Effectiveness of social prescribing for chronic disease prevention in adults: a systematic review and meta-analysis of randomised controlled trials

Htet Lin Htun ,[1] Achamyeleh Birhanu Teshale ,[1] Miranda S Cumpston ,[1] Lisa Demos,[1,2] Joanne Ryan ,[1] Alice Owen ,[1] Rosanne Freak-Poli [1,3]

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[1]School of Public Health and Preventive Medicine, Monash University, Melbourne, Victoria, Australia
[2]Australian Disease Management Association (ADMA), Melbourne, Victoria, Australia
[3]School of Clinical Sciences at Monash Health, Monash University, Melbourne, Victoria, Australia

Correspondence to
Dr Rosanne Freak-Poli, School of Clinical Sciences at Monash Health, Monash University, Melbourne, Victoria, Australia; rosanne.freak-poli@monash.edu

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ABSTRACT

Background Social prescribing (SP) enables healthcare professionals to link patients with non-medical interventions available in the community to address underlying socioeconomic and behavioural determinants. We synthesised the evidence to understand the effectiveness of SP for chronic disease prevention.

Methods A systematic literature search was conducted using five databases and two registries. Eligible studies included randomised controlled trials of SP among community-dwelling adults recruited from primary care or community setting, investigating any chronic disease risk factors defined by the WHO (behavioural factors: smoking, physical inactivity, unhealthy diet and excessive alcohol consumption; metabolic factors: raised blood pressure, overweight/obesity, hyperlipidaemia and hyperglycaemia). Random effect meta-analyses were performed at two time points: completion of intervention and follow-up after trial.

Results We identified nine reports from eight trials totalling 4621 participants. All studies evaluated SP exercise interventions which were highly heterogeneous regarding the content, duration, frequency and length of follow-up. Majority of studies had some concerns for risk of bias. Meta-analysis revealed that SP likely increased physical activity (completion: mean difference (MD) 21 min/week, 95% CI 3 to 39, I^2=0%; follow-up ≤12 months: MD 19 min/week, 95% CI 8 to 29, I^2=0%). However, SP may not improve markers of adiposity, blood pressure, glucose and serum lipid. There were no eligible studies that primarily target unhealthy diet, smoking and excessive alcohol drinking behaviours.

Conclusions SP exercise interventions probably increased physical activity slightly; however, no benefits were observed for metabolic factors. Determining whether SP is effective in modifying the determinants of chronic diseases and promotes sustainable healthy behaviours is limited by the current evidence of quantification and uncertainty, warranting further rigorous studies.

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WHAT IS ALREADY KNOWN ON THIS TOPIC

⇒ Although many countries are adopting social prescribing interventions to tackle the various social determinants of health, evidence is still sparse and inconclusive.

WHAT THIS STUDY ADDS

⇒ This systematic review and meta-analysis of randomised controlled studies provides the first evidence of social prescribing in modifying the behavioural and metabolic determinants of chronic diseases.

⇒ Meta-analyses were separately performed at two assessment time points (ie, at completion of trial intervention and follow-up after trial intervention), which allowed us to determine whether the beneficial effects, if there is any, can be sustained after the end of interventions.

⇒ Social prescribing exercise interventions were shown to promote physical activity at the trial completion and lasted at least until the end of follow-up duration (≤12 months), although they had little to no difference on markers of adiposity, blood pressure and serum lipid.

HOW THIS STUDY MIGHT AFFECT RESEARCH, PRACTICE OR POLICY

⇒ To have a complete understanding of the potential of social prescribing in modifying the chronic disease risk factors, further systematic and rigorous evaluations of social prescribing programmes are required due to methodological shortcomings in existing research and uncertainty.

INTRODUCTION

The World Health Organization (WHO) estimated that major chronic non-communicable diseases such as cardiovascular diseases, cancers, chronic respiratory diseases and diabetes are responsible for 41 million deaths annually, equivalent to over 7 out of 10 deaths worldwide,[1] posing a serious global public health concern. It is now increasingly recognised that in addition to pathophysiological causes, a range of social determinants significantly impacts the risk of chronic diseases.[2–4] Therefore, adherence to the traditional biomedical model of care has created deficits in the care continuum, particularly for non-communicable chronic illnesses. A paradigm shift towards the biopsychosocial care model has become necessary to develop a potential solution by extending the framework to include a previously neglected set of socioeconomic, behavioural and lifestyle determinants.[5]

In order to address these factors, a non-medical intervention known as 'social prescribing' (SP) is being proposed as an innovative holistic

community-centred approach to support healthcare professionals to improve care delivery and health outcomes.[6] There is no universally agreed definition of SP; however, it is generally defined as the process of enabling healthcare professionals to refer patients to a link worker to initiate a non-clinical social prescription to improve their health and well-being.[7] Although varying considerably within and across countries, a typical SP pathway comprises a referral from a general practitioner (GP) to a link worker who assesses the individual's needs and makes an onward referral to the available non-clinical community resources.[8] However, link worker involvement is not always an essential ingredient in SP.

Prescribing non-medical interventions was first introduced in the United Kingdom (UK) in the 1990s with exercise referral, followed by arts.[9] As such, exercise referral programmes paved the way for SP when the theoretical concept was in place, and the healthcare system has established the referral pathway from primary healthcare providers to community sectors. Although it may be arguable to state that SP originates from exercise referral schemes (ERS), it was the first scheme widely practised in the healthcare systems underpinned by the SP framework. Recently, SP has evolved into a more comprehensive service encompassing various social activities that voluntary and community sector organisations typically deliver. It is now growing internationally, with initiatives found in at least 17 countries as of 2021.[10] Morse *et al*[10] presented examples of prescribed social interventions based on the context: (1) lifestyle interventions to improve health behaviours (eg, exercise, diet, smoking); (2) services that address material needs (eg, food, housing, transportation) and (3) programmes to develop professional skills or social activities (eg, job training, education, volunteering, befriending, arts and crafts).

The beneficial effects of SP interventions have been demonstrated in several studies, including improving individuals' physical, mental and social health.[11–19] At the system level, some studies have shown reduced healthcare utilisation[13 19] and cost-effectiveness.[20] SP programmes are now progressively promoted as not only a scheme that can benefit patients but also a policy alternative that can alleviate the burden on the healthcare system.[21 22] Previous systematic reviews have explored the impact of SP on a range of individual's health outcomes including general health, quality of life, mental and social well-being.[23–31] Several of these reviews have identified the benefits of SP such as improvement in self-reported health,[24 26] well-being,[24 26] health-related behaviours,[26 30] quality of life,[27] self-esteem,[25 26] mental well-being,[25 27 28] anxiety[25 30] and depression.[25 30] A positive impact on social health including strengthening social contacts,[24 26 28] and minimising isolation[30] were also reported. However, some reviews concluded that there was little to no impact on loneliness, health and well-being measures.[23 29 31] Additionally, some studies gathered evidence around the effectiveness of SP on community/system levels[31 32] and cost-effectiveness,[31] or for specific populations such as migrants, older adults, people with diabetes and autism spectrum disorder.[33–36] More importantly, these reviews emphasised that evidence was mostly mixed with very low certainty, resulting in largely inconclusive findings. To date, no systematic review has evaluated the effect of SP with a primary focus on major health outcomes related to chronic non-communicable diseases. Given the uncertainty in the effectiveness of SP programmes and to fill the knowledge gap, we were motivated to conduct a systematic review and meta-analysis of randomised controlled trials (RCTs) aimed at evaluating whether SP through primary care modifies the determinants of chronic diseases in the community-dwelling adults.

METHODS

The protocol for this review was registered in PROSPERO. We conducted this review using the Cochrane Handbook for Systematic Reviews of Interventions[37] and reported under the Preferred Reporting Items for Systematic reviews and Meta-Analyses 2020 guidelines (online supplemental table 1).[38]

Data sources and search strategy

We systematically searched the following five databases via the Ovid platform and two registries from inception until 27 July 2022: MEDLINE in-process and other non-indexed citations, Embase classic and Embase, PsycINFO, the Cochrane central register of controlled trials (CENTRAL), Allied and Complementary Medicine Database (AMED), ClinicalTrials.gov and International Clinical Trials Registry Platform (ICTRP). Notably, CENTRAL is a concentrated source for bibliographic reports of RCTs, created from multiple bibliographic databases (including CINAHL).[39]

We developed our search strategy in collaboration with a specialist librarian using three concepts: (1) risk factors of chronic diseases, (2) SP and (3) RCTs (online supplemental tables 2-6). Our preliminary search strategy was first tested and refined to achieve high sensitivity (ie, ensuring that known studies were identified). During testing, we found that using the 'explode' function generated unmanageable records (>1 million). Therefore, with the librarian's support, we selected the most relevant subject headings by opening the subject trees in each database. We applied the MEDLINE strategy to other databases using database-specific subject headings. Backward and forward citation searching of included studies was performed using the Web of Science on 31 August 2022, to identify additional records. We did not restrict language, country and year of publication.

Eligibility criteria

Inclusion and exclusion criteria were defined using population, intervention, comparison, outcomes and study design (PICOS) framework[40] by contextualising the concept of SP.

Population

Eligible studies included community-dwelling adults recruited from primary care or community setting through screening or referral by healthcare workers or link workers. Studies involving institutionalised individuals, patients in end-of-life palliative care, participants recruited from non-primary care settings and volunteers were excluded.

Intervention

Although patients were first assessed and managed at clinical setting and referred by primary healthcare providers for SP, our criteria for exclusion were based on the community setting where SP intervention was delivered. Studies delivering community-based SP interventions led by non-healthcare professionals and linked the patients with available community resources were eligible. No exclusion was set based on the involvement of link workers and the level of therapist input. Studies were excluded if SP intervention: (1) was given by healthcare professionals, (2) was delivered in non-community/non-primary care settings such as hospitals and workplaces, (3) provided only general health advice, health education, health messaging or motivational interviewing without onward referral to gain access to community social services and (4) involved medical or pharmacological prescriptions as part of, or along with, the

social interventions being delivered in the community. To clarify, medical or pharmacological prescriptions provided by primary healthcare providers in a clinical setting did not affect the eligibility.

Comparator/control

The control group could include individuals undertaking a non-SP intervention, standard care, those on a waitlist or no intervention.

Outcomes

Studies measuring at least one chronic disease risk factor objectively or subjectively at any time point, in addition to baseline, were eligible. In this study, we adopted the WHO's four behavioural (tobacco smoking, physical inactivity, unhealthy diet and excessive alcohol consumption) and four metabolic (raised blood pressure, overweight/obesity, hyperlipidaemia and hyperglycaemia) risk factors of chronic illnesses.[1]

Study design

Only RCTs were included. We excluded observational studies, quasi-experimental trials, editorials and review articles.

Study selection

Identified reports were first deduplicated using EndNote V.20[41] before importing to Covidence[42] to perform a two-stage screening process to select studies meeting inclusion criteria. Both title and abstract screening and full-text review were assessed independently by two reviewers (HLH and ABT), and discrepancies were resolved by discussion. The corresponding authors were contacted when further information was required. The proportionate agreement at the full-text review stage was 98.5% (Cohen's κ=0.59).

Data extraction

Two reviewers (HLH and ABT) independently extracted the data, and disagreements were resolved through discussion. A standardised data collection form was used, including the following fields: title, authors, year of publication, study design, period and setting, sample size, inclusion and exclusion criteria, method of recruitment, participant characteristics, intervention details, control group descriptions, outcome measures, assessment time points and study results. WebPlotDigitizer V.4.6 was used to retrieve data when data were reported in graphical format.[43 44]

Risk of bias assessment

HLH and ABT independently assessed the risk of bias on the effect of assignment (intention-to-treat) for physical activity and body mass index (BMI) at the completion of intervention using version 2 of the Cochrane Risk of Bias tool (RoB 2),[45] which contains five domains of bias: randomisation, deviations from intended intervention, missing outcome data, measurement of the outcome and selection of the reported result. In addition to five domains, bias arising from the timing of identification and recruitment was assessed in cluster RCTs. Judgement for each domain was rated as either 'low', 'some concerns' or 'high' risk of bias. We classified the overall risk of bias as low if all domains were at low risk of bias, as high if at least one domain was at high risk of bias or as some concerns if at least one domain was at some concerns but none in high risk of bias. Disagreements were resolved through revision and discussion. There was no persisting difference in judgement; thus, a third reviewer's opinion was not sought. No study was excluded based on its quality. The results were visualised using *robvis*.[46]

Data synthesis and analysis

Study characteristics, sociodemographics of participants and intervention details were summarised descriptively. Post-intervention outcome data were organised in two domains: (1) at the completion of the trial intervention and (2) follow-up after the trial completion. We used the 4-month outcomes as 'at completion' values in Taylor's study[47] because it was the closest to the completion time point, although the intervention length was 2.5 months. We chose the more distal values when the outcomes were assessed more than once during follow-up.

When sufficiently similar outcome measures and reported statistics were available, we performed a meta-analysis comparing post-intervention data between the two arms. Where necessary, medians, IQRs and SEs of the mean were transformed into means and SD.[37 48] For a multiarm trial study (eg, intervention A vs intervention B vs control), we included each pairwise comparison in the meta-analysis (ie, intervention A vs control; and intervention B vs control). Unit-of-analysis error was then accounted for by dividing the control group sample size in half while keeping the mean and SD unchanged.[37] When more than one similar measurement was available for meta-analysis in the same outcome group, we selected the outcome measured in more studies (eg, BMI in preference to weight). Measurements of physical activity (ie, duration of physical activity and energy expenditure) were pooled separately. We standardised the unit of measurements when necessary (eg, conversion of high-density lipoprotein (HDL) (1 mmol/L=38.67 mg/dL) and triglycerides (1 mmol/L=88.57 mg/dL) to International System of Units).

We synthesised standardised mean differences (SMD) estimated by Hedges's g and 95% CI for studies using different scales to measure the outcomes (ie, physical activity domain) or mean differences (MD) otherwise. A random-effects DerSimonian-Laird model[49] was used based on the assumption that population, intervention and methodological heterogeneity were likely to exist. Statistical heterogeneity was assessed by calculating the I^2 statistic.[50] For interpretability, we re-expressed SMD and calculated the absolute difference in means by multiplying the SMD by an estimate of the SD associated with the most appropriate instrument.[40] Statistical significance was set at 5%, and all statistical analyses were undertaken using Stata/SE17 (College Station, Texas).[51]

Certainty assessment

We summarised the confidence in the body of evidence for intervention effect using the GRADE (Grading of Recommendations, Assessment, Development and Evaluations) framework[52] assessed for five selected main outcomes at both completion and follow-up time points. We considered the five standard domains (risk of bias, inconsistency, indirectness, imprecision and publication bias) for downgrading evidence to inform an overall assessment of certainty for each outcome, which was judged to be high, moderate, low or very low. Due to low number of studies in each outcome, quantitative publication bias estimation through funnel plot and Egger's test would be unreliable.[37] Hence, publication bias was conceptually assessed by identifying whether there was evidence of the selective under-reporting of studies that showed no effect.[37] All assessments were performed by HLH and verified by MSC.

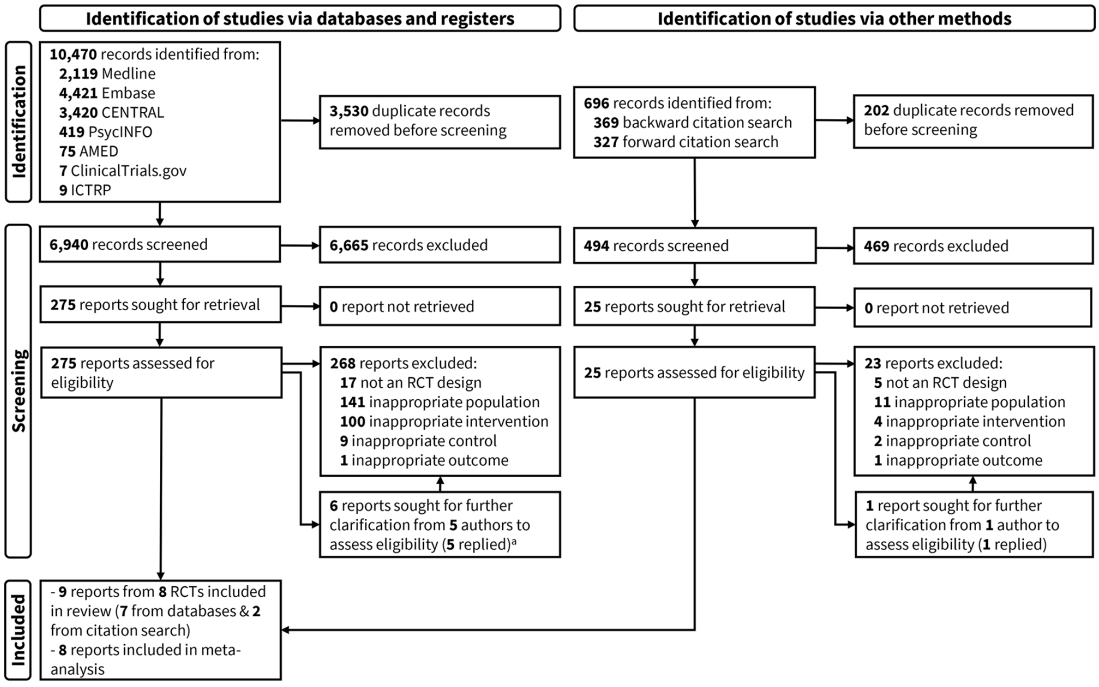

Figure 1 PRISMA flow diagram. [a]Two reports had the same corresponding author. All authors replied and all were ineligible. PRISMA, Preferred Reporting Items for Systematic reviews and Meta-Analyses; RCT, randomised controlled trial.

RESULTS

Study selection

The search strategy yielded 10 470 records, with 6940 unique records remaining after removing duplicates. Of these, 6665 records were excluded after screening titles and abstracts. Of the 275 retrieved articles, 268 papers were excluded after full-text review (online supplemental table 7). Thus, seven records met the inclusion criteria, and two were additionally identified from citation tracking (figure 1).

Study characteristics

Online supplemental table 8 presents the characteristics of the eight included trials (with nine publications as two articles arose from the same trial). There were seven individually-randomised parallel group designs and one cluster-randomised trial, evaluating a total of 4621 participants (range: 127–2160), including 2497 and 2124 participants in the intervention and control arms, respectively. All included articles were reported in English. No unpublished or ongoing trials were found. The earliest study was published in 1998; half of the remaining were published in the past 5 years, and the other half dated back to 2004–2012. Five trials originated from the UK,[47 53–56] and one trial each from Spain,[57] China[58] and Mexico.[59 60] Although primary care physicians or nurses were the main referrers in all studies, one study reported an additional effort of recruitment through advertisements at public places (7.6% of total participants).[58] One study re-randomised the control group to provide interventions, resulting in a larger number of participants at follow-up assessment.[54] Despite broad recruitment eligibility, most primary studies restricted eligibility to people with long-term health conditions (predefined cardiovascular risk factors: n=3,[53–55] hypertension: n=3,[47 58–60] overweight: n=3,[47 56 57] raised blood glucose: n=1[56] and mental health issues: n=2).[55 57] One trial included only women,[57] whereas the remaining seven recruited 30%–69% men. The age eligibility criteria varied across trials, with two recruiting all adults (>16[55] or ≥18[53] years), four recruiting adults under 70 or 75 years (starting from 18,[56] 35,[59 60] or 40[47 54] years), and two recruiting older adults aged ≥60 years.[57 58] This resulted in differing mean ages of recruited participants, approximately 50 years in three trials,[53 55 59 60] 55 years in one trial,[47] 60 years in two trials[54 56] and 70 years in two trials.[57 58]

Intervention characteristics

All eligible studies in our review delivered exercise interventions for physical activity promotion (online supplemental table 8). Most studies evaluated exercise referral programmes, whereas a study from China[58] investigated the effect of group-based Tai chi exercise. Although the core component was group education sessions to promote changes in diet and physical activity in Smith's study,[56] additional support was provided to engage in individually tailored activities such as walking groups, exercise, cooking and relaxation classes through existing community services. Scheme duration was typically 2.5–4 months,[47 53–55 59 60] and one further provided 8 months telephone contact to prevent relapse.[55] Three studies implemented a longer intervention (six[57 58] or 12 months[56]). Most schemes took place in leisure/community centres,[47 53–55 58–60] parks (and forest tracks)[54 57] or various community venues.[56] Most trials offered one-to-one or group-based exercises actively led by qualified exercise instructors,[53–55 57–60] whereas professional help was available only on a request basis in Taylor's study.[47] One recent study engaged volunteers to provide community services.[56] Exercise sessions were usually 2–3 times per week, lasting for an hour, and were offered for free or with a subsidised rate. Control participants mostly received written and verbal information on the benefits of physical activity or the usual management in the primary care setting.

Table 1 Outcomes assessed in primary studies

Outcomes	Gallegos-Carrillo 2017 [59] *	Gallegos-Carrillo 2021 [60] *	Gusi 2008 [57] †	Harrison 2005 [53] ‡	Isaacs 2007 [54] §	Ma 2018 [58] ¶	Murphy 2012 [55] **	Smith 2019 [56] ††	Taylor 1998 [47] ‡‡
Physical activity									
Physical activity assessed by accelerometer	✓							✓	
International physical activity questionnaire	✓								
7-day physical activity recall				✓	✓		✓		✓
New Zealand physical activity questionnaire								✓	
Energy expenditure					✓				✓
Diet									
27-item fat and fibre questionnaire								✓	
Overweight/obesity									
Body mass index		✓	✓		✓	✓		✓	✓
Absolute weight loss								✓	
Relative weight loss (>3% or >5%)								✓	
Weight		✓			✓				
Waist circumference		✓				✓		✓	
Waist-hip ratio		✓			✓				
Percentage body fat					✓				
Sum of skinfolds									✓
Raised blood pressure									
Systolic blood pressure		✓			✓	✓		✓	✓
Diastolic blood pressure		✓			✓	✓		✓	✓
Hyperlipidaemia									
Total cholesterol					✓				
High-density lipoprotein		✓			✓				
Low-density lipoprotein					✓				
Triglycerides		✓			✓				
Hyperglycaemia									
Fasting blood glucose		✓							
Glycated haemoglobin (HbA1c)								✓	

Outcomes were assessed at following time points.
[a]assessment during the trial, [b]assessment at the completion of trial, [c]assessment during follow-up after completion of trial.
*Baseline, 4 months[b], 6 months[c].
†Baseline, 6 months[b].
‡Baseline, 6 months[c], 9 months[c], 12 months[c].
§Baseline, 2.5 months[b], 6 months[c] (additional assessment at 12 months for intervention arms).
¶Baseline, 6 months[b].
**Baseline, 12 months[c].
††Baseline, 6 months[b] (additional assessment at 12 months for intervention arm).
‡‡Baseline, 2 months[a], 4 months[b], 6.5 months[c], 9 months[c].

Outcome measures

A summary of outcomes is shown in table 1, and the summary statistics and effect estimates for all outcomes collected are presented in online supplemental table 9.

Eligible studies measured the two behavioural (physical activity: n=6,[47 53–56 59] and diet: n=1)[56] and four metabolic (anthropometric measures for weight: n=6,[47 54 56–58 60] hypertension: n=5,[47 54 56 58 60] hyperlipidaemia: n=2[54 60] and hyperglycaemia: n=2)[56 60] factors.

Behavioural factors. Various measures were used in six studies to assess physical activity, including (1) either self-reported or accelerometer-based moderate physical activity (MPA),[47] vigorous physical activity (VPA),[47] moderate-to-vigorous physical activity (MVPA),[47 54 56 59] total physical activity[54 55] and (2) energy expenditure.[47 54] Two studies further reported a categorical outcome of physical activity, that is, people engaging in ≥90[53] or ≥150[59] min/week of MVPA. One study used a 27-item fat and fibre questionnaire to report a dietary behaviour fat and fibre score.[56]

Metabolic factors. BMI,[47 54 56–58 60] absolute weight[54 60] or weight loss from baseline,[56] waist circumference,[56 58 60] waist-hip ratio,[54 60] body fat percentage,[54] a sum of skinfolds[47] were used to assess weight status in six studies. Five trials[47 54 56 58 60] examined systolic blood pressure (SBP) and diastolic blood pressure (DBP) for hypertension. Fasting blood glucose (FBG)[60] and glycated haemoglobin (HbA1c)[56] were studied for hyperglycaemia. A lipid panel including HDL,[54 60] low-density lipoprotein (LDL),[54] triglycerides[54 60] and total serum cholesterol[54] was examined for hyperlipidaemia in two trials.

Of eight trials, six[47 54 56–60] reported the exit assessments at the completion of intervention, and five[47 53–55 59 60] reported follow-up measures ranging between 6 and 12 months from baseline assessment (which corresponded to 2–9 months from the end of intervention). Three trials[47 54 59 60] evaluated outcomes

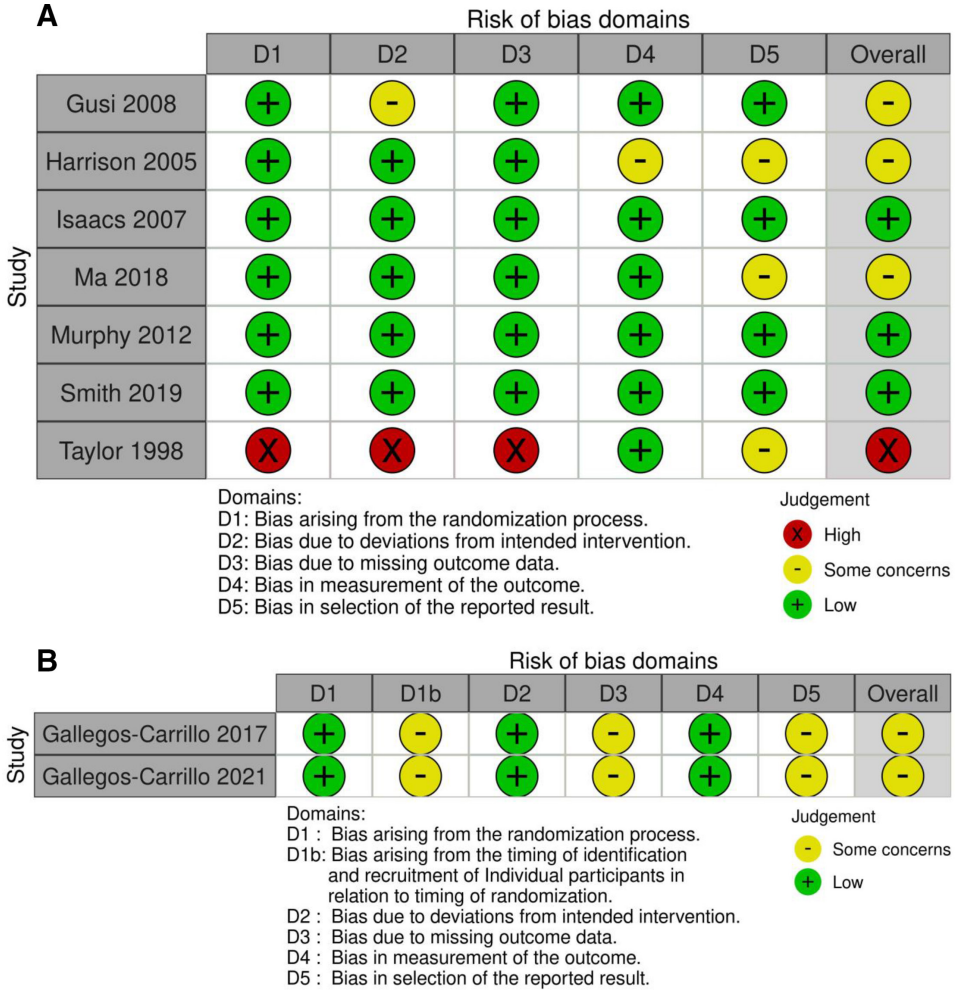

Figure 2 Risk of bias assessment for (A) individually-randomised parallel group trials and (B) cluster-randomised trials.

at both completion and follow-up. Two studies[47 53] measured outcomes at >1 time point during follow-up. Two studies[54 56] performed an extended outcome assessment only for participants receiving the intervention.

Risk of bias assessment

Risk of bias assessment of physical activity and/or BMI at completion of trial revealed a low risk in three studies,[54–56] some concerns in five studies[53 57–60] and a high risk in one study[47] (figure 2). Some concerns of bias originated from the randomisation process,[57] missing outcome data,[59 60] outcome measurement,[53] selection of the reported result[53 58–60] and the timing of identification and recruitment of individual participants in relation to randomisation.[59 60] One study[47] was rated high risk of bias because of randomisation, deviations from the intended intervention and missing outcome data.

Effect of intervention on study outcomes

We present our findings along with certainty of evidence to assist in interpretation. A summary of findings with the GRADE assessments for main outcomes was described in online supplemental tables 10 and 11.

Physical activity

Four studies assessed engagement of physical activity per week at completion,[47 54 56 59] with meta-analysis (figure 3) demonstrating that SP likely increased physical activity slightly (SMD 0.16, 95% CI 0.02 to 0.29, I^2=0%, moderate certainty). Similarly, an intervention effect was noted during follow-up by pooling four studies[47 54 55 59] (SMD 0.14, 95% CI 0.06 to 0.22, I^2=0%, moderate certainty). We performed a sensitivity analysis by excluding Murphy's study[55] that reported median and IQR during follow-up, and a similar result was found (SMD 0.14, 95% CI 0.02 to 0.26, I^2=0%) (data not shown). Evaluation of two studies[47 54] found that intervention probably increased the energy expenditure at follow-up (SMD 0.19, 95% CI 0.06 to 0.32, I^2=0%) but not at the point of completion (SMD 0.16, 95% CI −0.13 to 0.45, I^2=56%) (online supplemental figure 1).

We re-expressed the pooled SMD estimates using SDs from Isaac's trial[54] for an interpretation. On average, the MVPA in the intervention group was 21 (3–39) min/week and 19 (8–29) min/week higher than the comparison group at completion and follow-up, respectively. The energy expenditure was on average 5 (2–9) kcal/kg/week higher at follow-up.

Dietary habit

One study[56] evaluated dietary behaviour at the completion of the trial and reported a significant positive effect of the intervention on dietary fat (MD −0.11, 95% CI −0.19 to −0.03) and fibre score (MD 0.11, 95% CI 0.02 to 0.20) (online supplemental table 9).

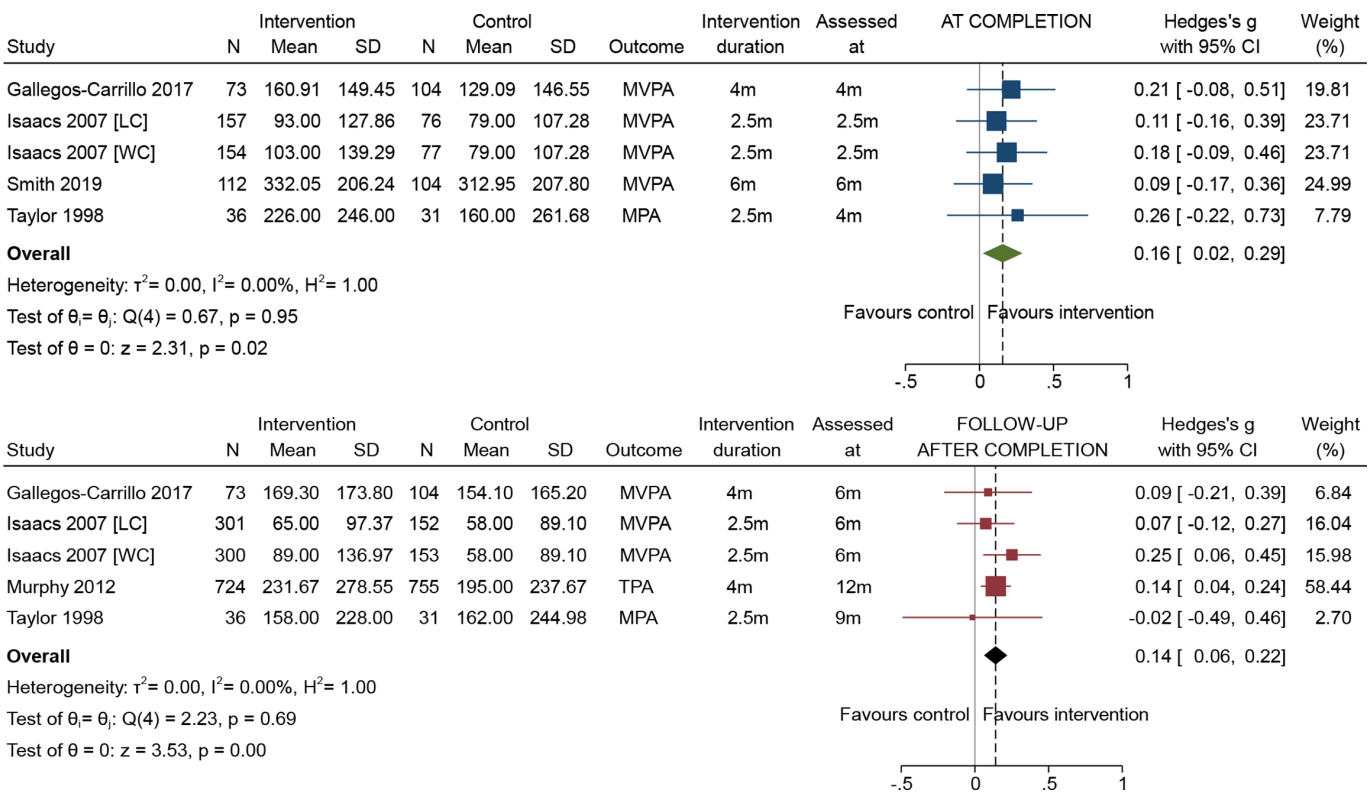

Figure 3 Meta-analysis of physical activity at the completion of intervention and follow-up after completion of intervention. Hedges's g standardised mean differences and 95% CIs were estimated by DerSimonian-Laird random effects model. LC, leisure centre exercise intervention; MPA, moderate physical activity; MVPA, moderate/vigorous physical activity; SD, standard deviation; TPA, total physical activity; WC, group-based walking classes. Isaacs 2007 study re-randomised the control group to provide interventions resulting in a larger number of participants at follow-up assessment.

Anthropometric outcomes

Figure 4A and online supplemental figures 2a-2b illustrate the pooled effect of anthropometric outcomes. SP may not improve BMI at completion (MD 0.13 kg/m^2, 95% CI −0.18 to 0.45, I^2=66%, low certainty) and follow-up (MD 0.03 kg/m^2, 95% CI −0.11 to 0.18, I^2=0%, high certainty) by evaluating six[47 54 56–58 60] and three trials,[47 54 60] respectively. The evidence from three studies[56 58 60] suggested that intervention may not reduce waist circumference at completion (MD 0.70 cm, 95% CI −1.61 to 3.01, I^2=62%). One study assessed waist circumference at follow-up,[60] and no effective reduction was noted (MD 2.90 cm, 95% CI 0.19 to 5.61). Pooled estimates of two studies[54 60] examining waist-hip ratio suggested little to no difference at both completion (MD −0.00, 95% CI −0.02 to 0.01, I^2=82%) and follow-up (MD 0.00, 95% CI −0.01 to 0.01, I^2=0%). Additional anthropometric outcomes are presented in online supplemental table 9.

Hypertension

Meta-analysis of five studies[47 54 56 58 60] showed little to no difference in SBP (at completion: MD 0.41 mm Hg, 95% CI −1.25 to 2.07, I^2=8%, high certainty; follow-up: MD −0.21 mm Hg, 95% CI −1.68 to 1.26, I^2=0%, high certainty; figure 4B) and DBP (at completion: MD −0.24 mm Hg, 95% CI −1.77 to 1.29, I^2=61%; follow-up: MD 0.85 mm Hg, 95% CI −0.93 to 2.63, I^2=66%; online supplemental figure 2c).

Hyperlipidaemia

Intervention likely results in little to no difference in serum lipid values: (1) HDL at completion: −0.02 mmol/L, 95% CI −0.05 to 0.02, I^2=0%, moderate certainty; follow-up: −0.01 mmol/L, 95% CI −0.04 to 0.01, I^2=0%, high certainty; (2) LDL at completion: −0.01 mmol/L, 95% CI −0.11 to 0.08, I^2=0%, moderate certainty; follow-up: 0.01 mmol/L, 95% CI −0.06 to 0.08, I^2=0%, moderate certainty; (3) triglycerides at completion: −0.03 mmol/L, 95% CI −0.16 to 0.10, I^2=0%; follow-up: 0.00 mmol/L, 95% CI −0.10 to 0.10, I^2=0% and (4) total cholesterol at completion: −0.03 mmol/L, 95% CI −0.13 to 0.08, I^2=0%; follow-up: 0.00 mmol/L, 95% CI −0.08 to 0.09, I^2=20% (figure 4C,D, online supplemental figure 2d-2e).

Hyperglycaemia

Meta-analysis was not performed due to insufficient data. FBG (at completion: −2.70 mg/dL, 95% CI −10.36 to 4.96; follow-up: −2.70 mg/dL, 95% CI −10.61 to 5.21) and HbA1c (at completion: −0.47 mmol/mol, 95% CI −1.55 to 0.61) were measured in Gallegos-Carrillo 2021[60] and Smith 2019[56] studies respectively, indicating the trivial improvement (online supplemental table 9).

Subgroup analysis

Only exercise interventions were present in our review, and aggregated outcomes were reported despite recruiting individuals with multiple comorbidities. These rendered subgroup analyses, as registered in our protocol, unfeasible.

Adverse events

Two studies[54 56] reported adverse events (online supplemental tables 10 and 11). In one study, the number of primary care

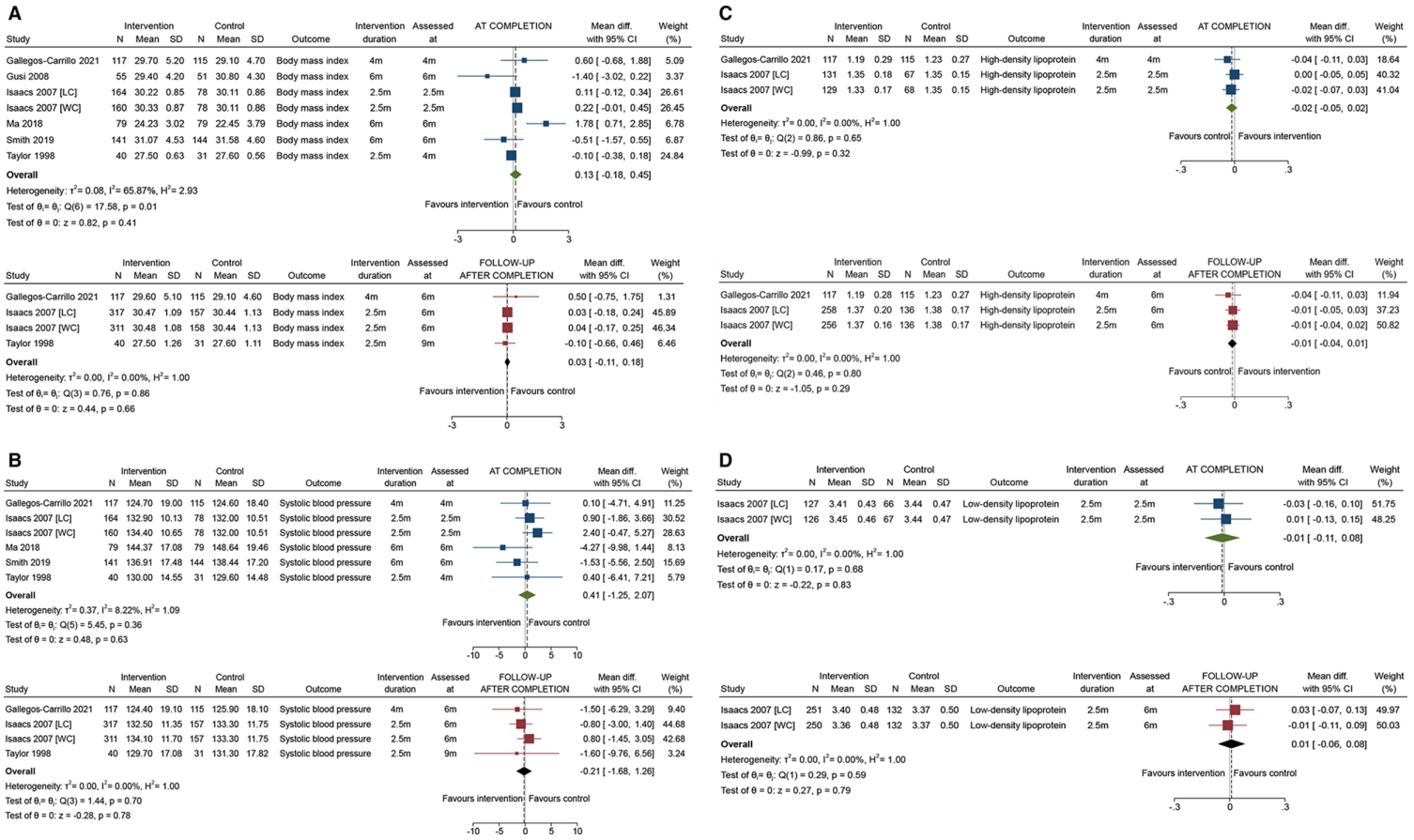

Figure 4 Meta-analysis of (A) body mass index, (B) systolic blood pressure, (C) high-density lipoprotein and (D) low-density lipoprotein at the completion of intervention and follow-up after completion of intervention. Mean differences and 95% confidence intervals were estimated by DerSimonian-Laird random effect model. LC, leisure centre exercise intervention; SD, standard deviation; WC, group-based walking classes. Isaacs 2007 study re-randomised the control group to provide interventions resulting in a larger number of participants at follow-up assessment.

visits due to falls increased slightly in the intervention groups.[54] Another study[56] revealed four non-serious short-term adverse events from increased exercises in the intervention group (pelvic pain, low back pain, shoulder injury, aggravation of existing sciatica), and none reported in control group.

DISCUSSION

Main findings

In this systematic review of eight RCTs conducted mainly in the UK, the core intervention components of most primary studies were SP exercise interventions. They were mostly ERS, and this might be due to the history of prescribing non-medical interventions in the UK. Although it varies, a standard ERS model comprises a referral by primary healthcare providers (GPs, nurses and disorder-specific specialists) to external service providers. The intervention usually occurs at leisure centres in the community delivered by exercise professionals.[61] As such, ERS included in this study operated within our definition of the SP model and pathway. Due to the close similarity between ERS and SP, the UK naturally leads in SP. The evidence base is expected to grow globally with increasing numbers of different SP schemes.

Several publications have reviewed ERS,[62–67] but our study additionally highlighted the evidence in the context of SP since not all ERS fit into the SP model due to intervention or participant characteristics. For example, ERS provided by allied health professionals in non-community settings are not considered SP in this study, and its effectiveness might be different due to factors such as personalised exercise intensity or continuity and coordination of care, requiring further investigations. We demonstrated a moderately certain intervention effect in physical activity promotion. This finding corroborates a previous study that meta-analysed the five RCTs documenting a significant increase in the proportion of participants engaging in MPA (pooled RR 1.20, 95% CI 1.06 to 1.35).[63] Similarly, Pavey *et al* found that the proportion of individuals achieving 90–150 min of activity of at least moderate intensity per week increased at 6–12 months follow-up (pooled RR 1.16, 95% CI 1.03 to 1.30) by pooling four studies.[64] An updated analysis of Pavey's study comprising five primary studies also reported similar finding (pooled RR 1.12, 95% CI 1.04 to 1.20).[65] Three out of four RCTs we included in the present review to study physical activity outcome were found in their analyses.[47 54 55]

We noted that the intervention appeared to have a legacy effect, increasing the energy expenditure after completion of the trial. Two previous reviews showed a non-significant effect of ERS on energy expenditure compared with usual care.[64 65] The authors analysed the findings from the same two RCTs included in this study. However, unlike the previous reviews, we separately analysed the two intervention arms (leisure-centre exercises and group walking classes) of Isaacs' study[54] at two time points and used a random-effect model. Our finding was consistent with a recent uncontrolled before–after study that evaluated the Luton SP programme linking individuals to third-sector service providers to initiate physical activities, gardening, social activities, stress management and relaxation.[68] They found that energy expenditure measured by metabolic equivalent per week increased post-intervention in all levels of physical activities. Our findings strengthen the suggestion that there may be an immediate but slight increase in physical activity at the completion of the trial and sustained until the end of the follow-up assessment, indicating that a behavioural shift could possibly be achieved. A study evaluating the extended time points only for intervention groups (data not included in meta-analysis) also showed that substantial and statistically significant beneficial effect of physical activity was still maintained compared with baseline at 12 months.[54]

By contrast, we showed no evidence to support the notion that SP exercise interventions improve the biological markers of adiposity, blood pressure and serum lipid values. Similarly, no improvement in metabolic outcomes was observed when one of the included studies extended the assessment 12 months from baseline.[56] Our findings align with Pavey *et al*'s systematic review of ERS,[64] which searched the literature until July 2011, but the results remained unchanged when Campbell *et al*[65] updated the review by extending the search to June 2013. The absence of evidence in serum lipid improvement in the current study may be attributed to the fact that the baseline mean cholesterol of the majority of participants were already within optimal or near-optimal range or that exercise was not of sufficient intensity. Likewise, baseline BP in most studies were within the normal range or at stage 1 hypertension.[69] A significant improvement in DBP was found in a study that recruited participants with physician-diagnosed essential hypertension (mean baseline BP∼150/90 mm Hg (ie, stage 2 hypertension)) and provided Tai chi group exercises for 6 months.[58] However, inconsistent findings of the association between Tai chi and BP were reported in earlier systematic reviews.[70–72] A previous review that analysed the data from ERS studies concluded that patients with underlying cardiovascular disorders showed significant reductions in BP and BMI.[67] When comparing against previous ERS reviews, one important criterion to highlight is that our eligible studies involved participants referred to the programmes through primary healthcare providers, underpinned by the SP concept. We excluded any studies that recruited volunteers (except Ma *et al*'s study[58] due to a small proportion of volunteers (7.6%)). This may have contributed to the inconsistent findings as the volunteering participants would likely be more intrinsically motivated to become physically active, according to self-determination theory,[73] in addition to the possibility of confounding due to observational study designs included in prior reviews. Furthermore, research has previously shown that exercise intensity and duration have to be sufficiently high to significantly improve BP.[74] An RCT study also highlighted that 52-week weight loss programme produced greater benefits on metabolic outcomes than 12-week programme.[75] It is, therefore, possible that differences in baseline values, severity of underlying diseases, medication use, volunteer effect, insufficient intervention intensity or duration could have contributed towards the inconclusive beneficial effect on BP and anthropometric indices.

No effect on glycaemic control was found in two studies,[56 60] a finding consistent with a prior review.[64] The interventions in these studies may not be optimal for addressing blood glucose management.[56 60] However, a recent UK study found that type 2 diabetes patients in the intervention group experienced a slight reduction in HbA1c (−0.10% (95% CI −0.17 to −0.03)) compared with the control group.[14] Their scheme involved a referral from primary healthcare providers to community health workers to identify condition management and social needs goals and to support patients in navigating through community resources to address these goals. Participants could remain within the intervention for up to 2 years. Nonetheless, further investigation is required to determine the effectiveness of SP in glycaemic management.

Strengths and limitations

A key strength of this systematic review is that it provides the first evidence of SP in addressing the determinants of chronic diseases by comprehensively synthesising the available data and applying the methodologically rigorous standards of conduct and reporting. It is recommended that this study should be interpreted together with other prior reviews to understand the impact of SP on the person (ie, physical, mental and social well-being), community groups

and the healthcare system.[23–36] A further strength was performing meta-analyses separately at two assessment points. This allowed us to determine whether the beneficial effects, if any, can be sustained post-intervention. We conducted a reproducible and rigorous search using five electronic databases and two international trial registries, thus ensuring to include all relevant publications. To reduce selection and information biases, two reviewers independently screened the literature, performed data extraction, assessed the risk of bias using the standardised RoB 2 tool and evaluated the certainty in the evidence. Furthermore, we did not apply language, period and country restrictions in the search strategy. This has ensured maximise the generalisability of our findings. We tried to extract and analyse as much data as possible (eg, graphical data extraction, conversion from median to mean) to reduce the bias arising from the exclusion of studies despite potentially having a margin of error associated with such approaches.

Our study has several limitations. First, although all eligible studies evaluated exercise programmes, considerable heterogeneity was noted in terms of the diversity of the intervention, including their structure, content, duration, frequency and length of follow-up, in addition to variability in participants and outcome measurements. This may have limited the generalisability of our findings. Second, while we intentionally focused on RCTs from peer-reviewed journals to achieve our aim of synthesising the best evidence, we acknowledge that body of evidence from non-controlled studies and non-peer-reviewed publications may have been missed. Third, our results indicate that the evidence base is pooled mainly within the UK. Therefore, differences in population characteristics, exercise behaviour, SP pathway and its linkage with the healthcare system, accessibility and availability of community services and voluntary organisations might have limited the interpretation and adoption of our findings in non-UK or low-income or middle-income settings. Fourth, although comprehensive search terms were used, no eligible studies examining smoking or excessive drinking behaviours were found. We could not perform a meta-analysis for hyperglycaemia and dietary behaviour outcomes due to data inadequacy, weighing uncertainty further. Likewise, we could not explore heterogeneity, quantitatively assess publication bias through funnel plot and Egger's test or conduct the planned subgroup analysis to test the benefits among different types of interventions and populations based on mental and social health conditions, given too few studies and the limitations of the data obtained. However, we anticipate that more diverse SP interventions and health outcomes evaluations will rapidly emerge in the literature, alongside recent global interest. Fifth, although included studies provided semi-flexible interventions within the scheme of pre-planned exercise routines for a larger pool of participants, there is a possibility that the effectiveness of individually co-designed exercise intervention, in line with the person's wishes, might be different. Therefore, our findings might be restricted to people willing to undertake the existing exercise programmes. Sixth, we observed that only one-third of our eligible studies had a low risk of bias, the certainty of the evidence in most outcomes was moderate, maximum follow-up was 12 months, and reporting of outcomes was inconsistent. These reduce our confidence in demonstrating the overall picture of clinically meaningful differences. Finally, we hypothesised that older participants are more likely to receive the benefits of SP exercise interventions, considering the fact that the two studies[57 58] restricting age ≥60 years seems to have a greater improvement in BMI or BP and older adults have a higher likelihood of worsen metabolic markers of health. While the primary studies had a wide range of eligibility for age, four had an upper limit of 70–75 years, and two had a lower limit of 60 years. As our included studies had fewer participants <40 years, along with the likelihood of better health, the benefits of SP exercise interventions we observed may not be generalisable to younger cohorts.

Implications for future research and practice

Our study resonates with preceding SP reviews regarding the limitations in the available evidence and the need for further high-quality research.[23 24 26 29 34 76–78] As we envisioned, there is limited development and evaluation of SP interventions to address the chronic disease risk factors in adults. It became apparent as we found no comparative RCT studies of non-exercise SP schemes.

The methodological quality of SP research repeatedly received criticism. Reviewing the health, well-being and service utilisation outcomes from 15 SP programmes from the UK, Bickerdike *et al*[23] concluded that most studies were small scale (n<100) and limited by a high risk of bias, poor reporting and design, a lack of standardised measuring tools, short follow-up durations and missing data, in addition to having unclear study population criteria. When this review was recently updated by Griffiths *et al*[24] to include the new evidence until July 2021, the limitations persisted. Similarly, many other reviews increasingly emphasised methodological deficits in the available studies that might undermine the efforts in judging clinical effectiveness.[26 29–31 33 36 76–78] Most biases in our eligible studies stemmed from outcome assessment, missing data and poor reporting. Thus, we further echoed the findings from previous systematic reviews and underlined a need for robust and rigorous research to realise the full potential of SP by addressing the shortcomings mentioned above. Subsequently, Elliott *et al*[77] recommended a series of SP evaluation frameworks and reporting standards to enhance methodological quality. Future SP programmes should adopt a robust evaluation plan built into the schemes as methodologically strong research has become of paramount importance, given that SP has not only attracted global attention but also received support and advocacy at the policy level in some countries.[10 79 80]

Findings from the present review and numerous SP reviews of other health outcomes[23 24 26 27 29 30 33 35 36 76 81] have clearly shown that the evidence was still insufficient and scientifically inconclusive to justify the recommendation reliably. Some immediate increases in physical activity and energy expenditure are noted in our study, but whether the benefits sustain post-trial is questionable. Although the evidence base has fallen behind the demand-driven implementation policy, many healthcare systems around the world recognise SP as an innovative tool that can be embedded within a community setting and integrated into the current and existing clinical practice. In January 2019, the National Health Service (NHS) England issued a long-term action plan called the comprehensive model for personalised care and presented SP as one of six interlinked pillars, taking a whole-system approach.[82] From the policymaker's perspective, suggesting patients undertake an exercise referral or other SP programmes is practical, given that only a small proportion of minor adverse events were reported in two of our eligible studies,[54 56] and SP schemes might be favourable in terms of cost-utility or social return on investment in some populations.[83–86] Moreover, qualitative studies reported that SP is well-received among patients revealing that interventions cultivated the feeling of self-confidence and had a positive impact on health-related behaviours,[26 87 88] contrasting the evidence from most quantitative studies. Importantly, stakeholders allocating resources to SP schemes to reduce lifestyle-related non-communicable diseases should recognise that notable and sustainable changes in health outcomes

of its beneficiaries could be challenging, despite SP having the potential to benefit individuals with complex health and social care needs significantly.

CONCLUSION

In conclusion, this is the first systematic review and meta-analysis examining the effectiveness of SP in modifying the behavioural and metabolic determinants of chronic diseases among community-dwelling adults. We demonstrated that current evaluations are limited to SP exercise programmes with various underlying health conditions. We identified evidence of a significant increase, albeit small, in physical activity; however, no beneficial effects on metabolic outcomes were observed.

Our review has not established that SP is ineffective. At the same time, despite the widespread popularity and implementation of SP, determining whether SP schemes reduce the risk of chronic diseases by promoting sustainable healthy behaviours is limited by the current evidence of quantification and uncertainty. Our study contributes to the literature on the need for evaluations of both quantitative and qualitative assessments to reflect patient outcomes and experiences adequately. Hence, there is an urgent need to develop a high-quality systematic and rigorous evaluation planned from the outset by addressing the methodological shortcomings to fully understand the potential of SP.

Twitter Htet Lin Htun @danny_hlh, Achamyeleh Birhanu Teshale @AchamTeshale, Miranda S Cumpston @mirandacumpston, Joanne Ryan @DrJoRyan and Rosanne Freak-Poli @DrFreakPoli

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Contributors HLH: identification and screening of records, data extraction, risk of bias assessment, formal analysis, investigation, validation, visualisation, data interpretation, writing—original draft, writing—review & editing, guarantor. ABT: screening of records, data extraction, risk of bias assessment, validation, writing—review & editing. MSC: investigation, data interpretation, validation, writing—review & editing. LD: writing—review & editing. JR: writing—review & editing. AO: supervision, writing—review & editing. RF-P: conceptualisation, supervision, data interpretation, writing—review & editing. All authors approved the final version for submission.

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ORCID iDs
Htet Lin Htun http://orcid.org/0000-0003-2307-0610
Achamyeleh Birhanu Teshale http://orcid.org/0000-0002-7572-9993
Miranda S Cumpston http://orcid.org/0000-0001-6564-8615
Joanne Ryan http://orcid.org/0000-0002-7039-6325
Alice Owen http://orcid.org/0000-0002-4807-6069
Rosanne Freak-Poli http://orcid.org/0000-0003-4072-8699

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