Cochrane Australia

Systematic review of evidence on the clinical effectiveness of Alexander Technique

Technical report prepared by Cochrane Australia

04 November 2024

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Contents

Scope of the technical report

This Technical Report includes a complete description of the methods for the review (Appendices A, B and G), results of the search and prioritisation process (Appendix A), citations for studies included in the evidence synthesis (Appendix D), risk of bias assessments for studies contributing to meta-analyses (Appendix F) and abbreviations used in the report (Appendix I).

It also includes an overview of Appendices C and E which are listed below but presented in separate files.

Appendices contained in this file are in light grey rows. Those in separate files are in blue rows.

Appendix A. Study eligibility criteria, identification and selection
Appendix B. Data collection, analysis and interpretation of findings
Appendix C. Lists of excluded studies, public submissions, studies awaiting classification, ongoing studies (1 file)
Appendix D. Citations for studies included in the evidence synthesis
Appendix E. Characteristics of studies included in the evidence synthesis (2 files)
Appendix F. Risk of bias assessments for studies contributing to meta-analyses (1 file)
Appendix G. Differences between the protocol and the review and methods not used
Appendix H. Response to methodological review
Appendix I. Abbreviations and list of measures

Appendix A. Study eligibility criteria, identification and selection

Overview of Appendix A

Appendix A is comprised of Appendices A1-A7 (below). These Appendices report the methods (grey rows) and results (blue rows) from the first four stages of the review (Figure A, 1-4). These stages encompass the initial specification of questions to be addressed in the synthesis and criteria for including studies in the review, the specification and implementation of search methods, and the selection of studies. From this set of studies, we compiled information about the populations and outcomes addressed in studies eligible for the review. This information was reviewed by the NHMRC, NTWC and NTREAP in order to confirm populations and outcomes for inclusion in the evidence synthesis.

Appendix A1. Review questions and criteria for considering studies for this review
Appendix A2. Search methods for identification of studies
Appendix A3. Methods for selecting studies
Appendix A4. Results of the search
Appendix A5. Prioritisation process: methods used to refine the questions addressed in the synthesis
Appendix A6. Final framework: synthesis questions and criteria for including studies in each synthesis
Appendix A7. Summary of inclusion decisions based on the final framework

Appendices A1-A3 and A5 report the pre-specified methods from the protocol endorsed by NTWC, prospectively registered on the International prospective register of systematic reviews (PROSPERO ID CRD42023467144). Appendix A6 reports the framework that resulted from the prioritisation process shown in Figure A and described in Appendix A5. The framework was finalised prior to commencing data extraction (Figure A, panel 5). It defines the scope of the evidence synthesis and specifies the synthesis questions and associated PICO (population, intervention, comparator, outcome) criteria for including studies in each synthesis.

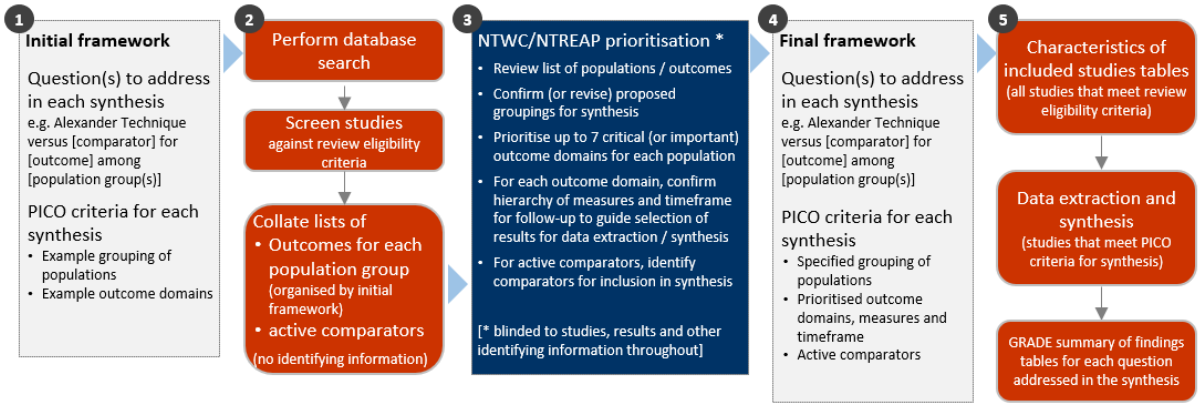

Fig A | Staged approach for developing the questions and analytic framework for this review. Active comparators were listed but did not contribute to the synthesis because the criteria for synthesis were not met (at least two low risk of bias studies with same comparator and population).

Appendix A1. Review questions and criteria for considering studies

The overall objective of this systematic review is to examine the evidence for the clinical effectiveness of the Alexander technique in preventing and/or treating injury, disease, medical conditions or preclinical conditions [1]. The questions for the review follow (framed as primary and secondary objectives). An initial analytic framework for the review was presented in the protocol to illustrate the breadth of questions and a possible structure for the synthesis, with indicative populations and outcome domains (Figure A1.1). The framework was refined through the prioritisation

process (described in Appendix A5) leading to the final framework and criteria for including studies in the synthesis (Appendix A6). Outcomes listed in the objectives were agreed through the prioritisation process.

Primary objective was to answer the following question

1. What is the effect of the Alexander technique compared to an inactive control (no intervention, sham, placebo, wait list control, or a co-intervention that was offered to both groups, or continuation of usual care) on outcomes for each underlying condition, pre-condition, injury or risk factor?

Secondary objectives related to the following questions

2. What is the effect of *the Alexander Technique* compared to evidence-based treatments (active comparators) on outcomes for each underlying condition, pre-condition, injury or risk factor?
3. What evidence exists examining the effects of the *Alexander technique* compared to other active comparators? (for inclusion in evidence inventory only, not the synthesis)

The criteria set out in the protocol were not met for synthesis of studies examining the effects of the Alexander technique compared to an evidence-based active comparator (at least two low risk of bias studies with same comparator and population). Hence, we do not report on the effects of the Alexander technique compared to any active comparator.

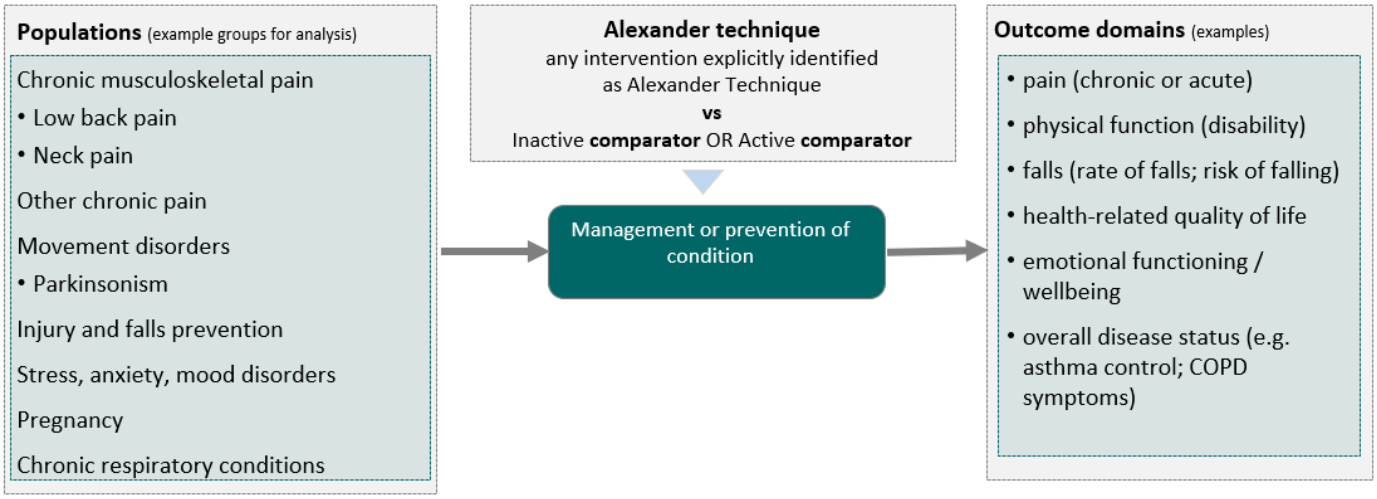

Fig A1.1 | Initial analytic framework for the review showing example population groups and outcome domains for the Evidence Synthesis. The framework was informed by research on the outcomes (and underlying conditions) for which the Alexander technique is commonly sought or prescribed, the wider literature on the Alexander technique, and consideration of frameworks for classifying disease and outcomes [2, 3].

A1.1 Criteria for considering studies for this review

A1.1.1 Types of studies

We included randomised controlled trials (RCTs) (including individually and cluster randomised, and cross-over trials).

Controlled trials in which the allocation sequence did not include a truly random element, was predictable, or was not adequately concealed from investigators were eligible as long as there was an attempt to have some kind of 'randomisation' to groups. Examples included studies that used methods for sequence generation based on alternation, dates (of birth or attendance at a clinic) and patient record numbers [4].

Non-randomised studies of interventions (NRSIs) with specific design features that are suitable for estimating a causal effect were eligible for inclusion in the review, in line with current Cochrane guidance. While study design labels were used as an aid to communicating about eligible designs and for use in the review, eligibility decisions were based on assessment of the specific design features of each study rather than the label used by the study authors (see checklist Appendix 2 in protocol published on PROSPERO CRD) [5, 6].

Eligible non-randomised study designs were those in which the following features are present.

- The intervention may be allocated to individuals or clusters. We anticipated that the Alexander technique (or the control) would be allocated to individuals in most studies, although clustering was likely in these studies given the way in which the Alexander technique lessons are delivered (i.e. the same teacher may deliver the intervention to multiple participants) [7].
- Treatment groups may be formed by some action of the researchers or in the course of usual treatment decisions (including healthcare decision makers, practitioners or participants/patients/peoples' choices).
- Studies must include a contemporaneous control.
- There must be an attempt to control for confounding (either by using methods that control in principle for confounding or that control for observed covariates)
- The design must be suitable for estimating a causal effect.

We excluded:

- Studies for which available reports had not been peer reviewed (grey literature, including theses).

Date and language restrictions.

There were no restrictions on publication date.

Potentially eligible studies published in languages other than English were not eligible for synthesis. In accordance with the protocol, these studies were to be included in the list of studies 'Awaiting classification' and coded according to whether they were likely to be eligible or whether eligibility could not be determined.

A1.1.2 Types of participants

Studies involving participants with any disease, medical condition, injury, or preclinical condition were eligible for the review. This included healthy participants with clearly-identified risk factors (e.g. biomedical, health behaviours, or other). There were no restrictions on age or other demographic factors.

For trials in which the Alexander Technique was used for primary or secondary prevention, participants must have had a clearly-identified factor that put them at heightened risk of the condition or injury that the intervention is intended to prevent compared to the population at large. Where possible, decisions about whether a population was at risk was informed by evidence from a systematic review of risk factors.

We operationalised the criteria for risk as follows:

- The risk factor(s) for the condition that the Alexander Technique was used to prevent was part of the eligibility criteria for the trial or reported in the baseline data (e.g. older age in a trial aimed at preventing falls; work that involves demanding posture or repetitive movement in a trial aiming to prevent workplace-related musculoskeletal conditions), and
- There was a direct link between the risk factor and the trial outcomes (i.e. an outcome that demonstrates progression to a diagnosable condition or pre-condition; musculoskeletal pain or injury in a trial that aims to prevent injury)

Criteria for screening such studies were refined by asking the NTWC to adjudicate on examples. Study PICO, aims and potential risk factors reported by the trialists were provided, without results or information that would identify the study.

We expected that studies would include participants within broad population groups, such as those shown in **Figure 2**. These were indicative groups, included to illustrate the breadth of populations eligible for the review and possible groupings for synthesis. Decisions about which groups to include in the final analytic framework were made through the prioritisation process (**Figure 1**).

Exclusions. Healthy populations seeking health improvement. This included healthy participants using the Alexander Technique to improve performance skills and enhance general well-being. For a sample of borderline decisions, the

NTWC was given example study PICO, aims and any information about potential risk factors reported by the trialists (without results or information that identifies the study) and asked to adjudicate.

Studies that included both healthy participants and participants eligible for the review, were to be included if separate data were available or a majority of participants met the review eligibility criteria as per guidance in the Cochrane handbook [8]. No such studies were identified.

While studies involving any population were to be included in the review (except for the specific exclusions above), if the number of eligible studies for synthesis was unmanageable, the synthesis could be limited to populations (conditions) most relevant to the use of the Alexander Technique in Australia. Population prioritisation was not needed for this review.

A1.1.3 Types of interventions

For the purpose of this review, the Alexander Technique was defined as a method that aims to:

- "… retrain habitual patterns of movement, [and] improve postural support and coordination … by consciously altering automatic responses and tonic muscular activity,
- re-educate basic muscular co-ordination patterns underlying all activity,
- reduce excessive and maladaptive tension …,
- and improve functional movement patterns in work and everyday life." [excerpt from [9]]

Because of the potential challenge of distinguishing components of the Alexander Technique from related modalities, and the likelihood of identifying studies in which the defining techniques and principles of the Alexander Technique are incompletely reported, studies were included if the therapy was described as the Alexander Technique.

Except for the specific exclusions below, Alexander Technique interventions were eligible irrespective of whether the study examined the effects of undertaking a series of lessons or the routine use of the Technique, mode of delivery (individual or group; face-to-face or virtual), whether the intervention was guided by a teacher or self-directed (the latter occurring when trained individuals used the Technique in daily life), the training or qualifications of the teacher or practitioner, the setting in which the Alexander Technique was taught or used, and the dose and duration of treatment. More details about each of these intervention features is considered in Section 3.3.2 Data extraction.

Excluded therapies: none

Comparisons

1. The Alexander Technique *versus* any inactive comparator (no intervention, sham, placebo, wait list control, or a co-intervention that was offered to both groups, or continuation of usual care).

2. The Alexander Technique *versus* evidence-based gold standard treatment(s) (see below for selection method)

3. The Alexander Technique *versus* other active comparators (for inclusion in evidence inventory only, not the synthesis – See below)

Any co-intervention was eligible (i.e. pharmacological or non-pharmacological). Usual care comparators were eligible if there was an explicit statement that indicated that participants could continue to access their routine care or therapy (including self-care). Where a comparator labelled as 'usual care' involved a defined intervention (i.e. specific treatments and processes selected by the researchers), this was deemed to be either an active intervention (if restricted to the comparator group) or a co-intervention (if able to be accessed by both groups, e.g. continuation of a specific medication).

Comparisons 1 and 2 were to be addressed in separate syntheses (meta-analyses). Where a study included multiple arms, with at least one eligible comparator (e.g. a placebo control arm), we included the eligible comparison(s).

For comparison 2, active comparators were listed but did not contribute to the synthesis because the criteria for synthesis were not met (at least two low risk of bias studies with the same comparator, population and outcome).

Characteristics of studies involving active comparators are briefly described in the evidence inventory of available evidence (Appendix E3).

Exclusions. In line with the main review objective, which was to examine the effects of the Alexander Technique rather than the comparative effects of different implementations of the Alexander Technique, head-to-head comparisons of the Alexander Technique were ineligible:

- a different dose (frequency, duration, schedule or combination thereof) of the same Alexander Technique methodology (e.g. different numbers of lessons)
- a different mode of delivery of the same Alexander Technique (e.g. individual versus group),
- where the person teaching the Alexander Technique has a different qualification, or level of experience (e.g. specialist teacher versus other health professional with teacher training),
- or combinations of the above.

A1.1.4 Types of outcomes

We considered for inclusion in the review any outcome that aligned with the reasons why the Alexander technique is sought by patients and prescribed by practitioners. In principle, this could include any patient-important outcome that helps elucidate the effects of the Alexander technique on an underlying condition or its symptoms, recovery, rehabilitation, or prevention of disease among people with specific risk factors or pre-conditions. Example outcome domains were shown in the initial analytic framework to illustrate the breadth of outcomes likely to be relevant across a wide range of conditions (Figure A1.1). The outcome domains were based on ICD11 codes and the COMET outcome taxonomy [2, 3]. These systems provide a widely agreed and understood structure for categorising different outcomes.

Studies were included in the review irrespective of the outcome(s) measured, but the summary and synthesis was limited to outcomes considered to be critical or important for each population group. Outcomes for inclusion in the synthesis were determined through the prioritisation process described in Appendix A5.

The outcome domains determined to be critical or important for the synthesis were as follows (see Appendix A6 and Figure A6.1 for details).

- pain
- health-related quality of life
- emotional functioning and mental health
- physical function (disability & mobility)
- falls
- global symptoms

From each study, we selected only one outcome per outcome domain for data extraction (results), risk of bias assessment and inclusion in the synthesis. In selecting outcomes for synthesis, we considered the outcome measure, timing of outcome measurement and data reported as follows.

Outcome measures. For each of these outcome domains, we considered for inclusion any measure of the outcome. Where studies reported multiple outcomes within an outcome domain, we used a population-specific hierarchy of outcomes measures to select the most relevant and valid outcome. The hierarchy of measures was proposed by the review team and agreed through the prioritisation process.

Outcome timing. Where trials reported outcomes measured at multiple timepoints, we selected the first measurement taken after the end of the Alexander technique intervention period (i.e. if administered three times over a week, we took the first measure after the third administration).

Data reported

- When authors reported results for both change scores (change from baseline) and post-intervention (final) values, we selected results for final values.
- If data for the preferred measure was incompletely reported or uninterpretable, we selected another measure.

Excluded outcomes. experience of care (e.g. satisfaction), safety, quality, and economic outcomes.

Appendix A2. Search methods for identification of studies

A2.1 Electronic searches

Studies were sought from the following databases: Cochrane Central Register of Controlled Trials (Cochrane Library, Issue 4, 2023), MEDLINE (Ovid), Embase (Ovid), Emcare (Ovid), AMED (Ovid), CINAHL (EBSCOhost) and Europe PMC. In addition, we searched two clinical trial registers for reports of ongoing or unpublished studies (ClinicalTrials.gov and WHO International Clinical Trials Registry Platform).

The search strategy comprised the text words “alexander technique” or “alexander method” and, where available, the relevant subject heading term. No study design filter was applied. Searches were run on 6 April 2023 and were not limited by language, year of publication or publication status (see Appendix A4).

A2.2 Searching other resources

We reviewed the studies included in the 2015 evidence evaluation for Alexander Technique and examined the reference lists of included studies and any other relevant systematic reviews.

Finally, we searched PubMed for retracted publications, expressions of concern and published errata, as well as the Retraction Watch database.

A2.3 Public submissions

Citations provided by the public via the Department’s call for evidence were deduplicated against the records retrieved by the search and screened for eligibility. We examined the reference lists of any relevant systematic reviews.

Appendix A3. Methods for selecting studies

A3.1 Selection of studies

Records from CENTRAL, PubMed, AMED and Emcare were imported into EndNote and duplicates removed. All remaining records were imported into Covidence for screening. Records submitted through the Department's public call for evidence were first deduplicated against these records. All were found in the search (see Appendix C2 for eligibility decisions).

Two reviewers (MM, SM) piloted guidance for title and abstract screening on a sample of 50 records to ensure the eligibility criteria were applied consistently. All records were reviewed independently by two reviewers at both the title and abstract screening and full-text review stages in Covidence. Disagreements at either stage were resolved by consensus among members of the review team. Advice from NTWC regarding inclusion was not required.

While screening full-text study reports in Covidence, we extracted the trial register and registry record number (if reported) into notes in Covidence. On completion of study report screening, we matched any registry record details in the included study notes (e.g. registry record number) with the registry records search results.

Unmatched registry records were then screened to identify potentially eligible trials for which there was no published report to include in a list of 'ongoing studies' (Appendix C4) and for assessment of bias due to missing results (B1.6).

Published protocols for studies confirmed as meeting the eligibility criteria, but for which results were not available in a published report, were checked against potentially eligible trials identified from registry records and included in the list of 'ongoing studies' (Appendix C4). These were also considered in the assessment of bias due to missing results (B1.6)

The following categories of studies were to be included in a list of 'studies awaiting classification', if identified:

- Studies that were only published as abstracts or for which a full report was not available (i.e. we did not seek further information from study authors to confirm eligibility).
- Studies for which a full report was available but the report was incomplete or ambiguous such that eligibility based on one or more PICO criteria or study design could not be confirmed.
- Studies confirmed as likely to be eligible, but for which no English language translation of the full-text publication was available.
- Studies for which eligibility could not be confirmed following translation of the title and abstract using Google translate (Figure A3.1.1)
- Studies for which there were concerns about data that could not be resolved from full report(s) (e.g. where there were important discrepancies in study characteristics or data reported across multiple publications from the same study).

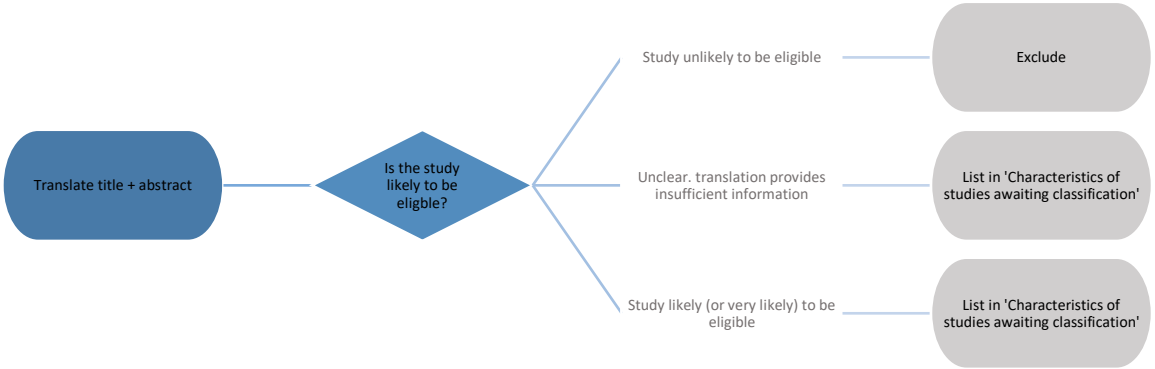

Fig. A3.1.1 | Flowchart showing handling of studies in languages other than English (reproduced from NHMRC framework for natural therapies systematic reviews [10]).

Studies that did not meet the eligibility criteria were excluded and the reason for exclusion was recorded at full-text screening. These studies are included in a list of excluded studies in which the reason for exclusion is reported (Appendix C1).

The search and study selection steps are summarised in the PRISMA flow diagram in Appendix A7.

For studies that originated from the call for evidence, we recorded and reported exclusion decisions irrespective of whether the study was excluded during title and abstract screening or full text review. We documented the flow of these studies through the review in the PRISMA flow chart and in Appendix C2.

Dealing with duplicate and companion publications

Multiple publications to the same study (e.g. protocols, trial registry entries, trial reports) were identified and linked at the study selection stage in Covidence. Identification and linking of multiple reports were also checked at data extraction in REDCap [11, 12]. Each study was given a unique identifier and all linked records are cited in the final report. Records were matched using trial registry numbers.

Dealing with multiple study IDs

If multiple study reports resulted in the same study ID (Author Surname, Year) and were reporting the same study, the study ID for index report was given the suffix '.1' after the Year (e.g. Ziyaeifard 2017.1), and the study ID for the secondary report was given the suffix '.2.' (e.g. Ziyaeifard 2017.2).

If multiple study reports resulted in the same study ID (Author Surname, Year) and were reporting different studies, the study IDs for each study were given the suffix 'a', 'b', etc after the Year (e.g. Ebrahimi 2021a, Ebrahimi 2021b) to differentiate them.

Appendix A4. Results of the search

Bibliographic databases

The search of bibliographic databases retrieved 504 records. After removing duplicates in EndNote and Covidence, 427 records were screened at title/abstract. The search strategies for each database are given below. The PRISMA flow diagram in Appendix A7 summarises inclusion decisions following title/abstract screening.

Trial register records

The search of ClinicalTrials.gov and WHO ICTRP retrieved 26 records, of which 13 were duplicates. Of the 13 unique records screened, 1 was ineligible and 12 eligible. Nine (9) of the eligible records are linked to the studies included in the review and 3 are unpublished (see Appendix C5). All of the unpublished studies were registered within the last 4 years (of 2024). As such, these 3 studies were judged likely to be ongoing.

2015 evidence evaluation for the Alexander Technique

The 2015 overview of the Alexander technique identified 3 systematic reviews that included studies of the Alexander Technique [13-15] the other reviews did not include any studies of the Alexander Technique). These reviews included 3 randomised trials. Two (2) of the 3 studies were retrieved by our search and are included in the meta-analysis. The third unique citation is of an unpublished study for which we were unable to retrieve the full-text. An additional citation was identified for a study that may be eligible, however we were unable to retrieve the full-text (see Appendix C4).

Published systematic reviews

We identified 2 additional studies from published systematic reviews. One was excluded at full-text review (see Appendix C1), and the second is included on the evidence inventory (see Appendix E3)

Public submissions

Sixteen (16) citations were received from the public via the Department's call for evidence. Of these, all 16 were duplicates retrieved by our search. Eligibility decisions for these records are reported in Appendix C2. Six of the submission studies were included in the review.

Retractions and published errata

No records were retrieved from PubMed or the Retraction Watch database.

Search strategies

PubMed (6 April 2023)

#	Search strategy	Results
1	"alexander technique"[All Fields] OR "alexander method"[All Fields]	117

Cochrane Central Register of Controlled Trials (Issue 4 of 12, April 2023)

#	Search strategy	Results
1	("alexander technique" or "alexander method"):ti,ab,kw	54

Embase Classic+Embase via Ovid (1947 to 2023 April 04)

#	Search strategy	Results
1	Alexander Technique/	144
2	(alexander technique or alexander method).mp.	219
3	or/1-2	219

AMED via Ovid (1985 to March 2023)

#	Search strategy	Results
1	Alexander Technique/	79
2	(alexander technique or alexander method).af.	94
3	or/1-2	94

Emcare via Ovid (1995 to 2023 Week 13)

#	Search strategy	Results
1	Alexander Technique/	87
2	(alexander technique or alexander method).mp.	118
3	or/1-2	118

CINAHL Complete via EBSCOhost (6 April 2023)

#	Search strategy	Results
1	SU Alexander Technique OR TI ("Alexander Technique" OR "Alexander Method") OR AB ("Alexander Technique" OR "Alexander Method")	242

Europe PMC

#	Search strategy	Results
1	((TITLE:"alexander technique") OR (ABSTRACT:"alexander technique") OR (TITLE:"alexander method") OR (ABSTRACT:"alexander method")) AND (SRC:"PPR")	1

ClinicalTrials.gov and WHO ICTRP

"alexander technique" or "alexander method" (not limited to any specific field

Appendix A5. Prioritisation process: methods used to refine the questions addressed in the synthesis

The final synthesis questions, and criteria for including studies in each synthesis, were decided through the pre-specified prioritisation process (Figure A). The process was designed to minimise bias in the selection of results for inclusion in the synthesis and ensure coverage of populations and outcomes relevant to the Australian context. All information provided to NTREAP, NTWC and the NHMRC was de-identified and presented in aggregate form so that it was not possible to identify the studies (no bibliographic information, titles etc). No information was provided about the number of studies, number of participants, methodological quality of studies or results.

Prioritisation of populations and grouping of conditions for the summary and synthesis

There was no need to limit populations in this review, so the provision in the protocol to prioritise populations (conditions) for inclusion in the synthesis was not implemented. NTWC endorsed the proposal to structure the synthesis by the population groups outlined in the analytic framework.

Prioritisation and selection of outcomes for the synthesis

For each population, we collated information about the outcomes addressed in all eligible studies. The purpose was twofold: (1) to enable prioritisation of the most important **outcome domains** for each population (irrespective of whether studies measured these domains), and (2) to facilitate selection of the **most relevant results** from each study.

Prioritisation of outcome domains

- All outcomes and outcome measure were listed under an **outcome domain** from the initial analytic framework for the review (Figure A1.1). For outcomes not covered by the initial framework, additional outcome domains were specified allowing categorisation of all outcomes and measures.
- For each condition, NTWC, with input from NTREAP, rated **outcome domains** as critical, important or of limited importance for understanding the effects of Alexander technique on each population group. The intent was to identify up to seven outcome domains for which results would be reported.
- Only critical and important outcome domains were considered in the summary and synthesis.

Outcome selection. From each study, we selected one result per outcome domain for data extraction, risk of bias assessment and reporting of results in the summary and synthesis (using the standardised mean difference to combined effects measured on different scales see B1.2 and B2.1). Selecting one result per study for inclusion in each analysis ensures that individual studies do not receive too much weight. In addition, we aimed to ensure that all studies that should contribute to each synthesis were included.

Overall, the approach deals with multiplicity of results that arises when

(1) the outcomes and measures of outcome domain vary across studies;
(2) individual studies report results for multiple outcomes, measures and timepoints within an outcome domain (e.g. for HR-QoL, reporting an overall score and subscale scores for specific domains of HR-QoL).

To determine which results to select the following was done.

- For each outcome domain, we presented an initial ranking of all outcomes and measures. Where available, the ranking was informed by recommendations in core outcome sets, outcome hierarchies in published Cochrane reviews, and systematic reviews of outcome measures (i.e. to establish relevance, validity, and reliability).
- The NTWC considered the ranking and either confirmed or reranked the outcomes and measures.
- The highest ranked outcome/measure was selected from each study for each outcome domain.
- If data for the highest ranked outcome/measure could not be included in the analysis (e.g. due to incomplete reporting of data), this was reported and the next highest ranked outcome was selected (and so forth).
- Where an outcome measure was potentially eligible for more than one outcome domain, we selected the measure that enabled us to include a study in the largest number of syntheses (e.g. if a study reported scores for the psychological and physical domains of a HR-QoL measure, but no measure of emotional functioning and mental health (EFMH), we chose the physical domain for HR-QoL and the psychological domain for EFMH).

Appendix A6. Final framework for summary and synthesis

Figure A6.1, panel A shows the final analytic framework for the evidence summary and synthesis. The framework provides a guide to the structure of the synthesis and reporting of results (see caption for details). We included all eligible studies in the summary and synthesis (i.e. no limitations by population or condition).

Prioritised outcomes and comparisons

The outcome domains specified in the initial analytic framework were endorsed. Other symptoms were not included in the list of domains rated by NTWC but are retained in the framework because of their relevance to populations/conditions for which no studies were found. Where trials measured outcomes at multiple timepoints, we selected the first measurement after the end of the intervention period or closest to.

Because there were few studies, we broadened criteria for inclusion of outcomes to include HR-QoL and physical function outcomes irrespective of population (i.e. not limited to chronic or longer-term conditions), duration of intervention period (i.e. not limited to weeks or longer) and length of follow-up (not limited to time-frames likely to detect meaningful improvement).

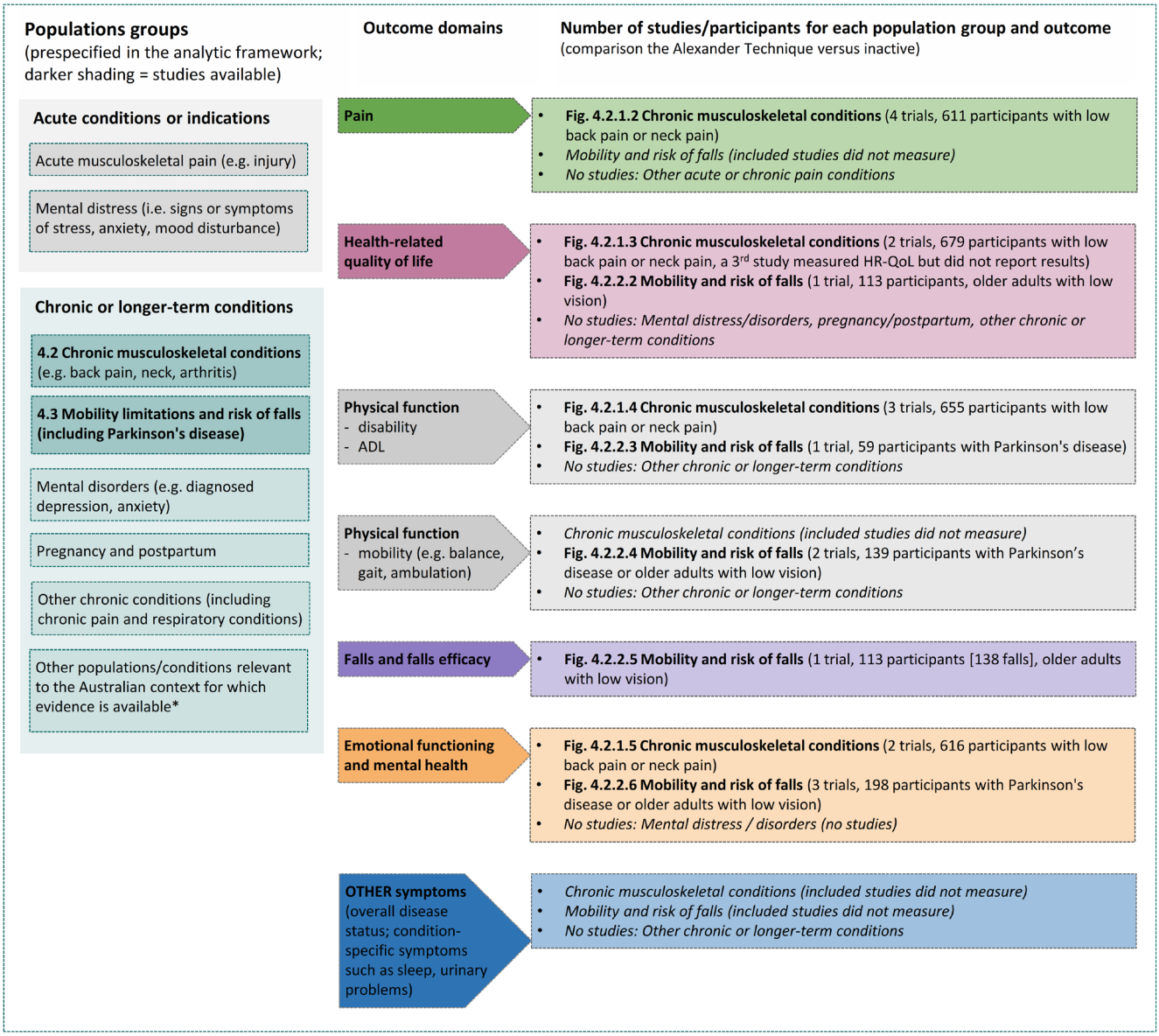

Fig A6.1 | Final analytic framework for the review as agreed through the prioritisation process (Appendix A5). Columns 1 to 2 show the populations and outcome domains for the evidence synthesis. Column 3 shows the populations and outcome domains for which studies were available for the comparison of the Alexander Technique vs. inactive control. Results are reported for each population group in the section indicated in column 1. Study-level data and meta-analyses are presented in the forest plot indicated in column 3. Population groups are those reported as often treated by Alexander technique teachers (UK data) except those marked *

Appendix A7. Summary of inclusion decisions based on the final framework

The flow of studies through the review is in Figure A7.1, the PRISMA flowchart. Inclusions for each synthesis are reported in Figure A6.1 and described in the main report.

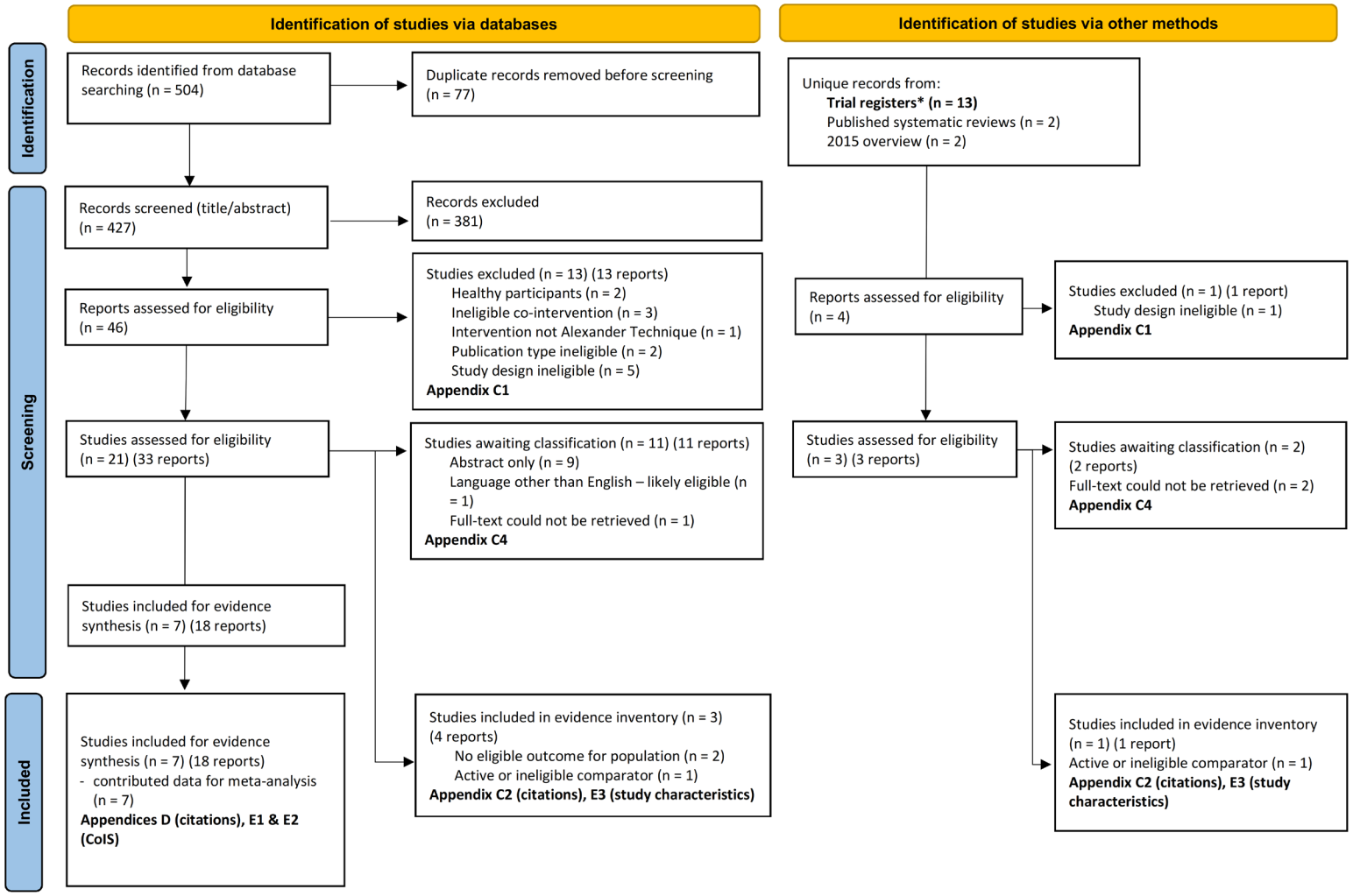

Fig. A7.1 | PRISMA diagram showing the flow of studies through the review (reproduced from main report Fig. 4.1.1). Studies are the unit of interest in the review. Each study could have multiple reports. CoIS: characteristics of included studies. There were no unique records identified from Public submissions (see main report section 4.1 'Public submissions') *See main report section 4.1 for flow of ongoing studies.

Appendix B. Data collection, analysis and interpretation of findings

B1 Data extraction and management

Study data were collected and managed using REDCap electronic data capture tools hosted at Monash University [11, 12]. The form for extracting results data was developed by the review biostatistician (JM). The form was developed for use by our team for the natural therapies reviews and had been applied to over 200 trials in the first review we conducted. Two authors (MM and SB) pre-tested the data extraction and coding form on a pilot study. Both authors discussed the coding after one author (MM) had reviewed the extracted and coded data on study characteristics for completeness, accuracy and consistency. Revisions to the data extraction form were made as required to maximise the quality and consistency of data collection.

We implemented a two-step process for data extraction. In the first step, studies were triaged by a senior author (MM). For each study we coded population groups, outcome domains and comparisons, and allocated the study to analyses according to the analytic framework for the review. We listed all outcomes measured and selected the outcomes for inclusion in the synthesis according to our pre-specified decision rules. During triage, study eligibility was confirmed and basic checks of methodology were done (e.g. confirming that a trial met the minimum requirements for randomisation). Questions about coding, allocation to analyses and outcome selection were referred to a senior author (SB).

For each included study, one review author (MM or SB) then extracted study characteristics and quantitative data using the data extraction and coding form. A second author (SB or MM) independently verified the coding, allocation to analyses, outcome selection and data extraction. All queries related to the quantitative data were referred to a biostatistician (ST). Discrepancies were resolved through discussion with a senior author (SB, JM) if agreement could not be reached or for more complex scenarios.

Where available, we extracted information relating to the characteristics of included studies and results as follows.

1. Study identifiers and characteristics of the study design
    - Study references (multiple publications arising from the same study were matched to an index reference; code as index paper, protocol, registry entry, results paper 1, 2, …)
    - Study name, location (country), enrolment dates (not reported by most studies), and trial registration number
    - Study design (categorised as ‘individually randomised’, ‘cluster randomised’, ‘crossover’, or ‘NRSI’); whether clustering was likely to arise because of the way Alexander technique was delivered (e.g. with one or two teachers at a practice; this information was used to determine which risk of bias tool to use for assessment).
    - Funding sources and funder involvement in study, financial and non-financial interests declared by investigators, potential conflicts (reviewer judgment), ethics approval.

2. Characteristics of each intervention group (including comparator groups)
    - Characteristics of the intervention covering domains of the Template for Intervention Description and Replication (TIDieR) checklist [16]
    - Alexander technique intervention goal (coded, for example: relieve symptoms of a condition, prevent a condition among people with risk factors)
    - Coding of comparators (e.g. inactive – sham, inactive – no intervention, active - massage)
    - Number of participants: randomised to each group, at follow up for selected outcome, and included in analysis and reasons for loss to follow-up

3. Characteristics of participants
    - Participant eligibility criteria (verbatim; precis of key criteria to characterise population)
    - Participant characteristics: age (e.g. mean, median, range), sex
    - Population group: coded using categories specified in the final analytic framework for the review (e.g. chronic musculoskeletal pain, headache or migraine, other chronic conditions)
    - Condition: specific underlying condition as described in study (e.g. cervical spine pain; chronic primary pain), including information about severity (if relevant) and closest ICD-11 code.

- Treatment/procedure: applied to studies in which Alexander technique was administered for the relief of symptoms or side effects of a treatment or procedure for an underlying condition (e.g. chemotherapy). Could include pharmacological treatment (e.g. chemotherapy), surgical, diagnostic or other procedures (as described in study).
- Other characteristics of importance within the context of each study

4. Outcomes assessed and results
   - Outcomes measured (list of all outcomes categorised as 'eligible' or 'ineligible' and categorised according to the final analytic framework; measures used for each)
   - For outcomes selected for inclusion in the summary and synthesis of results:
     - Outcome domain: categorised according to the outcome domains specified in the final analytic framework for the review (e.g. pain, emotional functioning and mental health, health-related quality of life, physical function)
     - Outcome as described in the included study (verbatim or precis)
     - Measurement method (e.g. WOMAC; overall score and pain, function and stiffness subscales), information required to interpret the measure (scale range and direction, minimally important difference) and timing of outcome measurement (exact timing; described in relation to timing of Alexander technique sessions (e.g. immediately after end of intervention period)
     - Results including: summary statistics by group (means and standard deviations, or number of events for outcomes that have been dichotomised, and sample size), estimates of intervention effect (e.g. mean differences (or adjusted mean differences), confidence intervals, t-values, p-values, or risk ratios/odds ratios for binary outcomes).
     - Data required to support risk of bias judgements (see Assessment of risk of bias of included studies) [17]

B1.1 Assessment of risk of bias of included studies

B1.1.1 Assessment of risk of bias in RCTs

We assessed the risk of bias in included studies using the revised Cochrane 'Risk of Bias' tool (RoB 2) for randomised trials [4, 17] for each outcome included in the synthesis.

RoB 2 addresses five domains:

- bias arising from the randomisation process;
- bias due to deviations from intended interventions;
- bias due to missing outcome data;
- bias in measurement of the outcome;
- bias in selection of the reported result.

We applied review-specific guidance developed for the suite of natural therapies reviews to ensure consistency across reviewers. This guidance had been used by the author team to assess over 200 natural therapies studies prior to application in the current review. One review author (MM or SB) then applied the tool to the selected results from each study following the RoB 2 guidance [4], and a second author (SB) checked a subset of assessments. Areas of uncertainty and frequently asked questions were shared with extractors to promote concordance. Advice was sought from the lead reviewer (SB) where there was uncertainty. Supporting information and justifications for judgements for each domain (low, some concerns, high risk of bias) was recorded. We derived an overall summary of the risk of bias from each assessment, following the algorithm in the RoB 2 guidance as implemented in the Excel assessment tool [4].

When multiple effects of the intervention using different approaches were presented in the trial report, we selected one effect for inclusion in the meta-analysis and for risk of bias assessment. The selected effect was chosen according to the following hierarchy, which orders the approaches from (likely) least to most biased for estimating the *effect of assignment to the intervention*: 1. the effect that corresponds to a full intention-to-treat analysis, where missing data have been multiply imputed, or a model-based approach has been used (e.g. likelihood-based analysis, inverse-probability weighting); 2. the effect corresponding to an analysis that adheres to intention-to-treat principles except

that the missing outcome data are excluded; 3. the effect that corresponds to a full intention-to-treat analysis, where missing data have been imputed using methods that treat the imputed data as if they were observed (e.g. last observation carried forward, mean imputation, regression imputation, stochastic imputation); or 4. the effect that corresponds to an 'as-treated' or 'per-protocol' analysis, or an analysis from which eligible trial participants were excluded [4, 17]. The effect used in the assessment was recorded in the data extraction form.

B1.1.2 Assessment of risk of bias in NRSIs

We had planned to use ROBINS-I [18, 19] to assess risk of bias in NRSIs, however there were no NRSIs in the included studies.

B1.2 Measures of treatment effect

We anticipated that many of the outcomes would be continuous (e.g. physical function, HR-QoL), and that varying measurement instruments would be used to measure the same underlying construct across the studies. For this reason, we quantified the effects of the Alexander technique using the standardised mean difference (SMD) (implementing the Hedges' adjusted *g* version). In trials where a continuous measure had been dichotomised (e.g. a continuous pain scale is dichotomised into improvement or no improvement) and analysed as binary outcomes, we re-expressed reported, or calculated, odds ratios as SMDs [20]. We did not report any of our meta-analysis results as dichotomous outcomes.

B1.2.1 Interpretation of treatment effects

Given the wide range of conditions, outcomes and measurement methods reported in the studies eligible for this review, it was not possible to specify thresholds for interpreting the size of the effect for each outcome measure. We planned to use Cohen's guiding rules for interpreting SMDs where 0.2 represents a small effect, 0.5 a moderate effect, and 0.8 a large effect [21]. In practice, our interpretation was based on whether there was an important effect or not [22, 23], with an SMD of 0.2 standard units set as the threshold for an important difference. If the SMD fell within the pre-specified range of -0.2 to 0.2 (i.e. within both thresholds), the effect of the Alexander technique was considered to be no different from control. An SMD above 0.2 or below -0.2 was interpreted as an important effect. We opted to use the most intuitive interpretation of effect estimates for each outcome, so positive values indicate benefit for some outcomes (an increase in health-related quality of life) and harm for other outcomes (an increase in pain). Because we were concerned that bias may be leading to exaggerated effect sizes, we chose not to describe the size of effect (i.e. we did not interpret effects as small, moderate or large).

For the rate of falls, we used a threshold of 5% (50 fewer falls per 1000 people over 1 year). The threshold was not pre-specified, but is in line with the interpretation in Cochrane reviews of falls prevention interventions. [1]

B1.3 Unit of analysis issues

There were no unit of analysis issues in studies included in this review (studies with more than two eligible groups (arms) for a comparison were included after combining data from both arms, and no cluster or cross over trials).

B1.4 Dealing with missing data

As planned in the protocol, we did not contact trial authors to obtain missing information (e.g. study characteristics, description of conduct of the trial) or aggregate level statistics (e.g. missing standard deviations). However, we attempted to calculate statistics necessary for meta-analysis using algebraic manipulation of reported statistics (e.g. computing the standard error for the treatment effect from a reported p-value). Studies for which we calculated or imputed statistics are annotated in forest plots. We planned to explore the impact of these decisions in sensitivity analyses but there were too few studies to do so. Studies for which we could not calculate or impute the statistics required for inclusion in the meta-analysis are listed in the forest plot and in Appendix E4 with reasons for why the results could not be included.

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[1] For example, see https://www.cochranelibrary.com/cdsr/doi/10.1002/14651858.CD013258.pub2/full

We planned to deal with missing outcome data within the primary trials through sensitivity analyses, where trials judged to be at a high risk of bias or some concerns would be excluded; however, this was not possible because there were too few trials included in the review [2]. Risk of bias 'due to missing outcome data' was considered within the overall bias judgement for each trial.

B1.5 Assessment of heterogeneity

We assessed statistical heterogeneity of the intervention effects visually by inspecting the overlap of confidence intervals on the forest plots. While we report formal tests for heterogeneity using the χ^2 test (using a significance level of α=0.1), and quantified heterogeneity using the I^2 statistic [24], these statistics are unlikely to be informative with so few studies. When there was evidence of heterogeneity, we judged its importance by considering where the point estimates for studies lay in relation to the threshold for an important difference (all on one side, indicating similar interpretations across the studies, or not).

B1.6 Assessment of biases due to missing results

We used a framework for assessing risk of bias due to missing results in which an assessment is made for each meta-analysis regarding the risk and potential impact of missing results from studies in which we knew an outcome was measured but not reported (termed 'known-unknowns') and the risk of other missing studies or results (termed 'unknown-unknowns') [25]. The assessment of 'known-unknowns' involves assessment of whether trials meeting the inclusion criteria for a particular meta-analysis have missing results through examination of the publication's methods section, trial registry entry (if available), and trial protocol (if available). We also examine the potential impact of studies for which data could not be included in the meta-analysis (see A1.1.1 Types of studies; A3.1 Selection of studies. We made an assessment as to whether the missing result was potentially due the result itself (e.g. 'not statistically significant'), and whether inclusion of the result could lead to a notable change in the meta-analysis (e.g. if the missing result is from a large trial). These assessments are reported in the results section and considered in the GRADE assessment of publication bias.

We also planned to consider whether there was evidence of selective non-reporting of results from the assessment of 'unknown unknowns'. In assessing 'unknown-unknowns', we planned to judge whether the trials not identified were likely to have results eligible for inclusion (i.e. for the outcome domain 'pain', is it likely that missing studies would have been eligible for inclusion in the overall analysis or for particular conditions). We were unable to use contour enhanced funnel plots to examine whether there was evidence of small study effects [26]. We were also unable to undertake sensitivity analyses to compare the combined effect estimated from the random-effects model (primary analysis) with that estimated from a fixed (common) effect model (together these analyses would inform a decision to downgrade for 'suspected' reporting (publication) bias). In the absence of these analyses, we considered whether there was concern about selective non-reporting arising from small study effects (multiple small studies reporting large effects) and evidence of selective non-reporting in the natural therapies literature more generally.

B2 Data synthesis

B2.1 Meta-analysis

Separate comparisons were set up for each population group and outcome domains agreed in the final framework (see Figure 3.5.1). Some comparisons were stratified by more specific conditions (with an overall estimate and estimate for each condition presented where appropriate) (see Figure A6.1 Appendix A6). Subgroup analysis by population group was used to examine whether these population groups explained any observed statistical heterogeneity in the intervention effects (see Subgroup analysis).

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[2] In the protocol we reported that we would conduct sensitivity analyses excluding trials judged at high or unclear risk of bias . The terminology "Unclear risk of bias" has been replaced in ROB2 with "some concerns". The approach described here is consistent with the protocol in that the sensitivity analyses were to be restricted to studies at low risk of bias.

We combined the effects using a random effects meta-analysis model, since we expected and found there to be clinical and methodological diversity across the trials that may contribute to statistical heterogeneity. These analyses used the restricted maximum likelihood estimator (REML) of between trial heterogeneity variance and the Hartung-Knapp-Sidik-Jonkman confidence interval method. Analyses were conducted in Stata Statistical Software [27]

Forest plots were used to visually depict the intervention effect estimates and their confidence intervals. Forest plots are stratified by condition and risk of bias (within population group). For completeness, results for all studies for which an effect estimate (SMD) could be calculated are presented on the forest plot, including where a single study contributed to the comparison. Studies that had missing or uninterpretable results, or for which an effect estimate (SMD) could not be calculated, are also depicted on the plot.

B2.2 Summary and synthesis when meta-analysis is not possible

Studies that were eligible for the evidence synthesis but could not be included in meta-analyses, are included in the characteristics of included studies table (Appendix E1). These studies are counted as 'missing results' rather than included in a summary or other synthesis (i.e. the data were unavailable because the results were not reported). Details for each of the missing studies are reported in the results for the synthesis for which the outcome would be eligible, together with the reason why data are missing. We did not assess risk of bias because bias (under- or over-estimating the effect) is only relevant if results are included in a meta-analysis or reported. The reasons why these studies were not included in the analysis do not relate to bias (i.e. incomplete reporting of effects and their variances, errors in reporting or analysis of data, no information to interpret), so a risk of bias assessment would not characterise the problems with these studies.

B2.3 Subgroup analysis and investigation of heterogeneity

For mobility limitations and risk of falls, we undertook a subgroup analysis to examine whether population group explains any observed statistical heterogeneity in the intervention effects, using the pre-defined groups specified in the final framework (see Figure A6.1 for population groups in each meta-analysis). However, these analyses provide limited additional information due to the small number of studies.

B2.4 Sensitivity analyses

We planned to undertake and report sensitivity analyses examining if the meta-analysis estimates were robust to the meta-analysis mode, assumptions made to enable inclusion of results in the meta-analysis, and the impact of excluding studies at risk of bias. However, there were too few studies for these analyses.

B2.5 Summary of findings tables and assessment of certainty of the body of evidence

We prepared GRADE summary of findings tables for each of the main comparisons, reporting results for critical and important outcome domains (up to seven). For each result, one author (SB) used the GRADE approach to assess our confidence in where the effect lies relative to our threshold for a small effect (the certainty of evidence) (see Measures of treatment effect). In accordance with detailed GRADE guidance [23, 28, 29], an overall GRADE of high, moderate, low or very low certainty is reported for each result based on whether there are serious, very serious or no concerns in relation to each of the following domains [22].

1. **Risk of bias**. We assessed the overall risk of bias across all studies contributing to each synthesised result. There were too few studies to perform sensitivity analyses to examine whether removing studies at high risk of bias or some concerns changed the direction or size of effect estimate importantly (a reduction in benefit or an increase in harm being most concerning) (see Sensitivity analyses) [3]. We therefore considered the weight that studies at risk of

[3] In the protocol we reported that we would conduct sensitivity analyses excluding trials judged at high or unclear risk of bias. The terminology "Unclear risk of bias" has been replaced in ROB2 with "some concerns". The approach described here is consistent with the protocol in that the sensitivity analyses were to be restricted to studies at low risk of bias.

bias contributed to each result. Where the majority of studies were at high risk of bias, we rated down for very serious concerns.

2. **Imprecision**. We judged imprecision by examining where the 95% confidence interval for each pooled effect estimate lay in relation to our threshold for an important effect (an SMD of -0.2 or 0.2 or the threshold for rate of falls; see Measures of treatment effect). Where the confidence interval clearly crossed a threshold leading to different interpretations (e.g. interpretation of the upper bound of the interval was 'an important effect' and the lower bound 'little or no effect'), we considered rating down for imprecision. If the extent to which the confidence interval crossed the threshold was modest, and the direction was consistent with the point estimate, we did not rate down (e.g. if the upper bound of the confidence interval was an SMD of -0.15 and the point estimate -0.50). We rated down for serious imprecision if the confidence interval crossed one threshold (important benefit or important harm) and the interpretation of either the upper or lower bound of the interval was different from the point estimate (e.g. if the upper bound of the confidence interval was an SMD of 0.40 indicating an important increase in pain, and the point estimate was -0.15 indicating an unimportant reduction in pain). We rated down for very serious imprecision if the confidence interval crossed two thresholds (important benefit and important harm) and for extremely serious imprecision where the confidence interval was so wide that the result was considered uninterpretable. In line with GRADE guidance, we considered the likely impact of inconsistency when rating imprecision since inconsistency can contribute to imprecision [30, 31].

3. **Inconsistency**. We assessed whether there was important, unexplained inconsistency in results across studies considering the overlap of confidence intervals (non-overlap indicating potentially important differences in direction or size of effect). Where there were concerns about inconsistency based on non-overlapping confidence intervals, we considered where the point estimates lie in relation to the threshold for an important effect (if all to one side of a threshold, we were less concerned). While we calculated statistical measures to quantify and test for heterogeneity (I^2 statistic, χ^2 test), there were too few studies for these statistics to be informative. To enhance our interpretation of whether inconsistency is important, we planned to calculate and examine the prediction interval, considering whether it included values that lead to a different conclusion than an assessment based on the confidence interval [32]. However, this is only informative with more than 10 studies, so the method could not be used. Due to the small number of studies, we were unable to use results of subgroup analyses to explain the inconsistency (see Assessment of heterogeneity; specifically, the population subgroups). Where inconsistency was not explained, we rated down. Where a result was based on a single study, inconsistency was not rated [30].

4. **Indirectness.** We assessed whether there are important differences between the characteristics of studies included in each synthesis and the question we were seeking to address, such that the effects observed may not apply to our question (i.e. the applicability of the evidence). For example, differences between the interventions delivered and delivery of Alexander technique in Australia that are likely to influence the size of effect. Where results came from a single small study, we were concerned that similar effects might not be observed in the population of interest more generally, and rated down for serious indirectness. Where the included studies addressed only part of the population of interest (e.g. the only form of chronic musculoskeletal pain was low back pain), we did not rate down for indirectness. Instead, we specified the population from which data came when interpreting results and indicated uncertainty for the population group more generally.

5. **Publication bias**. Our judgement of publication bias was based on assessment of bias due to missing results, primarily from interpretation of known unknowns as per Cochrane guidance for reviews with a small number of studies, where methods for investigating unknown unknowns are less useful (see Assessment of biases due to missing results). We planned to consider the potential impact of excluding studies in languages other than English, but did not identify any studies in languages other than English.

6. **Upgrading domains** (large effect size, dose response gradient, opposing plausible residual confounding). While, in principle, these domains apply to randomised trials, there is no precedent for rating up the evidence from randomised trials, and we did not have reason to apply them in this review.

Using GRADE decision rules, we derived an overall GRADE for the certainty of evidence for each result included in the summary of findings table [29]. A result from a body of evidence comprised of randomised trials begins as 'high' certainty evidence (score=4), and can be rated down (-1 or -2) for serious or very concerns on any GRADE domain that reduces confidence that Alexander technique has an important effect (as determined by the pre-specified thresholds)

[28, 29, 33]. As indicated in point 2, we applied the most recent GRADE guidance which has provision for rating down (-3) for extremely serious imprecision.

Summary of findings tables were prepared using the GRADEpro GDT software [34]. The tables include:

- estimates of the effects of Alexander technique reported as standardised mean differences
- the overall GRADE (rating of certainty) and an explanation of the reason(s) for rating down (or borderline decisions) [35].
- the study design(s), number of studies and number of participants contributing data
- a plain language statement interpreting the evidence for each comparison and outcome, following GRADE guidance for writing informative statements (see B2.6 interpretation of findings) [36].

We present the certainty of evidence in summary of findings tables using one of four levels as explained below.

Certainty	GRADE interpretation	Implications
High (⊕⊕⊕⊕)	we are very confident that the true effect lies close to that of the estimate of the effect	further research is very unlikely to change the confidence in the estimate of effect
Moderate (⊕⊕⊕⊖)	we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different.	further research is likely to have an important impact in the confidence in the estimate of effect
Low (⊕⊕⊖⊖)	our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect.	further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate
Very low (⊕⊖⊖⊖)	we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect.	any estimate of effect is very uncertain

B2.6 Interpretation of findings (evidence statements)

When interpreting results, we followed GRADE guidance for writing informative statements [36]. All interpretations are based on where the point estimate lies in relation to the pre-specified thresholds for an important effect (an important effect or not) and the direction of effect (beneficial or harmful). The certainty of evidence is communicated by qualifying the interpretation of effect (e.g. 'probably' improves for moderate certainty). For low certainty evidence the interpretation is qualified with the word 'may'. For example, 'Alexander technique may improve function' indicates that the point estimate lies above the threshold for important benefit (an SMD >0.2) and that the evidence is of low certainty.

For very low certainty evidence, we do not provide an interpretation of the result except to state 'The evidence is very uncertain about the effect of Alexander technique on outcome'. This is one of two options that GRADE provides for interpreting findings based on very low certainty of evidence: "one option gives the direction of the effect, the other does not" [36]. The decision not to interpret very low certainty results was made independently by the NTWC to ensure a consistent and clear interpretation of findings across Natural Therapy Review reports (see Appendix G).

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30. Guyatt G, Zhao Y, Mayer M, Briel M, Mustafa R, Izcovich A, Hultcrantz M, Iorio A, Alba AC, Foroutan F, et al: **GRADE Guidance 36: Updates to GRADE's approach to addressing inconsistency.** *J Clin Epidemiol* 2023.
31. Zeng L, Brignardello-Petersen R, Hultcrantz M, Mustafa RA, Murad MH, Iorio A, Traversy G, Akl EA, Mayer M, Schunemann HJ, Guyatt GH: **GRADE Guidance 34: update on rating imprecision using a minimally contextualized approach.** *J Clin Epidemiol* 2022, **150:**216-224.
32. Nikolakopoulou A, Higgins JPT, Papakonstantinou T, Chaimani A, Del Giovane C, Egger M, Salanti G: **CINeMA: An approach for assessing confidence in the results of a network meta-analysis.** *PLOS Medicine* 2020, **17:**e1003082.
33. Hultcrantz M, Rind D, Akl EA, Treweek S, Mustafa RA, Iorio A, Alper BS, Meerpohl JJ, Murad MH, Ansari MT, et al: **The GRADE Working Group clarifies the construct of certainty of evidence.** *J Clin Epidemiol* 2017, **87:**4-13.
34. GDT G: **GRADEpro Guideline Development Tool [Software]. McMaster University and Evidence Prime, 2024. Available from gradepro.org.**
35. Santesso N, Carrasco-Labra A, Langendam M, Brignardello-Petersen R, Mustafa RA, Heus P, Lasserson T, Opiyo N, Kunnamo I, Sinclair D, et al: **Improving GRADE Evidence Tables part 3: Guidance for useful GRADE certainty in the evidence judgments through explanatory footnotes.** *J Clin Epidemiol* 2016.
36. Santesso N, Glenton C, Dahm P, Garner P, Akl EA, Alper B, Brignardello-Petersen R, Carrasco-Labra A, De Beer H, Hultcrantz M, et al: **GRADE guidelines: 26. informative statements to communicate the findings of systematic reviews of interventions.** *Journal of Clinical Epidemiology* 2019.

Appendix C. Lists of studies considered for review

Overview of Appendix C – separate file

Appendix C is comprised of four parts (below).

These Appendices report the studies excluded at full text review with reason for exclusion, the public submissions and eligibility decision for each, the studies awaiting classification, and ongoing studies.

Appendix C1. Citation details of studies from search results excluded
Appendix C2. Citation details of studies from public submissions
Appendix C3. Citation details of studies on evidence inventory
Appendix C4. Citation details of studies awaiting assessment
Appendix C5. Characteristics of ongoing studies

Appendix D. Citations for studies included in the evidence synthesis

If multiple reports, the first citation is the index (marked *).

Gleeson 2015	*Gleeson, M.; Sherrington, C.; Lo, S.; Keay, L. Can the Alexander Technique improve balance and mobility in older adults with visual impairments? A randomized controlled trial. 2015. Clin Rehabil; 29(3) 244-60. doi: 10.1177/0269215514542636
	Gleeson, M.; Sherrington, C.; Borkowski, E.; Keay, L. Improving balance and mobility in people over 50 years of age with vision impairments: can the Alexander Technique help? A study protocol for the VISIBILITY randomised controlled trial. 2014. Inj Prev; 20(1) e3. doi: 10.1136/injuryprev-2012-040726
	Gleeson, M.; Sherrington, C.; Lo, S.; Auld, R.; Keay, L. Impact of the Alexander technique on well-being: a randomised controlled trial involving older adults with visual impairment. 2017. Clin Exp Optom; 100(6) 633-641. doi: 10.1111/cxo.12517
	Keay, L. J.; Gleeson, M. G.; Lo, S.; Sherrington, C. Can the Alexander Technique improve balance and mobility in adults over 50 years of age with visual impairments? A singleblind randomized controlled trial. 2014. Investigative Ophthalmology and Visual Science; 55(13) 4579.
Hafezi 2022	*Hafezi, M.; Rahemi, Z.; Ajorpaz, N. M.; Izadi, F. S. The effect of the Alexander Technique on pain intensity in patients with chronic low back pain: A randomized controlled trial. 2022. J Bodyw Mov Ther; 29() 54-59. doi: 10.1016/j.jbmt.2021.09.025
Little 2008.1	*Little, P.; Lewith, G.; Webley, F.; Evans, M.; Beattie, A.; Middleton, K.; Barnett, J.; Ballard, K.; Oxford, F.; Smith, P.; Yardley, L.; Hollinghurst, S.; Sharp, D. Randomised controlled trial of Alexander technique lessons, exercise, and massage (ATEAM) for chronic and recurrent back pain. 2008. BMJ; 337() a884. doi: 10.1136/bmj.a884
	Little, P.; Lewith, G.; Webley, F.; Evans, M.; Beattie, A.; Middleton, K.; Barnett, J.; Ballard, K.; Oxford, F.; Smith, P.; Yardley, L.; Hollinghurst, S.; Sharp, D. Randomised controlled trial of Alexander technique lessons, exercise, and massage (ATEAM) for chronic and recurrent back pain. 2008. Br J Sports Med; 42(12) 965-8.
	Beattie, A.; Shaw, A.; Yardley, L.; Little, P.; Sharp, D. Participating in and delivering the ATEAM trial (Alexander technique lessons, exercise, and massage) interventions for chronic back pain: A qualitative study of professional perspectives. 2010. Complement Ther Med; 18(45385) 119-27. doi: 10.1016/j.ctim.2010.05.037
	Hollinghurst, S.; Sharp, D.; Ballard, K.; Barnett, J.; Beattie, A.; Evans, M.; Lewith, G.; Middleton, K.; Oxford, F.; Webley, F.; Little, P. Randomised controlled trial of Alexander technique lessons, exercise, and massage (ATEAM) for chronic and recurrent back pain: economic evaluation. 2008. BMJ; 337() a2656. doi: 10.1136/bmj.a2656
	Yardley, L.; Dennison, L.; Coker, R.; Webley, F.; Middleton, K.; Barnett, J.; Beattie, A.; Evans, M.; Smith, P.; Little, P. Patients' views of receiving lessons in the Alexander technique and an exercise prescription for managing back pain in the ATEAM trial. 2010. Fam Pract; 27(2) 198-204. doi: 10.1093/fampra/cmp093
	Yardley, Lucy; Dennison, Laura; Coker, Rebecca; Webley, Frances; Middleton, Karen; Barnett, Jane; Beattie, Angela; Evans, Maggie; Smith, Peter; Little, Paul Alexander Technique appeals to people with back pain. 2010. Massage Magazine; (175) 87.
Little 2014.1	*Little, P.; Stuart, B.; Stokes, M.; Nicholls, C.; Roberts, L.; Preece, S.; Cacciatore, T.; Brown, S.; Lewith, G.; Geraghty, A.; Yardley, L.; O Alexander Technique and Supervised Physiotherapy Exercises in back paiN (ASPEN) Feasibility Trial. 2014. Efficacy and Mechanism Evaluation; 1(2) . doi: 10.3310/eme01020
	Little, P.; Stuart, B.; Stokes, M.; Nicholls, C.; Roberts, L.; Preece, S.; Cacciatore, T.; Brown, S.; Steel, C.; Lewith, G.; Geraghty, A.; Yardley, L.; O'Reilly, G.; Chalk, C.; Sharp, D. Alexander technique and supervised physiotherapy exercises in back pain (aspen) feasibility trial. 2014. Journal of Alternative and Complementary Medicine; 20(5) A60. doi: 10.1089/acm.2014.5155
MacPherson 2015	*MacPherson, H.; Tilbrook, H.; Richmond, S.; Woodman, J.; Ballard, K.; Atkin, K.; Bland, M.; Eldred, J.; Essex, H.; Hewitt, C.; Hopton, A.; Keding, A.; Lansdown, H.; Parrott, S.; Torgerson, D.; Wenham, A.; Watt, I. Alexander Technique Lessons or Acupuncture Sessions for Persons With Chronic Neck Pain: A Randomized Trial. 2015. Ann Intern Med; 163(9) 653-62. doi: 10.7326/m15-0667
	Correction: Alexander Technique Lessons or Acupuncture Sessions for Persons With Chronic Neck Pain. 2016. Ann Intern Med; 164(3) 204. doi: 10.7326/l15-0020

MacPherson, H.; Elliot, B.; Hopton, A.; Lansdown, H.; Birch, S.; Hewitt, C. Lifestyle Advice and Self-Care Integral to Acupuncture Treatment for Patients with Chronic Neck Pain: Secondary Analysis of Outcomes Within a Randomized Controlled Trial. 2017. J Altern Complement Med; 23(3) 180-187. doi: 10.1089/acm.2016.0303

MacPherson, H.; Tilbrook, H. E.; Richmond, S. J.; Atkin, K.; Ballard, K.; Bland, M.; Eldred, J.; Essex, H. N.; Hopton, A.; Lansdown, H.; et al., Alexander Technique Lessons, Acupuncture Sessions or usual care for patients with chronic neck pain (ATLAS): study protocol for a randomised controlled trial. 2013. Trials; 14() 209. doi: 10.1186/1745-6215-14-209

Woodman, J.; Ballard, K.; Hewitt, C.; MacPherson, H. Self-efficacy and self-care-related outcomes following Alexander Technique lessons for people with chronic neck pain in the ATLAS randomised, controlled trial. 2018. Eur J Integr Med; 17() 64-71. doi: 10.1016/j.eujim.2017.11.006

Drysdale, H.; Milosevic, I.; Goldacre, B. Reported Outcomes of the Alexander Technique Lessons or Acupuncture Sessions for Persons With Chronic Neck Pain Article. 2016. Ann Intern Med; 164(5) 375-6. doi: 10.7326/l15-0633

Essex, H.; Parrott, S.; Atkin, K.; Ballard, K.; Bland, M.; Eldred, J.; Hewitt, C.; Hopton, A.; Keding, A.; Lansdown, H.; et al., An economic evaluation of Alexander Technique lessons or acupuncture sessions for patients with chronic neck pain: a randomized trial (ATLAS). 2017. PLoS One; 12(12) e0178918. doi: 10.1371/journal.pone.0178918

Tilbrook, H. E.; Becque, T.; Buckley, H.; Macpherson, H.; Bailey, M.; Torgerson, D. J. Randomized trial within a trial of yellow 'post-it notes' did not improve questionnaire response rates among participants in a trial of treatments for neck pain. 2015. Journal of Evaluation in Clinical Practice; 21(2) 202-204. doi: 10.1111/jep.12284

Wenham, A.; Atkin, K.; Woodman, J.; Ballard, K.; MacPherson, H. Self-efficacy and embodiment associated with Alexander Technique lessons or with acupuncture sessions: A longitudinal qualitative sub-study within the ATLAS trial. 2018. Complement Ther Clin Pract; 31() 308-314. doi: 10.1016/j.ctcp.2018.03.009

Sedaghati 2018

*Sedaghati, P.; Goudarzian, M.; Daneshmandi, H.; Ardjmand, A. Effects of alexander-based corrective techniques on forward flexed posture, risk of fall, and fear of falling in idiopathic Parkinson's disease. 2018. Archives of Neuroscience; 5(2) e61274. doi: 10.5812/archneurosci.61274

Stallibrass 2002

*Stallibrass, C.; Sissons, P.; Chalmers, C. Randomized controlled trial of the Alexander technique for idiopathic Parkinson's disease. 2002. Clin Rehabil; 16(7) 695-708. doi: 10.1191/0269215502cr544oa

Stallibrass, C.; Frank, C.; Wentworth, K. Retention of skills learnt in Alexander technique lessons: 28 People with idiopathic Parkinson's disease. 2005. Journal of Bodywork and Movement Therapies; 9(2) 150-157. doi: 10.1016/j.jbmt.2004.06.004

Appendix E. Characteristics of studies included in the review

Overview of Appendix E – separate file

Appendix E is comprised of three parts, each in a separate file.

Appendix E1 provides information about the characteristics of each of the studies eligible for the evidence synthesis.

- study ID, location, setting, and study design
- the population eligibility criteria, number of participants randomised, participant characteristics, and ICD codes
- the Alexander technique treatment goal, and details about the Alexander technique intervention(s) and comparator(s)
- a list of all reported outcome(s) categorised according to whether they were eligible or ineligible for the synthesis, the measurement method for each eligible outcome, the timing of outcome measurement, and the outcome(s) selected for inclusion in the synthesis for each outcome domain

Appendix E2 provides information about funding, declaration of interest and ethics approval for each study.

Studies were included in E1 and E2 irrespective of whether they provided data that could be included in the meta-analysis.

Appendix E3 provides information about the characteristics of each of the studies included in the evidence inventory.

Appendices are as follows

E1. Characteristics of studies included in the evidence synthesis
E2. Funding sources, potential conflicts of interest and ethics approval for studies included in the evidence synthesis
E3. Characteristics of studies included in the evidence inventory

Appendix F. Risk of bias assessments

All studies in this review were individually randomised, hence all assessments use the ROB 2 tools for trials with a parallel design. Assessments are presented in alphabetical order by study ID.

For each study, an assessment was done for each outcome and comparison contributing to the meta-analysis (MA; or where results could not be included in the MA but were tabulated).

For each study we report

- the comparison for the assessment,
- the outcome domain for the assessment,
- other outcomes included in MAs for the study (noting if the assessment was the same for these or other comparisons),
- the study design (parallel trial)

Where the RoB assessment was the same for all outcomes, comparisons or both, only one assessment is reported.

The assessment includes

- The overall risk of bias judgement
- The judgement for each domain, with an explanation provided for each signalling questions for which the response could lead to a judgement of high risk of bias or some concerns
- The response to each signalling question (numbers, the questions are reported in full below)

We did not assess studies that were counted as 'missing results' (i.e. those studies where the result was judged to be uninterpretable or where there were major concerns about the integrity of the data such that it would be misleading to report the results). In such cases, concerns about bias leading to an under- or over-estimate of effect are inconsequential compared to the impact of major errors in reported data or the interpretation of that data.

Box F1. Signalling questions from the revised Cochrane risk of bias (ROB 2) tool for randomised trials (parallel design)

Parallel (individually randomised)
Domain 1. Bias arising from the randomisation process
1.1 Was the allocation sequence random?
1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?
1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?
Domain 2. Bias due to deviations from intended interventions
2.1 Were participants aware of their assigned intervention during the trial?
2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?
2.3 If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the trial context?
2.4 If Y/PY to 2.3 Were these deviations likely to have affected the outcome?
2.5 If Y/PY to 2/4: Were these deviations from intended intervention balanced between groups?
2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?
2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized?
Domain 3. Bias due to missing outcome data
3.1 Were data for this outcome available for all, or nearly all, participants randomized?
3.2 If N/PN/NI to 3.1a or 3.1b: Is there evidence that the result was not biased by missing data?
3.3 If N/PN to 3.2 Could missingness in the outcome depend on its true value?
3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?
Domain 4. Bias in the measurement of the outcome
4.1 Was the method of measuring the outcome inappropriate?
4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?
4.3 If N/PN/NI to 4.1 and 4.2: Were outcome assessors aware of the intervention received by study participants?
4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?
4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?
Domain 5. Bias from selection of the reported result
5.1 Were the data that produced this result analysed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?

Parallel (individually randomised)
5.2 … multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?
5.3 … multiple eligible analyses of the data?

Study ID. **Gleeson 2015**	**Outcome domain.** EFMH **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Sequence generation random and allocation sequence concealed (due to different block size and use of central service): "block-randomized (block permutation size 1, 2 and 4) using a computer generated list from http://www.randomization. com kept by a separate centre-based investigator who had no contact with the participants."	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Participants and those delivering the intervention were aware of their assigned intervention. The comparator group received ongoing care from the Guide Dogs association. Modified intention-to-treat (mITT) analysis (excluding participants with missing outcome data)	Y	Y	N	NA	NA	Y	NA
3. Bias due missing outcome data	Low	I: 55/60 (8% missing) C: 58/60 (3% missing) Analysis method did not correct for bias; no sensitivity analysis In theory, missingness could depend on the true value of the outcome, however, there is little or no difference in depression scores between groups, and scores are similar to baseline, so it seems unlikely that missingness depended on the true value of the outcome. Reasons given for loss to follow up: I. 3 withdrawals, 1 refusal v 1 withdrawal (other reasons death, hospitalisation). Slightly higher withdrawals in intervention group, most likely due to treatment burden.	PN	PN	PN	NA			
4. Bias in the measurement of the outcome	Some concerns	Self-report measure of depression. Participants were aware of the treatment group that they were assigned to. Trial outcome assessors were masked to treatment group and participants were asked not to reveal their treatment group, but assessments were made in participants homes and it was possible that assessors may become aware of group assignment. Participants' knowledge of the intervention they received could have influenced their response; however given depression is unlikely to be perceived as the main reason for undertaking Alex T, it seems unlikely that this outcome would be influenced by knowledge of the intervention at 3 months.	N	PN	Y	PY	PN		

Study ID. Gleeson 2015	**Outcome domain.** EFMH **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
5. Bias in the selection of the reported results	Low	A statistical analysis plan was developed prior to analysis and is available from the corresponding author on request. A published protocol is also available. Changes to primary outcomes documented and made prior to unblinded outcome data were available for analysis. Multiple measures eligible for the meta-analysis of EFMH are fully reported in the paper, at multiple time points (and confirmed in published protocol and registry record). It is unlikely that there were other results from which these measures were selected. Results are reported for multiple ways of analysing/handling the outcome, and it is unlikely that these were selected from other analyses.	PY	PN	PN				
OVERALL risk of bias	**Some concerns**								

Study ID. Gleeson 2015	**Outcome domain.** Falls **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Sequence generation random and allocation sequence concealed (due to different block size and use of central service): "block-randomized (block permutation size 1, 2 and 4) using a computer generated list from http://www.randomization. com kept by a separate centre-based investigator who had no contact with the participants."	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Participants and those delivering the intervention were aware of their assigned intervention. The comparator group received ongoing care from the Guide Dogs association. Modified intention-to-treat (mITT) analysis (excluding participants with missing outcome data)	Y	Y	N	NA	NA	Y	NA
3. Bias due missing outcome data	Low	I: 55/60 (8% missing) C: 56/60 (7% missing) Analysis method did not correct for bias; no sensitivity analysis Reasons for withdrawal (I: 4, C: 2) and refusal (I: 1) not reported, and it is unclear if related to the outcome (falls over 12 months). However withdrawals are balanced between the groups.	PN	PN	PN	NA			

Study ID. Gleeson 2015	**Outcome domain.** Falls **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
4. Bias in the measurement of the outcome	Low	Self-report of falls (falls diary) by participants who were aware of whether they received Alexander technique or usual care. Participants were required to self-report any falls over the previous month to the research team. Participants' knowledge of the intervention they received could have influenced their response, however it is unlikely that falls were under- or over-reported.	N	PN	Y	PN	NA		
5. Bias in the selection of the reported results	Low	A statistical analysis plan was developed prior to analysis and is available from the corresponding author on request. A published protocol is also available. Changes to primary outcomes documented and made prior to unblinded outcome data were available for analysis. Multiple measures eligible for the meta-analysis of falls are fully reported in the paper (and confirmed in published protocol and registry record). It is unlikely that there were other results from which these measures were selected. Results are reported for multiple ways of analysing/handling the outcome, and it is unlikely that these were selected from other analyses.	PY	PN	PN				
OVERALL risk of bias	**Low**								

Study ID. Gleeson 2015	**Outcome domain.** HR-QoL **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Sequence generation random and allocation sequence concealed (due to different block size and use of central service): "block-randomized (block permutation size 1, 2 and 4) using a computer generated list from http://www.randomization. com kept by a separate centre-based investigator who had no contact with the participants."	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Participants and those delivering the intervention were aware of their assigned intervention. The comparator group received ongoing care from the Guide Dogs association.	Y	Y	N	NA	NA	Y	NA

Study ID. Gleeson 2015	**Outcome domain.** HR-QoL **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		Modified intention-to-treat (mITT) analysis (excluding participants with missing outcome data)							
3. Bias due missing outcome data	Low	I: 55/60 (8% missing) C: 58/60 (3% missing) Analysis method did not correct for bias; no sensitivity analysis In theory, missingness could depend on the true value of the outcome, however, the it seems unlikely that that this would bias the result given how few participants would be missing for this reason (i.e. are unaccounted for). Reasons given for loss to follow up: I. 3 withdrawals, 1 refusal v 1 withdrawal (other reasons death, hospitalisation). Slightly higher withdrawals in intervention group, most likely due to treatment burden.	PN	PN	PN	NA			
4. Bias in the measurement of the outcome	Some concerns	Self-report measure of HRQOL. Participants were aware of the treatment group that they were assigned to. Trial outcome assessors were masked to treatment group and participants were asked not to reveal their treatment group, but assessments were made in participants homes and it was possible that they may become aware of group assignment. Participants' knowledge of the intervention they received could have influenced their response; however it seems unlikely that this outcome would be influenced by knowledge of the intervention at 3 months.	N	PN	Y	PY	PN		
5. Bias in the selection of the reported results	Low	A statistical analysis plan was developed prior to analysis and is available from the corresponding author on request. A published protocol is also available. Changes to primary outcomes documented and made prior to unblinded outcome data were available for analysis. The measures eligible for the meta-analysis of HR-QoL is fully reported in the paper, at multiple time points (and confirmed in published protocol and registry record). Results for subgroups have also been reported. It is unlikely that there were other results from which these measures were selected. Results are reported for multiple ways of analysing/handling the outcome, and it is unlikely that these were selected from other analyses.	PY	PN	PN				

Study ID. Gleeson 2015	**Outcome domain.** HR-QoL **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
OVERALL risk of bias	**Some concerns**								

Study ID. Gleeson 2015	**Outcome domain.** physical function (mobility) **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Sequence generation random and allocation sequence concealed (due to different block size and use of central service): "block-randomized (block permutation size 1, 2 and 4) using a computer generated list from http://www.randomization. com kept by a separate centre-based investigator who had no contact with the participants."	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Participants and those delivering the intervention were aware of their assigned intervention. The comparator group received ongoing care from the Guide Dogs association. Modified intention-to-treat (mITT) analysis (excluding participants with missing outcome data)	Y	Y	N	NA	NA	Y	NA
3. Bias due missing outcome data	Low	I: 55/60 (8% missing) C: 58/60 (3% missing) Analysis method did not correct for bias; no sensitivity analysis reasons for withdrawal (I: 4, C: 2) and refusal (I: 1) not reported, and it is unclear if related to the outcome (mobility limitations). However withdrawals are balanced between the groups.	PN	PN	PN	NA			
4. Bias in the measurement of the outcome	Low	Tests were performed by trial outcome assessors who were masked to treatment group and participants were asked not to reveal their treatment group. However, assessments were made in participants homes and it was possible that assessors may become aware of group assignment. The outcome measures for mobility are comparatively objective (timed tests), so unlikely to be be influenced importantly by knowledge of the intervention received.	N	PN	Y	PN	NA		
5. Bias in the selection of the reported results	Low	A statistical analysis plan was developed prior to analysis and is available from the corresponding author on request. A published protocol is also available. Changes to primary outcomes	PY	PN	PN				

Study ID. Gleeson 2015	**Outcome domain.** physical function (mobility) **Assessments**. pain, HRQOL, EFMH, function - mobility, falls		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		documented and made prior to unblinded outcome data were available for analysis. Multiple measures eligible for the meta-analysis of function are fully reported in the paper, at multiple time points (and confirmed in published protocol and registry record). It is unlikely that there were other results from which these measures were selected. Results are reported for multiple ways of analysing/handling the outcome, and it is unlikely that these were selected from other analyses.							
OVERALL risk of bias	**Low**								

Study ID. Hafezi 2022	**Outcome domain.** pain **Assessments**. pain		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Some concerns	Reported that "A total of 80 patients ... were randomly assigned into ... groups using the block randomization method (Fig. 1)." No description of how the sequence was generated, by whom (stated that "the statistical specialist was also unaware of the participant group assignment" but no mention of their role). No information to determine who enrolled, and whether they had knowledge of the sequence. No statement about block size to ascertain if predictable. No baseline differences reported between groups (error in reported % of female in control group). However, the absence of any information about randomisation or allocation concealment raises concerns that the study may be at high risk of bias.	NI	NI	N				
2. Bias due to deviations from the intended intervention	Low	There are no reported dropouts.	Y	Y	PN	NA	NA	Y	NA
3. Bias due missing outcome data	Low	The trialists report that all 80 participants were retained in the study at follow-up, which is unusual given the length of intervention period / follow-up. Alex T intervention period is 12 weeks, 3 sessions per week, and the control group receives usual care.	PY	NA	NA	NA			
4. Bias in the measurement of the outcome	High	Self-report measure and participants were aware of the intervention group	N	PN	Y	PY	PY		

Study ID. Hafezi 2022	**Outcome domain.** pain **Assessments**. pain		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		they were assigned to. The pain measure (VAS) was completed in the presence of one of the investigator team who "was unaware of the participant group assignment" however, it is plausible that they could be unblinded by participants indicating the treatment they received, especially given the length/intensity of the Alex T intervention. Participants' knowledge of the intervention they received could have influenced their response and the circumstances of the trial make it likely. The intervention involves 3 visits per week to the therapist over 3 months (36 sessions), and the single outcome measure (pain, VAS) is completed in the presence of one of the trialists. After an intervention of this duration/intensity, it is likely that participants will hold positive beliefs about the intervention and wish to please the trialists when asked about their pain immediately after the end of the intervention period.							
5. Bias in the selection of the reported results	Some concerns	There is no mention of an analysis plan and the trial was registered after recruitment commenced. https://irct.behdasht.gov.ir/trial/37096 The trial registry report use of a single measure of pain (VAS) immediately after (prioritised timepoint for SR) and 1 month after the 12 week intervention. Both results are reported. Results are reported as summary statistics and it is unlikely that these were selected from other analyses	NI	PN	PN				
OVERALL risk of bias	**High**								

Study ID. Little 2008	**Outcome domain.** physical function (disability) **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Central randomisation service, computer generated random numbers (sequence generated by statistician). Block randomisation (unequal block sizes, and practices not aware of block size or number randomised)	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Participants advised of "suggestive preliminary evidence to support each intervention (Alexander technique,	Y	PY	PN	NA	NA	Y	NA

Study ID. **Little 2008**	**Outcome domain.** physical function (disability) **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		massage, and exercise)" and a small number received only usual care. Unlikely that additional interventions were used beyond expected through usual care. Analysis appears to be ITT (number in flowchart matches those reported for results at each follow-up)							
3. Bias due missing outcome data	Low	"579 people were randomised and completed the baseline questionnaires, 469 (81%) completed the questionnaires at three months". Attrition was similar across groups (Fig 1). Alex T (4 groups). 78-88% remained C. 74-76% remained) Responder/non-responder analysis showed that "response was not related to baseline Roland disability scores"	PN	PY	NA	NA			
4. Bias in the measurement of the outcome	Low	We measured outcomes at baseline, three months, and one year using postal questionnaires, with two mailings to non-responders and telephone follow-up for a smaller dataset (Roland disability scale, days in pain, Von Korff scale, health transition) for those not responding. Most data collected via postal questionnarie, but measures were self-report and participants were aware of their group allocation. Data entry was blind to study group. While it is possible that participants' knowlege of the intervention received could influence, given the length of intervention, follow-up and nature of the outcomes, it seems unlikely that this would have influenced the assessment of the outcome importantly.	N	PN	Y	PY	N		
5. Bias in the selection of the reported results	Low	Plan not published (but common for trial of this age): "The analysis plan was agreed in advance by the trial management group." Multiple measures eligible for the meta-analysis of this outcome are fully reported in the paper, at multiple time points. It is unlikely that there were other results from which these measures were selected.	PY	N	PN				
OVERALL risk of bias	**Low**								

Study ID. **Little 2014**	**Outcome domain.** physical function (disability) **Assessments**. pain, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Central randomisation service, computer generated random numbers (randomisation supervised by statistician). Block randomisation; randomisation executed by a nurse from each practice ringing central service. RMDQ and pain scores at baseline similar in Alexander technique and control (usual care) groups.	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	Adherence was measured - no reports of deviations due to trial context. "intention-to-treat analysis of covariance to estimate the main effects of the interventions, with no imputation of missing data"	Y	Y	PN	NA	NA	Y	NA
3. Bias due missing outcome data	Low	Alex T: 15/17(11% missing) C: 13/17(23% missing). All participants were accounted for; reason for missingness for most is unrelated to outcome. One in each group did not give a reason. One in Alex T group reported back pain had resolved. If this were due to the effects of Alex T, the missing data would bias effect in favour of control.	N	PN	PY	PN			
4. Bias in the measurement of the outcome	Some concerns	Data for selected measures was collected via postal questionnarie, but measures were self-report and participants were aware of their group allocation. Participants also underwent a 2 hour assessment of biomechanical outcomes, and other assessments of feasibility of trial methods. Each required interaction with the study team. It is unclear how much interaction preceeded the postal questionnaire. Participants also reported low expectations of usual care for low back pain, but rated the study interventions favourably. In combination, we can't rule out that this may have led Alex T participants to report outcomes more favourably.	N	PN	Y	Y	PN		
5. Bias in the selection of the reported results	Low	Analysis plan pre-specified (actual plan not reported, but no concerns). Since this was a feasibility trial, Multiple measures of disability (RMDQ, Oswestry, von Korff disability scale) but effect estiamtes for all are reported and RMDQ is the prioritised measure. Summary statistics are not reported for all outcomes, but RMDQ is primary outcomes and reported (although at 10 weeks, not 12) but there is no obvious reason to be concerned about selective non-reporting	Y	PN	PN				

Study ID. Little 2014	**Outcome domain.** physical function (disability)		**Comparison.** Alexander technique v usual care						
	Assessments. pain, function - disability,		**Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
OVERALL risk of bias	**Some concerns**								

Study ID. MacPherson 2015	**Outcome domain.** physical function (disability)		**Comparison.** Alexander technique v usual care						
	Assessments. pain, HRQOL, EFMH, function - disability,		**Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	"...secure randomization system allocated patients to the intervention groups, with varied block size dynamically generated depending on the number of patients allocated each week. Blocks could include patients from more than 1 practice. The randomization sequence was concealed ... Researchers were then informed of allocations" Table 1, Baseline characteristics similar including for main outcomes.	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	"The randomization sequence was concealed .. Researchers were then informed of allocations, communicated them to participants and their GP practice, and arranged initial appointments with practitioners. Masking was not feasible because of the active self-care components that were specific to the interventions." No indication of deviations due to trial context (non-adherence as might be expected in practice) Analysed as randomised "The analyses retained all participants in the groups to which they were originally randomly assigned."	Y	Y	PN	NA	NA	Y	NA
3. Bias due missing outcome data	Low	For this outcome, 6 month follow-up is available (which is the priortised timeframe). At 6 months: Alex T. 143/172 (17% missing), C. 148/172 (14% missing) For main analysis (disability outcome), sensitivity analyses were used to assess departures from the assumption that missing data were missing at random. Sensitivity analyses showed that the results were robust to departures from the MAR assumption when the departures were similar in the intervention and usual care groups or occurred in the usual care group only".	PN	PY	NA	NA			
4. Bias in the measurement of the outcome	Low	All measures were self-report and participants were aware of their allocated intervention.	N	PN	Y	PY	PN		

Study ID. MacPherson 2015	**Outcome domain.** physical function (disability) **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		Assessment could have been influenced by knowledge of intervention, but unlikely that this was the case at 6 month follow-up. Data were collected by postal questionnaire, so less likely that participants would respond in a manner intended to please the study team. "Data on the NPQ were collected at baseline and by postal questionnaire at 3, 6, and 12 months." Although participants had higher expectations that Alex T would be effective than they did for usual care, it seems unlikely that this would influence outcomes 1 month after the end of intervention.							
5. Bias in the selection of the reported results	Low	The authors changed their primary analysis for the disability (primary) outcome, but not that for other outcomes. The reason was to comply with the journal's editorial policy, which seems plausible. The planned analysis was also conducted and reported in full. Reported an effect estimate at 12 months, but not at 6 months: "We found no significant differences between the interventions and usual care for the physical component score of the SF-12v2 at 6 or 12 months (… Alexander lessons, 0.38 [CI, 21.54 to 2.30] [P = 0.69]) …. " The reported effect is very small, so it is unlikley that the result not reported has been selective non-reported.	PY	PN	PN				
OVERALL risk of bias	**Low**								

Study ID. MacPherson 2015	**Outcome domain.** EFMH **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	"...secure randomization system allocated patients to the intervention groups, with varied block size dynamically generated depending on the number of patients allocated each week. Blocks could include patients from more than 1 practice. The randomization sequence was concealed ... Researchers were then informed of allocations" Table 1, Baseline characteristics similar including for main outcomes.	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	"The randomization sequence was concealed .. Researchers were then informed of allocations, communicated them to participants and their GP	Y	Y	PN	NA	NA	Y	NA

Study ID. MacPherson 2015	**Outcome domain.** EFMH **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		practice, and arranged initial appointments with practitioners. Masking was not feasible because of the active self-care components that were specific to the interventions." No indication of deviations due to trial context (non-adherence as might be expected in practice) Analysed as randomised "The analyses retained all participants in the groups to which they were originally randomly assigned."							
3. Bias due missing outcome data	Low	For this outcome, only 12 month follow-up is available (results for priortised timeframe of 6 months are not reported). At 12 months: Alex T. 145/172 (16% missing), C. 144/172 (16% missing) For main analysis (disability outcome), senstivity analyses were used to assess departures from the assumption that missing data were missing at random. Sensitivity analyses showed that the results were ro- bust to departures from the MAR assumption when the departures were similar in the intervention and usual care groups or occurred in the usual care group only. These analyses were not done for HRQOL, but it seems likely that the assumptions would hold for HRQOL.	PN	PY	NA	NA			
4. Bias in the measurement of the outcome	Low	All measures were self-report and participants were aware of their allocated intervention. Assessment could have been influenced by knowledge of intervention, but unlikely that this was the case at 12 month follow-up. Data were collected by postal questionnaire, so less likely that participants would respond in a manner intended to please the study team. "Data on the NPQ were collected at baseline and by postal questionnaire at 3, 6, and 12 months." Although participants had higher expectations that Alex T would be effective than for usual care, it seems unlikely that this would influence outcomes 7 months after the end of intervention.	N	PN	Y	PY	PN		
5. Bias in the selection of the reported results	Low	The authors changed their primary analysis for the disability (primary) outcome, but not that for other outcomes. The reason was to comply with the journal's editorial policy, which seems plausible. The planned analysis was also conducted and reported in full.	PY	PN	PN				

Study ID. MacPherson 2015	**Outcome domain.** EFMH **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		Reported an effect estimate at 12 months, but not at 6 months (the result which is unavailable is the prioritised result for MA): We found no significant differences between the interventions and usual care for the … mental component score at 6 months. However, significantly larger improvements in the mental component score occurred in the intervention groups than in the usual care group at 12 months (… Alexander lessons, 2.12 [CI, 0.42 to 3.82] [P = 0.016]). This means that the prioritised result is less favourable, and hence there is concern about bias due to selective non reporting on the basis of the result for SF-36. Rather than report this result, we report the PSS scale and do not rate down for bias in the selection of the reported result.							
OVERALL risk of bias	**Low**								

Study ID. MacPherson 2015	**Outcome domain.** HR-QoL **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	"...secure randomization system allocated patients to the intervention groups, with varied block size dynamically generated depending on the number of patients allocated each week. Blocks could include patients from more than 1 practice. The randomization sequence was concealed ... Researchers were then informed of allocations" Table 1, Baseline characteristics similar including for main outcomes.	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	"The randomization sequence was concealed .. Researchers were then informed of allocations, communicated them to participants and their GP practice, and arranged initial appointments with practitioners. Masking was not feasible because of the active self-care components that were specific to the interventions." No indication of deviations due to trial context (non-adherence as might be expected in practice) Analysed as randomised "The analyses retained all participants in the groups to which they were originally randomly assigned."	Y	Y	PN	NA	NA	Y	NA

Study ID. **MacPherson 2015**	**Outcome domain.** HR-QoL **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
3. Bias due missing outcome data	Low	For this outcome, only 12 month follow-up is available (results for priortised timeframe of 6 months are not reported). At 12 months: Alex T. 145/172 (16% missing), C. 144/172 (16% missing) For main analysis (disability outcome), senstivity analyses were used to assess departures from the assumption that missing data were missing at random. Sensitivity analyses showed that the results were robust to departures from the MAR assumption when the departures were similar in the intervention and usual care groups or occurred in the usual care group only." These analyses were not done for HRQOL, but it seems likely that the assumptions would hold for HRQOL.	PN	PY	NA	NA			
4. Bias in the measurement of the outcome	Low	All measures were self-report and participants were aware of their allocated intervention. Assessment could have been influenced by knowledge of intervention, but unlikely that this was the case at 12 month follow-up. Data were collected by postal questionnaire, so less likely that participants would respond in a manner intended to please the study team. "Data on the NPQ were collected at baseline and by postal questionnaire at 3, 6, and 12 months." Although participants had higher expectations that Alex T would be effective than they did for usual care, it seems unlikely that this would influence outcomes 7 months after the end of intervention.	N	PN	Y	PY	PN		
5. Bias in the selection of the reported results	Low	The authors changed their primary analysis for the disability (primary) outcome, but not for other outcomes. The reason was to comply with the journal's editorial policy, which seems plausible. The planned analysis was also conducted and reported in full. Reported an effect estimate at 12 months, but not at 6 months: "We found no significant differences between the interventions and usual care for the physical component score of the SF-12v2 at 6 or 12 months (… Alexander lessons, 0.38 [CI, 21.54 to 2.30] [P = 0.69]) …. " The reported effect is very small, so it is unlikely that the result not reported has been selectively non-reported.	PY	PN	PN				
OVERALL risk of bias	**Low**								

Study ID. **MacPherson 2015**	**Outcome domain.** pain **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	"...secure randomization system allocated patients to the intervention groups, with varied block size dynamically generated depending on the number of patients allocated each week. Blocks could include patients from more than 1 practice. The randomization sequence was concealed ... Researchers were then informed of allocations" Table 1, Baseline characteristics similar including for main outcomes.	Y	Y	N				
2. Bias due to deviations from the intended intervention	Low	"The randomization sequence was concealed .. Researchers were then informed of allocations, communicated them to participants and their GP practice, and arranged initial appointments with practitioners. Masking was not feasible because of the active self-care components that were specific to the interventions." No indication of deviations due to trial context (non-adherence as might be expected in practice) Analysed as randomised "The analyses retained all participants in the groups to which they were originally randomly assigned."	Y	Y	PN	NA	NA	Y	NA
3. Bias due missing outcome data	High	There is a large amount of missing data for this outcome (which was collected by text messaging fortnightly) At 6 months: Alex T. 84/172 (51% missing), C. 83/172 (52% missing) For main analysis (disability outcome), sensitivity analyses were used to assess departures from the assumption that missing data were missing at random. Sensitivity analyses showed that the results were robust to departures from the MAR assumption when the departures were similar in the intervention and usual care groups or occurred in the usual care group only. Given the amount of missing data for the pain outcome, it is unclear that the assumptions about data missing at random would hold for this outcome measure. It is plausible that those who did not respond at 6 months (or other timepoints) did not respond for reasons related to their outcomes.	PN	N	PY	PY			
4. Bias in the measurement of the outcome	Some concerns	All measures were self-report and participants were aware of their allocated intervention. Assessment could have been influenced by knowledge of intervention. Data were collected by text message for this pain	N	PN	Y	PY	PN		

Study ID. MacPherson 2015	**Outcome domain.** pain **Assessments**. pain, HRQOL, EFMH, function - disability,		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		outcome, so less likely that participants would respond in a manner intended to please the study team. Participants had higher expectations that Alex T would be effective than they did for usual care; however, it seems unlikely that knowledge of the allocated intervention would influence outcomes 1 months after the end of intervention.							
5. Bias in the selection of the reported results	Low	The authors changed their primary analysis for the disability (primary) outcome, but not that for other outcomes. The reason was to comply with the journal's editorial policy, which seems likely for this journal. The planned analysis was also conducted and reported in full. This is the priortised outcome; results reported in full.	PY	N	PN				
OVERALL risk of bias	**High**								

Study ID. Sedaghati 2018	**Outcome domain.** physical function (mobility) **Assessments**. EFMH, function - mobility		**Comparison.** Alexander technique v no intervention **Design.** parallel (individually randomised)					
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**					
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Some concerns	Random allocation only mentioned in abstract. No information regarding randomisation or allocation methods in report.	PY	NI	N				
2. Bias due to deviations from the intended intervention	High	Intervention group received Alexander Technique and comparator no intervention, so participants and those delivering the intervention were aware of their assigned intervention. No reporting of participant flow, dropouts or analysis method.	Y	Y	PN	NA	NA	NI	NI
3. Bias due missing outcome data	High	No reporting of participant flow, dropouts or missing data.	NI	PN	NI	NI			
4. Bias in the measurement of the outcome	High	Participants (i.e. the outcome assessors) were aware that they had received Alexander Technique lessons or no intervention. Participants' knowledge of the intervention they received could have influenced their response. Participants were likely to have had a prior belief about the benefits of Alexander Technique compared to no treatment that were likely to influence the outcome.	N	PN	Y	PY	PY		

Study ID. **Sedaghati 2018**	**Outcome domain.** physical function (mobility) **Assessments**. EFMH, function - mobility		**Comparison.** Alexander technique v no intervention **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
5. Bias in the selection of the reported results	Some concerns	There is only one possible way in which the outcome can be measured (and at a single timepoint) Results are reported as summary statistics or with minimal analysis, and it is unlikely that these were selected from other analyses.	NI	PN	PN				
OVERALL risk of bias	**High**								

Study ID. **Stallibrass 2002**	**Outcome domain.** physical function (disability) **Assessments**. function - disability, EFMH		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
1. Bias arising from the randomisation process	Low	Sequence generated by 'randomizing statistician'. Not explicit that allocation sequence was concealed, but likely given the following: "The randomization was performed by an independent statistician and group identity of participants was concealed from the research staff who performed the data collection and analysis." Baseline characteristics, including outcome measures seem balanced.	Y	PY	N				
2. Bias due to deviations from the intended intervention	Low	"Allocation between the Alexander Technique and the no additional intervention groups was not concealed from the patients " Communication with participants was intended to ensure similar expectations irrespective of assigned treatment group “the trial was consistently presented as equally about both interventions.“ No (or minimal) loss to follow up - analyses appear to include participants as randomised.	Y	Y	PN	NA	NA	PY	NA
3. Bias due missing outcome data	Low		Y	NA	NA	NA			
4. Bias in the measurement of the outcome	Some concerns	Self-report measures and participants were aware of their treatment group. Self-report outcomes could be influenced by knowledge of the treatment group, however participants were given information about the trial interventions that "to minimize bias due to participants trying to ‘please’ the research team, the trial was consistently presented as equally about both interventions", and questionnaires were administered by post. While it is possible that outcomes were influenced by knowledge of the	N	PN	Y	PY	PN		

Study ID. **Stallibrass 2002**	**Outcome domain.** physical function (disability) **Assessments**. function - disability, EFMH		**Comparison.** Alexander technique v usual care **Design.** parallel (individually randomised)						
Domain	**Judgment**	**Explanation** (for concerns that lead to high or some concerns about RoB)	**Response to signalling questions**						
			SQ1	SQ2	SQ3	SQ4	SQ5	SQ6	SQ7
		intervention, it seems unlikey that there would be influence 3 months after the intervention.							
5. Bias in the selection of the reported results	Low	A protocol is mentioned, but unclear extent to which analysis plan was specified or followed. Results appear to be reported in full. No concerns about selective non-reporting based on results.	NI	PN	PN				
OVERALL risk of bias	**Some concerns**								

Appendix G. Differences between the protocol and the review

Changes from the protocol and methods not implemented

	Section	Planned method	Change	Details (text, rationale or both)
	A1. Objectives A1.1.3	In our protocol, we planned an overall synthesis across any condition for each outcome domain.	Not done	The plan to synthesise across conditions was a contingency for reviews that included a large number of studies examining effects diverse conditions. This was not the case for this review. As such, at the prioritisation step, the NHMRC endorsed a proposal to structure and report the summary and synthesis by population group, without reporting an overall analysis across conditions.
	A1. Objectives A1.1.3	We planned to examine the effects of Alexander technique compared to "evidence-based" treatments, in the exceptional circumstance that there were studies at low risk of bias that could be combined in a synthesis.	Not possible	Not two studies in the same population had the same active comparator.
	A3.1 Selection of studies	We had planned to pilot title and abstract screening by three reviewers.	Change in process	We piloted title and abstract screening by two reviewers.
	B1.2 Measure of treatment effects	We planned to use Cohen's guiding rules for SMDs where 0.2 represents a small effect, 0.5 a moderate effect, and 0.8 a large effect.	We used a single threshold for an important effect (0.2) and did not interpret effect size.	**Revised text (and rationale).** Because we were concerned that bias may be leading to exaggerated effect sizes, we chose not to describe the size of effect (i.e. we did not interpret effects as small, moderate or large) because this is likely to be misleading. **Implications.** This has no implications for the certainty of evidence because our a priori plan was to assess certainty in relation to whether there was an important effect or not (i.e. in relation to a threshold for an important difference of an SMD of 0.2), not our certainty in the magnitude of effect (trivial, small, moderate or large).
	B1.2 Measure of treatment effect	Where a valid and reliable minimal important difference (MID) is available for a familiar measure of relevance to the population groups in the meta-analysis, we will re-express the SMD in units of the measure and interpret the effect in relation to the MID if feasible to do so.	We did not re-express SMDs in units of a familiar measure	**Rationale**. We followed GRADE and Cochrane guidance which recommends use of SMD for interpreting continuous outcomes in the absence of well-established MIDs. In addition using SMDs provided a consistent basis for interpretation across all results.
	B1.2 Measure of treatment effect	We did not pre-specify a threshold for an important effect for the outcome risk of falls	A threshold was specified	For the rate of falls, we used a threshold of 5% (50 fewer falls per 1000 people over 1 year). The decision was informed by interpretation of effects in Cochrane Systematic reviews of falls prevention interventions (e.g. https://www.cochranelibrary.com/cdsr/doi/10.1002/14651858.CD013258.pub2/full)
	B2.4 Sensitivity analysis	Analysis to examine if the meta-analysis estimates were robust to the meta-analysis mode, assumptions made to enable	Could not be done	**Revised text.** There were too few studies to undertake these analyses.

	Section	Planned method	Change	Details (text, rationale or both)
		inclusion of results in the meta-analysis, and the impact of excluding studies at risk of bias.		
	B2.4 Sensitivity analysis	Our stated method was to undertake and report sensitivity analyses in which we excluded "trials judged to be at an overall high or unclear risk of bias."	Terminology corrected (not a change to protocol)	**"**Unclear risk of bias" is the terminology used in the original ROB tool. Updated ROB2 terminology replaces this wording with "some concerns".
	B2.5 GRADE assessments – risk of bias	As per B2.4 we did not use the term 'some concerns' when describing our approach to rating down for risk of bias	Terminology corrected (not a change to protocol)	The use of 'some concerns' is consistent with the ROB2 tool. Our approach to GRADE is consistent with that for sensitivity analyses where downgrades of -1 are considered where the majority of studies are rated as 'some concerns' or studies with the majority of weight in the analysis are rated as 'high risk of bias'. Downgrades of -2 are made where most or all studies are at high risk of bias. Decisions not to rate down in these circumstances would be warranted if sensitivity analyses showed removal of studies at risk of bias did not materially alter the effect estimate.
	B2.6 Interpretation of findings	Our endorsed protocol stated that we would report "a plain language statement interpreting the evidence for each comparison and outcome, following GRADE guidance for writing informative statements". We did not specify which option would be used for very low certainty evidence (i.e. give the direction of the effect, or limit to a statement that the 'evidence is very uncertain').	Prior to submission of the draft report, NTWC advised not to include direction of effect for very low certainty evidence.	The decision not to interpret very low certainty results was made independently by the NTWC to ensure a consistent and clear interpretation of findings across Natural Therapy Review reports.
	B2.2 Summary and synthesis when meta-analysis is not possible	For a particular comparison, if we are unable to analyse most of the effect estimates (due to incomplete reporting of effects and their variances, variability in the effect measures across the studies), we will consider alternative synthesis method.	Other synthesis methods not used. We report available data if interpretable.	**Rationale.** Where possible, we report available data and present the studies on the meta-analyses. We do not include these studies in another synthesis because the data are incompletely reported and any interpretation thereof would be inconsistent with that for other results.

Appendix H. Response to comments from the Methodological review

Methodological review (or peer review) was conducted to appraise the methodological quality and assess the appropriateness of reporting for this systematic review (including appendices).

For reporting, the methodological review assessed the systematic review against the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analysis) Checklist (2020) and where applicable, the MECIR (Methodological Expectations of Cochrane Intervention Reviews) manual to ensure the systematic review was designed and conducted in accordance with:

- NHMRC's Developing your Guideline module in NHMRC's Guidelines for Guidelines Handbook
- Cochrane Handbook for Systematic Reviews of Interventions (updated 2022)
- GRADE guidance and GRADE working group criteria for determining whether the GRADE approach was used (GRADE handbook).

Assessment included the application of criteria for considering studies for the review and synthesis, search methods, data extraction and analysis, assessment of risk of bias of studies, assessment of the certainty of evidence using GRADE, and the interpretation and summary of findings.

The systematic review (including appendices) has been updated to reflect the amendments suggested by methodological review and NHMRC's Natural Therapies Working Committee, where appropriate. In summary, updates included additional information and/ or clarification of the Plain Language Summary, Executive Summary, Results sections and Appendices, including:

- Clarifications to the definition of comparator interventions, especially usual care.
- GRADE judgements clarified and confirmed where appropriate.
- Clarifications to the PRISMA diagram.
- Rewording in various parts of the report for clarity and to align with other reports.
- Requests to change the implications for practice/research to standardise across natural therapies.

A detailed record of responses to all comments indicating changes that were made was provided to NHMRC together with the amended Report and Appendices documents.

Appendix I. Abbreviations

Below is a list of abbreviations used in the report. Abbreviations for outcome measures are in a table following the list.

AMED: Allied and Complementary Medicine Database
AUSTAT: Australian Society of Teachers of the Alexander Technique
CAM: complementary and alternative medicine
CENTRAL: Cochrane Central Register of Controlled Trials
CINAHL: Cumulative Index of Nursing and Allied Health Literature
CI: confidence interval
CM: Complementary Medicine
COMET: Core Outcome Measures in Effectiveness Trials
CTM: connective tissue massage
DEFF: design effect
EFMH: Emotional functioning and mental health
EUROPE PMC: Europe PubMed Central
GRADE: Grading of Recommendations, Assessment, Development and Evaluation
Grp. Group
HR-QoL: health-related quality of life
ICC: intra-cluster correlation
ICD-11: International Classification of Diseases 11th Revision
ICTRP: International Clinical Trials Registry Platform
MA: Meta-analysis
MeSH: Medical Subject Headings
MID: minimal important difference
NR: not reported
NHMRC: National Health and Medical Research Council
NRSI: non-randomised study of interventions
NTREAP: Natural Therapies Review Expert Advisory Panel
NTWC: Natural Therapies Working Committee
PICO: population, intervention, comparator, outcome
PRACI: Practitioner Research and Collaboration Initiative
PRISMA: Preferred Reporting Items for Systematic review and Meta-Analyses
PRISMA-P: Preferred Reporting Items for Systematic review and Meta-Analyses Protocols
PROSPERO: International prospective register of systematic reviews
RCT: randomised controlled trial
REML: restricted maximum likelihood estimator
ROB: risk of bias
RR: risk ratios
SD: standard deviations
STAT: Society of Teachers of the Alexander Technique
SMD: standardised mean difference
TIDieR: Template for Intervention Description and Replication

TGA: Therapeutic Goods Administration

UK: United Kingdom

Abbreviations for measures reported in this review

Abbreviation	Measure
BDI	Beck Depression Inventory
EQ-5D	European Quality of Life with 5 Dimensions
FES-I	Falls Efficacy Scale - International
FOG	Freeze of Gait Questionnaire
GCPS	Graded Chronic Pain Scale
GDS-5	Geriatric Depression Scale - 5-item
IVI	Impact of Vision Impairment Profile
NPQ	Northwick Park Neck Pain Questionnaire
NPRS	Numeric Pain Rating Scale
PSS	Perceived Stress Scale
RMDQ	Roland-Morris Disability Questionnaires
SF-12	12-Item Short Form Health Survey
SF-36	Short Form Health Survey
SFES-I	Short Falls Efficacy Scale – International
SPDDS	Self-assessment Parkinson's Disease Disability Scale
SPPB	Short Physical Performance Battery
VAS	Visual Analogue Scale