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Cognitive-behavioural treatment for subacute and chronic neck pain (Protocol)

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[Intervention Protocol]

Cognitive-behavioural treatment for subacute and chronic neck pain

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ABSTRACT

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

To assess the effects of CBT on pain among individuals with subacute and chronic NP.

BACKGROUND

Neck pain (NP) is frequently experienced by people of all ages and both genders (Hogg-Johnson 2008). One-year prevalence ranges from 12.1% to 71.5% in the general population, and from 27.1% to 47.8% among the employed. For chronic NP, one-year prevalence ranges from 1.7% to 11.5% in the general population and chronic NP is responsible for most of the social and economic costs of this condition (Cotè 2008).

Although research on non-surgical treatments for NP is progressing (e.g. reassurance, education, promotion of a timely return to normal activities, appropriate use of painkillers and supervised exercises (Hoving 2001, Binder 2006; Hurwitz 2008)), there remains uncertainty about the efficacy of cognitive-behavioural treatment (CBT) for this population. Addressing cognitive and behavioural factors might reduce the clinical burden and the costs of NP in society.

Description of the condition

NP is defined as pain, muscle tension, or stiffness localized below the superior nuchal line and above the spine of the scapula line from the back, and below the superior nuchal line and the external occipital protuberance line and above the superior border of the clavicle and the suprasternal notch from the side (Guzman 2008).

NP may originate from many structures in the cervical region, including the spine or soft tissues, and its aetiology is multifactorial (Binder 2007; Croft 2001). Factors that contribute to its development include age, gender, a history of NP, the occurrence of other musculoskeletal problems (e.g. low back pain), poor posture, repetitive strain, poor self-rated health, and social and psychological factors (Binder 2007; Croft 2001). Also, prognosis appears to be influenced by several factors such as age, prior NP episodes and conditions of poor psychological health (Carroll 2008).

Research conducted over the past decade links persistent NP to poor psychological factors, including cognitive distress, anxiety and depressed mood (Linton 2000). These psychological factors may play a role in the chronicity of symptoms and may contribute to a downward spiral of increasing avoidance, disability and pain (Ariens 2001; Foster 2003).

Description of the intervention

CBT is a psychological management strategy that can be used in subacute and chronic NP, alone or in conjunction with other therapeutic modalities (e.g. exercise, physical modalities). Cognitive-behavioural treatment encompasses a wide set of interventions conducted by health professionals (e.g. psychologists, medical doctors, physiotherapists, occupational therapists, teams devoted to the management of chronic pain and rehabilitative teams) that include cognitive reconditioning (e.g. cognitive restructuring, imagery, attention diversion, relaxation techniques) and behavioural modifications of specific activities (e.g. operant treatment, pacing, graded exposure approaches) to modify and/or reduce the impact of pain and physical and psychosocial disability and to overcome dangerous barriers to physical and psychosocial recovery (Turk 1984; Vlaeyen 2000; Pincus 2002; Butler 2006; Morley 2011). A main assumption of these interventions is that pain and pain disability are influenced not only by somatic pathology, but also by psychological and social factors (e.g. patients' attitudes and beliefs, psychological distress, illness behaviours). Consequently, the treatment of persistent pain is primarily focused not on removing an underlying organic pathology, but on the reduction of disability through modification of environmental contingencies and cognitive processes (Main 2008).

Little evidence is available to establish whether different treatment methods have different outcomes on subgroups of patients with different characteristics, but it has been suggested that treatment efficacy may be improved by matching treatments to patient characteristics (Vlaeyen 2005).

How the intervention might work

Under the supervision of psychologists or health professionals specifically trained in CBT, the intervention works by means of modifying maladaptive and dysfunctional thoughts (e.g. catastrophising, fear of movement) and improving mood (e.g. anxiety and depression), leading to gradually changed maladapted cognitions and illness behaviours. Patients are progressively educated to view their pain and the related disability as something that can be self-managed rather than as a serious disease that requires ongoing intervention. Individual information processing of internal and external stimuli is central to cognitive-behavioural approaches, so that cognitions may change behaviours by their direct influence on emotional and physiological responses (Vlaeyen 2005).

Cognitive relearning is based on accepting pain, developing awareness of the problem and seeking a means of reacting to frightening thoughts and mood alterations. Participants are assisted in transferring attention from erratic thoughts and fears to increasing the level of activity by means of pacing and graded exposure to situations they had previously avoided. Acquisition or re-acquisition of adaptive coping strategies is strongly encouraged and promoted through communication between the health professional and the patient, and the definition of realistic and meaningful goals is provided (Turk 1984; Vlaeyen 2000; Pincus 2002; Butler 2006; Morley 2011). As functional outcomes may rely in part on patient self-management and active participation in the recovery process, the identification of cognitive and behavioural factors amenable to change and of treatment strategies favouring these changes is of considerable interest (Pincus 2006; Hazard 2012).

Why it is important to do this review

CBT is commonly used in the management of persistent low-back pain to reduce disability through modification of cognitive processes and maladaptive pain behaviours (Henschke 2010). However, it is still debated whether treating cognitive and behavioural factors in patients with subacute and chronic NP can actually lead to clinically meaningful changes in disability, dysfunctional thoughts, pain and quality of life.

This systematic review is particularly topical at the present time, as growing attention is devoted to cognitive-behavioural interventions for spinal disorders, including subacute and chronic NP. The main aim of conservative interventions for subacute and chronic NP not only should be targeted at treating “pain” or “physical dysfunction” but should also attempt to

modify maladaptive cognitions and illness behaviours, which are dangerous barriers to recovery.

OBJECTIVES

To assess the effects of CBT on pain among individuals with subacute and chronic NP.

METHODS

Criteria for considering studies for this review

Types of studies

Only randomised controlled trials (RCTs) will be included.

Types of participants

RCTs will be included if they examine adult participants (male and female) with a clinical diagnosis of subacute (i.e. a documented history of pain lasting for at least one month and not longer than three months) or chronic NP (i.e. a documented history of pain lasting for at least three months), irrespective of the presence of radiculopathy or whiplash injury.

If an RCT recruits participants with both subacute and chronic NP, it will be considered eligible only if data for participants with subacute and chronic NP are presented separately.

Types of interventions

RCTs will be included if they analyse one or more types of CBT for subacute and chronic NP. CBT encompasses a wide set of interventions, including cognitive reconditioning and behavioural modifications of specific activities to modify and/or reduce the impact of pain and physical and psychosocial disability (Turk 1984; Vlaeyen 2000; Pincus 2002; Butler 2006; Morley 2011). Only trials that specify the use of treatment based on cognitive-behavioural principles will be considered eligible.

The following comparisons will be investigated.

- CBT versus placebo, no treatment, or waiting list controls.
- Comparisons between different types of CBT (i.e. cognitive, operant, and respondent treatments).
- CBT versus other types of treatment.
- CBT in addition to another intervention (e.g. physiotherapy) versus the other intervention alone.

We expect that high variability in the type of CBT provided will be noted (i.e., cognitive, respondent or operant treatments and varying modalities of administration), and we anticipate uncertainty about what was actually done as practical intervention. Doubts about the types and treatment characteristics of CBT will be resolved through discussion or by contacting the authors of the study for additional information or finding a process paper associated with the study that provides further information.

Types of outcome measures

To be considered eligible for inclusion in this review, trials must report on at least one of the outcomes described in the following sections. Outcomes measured closest to four weeks will be considered short-term follow-up, and outcomes measured closest to one year will be considered long-term follow-up.

Primary outcomes

The primary outcome chosen for this review is pain (expressed by means of a visual analogue scale (VAS) or a numerical rating scale (NRS) (Huskinson 1974)).

We reasoned that pain is a participant-centred outcome that has better responsiveness, particularly in subacute participants, compared with disability. Furthermore, we expect trials in this field to have limited time of follow-up to allow disability improvement.

Secondary outcomes

We will also include the following secondary outcomes.

- Disability (e.g. 10-item Neck Disability Index (NDI) (Vernon 1991); 20-Item Neck Pain and Disability Scale (NPDS) (Wheeler 1999)).
- Psychological indicators, such as fear of pain, fear of movement, catastrophising, coping strategies, anxiety, depression (e.g. Tampa Scale for Kinesiophobia (Kori 1990); Pain Catastrophizing Scale (Sullivan 1995)).
- Global improvement or perceived recovery (overall improvement, proportion of participants recovered, subjective improvement of symptoms).
- General life status (e.g. assessed using the Short-Form Health Survey Questionnaire (SF-36) (Ware 1992)).
- Return to work/absenteeism (e.g. estimated trough and the proportion of participants returned to work, the number of days of sick leave).
- Satisfaction with treatment (e.g. Global Perceived Effect (GPE) (Kamper 2010)).
- Adverse events.
- Reduction in frequency or number of medications used.

Search methods for identification of studies

Electronic searches

We will search the following databases, from the first record to the present:

Cochrane Central Register of Controlled Trials (CENTRAL), Cochrane Back Review Group Trials Register (*The Cochrane Library*), PubMed, MEDLINE, EMBASE, SCOPUS, CINAHL, Web of Science, PsycINFO.

We will use the search strategy recommended by the Cochrane Back Review Group (Furlan 2009). The validated Cochrane search filter for clinical trials will be combined with the search terms "neck pain" and "CBT". No language or date restrictions will be applied to any of the searches.

The search strategies are reported in Appendix 1. These strategies will be adapted for the other databases.

Searching other resources

We will screen the reference lists of all included studies and systematic reviews pertinent to this topic.

Data collection and analysis

Selection of studies

Two review authors will independently select the citations identified in the literature search on the basis of title and abstract,

discarding any that do not meet the inclusion criteria. All potentially relevant articles will be retrieved for an assessment of the full text. The assessment of eligibility will be conducted independently by two review authors. If any doubt arises that a study meets the inclusion criteria, a consensus meeting will be held to resolve disagreements concerning the inclusion of RCTs, and another review author (LM) will be consulted if disagreements persist. We will document excluded studies in the 'Characteristics of excluded studies' table and will provide a reason for exclusion for each. Review authors who are authors of trials will be excluded from eligibility or risk of bias decisions about their own studies.

Data extraction and management

Review authors will use a customised data extraction form, which will be piloted before use. Two review authors will independently document the following information.

- Participants: patient population source and setting, number of participants, age, gender, baseline functional status or level of impairment.
- Methods: inclusion criteria, time since NP; types, symptoms and characteristics of pain. We will document the method of diagnosing NP.
- Interventions: description of interventions given to each treatment group, including duration, type, frequency and cointervention. If reported, we will document the background of the person providing the intervention (e.g. psychologist, medical doctor, occupational therapist, physiotherapist, physiotherapy/occupational therapy assistant, family). We will note any important confounding variables. If more than two intervention groups are included in the study, we will note the method of including these groups in any subsequent analysis. The two review authors will resolve any data extraction discrepancies through discussion. If disagreement persists, a third review author will resolve the disagreement.
- Outcomes: We will document primary and secondary outcomes relevant to this review in the light of the following domains.
  - Cognitive-behavioural outcomes (e.g. catastrophising, fear of pain/movement, mood disorders).
  - NP-specific functional status.
  - Generic functional status.
  - Pain intensity.
  - Quality of life.
  - Return to work or resumption of previous level of participation.
  - Adverse events.

If a study has used different methods of measuring the same outcome, we will note the outcome to be used for any subsequent analysis.

Measures of effect and estimates of variability will be extracted in the form of follow-up (postintervention) measurements or change scores from baseline in all intervention and control groups. Where possible, follow-up measures will be entered into the meta-analyses.

We will contact authors of published trials to clarify or provide additional information (e.g. clarification about the type of intervention), if needed.

The clinical relevance of each included trial will be assessed by two review authors. A list of five questions has been recommended to facilitate decisions about the applicability of the results to other populations (Furlan 2009; Malmivaara 2006) (Appendix 2). A clinically important treatment effect (i.e. the smallest change in score of the construct to be measured that participants perceive to be important) for our primary outcome (pain) will be achieved if improvement of at least 2.5 points is seen on a 0 to 10 VAS/NRS scale; a 25% relative improvement will be taken into account as a clinically important treatment effect for all secondary outcomes. Data on adverse events will be collected, including types, rates, severity and duration of harmful events.

Assessment of risk of bias in included studies

Two review authors will independently assess the risk of bias of each included RCT using the 12 criteria recommended by the Cochrane Back Review Group (Furlan 2009), which are an expansion of the criteria described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011), with the only exception of blinding of participants, which is impractical in studies focused on this intervention, and they will be considered at high risk of bias.

For each study, each criterion will be assessed as "low risk", "high risk" or "unclear" and will be reported in the 'Risk of bias' table. Studies with a low risk of bias are defined as meeting six or more criteria in the absence of other obvious serious methodological weakness, whereas studies satisfying fewer than six criteria or with serious weakness will be considered as having a high risk of bias. We will consider the weakness based on recommendations made in the *Cochrane Handbook for Systematic Reviews of Interventions*, including (1) a dropout rate greater than 50% at the follow-up measurement period of interest; (2) clinically relevant baseline differences for one or more primary outcomes, indicating unsuccessful randomisation; or (3) unacceptable adherence to the CBT program (defined as < 50% adherence in supervised programs). Risk of bias will not be used to select trials for inclusion. The criteria and the instructions for performing these assessments are provided in Appendix 3.

The articles will not be blinded for authors, institution and journal because the review authors who performed the risk of bias assessments are familiar with the literature.

We will produce a 'Risk of bias' table, graph and summary figure to illustrate potential biases within each of the included studies.

Measures of treatment effect

We will consider separately the effects of CBT for populations with subacute and chronic NP.

We will analyse the data using Review Manager 5. We will assess the treatment effects for dichotomized outcomes using the risk ratio (RR), and for continuous outcomes we will use the mean difference (MD) or the standardised mean difference (SMD) when the outcome is measured using different instruments, along with 95% confidence intervals. For continuous outcomes, a negative effect size will indicate that CBT is more beneficial than the comparison therapy, meaning that participants have better pain relief and show better improvement in functional status if we use final scores. For dichotomous outcomes (e.g. recovery), we will calculate an RR. An RR below 1 will indicate that CBT results in

greater improvement than the comparison therapy (e.g. impact of pain, physical and psychosocial disability reduced).

Unit of analysis issues

We anticipate that most trials will randomly assign at the participant level. However, if we identify a cluster RCT, we will include it, and when possible, we will extract effect measures and standard errors from an analysis that takes clustering into account. If this is not possible, we will extract the number of clusters and estimate the intracluster correlation coefficient to inform a reliable analysis. When this is not possible, we will disregard the clustering if it will make a modest contribution to the combined analysis and will investigate the effect of this in a sensitivity analysis.

Dealing with missing data

For included studies, we will extract levels of and reasons for attrition. Missing data will be treated according to whether data are 'missing at random' or 'not missing at random'. In relation to the former, we will analyse available data and ignore missing data. For studies that report a mean difference but no standard deviation (SD) or other statistic that can be used to compute the SD via appropriate methods, as outlined in Higgins 2011, we will use imputation (Furlan 2009). For each outcome, we will impute missing SDs as the pooled SD from all other trials in the same meta-analysis by treatment group. This is a safe method of analysis, provided that most studies in a meta-analysis do not have missing SDs. If the proportion of trials missing parameter variability data for a particular outcome is high (> 20%), or if data are not missed at random, imputation methods will not be appropriate, and we will conduct analyses using only available data (i.e. we will not impute missing data), and implications will be discussed in the text.

Assessment of heterogeneity

Between-trial statistical heterogeneity will be assessed using the I^2 statistic and the Chi^2 test. For the meta-analyses, we will use a fixed-effect model if trials are sufficiently homogeneous (i.e. $I^2 < 25\%$) and a random-effects model if trials present moderate levels of heterogeneity (i.e. $I^2 > 25\%$ but $< 75\%$). If considerable between-group statistical heterogeneity is detected (i.e. $I^2 > 75\%$), we will not perform a meta-analysis. Clinical heterogeneity among studies will be explored in a subgroup analysis, as described in the following sections.

Assessment of reporting biases

We will use funnel plots to explore the likelihood of reporting biases when at least 10 studies are included in the meta-analysis and studies are not of similar size. First, we will assess funnel plot asymmetry visually, integrating visual inspection with the use of formal tests for funnel plot asymmetry. For continuous outcomes, we will use the test proposed by Egger 1997, and for dichotomous outcomes, we will use the test proposed by Harbord 2006. If asymmetry is detected in any of these tests or is suggested by visual assessment, we will discuss possible explanations (such as publication bias, poor methodological quality, true heterogeneity, artefact or chance) on the basis of available information (Higgins 2011) and will perform sensitivity analyses to consider implications of the review findings. Funnel plots will be interpreted cautiously as they may be misleading. We will also check for inconsistencies between the information presented in clinical trial registries and that provided in published reports of trials. Review authors who are authors of trials will be excluded from decisions about their own studies.

Data synthesis

The results from individual trials will be combined if possible through a meta-analysis. The main analysis will be performed irrespective of the presence/absence of participants with cervical radiculopathy or whiplash injury. This pooling of the data (if applicable) will be dependent on the level of heterogeneity of retrieved studies. Results will be combined in a meta-analysis using a random-effects model if $I^2 < 50\%$. If substantial heterogeneity is present, the results will not be combined but will be presented as a narrative synthesis.

Regardless of whether available homogeneous data are sufficient to allow review authors to quantitatively summarise the data, we will assess the overall quality of the evidence for each outcome. To accomplish this, we will use the GRADE approach, as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) and adapted in the updated Cochrane Back Review Group method guidelines (Furlan 2009). The quality of the evidence on a specific outcome is based on the performance of studies against five factors: study design and limitations, consistency of results, directness (generalisability), precision (sufficient data) and reporting of results across all studies that measure that particular outcome. The quality starts at *high* when high-quality RCTs provide results for the outcome and is reduced by one level for each of the factors not met.

High-quality evidence: Consistent findings have been noted among at least 75% of RCTs with no limitations on study design; with consistent, direct and precise data; and with no known or suspected publication biases. Further research is unlikely to change the estimate or our confidence in the results.

Moderate-quality evidence: One of the domains is not met. Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.

Low-quality evidence: Two of the domains are not met. 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-quality evidence: Three of the domains are not met. We are very uncertain about the results.

No evidence: No RCTs were identified that addressed this outcome.

Subgroup analysis and investigation of heterogeneity

Subgroup analyses will explore heterogeneity due to study-level variables, such as population source and characteristics, and group-level variables such as CBT characteristics and type.

We will assess treatment effect differences according to gender and the particular type of CBT provided (program design (individually designed, group-based designed); delivery type (in-hospital treatment, home treatment, group supervision, individual supervision, psychologist based, rehabilitative teams based); dose/intensity; inclusion of additional interventions; time of outcome assessment (short-term vs end of follow-up)) and specific types of CBT (e.g. cognitive restructuring, imagery, attention diversion,

relaxation techniques, operant treatment, pacing, graded exposure approaches). Finally in a subgroup analysis, we will explore the possible interaction between treatment effect and the presence/absence of cervical radiculopathy or whiplash injury. Studies (or subgroups of participants within studies if data are stratified separately from those of participants with and without radiculopathy or whiplash injury) will be divided into subgroups (e.g. with and without radiculopathy) and the effects of the covariates analysed. Studies mixing participants with and without the strata of interest will be excluded.

Subgroup analyses will be carried out if ten or more studies are retrieved in the data collection process, as it is unlikely that the investigation of heterogeneity will produce useful findings unless a substantial number of studies are identified (Higgins 2011). However, given that we expect to retrieve only a small number of studies, and given the potential value of identifying factors that differentiate between effective and ineffective CBT in terms of improvement in participant outcomes, we will try to offer at least a tentative view, with appropriate caveats, of the two characteristics that are most likely to affect success. These characteristics are "type of CBT" and "presence/absence of radiculopathy", which have been selected by the review authors through a consensus approach, with agreement on the two factors judged most important and feasible to extract from published reports.

Sensitivity analysis

Studies with substantial missing data (> 20% of treated participants excluded from the final analysis) will be excluded in a sensitivity analyses to allow investigation of any bias they may confer on the results.

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APPENDICES

Appendix 1. MEDLINE search strategy

1. randomized controlled trial. pt.

2. controlled clinical trial. pt.

3. randomized. ti, ab.

4. placebo. ti, ab.

5. drug therapy. ti, ab.

6. randomly. ti, ab.

7. trial. ti, ab.

8. groups. ti, ab.

9. or/ 1-7

10. ((animals) AND humans AND animals) (all fields)

11. 9 not 10

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12. Neck Pain/

13. neck pain. ti, ab.

14. Cervico Brachial Neuralgia/

15. cervico brachial neuralgia. ti, ab.

16. Headache/

17. headache. ti, ab.

18.Cervicogenic headache.mp.

19. Neckache/

20. neckache. ti, ab.

21. Cervicalgia/

22. cervicalgia. ti, ab.

23. Spondylosis OR Spondylolysis OR Spondylolisthesis/

24. spondylosis OR spondylolysis OR spondylolisthesis. ti, ab.

25. spinal osteophytosis/

26. intervertebral disk degeneration/

27. intervertebral disk displacement/

28. “ossification of posterior longitudinal ligament”/

29. whiplash/

30. whiplash. ti, ab.

31. Cervical Pain/

32. cervical pain. ti, ab.

33. Cervicodynia/

34. cervicodynia. ti, ab.

35. Brachialgia/

36. brachial plexus neuritis

37. rachialgia. ti, ab.

38. radiculopathy/

39. poliradiculopathy/

40. Neck Injur*/

41. neck injur. ti, ab.

42. Torticollis/

43. Cervicobrachial Neuralgia/

44. cervicobrachial neuralgia. ti, ab.

45. exp.arthritis

46. Cervical riab syndrome/

47. Exp. Myofascial pain syndrome/

48. Fibromialgia/

49. or/ 12-48

50. Behavior Therapy/

51. behavior therapy. ti, ab.

52. Conditioning, Operant/

53. operant conditioning. ti, ab.

54. respondent treatment. ti, ab.

55. behavioral therapy. ti, ab.

56. behavioural therapy. ti, ab.

57. cognitive therapy. ti, ab.

58. cognitive treatment. ti, ab.

59. behavior treatment. ti, ab.

60. relaxation. ti, ab. or Relaxation/

61. graded activity. ti, ab.

62. Reinforcement (Psychology)/

63. psychotherapy, rational/emotive

64. reality therapy

65. CBASP.mp.

66.mindfulness.mp

67. functional analytic psychotherapy

68. counseling

69. biofeedback

70. metacognitive therapy

71. or/ 50-68

72. 11 and 49 and 71

Appendix 2. Questions for clinical relevance

1. Are the participants described in detail so that you can decide whether they are comparable with those that you see in your practice?

2. Are the interventions and treatment settings described well enough so that you can provide the same for your patients?

3. Were all clinically relevant outcomes measured and reported?

4. Is the size of the effect clinically important?

5. Are the likely treatment benefits worth the potential harms?

Appendix 3. Criteria for assessing risk of bias for internal validity

Random sequence generation (selection bias)

Selection bias (biased allocation to interventions) due to inadequate generation of a randomised sequence

Risk of selection bias is low if the investigators describe a random component in the sequence generation process, such as referring to a random number table, using a computer random number generator, coin tossing, shuffling cards or envelopes, throwing dice, drawing lots, minimising (minimisation may be implemented without a random element, and this is considered equivalent to being random).

Risk of selection bias is high if the investigators describe a non-random component in the sequence generation process, such as sequence generated by odd or even date of birth, date (or day) of admission, hospital or clinic record number; or allocation by judgement of the clinician, preference of the participant, results of a laboratory test or a series of tests or availability of the intervention.

Allocation concealment (selection bias)

Selection bias (biased allocation to interventions) due to inadequate concealment of allocations before assignment

Risk of selection bias is low if participants and investigators enrolling participants could not foresee assignment because one of the following, or an equivalent method, was used to conceal allocation: central allocation (including telephone, Web-based and pharmacy-controlled randomisation); sequentially numbered drug containers of identical appearance or sequentially numbered, opaque, sealed envelopes.

Risk of bias is high if participants or investigators enrolling participants could possibly foresee assignments and thus introduce selection bias, such as allocation based on using an open random allocation schedule (e.g. a list of random numbers); assignment envelopes used without appropriate safeguards (e.g. if envelopes were unsealed or non-opaque or not sequentially numbered); alternation or rotation; date of birth; case record number or other explicitly unconcealed procedures.

Blinding of participants

Performance bias due to knowledge of the allocated interventions by participants during the study

Risk of performance bias is low if blinding of participants was ensured and it was unlikely that the blinding could have been broken; or if no blinding or incomplete blinding was provided, but the review authors judge that the outcome is not likely to be influenced by lack of blinding.

Blinding of personnel/ care providers (performance bias)

Performance bias due to knowledge of the allocated interventions by personnel/care providers during the study

Risk of performance bias is low if blinding of personnel was ensured and it was unlikely that the blinding could have been broken; or if no blinding or incomplete blinding was provided, but the review authors judge that the outcome is not likely to be influenced by lack of blinding.

Blinding of outcome assessor (detection bias)

Detection bias due to knowledge of the allocated interventions by outcome assessors

Risk of detection bias is low if blinding of the outcome assessment was ensured and it was unlikely that the blinding could have been broken; or if no blinding or incomplete blinding was provided, but the review authors judge that the outcome is not likely to be influenced by lack of blinding, or:

- for participant-reported outcomes in which the participant was the outcome assessor (e.g. pain, disability): Risk of bias for outcome assessors is low if risk of bias for participant blinding is low (Boutron 2005);
- for outcome criteria that are clinical or therapeutic events that will be determined by the interaction between participants and care providers (e.g. cointerventions, length of hospitalisation, treatment failure), in which the care provider is the outcome assessor: Risk of bias for outcome assessors is low if risk of bias for care providers is low (Boutron 2005); and
- for outcome criteria that are assessed from data from medical forms: Risk of bias is low if the treatment or adverse effects of the treatment could not be noticed in the extracted data (Boutron 2005).

Incomplete outcome data (attrition bias)

Attrition bias due to amount, nature or handling of incomplete outcome data

Risk of attrition bias is low if no outcome data are missing; reasons for missing outcome data were unlikely to be related to the true outcome (for survival data, censoring unlikely to be introducing bias); missing outcome data were balanced in numbers, with similar reasons for missing data across groups; for dichotomous outcome data, the proportion of missing outcomes compared with the observed event risk was not enough to have a clinically relevant impact on the intervention effect estimate; for continuous outcome data, the plausible effect size (difference in means or standardised difference in means) among missing outcomes was not enough to have a clinically relevant

impact on observed effect size, or missing data were imputed using appropriate methods (if dropouts are very large, imputation using even "acceptable" methods may still suggest a high risk of bias) (van Tulder 2003). The percentage of withdrawals and dropouts should not exceed 20% for short-term follow-up and 30% for long-term follow-up and should not lead to substantial bias (these percentages are commonly used but arbitrary and are not supported by literature) (van Tulder 2003).

Selective Reporting (reporting bias)

Reporting bias due to selective outcome reporting

Risk of reporting bias is low if the study protocol is available and all of the study's prespecified (primary and secondary) outcomes that are of interest in the review have been reported in the prespecified way, or if the study protocol is not available, but it is clear that the published reports include all expected outcomes, including those that were prespecified (convincing text of this nature may be uncommon).

Risk of reporting bias is high if not all of the study's prespecified primary outcomes have been reported; one or more primary outcomes are reported using measurements, analysis methods or subsets of the data (e.g. subscales) that were not prespecified; one or more reported primary outcomes were not prespecified (unless clear justification for their reporting is provided, such as an unexpected adverse effect); one or more outcomes of interest in the review are reported incompletely so that they cannot be entered into a meta-analysis or the study report fails to include results for a key outcome that would be expected to have been reported for such a study.

Group similarity at baseline (selection bias)

Bias due to dissimilarity at baseline for the most important prognostic indicators

Risk of bias is low if groups are similar at baseline for demographic factors, value of main outcome measure(s) and important prognostic factors (examples in the field of back and neck pain include duration and severity of complaints, vocational status and percentage of participants with neurological symptoms) (van Tulder 2003).

Cointerventions (performance bias)

Bias because cointerventions were different across groups

Risk of bias is low if no cointerventions were provided, or if cointerventions were similar between index and control groups (van Tulder 2003).

Compliance (performance bias)

Bias due to inappropriate compliance with interventions across groups

Risk of bias is low if compliance with the interventions was acceptable on the basis of reported intensity/dosage, duration, number and frequency for both index and control intervention(s). For single-session interventions (e.g. surgery), this item is irrelevant (van Tulder 2003).

Intention-to-treat-analysis

Risk of bias is low if all randomly assigned participants were reported/analysed in the group to which they were allocated by randomisation.

Timing of outcome assessments (detection bias)

Bias because important outcomes were not measured at the same time across groups

Risk of bias is low if all important outcome assessments for all intervention groups were measured at the same time (van Tulder 2003).

Other bias

Bias due to problems not covered elsewhere in the table

Risk of bias is low if the study appears to be free of other sources of bias not addressed elsewhere (e.g. study funding).

CONTRIBUTIONS OF AUTHORS

Conception, design and drafting of the protocol: Marco Monticone, Christine Cedraschi, Barbara Rocca, Simona Ferrante, Roberta Fiorentini, Silvia Eleonora Gianola, Maddalena Restelli.

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Final approval of the protocol: all authors.

DECLARATIONS OF INTEREST

None.

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NOTES

None.

INDEX TERMS

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Acute Pain [psychology] [*therapy]; Chronic Pain [psychology] [*therapy]; Neck Pain [psychology] [*therapy]; Pain Management [*methods]; Randomized Controlled Trials as Topic; Selection Bias

MeSH check words

Humans