CI = −0.09 to 0.07), whereas reliable estimates for BMI *z*-score and %BF could not be computed because of limited number of studies. By contrast, pooled results for interventions aiming to increase PA while also trying to reduce sedentary behaviours favoured interventions for BMI (SMD = −0.07; 95% CI = −0.13 to −0.00), and BMI *z*-score (SMD = −0.06; 95% CI = −0.09 to −0.03) but not for %BF (SMD = −0.01; 95% CI = −0.08 to 0.06). Heterogeneity was still quite large in all four groups of interventions suitable for meta-analysis and ranged from I^2 = 76% to I^2 = 94%.

Modifications of the effects by intervention characteristics are displayed in Table S4. Longer weekly PA duration translated to larger

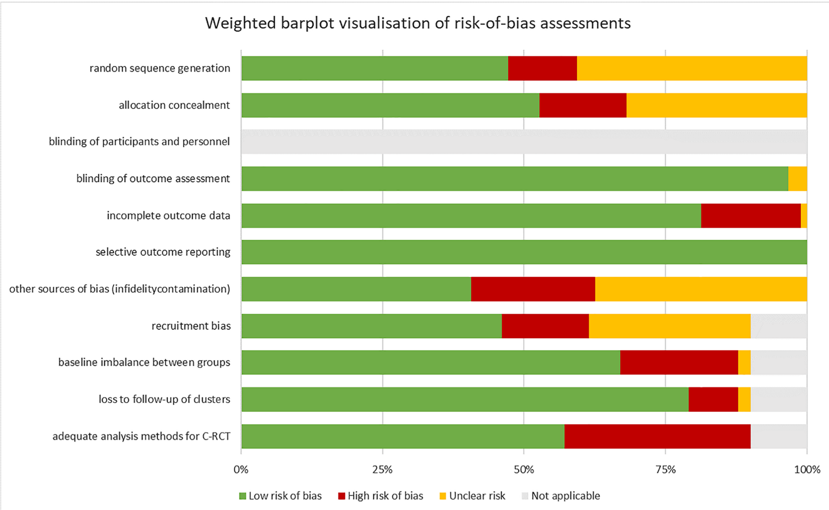

FIGURE 2 Risk of bias across domains for randomized controlled trials (Cochrane 'risk of bias' assessment tool for randomized studies[23])

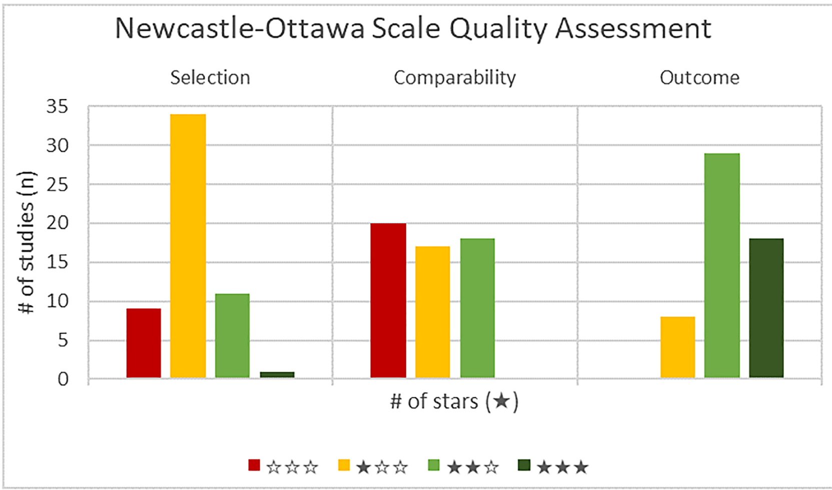

FIGURE 3 Risk of bias across domains for nonrandomized study designs (modified Newcastle–Ottawa scale[24])

effects on BMI for both fitness-only and fitness + sedentary behaviour interventions (SMD = −0.12, 95% CI = −0.21 to −0.03 and SMD = −3.8, 95% CI = −6.4 to −1.1, respectively). More intense PA was related to larger effects for fitness-only interventions (SMD = −0.15; 95% CI = −30 to −0.00 for BMI and SMD = −0.18, 95% CI = −0.30 to −0.05 for BMI *z*-score) and for PA + sedentary behaviour interventions (SMD = −0.63, 95% CI = −1.04 to −0.21 for BMI *z*-score). On the other hand, including a diet component improved only the effect of PA interventions on BMI *z*-score (SMD = −0.06, 95% CI = −0.12 to −0.01). Regarding other characteristics, parent involvement markedly improved the effect on %BF for fitness-only interventions. Similarly, the effects on %BF seem to be much larger in this group of interventions when the interventions extend to more than one academic year. At the same time, the effects of PA-only programmes were somewhat smaller in long term compared with short term for BMI *z*-score.

Mean pooled results comparing the effectiveness of different types of interventions by gender are presented in Table S5. The number of studies included in these analyses was fairly small (n = 3–7 for PA and n = 2–10 for fitness), hence smaller power and large confidence intervals. Still, point estimates for interventions that aimed to increase PA were generally larger in girls for most outcomes, whereas for interventions that were designed to improve physical fitness, the opposite was true.

Finally, the findings from sensitivity analyses (see Table S6) showed that the results were fairly robust, except when considering study design for PA interventions and age group for fitness interventions, and only for BMI as the outcome. Specifically, the effects of PA interventions were lower in RCTs compared with studies that employed other designs, whereas the effects of fitness interventions were larger in the younger age group.

3.4 | Effectiveness of interventions in vulnerable groups of children

Our search identified 26 studies that included predominantly economically deprived children and reported data appropriate for a meta-analysis. In general, the effects on BMI were not seen when interventions were delivered to vulnerable groups of children

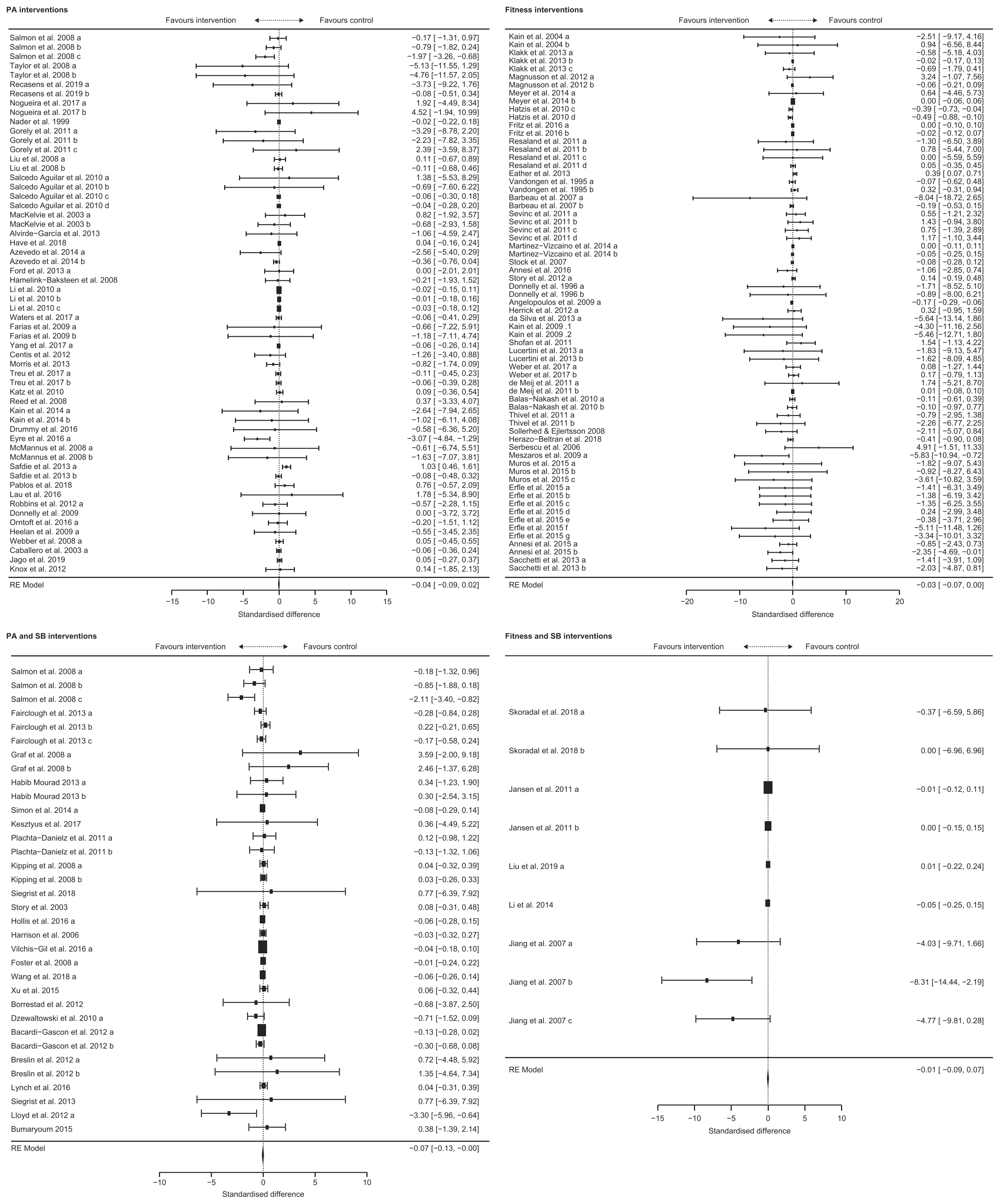

FIGURE 4 Forest plot of standardized mean differences in change in body mass index between the intervention group and the control group for physical activity (PA), fitness, PA + sedentary behaviour and fitness + sedentary behaviour interventions

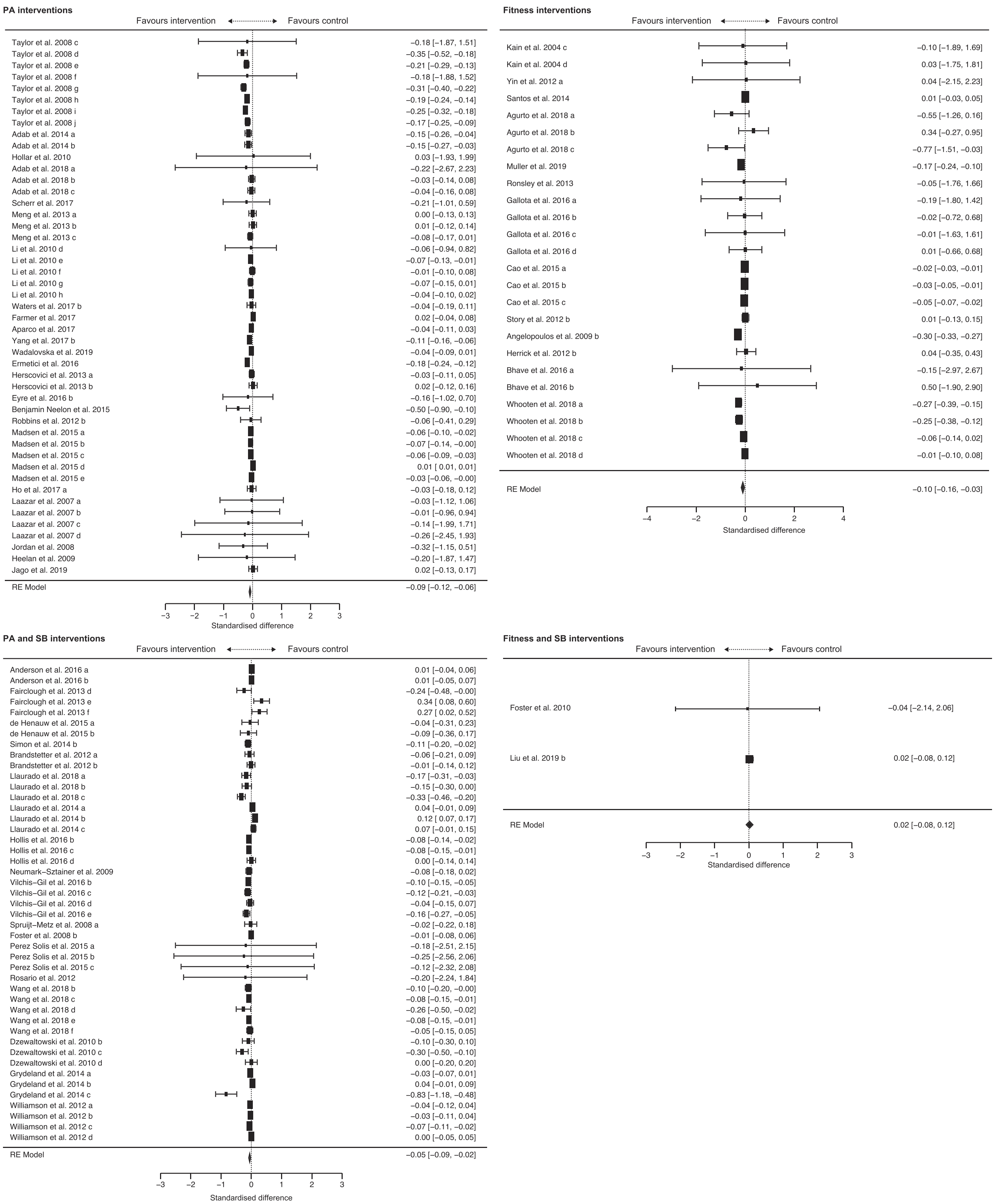

FIGURE 5 Forest plot of mean differences in change in body mass index *z*-score between the intervention group and the control group for physical activity (PA), fitness, PA + sedentary behaviour and fitness + sedentary behaviour interventions

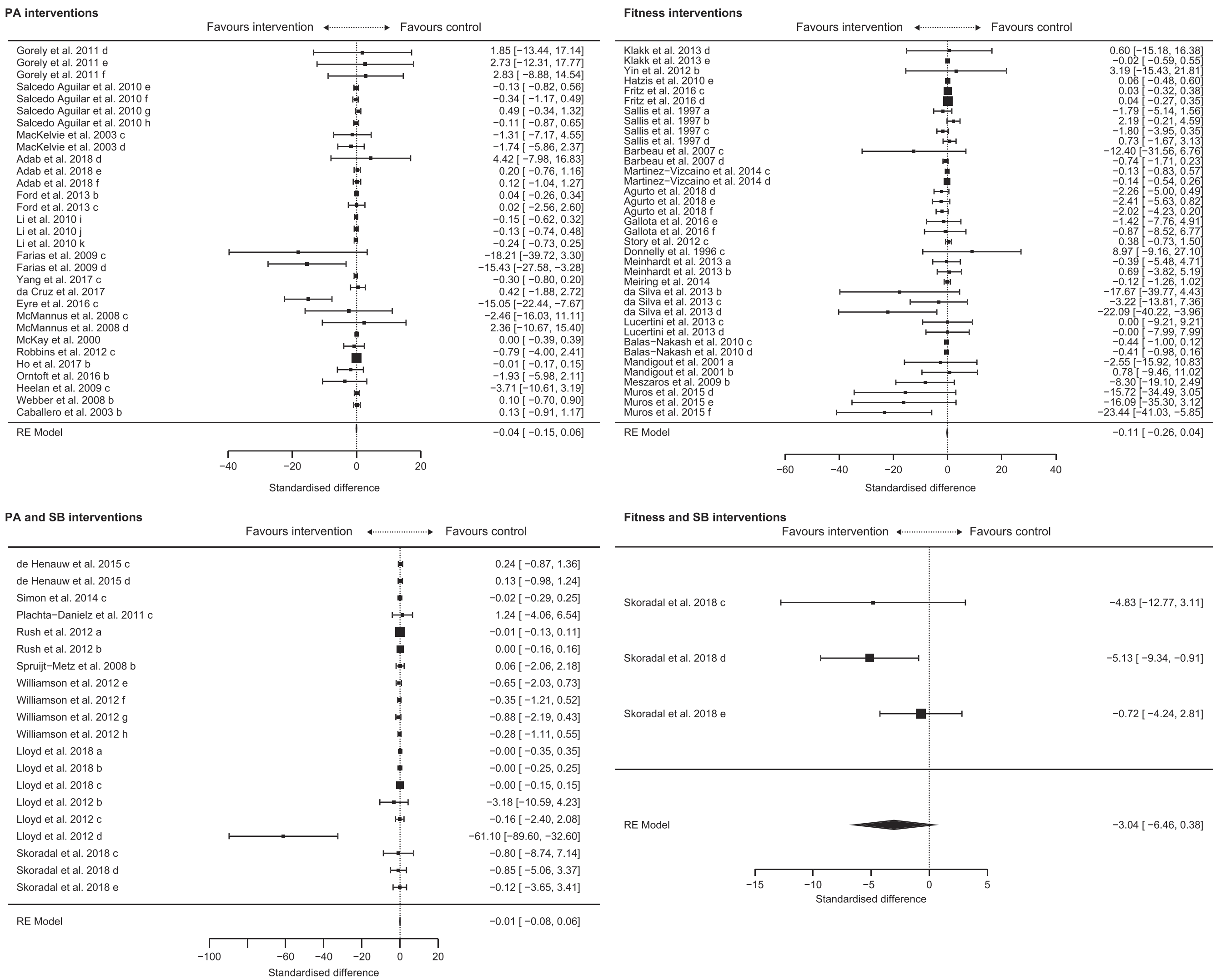

FIGURE 6 Forest plot of standardized mean differences in change in percentage body fat between the intervention group and the control group for physical activity (PA), fitness, PA + sedentary behaviour and fitness + sedentary behaviour interventions

(SMD = −0.01, 95% CI = −0.29 to 0.19), whereas interventions delivered to the general population seem to be effective (SMD = −0.05, 95% CI = −0.07 to −0.02). Of note, this holds for both interventions that focused on PA or fitness and for interventions that additionally included sedentary behaviour modification. Namely, although both PA and fitness interventions favourably affected BMI in general child population with the same standardized difference between intervention and control groups (−0.05; 95% CI = −0.10 to −0.00), for interventions that exclusively involved disadvantaged children, the pooled results showed nonsignificant SMD in BMI of 0.06 (95% CI = −0.20 to 0.32) for PA interventions and 0.00 (95% CI = −0.08 to 0.08) for fitness interventions. SMDs between intervention groups that were included in PA + sedentary behaviour programmes and controls amounted to −0.08 (95% CI = −0.15 to −0.00) in general population, whereas the effects were lower in studies with primarily disadvantaged children involved (−0.04, 95% CI = −0.15 to 0.07).

On the other hand, the effects on BMI *z*-score in underprivileged children were more comparable with the ones noted for general population (low SES: −0.04, −0.09 to 0.00; general population: −0.08, −0.10 to −0.06). There was an indication that this effect was modified by the type of the intervention, with PA interventions being more successful in affecting weight change in disadvantaged groups compared with fitness interventions. Specifically, for PA interventions, pooled results showed a very similar reduction in BMI *z*-score by −0.08 in deprived children (95% CI = −0.15 to −0.00) versus −0.09 in the general population (95% CI = −0.12 to −0.06). On the other hand, pooled effects for fitness interventions point to a reduction in BMI *z*-score in the general population (−0.11, 95% CI = −0.18 to −0.04) and no effect on BMI *z*-score in underprivileged children (0.04, 95% CI = −0.34 to 0.40).

Comparisons of the effects on body fat were impeded by too few studies that focused on low SES and included body fat as an outcome. Still, estimates from the few studies available show no effect in disadvantaged children (standardized mean difference = −0.01, 95% CI = −0.13 to 0.12), and a trend to reduction in %BF in the general population (standardized mean difference = −0.04, 95% CI = −0.10 to 0.02).

3.5 | Assessment of publication bias

A formal evaluation of all available results using Egger's regression did not show evidence of publication bias for neither BMI (z = −0.11, p = 0.91), BMI z-score (z = −1.36, p = 0.17) nor %BF (z = −0.80, p = 0.42). Similarly, when stratified by the type of intervention (i.e. PA/fitness/PA + sedentary behaviour/fitness + sedentary behaviour), publication bias was not seen for any of the three outcomes analysed (p values ranged from 0.12 to 0.96).

4 | DISCUSSION

In this systematic review, we compared the effects of interventions that intended to increase PA with interventions that were designed to improve physical fitness and with interventions that aimed to reduce sedentary behaviour on obesity-related outcomes in 6- to 12-year-old children. The main results of our study include the following: (1) school-based PA interventions appear to be an effective strategy in the primary prevention of childhood obesity among 6- to 12-year-old children; (2) interventions that combined PA or fitness component with strategies to reduce sedentary behaviour were actually less effective in controlling weight gain than were PA- or fitness-only interventions; hence, including behavioural strategies to reduce sedentary behaviour to PA or fitness programmes does not provide additional benefits for primary prevention of obesity; (3) interventions that were designed to improve physical fitness produced slightly larger effects than interventions that strived to increase PA; (4) intervention effects were generally larger in girls than in boys, especially for programmes that included both PA/fitness and a sedentary behaviour component; and (5) interventions that were delivered exclusively to economically deprived children analysed here were less able to induce favourable effects on BMI compared with interventions conducted in more general settings.

We found an overall difference in favour of the intervention group of −0.16 kg m^{-2} for BMI (95% CI = −0.25 to −0.07), −0.07 for BMI z-score (95% CI = −0.10 to −0.05) and −0.34% for %BF (95% CI = −0.55 to −0.13). This overall effect size for all types of PA interventions reported here is notably larger than the overall effect of all kinds of school-based obesity prevention initiatives on BMI of −0.08 kg m^{-2} (95% CI = −0.11 to −0.05) reported in a recent meta-synthesis of 10 different meta-analyses[28] but quite similar to the effect previously reported for school-based programmes that included PA.[15,16] Although clinical importance of the effect size reported here is probably trivial, such small shifts at the population level can produce significant public health benefits by reducing weight gain in normal-weight children. Plus, it is worth noting that the effects of the PA interventions are probably underestimated because of the well-known limitations of BMI in distinguishing fat from fat-free mass on one side and the large measurement error of commonly used methods for assessing body composition on the other side.

In terms of characteristics that moderate the effectiveness of these type of interventions, the World Health Organization has recommended that obesity prevention programmes should span over at least 1 year, include both PA and a diet component, and involve parents, if possibly extending also to the home and community settings.[29] Our findings supplement these guidelines by indicating that interventions should be designed to improve fitness in order to maximize the effects on obesity prevention in 6- to 12-year-old children. However, this finding needs to be corroborated in future studies, as there was considerable overlap in confidence intervals of the effects of PA and fitness interventions studied here. Next, when analysing a smaller number of studies that reported effects by gender, we found evidence that fitness-oriented interventions are more effective than the ones directed to PA only in boys but not among girls. Therefore, more evidence is needed that this applies to both genders. Still, epidemiological studies support evidence from trials described here by reporting stronger cross-sectional associations with cardiometabolic risk factors for fitness than for PA.[10] Similarly, physical fitness has been identified as a moderator of the relationship between PA and cardiometabolic risk in children. More specifically, PA was associated with cardiometabolic risk factors in low-fit children but not in their fit peers.[30]

The finding that the interventions that encompass several behaviours are not superior to programmes that focus on just one behaviour has already been reported for combination of PA with a diet component. Although evidence on this is not unequivocal, it was previously shown that diet + PA interventions in a variety of settings are not superior to programmes that target a single behaviour[28] and that these kinds of combined interventions have an even smaller impact on obesity-related outcomes than single-component programmes when set in schools.[15] Similarly, a meta-analysis of mostly nonschool-based programmes showed that interventions targeting sedentary behaviour and PA simultaneously were not more effective in BMI reduction than interventions that focused exclusively on sedentary behaviour.[31] Our search strategy allowed for only two studies that focused only on reducing sedentary behaviour to be included in the quantitative synthesis. This precluded us from estimating reliable pooled effect sizes for any of the obesity-related outcomes assessed. However, we were able to estimate the impact of adding sedentary behaviour component to PA or fitness intervention programmes on the primary prevention of obesity and found no added value of including sedentary behaviour component. Prior studies that included a variety of settings and a wider age range also failed to show the effectiveness of these types of intervention in obesity prevention.[31,32] This is hardly surprising given the low intensity of these kinds of programmes, strong reliance on educational content only and the high reinforcement of media use in today's cultures. Although it has been reported that these types of programmes can produce a significant decrease in sedentary behaviours, the effect size is too small to have an impact on weight regulation.[32] Nevertheless, given the unprecedented increase in exposure to screens faced by contemporary children,[33] it is of paramount importance to increase the efforts in redesigning strategies for controlling the amount of time children spend in front of the screens. To this end, behavioural strategies that have been a cornerstone of strategies to reduce sedentary time up to now should be supplemented with policies oriented at changes in the environment.

As about two thirds of studies included in this review had included a diet component, it is difficult to ascribe positive findings of interventions reported here specifically to increase in energy expenditure. Yet subgroup analyses provided only limited evidence for moderating effect of diet component in PA-directed programmes for one outcome measure and no such effect in fitness-oriented interventions. In addition, several previous reviews have found that diet-only interventions are less effective than the ones focused exclusively on PA.[14,15] Nevertheless, given the complex nature of the disease, multifaceted interventions targeting both sides of the energy equation should be advocated as the most beneficial approach for primary prevention of obesity. Hence, delineating the effects of diet and PA strategies in real-world setting is neither possible nor required.

Overall, mean pooled effects of interventions for primary prevention of obesity analysed in this review were larger in girls than in boys, especially for interventions aimed at both PA or fitness increase and a reduction in sedentary behaviours, although it has to be emphasized that the confidence intervals did not overlap only for BMI as an outcome measure. It is well known that school-aged girls are less physically active than boys.[34] To that end, the amount of PA typically used in intervention studies probably contributes more to the overall daily PA of girls. This, in turn, might lead to larger effects on energy expenditure and weight regulation.

The increasing burden of obesity and inactivity across SES has been well documented.[35] We found evidence that interventions that were delivered to economically deprived children analysed here were less able to induce favourable effects on each of the obesity-related outcomes studied than interventions in general population of children. However, this should be interpreted with caution, as these two groups of interventions differed is several characteristics. Namely, despite having similar characteristics in terms of components of movement behaviour included, involving parents and including a diet component, 35% of interventions directed to disadvantaged children were shorter than 6 months compared to only 22% such interventions addressed at general population. In addition, this group of interventions introduced less PA (median PA duration = 60 compared with 90 min week^{-1} in general population). Still, echoing our findings in 6- to 12-year-old children, a review of obesity interventions that focused on disadvantaged adolescents reported that only two out of six school-based obesity prevention initiatives managed to produce beneficial effects on BMI.[36] As parents of this group of children can be very hard to reach, schools and the community remain the settings that should be a focus of public health policies aimed at reducing health inequalities. On the other hand, it is obvious that school-based intervention strategies directed at underprivileged children need to be redesigned in order to achieve effects observed in more affluent children. To that end, a recent review that analysed the effects of obesity prevention programmes across socio-economic position has shown that interventions targeting individual-level behaviour change may be less successful in disadvantaged children and that structural changes to the environment might be a better approach in reducing inequalities.[22] In addition, addressing social determinants of health outside the school setting is mandatory to ensure a sustainable reduction in the socio-economic disparities in children's health.

Finally, although only a handful of analysed studies provided data on adverse outcomes, we found no evidence for changes in body satisfaction, eating behaviours or underweight prevalence. In addition, the incidence of injuries was very low, even in studies with large volume of PA. Hence, school-based PA programmes can be considered very safe, regardless of the components used or PA volume implemented.

4.1 | Strengths and limitations

Our review has many strengths. First, we did not rely on search strategies set by prior reviews. Instead, we searched eight databases, including grey literature sources. Second, unlike most of the previous similar reviews, we did not limit our search to English language, thus increasing the probability of detecting evidence from low- to middle-income countries. Third, we accepted different study designs instead of constraining to RCTs while insisting on the control group to minimize bias. Fourth, we gathered very detailed data on the content of interventions, with a special reference to the frequency, intensity, duration and type of PA. Fifth, we included measures of body composition instead of relying only on BMI, which is regularly critiqued as an imperfect measure of adiposity. Furthermore, BMI can be affected by PA through an increase in lean body mass, which then typically leads to underestimation of intervention effects on adiposity.

Several limitations of this review are also worth noting. First, although unlike prior reviews we extended our search beyond English language, we could not include non-European languages, so we might have missed studies from Asia or Africa. Second, large variability in intervention characteristics led to statistical heterogeneity, which warrants caution when interpreting the results of meta-analysis. Third, over one third of studies that met inclusion criteria failed to provide all the data needed for a meta-analysis. Given the large number of studies included in the quantitative synthesis, we did not perform a qualitative synthesis of these studies. Hence, we cannot infer that the results of this additional qualitative evidence synthesis would agree with our conclusions. Fourth, a large number of studies describing PA interventions failed to document the exact duration of PA, and even fewer studies have quantified the intensity of implemented activities. This limitation precluded us from describing the dose–response relationship. Fifth, although we restrained from predefining specific obesity-related outcomes, a sufficient number of studies for a meta-analysis were found only for BMI, BMI *z*-score and %BF. Both BMI and BMI *z*-score have often been criticized for inadequately assessing change in adiposity.[37] Abundant evidence has emerged that supports replacing BMI or BMI *z*-score with alternative metrics that can better capture longitudinal changes in obesity (e.g. Percent Over BMI—BMI50 and BMI85).[38] Unfortunately, these alternative metrics are still seldomly considered when examining the effects of PA intervention studies. Similarly, the large variability in reporting prevented us from analysing the effects of PA interventions on overweight prevalence and abdominal obesity. Next, a limited number of studies identified in some subgroups impeded the assessment of effect modification. Finally, as we found only two studies that focused exclusively on reducing sedentary behaviour, we were unable to compute reliable estimates of the effects of such interventions on obesity-related outcomes.

4.2 | Implications for future research

Important gaps in the evidence were uncovered by our analyses. For example, very few studies identified by our review focused exclusively on decreasing sedentary behaviour, and studies that added sedentary behaviour component to a PA or fitness-oriented programme rarely went beyond educational activities. Hence, there is a clear need for more trials with a strict focus on sedentary behaviours, particularly such that would use an innovative approach, aligned with interests and routines of today's children. To this end, we did not find studies that have evaluated the effectiveness of wearable technology (e.g. activity trackers) or smartphones in obesity prevention. In view of excessive reliance on mobile phones of contemporary children, wearable and mobile technology could prove to be a powerful agent in physical fitness enhancement but also in the reduction of screen time. Preliminary evidence that interventions that used screen-based technology have successfully reduced screen time is already available.[39] Next, poor reporting on the dose of PA introduced by obesity prevention interventions precluded us from detecting a 'best buy' quantity of PA that would provide optimal effects with as little time and resources invested as possible. In order to enable such dose–response analyses, future studies should include comprehensive assessment of PA volume introduced and ensure to report this in sufficient detail. Lastly, although we found a number of studies that focused on disadvantaged children, very few studies have examined how the effectiveness of PA interventions varies across different socio-economic strata. Such direct comparisons across populations are warranted to allow tailoring interventions to specific groups of children.

ACKNOWLEDGEMENTS

This study is a part of the Science and Technology for childhood Obesity Policy (STOP) that has received funding from the European Union's Horizon 2020 (H2020 Societal Challenges) research and innovation programme under Grant Agreement No. 774548. P.J.'s and J.K.'s work is funded by the Croatian Science Foundation (Ggrant Nos. DOK-2018-09-8532 and DOK-2018-01-2328, respectively). The authors would like to thank Marijana Briški Gudelj and Rebbeca S. Jones for their dedicated help in designing, adapting and conducting the search of the literature.

CONFLICT OF INTEREST

M.S. reports grant from European Union's Horizon 2020 research and innovation programme under Grant Agreement No. 774548. P.J. and J.K. report grants by the Croatian Science Foundation (Grant Nos. DOK-2018-09-8532 and DOK-2018-01-2328, respectively).

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

How to cite this article: Podnar H, Jurić P, Karuc J, et al. Comparative effectiveness of school-based interventions targeting physical activity, physical fitness or sedentary behaviour on obesity prevention in 6- to 12-year-old children: A systematic review and meta-analysis. *Obesity Reviews*. 2021; 22:e13160. https://doi.org/10.1111/obr.13160