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[Intervention Protocol]

Remote, face-to-face, and group-based interventions for promoting strength training in healthy community-based adults

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ABSTRACT

Objectives

This is a protocol for a Cochrane Review (intervention). The objectives are as follows:

Primary objective

To assess the benefits and harms of remote, face-to-face, and group-based interventions for promoting strength training on participation in healthy, community-based adults.

Secondary objectives

1. To assess how the method of intervention delivery (for example the delivering professional, face-to-face versus remote, group-based versus individual, supervised versus unsupervised) influences the effectiveness of the intervention.
2. To assess how the intensity of the intervention delivery (for example number of sessions per week) influences the effectiveness of the intervention.
3. To assess how the type of intervention (for example muscle strengthening activities alone versus multicomponent activities) influences the effectiveness of the intervention.
4. To assess if the effectiveness of interventions to promote strength training differs in older adults (aged 65 years and older).

BACKGROUND

Description of the condition

Physical activity has significant and wide-ranging benefits for physical and mental health and well-being across all age groups. There is strong evidence that regular physical activity is associated with reduced all-cause mortality (Ekelund 2019), and plays an important role in preventing and managing long-term conditions, such as cardiovascular disease, type II diabetes, osteoarthritis, chronic low back pain, osteoporosis, breast cancer, colon cancer, depression, and anxiety (PAGAC 2018; Pedersen 2007; Warburton 2017). Physical activity can be split into two main components; aerobic (also known as cardiovascular) activities and muscle strengthening (resistance) activities. Cohort studies have demonstrated an association between participation in muscle strengthening activities and a reduction in all-cause, cardiovascular, cancer, and chronic lower respiratory tract disease mortality (Stamatakis 2018; Zhao 2020). A cohort study in women also demonstrated that participation in strength training is associated with a reduced risk of type II diabetes and cardiovascular disease (Shiroma 2017). In all of these studies, engaging in both muscle strengthening and aerobic was associated with greater reductions in mortality, compared to either activity alone or no activity, suggesting the benefits of muscle strengthening activities are independent of, and in addition to the benefits of aerobic activity.

In addition, improved performance in measures of musculoskeletal fitness, such as gait speed, handgrip strength, maximum leg strength, and the five times sit to stand test (5XSST), are associated with reduced all-cause mortality (Cooper 2010; Volaklis 2015). A review of muscle strengthening activity reviews also found that a combination of high intensity strength training and impact activities lead to increased bone mineral density in middle-aged and older women, and high intensity strength training and balance exercises reduced risk of falls in older adults (Hillsdon 2018). A more recent meta-analysis found that strength training may improve cardiometabolic health outcomes, such as blood pressure and insulin resistance, in both healthy adults and those with cardiometabolic risk factors, but the certainty of evidence was low or very low (Ashton 2020). Another meta-analysis showed that strength training may improve the symptoms of depression and anxiety (Gordon 2017; Gordon 2018).

Muscle strength peaks in early adulthood, gradually declining in later life, and has an important role in physical function. Loss of muscle strength, especially in older adults, can contribute to functional impairments, such as loss of independence and impaired mobility (Skelton 2018). Regular participation in strength training has been shown to result in increased muscle strength and muscle size (hypertrophy) across all adult age ranges, including older adults (Grgic 2020). In older adults (including frail older adults), strength training can also improve gait speed and other measures of physical function (Jadczak 2018; Liu 2009).

Global World Health Organization (WHO) and national guidelines for physical activity are similar (Bull 2020; Davies 2019; PAGAC 2018). The WHO guidelines on physical activity and sedentary behaviour were updated in 2020, and recommend that adults, including those living with chronic conditions or disability, should aim to do at least 150 to 300 minutes of moderate intensity aerobic physical activity, or at least 75 to 150 minutes of vigorous intensity aerobic physical activity (or an equivalent combination of both) per week; and perform muscle strengthening activities at moderate or greater intensity that involve all major muscle groups on at least two days of each week (Bull 2020). The WHO guidelines recommend that older adults (aged 65 years and older) also perform multicomponent physical activity (functional balance and strength training) on three or more days per week, at moderate or greater intensity, to enhance functional capacity and prevent falls (Bull 2020).

Description of the intervention

Bodyweight exercises, free weights, resistance machines, or resistance bands can be used for strength training, as well as other activities with a strength component, such as ball games and racquet sports (Foster 2018), and activities of daily living, for example, climbing stairs, carrying shopping bags, wheeling a wheelchair, and gardening (Davies 2019). There are multiple variables within strength training than can affect muscle strength gains, including duration of training (number of weeks), volume (frequency of training, number of repetitions, number of sets), and intensity (percentage of one-repetition maximum (1RM), time muscle is under tension).

Attempts have been made to identify dose-response relationships between strength training variables and muscle strength, but uncertainty still exists regarding the optimal dose of strength training; this may vary between populations (Borde 2015; Rhea 2003). In a meta-analysis of untrained adults, strength training on three days per week, at an intensity of 60% of 1RM, for four sets, had the largest effect size on muscle strength, but beneficial effects were also seen at lower intensities and volumes, and there may be diminishing returns with higher intensities and sets (Rhea 2003). In older adults, an intensity of 70% to 79% of 1RM and two strength training sessions per week produced the largest increase in muscle strength, with little or no difference seen with different numbers of sets and repetitions (Borde 2015). It is unclear what the optimal strength training variables for health outcomes are. Reviews in cancer survivors (Strasser 2013), and in people with osteoarthritis (Turner 2020), were unable to identify optimal strength training variables. However, prospective studies showed a dose-response relationship; increased time spent weight training was associated with reduced risk of type II diabetes in both men (Grøntved 2012), and women (Grøntved 2014).

For the purposes of this review, we will define an intervention as any deliberate attempt to increase participation in strength training at an individual (participant) level. Interventions may be delivered by a range of professionals (for example healthcare professional, exercise specialist, physical activity researcher). They may be delivered remotely or face-to-face; to an individual or group.

How the intervention might work

Interventions to promote physical activity typically use behavioural change interventions: co-ordinated sets of activities designed to change specified behaviour patterns (Michie 2011). Multiple theories on behavioural change have been put forward over the last 40 years, including the Health Belief Model (Becker 1974), Social Cognitive Theory (Bandura 1986), Transtheoretical Model of behaviour change (Prochaska 1997), and the Behaviour Change Wheel (BCW (Michie 2011)). Physical activity interventions are often designed to address specific motivators and barriers to increasing

participation in physical activity. Many of the barriers and motivators to participation in muscle strengthening activities are shared with physical activity more generally, for example potential fitness and health benefits (Cavill 2018). Motivators specific to muscle strengthening activities included improved ability to complete daily activities, prevention of deterioration and disability, reduced risk or fear of falling, and building muscle. Mental health benefits, reduced feeling of isolation, and maintenance of relationships were also identified as motivators. Barriers included lack of time, lack of motivation, inability to access appropriate facilities or programmes, perceived risk of heart attack, stroke or death, and fear of looking too muscular (Cavill 2018). Depending on the muscle strengthening activity, other challenges to participation may include the requirement to understand terminology relating to the activity (sets, repetitions, etc.), self-efficacy to perform the activity, and access to equipment (Bennie 2020).

At a cellular level, the changes that underpin the body's response to muscle strengthening activities are not fully understood. The repeated mechanical loading of skeletal muscle (i.e. muscle strengthening activities) is thought to increase muscle protein synthesis more than muscle protein breakdown, which in turn leads to muscle growth and increased strength (Damas 2015). Structural changes that occur in skeletal muscle include the growth of muscle fascicles and myofibers (Jorgenson 2020). Other benefits of muscle strengthening activities include stimulation of muscle mitochondrial biogenesis and respiratory function, but to a lesser degree than in aerobic exercise (Groennebaek 2017). Skeletal muscle is an active endocrine organ, and secretes multiple signalling molecules, including myokines, which exert either paracrine (acting locally) or endocrine (acting via hormones throughout the body) effects (Pedersen 2007). Physical activity alters the secretory profile of skeletal muscle (Florin 2020), resulting in a reduction in chronic low-grade systemic inflammation, through changes to adipose tissue, and lipid and glucose metabolism (Leal 2018). The reduction in chronic low-grade systemic inflammation is thought to underpin the beneficial effect of physical activity in the prevention and management of many long-term conditions (Burini 2020).

Why it is important to do this review

Given the well documented benefits of muscle strengthening activities on musculoskeletal fitness, physical function, and health, and current low levels of participation, there is a potential benefit for the adult population if they increase participation in muscle strengthening activities. However, there is currently a lack of evidence on how this can be effectively encouraged. Several Cochrane Reviews examined interventions to promote physical activity at both the individual and community level, but these looked solely at the aerobic component of physical activity (Baker 2015; Foster 2013; Richards 2013). Other Cochrane Reviews looked at interventions to promote both aerobic and strength activities, but in specific populations, such as people with chronic obstructive pulmonary disease (COPD), cystic fibrosis, or those living with or beyond cancer (Burge 2020; Cox 2013; Turner 2018). We are not aware of any published systematic reviews investigating the effectiveness of interventions to promote participation in strength training in general adult populations. Therefore, the findings of this review will address an important evidence gap, and inform evidence-based decision making by physical activity policy makers and practitioners.

The timing of this review is also important, given the negative impact that the COVID-19 pandemic has had on physical activity levels. Globally, access to opportunities for physical activity was reduced, due to requirements to stay at home, and government-mandated closures of leisure facilities, workplaces, and shops. In April 2020, an international survey of physical activity behaviours during the first wave of COVID-19 stay-at-home restrictions found a reduction of nearly 35% in the minutes per week that adults spent walking or engaging in physical activity (Ammar 2020). The Active Lives Adults Survey in England found more than three million fewer adults (7.1% total adult population) were classed as active (at least 150 minutes per week of moderate intensity activity) between mid-March and mid-May 2020, compared to the same period in the previous year. The survey also noted a small increase in the number of adults participating in at least two body weight exercise sessions over the last 28 days; however, this was offset by larger scale reductions in the number of adults who participated in gym and weights sessions (Sport England 2020).

Adults may be willing to alter their physical activity in light of COVID-19 restrictions. In Ireland, one in five adults have taken up a new physical activity, and participation in home exercises, running, hill walking, cycling, weights, and yoga increased during stay-at-home restrictions. However, the proportion of adults taking up a new physical activity varied with age; only 6% of adults over the age of 65 years took up a new activity, compared to 53% of adults under the age of 25 years (Sport Ireland 2020).

Overall, concerns remain that the COVID-19 pandemic will lead to a fall in physical activity levels, which may in turn lead to physical deconditioning, particularly in older adults. Muscle loss can occur rapidly in response to reduced activity. In healthy older adult volunteers, reducing daily step count by approximately three quarters, to 1413 ± 110 steps per day for 14 days, has been show to lead to nearly 4% loss in leg fat-free mass (Breen 2013). Hence, understanding which interventions are effective for promoting participation in strength training is even more crucial.

Previous Cochrane Reviews on interventions to promote physical activity found there were multiple types of interventions used, including theory-based counselling, brief advice, educational materials, exercise prescriptions, and free or subsidised access to leisure facilities (Foster 2013; Richards 2013). Our review will assess the effectiveness of interventions that aim to promote participation in strength training in adults.

This review will be conducted according to the guidelines recommended by the Cochrane Musculoskeletal Group Editorial Board (Ghogomu 2014).

OBJECTIVES

Primary objective

To assess the benefits and harms of remote, face-to-face, and group-based interventions for promoting strength training on participation in healthy, community-based adults.

Secondary objectives

1. To assess how the method of intervention delivery (for example the delivering professional, face-to-face versus remote, group-based versus individual, supervised versus unsupervised) influences the effectiveness of the intervention.

2. To assess how the intensity of the intervention delivery (for example number of sessions per week) influences the effectiveness of the intervention.
3. To assess how the type of intervention (for example muscle strengthening activities alone versus multicomponent activities) influences the effectiveness of the intervention.
4. To assess if the effectiveness of interventions to promote strength training differs in older adults (aged 65 years and older).

METHODS

Criteria for considering studies for this review

Types of studies

We will include randomised controlled trials (RCTs), controlled clinical trials using a quasi-randomised method of allocation, such as by alternation or date of birth, and cluster-randomised trials. We will exclude cross-over trials, due to the risk of serious carry-over effect.

Types of participants

We will include studies involving adults (age 16 and older) living in the community who are free from pre-existing medical conditions that may limit participation in strength training, for example, unstable angina, uncontrolled hypertension or arrhythmia, acute heart failure, or critical aortic stenosis.

We will exclude interventions for trained athletes or sports students.

Types of interventions

We will include trials that compare remote, face-to-face, and group-based interventions with either our main comparison: placebo, or our secondary comparison: no or minimal intervention.

Minimal interventions may include attention control (for example general health education), brief advice, or a waiting list control group. Interventions may be delivered using a supervised or unsupervised approach, or a mixture of both. Remote interventions may be delivered by newer technologies (for example web based, smartphones and applications (apps), wearable technology) or more traditional methods, such as telephone or surface mail. Interventions can involve a one-off interaction, or ongoing interactions between the implementer and the participant.

Remote, individually delivered interventions may include written, telephone, internet-based, mobile apps, or telehealth advice or support, and physical activity programmes that are self-directed or supervised remotely.

Face-to-face, individually delivered interventions may include advice or support, and directly supervised physical activity programmes.

Group-based interventions may include group advice or support, or group physical activity programmes, delivered face-to-face or remotely.

We will exclude interventions that are targeted to participants with a specific health condition, as the results may not be generalisable to wider populations, and some conditions fall under the scope of separate systematic review or Cochrane Reviews, for example chronic obstructive pulmonary disease and cancer (Burge 2020; Turner 2018). We will exclude studies in which the main aim of the intervention is not the promotion of participation in strength training, for example falls prevention programmes, or interventions designed to improve bone density. We will also exclude studies that look at the effect of different doses of training on strength outcomes if there is no placebo, minimal, or no intervention control arm.

We will exclude mass media and multiple risk factor interventions.

To allow us to assess behavioural changes over time, the included studies must have a minimum of three months follow-up from the start of the intervention to the final results.

Types of outcome measures

Major outcomes

1. Participation in strength training, measured by the participant's self-report, or recorded as part of the intervention. We will prioritise participation in strength training outcomes in the following order: frequency (number of sessions per week), duration (total minutes per week), and adherence to intervention (e.g. attendance at exercise classes).
2. Number of participants who met the WHO strength training recommendations (two or more sessions per week)
3. Muscle strength. We will prioritise muscle strength outcomes in the following order: lower limb strength; handgrip strength; any other measures of upper limb strength.
4. Physical function. We will prioritise physical function outcomes in the following order: multicomponent measures of physical function (such as Short Physical Performance Battery (SPPB), Short-Form 36 (SF-36) physical function domain); walk tests (such as 10-metre walk test and six-minute walk distance (6MWD)); single component measures of physical function (such as timed up and go test, five times sit to stand test (5XSST)).
5. Number of participants who experienced any adverse event
6. Number of participants who experienced a serious adverse event, such as cardiovascular event, hospital admission, or death, during the intervention period
7. Number of participant who withdrew due to adverse events

Timing of outcome assessments:

Where feasible, we will measure outcomes at the following time points.

- End of intervention
- Intermediate follow-up (three months to six months after end of intervention, primary time point)
- Long-term follow-up (longer than six months after end of intervention)

If an included study reports multiple measures for the same outcome within a time point (three to six months, or longer than six months), we will include data from the latest point.

When possible, we will base the minimal important difference (MID) for outcomes on established values from the literature, for example 6 kg for hand grip strength (Bohannon 2019), 0.5 points for the SPPB, 0.05 m/s for gait speed, and 50 m for the 6MWD, represent small meaningful changes (MID) in older adults (Perera 2006). When

MIDs are unavailable, we will report this in the interpretation of the outcomes.

Search methods for identification of studies

Electronic searches

We will search these databases, and impose no restriction on language of publication.

- the Cochrane Central Register of Controlled Trials (CENTRAL; current issue) in the Cochrane Library
- MEDLINE OvidSP (1946 to present)
- Embase OvidSP (1974 to present)
- PsycINFO OvidSP (1806 to present)
- ProQuest Dissertations & Theses Global (1637 to present)
- PEDro (Physiotherapy Evidence Database; pedro.org.au/; present)
- US National Institutes of Health Ongoing Trials Register ClinicalTrials.gov (www.clinicaltrials.gov; present)
- World Health Organization International Clinical Trials Registry Platform (ICTRP; apps.who.int/trialsearch; present)

We will contact authors and research groups for information about unpublished or ongoing studies.

See Appendix 1 for the MEDLINE search strategy.

Searching other resources

We will check reference lists of all primary studies and review articles for additional references. We will search for errata or retractions from included studies published in full text on PubMed (www.ncbi.nlm.nih.gov/pubmed), and report the search date in the review.

Data collection and analysis

Selection of studies

Two of three review authors (RG, EM, CF) will independently screen titles and abstracts to select all the potentially relevant studies we identify as a result of the search, and code them as retrieve (eligible, potentially eligible, or unclear) or do not retrieve. We will retrieve the full-text study reports and publications, and two of the same three review authors will independently screen the full text to identify studies for inclusion, and identify and record reasons for exclusion of the ineligible studies. We will resolve any disagreement through discussion; if required, we will consult a third person (MH). We will identify and exclude duplicates, and collate multiple reports of the same study, so that each study, rather than each report, is the unit of interest in the review. We will record the selection process in sufficient detail to compete the characteristics of excluded studies table, and a PRISMA flow diagram (Moher 2009).

Data extraction and management

We will use a data collection form for the study characteristics and outcome data, which has been piloted on at least one included study. Two of three review authors (RG, EM, CF) will independently extract the following study characteristics from included studies.

1. Methods: study design, total duration of the study, number of study centres and location, study setting, withdrawals, and date of study
2. Participants: N, mean age, age range, sex, place of residence, ethnicity, occupation, religion, education, socioeconomic status, social capital, comorbidities, inclusion criteria, and exclusion criteria
3. Interventions: intervention characteristics, based on the Consensus on Exercise Reporting Template (CERT) checklist (Slade 2016), comparisons, and concomitant interventions
4. Outcomes: major outcomes specified and collected, and time points reported
5. Characteristics of the design of the trial as outlined in the Assessment of risk of bias in included studies section
6. Notes: funding for the trial, and notable declarations of interest from the trial authors

Two of three review authors (RG, EM, CF) will independently extract outcome data from included studies. We will extract the number of events and number of participants per treatment group for dichotomous outcomes; and means, standard deviations, and number of participants per treatment group for continuous outcomes. We will note in the Characteristics of included studies table if outcome data were not reported in a usable way, and when data were transformed or estimated from a graph. We will resolve disagreements by consensus or by involving a third person (MH). One review author (RG) will transfer data into the Review Manager Web file (RevMan Web 2020). We will double-check that data are entered correctly by comparing the data presented in our systematic review with the study reports.

Two review authors will independently extract data from graphs or figures, using WebPlotDigitizer (WebPlotDigitizer 2020).

If we encounter multiple measures for the same outcome, we will preferentially extract final values, rather than change from baseline values; adjusted, rather than unadjusted values; and data analysed with an intention-to-treat approach, rather than per-protocol or as-treated.

Assessment of risk of bias in included studies

Two of three review authors (RG, EM, CF) will independently assess risk of bias for each study, using the RoB 1 tool, and criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2017). We will resolve any disagreements by discussion or by involving another author (MH). We will assess the risk of bias according to the following domains.

1. Random sequence generation (selection bias)
2. Allocation concealment (selection bias)
3. Blinding of participants and personnel (performance bias)
4. Blinding of outcome assessment (detection bias)
5. Incomplete outcome data (attrition bias)
6. Selective outcome reporting (reporting bias)
7. Other bias: unequal use of co-interventions, inappropriate administration of an intervention (or co-intervention)

We will classify each potential source of bias as high, low, or unclear risk, and provide a quote from the study report together with a

justification for our judgment in the risk of bias table. We will summarise the risk of bias judgements across studies for each of the domains listed. We will consider blinding separately for different key outcomes where necessary (e.g. risk of bias for participation in muscle strengthening activity identified from attendance records may be different than for participant self-reported activity). As well, we will consider the impact of missing data by key outcomes.

Where information on risk of bias relates to unpublished data or correspondence with a trialist, we will note this in the risk of bias table.

When considering treatment effects, we will take into account the risk of bias for the studies that contribute to that outcome.

We will present the figures generated by the RoB 1 tool to provide summary assessments of the risk of bias.

We will conduct the review according to the published protocol, and report any deviations in the Differences between protocol and review section of the review.

Measures of treatment effect

We will analyse dichotomous data as risk ratios (RR), or Peto odds ratios (OR) when the outcome is a rare event, and use 95% confidence intervals (CIs). We will analyse continuous data as mean difference (MD) or standardised mean difference (SMD), depending on whether the same scale is used to measure an outcome, and 95% CIs.

When difference scales are used to measure the same conceptual outcome (e.g. strength), we will calculate SMDs, with the corresponding 95% CIs. We will back-translate SMDs to a typical scale (e.g. Newton metres for strength) by multiplying the SMD by a typical among-person standard deviation (e.g. the standard deviation of the control group at baseline from the most representative trial (Schünemann 2021b)).

In the Measures of treatment effect section, and the Comments column of the summary of findings table, we will provide the absolute per cent difference, the relative per cent change from baseline, and the number needed to treat for an additional beneficial outcome (NNTB), or the number needed to treat for an additional harmful outcome (NNTH). We will only provide the NNTB or NNTH when the outcome shows a clinically significant difference.

For dichotomous outcomes, we will calculate the NNTB or NNTH from the control group event rate and the relative risk, using the Visual Rx NNT calculator (Cates 2016). We will calculate the NNTB or NNTH for continuous measures using the Wells calculator, which is available at the Cochrane Musculoskeletal Group's editorial office (musculoskeletal.cochrane.org).

For continuous outcomes, we will calculate the absolute benefit as the improvement in the intervention group minus the improvement in the control group, in the original units, expressed as a percentage.

We will calculate the relative per cent change for dichotomous data as the risk ratio -1, expressed as a percentage. For continuous outcomes, we will calculate the relative difference in the change from baseline as the absolute benefit divided by the baseline mean of the control group, expressed as a percentage.

Unit of analysis issues

When multiple trial arms are reported in a single trial, we will include only the relevant arms. If two comparisons (e.g. intervention A versus no intervention and intervention B versus no intervention) are combined in the same meta-analysis, we will halve the control group to avoid double-counting.

If data are reported as both adjusted for baseline values (such as physical activity) and non-adjusted, we will preferentially extract adjusted values.

If possible, we will re-analyse studies that randomise or allocate by clusters but do not account for clustering during analysis. When participation in strength training is stated as a percentage of the population meeting a specified attainment level (two or more sessions per week), we will consider that the analysis is at the same level as allocation for each cluster. Alternatively, if appropriate, we will use statistical methods that allow analysis at the level of the individual while accounting for clustering in the data. If successful, effect estimates and their standard errors from correct analyses of cluster-randomised trials may be meta-analysed using the generic inverse-variance method in RevMan Web (Higgins 2021c).

Dealing with missing data

We will try to contact investigators or study sponsors to verify key study characteristics and obtain missing numerical outcome data, as needed (e.g. when a study is identified as abstract only, or when data are not available for all participants). When this is not possible, and the missing data are thought to introduce serious bias, we will explore the impact of including such studies in the overall assessment of results with a sensitivity analysis. We will clearly describe any assumptions and imputations to handle missing data, and explore the effect of imputation with a sensitivity analyses.

For dichotomous outcomes assessing benefit, we will calculate the rate using the number of participants randomised as the denominator.

For dichotomous outcomes assessing adverse events (e.g. number of withdrawals due to adverse events), we will calculate the rate using the number of participants randomised to, and receiving the intervention, as the denominator.

For continuous outcomes (e.g. mean change in pain score), we will calculate the MD or SMD, based on the number of participants analysed at that time point. If the number of participants analysed is not presented for each time point, we will use the number of participants randomised to each group at baseline.

When possible, we will compute missing standard deviations from other statistics, such as standard errors, CIs, or P values, according to the methods recommended in the *Handbook* (Higgins 2021a). If we cannot calculate standard deviations, we will impute them, e.g. from other studies in the meta-analysis (Higgins 2021a).

Assessment of heterogeneity

Refering to the data extraction tables, we will assess the clinical diversity of the participants, interventions, outcomes, and study characteristics from the included studies to determine whether a meta-analysis is appropriate. We will assess statistical heterogeneity by visually inspecting the forest plots for obvious

differences in results between the studies, and using the I^2 and Chi^2 statistical tests.

As recommended in the *Handbook*, we will interpret the I^2 values as follows, bearing in mind that the importance of I^2 depends on the: (i) magnitude and direction of effects, and (ii) strength of evidence for heterogeneity:

- 0% to 40% might not be important;
- 30% to 60% may represent moderate heterogeneity;
- 50% to 90% may represent substantial heterogeneity; and
- 75% to 100% may represent considerable heterogeneity.

When a Chi^2 test has a $P \leq 0.10$, we will interpret it as an indication of statistical heterogeneity.

If we identify substantial heterogeneity (i.e. $I^2 \geq 50\%$), we will report it and investigate possible causes with subgroup analyses (Deeks 2021).

Assessment of reporting biases

We will create and examine a funnel plot to explore possible small study biases. In interpreting funnel plots, we will examine the different possible reasons for funnel plot asymmetry, and relate this to the results of the review. If we are able to pool more than 10 trials, we will undertake formal statistical tests to investigate funnel plot asymmetry, following the recommendations in the *Handbook* (Page 2021).

To assess outcome reporting bias, we will check trial protocols against published reports. For studies published after 1 July 2005, we will screen the ICTRP for the a priori trial protocol.

Data synthesis

We will undertake meta-analyses only when this is meaningful, i.e. if the participants, interventions and common comparators, outcome measures, and timing of outcome measurement are similar enough for pooling to make sense, using RevMan Web software (RevMan Web 2020).

We will use a random-effects model and perform a sensitivity analysis with the fixed-effect model.

Main planned comparisons

Our main comparisons will be:

- Remote (individual) interventions versus placebo;
- Face-to-face (individual) interventions versus placebo;
- Group-based (remote) interventions versus placebo;
- Group-based (face-to-face) interventions versus placebo.

Our other comparators include no or minimal intervention.

Subgroup analysis and investigation of heterogeneity

We plan to carry out the following subgroup analyses.

1. Delivering professional: healthcare versus non-healthcare professional. Previous Cochrane Reviews on interventions to promote physical activity have investigated the effect of the type of professional delivering the intervention (Richards 2013).
2. Intensity of the intervention delivery (for example the number of sessions per week)
3. Type of intervention: we will consider the influence of the type of intervention (muscle strengthening alone versus muscle strengthening combined with other type(s) of exercise, such as aerobic exercise or balance training)
4. Age of participant: we will consider the influence of age on outcomes (< 65 years versus 65 years and older)

We will undertake subgroup analyses for the following outcomes, chosen as they are the most relevant to our primary objective, at the primary time point (three months to six months after end of intervention).

1. Participation in strength training
2. Number of participants who experienced any adverse event

Sensitivity analysis

We plan to carry out the following sensitivity analyses to investigate the robustness of the intervention effect on the outcomes: participation in strength training, and number of participants who experienced any adverse event, at the primary time point (three months to six months after end of intervention).

1. Impact of risk of bias — we will exclude studies with high or unclear risk of selection, performance, or detection bias.
2. Effect of imputed data — we will exclude studies with imputed data.

Summary of findings and assessment of the certainty of the evidence

We will create summary of findings (SoF) tables for our main comparisons: remote (individual) interventions versus placebo; face-to-face (individual) interventions versus placebo; group-based (remote) interventions versus placebo; group-based (face-to-face) interventions versus placebo, at the primary time point of three months to six months after end of intervention with the following outcomes:

1. Participation in strength training
2. Number of participants who met the WHO strength training recommendations
3. Muscle strength
4. Physical function
5. Number of participants who experienced any adverse event.
6. Number of participants who experienced serious adverse events
7. Number of participant who withdrew due to adverse events.

Two of three review authors (RG. EM, CF) will independently assess the quality of the evidence. We will use GRADEpro GDT software to prepare the SoF tables, and assess the quality of the body of evidence for each outcome against the five GRADE considerations (study limitations, inconsistency of effect, imprecision, indirectness, and publication bias (GRADEpro GDT)). We will classify and report the quality of evidence as high, moderate, low, or very low. We will downgrade the quality by one level if one of the GRADE considerations is present and we rate it as serious, or by two levels if we rate it as very serious, following the methods and recommendations described in the *Handbook* (Higgins 2021b; Schünemann 2021a; Schünemann 2021b). We will

justify all decisions to downgrade the quality of the evidence using footnotes, and provide comments to aid the reader's understanding of the review where necessary. We will provide the NNTB or the NNTH, and the absolute and relative per cent change in the Comments column of the SoF table, as described in the Measures of treatment effect section.

Interpreting results and reaching conclusions

We will follow the guidelines in the *Handbook* for interpreting results, and ensure we distinguish between a lack of evidence of effect and a lack of effect (Schünemann 2021b). We will base our conclusions only on findings from the quantitative or narrative synthesis of included studies for this review. We will avoid making recommendations for practice; our implications for research will suggest priorities for future research and outline the remaining uncertainties in the area.

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APPENDICES

Appendix 1. MEDLINE search strategy

1	Muscle Strength/ and (exp Exercise/ or exp Exercise/ or Physical Fitness/)
2	exp physical conditioning, human/
3	Weight Lifting/
4	((muscle or resistance or strength* or plyometric or circuit or endurance) adj3 (exercis* or train* or physical* activ*)).tw.
5	((muscle adj2 (mass or strength* or power or endurance or function)) and (exercis* or train* or physical activ*)).tw.
6	((weight* adj2 (lift* or train*)) or weightlift*).tw.
7	plyometrics.tw.
8	(free weight? or exercis* band? or resistance band?).tw.

(Continued)

9	(elastic band? and (exercis* or fit* or physical* activ*)).tw.
10	((multicomponent or multi-component or multimodal or multi-modal) adj2 (exercis* or train* or physical* activ*)).tw.
11	(functional adj2 (exercis* or train* or physical* activ*)).tw.
12	1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11
13	health education/ or consumer health information/ or exp health promotion/ or Geriatric Assessment/
14	patient education as topic/ or pamphlets/ or exp Video Recording/
15	((exercis* or physical activ*) adj3 (educat* or promot* or intervention?)).tw.
16	((educat* or promot*) adj3 (intervention? or program*)).tw.
17	(dvd? or video* or leaflet? or pamphlet? or ((written or educational or promotional) adj2 (information or material?))).tw.
18	behavior therapy/ or cognitive behavioral therapy/ or psychotherapy, brief/
19	counseling/ or directive counseling/ or motivational interviewing/ or distance counseling/
20	(((behav* or cognitiv*) adj2 (therap* or intervention? or program*)) or cbt).tw.
21	(counsel* or coach*).ti. or ((counsel* or coach* or mindfulness) adj2 (intervention? or program*)).tw.
22	motivat*.ti. or (motivation* adj2 (support* or interview* or intervention? or program*)).tw.
23	(brief adj2 intervention?).tw.
24	((selfdirect* or self direct* or selfmotivat* or self-motivat* or selfcare or self care) adj2 (intervention? or program*)).tw.
25	(home* adj2 (intervention? or program*)).tw.
26	((workplace based or work place based or community based or group based) adj2 (intervention? or program*)).tw.
27	((selfdirect* or self direct* or selfmotivat* or self-motivat* or selfcare or self care) adj2 (exercis* or physical* activ*)).tw.
28	(home adj2 (exercis* or physical* activ*)).tw.
29	((workplace based or work place based or community based or group based) adj2 (exercis* or physical* activ*)).tw.
30	((remote or online or virtual or digital or electronic or internet or web* or computer based) adj2 (therap* or counsel* or intervention? or program*)).tw.
31	Remote Consultation/ or Telemedicine/
32	(telemed* or tele-med* or telehealth* or tele-health* or ehealth or e-health or mhealth or m-health).tw.

(Continued)

33	Prescriptions/ and exp Exercise/
34	((social or exercis* or physical activ* or gym* or leisure center? or leisure centre? or leisure facilit* or sport? club* or sport? facilit*) adj2 (prescri* or referral?)).tw.
35	(patient* adj2 navigat*).tw.
36	Motivation/ or Self Care/
37	(motivat* or incentiv* or subsid* or voucher? or token?).ti.
38	((cash or financial or economic* or monetary) adj2 (incentive* or subsid* or voucher* or token* or support or assist*)).tw.
39	((gym* or leisure center? or leisure centre? or leisure facilit* or sport? club* or sport? facilit*) adj2 (incentiv* or subsid* or voucher* or token* or membership?)).tw.
40	((free or subsid*) adj2 membership?).tw.
41	13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26 or 27 or 28 or 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37 or 38 or 39 or 40
42	randomized controlled trial.pt.
43	controlled clinical trial.pt.
44	randomized.ab.
45	placebo.ab.
46	drug therapy.fs.
47	randomly.ab.
48	trial.ab.
49	groups.ab.
50	42 or 43 or 44 or 45 or 46 or 47 or 48 or 49
51	exp animals/ not humans.sh.
52	50 not 51
53	12 and 41 and 52
54	exp child/ not (exp child/ and exp Adult/)
55	53 not 54

CONTRIBUTIONS OF AUTHORS

This protocol was initially drafted by Rebecca Gould and Charlie Foster. Nia Roberts developed the search strategy. All authors contributed to the final version of the protocol.

DECLARATIONS OF INTEREST

RG, NR, EM, MH, and CF have no conflicts of interest to declare.

SOURCES OF SUPPORT

Internal sources

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External sources

- No sources of support provided