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Effectiveness of warm-up interventions on work-related musculoskeletal disorders, physical and psychosocial functions among workers: a systematic review

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ABSTRACT

Objectives The aim of this systematic review was to identify from published literature the available evidence regarding the effects of warm-up intervention implemented in the workplace on work -related musculoskeletal disorders (WMSDs) and physical and psychosocial functions.

Design Systematic review.

Data sources The following four electronic databases were searched (from inception onwards to October 2022): Cochrane Central Register of Controlled Trials (CENTRAL), PubMed (Medline), Web of Science and Physiotherapy Evidence Database (PEDro).

Eligibility criteria Randomised and non-randomised controlled studies were included in this review. Interventions should include a warm-up physical intervention in real-workplaces.

Data extraction and synthesis The primary outcomes were pain, discomfort, fatigue and physical functions. This review followed the Preferred Reporting Items for Systematic Reviews and Meta-Analyses guidelines and used the Grading of Recommendations, Assessment, Development and Evaluation evidence synthesis. To assess the risk of bias, the Cochrane ROB2 was used for randomised controlled trial (RCT) and the Risk Of Bias In Non-randomised Studies-of Interventions was used for the non-RCT studies.

Results Three studies met the inclusion criterion, one cluster RCT and two non-RCTs. There was an important heterogeneity in the included studies principally concerning population and warm-up intervention exercises. There were important risks of bias in the four selected studies, due to blinding and confounding factors. Overall certainty of evidence was very low.

Conclusion Due to the poor methodological quality of studies and conflicting results, there was no evidence supporting the use of warm-up to prevent WMSDs in the workplace. The present findings highlighted the need of good quality studies targeting the effects of warm-up intervention to prevent WMSDs.

PROSPERO registration number CRD42019137211.

STRENGTHS AND LIMITATIONS OF THIS STUDY

- ⇒ This study is the first review to critically appraise the effectiveness of warm-up intervention to prevent work-related musculoskeletal disorders in workplaces.
- ⇒ Reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses statement.
- ⇒ This study included both randomised controlled trial (RCT) and non-RCT.
- ⇒ A low number of studies and significant heterogeneity limit performing a meta-analysis of the results.

INTRODUCTION

Work-related musculoskeletal disorders (WMSDs) are painful diseases of muscles, tendons, nerves, ligaments, joints or spinal discs which can affect all body parts, particularly the neck, the upper limb or the back.[1] All over the world, WMSDs are considered as a major public health problem, with adverse consequences on quality of life and work participation.[2–6] Developing and implementing effective prevention strategies or curative interventions is more than necessary. Workplace health promotion interventions are considered to have great potential improving health and preventing WMSDs, as most people spend a major part of their adult life at work.[7–10] The workplace environment seems to be the perfect area to reach and to raise awareness of a large number of workers.[11] For these reasons, among all the interventions to prevent WMSDs, those implemented at the workplace and offering physical activity appears to be of particular interest.[12–20] In this sense, a recent review of literature by Chen *et al*[13] assessing the effects of workplace physical activity programmes to prevent WMSDs of the neck has reported a plethora of articles among office workers. At the opposite, numerous authors question the accessibility of such programmes for employees at risk of developing WMSDs (ie, low-status, low income and blue-collar workers).[7 21–23] Finally, regardless of the population and despite positive effects, numerous studies reported a low compliance rate during the implementation of workplace physical activity programme. For instance, a 40%–60% compliance is commonly observed whatever the duration of the programmes.[24–30] This last point constitutes a major limitation of such programmes and may question its sustainability. It is recognised that this result may stem from barriers such as time constraints, time of the day, duration of the training sessions and supervision.[31–35] Therefore, to be sustainable, feasible and consequently increasing this compliance rate, a balance should be found between optimal physiological recommendations and these barriers. In this sense, a promising solution easy to fit into organisational routines consisted of dividing the duration of the training sessions, ie, generally one continuous hour[36 37] into short bouts of physical activity, for example, 15/20 min repeated several times over a week.[8 36 38] Using such a design, Andersen *et al*[36] among 447 office workers have demonstrated that performing short bouts of physical activity over a 10-week workplace physical activity programme (1) reduced pain as much as longer training sessions and (2) increased compliance rate. These authors have reported that adherence among office workers was significantly higher when the training volume was divided into several weekly training sessions. Indeed, in this study, in both the 3 and the 9 weekly sessions' groups, adherence was achieved by 60%, while 1 weekly session group only achieved 49%. Daily short bouts of physical activity is a modality reminiscent of warm-up interventions implemented prior the beginning of the working days and increasingly adopted the last few years in companies to manage WMSDs (INRS 2018). In these companies, it is common to observe warm-up interventions lasting between 5 and 15 min a day as well supervised by professionals such as sport trainer or physiotherapist as trained employees.[39] These interventions are developed with the hypothesis that positive effects observed in sports on performance and injury prevention,[40–45] could be similar in a work environment. However, scientific data on the effects of warm-up interventions on WMSDs are scarce and, when available, lead to rather conflicting/inconclusive results.[46–49] Within this context, the aim of this systematic review was to evaluate the effectiveness of workplace warm-up interventions on WMSDs and physical and psychosocial functions among workers.

METHODS

The review protocol concerning the present systematic review was registered within the International Prospective Register of Systematic Reviews (PROSPERO) (registration number: CRD42019137211). The protocol review was published online in BMJ open (doi:10.1136/bmjopen-2020-039063).[50] This review is reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analysis (PRISMA) statement.

Criteria for considering studies for this review

Type of studies

Original quantitative research studies that assessed the effect of a warm-up intervention in a workplace setting aiming at preventing WMSDs or musculoskeletal pain or discomfort or fatigue of workers were included in the review. As correctly argued, randomised controlled trials (RCTs) are considered as the gold standard to assess the effectiveness of an intervention.[51] However, its implementation in occupational setting may not always be feasible and its achievement is called into question.[52–56] In that specific case, recent studies have suggested that non-RCT may maximise the body of evidence and have suggested including non-RCT in systematic-reviews.[57–59] For these reasons and as previously done in recent systematic reviews covering the scope of the present review[12 54] both randomised and non-randomised controlled studies were included. Therefore, quasi-RCTs (participants not randomly allocated), cluster randomised trials (ie, randomisation of a group of people, eg, randomisation at a company level), preference trials (patients can choose their treatment) and before-and-after study are designs where included. Period of studies publication was defined from inception onwards to October 2022. Finally, to be eligible for inclusion, studies had to be published in English in peer-reviewed scientific journals.[54 60] As only studies in English were included and may lead to reporting bias, we have also reported potentially eligible studies in other languages. The following study designs were ineligible: case reports, abstracts, editorials, conference abstracts, letters to the editor, reviews and meta-analysis. Studies were also excluded if the intervention was partially or totally implemented outside of the workplace, for example, in a clinical setting and if the intervention was implemented in combination with another intervention, for example, ergonomics or strengthening. Therefore, studies were excluded when differences can not only be attributed only to the warm-up intervention.

Types of participants

This review only included adult employees (18 years of age or older) and excluded adults with specific comorbidities or diseases (such as diabetes, arthritis, cancer, stroke) and/or special populations (pregnant, severe or rare physical disability, or cognitive disability).

Types of intervention

This review included studies which have implemented warm-up interventions in workplaces. To facilitate the comprehension of a warm-up intervention, we used the definition given by Woods *et al*,[61] that is, 'a warm-up' is a short bout of exercise realised before work and aiming to (1) improve muscle dynamic's to prevent injury and (2) to prepare the worker to realise its task.

Comparator

Inclusion criteria: Studies that compared the warm-up intervention with a no treatment control group (eg, no

Table 1 Databases search strategy terms (ti: tittle; ab: abstract)

Medline		Pedro		Web of Science		Cochrane	
1	Workplace(Mesh)	1	Work* ti,ab	1	Workplace ti,ab	1	Work* ti,ab
2	Work* ti,ab	2	Warm* ti,ab	2	Work ti,ab	2	Employ* ti,ab
3	Employ* ti,ab	3	Pain* ti,ab	3	Employee ti,ab	3	Compan* ti,ab
4	Compan* ti,ab	4	1 AND 2 AND 3	4	Company ti,ab	4	1 OR 2 OR 3 OR 4
5	1 OR 2 OR 3 OR 4			5	1 OR 2 OR 3 OR 4		
		5	Work* ti,ab			5	Warm* ti,ab
6	Warm-Up Execise(Mesh)	6	Employ* ti,ab	6	Warmup	6	Pre-exercise ti,ab
7	Pre-shift ti,ab	7	Compan* ti,ab	7	Warm-up	7	Pre-activit* ti,ab
8	Pre-exercise* ti,ab	8	5 OR 6 OR 7	8	Warm up	8	6 OR 7 OR 8
9	Pre-activit* ti,ab			9	Warming-up		
10	6 OR 7 OR 8 OR 9	9	Warm* ti,ab	10	Pre-shift ti,ab	9	Musculoskeletal ti,ab
		10	Pre-exercise* ti,ab	11	Pre-exercise ti,ab	10	Disease ti,ab
11	Musculoskeletal diseases(Mesh)	11	Pre-activit* ti,ab	12	Pre-activity ti,ab	11	WMSD* ti,ab
12	Pain(Mesh)	12	9 OR 10 OR 11	13	6 OR 7 OR 8 OR 9 OR 10 OR 11 OR 12	12	Pain ti,ab
13	Musculoskeletal Pain(Mesh)					13	(endurance or strength or flexibility) ti,ab
14	WMSD* ti,ab	13	Musculoskeletal ti,ab	14	Musculoskeletal ti,ab	14	(quality of life or job satisfaction or work ability or well-being or stress or disabilit* or health or discomfort or comfort or fatigue or injur*) ti,ab
15	Pain ti,ab	14	Disease ti,ab	15	Disease ti,ab		
16	(endurance or strength or flexibility) ti,ab	15	WMSD* ti,ab	16	WMSD* ti,ab		Combining search terms
17	(quality of life or job satisfaction or work ability or well-being or stress or disabilit* or health or discomfort or comfort or fatigue or injur*) ti,ab	16	Pain ti,ab	17	Pain ti,ab	15	5 AND 13 AND 20
18	11 OR 12 OR 13 OR 14 OR 15 OR 16 OR 17	17	13 OR 14 OR 15 OR 16	18	(endurance or strength or flexibility) ti,ab		
				19	(quality of life or job satisfaction or work ability or well-being or stress or disabilit* or health or discomfort or comfort or fatigue or injur*) ti,ab		
	Combining search terms			20	14 OR 15 OR 16 OR 17 OR 18 OR 19		
19	5 AND 10 AND 18						
					Combining search terms		
				21	5 AND 13 AND 20		

intervention or usual activity or another type of workplace physical activity) or a non-active comparator (eg, leaflets on benefits of physical activity).

Exclusion criteria: Studies with no comparison measures.

Types of outcome measures

Main outcomes

WMSDs are defined as a group of conditions or health problems affecting the locomotor apparatus. These conditions are characterised by pain, impaired function and overall fatigue.[1 9] Therefore, among primary outcomes, we included all the outcomes associated with work-related musculoskeletal issues that are (1) participant musculoskeletal pain through the use of pain scales (eg, Numeric Rating Scale (NRS) or Visual Analogue Scale (VAS)) or questionnaire (eg, McGill pain questionnaire)[62] and (2) participant discomfort or fatigue[12 62] through validated scales and (3) physical function as measured or estimated by questionnaires, performance and/or specific tests. Dichotomous data such as presence/absence of symptoms or injury rate were also considered.

Secondary outcomes

For the prevention of the consequences of WMSDs, we included as secondary outcomes, all the outcomes

Table 2 Risk of bias judgement for a specific domain in RCT (from Sterne *et al*[68])

Overall risk-of-bias judgement	Criteria
Low risk of bias	The study is judged to be at low risk of bias for all domains for this result
Some concerns	The study is judged to raise some concerns in at least one domain for this result, but not to be at high risk of bias for any domain
High risk of bias	The study is judged to be at high risk of bias in at least one domain for this result, or the study is judged to have some concerns for multiple domains in a way that substantially lowers confidence in the result

RCT, randomised controlled trial.

associated with psychosocial function such as the measure of quality of life, job satisfaction, job control or motivation at work. In this review job control was considered as an indicator of psychosocial stress at work.[63]

Information sources and search strategy

Four electronic databases—Cochrane Central Register of Controlled Trials (CENTRAL), PubMed (Medline), Web of Science and Physiotherapy Evidence Database (PEDro)—were searched systematically from inception onwards to October 2022 to identify studies satisfying the search criteria. Note that these databases have previously used in published reviews covering the scope of this review.[54 62 64 65] The proposed search strategy terms for the four databases are listed in table 1.

Additional intended information sources

To be sure not to miss relevant studies for this review, the reference list of for all eligible articles was checked. Then, a grey literature search was performed on ClinicalTrials.gov. Finally, we contacted experts in this domain to collect information on unknown or ongoing studies.

Data collection

Study selection process

All studies that met inclusion criteria passed through a data extraction and quality assessment process performed by two independent reviewers (NL and RB). A third reviewer (NV) was requested to resolve disagreement when consensus was not reached. At stage 1, NL and RB screened abstracts and titles identified from the search strategy. At stage 2, the same two reviewers screened the full-text articles for inclusion. At this stage, all reasons for exclusion of articles were recorded and reported (see online supplemental appendix 1). Finally, the relevant studies, which respected eligibility criteria, were screened by the third reviewer (NV) to be included in the systematic review.

Data extraction and management

First, a data extraction form was created and validated by the three team members. This data collection form was fulfilled by one team member (NL) and corrected by another team member (RB). Any disagreement between the two reviewers was resolved by consensus or discussion with the third review team member (NV). This extraction form could be modified from the information collected in the eligible studies but should at least specify the following information:[62 64 66]

- General: authors, year of publication, journal's name, source of funding (if any) and country of the study.
- Methods: study design, total duration of study, follow-up when data were collected, study setting and withdrawals.
- Participants: number, age, gender, inclusion/exclusion criteria, type of workplace or job task, health of the workers/health status, that is, asymptomatic or symptomatic, year of work experience.
- Interventions: description of the type, duration, frequency, intensity, supervision of the warm-up programme, description/content of the comparison/control group and number of participants allocated to each group.
- Data collection: primary and secondary outcomes, measurement tools, questionnaires, tests.
- Statistical tests.
- Main results.

Risk of bias assessment

Two team members (NL and RB) independently assessed the risk of bias for each included study. Any disagreement between team members was solved by consensus or discussion with the third team member (NV). As both randomised and non-randomised controlled studies were included in this review, two 'risk of bias' tools were used, the Cochrane ROB2 and the Risk Of Bias In Non-randomised Studies-of Interventions (ROBINS-I).

For RCT

The Cochrane tool for assessing risk of bias from the Cochrane Handbook for Systematic Reviews of Interventions was used to assess potential biases of the included RCT studies. This tool is a well-known and validated instrument to assess the risk of bias in RCTs.[67] This tool has been revised in 2019 by Sterne *et al*[68] and has now five domains to assess bias arising from: (1) randomisation process, (2) deviation from the intended intervention, (3) missing outcome data, (4) measurement of the outcome and (5) selection of the reported result. Each domain was scored as follows (see table 2): 'high risk of bias', 'low risk of bias' and 'some concerns'.[68]

Table 3 Risk of bias judgement for a specific domain in non-RCT (from Sterne *et al*[69])

Judgement	Within each domain	Across domains	Criterion
Low risk of bias	The study is comparable to a well-performed randomised trial with regard to this domain	The study is comparable to a well-performed randomised trial	The study is judged to be at low risk of bias for all domains
Moderate risk of bias	The study is sound for a non-randomised study with regard to this domain but cannot be considered comparable to a well performed randomised trial	The study provides sound evidence for a non-randomised study but cannot be considered comparable to a well-performed randomised trial	The study is judged to be at low or moderate risk of bias for all domains
Serious risk of bias	The study has some important problems in this domain	The study has some important problems	The study is judged to be at serious risk of bias in at least one domain, but not at critical risk of bias in any domain
Critical risk of bias	The study is too problematic in this domain to provide any useful evidence on the effects of intervention	The study is too problematic to provide any useful evidence and should not be included in any synthesis	The study is judged to be at critical risk of bias in at least one domain
No information	No information on which to base a judgement about risk of bias for this domain	No information on which to base a judgement about risk of bias	There is no clear indication that the study is at serious or critical risk of bias *and* there is a lack of information in one or more key domains of bias (a judgement is required for this)

RCT, randomised controlled trial.

For non-RCT

The ROBINS-I was used to assess potential biases of the included non-RCT.[69] This tool has seven domains to assess bias arising from (1) confounding, (2) selection of participants, (3) classification of the intervention, (4) deviations from the intended intervention, (5) missing data, (6) measurement of outcomes and (7) selection of the reported result. Each domain was scored as follows (see table 3): ‘low risk of bias’, ‘moderate risk of bias’, ‘serious risk of bias’ and ‘critical risk of bias’.[69]

Measures of treatment effect

For studies using continuous data, treatment effect was reported as mean difference with 95% CI. In case the studies evaluated the same outcome with different scales, standardised mean difference with 95% CI was calculated. Regarding dichotomous/categorical variables, the treatment effect was calculated using the relative risk (RR) with 95% CI.[70–73]

Since the number of included studies is greater than 5[71] and when these studies are considered as sufficiently homogeneous, outcome data could be synthesised using a random effect meta-analysis.[62 72 74 75] If meta-analysis is not possible due to heterogeneity or if we are unable to pool the outcomes, a narrative synthesis could be performed using text and table formats.

Assessment of statistical heterogeneity

Statistical heterogeneity, defined as variability in the intervention effects, was estimated using the χ^2 test, with χ^2 p>0.10 provides significant evidence of heterogeneity. χ^2 assesses whether heterogeneity is only due to chance. To ensure a right comprehension of heterogeneity, χ^2 was completed with I^2 statistics particularly relevant when studies have small sample size or are few in numbers. Heterogeneity was categorised as follows:[75]

- 0%–40%: not be important.
- 30%–60%: moderate heterogeneity.
- 50%–90% substantial heterogeneity.
- 75%–100%: considerable heterogeneity.

Quality assessment and strategy for data synthesis

To assess quality of evidence of the included studies, we used the Grading of Recommendations, Assessment, Development and Evaluation (GRADE) approach.[76] This approach grades studies as followed: very low, low, moderate and high evidence. High-quality evidence means further research is very unlikely to change confidence in the estimate of effect; moderate-quality evidence means further research is likely to have an important impact on confidence in the estimate of effect and may change the estimate; low-quality evidence means further research is very likely to have an important impact on confidence in estimate of effect and is likely to change the estimate; and very low-quality evidence means very little confidence in the effect estimate. As suggested by Bordado *et al*,[60] the quality assessment was based on the findings in data extraction, and followed the domains of quality evaluation in the GRADE approach: risk of bias, inconsistency, indirectness and imprecision. Two team members (NL and RB) independently assessed

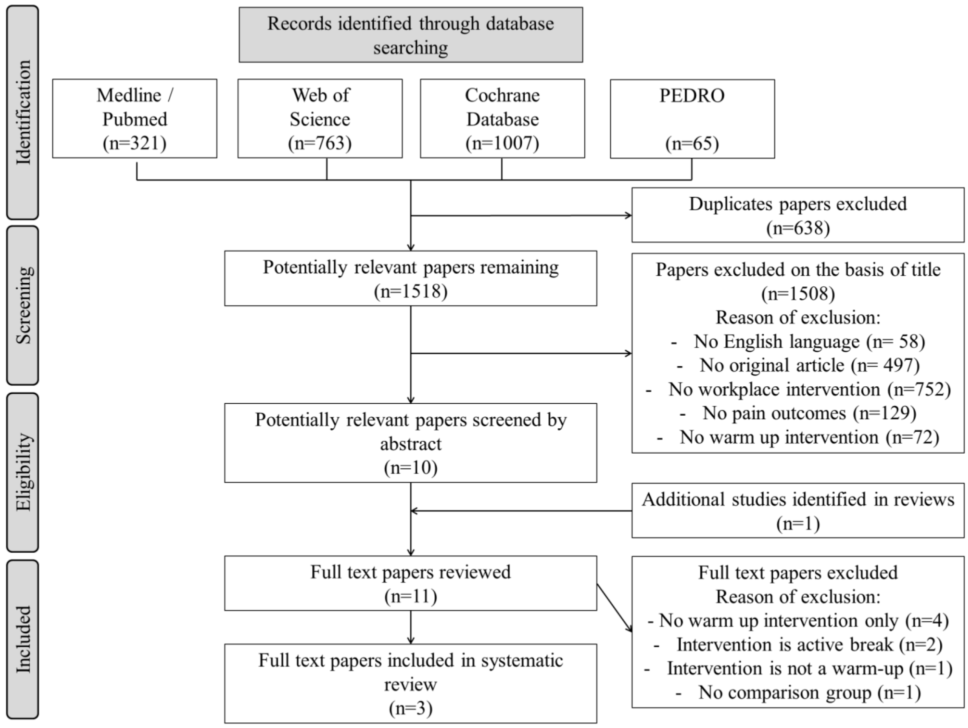

Figure 1 PRISMA flow chart of study selection process. PRISMA, Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

the quality of evidence of the included studies with the GRADE approach. A third reviewer (NV) was requested to resolve disagreement when consensus was not reached.

Patient and public involvement

No patients were involved in this study.

RESULTS

Description of studies

The electronic searches identified a total of 2156 references (figure 1), collected from several databases: 1007 studies in Cochrane, 763 studies in Web of Science, 321 studies in Medline PubMed and 65 studies in PEDRO. A total of 2156 references were reduced to 1518 after removing 638 duplicates. The titles, keywords and abstracts of the 1518 potentially relevant references were independently screened by two review authors (NL and RB). Finally, 10 references were selected for full-text analysis. After a manual searching through selected systematic reviews and included studies, an additional reference was found to extend the selection to 11 potential included studies. Two review authors (NL and RB) independently read and analysed the full texts, and finally included three studies in this review.

Included studies

The summary of findings is presented in tables 4–7.

Study design

One of the included studies was a cluster RCT,[48] while the two remaining studies were non-RCTs.[47 49]

Participants and location

The included studies involved a total of 1230 participants, 1053 males (85.6%) and 177 females (14.4%). A vast majority of participants (n=1104, 89.8%) were US young military recruits,[48] while the remaining participants (n=126, 10.2%) were construction or manual workers.[47 49] Gartley and Prosser[47] analysed 79 manual workers, 78 males (98.7%) and 1 female (1.3%) in the north-eastern of the USA while Holmström and Ahlborg[49] assessed the effects of a workplace warm-up intervention among 47 males (100%) construction workers in Sweden.

Interventions

In the study of Gartley and Prosser,[47] warm-up was a passive stretching intervention targeting the whole body.[47] This warm-up programme was designed with a chiropractor specialised in stretching protocols. It consisted of nine stretching exercises targeting the neck, shoulders, upper and lower back, quadriceps, hamstrings, arms and ankles. Participants were instructed to hold each stretch during 10–15 s, in a standing position, without any material.[47] In the two other studies, DiStefano *et al*[48] and Holmström and Ahlborg,[49] warm-up intervention consisted in combination of exercises based on dynamic movements. DiStefano *et al*[48] compared two different warm-up interventions among young military recruits. The warm-up in the intervention group consisted of dynamic flexibility, strengthening, agility and plyometric exercises. This intervention places an increased emphasis on balance exercises to prevent lower extremity injury. Participants in the group considered as the control group performed 10

Table 4 Summary of findings 1/4

Study	Design	Location	Industry/sector	Funding	Study duration	Participants	Average age	Gender
DiStefano *et al*[48]	Cluster RCT	USA	Military academy	Financial support was received from the National Athletic Trainers' Association Research and Education Foundation and the Congressionally Directed Medical Research Programme's Peer Reviewed Medical Research Programme Award (#W81XWH-11-2-0176).	9 months	1104	Range in age: 17–22 years	928 males (84%) 176 females (16%)
Gartley and Prosser[47]	Non-RCT	USA	Tin Mill Warehouse Drivers	Partial support from Eta Chapter. Sigma Theta Tau International (May 2009).	12 months	79	Tin Mill: 57.3 years Warehouse: 44.5 years Drivers: 49.3 years Total: 50.4 years	78 males (98.7%) 1 female (1.3%)
Holmström and Ahlborg[49]	Non-RCT	Sweden	Construction sites	Supported by the Swedish Council for Building Research and the Research Foundation for Working Environment in the Construction Industry.	3 months	47	Warm-up group: 39.7 (11.8) years control group: 39.8 (15.4) years	47 males (100%)

RCT, randomised controlled trial.

standard exercises commonly used in the US Army before physical fitness training. For Holmström and Ahlborg,[49] participants were invited to perform exercises during 10 min using working clothes. In this study, exercises were designed by a physiotherapist to increase heart rate frequency, to limber up and to stretch and were focused on the arms, shoulders, trunk and legs. This warm-up modality[49] was a combination of dynamic and stretching exercises.

Outcomes

Pain outcomes

Holmström and Ahlborg[49] evaluated pain, experienced workload, leisure time activity and dysfunction with a questionnaire included in the Back Pain Monitor (BPM) package system. Pain was rated on four category scales with a sum index from 0 to 36.

Injury rate was reported by Gartley and Prosser[47] 1 year prior the programme and during or immediately after the programme. DiStefano *et al*[48] assessed lower extremity injury rate with the Cadet Illness and Injury Tracking System (CIITS), an injury-surveillance database used in the military academy. Injury data were collected between the post-test session and the end of follow-up, at 8 months.

Physical outcomes

Physical-related outcomes were not evaluated by Gartley and Prosser.[47] DiStefano *et al*[48] assessed a technical task with the Landing Error Scoring System (LESS) and the peak Vertical Ground-Reaction Force (VGRF). The LESS is a clinical movement assessment tool that has been validated in the military academy population[77] which can predict subsequent injury risks.[78]

Holmström and Ahlborg[49] reported mobility of the spine and extremities, muscle stretchability, muscle strength and muscle endurance of the trunk and extremities. A liquid goniometer (MIE Medical Research) was used to measure hamstring muscles flexibility, the mobility of the spine and shoulder joints.[79] The dynamic performance of the upper limbs was measured as maximum arm elevations with 10 kg in each hand.[80] The dynamic performance of the lower limbs was measured as the maximum number of rising up from half-kneeling position to standing up position.[81] The static performance of the trunk extensors was measured lying in prone position with 240 s as the predetermined limit of termination.[82 83]

Psychosocial outcomes

None of the included studies assessed psychosocial outcomes.

Follow-up period

The follow-up periods observed in the four included studies ranged from no follow-up[47 49] to 8 months.[48]

Excluded studies

During the full-text assessment, eight studies were excluded.[39 46 84–89] Four studies were excluded because the warm-up intervention was part of a bigger programme

Table 5 Summary of findings 2/4

Study	Comparison	Health status	Dropout rate	Participation rate	Adherence
DiStefano *et al*[48]	IG1: DIME group n=4 companies of approximately 135 participants each IG2: SWU group n=4 companies of approximately 135 participants each	Injury-free	0	1104/1193 (93%)	Unclear About 77%
Gartley and Prosser[47]	IG: Programme participants (Tin Mill+Warehouse+Drivers) n=79 CG: eligible population n=1248	Injury-free	16/95 (17%) 1 WMSD	79/1248 (6.33%)	100%
Holmström and Ahlborg[49]	IG: MWU group n=30 CG: n=17	Injury-free	10/57 (17.5%) IG=7 excluded or dropped out CG=3 dropouts	N/A	Unclear About 80%

CG, control group; DIME, Dynamic Integrated Movement Enhancement; IG, intervention group; MWU, morning warming-up; N/A, not available; SWU, standard warm-up; WMSDs, work-related musculoskeletal disorders.

and could not be assessed separately.[39 84–86] Two studies were also excluded because physical exercises were performed during the working day or lunch break and therefore do not correspond to the definition of warm-up intervention.[87 89] Another study was excluded because the intervention was not considered as warm-up.[88] Finally, one study was a retrospective study without a control group. This study[46] analysed 146 food factory workers in Minnesota (USA), 98 males (67%) and 48 females (33%). Aje *et al*[46] consulted a certified stretching specialist to develop their intervention. This intervention consisted of 10 stretching exercises targeting the same body parts as Gartley and Prosser.[47]

Quality assessment including risk of bias

Risk of bias for RCT

The cluster RCT of DiStefano *et al*[48] had some concerns overall risk of bias. DiStefano *et al*[48] assessed the effects of warm-up modalities among US young recruits. In this study, both participants and implementers were not blinded to group allocation. This point led to have some concerns regarding the risk of bias due to deviations from the intended intervention. A low risk of bias was reported for the other items, that is, bias arising from the randomisation process, bias due to missing data, bias in measurement of the outcome and bias in selection of the reported results Therefore, according to the risk of bias judgement for a specific domain in RCT (from Sterne *et al*[69]), the DiStefano *et al*[48] study had some concerns overall risk of bias. For detailed risk of bias for the included RCT study, see online supplemental table 1.

Risk of bias for non-RCT

The two non-RCTs[47 49] were considered at serious risk of bias. All non-RCTs studies included in this review present serious risk of bias regarding confounding and measurement of outcomes. For instance, the participants of these studies volunteered to participate were all injury-free at the beginning of the studies. Regarding bias in measurements of outcomes, examiners in the study of Holmström and Ahlborg[49] were not blinded to group assignment. In the study of Gartley and Prosser,[47] investigators were employed by the companies in which the study took place. In the same study, there were also differences in the implementation of the intervention between participants in the same group. For instance, a subgroup of participants, that is, truck drivers performed warm-up before beginning of their route whereas the others start their route and then warmed up at their first delivery stop. For detailed risk of bias for the two included non-RCTs studies, see online supplemental table 2.

Assessment of indirectness

The two non-RCTs studies[47 49] met the inclusion criteria regarding participants, warm-up interventions and pain or related outcomes. In this sense, we can consider that evidence regarding participants, intervention and outcomes is direct. However, the results show important differences between studies regarding population, (ie, gender differences) and work characteristics. For example, females represented 16% of the sample in the DiStefano *et al*[48] study vs 0 or 1% in the Holmström and Ahlborg[49] and the Gartley and Prosser[47] studies. Furthermore, warm-up interventions were not similar although there were warm-ups. Gartley and Prosser[47] implemented a warm-up based on stretching exercises while Holmström and Ahlborg[49] set up a more various warm-up, with heart-rate increasing and limbering-up activities. Concerning outcomes, and specifically pain outcomes, only Holmström and Ahlborg[49] assessed pain with subjective scale (VAS) while Gartley and Prosser[47] limited themselves to the injury rate. However, the injury rate is not the predefined outcome of interest to assess participants'

Table 6 Summary of findings 3/4

Study	Characteristics	Supervision	Nature of supervisors	Training period	Frequency	Duration	Length of follow-up
DiStefano *et al*[48]	1. Dynamic exercises 2. Strengthening 3. Agility	2 companies of the IG1 were supervised by cadre and the two others by expert	Physical education instructors	6 weeks	2 or 3 times per week	10 min	8 months
Gartley and Prosser[47]	Stretching	Warehouse: Supervised by a stretching specialist during the first week and by work supervisors during the rest of the programme Tin Mill: Without supervision Drivers: Without supervision	Chiropractor work supervisors	3 months	Daily	6 min	No follow-up
Holmström and Ahlborg[49]	1. Dynamic exercises 2. Stretching	By a trained worker	Programme composed by physiotherapists but conducted by a trained worker	3 months	Every morning	10 min	No follow-up

pain and was hence considered as a surrogate outcome for pain measurement. For all these reasons, we decided to rate down the overall quality of evidence due to indirectness by 1 point.

The RCT study of DiStefano *et al*[48] also met the inclusion criteria but the observed population was very specific as participants were young soldiers. In this sense, it seemed complicated to generalise such results to the global workforce. For this reason, we decided to rate down the overall quality of evidence due to indirectness by 1 point.

Assessment of inconsistency

Considering the included studies,[47–49] we decided not to combine results because the range of patients, the type of warm-up interventions, and the outcomes considered were too different to perform a global analysis. DiStefano *et al*[48] did not observe any significant differences in the 1-year lower extremity injury rates between the two intervention groups (p=0.44; RR=0.88; 95% CI 0.62 to 1.23). The RR was close to 1 and indicated a small effect. The direction and/or magnitude of effect on injury rate was inconsistent across the included studies as Gartley and Prosser[47] indicated a significant difference in favour of the warm-up intervention. We judged the evidence to have serious inconsistency.

Assessment of imprecision

A posterior calculation of the sample size indicates that 385 measurements were needed to have a confidence level of 95%. There were 1104 participants involved in the DiStefano *et al* study.[48] DiStefano *et al*[48] did not observe any significant differences regarding the 1-year lower extremity injury rates between the two intervention groups (p=0.44; RR=0.88; 95% CI 0.62 to 1.23). The RR was close to 1 and the 95% CI included RR of 1.

The cumulative sample size of Holmström and Ahlborg and Gartley and Prosser[47 49] was 126 participants. As the optimal information size criterion was not met, we decided to rate down for imprecision. In the study of Gartley *et al*,[47] concerning the injury rate, the RR was 0.14 for the intervention group and 6.70 for the control group. Authors indicated that the odds of experiencing a work-related musculoskeletal injury were 7.69 times higher for control group than for intervention group.

Assessment of publication bias

Tests for funnel plot asymmetry should be used only when 10 studies are included in the meta-analysis. Less studies lead to decrease the power of the assymmetry test and therefore limit the possibility to distinguish chance from real asymmetry.[90] As a result, we were unable to process the funnel plots asymmetry test.

Assessing certainty in evidence

There were serious concerns with indirectness for the study of DiStefano *et al* as the population was very specific. Moderate evidence was found for this study for dynamic warm-up intervention decrease lower extremity injury rate. There were serious or very serious concerns with

Table 7 Summary of findings 4/4

Study	Pain outcomes	Effect on pain	Physical capacities outcomes	Effects on physical capacities	Psychosocial factors outcomes	Effect on psychosocial factors
DiStefano *et al*, 2016[48]	Lower extremity injury with the CIITS	No significant differences 1. POST2M DIME group 5.22 SWU group 5.97 RR(95% CI): 0.97 (0.50 to 1.90) 2. POST4M DIME group: 2.17 SWU group: 2.87 RR (95% CI): 0.84 (0.30 to 2.36) 3. POST6M DIME group: 3.48 SWU group: 5.73 RR(95% CI): 0.67 (0.31 to 1.48)	Peak VGRF	Greater decrease in peak VGRF (% of body weight) in the DIME group compared with the SWU group 1. POST2M vs PRE: SWU Group: –0.13±0.82 (–0.29 to 0.03) DIME Group: –0.62±0.91 (–0.87 to –0.37) (p<0.05) 2. POST4M vs PRE: SWU Group: –0.15±0.98 (–0.32 to 0.02) DIME Group: –0.46±0.64 (–0.71 to –0.21) (p<0.05) 3. POST6M vs PRE: SWU Group: –0.04±0.96 (–0.25 to 0.17) DIME Group: –0.53±0.83 (–0.79 to –0.27) (p<0.05)	Not evaluated	N/A
Gartley *et al*, 2011[47]	Injury rate	Significant difference between CG at T1 and IG at T2; and CG at T2 and IG at T2 1. CG (Tl): 6.5% (51 of 785) RR: 5.13 2. IG (T2): 1.3% (1 of 79) RR: 0.14 3. CG (T2): 8.5% (106 of 1248) RR: 6.70	Not evaluated	N/A	Not evaluated	N/A

Continued

Table 7 Continued

Study	Pain outcomes	Effect on pain	Physical capacities outcomes	Effects on physical capacities	Psychosocial factors outcomes	Effect on psychosocial factors
Holmström *et al*, 2005[49]	A questionnaire included in the Back Pain Monitor package system, concerning experienced workload, leisure time activity, pain and dysfunction	No significant differences for pain 1. IG before: 3.0 (16.0) 2. IG after: 2.0 (17.0) 3. CG before: 0.0 (19.0) 4. CG after: 1.0 (17.0)	1. Mobility of the spine and extremities 2. Muscle stretchability 3. Muscle strength 4. Endurance of the trunk and extremities	1. Significant positive effect (p<0.05) on Thoracic flexion in IG (86°–90°) and Trunk flexion in IG (63°–66°) IG vs CG : Significant differences after (p<0.001) 2. Significant positive effect (p<0.001) on hamstring stretchability in IG (54 to 66 dx deg) IG vs CG : Significant differences after (p<0.001) Significant positive effects on thigh stretchability (p<0.01) in IG (–10 to –17 dx deg) IG vs CG : Significant differences after (p<0.01) 3. Significant positive effect (p<0.05) on left side leg muscle in IG (38–40 sin(no)) but no significant differences between groups 4. No significant effect on back muscle isometric endurance in IG (153 to 153 s) but significant decrease (p<0.01) in CG (171 to 140 s) IG vs CG : Significant difference after (p<0.01)	Not evaluated	N/A

CG, control group; CIITS, Cadet Illness and Injury Tracking System; DIME, Dynamic Integrated Movement Enhancement; IG, intervention group; N/A, not available; PRE, immediately before intervention; RR, relative risk; SWU, standard warm-up; VGRF, vertical ground-reaction force.

four GRADE domains (ROB, inconsistency, indirectness and imprecision associated with the findings) across the non-RCT that measured injury rate (tables 4–6). The certainty of evidence found was very low for the effects of a warm-up intervention on injury rate. For detailed certainty of evidence of included studies, see online supplemental table 3.

Effects of workplace warm-up interventions

Effect on pain

In the Holmström and Ahlborg study,[49] there was no significant difference concerning pain between the intervention group and control group neither before nor after the 3-month period. In the intervention group, the median (range) score for pain was 3.0 (16.0) before the intervention and 2.0 (17.0) after the intervention.

Effect on injury rate

In Gartley and Prosser[47] for the 3-month period 1 year prior to the programme (Tl), 51 injuries were recorded among 785 employees representing an injury rate of 6.5%. The injury rate varied slightly according to working activity. For instance, tin mill workers had an injury rate of 6.5% (42 of 648) and beverage workers had an injury rate of 5.8% (9 of 156). At T2, during the intervention period, the amount of eligible employees was 1248 and 106 injuries occurred, for an injury rate of 8.5%. At T2 again, 78 participants had completed the study; 24 tin mill participants and 53 beverage company participants completed the protocol without any injury, while one beverage company participant experienced one injury. The injury rate in the intervention group was 1.3% (1 out of 79).

In the DiStefano *et al*[48] trial, 129/1104 participants had experienced a lower extremity injury during the follow-up period; that is, 8 months. Over the 129 participants injured, 90 of them were in the control group (incidence proportion, 19.4%) and 39 of them were in the intervention group (incidence proportion, 17.0%). No significant differences in the 1-year lower extremity injury rates was observed between the two groups (p=0.44; RR=0.88; 95% CI 0.62 to 1.23).

Effect on physical outcomes

In the Holmström and Ahlborg study,[49] significant increases over the warm-up intervention were observed in the intervention group for the trunk flexion and the stretchability of hamstring muscles. No increase was observed in this group for back muscle isometric endurance. No significant change was reported for the control group regarding trunk flexion, stretchability of hamstring muscles. The back muscle isometric endurance time decreased significantly in the control group. The difference between the groups after the 3-month period was significant. No other tests of muscular strength and endurance demonstrated any significant changes in neither IG nor CG. DiStefano *et al* reported a greater decrease in peak VGRF in the intervention group compared with the control group at all assessment time points.

DISCUSSION

This systematic review aimed to identify from published literature the available evidence regarding the effects of workplace warm-up on WMSDs and physical and psychosocial functions. Positive effects of workplace warm-up interventions were reported on pain outcomes such as injury rate[47 48] and physical capacities.[48 49] The present review also revealed that no study has assessed the effects of such intervention on psychosocial function. While results of the four included studies are encouraging and highlight the need for further studies, three main findings can be summarised as follows: (1) the number of included studies is low, (1) none of these studies are graded as high quality and (3) there is consequently no quality evidence for the effectiveness of workplace warm-up interventions on WMSDs, physical and psychosocial functions.

Methodological considerations

First, the three included studies present alternative designs to the ‘classic’ RCT. DiStefano *et al*[48] used a cluster-RCT design while the two remaining studies used a non-RCT design. Including such designs in this review was a choice shared by others[91 92] in order not to be too restrictive to figure out the effects of warm-up interventions in workplaces where classic RCT are rare and difficult to set up.[1 22 36 51–56 93 94] Therefore, cluster RCT is an excellent alternative design as all employees working in a same department or team are recruited in a same group.[36] One of the main issues of cluster RCT is the risk of baseline difference between clusters. In the study of DiStefano *et al*,[48] no difference was observed at baseline between groups regarding the characteristics of the participants (17–22 years) and physical-related outcomes such as scores in VGRF. The two remaining non-RCTs studies[47 49] present important methodological issues. Indeed, all of them have serious risk of bias due to confounding and outcome measurements. This risk mainly stems from the absence of blinding for examiners and should be taken into consideration especially for the assessment of physical outcomes. For instance, Holmström and Ahlborg[49] have reported positive effects of workplace warm-up intervention on muscle endurance and strength. However, the performance during these two tests depends to a large extent on the motivation of the participants and also on the enthusiasm or lack of enthusiasm of the examiners. Therefore the positive effects reported in this study may be questioned. Consequently, to increase internal validity of further non-RCT, an effort should be made to ensure assessor examiners’ blinding.

Study characteristics

Population

The three studies included in this review present heterogeneity in terms of the population studied. First, this review included 1230 participants. Most of them (n=1104, 89.8%) are US young recruits while the remaining participants (n=126, 10.2%) are construction or manual workers from USA or Sweden. Construction workers such as manual workers are highly exposed to physical WMSDs risk factors such as vibrations, working in low-temperature, lifting or carrying heavy loads. Therefore, for construction workers the prevalence of WMSDs is frequently above 25%.[95 96] These figures highlight the importance of WMSDs prevention among these populations and make relevant further investigations about the effects of workplace warm-up interventions. However, as mentioned earlier, almost 90% of the participants in this review are US young recruits. Differences between military and construction or industrial workers in workplace settings and social norms limit the generalisability of the positive effects reported by DiStefano *et al*[48] and strengthen the importance of increasing the number of participants. Then, further studies assessing the effects of workplace warm-up interventions should also focus on other occupational sectors affected by WMSDs such as agriculture or healthcare in which the prevalence of such conditions may reach 65%.[97] To go further, the heterogeneity in terms of population also concerns gender difference within studies. Interestingly, over the 1230 participants, 1053 are men (85.64%) while the remaining 177 participants (14.36%) are women. Even if this proportion is in line with the one reported in epidemiological studies,[96] the recent review of Umer *et al*[96] assessing the prevalence of WMSDs respectively among construction workers and industrial workers highlighted higher WMSDs prevalence among females as compared with their male counterparts.[96] Therefore, future studies are needed to examine whether gender difference can further enhance workplace warm-up interventions effects. This is of particular interest since a recent review of Prieske *et al*[30] assessing the effects of workplace physical activity programmes on physical fitness have reported no difference in training-induced gains between genders. Another important dimension that should be emphasised is that participants recruited in the four included studies have no musculoskeletal symptoms at baseline. Indeed, it appears that the effect of workplace physical activity to prevent musculoskeletal pain for a painful population may be substantial, but the perceptible benefits for generally healthy workers can be quite small.[15] As suggested by Chen *et al*[13] future studies should use a combination of pain intensity and incidence outcomes. These future studies should perform subgroup analyses of symptomatic, asymptomatic and possibly 'at-risk' groups to a better understanding of the effect of a workplace physical activity intervention on WMSDs.[13]

Intervention

Warm-up interventions could be subdivided into stretching,[47] dynamic[48] and hybrid warm-up intervention.[49] The stretching warm-up implemented in the studies of Gartley and Prosser[47] was exclusively constituted of stretching exercises focused on the whole body. The dynamic warm-up in DiStefano *et al*[48] was constituted of dynamic exercises that engage numerous muscles. This aim of this intervention was to increase heart rate frequency and places an increased emphasis on balance exercises. Lastly, the hybrid warm-up in Holmström and Ahlborg[49] was a combination of dynamic and stretching exercises. In the present review, duration of the workplace warm-up interventions is ranged from 6 to 12 weeks. This programme duration is similar to other workplace physical activity programmes implementing to prevent WMSDs[13 30] and is appropriate to observe effects on WMSDs-related outcomes.[36 92] In the three included studies[47–49] of this review the adherence rate is ranged from 77% to 100%, that is, a full adherence rate. The high adherence rate observed is not surprising since short bouts of exercises were previously identified as a determinant of adherence during workplace physical activity programmes.[36 38 93] These high adherence rates also suggest that warm-up have the potential to be easily integrated into the working hours and finally attest of the feasibility of workplace warm-up interventions. This result can also be considered as an argument to convince employers implementing such interventions. However, the duration of the workplace warm-up interventions of the four included studies question its sustainability. As recently suggested by numerous reviews reporting the effectiveness of workplace physical activity interventions; follow-up periods exceeding 12 weeks are needed to ensure (1) the sustainability of an intervention and (2) possible delayed intervention effects.[12 92 98–100]

Finally, one important point to discuss concerns the supervision of the workplace warm-up intervention. In two of the three included studies, warm-up sessions were supervised over the entire duration of the intervention by a trained worker, that is, a colleague or a direct supervisor. In this review, warm-up supervision can partly explain the high adherence rates mentioned earlier.[31 38 93] To go further, Prieske *et al*[30] reported that physical gains can be induced with lower levels of supervision (50% of supervised sessions) in young workers. However, supervision may be even more important with older workers to enhance motivation. These results suggest that supervision may have played an important role in maintaining participants in a training intervention, so the value of supervision should not be underestimated.[30 38 100]

Outcomes

As expected, the three included studies present heterogeneity regarding the outcomes used. This is particularly true regarding pain-related outcomes. DiStefano *et al*[48] used a specific military tool, that is, the CIITS while Gartley and Prosser[47] assessed the injury rate. Finally, Holmström and Ahlborg[49] used the BPM package. The assessment of injury rate is relevant since this indicator may be considered as an incidence outcome. However, using injury rate as the only pain-related outcome may have troubled estimating the effect of the exercise intervention. Surprisingly, none of the included studies assessed pain using VAS or NRS. These simple, valid and reliable tools are widely used and are considered as a gold standard to assess pain intensity over the last 7 days or the last 3 months.[13 16 92] Therefore, further researches should associate VAS or NRS (ie, questioning pain over the last week or last 3 months[13 17] with incidence rate.[13]

To take the analysis one step further, numerous studies have also suggested combining VAS or NRS with semiobjective measurement tool such as pressure algometry.[24 39 94] Pressure algometry has previously been used in workers population to assess pain sensitivity.[101–103] In the present review, two studies assessed physical capacities using peak VGRF,[48] mobility, strength and endurance tests of the back muscles.[49] In the study of Holmström and Ahlborg,[49] the authors observed an increased thoracic and trunk flexion while no significant difference was observed for trunk muscle endurance. Such findings are common with shorts bouts of exercise and were also linked to the adherence rate.[36] However, a recent review by Sjøgaard *et al*[8] has highlighted that for physically demanding work or repeated working tasks (such as those performed in the construction industry), increasing strength and/or endurance is of importance to potentially reducing pain and decreasing muscle load during the achievement of working tasks.[8] Based on this result, future studies should consider trunk muscle endurance as a primary outcome.

An unexpected finding of this review is the absence of studies assessing the effects of workplace warm-up interventions on quality of life, job satisfaction, workability or well-being. Regarding workability commonly assessed using self-administered questionnaire such the Work Ability Index, longer follow-up periods are needed to observe significant changes.[104] However, the work environment is recognised as an important source of psychological stress due to work demands and pressure. This may lead to adverse mental health outcomes and impaired psychological well-being. Numerous authors have pointed that workplace physical activity may be an effective intervention on such issues.[60 105 106] Self-efficacy could complement these variables.[107 108] Self-efficacy can influence how much effort will be expended on a health behaviour change, (eg, doing daily warming up), and how long it will be sustained.[109–111] Therefore, this last point led us questioning about the characteristics of the intervention implemented in the four included studies of this review. In order to draw further conclusions regarding work-related outcomes, controlled high-quality studies with long-term follow-up and using objective outcomes and/or validated questionnaires are required.

Potential biases in the review process

We conducted a comprehensive and transparent review process, where two review authors (NL and RB) independently performed the selection of studies, data extraction and 'Risk of bias' assessment. To minimise selection bias, publications dates were not limited and searching through several electronic databases. Then, reference lists of included studies were screened which led to scan one study[46] which did not appear during the initial search strategy. During the full-text assessment, seven studies were excluded.[39 84–89] Disagreements in the selection process were resolved through consensus, meaning that a third assessor (NV) was not necessary. Due to the very small number of studies included in the current review (n=3), we could not create funnel plots and assess publication bias. In case more studies can be included in an update of this review, we aim to assess publication bias.

Strengths

One of the strengths of this systematic review is the comprehensive search strategy used across major electronic databases. This choice facilitated a more evidence-based approach to literature searching. This review included searches among four electronic databases (Cochrane Central Register of Controlled Trials (CENTRAL), PubMed (Medline), Web of Science and Physiotherapy Evidence Database (PEDro), thus reducing the risk of missing relevant studies for inclusion. By following the PRISMA guidelines for systematic reviews, we ensured a systematic research process, which included a parallel independent screening, data extraction and risk of bias assessment by two review authors (NL and RB) independently to minimise potential biases in the review process. A third review author (NV) supervised all the process and validated the agreements found by the two other authors. We resolved any disagreement through discussion. Overall, the use of recommended standard reporting instruments such as, Cochrane tool for RCT risk of bias, ROBINS-I tool and GRADE guidelines strengthens the conclusion of the review.

Limitations

On the one hand, the lack of studies and especially high-quality studies can be considered as the strongest limitation of this review. Among the three included studies, none of them were graded as having high-quality evidence. All studies were at overall moderate to serious risk of bias.

On the other hand, due to the variety of study designs (population, types and duration of exercise interventions, tools used for outcomes), it was difficult to perform a synthesis across studies, which affects the possibility of drawing overall conclusions.

Recommendations for future research

The three included studies in this review showed positive effects of workplace warm-up interventions on injury rate[47] and physical function.[48 49] However, this result should be interpreted with caution since all studies present serious risk of bias and low quality evidence. Therefore, the effectiveness of workplace warm-up interventions to prevent WMSDs still remains to be established and does require further work to improve the quality of studies. Key improvement issue evidence from this review indicates that further studies should give a particular attention to blind participants and examiners. Different warm-up designs in terms of content, duration, intensity and supervision should also be assessed to establish an effective and specific warm-up to each 'at-risk' population. Such designs should assess pain using the combination of incidence rate and pain intensity and use similar related outcomes to a better comparison. They should also allow a sex, age, pain intensity or supervision comparison between groups for a more detailed and relevant analysis.

CONCLUSION

Due to the low number of studies (n=3), the heterogeneity of the warm-up interventions and the inconsistency of effects on pain, there was no evidence supporting the use of warm-up to prevent WMSDs in the workplace. The present findings highlighted the need of good quality studies targeting the effects of the different warm-up interventions (stretching, dynamic and hybrid) to prevent WMSDs.

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Study	Design	Location	Industry/sector	Intervention	Participants	Pain outcome	Reason for exclusion
Balaguier, 2017	Non-RCT	France	Vineyards	Physical Activity program: daily 15min warm-up + Strengthening and stretching sessions	17 vineyard-workers	Pressure pain thresholds over the lower back region	No warm-up only
Correa, 1989	Non-RCT	Puerto Rico	School of Medicine	Warm-up + aerobic training and resistive strength training	14 male employees or students	N/A	No warm-up only No pain outcome
Galka, 1991	Non-RCT	USA	Nurses	Back injury prevention program	Nurses	Rate of low-back injuries	No warm-up only
Kluth, 2013	Non-RCT	Germany	Cold-storage depots	20 min active breaks	30 storage employees	N/A	Active break during the working day No pain outcome
Mischishita, 2017	RCT	Japan	Office	Active rest during lunch breaks	59 white-collar workers	N/A	Active break during the lunch break No pain outcome
Springer, 2009	Clinical study	USA	Health/Fitness facilities	No intervention	63 commercial members 24 corporate employees 23 community members 13 academic students	N/A	No warm-up No pain outcome
Vercruysse, 2016	RCT	Belgium	Secondary school	Theoretical intervention, exercises, warm-up and stretching	55 physical education teachers	Number of injuries per 1000 hours of exposure	No warm-up only

Appendix 1. Excluded studies after full text screening an reason for exclusion

Included Studies	Outcome variable	Bias arising from the randomization process	Bias due to deviations from intented interventions	Bias due to missing data	Bias in measurement the outcome	Bias in selection of the reported result	Overall bias
DiStefano et al 2016	Injury rate	Low	Some concerns	Low	Low	Low	Serious

Table 1. Summary of review authors' judgments about each risk of bias for RCT study

Included Studies	Outcome variable	Bias due to confounding	Bias in selection of participants into the study	Bias in classification of interventions	Bias due to deviations from intented interventions	Bias due to missing data	Bias in measurement of outcomes	Bias in selection of the reported result	Overall bias
Gartley et al 2011	Injury rate	Serious	Low	Low	Low	Moderate	Serious	Low	Serious
Holmström et al 2005	Pain	Serious	Low	Moderate	Low	Low	Serious	Low	Serious

Table 2. Summary of review authors' judgments about each risk-of-bias item for the three non-RCTs studies

Certainty assessment							No of patients		
No of studies	**Study design**	**Risk of bias**	**Inconsistency**	**Indirectness**	**Imprecision**	**Other considerations**	**Warm-up intervention**	**Control group**	**Certainty**
Lower extremity injury rate									
1	RCT	Not serious	Not serious	Serious (-1)	Not serious	None	192	222	⊕⊕⊕⊝
Injury rate									
1	Non-RCT	Very serious (-2)	Serious (-1)	Serious (-1)	Serious (-1)	None	79	1248	⊕⊝⊝⊝
Pain									
1	Non-RCT	Very serious (-2)	Serious (-1)	Serious (-1)	Serious (-1)	None	30	17	⊕⊝⊝⊝

Table 3. GRADE quality of evidence score