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Workplace interventions for increasing standing or walking for decreasing musculoskeletal symptoms in sedentary workers (Protocol)

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[Intervention Protocol]

Workplace interventions for increasing standing or walking for decreasing musculoskeletal symptoms in sedentary workers

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ABSTRACT

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

To investigate the effectiveness of workplace interventions to increase standing or walking for decreasing musculoskeletal symptoms in sedentary workers.

BACKGROUND

Description of the condition

Musculoskeletal symptoms (such as pain and discomfort in various body areas including back, neck and lower and upper extremities) are a common problem, with approximately 40% of the general population reporting pain annually (Hoy 2012) and transient pain being a high risk of eventually leading to chronic symptoms (Kovacs 2005). Musculoskeletal symptoms are among the most prevalent occupational problems (Andersen 2007; Janwantanakul 2008) putting a large burden on the working population. In the top ten of causes of years lived with disability, low back and neck pain are ranked the first and fourth, respectively (GBDSC 2015) while they also impact on medical costs, work productivity, work disability and absenteeism (Bevan 2015; Buchbinder 2013; CDC 2013; Lambeek 2011).

In particular among sedentary workers, there is a high prevalence of musculoskeletal symptoms (Cho 2012) that is reported in more than 90% of office workers (Widanarko 2011). Occupational sedentary behaviour has been associated with musculoskeletal symptoms including pain in the low back (Al-Eisa 2006) and the lower extremities (Messing 2008; Reid 2010). Spinal loading associated with sustained sitting (Pope 2002), increased activation of spinal muscles in specific sitting postures (Curran 2015; Waongenngarm 2015) as well as lack of variation in movements (Srinivasan 2012) are among the suggested mechanisms explaining the occurrence of musculoskeletal symptoms during sitting. Moreover, prolonged keyboard and mouse use, high mental workload and stress are hypothesized to contribute to the occurrence of musculoskeletal symptoms among sedentary office workers (Chiu

2002; Cho 2012; Hannan 2005; Hush 2009; Huysmans 2012; Jensen 2003; Kiss 2012). Despite this evidence, the association between sedentary behaviour and the occurrence of musculoskeletal symptoms remains inconsistent (Bakker 2009; Chen 2009; da Costa 2010; Lin 2011; Waersted 2010).

Innovations in technology have resulted in a shift of the workforce into more sedentary roles (Borodulin 2007; Brownson 2005; Juneau 2010) causing a substantial increase in sedentary occupations in developed countries over the last decades (Church 2011; Kohl 2012). Recent studies of accelerometer determined sedentary time estimate that office workers spend 77% to 82% of their working time being sedentary (Parry 2013; Thorp 2012). This high amount of sedentary time at work combined with its musculoskeletal (and other) health risks underlines the importance of a better understanding of the development of musculoskeletal symptoms in sedentary workers.

Description of the intervention

As sedentary workers can spend the majority of working hours sedentary (Parry 2013; Thorp 2012), the workplace is a convenient and practical venue to target interventions to modify these behaviours. There is growing evidence to suggest that these interventions might reduce or break up sedentary behaviour (Commissaris 2016; Shrestha 2016) thereby reducing cardiometabolic risk factors (Peddie 2013; Thorp 2014a). However, the impact of these interventions on reducing musculoskeletal symptoms is not well understood. Workplace interventions that will be examined in this review are interventions that specifically aim to reduce or break up sedentary behaviour by increasing standing or walking.

Workplace interventions that reduce or break up sedentary behaviour by increasing standing or walking may fall into the following categories.

1. Interventions targeted at the physical work environment - including the provision of an activity permissive workstation such as a treadmill or sit-stand workstation or changes to the built environment.
2. Interventions targeted at the individual - including tailored walking programs in work breaks or 'incidental' walking programs and the promotion of the use of stairs during work hours, break-reminding software and individual counselling programs.
3. Interventions targeted at the organisation - such as workplace policy modifications to encourage workplace activity, for example, standing meetings and 'active/walking' emails.

Workplace interventions may also be multi-component whereby interventions employ a combination of intervention approaches.

How the intervention might work

Alternatives to sitting, such as standing and walking, may result in improvements in musculoskeletal symptoms by reducing or breaking up prolonged sitting, modifying the sustained spinal load that occurs in prolonged sitting. Breaking up periods of prolonged sitting with standing or walking can increase muscle activity (Tikkanen 2013) and also create movement and postural variation, reducing the risk of static muscle overload and increasing blood circulation (Srinivasan 2012). Interventions that promote the graded introduction of standing and walking may therefore improve the general musculoskeletal health of workers. This is supported by findings from recent systematic reviews on laboratory studies showing that interventions targeted at breaking up sitting, in particular those involving sit-stand workstations, were able to reduce musculoskeletal discomfort (Healy 2012; Karakolis 2014; Thorp 2014).

However, alternatives to sitting (e.g. standing and walking) have also been associated with musculoskeletal symptoms. Occupational standing has been linked with musculoskeletal symptoms, including pain in the low back (Andersen 2007; Tissot 2009) and the lower extremities (Messing 2008; Reid 2010). Associations between musculoskeletal pain and non-neutral (e.g. sway and lordotic) lumbar postures during standing have been reported (O'Sullivan 2011), with the proposed mechanism being altered patterns of loading on the spine (van Deursen 2005). Other authors have reported increased patterns of trunk muscle activity linked to musculoskeletal symptoms in sustained standing (Gregory 2008; Nelson-Wong 2010). Other potential mechanisms include: muscle fatigue (Balasubramanian 2009), and swelling of the lower limbs due to blood pooling (Chester 2002). There is, however, a lack of evidence conclusively supporting the above hypotheses. Associations between occupational walking and the occurrence of musculoskeletal symptoms (including leg pain) have been reported (Engels 1996), but evidence is inconclusive (Roffey 2010). However, there is no clear evidence about thresholds for prolonged standing and walking and associated adverse effects.

Therefore, although the reduction of occupational sitting may result in the improvement of some musculoskeletal symptoms, replacing it with standing or walking may cause alternate problems. For example, in a study among bank tellers who either just sat, just stood or alternated sitting and standing every 30 minutes, it was shown that workers had more discomfort in the upper limbs whilst sitting, and more discomfort in the lower limbs whilst standing (Roelofs 2002). This is highlighted in a review looking at the effects of activity-permissive workstations among office workers (i.e. sit-stand workstations, but also under-desk cycling and treadmill workstations) which reported both beneficial and detrimental impact on musculoskeletal outcomes (Neuhaus 2014).

Given that there may be individual vulnerability to musculoskeletal discomfort in standing and sitting, the response to standing or walking interventions is likely to vary between workers (Gregory 2008). Personal factors such as gender (Hooftman 2004),

age (Viester 2013) and adiposity (Hooftman 2004; Moreira-Silva 2013; Oha 2014) are known to play an important role in the occurrence and recurrence of musculoskeletal symptoms among workers. Such factors may impact the effectiveness of these interventions.

Why it is important to do this review

Musculoskeletal disorders contribute significantly to the global burden of diseases (GBDSC 2015), and are associated with substantial economic and productivity costs within work settings (Bevan 2015; Buchbinder 2013). Sedentary workers report a high prevalence of musculoskeletal symptoms (Cho 2012; Harcombe 2009; Janwantanakul 2008) and may also be at increased risk of adverse cardiometabolic, cancer, and even mental health outcomes (Carson 2014; Chau 2014; Dunstan 2012; Parry 2013; Straker 2014; Vallance 2011). Because of these risks, there has been a rapid increase in workplace interventions to reduce sedentary behaviour, such as the introduction of activity-permissive workstations. However, it is not clear whether such workplace changes aimed at reducing sedentary behaviour will impact on musculoskeletal symptoms.

Previous reviews have focused on workplace interventions to increase physical activity (Freak-Poli 2013) or to reduce sitting (Shrestha 2016) but have not specifically explored the potential impacts of changing workplace activity on musculoskeletal symptoms. Therefore, in relation to interventions that aim to reduce workplace sedentary behaviour by increasing standing or walking, it is important to examine not only the changes to sedentary behaviour and cardiometabolic health outcomes, as considered in previous reviews, but also musculoskeletal health. The findings of this review will provide high-quality evidence to assist in the management of work-related musculoskeletal symptoms.

This is a partner to another review on similar workplace interventions for preventing, rather than decreasing, musculoskeletal symptoms in sedentary workers (Parry 2017).

OBJECTIVES

To investigate the effectiveness of workplace interventions to increase standing or walking for decreasing musculoskeletal symptoms in sedentary workers.

METHODS

Criteria for considering studies for this review

Types of studies

We will include all eligible randomised controlled trials (RCTs), quasi-RCTs (in which methods of allocating participants are not random, such as alternate allocation, allocation by date of birth or day of the week) and cluster randomised trials (randomisation of a group of people such as a work group or workplace rather than randomisation of individual people). In some workplace interventions, the implementation of interventions is difficult to apply to an individual, so interventions operate on a group level (Ijaz 2014). In this situation, where randomisation of only a few units is available, we will also include controlled before-and-after studies (CBAs) that use a concurrent control group for the intervention. We will include studies reported as full-text, those published as abstract only, and unpublished data.

Types of participants

We will include studies conducted with adult workers aged 18 or older working in sedentary occupations (workers sedentary for more than 50% of the working day), such as seated office workers and laboratory technicians. We will exclude sedentary workers where it may not be possible to modify workplace posture, such as transport workers. Studies that do not report the proportion of sedentary time but describe workers as 'sedentary workers' will be included. Where studies have workers from different occupations we will only include results from participants that are identified as 'sedentary workers' or have reported sedentary time of more than 50%. We will exclude studies which specifically focus on participants with the following comorbidities or characteristics:

- inflammatory systematic diseases such as rheumatoid arthritis; and
- diseases of the central nervous system such stroke and multiple sclerosis.

Sedentary workers who report the presence of musculoskeletal symptoms in at least one of the following regions: cervical spine, mid-back, lower back, upper limb, hip or lower limb, will be included as participants with symptoms.

In studies that have a mixture of participants reporting and not reporting musculoskeletal symptoms, only participants with symptoms will be included in the analyses.

We will include studies conducted with participants who report pain. 'Participants with pain' thresholds are defined as:

- 'yes' on a dichotomous symptom scale;
- 'greater than 0' on a visual analogue symptoms scale out of 10;
- 'greater than 0' on a numerical rating scale out of 10;
- 'greater than 0' on the McGill Pain Questionaire;
- 'greater than 0' on the 18, 23 or 14 point versions of the Roland Morris Disability Questionnaire; and
- 'greater than 0%' for overall score on the Oswestry Disability Index.

Types of interventions

We will include trials that have evaluated the effectiveness of interventions to reduce or break up workplace sitting by encouraging standing or walking at the workplace. Eligible interventions include the following.

1. Interventions targeted at the physical work environment.
   - Provision of an activity-permissive workstation (sit/stand or treadmill).
   - Interventions that modify the built environment such as modification to office layout that encourages standing or walking.

2. Interventions targeted at the individual.
   - Behavioural modification or counselling programs that promote increased standing or walking.
   - Ergonomic interventions that promote standing or walking, such as the promotion of 'active' work breaks.
   - Workplace walking programs including 'pedometer challenges'.
   - Promotion of the use of stairs during work hours.
   - Break-reminding software.

3. Interventions targeted at the organisation.
   - Workplace policy modifications such as standing meetings and 'active/walking' emails

We will include multi-component trials that combine elements of the above interventions.

We will include trials that compare the effectiveness of workplace interventions to increase standing or walking with usual care, no intervention or to another active intervention such as specific targeted musculoskeletal interventions.

We will exclude interventions that focus on specific strengthening or stretching programs that do not promote standing or walking. For example, an exercise program that replaces sedentary time (with standing or walking) would be included as an intervention whereas a seated exercise program (seated stretching/strengthening program) would not be included.

Types of outcome measures

Primary outcomes

We will include trials that have evaluated the effectiveness of interventions on self-reported musculoskeletal symptoms by body region.

1. Musculoskeletal symptoms may be reported as pain on a scale (as listed below) or be reported as 'discomfort' or 'trouble' on similar scales.
   - Presence of musculoskeletal symptoms may be reported on a dichotomous scale (yes/no) by outcome measures such as the Nordic Musculoskeletal Questionnaire (Kuorinka 1987).
   - Intensity of musculoskeletal symptoms may be reported on a visual analogue scale (or similar), numerical rating scale, a Likert scale (Bond 1966; Harland 2015), or a McGill Pain Questionnaire (Melzack 1975).

2. Impact of pain such as disability.
   - Change in disability may be assessed by outcome measures such as the Oswestry Disability Index, Roland Morris Disability Questionnaire (Roland 2000) and Neck Disability Index (Vernon 2008).

Secondary outcomes

The following secondary outcomes will be used.

1. Work performance and productivity.
   - Level of work function, change in work productivity, work time loss assessed by outcome measures such as Work Ability Index (de Zwart 2002; van den Berg 2008).

2. Sickness absenteeism.
3. Adverse events such as venous disorders or perinatal complications.

Reporting one or more of the secondary outcomes listed here is not an inclusion criterion for the review. In addition, secondary outcomes will be used only for supporting the conclusions of the primary outcomes and not for drawing conclusions on the effectiveness of the interventions.

The primary measurement time point will be short-term (less than 6 months). We will categorise additional follow-up times as medium term (6 months to less than 12 months) and long term (12 months or greater).

Search methods for identification of studies

Electronic searches

We will conduct a systematic literature search to identify all published and unpublished trials that can be considered eligible for inclusion in this review. We will adapt the search strategy we developed for MEDLINE (see Appendix 1) for use in the other electronic databases. The literature search will identify studies in all languages. We will arrange for the translation of key sections of potentially-eligible non-English language papers or we will arrange that people who are proficient in the publications' languages fully assess them for potential inclusion in the review as necessary.

We will search the following electronic databases from inception to present:

- Cochrane Central Register of Controlled Trials (CENTRAL) (Wiley Online Library);
- MEDLINE (PubMed) (Appendix 1);
- Embase (embase.com);
- NIOSHTIC (OSH-UPDATE);

- NIOSHTIC-2 (OSH-UPDATE);
- HSELINE (OSH-UPDATE);
- CISDOC (OSH-UPDATE); and
- PEDro.

We will also conduct a search of unpublished trials in ClinicalTrials.gov (www.ClinicalTrials.gov) and the WHO trials portal (www.who.int/ictrp/en/). We will impose no restriction on language of publication.

Searching other resources

We will check reference lists of all primary studies and review articles for additional references. We will contact experts in the field to identify additional unpublished materials.

Data collection and analysis

Selection of studies

We will conduct the selection of eligible studies in two stages. First, two review authors (SP and PC) will independently screen titles and abstracts of all the potentially- relevant studies we find with our systematic search to identify studies for inclusion. The same authors will code them as 'include' (eligible or potentially eligible/unclear) or 'exclude'. At this stage we will exclude all references that clearly do not fulfil our inclusion criteria or that do fulfil our exclusion criteria. At the second stage, we will retrieve the full-text study reports/publications and two review authors (SP and PC) will independently assess the full-text and identify studies for inclusion. At this stage we will include all references that do fulfil our inclusion criteria. We will record reasons for exclusion of the ineligible studies assessed as full-texts so that we can report these in a 'Characteristics of excluded studies' table. We will resolve any disagreement through discussion or, if required, we will consult a third review author (LS). We will identify and exclude duplicates and collate multiple reports of the same study so that each study rather than each report is the unit of interest in the review. We will record the selection process in sufficient detail to complete a PRISMA study flow diagram.

Should our systematic searches identify studies conducted by authors of this review, we will avoid conflict of interest by having all decisions concerning inclusion and exclusion made by review authors who were not involved with the study.

Data extraction and management

We will use a data collection form for study characteristics and outcome data which has been piloted on at least one study in the review. Two review authors (SP and PC) will extract the following study characteristics from included studies.

1. Authors and year of publication.
2. Methods: study design, total duration of study, study location, study setting, withdrawals, and date of study.
3. Participants: N, mean age or age range, sex/gender, severity of condition, intensity of sedentary work (percentage of work day sedentary), type of sedentary work, diagnostic criteria if applicable, inclusion criteria, and exclusion criteria.
4. Interventions: description of intervention, comparison, duration, intensity, content of both intervention and control condition, and cointerventions.
5. Outcomes: description of primary and secondary outcomes specified and collected, and at which time points reported.
6. Notes: funding for trial, and notable conflicts of interest of trial authors.

Two review authors (SP and PC) will independently extract outcome data from included studies. We will note in the 'Characteristics of included studies' table if outcome data were not reported in a usable way. We will resolve disagreements by consensus or by involving a third review author (LS). One review author (SP) will transfer data into the Review Manager (RevMan 2014) file. We will double-check that data are entered correctly by comparing the data presented in the systematic review with the study reports. A second review author (PC) will spot-check study characteristics for accuracy against the trial report. Should we decide to include studies published in one or more languages in which our author team is not proficient, we will arrange for a native speaker or someone sufficiently qualified in each foreign language to fill in a data extraction form for us.

Assessment of risk of bias in included studies

Two review authors (SP and PC) will independently assess risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We will resolve any disagreements by discussion or by involving another author (LS). We will assess the risk of bias according to the following domains.

1. Random sequence generation.
2. Allocation concealment.
3. Blinding of participants and personnel.
4. Blinding of outcome assessment.
5. Incomplete outcome data.
6. Selective outcome reporting.
7. Other bias such as participation bias.

In addition, if cluster randomised trials are identified and included in the review, we will consider the following additional biases.

1. Recruitment bias.
2. Baseline imbalance.
3. Loss of clusters.
4. Incorrect analysis.
5. Comparability with individually-randomised trials.

We will grade each potential 'Risk of bias' item as high, low or

unclear, and provide a quote from the study report together with a justification for our judgment in the 'Risk of bias' table. We will summarise the 'Risk of bias' judgements across different studies for each of the domains listed. We will consider blinding separately for different key outcomes where necessary (e.g. for unblinded outcome assessment, risk of bias for work productivity may be very different than for a patient-reported pain scale). Where information on risk of bias relates to unpublished data or correspondence with a trialist, we will note this in the 'Risk of bias' table.

We consider allocation concealment, blinding of participants and outcome assessors and incomplete outcome data to be key domains. We will judge a study to have a high risk of bias when we judge one or more key domains to have a high risk of bias. Conversely, we will judge a study to have a low risk of bias when we judge low risk of bias for all key domains

For controlled before-after studies, we will use the validated instrument for appraising risk of bias of controlled before-after studies by Downs (Downs 1998). The instrument has been shown to have good reliability and internal consistency and validity. The list consists of five different subscales: reporting, external validity, bias, confounding, and power). We will only use the combined score on the two internal validity subscales (bias and confounding) to judge the risk of bias of the included controlled before-after studies. We will use an arbitrary cut-off score of 50% of the maximum attainable score of the internal validity scale to discern low from high risk of bias. We will modify the criteria for risk of bias so that they fit the 'Risk of bias' tool as implemented in RevMan (RevMan 2014) by changing them from 0 and 1 to high, low, and unclear.

We will also check for relevant and considerable baseline differences between control and intervention groups based on age and gender.

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

Assesment of bias in conducting the systematic review

We will conduct the review according to this published protocol and report any deviations from it in the 'Differences between protocol and review' section of the systematic review.

Measures of treatment effect

We will enter the outcome data for each study into the data tables in RevMan (RevMan 2014) to calculate the treatment effects. We will use odds ratio/risk ratio for dichotomous outcomes, and mean differences or standardised mean differences for continuous outcomes, or another type of data as reported by the authors of the studies. If only effect estimates and their 95% confidence intervals or standard errors are reported in studies we will enter these data into RevMan using the generic inverse variance method. We will ensure that higher scores for continuous outcomes have the same meaning for the particular outcome, explain the direction to the reader and report where the directions were reversed if this was necessary. When the results cannot be entered in either way, we will describe them in the 'Characteristics of included studies' table, or enter the data into Additional tables.

Unit of analysis issues

For studies that employ a cluster-randomised design and that report sufficient data to be included in the meta-analysis but do not make an allowance for the design effect, we will calculate the design effect based on a fairly large assumed intra-cluster correlation of 0.10. We base this assumption of 0.10 being a realistic estimate by analogy on studies about implementation research (Campbell 2001). We will follow the methods stated in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) for the calculations.

Dealing with missing data

We will contact investigators or study sponsors in order to verify key study characteristics and obtain missing numerical outcome data where possible (e.g. when a study is identified as abstract only). Where this is not possible, and the missing data are thought to introduce serious bias, we will explore the impact of including such studies in the overall assessment of results by a sensitivity analysis.

If numerical outcome data are missing, such as standard deviations or correlation coefficients and they cannot be obtained from the authors, we will calculate them from other available statistics such as P values according to the methods described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011).

Assessment of heterogeneity

We will assess the clinical homogeneity of the results of included studies based on similarity of population, intervention, outcome and follow-up. We will consider populations as similar when sedentary work is being conducted for more than 50% of working hours or if participants are described as 'sedentary workers'. Populations that report musculoskeletal symptoms in one or more body region, of any intensity, will be considered similar. We will consider interventions as similar when they target workplace sedentary behaviour by promoting standing or walking according to the category of the intervention as defined in Types of interventions. We will not consider interventions that implement exercise or education programs to target specific muscle groups such as neck/shoulder or low back exercise as similar to sedentary behaviour modification programs (as stated in Types of interventions). We will consider all outcome measures of pain or discomfort including dichotomous measures, Likert scale, visual analogue scale and standardised questionnaires such as the Nordic Musculoskeletal Ques-

tionnaire as similar. For measurement of work performance, disability and work productivity we will consider all self-reported outcomes from standardised questionnaires (e.g. Work Performance Index, Neck Disability Index) as similar. We will regard follow-up times of up to 6 months as short term, from 6 months to less than 12 months as medium term and from 12 months onwards as long-term outcomes and treat these outcomes as different.

Assessment of reporting biases

If we are able to pool more than five trials in any single meta-analysis, we will create and examine a funnel plot to explore possible small study biases.

Data synthesis

We will pool data from studies we judge to be clinically homogeneous using Review Manager 5.3 software (RevMan 2014). If more than one study provide usable data in any single comparison, we will perform meta-analysis. We will use a random-effects model when I^2 is above 40%; otherwise we will use a fixed-effect model. When I^2 is higher than 75% we will not pool results of studies in meta-analysis.

We will narratively describe skewed data reported as medians and interquartile ranges.

Where multiple trial arms are reported in a single trial, we will include only the relevant arms. If two comparisons (e.g. provision of sit-stand desk versus standard desk and behavioural modification versus standard desk) are combined in the same meta-analysis, we will halve the control group to avoid double-counting.

Minimally important differences for validated outcome measures will be considered when discussing the magnitude of the effect size. We will consider that pooled effects sizes greater than the minimally important difference to be clinical significant.

'Summary of findings' table

We will create a 'Summary of findings' table using the following outcomes.

1. Presence of musculoskeletal symptoms.
2. Intensity of musculoskeletal symptoms.
3. Duration of musculoskeletal symptoms.
4. Disability.

We will use the five GRADE considerations (study limitations, consistency of effect, imprecision, indirectness and publication bias) to assess the quality of a body of evidence as it relates to the studies that contribute data to the meta-analyses for the prespecified outcomes. We will use methods and recommendations described in section 8.5 and Chapter 12 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) using GRADEpro software. We will justify all decisions to down- or upgrade the quality of studies using footnotes.

We will also compile an additional GRADE table showing all our decisions about the quality of evidence and their justifications.

Subgroup analysis and investigation of heterogeneity

We plan to carry out the following subgroup analyses.

1. Intervention approach (workstation design, workplace built environment, workplace policy and interventions in non-productive periods (work breaks)).
2. Intervention effects on different body regions (cervical spine, mid back, lower back, upper limb/shoulder and lower limb)
3. Participant characteristics (age, gender and body mass index).
4. Participant work group characteristics (specific occupations).

We will use the following outcomes in subgroup analyses.

1. Musculoskeletal symptoms (pain/discomfort).
2. Disability.

We will use the Chi² test to test for subgroup interactions in Review Manager (RevMan 2014).

Sensitivity analysis

We will perform sensitivity analyses to determine whether our findings are affected by high risk of bias and baseline pain of low intensity. In order to perform sensitivity analysis, we will define 'high quality' as studies with appropriate random allocation and concealment and attrition bias of less than 20%. We define low intensity pain threshold as 3 out of 10 on a pain intensity scale (Moore 2013).

Reaching conclusions

We will base our conclusions only on findings from the quantitative or narrative synthesis of included studies for this review. We will avoid making recommendations for practice based on more than just the evidence, such as values and available resources. Our implications for research will suggest priorities for future research and outline what the remaining uncertainties are in the area.

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* *Indicates the major publication for the study*

APPENDICES

Appendix 1. MEDLINE search strategy

#1 work*.mp.
#2 occupation*.mp. or Occupations/
#3 vocation.mp.
#4 job.mp.
#5 office.mp.
#6 sedentary.mp.
#7 1 or 2 or 3 or 4 or 5 or 6
#8 sit.mp.
#9 sitting.mp.
#10 walk*.mp.
#11 sedentary.mp.
#12 sedentary behaviour.mp.
#13 sedentary behavior.mp.
#14 inactiv*.mp.
#15 sit-stand desk.mp.
#16 sit stand desk.mp.
#17 sit-stand workstation.mp.
#18 sit stand workstation.mp.
#19 workstations.mp.
#20 pedometer.mp.
#21 wearable device.mp.
#22 Workplace/ or workplace.mp.
#23 worksite.mp.
#24 (work* or vocation* or occupation*).tw.
#25 ("office worker*" or "sedentary worker*").tw.
#26 ("sit-stand desk" or "sit stand desk" or "sit-stand workstation" or "sit stand workstation" or "pedometer" or "wearable device").tw.
#27 8 or 9 or 10 or 11 or 12 or 13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 24 or 25 or 26
#28 7 and 27
#29 limit 28 to (clinical trial, all or randomized controlled trial)
#30 exp animals/not humans.sh.
#31 29 not 30

CONTRIBUTIONS OF AUTHORS

Conceiving the protocol: SP

Designing the protocol: SP, PC, LS, CM, PO

Coordinating the protocol: SP

Designing search strategies: SP, PC

Writing the protocol: SP, PC

Providing general advice on the protocol: LS, CM, PO

DECLARATIONS OF INTEREST

Sharon Parry: None known

Pieter Coenen: None known

Peter O'Sullivan: None known

Chris Maher: Member of Editorial Board of the Cochrane Back and Neck Group

Leon Straker: None known

NOTES

Parts of the methods section and Appendix 1 of this protocol are based on a standard template established by the Cochrane Work Review Group.