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Workplace pedometer interventions for increasing physical activity (Review)

Freak-Poli RLA, Cumpston M, Peeters A, Clemes SA

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[Intervention Review]

Workplace pedometer interventions for increasing physical activity

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ABSTRACT

Background

The World Health Organization and the World Economic Forum have recommended further research to strengthen current knowledge of workplace health programmes, particularly on effectiveness and using simple instruments. A pedometer is one such simple instrument that can be incorporated in workplace interventions.

Objectives

To assess the effectiveness of pedometer interventions in the workplace for increasing physical activity and improving subsequent health outcomes.

Search methods

Electronic searches of the Cochrane Central Register of Controlled Trials (671 potential papers), MEDLINE (1001), Embase (965), CINAHL (1262), OSH UPDATE databases (75) and Web of Science (1154) from the earliest record to between 30th January and 6th February 2012 yielded 3248 unique records. Reference lists of articles yielded an additional 34 papers. Contact with individuals and organisations did not produce any further records.

Selection criteria

We included individual and cluster-randomised controlled trials of workplace health promotion interventions with a pedometer component in employed adults. The primary outcome was physical activity and was part of the eligibility criteria. We considered subsequent health outcomes, including adverse effects, as secondary outcomes.

Data collection and analysis

Two review authors undertook the screening of titles and abstracts and the full-text papers independently. Two review authors (RFP and MC) independently completed data extraction and risk of bias assessment. We contacted authors to obtain additional data and clarification.

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Main results

We found four relevant studies providing data for 1809 employees, 60% of whom were allocated to the intervention group. All studies assessed outcomes immediately after the intervention had finished and the intervention duration varied between three to six months. All studies had usual treatment control conditions; however one study's usual treatment was an alternative physical activity programme while the other three had minimally active controls. In general, there was high risk of bias mainly due to lack of blinding, self reported outcome measurement, incomplete outcome data due to attrition, and most of the studies had not published protocols, which increases the likelihood of selective reporting.

Three studies compared the pedometer programme to a minimally active control group, but the results for physical activity could not be combined because each study used a different measure of activity. One study observed an increase in physical activity under a pedometer programme, but the other two did not find a significant difference. For secondary outcomes we found improvements in body mass index, waist circumference, fasting plasma glucose, the quality of life mental component and worksite injury associated with the pedometer programmes, but these results were based on limited data from one or two small studies. There were no differences between the pedometer programme and the control group for blood pressure, a number of biochemical outcomes and the quality of life physical component. Sedentary behaviour and disease risk scores were not measured by any of the included studies.

One study compared a pedometer programme and an alternative physical activity programme, but baseline imbalances made it difficult to distinguish the true improvements associated with either programme.

Overall, there was insufficient evidence to assess the effectiveness of pedometer interventions in the workplace.

There is a need for more high quality randomised controlled trials to assess the effectiveness of pedometer interventions in the workplace for increasing physical activity and improving subsequent health outcomes. To improve the quality of the evidence available, future studies should be registered in an online trials register, publish a protocol, allocate time and financial support to reducing attrition, and try to blind personnel (especially those who undertake measurement). To better identify the effects of pedometer interventions, future studies should report a core set of outcomes (total physical activity in METs, total time sitting in hours and minutes, objectively measured cardiovascular disease and type II diabetes risk factors, quality of life and injury), assess outcomes in the long term and undertake subgroup analyses based upon demographic subgroups (e.g. age, gender, educational status). Future studies should also compare different types of active intervention to test specific intervention components (eligibility, duration, step goal, step diary, settings), and settings (occupation, intervention provider).

Authors' conclusions

There was limited and low quality data providing insufficient evidence to assess the effectiveness of pedometer interventions in the workplace for increasing physical activity and improving subsequent health outcomes.

PLAIN LANGUAGE SUMMARY

Do workplace pedometer interventions increase physical activity?

The World Health Organization recommends that most people should undertake at least 30 minutes of moderate-intensity physical activity on most days, as it reduces the risk of cardiovascular disease, diabetes and some cancers. However, less than 40% of the world's population are undertaking adequate amounts of physical activity and rates have been declining. Here we assess whether pedometer workplace interventions increase physical activity and thereby lead to subsequent health benefits.

To assess this, we searched for randomised controlled trials of workplace health promotion interventions that involved the use of a pedometer undertaken in employed adults. Between 30th January and 6th February 2012 we searched a range of electronic libraries and references of relevant papers, retrieving 3282 potential papers.

We eventually included four studies in the review. One study compared pedometer programmes with an alternative physical activity programme, but there were important baseline differences between the intervention and control groups that made it difficult to distinguish the true effect. The three remaining studies compared pedometer programmes with minimally active control groups. One study observed an improvement in physical activity in the pedometer programme, but two other studies found no significant difference between the pedometer group and the control group. We could not combine these results together, as each study used a different measure for physical activity, so it is not clear what the overall effect is. Single studies found beneficial changes in body mass index, fasting plasma glucose, the mental component of quality of life and worksite injury associated with the pedometer programmes as

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opposed to the control group. However, none of the studies identified consistent differences between the pedometer programme and the control group for waist circumference, blood pressure and quality of life outcomes. In addition, we judged the majority of included studies to have a high risk of bias, mainly due to participants and staff knowing who was in the intervention and who was in the control group, attrition of participants and not having published a protocol prior to running the study.

We conclude that there was insufficient evidence to assess whether workplace pedometer interventions are of benefit. There is a need for further high quality randomised controlled trials to be undertaken with a range of health outcomes and assessment in the long term.

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SUMMARY OF FINDINGS FOR THE MAIN COMPARISON [Explanation]

Workplace pedometer programmes compared to 'no intervention' control for increasing physical activity

Patient or population: Employees
Settings: In the workplace
Intervention: Workplace pedometer programmes
Comparison: No intervention

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	No intervention	Workplace pedometer programs				
Physical activity Follow-up: 3 - 4 months	See comment	See comment	Not estimable	1045 (3 studies)	⊕○○○ **very low**[1,2,3,4,5,6,7]	Studies used different measures and consequently their results could not be combined in meta-analysis. One study found pedometer programmes significantly increased activity; two others found no effect
Sedentary behaviour - not measured	See comment	See comment	Not estimable	-	See comment	None of the included studies measured this outcome.
Body Mass Index (BMI) (CVD and diabetes risk factor) Follow-up: 3 - 4 months	The median change in BMI in the control groups was **-0.8 kg/m²**	The mean BMI in the intervention groups was **0.92 lower** (1.82 to 0.02 lower)		197 (2 studies)	⊕○○○ **very low**[2,5,8,9,10,11]	

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Systolic blood pressure (CVD and diabetes risk factor) Follow-up: 3 - 4 months	The median change in systolic blood pressure in the control groups was **-1.05 mmHg**	The mean systolic blood pressure in the intervention groups was **3.11 lower** (8.39 lower to 2.17 higher)		197 (2 studies)	⊕⊕○○ **low**[2,5,8,11,12]	
Low-density lipid (LDL) cholesterol (CVD and diabetes risk factor) Follow-up: 4 months	The mean change in LDL cholesterol in the control group was **-2.2 mg/dL**	The mean LDL cholesterol in the intervention group was **3.60 lower** (11.78 lower to 4.58 higher)		83 (1 study)	⊕⊕○○ **low**[2,5,8,10,11]	
Quality of life SF-12 physical component. Scale from: 0 to 100. Follow-up: 3 months	The mean quality of life in the control group was **50.7 points**	The mean quality of life in the intervention group was **2.80 higher** (0.24 lower to 5.84 higher)		110 (1 study)	⊕⊕⊕○ **moderate**[2,10,11,13,14]	Higher scores on the SF-12 scale indicate better quality of life
Worksite injuries (Adverse effects) Follow-up: 12 months	The mean number of worksite injuries in the control group was **0.4 injuries per person**	The mean number of worksite injuries in the intervention group was **0.30 lower** (0.52 to 0.08 lower)		110 (1 study)	⊕⊕⊕○ **moderate**[2,11,13,14]	

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% CI) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

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[1] Allocation concealment was unclear for one study. The study was also at additional risk of selection bias during recruitment, as the allocation of clusters to the intervention or control group was known at the time of individual recruitment.

[2] No study successfully blinded participants and personnel. All studies had high levels of incomplete outcome data.

[3] Two studies did not use blinded or objective outcome assessment for this outcome.

[4] Two studies could not be assessed for selective outcome reporting as protocols were not available.

[5] One study had substantial baseline imbalance in levels of physical activity.

[6] One study found a significant benefit for the pedometer programme, while the other two found no significant effect. These results could not be combined via meta-analysis or directly compared due to the use of different measures of physical activity.

[7] The results of the studies encompassed a range of effects including the possibility of both benefit and harm from pedometer programmes.

[8] One study could not be assessed for selective outcome reporting as the protocol was not available.

[9] Heterogeneity between studies was high ($I^2 = 89\%$).

[10] The results of the studies encompassed a range of effects including the possibility of either a clinically significant benefit or a negligible effect for pedometer programmes.

[11] The overall sample size for this outcome is small.

[12] This result does not encompass either a clinically significant improvement or harm.

[13] This study did not use blinded or objective outcome assessment for this outcome.

[14] Despite issues noted, we judged this study to be overall at low risk of bias.

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BACKGROUND

Description of the condition

The World Health Organization (WHO) recommends that most people should undertake at least 30 minutes of moderate-intensity physical activity on most days, as it reduces the risk of cardiovascular disease, diabetes and some cancers (WHO 2004). Although the health benefits of physical activity are recognised, less than 40% of the world's population are undertaking adequate amounts of physical activity (WHO 2010) and rates have been declining (Brownson 2005; Food and Agricultural Organization of the UN 2006; Norman 2003). This trend is likely to continue as physical activity is continuously being reduced in all life environments including at home, during school/work, during recreation and in transport (Brownson 2005; WHO 2004; WHO 2010). Currently, physical inactivity is the fourth leading global risk for mortality and the eleventh leading global risk for burden of disease (WHO 2009). In percentage terms, physical inactivity is responsible for 6% of global deaths and 2% of disability-adjusted life years (WHO 2009).

Description of the intervention

Workplace as a setting for health promotion

The workplace has become a key setting for health promotion and disease prevention (Freak-Poli 2010; WHO 2002; WHO 2004; WHO & WEF 2008). The potential to influence behaviour in the workplace setting positively is especially important as occupations have gradually become more sedentary (Ferro-Luzzi 1996; Puig-Ribera 2008; WHO 2000; WHO 2009). Workplace health programme evaluations have demonstrated improvements in the leading global risk factors for chronic disease (WHO 2004) and have also been found to benefit the employer (Batt 2009; Speck 2009; WHO & WEF 2008).

Pedometer use in health promotion

A pedometer, or step counter, is a small, light, portable and easy-to-use electronic device that counts the number of steps taken by an individual. Pedometers are usually around the size of a matchbox, and can be worn clipped to the person's clothing at the hip, or another convenient place. They are low-cost, usually priced between USD 20 and USD 35, making them an accessible and feasible intervention.

By wearing the pedometer for a period of time, either during ordinary daily activities or a specific period of walking, the individual receives feedback on the number of steps taken and thereby a measure of their physical activity. Pedometers have been used as a measurement tool by athletes and for fitness training programmes, as well as health promotion programmes aimed at increasing physical activity levels. Health promotion programmes usually encourage participants to wear a pedometer during waking hours to record and give feedback on the number of steps taken on a daily or weekly basis (Bravata 2007; Freak-Poli 2011; Kang 2009; Ogilvie 2007; Richardson 2008). The programmes encourage participants to increase their levels of walking (a moderate-intensity activity) or running (a vigorous-intensity activity), and often provide a target step goal for participants, such as the commonly used 10,000 steps per day (Behrens 2007; Dishman 2009; Low 2007; Maruyama 2010; Rush 2009; Warren 2010).

Pedometers are rarely used alone, and health programmes may also include a variety of additional components such as a diary or website for logging steps, dissemination of additional health promotion information, motivational reminders, shared reporting of step counts, counselling sessions, group facilitators, weekly meetings, a website for communication among participants, team competition, participation rewards or group physical activity sessions (Aittasalo 2004; Behrens 2007; Chan 2004; Croteau 2004; De Cocker 2010; Dishman 2009; Faghri 2008; Farag 2010; Freak-Poli 2011; GCC 2010; Gemson 2008; Gilson 2007; Goetzel 2009; Goetzel 2010; Haines 2007; Kwak 2010; Low 2007; Lubans 2009; Maruyama 2010; Naito 2008; Puig-Ribera 2008; Racette 2009; Rush 2009; Speck 2009; Thomas 2006; Warren 2010). Pedometer use can also be incorporated as a component of broader health promotion programmes incorporating elements such as mass media, community-based activities, physical health checks and healthy eating initiatives.

This review focuses on health promotion programmes which include the use of pedometers in a workplace setting. Health promotion programmes are increasingly conducted at workplaces to access groups of participants in their daily lives, and for employers to improve worker health, reduce absenteeism and increase productivity (Marshall 2004; WHO & WEF 2008).

How the intervention might work

Pedometers provide immediate, specific feedback on levels of physical activity that is intended to motivate individuals to increase their activity over time (Matevey 2006). Health programmes that utilise pedometers are generally based on Social Cognitive Theory, identifying self efficacy as the main driver to positive physical activity and health behaviour change (Bandura 2001; Culos-Reed 2001; De Cocker 2010; Lemon 2010; Lubans 2009; Maruyama 2010; Prabhakaran 2009; Prodaniuk 2004; Tudor-Locke 2009). Pedometer-based programmes promote self efficacy by focusing on walking or running activities which usually have few barriers to participation. A pedometer can facilitate progressive individual goal-setting, and allow the participant to be flexible in the amount and the scheduling of their physical activity. In this way, the pedometer acts both as a motivator and a monitor of activity. The use of additional components such as targets, education and rewards are intended to increase that motivation.

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By setting a health promotion programme incorporating pedometers in a workplace context, social-cognitive motivation is combined with an ecological approach, addressing the environment in which people interact (Prodaniuk 2004). The workplace is a pre-existing social setting, in which collegiate camaraderie and the endorsement of leaders can reinforce participation in programmes, available facilities can be used to undertake physical activity and existing communication networks, e.g. email or a common notice board, can be used to encourage and inform participants (Freak-Poli 2011).

This review aims to measure the effects of the unique monitoring and motivational role of pedometers to increase physical activity in workplace settings, including relatively simple programmes in which pedometers are the main intervention (although they may be supported by the components listed above), and broader programmes incorporating pedometers as a component. Although it is more difficult to assess the impact of pedometers in the context of a complex, multi-component intervention, it is important to consider the evidence for these programmes, as they are often how pedometers are used in health promotion practice.

The impact of programmes incorporating pedometers can be measured in the short term, but it is anticipated that in the long term an increase in physical activity will lead to a reduction in the risk factors for and incidence of a range of chronic diseases such as cardiovascular disease and diabetes. To date, it has been reported that pedometer programmes that are longer in duration have been associated with a greater decrease in body mass index (Bravata 2007), a chronic disease risk factor. However, other health outcomes such as blood pressure, cholesterol, triglycerides and fasting glucose have either been found to have no association with pedometer programme duration (Bravata 2007), or the association with walking programmes has not been tested (Kelley 2004; Murphy 2006).

Why it is important to do this review

The World Health Organization and the World Economic Forum (WHO & WEF 2008) recommend that further research is needed to strengthen current knowledge of workplace health programmes, particularly on effectiveness and using simple instruments.

A number of Cochrane reviews have assessed the evidence surrounding the effectiveness of different interventions to increase physical activity, including community-wide interventions (Baker 2011), school-based interventions (Dobbins 2009), face-to-face and group interventions (Foster 2005), and supervised or individualised programmes for adults with chronic pain (Jordan 2010). Reviews are also underway to evaluate organisational interventions in the workplace, such as infrastructure, social or communication norms, or organisation and management-related changes (Christie 2010).

No Cochrane review has previously brought together all workplace interventions involving the feedback and motivational mechanism of pedometers. Only one other review has examined pedometers in a workplace context (Bravata 2007), but found inconclusive results.

To understand whether workplace health programmes incorporating pedometers offer an avenue for improving physical activity and consequent health risk factors, a Cochrane review incorporating the current literature is required.

OBJECTIVES

To assess the effectiveness of pedometer interventions in the workplace for increasing physical activity and improving subsequent health outcomes.

METHODS

Criteria for considering studies for this review

Types of studies

We included individual and cluster-randomised controlled trials (RCTs).

Types of participants

We included studies conducted with employed adults. Adults were defined as aged 16 years or older. We included studies in mixed-age populations only if a separate analysis of adult participants was available. We excluded studies conducted with trained athletes.

Types of interventions

We included any workplace health programme that incorporated the use of a pedometer. For inclusion in this review, the pedometer had to be incorporated into the health programme for the entire length of the programme, and the participants had to be able to view their step count. We included studies in which the pedometer was the sole component of the intervention; interventions in which the pedometer was the main focus of the intervention but was supported by other intervention components like step goals, diaries, teams, or rewards to increase motivation; and broader health promotion interventions that incorporated pedometers as one of many components. We aimed to explore the modifying effects of additional intervention components through subgroup analysis.

We did not include health programmes incorporating accelerometers rather than pedometers. Although accelerometers and pedometers are both unobtrusive, accurate motion sensors, there are four main distinctions. Firstly, the mechanics of an accelerometer function differently to a pedometer, allowing detection of

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three-dimensional movement in addition to simple step counting (Tudor-Locke 2002). Secondly, an accelerometer allows more complex analysis, with the capacity to segregate the recorded movements into subsets of time and analyse the frequency or intensity (Tudor-Locke 2002). Additional information such as speed, distance, caloric expenditure and total physical activity time may be available, dependent on the brand, and could be an extra motivator for the wearer. Thirdly, an accelerometer unit is at least four times more expensive than a pedometer unit; the usual price ranges between USD 120 and USD 299 but can cost up to USD 450 per unit (Tudor-Locke 2002; Tudor-Locke 2004b). Fourthly, to access the information that an accelerometer provides, it usually needs to be plugged in to a computer with specific software. The cost of the accelerometer, use of a computer, costs of specific software, cost of calibration hardware and related personnel expertise and time, dramatically increase the cost and feasibility of accelerometer use in health promotion (Tudor-Locke 2002; Tudor-Locke 2004b). Due to the differing mechanical function, additional information, lag time in feedback and increased cost, we did not view accelerometers as a low-cost, easy-to-use device and therefore did not include studies that used them as a motivational tool. However, we did include studies that used accelerometers solely to measure physical activity.

We included all comparator groups in the review, including any intervention without a pedometer, or no intervention.

Types of outcome measures

We aimed to report the following outcomes, but only the primary outcome was required as part of the eligibility criteria of studies for the review.

Primary outcomes

The primary outcome was physical activity, measured as self reported, objectively measured or observed activity such as step count, duration of physical activity, physical activity incorporated into work or leisure time (Prodaniuk 2004) also known as 'incidental' activity, leisure-time physical activity (Godin 1985), the Stanford Usual Activity Questionnaire (Sallis 1985), the Dutch short questionnaire to assess health-enhancing physical activity (Wendel-Vos 2003) and the International Physical Activity Questionnaire (IPAQ 2011).

The primary measurement time point of interest was at long-term follow-up. We categorised follow-up time as short-term (less than one month), medium-term (more than a month but less than one year) and long-term (equal to or more than one year).

Secondary outcomes

If pedometer-based, workplace health programmes were found to be effective at improving physical activity, we assessed the impact of this improvement on other health risk factors. The health risk factors of interest included:

- sedentary behaviour (e.g. time sitting, time watching television or other media, time spent under 1.5 metabolic equivalent of task units (METs) - a measure of energy consumption);
- cardiovascular disease and type II diabetes risk factors;
  - anthropometric measures (e.g. waist circumference, weight, body mass index, hip circumference, waist-to-hip ratio and body fat);
  - blood pressure (e.g. systolic blood pressure, diastolic blood pressure, hypertension, resting heart rate (for comparison, as heart rate should not change due to the health programme));
  - biochemical measures (e.g. blood glucose, blood cholesterol (high-density lipids, low-density lipids, total), blood triglycerides); and
  - disease risk scores (e.g. cardiovascular disease risk (D'Agostino 2008) or type II diabetes risk (Baker IDI Heart and Diabetes Institute 2008));
- quality of life (e.g. Short Form 36 or 12 Health Survey (SF-36 2011); the Social Support Inventory (Dunkel-Schetter 1986), the Satisfaction With Life Scale (Diener 1985)); and
- adverse effects including injury.

Search methods for identification of studies

Electronic searches

We searched the following sources from the earliest record to the current date. We ran the searches between 30th January and 6th Feburary 2012:

- CENTRAL (the Cochrane Central Register of Controlled Trials, *The Cochrane Library*);
- CINAHL (Cumulative Index to Nursing & Allied Health Literature);
- MEDLINE;
- Embase;
- OSH UPDATE (CISDOC, HSELINE, NIOSHTIC, NIOSHTIC-2, RILOSH, IRSST and INTERNATIONAL BIBLIOGRAPHIC databases); and
- Web of Science.

We developed a systematic search strategy with help from the Cochrane Occupational Safety and Health Review Group's Trials Search Co-ordinator, Leena Isotalo. We tested the strategy against a set of 13 known relevant studies from across the globe before running final searches in January and Feburary 2012. We used adapted search strategies to search CENTRAL (Appendix 1), CINAHL through EBSCOhost (Appendix 2), MEDLINE through PubMed (Appendix 3), Embase through Embase.com (Appendix 4), OSH UPDATE (Appendix 5) and Web of Science (Appendix 6). We did not limit the search by language.

We updated the search on March 28th 2013.

See Figure 1 for a summary of the search and inclusion process.

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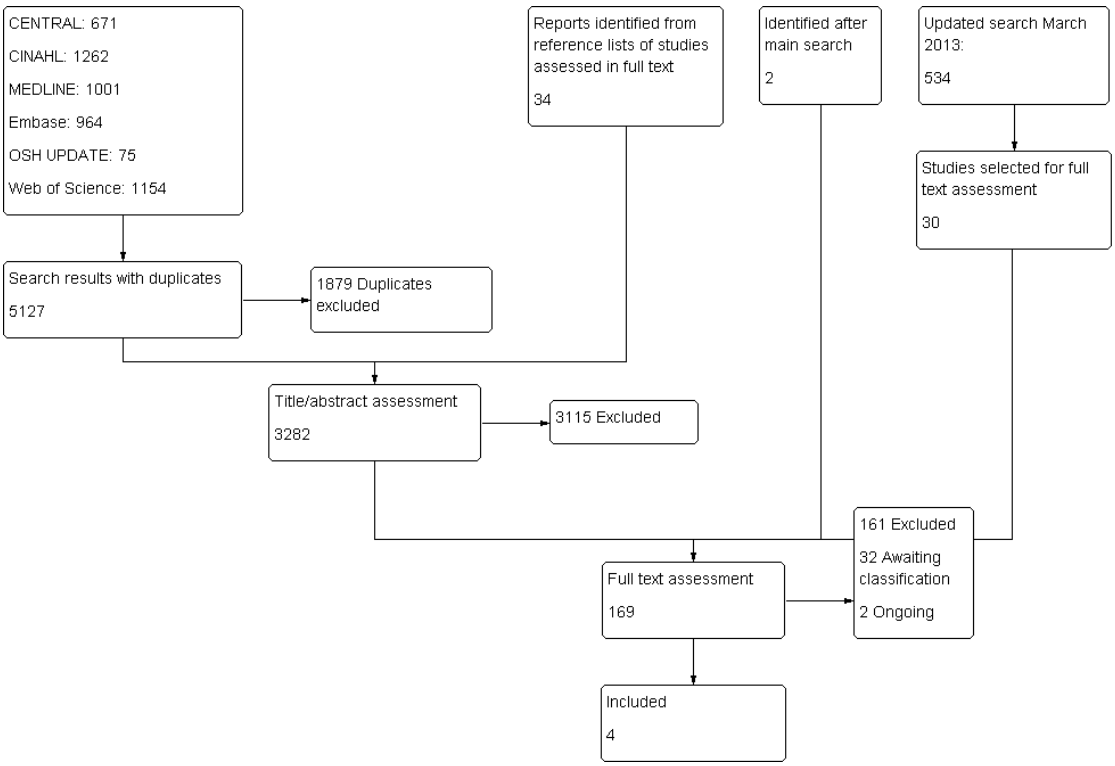
Figure 1. Literature search results



Searching other resources

Cochrane Review Groups in areas related to this review include the Cochrane Public Health Group and the Cochrane Heart Group, and we requested these groups to search their trial registers for relevant trials.

We searched the websites of organisations actively involved in workplace physical activity programmes. For example, the World Health Organization, including the *Global Strategy on Diet, Physical Activity and Health* (WHO 2004) and *Preventing Noncommunicable Diseases in the Workplace through Diet and Physical Activity* (WHO & WEF 2008).

After the full-text rejection stage, we scanned the article references as a source of RCTs. At this full-text stage, we sent a comprehensive list of relevant articles together with the inclusion criteria for the review to the first author of each paper that met the inclusion criteria, asking if they knew of any additional published or unpublished studies which might be relevant.

Data collection and analysis

Selection of studies

The initial search strategy yielded a set of 3248 references. Two authors (RFP and either MC, SC or AP) undertook an initial screening of titles and abstracts independently, to remove those which were obviously outside the scope of the review. We sought full-text translations or evaluations of all relevant non-English articles. We rejected articles at the initial screening stage if both authors agreed based on the title and abstract that:

- the article was not a report of a randomised controlled trial; or
- the trial did not address a pedometer-based physical activity intervention; or
- the intervention was not tested on employed adults.

We were over-inclusive at this stage and, if in doubt, we included

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the paper and obtained the full text of the article for further evaluation.

We obtained the full text of all the papers potentially meeting the inclusion criteria. We linked multiple publications and reports on the same study together. Two authors (RFP and either MC, SC or AP) screened all the full-text papers independently. We rejected articles at this stage if both authors agreed, based on the full text, that:

- the article was not a report of a randomised controlled trial; or
- the trial did not address a pedometer-based physical activity intervention; or
- the intervention was not tested on employed adults

We did not disagree on any article, and hence a third author was not required to review additional papers. If inclusion criteria were unclear, we corresponded with the publication author via email for further information. We updated the search on March 28th 2013 and this yielded a total of 534 new references. We screened these and included 30 more potential papers as Studies awaiting classification.

Data extraction and management

Two review authors (RFP and MC) independently completed a data extraction form for each included study. We tailored the data extraction form to the requirements of this review and we piloted it to assess its ability to capture study data. We incorporated items for assessing risk of bias into the data extraction form. In addition, we assembled and compared multiple reports and publications of the same study to ensure completeness and to identify possible contradictions.

We collected data on the study population, study environment, intervention specifics, study methodology and outcomes of each study. We recorded all measures identified as primary or secondary outcomes, regardless of how the information was reported (for example, categorical cut-offs or continuous mean ± standard deviation data). Where studies reported more than one time point, we collected all the outcomes at all time points.

Assessment of risk of bias in included studies

Two review authors (RFP and MC) independently assessed the risk of bias of each included study using a descriptive approach, as outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Cochrane Handbook). We assessed the following key criteria: random sequence generation; allocation concealment; blinding of participants and personnel, blinding of outcome assessment; incomplete outcome data; selective outcome reporting; and other sources of bias (e.g. baseline imbalance, and risks associated with cluster-randomised designs such as differences in recruitment and comparability of clusters) (Cochrane Handbook). We assessed each study as having either a low, high, or unclear risk of bias. A judgment of unclear risk of bias indicated either lack of information or uncertainty over the potential for bias. There was no persisting difference of opinion, so a third author was not required to review additional papers.

We considered trials that failed to meet three or more of the above criteria, excluding blinding, to be at high risk of bias. Although failure of blinding can have a serious effect on study outcomes, we hypothesised that this criterion would not be met by most of the studies included in the review, and would not assist in discriminating between higher and lower risk studies.

Assessment of quality of the evidence in included studies

For an outcome which has results from three or more studies, the quality of evidence was assessed using the GRADE system as outlined in Chapter 12 of the Cochrane Handbook. The general statement regarding the quality of the body of evidence as 'High', 'Moderate', 'Low', or 'Very Low' was based upon risk of bias, study methodology including the design and implementation, directness of the evidence, heterogeneity and its causes, precision and publication bias.

Measures of treatment effect

We expressed the effect sizes for dichotomous outcomes as risk ratios (RRs). For continuous outcomes, we used mean differences (MD) between the postintervention values of the intervention and control groups to express effect sizes where possible. Where studies used different scales to measure the same outcome, we used standardised mean differences (SMD). We report all effect measures with a 95% confidence interval (CI).

Unit of analysis issues

If a study had more than two arms, we considered the interventions in each arm and did not analyse any arms not relevant to the review. Where two intervention arms were considered comparable for the purposes of this review, we combined the data to provide an overall assessment of the effect of the intervention versus control. Where it was not appropriate to combine groups, we conducted separate comparisons of each arm of interest (e.g. one intervention arm versus control, and then the second intervention arm versus control), taking care not to include the same participants twice within a meta-analysis and preventing unit of analysis error. When a standard deviation was not available for a continuous outcome, we used the methods demonstrated by the Cochrane Handbook to obtain one.

It was likely that the review would include cluster-randomised controlled trials, in which participants are allocated to the intervention or control in groups (e.g. workplaces). Unit of analysis errors may occur when studies allocate participants in clusters, but analyse the results by the total number of individuals. This can

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result in overestimation of the statistical significance of the results by not accounting for the clustering of individuals in the data. Correcting the error by analysing results by the unit of randomisation (the cluster) can underestimate the statistical significance of the results, particularly where clusters are very large. In our meta-analysis we assessed the included cluster-randomised trials for unit of analysis errors. Where analyses correctly accounted for clustering, we analysed results presented as overall effect estimates (e.g. odds or risk ratios based on a multilevel model) using the generic inverse variance method (Cochrane Handbook). Where clustering was not correctly accounted for, we re-analysed outcomes where possible in accordance with the methods outlined in the Cochrane Handbook. If a measure was re-analysed, we noted this in the review. If re-analysis of outcomes that did not account for clustering was not possible, we reported only the point estimate without a measure of variance.

Dealing with missing data

Where information was missing from the included studies, we contacted the study authors to provide additional information. We reported the author correspondence and outcome in the 'Risk of bias' table. We assessed the risk of bias arising from incomplete outcome data as part of the overall risk of bias assessment.

Assessment of heterogeneity

We considered the clinical heterogeneity of the included studies before conducting any analyses. We aimed to analyse the following categories separately:

- Studies of pedometers alone, pedometer-focused interventions with supporting components to increase motivation (e.g. step goals, diaries, teams, rewards) and broader health promotion interventions that incorporated pedometers as one of many components.
- Short-term and long-term interventions.
- Studies comparing pedometer interventions to no intervention, similar components without a pedometer, larger-scale health promotion interventions and other active interventions.

We quantified and evaluated the amount of statistical heterogeneity to determine whether the observed variation in the study results was compatible with the variation expected by chance alone (Higgins 2003). We assessed heterogeneity through examination of the forest plots and quantified it using the I^2 statistic. Where we observed an I^2 statistic greater than 90%, we considered heterogeneity to be too high to conduct a meta-analysis.

Assessment of reporting biases

If ten or more studies were included in a meta-analysis, we aimed to assess the possibility of publication bias using funnel plots (Cochrane Handbook). We also aimed to investigate alternative explanations for funnel plot asymmetry (such as clinical or methodological heterogeneity, statistical artefacts or chance) (Egger 1998). We aimed to assess the potential impact of any suspected small study effects using a comparison between fixed-effect and random-effects meta-analysis models.

We also aimed to assess the risk of bias arising from selective outcome reporting within studies as part of the overall risk of bias assessment.

Data synthesis

For data synthesis we followed Chapter 9: 'Analysing data and undertaking meta-analyses' of the Cochrane Handbook. Where studies were considered by the authors to be sufficiently clinically and methodologically homogeneous, and where comparable data were available from at least two studies measuring the same outcome, we performed meta-analyses using Review Manager 5 software (RevMan 2011). We used a random-effects model as the default to incorporate the assumption of heterogeneity between studies.

Subgroup analysis and investigation of heterogeneity

If more than two trials were available that reported data in each category, we aimed to explore the following participant characteristics using subgroup analyses:

- gender;
- age (as the probability of maintaining good health diminishes as an individual gets older (AIHW 2008), there may be differing motivations for participation in pedometer-based workplace health programmes depending on age); and
- educational status (completion of tertiary education).

If more than two trials were available that reported data in each category, we aimed to explore the following intervention characteristics.

- Eligibility of participants:
  - Are interventions targeting high-risk employees more effective than interventions recruiting all employees?
  - Are interventions targeting sedentary or office-based employees more successful than interventions targeting active or manual employees?
- Step goal: are interventions that utilise a daily step goal (e.g. 10,000 steps per day) more effective than non-step goal-defined interventions?
- Step diary: are interventions that utilise a step diary (e.g. daily or weekly record of steps) more effective in changing physical activity than non-diary interventions?
- Duration: are short duration interventions (less than one month), medium duration interventions (more than a month but less than one year) or longer duration interventions (equal to or more than one year) more effective?

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- Provider: are interventions with an external programme provider more effective than interventions undertaken internally within the workplace?

Sensitivity analysis

We aimed to carry out a sensitivity analysis for studies with low risk of bias, defined as meeting at least three of the following criteria: random sequence generation; allocation concealment; incomplete outcome data; selective outcome reporting; and other sources of bias.

We aimed to use sensitivity analysis to assess the impact of suspected publication bias, by comparing the fixed-effect and random-effects meta-analysis. We also aimed to use additional sensitivity analyses to assess the potential impact on results of decisions made by the authors, including any assumptions made about measurement choice, duration of follow-up, missing data and correlation coefficients used in the adjustment of cluster-randomised trials.

RESULTS

Description of studies

See: Characteristics of included studies; Characteristics of excluded studies; Characteristics of studies awaiting classification; Characteristics of ongoing studies.

See: Figure 1, Characteristics of included studies table, Characteristics of excluded studies table, Studies awaiting classification table.

Results of the search

As outlined in Figure 1, the electronic searches in CENTRAL (Appendix 1, 671 potential papers), CINAHL (Appendix 2,1262), MEDLINE (Appendix 3, 1001), Embase (Appendix 4, 965), OSH UPDATE (Appendix 5, 75) and Web of Science (Appendix 6, 1154) yielded a total of 3248 references following the removal of duplicates. We completed the study screening process between February and July 2012. Of the 3248 references, we considered 146 potentially eligible based on their title and abstract, and assessed them in full text. From the reference lists of these papers, we identified a further 34 potentially eligible papers. Of these, we determined 13 to be ineligible based on their title and abstract, and assessed 21 in full text. Of the 167 papers assessed in full text, we excluded 161; one met our inclusion criteria but results were not available at the time of this review (Thøgersen-Ntoumani 2010, see the 'Characteristics of ongoing studies' table), one study was unclear in its target population and could not be classified prior to publication (Butler 2004, see the 'Characteristics of studies awaiting classification' table), and we included four studies. Subsequent to the search, but prior to publication, we became aware of two additional studies: one awaits classification (Aittasalo 2012), and one is ongoing (Pillay 2012).

We corresponded with study authors to clarify eligibility, identify additional related publications, obtain outcome results (Thøgersen-Ntoumani 2010), obtain outcome-specific sample sizes (Morgan 2011) and obtain intraclass correlation coefficients (ICCs) (Dishman 2009).

We updated the search on March 28th 2013 and this yielded a total of 534 new references, which included 30 more potential papers that are still waiting full text assessment (see Studies awaiting classification).

Included studies

We included four studies in this review (Dishman 2009; Maruyama 2010; Morgan 2011; Talbot 2011). In total, the included studies had recruited 1809 employees, with the greatest contribution of 1442 employees coming from Dishman 2009. The remaining three studies contributed 156 participants (Talbot 2011), 110 participants (Morgan 2011) and 101 participants (Maruyama 2010). Overall, 60% of study participants were allocated to the intervention groups, ranging between 51% (Maruyama 2010) and 61% (Dishman 2009). Key features of the studies are summarised below, and more detailed descriptions are given in the 'Characteristics of included studies' table.

Intervention

All included studies were of medium duration, ranging between three (Dishman 2009) and six months (Talbot 2011). No studies assessed short- or long-term interventions. All included studies used broad health promotion interventions that incorporated pedometers as one of many components. Programmes were heterogeneous in terms of the other components they incorporated. No studies used pedometers alone, nor pedometer-focused interventions with supporting components to increase motivation (e.g. step goals, diaries, teams, rewards).

Two programmes had a theoretical basis. These included theory-based behaviour modification principles built around goal-setting theory (Dishman 2009) and Social Cognitive Theory and behaviour change strategies (Morgan 2011). One used organisational action where management and employees were involved in the project objectives, implementation and encouragement mainly via joint employee-management steering committees (Dishman 2009). Three used professional individualised contact either with a dietitian and physical trainer (Maruyama 2010), a researcher (Morgan 2011) or a counsellor (Talbot 2011). One used monthly group meetings (Talbot 2011). Two used personalised websites (Maruyama 2010; Morgan 2011). Two used group-based incentives for reaching designated goals such as lunch bags, programme

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t-shirts, recognition plaques, free catered lunch and local sporting equipment store gift vouchers (Dishman 2009; Morgan 2011). Two used environmental prompts such as signage (Dishman 2009) and motivational postcards (Talbot 2011). All included studies used personal goal setting, with two specifically having the goal of 10,000 or more pedometer steps each day (Maruyama 2010; Talbot 2011) and the other two used the 10,000 steps goal if individually chosen with or without an alternative physical activity goal (Dishman 2009) or amongst other personalised strategies to address weight loss, reduce energy intake and increase energy expenditure (Morgan 2011). Two studies used team goals (Dishman 2009; Morgan 2011). Three used step diaries (Dishman 2009; Maruyama 2010; Talbot 2011). Two studies used external programme providers (Maruyama 2010), while the other two utilised internal staff (Dishman 2009; Talbot 2011).Three studies used the Yamax pedometer brand model SW 200 (Dishman 2009; Morgan 2011; Talbot 2011), and one used a computerised Omron pedometer model HJ-7101T (Maruyama 2010).

Control

All the included studies compared pedometer interventions to what they considered to be a 'usual treatment' control condition. However, components of the usual treatment conditions varied. We determined that three studies effectively used a 'no intervention' control condition: in one study this was simply no intervention (Maruyama 2010), one study used a wait-list control (Morgan 2011), and one control group received a minimal intervention (completing the CDC health-risk appraisal and receiving monthly newsletters describing the health benefits of physical activity; Dishman 2009).

The fourth study compared the pedometer intervention to an alternative physical activity programme using similar components without a pedometer, in this case the US National Guard's usual fitness improvement programme (Talbot 2011). We considered this study separately from the other three.

Eligibility & Recruitment

Pedometer programmes versus 'no intervention' control

Participant eligibility was based on health status in all three studies in this group. One study required participants to have a particular health risk and to be otherwise healthy (Morgan 2011), one study only recruited healthy participants (Dishman 2009), and one study only recruited unhealthy participants (Maruyama 2010).

Two studies recruited participants through a series of promotional actions seeking volunteers (Dishman 2009; Morgan 2011), while one study identified potential participants through regular medical or fitness check-ups that were not part of the study (Maruyama 2010), and directly approached individuals who were overweight.

Pedometer programme versus alternative programme without pedometer

Talbot 2011's participant eligibility was based on health status. The study required participants to have a particular health risk and to be otherwise healthy. The study identified potential participants through regular fitness tests that were not part of the study, and directly approached individuals who had lower fitness.

Employee demographics

Pedometer programmes versus 'no intervention' control

Workplaces in this group included a health insurance association (Maruyama 2010), a home improvement store chain (Dishman 2009) and an aluminium factory (Morgan 2011). The employees' work roles (sedentary/office or active/manual) were not defined in one study (Dishman 2009). One study recruited office workers (assumed to be sedentary; Maruyama 2010) and one study recruited factory crews (assumed to be manual workers; Morgan 2011).

The proportion of male participants ranged from 31% (Dishman 2009) to 100% (Maruyama 2010; Morgan 2011). Two studies recruited adults aged between 18 to 19 and 64 to 65 years (Dishman 2009; Morgan 2011), while one recruited 30 to 59-year-olds (Maruyama 2010). The mean age of participants ranged from 36 (Dishman 2009) to 44 years (Morgan 2011).

Pedometer programme versus alternative programme without pedometer

Talbot 2011 was undertaken within a national army reserve, where the employees' work roles (sedentary/office or active/manual) varied as they were primarily employed elsewhere, but were also part-time Army National Guards.

The proportion of male participants was 69% in the intervention group and 80% in the control group. No age range was reported, but the mean age of participants was 33 years.

Excluded studies

Of the 167 papers assessed in full text, we found 161 to be ineligible on the basis that they were irrelevant conference papers (21), were not randomised controlled trials (89), recruited participants who were not employed (19), were not undertaken in a workplace setting (9), did not use a pedometer (12), did not use a pedometer throughout the intervention period (5), used accelerometers (2), also provided pedometers to the control group (7), did not allow participants to view their step count (1) or did not measure a physical activity outcome (1) (see the 'Characteristics of excluded studies' table for further details). We also excluded an additional study (Racette 2009) because the authors allocated only one workplace cluster to each of the intervention and control arms. In our

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opinion this was not adequate to reduce the risk of imbalance of confounders between the two study arms. This was an additional criterion not originally planned at the protocol stage.

Risk of bias in included studies

For details of risk of bias in the included studies, see the Characteristics of included studies tables. A brief visual summary is given in Figure 2.

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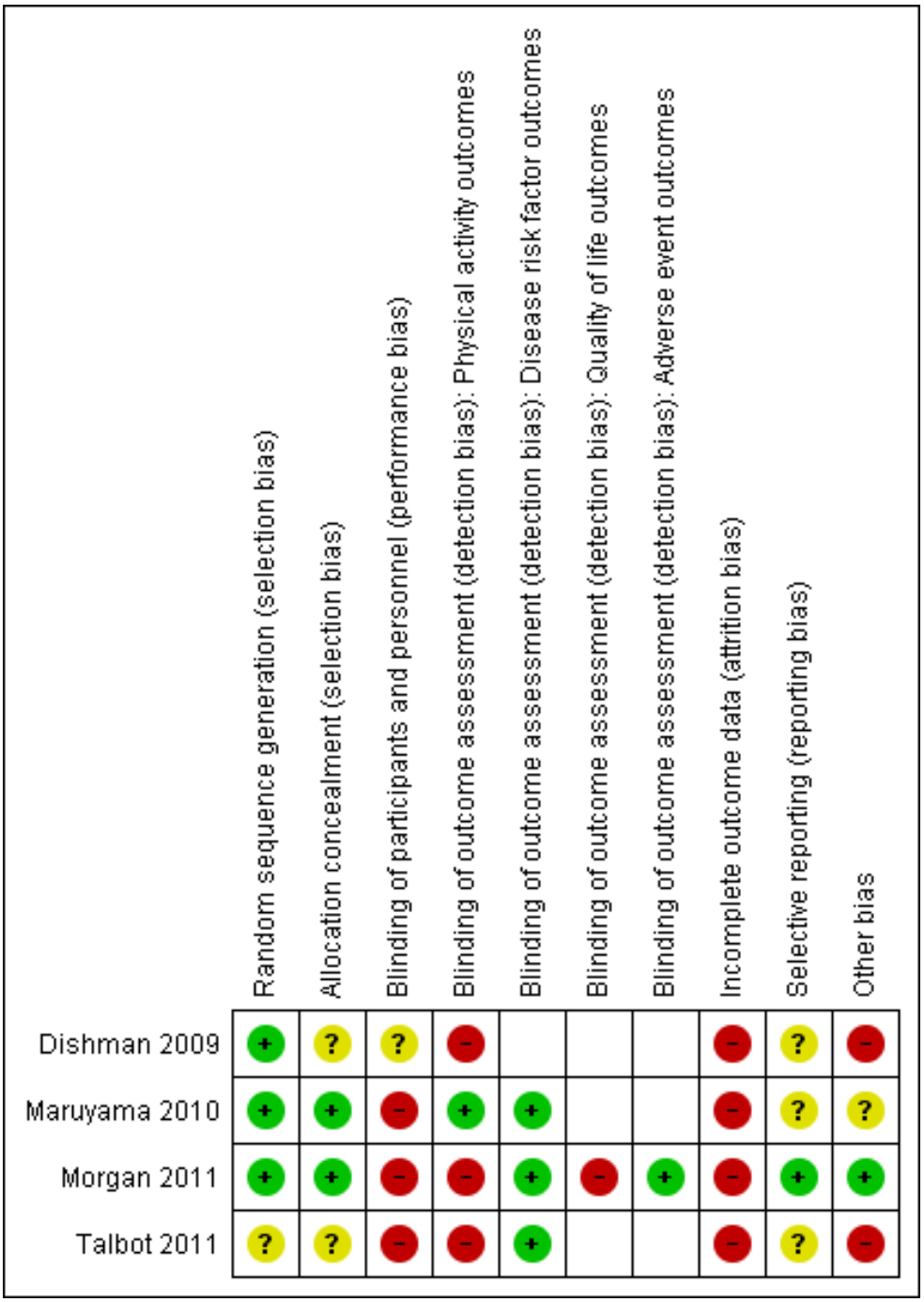
Figure 2. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



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Pedometer programmes versus 'no intervention' control

As we had hypothesised, given the nature of physical activity as an intervention, none of the studies was completely successful in minimising risk by blinding participants and personnel or outcome assessors. Leaving these criteria aside, we judged one of the included studies (Morgan 2011) to be overall at low risk of bias, assessing it as low risk against four of the remaining five criteria. We judged the two other studies (Dishman 2009; Maruyama 2010) to be overall at high risk of bias, assessing them as low risk against only one and two of the remaining five criteria respectively.

Pedometer programme versus alternative programme without pedometer

We judged Talbot 2011 as overall at high risk of bias. Similar to the other studies above, it was not completely successful in minimising risk by blinding, and we judged it to be at high risk of bias against all of the remaining criteria.

Allocation

Pedometer programmes versus 'no intervention' control

We judged randomisation for the three studies to be at low risk of bias as it was undertaken by means of a computer programme (Dishman 2009; Maruyama 2010; Morgan 2011).
We judged allocation concealment to be at unclear risk for Dishman 2009, as the authors provided insufficient information. We judged allocation concealment to be at low risk of bias for two studies as the randomisation process was undertaken by people other than those managing the study and contacting participants (Maruyama 2010; Morgan 2011).

Pedometer programme versus alternative programme without pedometer

We judged sequence generation and allocation concealment to be at unclear risk for Talbot 2011 as insufficient information was provided by the authors.

Blinding

As mentioned above, no studies were able fully to meet the blinding criteria. We judged all studies at high risk of performance bias as awareness of the intervention by participants and personnel may have affected their behaviour and hence the outcomes. When considering blinding of outcome assessors, we considered some outcomes to be more objective than others in the way they were measured.

Pedometer programme versus 'no intervention' control

For assessment of physical activity outcomes, we judged Dishman 2009 and Morgan 2011 to be at high risk of bias as they used self reporting. We judged Maruyama 2010 to be at low risk of bias as the authors collected physical activity data by uploading measurements electronically through a linkable pedometer.
Maruyama 2010 and Morgan 2011 also collected disease risk factor outcomes, and we judged these to be at low risk as they were objectively measured. One study collected quality of life outcomes, which we judged to be at high risk as it was self reported (Morgan 2011). One study collected adverse event outcomes, which we judged as low risk as they were independently obtained from the worksites (Morgan 2011).

Pedometer programme versus alternative programme without pedometer

We judged Talbot 2011 as having a high risk of bias for physical activity outcomes as they used self reporting, but low risk of bias for disease risk factor outcomes as they were objectively measured.

Incomplete outcome data

We judged all studies to be at high risk of bias for incomplete outcome data due to the high levels of attrition. Morgan 2011 suffered attrition rates ranging between 17% and 32% for each outcome; Dishman 2009 lost between 25% and 56% for each intervention group and site; Maruyama 2010 lost 35%; and Talbot 2011 lost 40%.

Selective reporting

Pedometer programme versus 'no intervention' control

We judged Morgan 2011 to be at low risk of bias as all the outcomes planned in their study protocol were reported. We judged the other two studies in this group to be at unclear risk due to the lack of available protocols (Dishman 2009; Maruyama 2010).

Pedometer programme versus alternative programme without pedometer

We judged Talbot 2011 to be at unclear risk due to the lack of a published protocol and four unreported outcomes.

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Other potential sources of bias

Pedometer programme versus 'no intervention' control

We judged Dishman 2009 to be at high risk due to the nature of their recruitment process. Although worksites were allocated at random, the nature of the programme offered in each specific site (both in the intervention and less active control worksites) would have been clear to individual participants prior to voluntarily enrolling. We judged Maruyama 2010 to be at high risk due to an imbalance in levels of physical activity between the intervention groups at baseline. We judged Morgan 2011 to be at low risk for this criterion.

Pedometer programme versus alternative programme without pedometer

We judged Talbot 2011 to be at high risk due to baseline imbalances in physical activity which made it difficult to distinguish the true improvements associated with either the pedometer or the alternative physical activity programme.

Effects of interventions

See: **Summary of findings for the main comparison** Workplace pedometer programs compared to 'no intervention' control for increasing physical activity; **Summary of findings 2** Workplace pedometer programs vs alternative physical activity program

We were able to undertake a meta-analysis for only a limited number of outcomes, due to differences in the outcome measures used between studies. No results were available for two secondary outcome categories (sedentary behaviour and disease risk scores), and no studies measured outcomes in the long term. In addition, some pre-planned analyses were not possible given the limited data available, including the assessment of publication bias using funnel plots, investigation of heterogeneity using subgroup analyses (e.g. age, gender, educational status) and sensitivity analyses based on risk of bias.

Pedometer programme versus 'no intervention' control

Primary outcome: Physical activity

The three studies in this group (Dishman 2009; Maruyama 2010; Morgan 2011) found inconsistent results for physical activity. We could not combine the results in a single meta-analysis because the studies differed too greatly in how they measured physical activity (see the 'Characteristics of included studies' table for details of the measures used). Where multiple measures of physical activity were available from the same study, we report the measures which were most direct and comparable.

- Maruyama 2010 measured the change in the number of pedometer steps recorded by participants during the study, and found no significant difference between the pedometer programme and the control (mean difference (MD) 649 steps per day over one week, 95% confidence interval (CI) -630.75 to 1928.75).
- Dishman 2009 measured walking, moderate and vigorous physical activity using 'metabolic equivalent of task' (METs) units, and found that, on average, those allocated to the pedometer programme were more vigorously active (MD 8.80 METs per week, 95% CI 3.95 to 13.65), more moderately active (2.70 METs, 95% CI 0.14 to 5.26) and walked more (3.60 METs, 95% CI 0.74 to 6.46) at the end of the programme compared to the control group. We re-analysed the data in this study to correctly account for clustering (Cochrane Handbook) using outcome-specific intraclass correlation coefficients (ICCs) provided by the study authors.
- Morgan 2011 measured overall physical activity during leisure time using METs, and found no statistically significant difference between the pedometer programme and the control (MD 0.30 METs, 95% CI -0.04 to 0.64). We obtained correct sample sizes through correspondence with the authors.

These results are not necessarily inconsistent with each other, but we cannot conclusively say that each study has demonstrated a positive effect of pedometer programmes.

Secondary outcome: Sedentary behaviour

None of the included studies reported sedentary behaviour as an outcome.

Secondary outcome: cardiovascular disease and type II diabetes risk factors

1) Anthropometric measures

Two studies (Maruyama 2010; Morgan 2011) reported change from baseline for body weight, body mass index and waist circumference. Where multiple anthropometric measures were available from the same study, we used the measures which were most direct and comparable.

- Maruyama 2010 and Morgan 2011 both reported that, on average, those allocated to the pedometer programme had a greater reduction in body mass index from baseline than those allocated to the control group. Maruyama 2010 found a MD of -0.48 kg/m², 95% CI -0.82 to -0.14, whereas Morgan 2011 found a MD of -1.40 kg/m², 95% CI -1.89 to -0.91 (Analysis 1.1). Morgan 2011 reported similar results after adjustment for socioeconomic position (-1.4 kg/m², 95% CI -0.9 to -2.0). Meta-analysis using a random-effects model revealed that on average, those in the pedometer programme reduced their body

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mass index by 0.92 kg/m² (95% CI -1.82 to -0.02; Analysis 1.1) more than the control group. There was high heterogeneity between the studies (I^2 = 89%).

- Morgan 2011 reported that, on average, those allocated to the pedometer programme had a greater reduction in waist circumference from baseline (MD -5.90 cm, 95% CI -7.56 to -4.24) than those allocated to the control group (Analysis 1.2). Maruyama 2010 also reported a greater reduction in waist circumference from baseline for those allocated to the pedometer programme, but the difference was not statistically significant (MD -0.80 cm, 95% CI -2.42 to 0.82). We refrained from entering results data for waist circumference into meta-analysis due to the very high level of heterogeneity between the two studies (I^2 = 95%). Hence we could not reach a firm overall conclusion on effects measured as waist circumference.

2) Blood pressure

Two studies reported change from baseline for systolic and diastolic blood pressure (Maruyama 2010; Morgan 2011). Meta-analyses showed that there were no significant differences between the pedometer programme and the control group for systolic blood pressure (MD -3.11 mmHg, 95% CI -8.39 to 2.17; Analysis 1.3) or diastolic blood pressure(MD -1.14 mmHg, 95% CI -3.45 to 1.16; Analysis 1.4).

- Morgan 2011 reported that on average, those allocated to the pedometer programme had a greater improvement in systolic blood pressure from baseline (MD: -6.00 mmHg, 95% CI -11.14 to -0.86) than those allocated to the control group (Analysis 1.3). Morgan 2011 reported similar results after adjustment for socioeconomic position (-6.0 mmHg, 95% CI -0.8 to -11.2). Maruyama 2010 reported a smaller improvement in change from baseline for those allocated to the pedometer programme, but the difference was not significant (MD: -0.60 mmHg, 95% CI -4.90 to 3.70). Meta-analysis using a random-effects model showed no statistically significant difference between the pedometer programme and the control group for systolic blood pressure (MD: -3.11 mmHg, 95% CI -8.39 to 2.17). There was moderate heterogeneity between the studies (I^2 = 60%).

- Maruyama 2010 and Morgan 2011 both reported no significant difference between the pedometer programme and the control group for diastolic blood pressure. Maruyama 2010 found a MD of -1.10 mmHg (95% CI -4.19 to 1.99) whereas Morgan 2011 found a MD of -1.20 mmHg (95% CI -4.67 to 2.27). Morgan 2011 was able to show a more precise, statistically significant effect after adjustment for socioeconomic position (-1.2 mmHg, 95% CI -2.4 to -4.7). Meta-analysis with a random-effects model using the unadjusted results confirmed that there was no statistically significant difference in diastolic blood pressure (MD: -1.14 mmHg, 95% CI -3.45 to 1.16; Analysis 1.4). There was minimal heterogeneity between the studies (I^2 = 0%), indicating little variation between the studies that cannot be explained by chance.

One study assessed resting heart rate (Morgan 2011) and reported that on average those allocated to the pedometer programme had a greater improvement in resting heart rate from baseline (MD -7.90 beats per minute, 95% CI -11.59 to -4.21) than those allocated to the control group. We hypothesised that resting heart rate would not change due to a low impact health programme. It is important to note that this benefit may be due to other factors such as increased participant calmness at the second round of data collection. Hence, the significant benefits for resting heart rate should be interpreted with caution.

3) Biochemical measures

One study reported change from baseline for a range of biochemical outcomes (Maruyama 2010). There was no significant difference for the average change from baseline between the pedometer programme and the control group for total cholesterol (MD -6.30 mg/dL, 95% CI -15.81 to 3.21), high-density lipids (MD -0.80 mg/dL, 95% CI -3.73 to 2.13), low-density lipids (MD -3.60 mg/dL, 95% CI -11.78 to 4.58) or triglycerides (MD -9.70 mg/dL, 95% CI -39.15 to 19.75).
On average, those allocated to the pedometer programme had a greater improvement in blood glucose from baseline (MD -4.80 mg/dL, 95% CI -9.14 to -0.46) than those allocated to the control group.

4) Disease risk scores

None of the included studies reported disease risk scores as an outcome.

Secondary outcome: Quality of life

One study (Morgan 2011) reported change from baseline for the physical and mental components of the 12-Item Short Form Health Survey. On average, those allocated to the pedometer programme had a greater improvement from baseline in the mental component score (MD 5.60 SF-12 units, 95% CI 1.87 to 9.33) than those allocated to the control group. There was no significant difference between groups in the physical component score (MD 2.80 SF-12 units, 95% CI -0.24 to 5.84).

Secondary outcome: Adverse effects

One study (Morgan 2011) compared worksite injuries in the 12 months before to the 12 months following programme implementation. On average, those allocated to the pedometer programme had fewer worksite injuries after the programme began (MD -0.30 injuries per person, 95% CI -0.52 to -0.08) than those allocated to the control group.

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Pedometer programme versus alternative programme without pedometer

Talbot 2011 compared a multi-component programme including a pedometer with an alternative physical activity programme including similar components without a pedometer. At baseline, the control group performed higher levels of moderate and very high intensity physical activity, making it difficult to interpret any observed differences in physical activity. For example, it has been suggested that a healthier, more motivated group may be more likely to attain the programme goal, leading the study to overestimate the health benefits of the programme in this group (Freak-Poli 2011). However, a group that was healthier and more active at baseline would also have less room to improve, thereby leading the study to underestimate the general health benefits of participation.

Primary outcome: Physical activity

Talbot 2011 found positive improvements in hard physical activity associated with the alternative physical activity programme that did not include a pedometer. The authors observed no differences between the pedometer programme and the alternative physical activity programme for total physical activity, very hard physical activity, moderate physical activity or pedometer steps immediately at the end of the programme. However, when baseline imbalances are considered, the pedometer programme may favour improvements in moderate activity and number of steps counted by the pedometer, although interpreting these results is not as straightforward as subtracting baseline from postintervention values, for the reasons outlined above.

- The authors found no statistically significant difference between the pedometer programme and the alternative physical activity programme in the number of steps counted by the pedometer (MD 224.00 steps per day over one week, 95% CI -954.79 to 1402.79) at the end of the programme. However, at baseline the alternative programme group undertook 885 more steps per day over the week than the pedometer programme group.
- The authors found no statistically significant difference between the pedometer programme and the alternative physical activity programme in physical activity measured either as total (MD -25.70 kcal/kg/wk or METs, 95% CI -54.72 to 3.32; baseline MD -21.2), very hard (MD -7.70 METs, 95% CI -18.40 to 3.00; baseline MD -8.4) or moderate physical activity (MD 1.40 METs, 95% CI -15.81 to 18.61; baseline MD -20.3) at the end of the programme. It is important to note that at baseline, those allocated to the alternative physical activity programme were undertaking 20.3 METs more moderate activity per week than those allocated to the pedometer programme. Hence, the pedometer programme might have increased moderate activity had there not been such a large baseline imbalance.
- On average, those allocated to the pedometer programme undertook 19.40 METs (95% CI 3.59 to 35.21) less hard physical activity at the end of the programme than those in the alternative physical activity programme. However at baseline, those in the pedometer programme were undertaking 11.99 hard activity METs more than those in the alternative programme. Hence, the magnitude of this result might have been smaller without such a baseline imbalance.

Secondary outcome: Sedentary behaviour

Talbot 2011 did not measure sedentary behaviour as an outcome.

Secondary outcome: cardiovascular disease and type II diabetes risk factors

1) Anthropometric measures

Talbot 2011 did not find a statistically significant difference between the pedometer programme and the alternative physical activity programme for body mass index at the end of the programme (MD -1.10 kg/m², 95% CI -2.86 to 0.66; baseline MD -1.3).

2) Blood pressure

Talbot 2011 did not find a statistically significant difference between the pedometer programme and alternative physical activity programme for blood pressure at the end of the programme (systolic MD -4.00 mmHg, 95% CI -10.15 to 2.15; diastolic MD -2.20 mmHg, 95% CI -6.74 to 2.34; baseline MD: systolic -5.5, diastolic -2.8).

3) Biochemical measures

Talbot 2011 did not find a statistically significant difference between the pedometer programme and the alternative physical activity programme for total cholesterol (MD: 6.70 mg/dL, 95% CI -8.64 to 22.04; baseline MD 2.4), low-density lipids (MD 0.60 mg/dL, 95% CI -15.08 to 16.28; baseline MD -2.1) or the total cholesterol to high-density lipid ratio (MD -0.24 TC:HDL-C ratio, 95% CI -0.79 to 0.31; baseline MD -0.39) at the end of the programme. However, the mean difference at baseline for low-density lipids was 2.1 mmHg between the groups in the opposite direction, and hence the effect might have been in favour of the alternative physical activity programme without this baseline imbalance.

On average, those allocated to the pedometer programme had higher high-density lipids (HDL-C; 7.50 mg/dL, 95% CI 1.55 to 13.45) than those allocated to the alternative physical activity programme at the end of the programme. However, the mean difference at baseline was 8.3 mg/dL between the groups in the same

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direction. Hence, this result may be due to baseline imbalances rather than the differences in the programme.
Triglycerides were included in the Talbot 2011 protocol, but the authors did not include these results in their study report.

4) Disease risk scores

Talbot 2011 did not measure disease risk scores as an outcome.

Secondary outcome: Quality of life

Talbot 2011 did not measure quality of life as an outcome.

Secondary outcome: Adverse effects

Talbot 2011 did not measure adverse effects as an outcome.

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ADDITIONAL SUMMARY OF FINDINGS [*Explanation*]

Workplace pedometer programmes vs alternative physical activity programme

Patient or population: Employees
Settings: In the workplace
Intervention: Workplace pedometer programmes
Comparison: Alternative physical activity programmes

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Alternative physical activity programs	Workplace pedometer programmes				
Physical activity Pedometer steps per week Follow-up: six months	The mean physical activity in the control group was **7478 steps**	The mean physical activity in the intervention groups was **224 higher** (954.79 lower to 1402.79 higher)		94 (1 study)	⊕⊕○○ **low**[1,2,3,4]	
Sedentary behaviour - not measured	See comment	See comment	Not estimable	-	See comment	The included study did not measure this outcome.
Body Mass Index (CVD and diabetes risk factor) Follow-up: six months	The mean body mass index in the control group was **29.8 kg/m²**	The mean body mass index in the intervention groups was **1.10 lower** (2.86 lower to 0.66 higher)		94 (1 study)	⊕⊕○○ **low**[1,4,5]	

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Systolic blood pressure (CVD and diabetes risk factor) Follow-up: six months	The mean systolic blood pressure in the control group was **124.5 mmHg**	The mean systolic blood pressure in the intervention groups was **4.00 lower** (10.15 lower to 2.15 higher)		94 (1 study)	⊕⊕○○ **low**[1,4,5]	
Low-density lipid (LDL) cholesterol (CVD and diabetes risk factor) Follow-up: six months	The mean LDL cholesterol in the control group was **112.6 mg/dL**	The mean LDL cholesterol in the intervention groups was **0.60 higher** (15.08 lower to 16.28 higher)		94 (1 study)	⊕⊕○○ **low**[1,3,4]	
Quality of life - not measured	See comment	See comment	Not estimable	-	See comment	The included study did not measure this outcome.
Adverse effects - not measured	See comment	See comment	Not estimable	-	See comment	The included study did not measure this outcome.

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% CI) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Random sequence generation and allocation concealment were unclear. The study did not successfully blind participants or personnel and had a high level of incomplete outcome data. Some planned outcome measures were not reported but the risk of bias was unclear as those reported included a mix of significant and nonsignificant results. The study had important baseline imbalances making the results difficult to interpret.
[2] The study did not use blinded or objective outcome assessment for this outcome.
[3] This result encompasses a range of possible effects including both significant benefit and harm.
[4] The sample size for this outcome is small.
[5] This result encompasses a range of possible outcomes including both significant improvement and negligible harm.

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DISCUSSION

Summary of main results

We included four studies in this review seeking to assess the effectiveness of pedometer interventions in the workplace for increasing physical activity and improving subsequent health outcomes. Overall, there is limited and low quality evidence of the health benefits associated with participation in these worksite pedometer health programmes. There were only limited data that we could assess via meta-analysis.

Three studies compared outcomes to what we considered to be a no-intervention control group. These three studies were heterogeneous in the age of recruited participants, the duration of programme, the multi-component nature of the health programme, and the timing of outcome assessment. All three studies reported results for physical activity outcomes but each used a different physical activity measure, and we therefore could not combine their results. While one study found a significant improvement in physical activity for the pedometer programme, we could not demonstrate a clear overall result. Those allocated to the pedometer programme had a greater reduction in body mass index than the control group, but there was no clear improvement in waist circumference or blood pressure. One study reported an improvement in fasting plasma glucose associated with a pedometer programme, but showed no differences in total cholesterol, high-density lipids, low-density lipids or triglycerides. One study reported quality of life outcomes and reported that those allocated to the pedometer programme had a greater improvement in the mental component score, but not the physical component score. One study reported a reduction in worksite injuries in those allocated to the pedometer group over a 12-month period.

One study compared a pedometer programme to an alternative physical activity programme including similar components without a pedometer. However, due to baseline imbalances it was difficult to distinguish the true effects associated with either programme.

Overall completeness and applicability of evidence

There is evidence that multi-component health promotion programmes that incorporate a pedometer improve physical activity and consequent risk markers in a range of programmes and settings. This review aimed to investigate whether introducing such programmes in a workplace setting would also be effective, or perhaps more so given the collegiate environment and availability of resources to support the programme. The results we found provide no evidence that the impact of pedometer programmes is likely to be reduced in a workplace setting. However, there are a number of limitations to the studies included in this review, and therefore we could not draw any firm conclusions. While the studies included broad working populations and used intervention designs that are likely to be generally applicable to the working population, there were limitations in the completeness of the available evidence. Firstly, most included studies used the pedometer as part of a multi-component programme, compared to no intervention. This makes it difficult to draw conclusions on the effectiveness of pedometers as a specific component to increase physical activity and improve subsequent health outcomes, due to the potential confounding effects of other intervention components. The one study that compared two multi-component interventions with and without a pedometer was affected by baseline imbalance, and the results remained difficult to interpret. Sufficient data were not available to explore the programmes' multiple components, nor other possible sources of heterogeneity including the age, gender and educational status of the participants, or the impact of risk of bias on the results. Secondly, the number of studies and participants in each study were limited, which reduced the strength and number of conclusions that we could draw. Some outcomes of interest were not measured (sedentary behaviour and disease risk scores), and no studies reported outcomes in the long term.

Quality of the evidence

Using the GRADE approach (Cochrane Handbook), we assessed the overall quality of evidence for most of our outcomes to be low or very low (see Summary of findings for the main comparison; Summary of findings 2). There was a high risk of bias due to high levels of attrition and lack of blinding. Many of the confidence intervals around the outcome estimates encompassed conflicting conclusions, including both the possibility of benefits but also negligible results or in some cases harm, and many outcomes were based on small sample sizes that limited the precision of the results. In the comparison with no-intervention controls, both physical activity and body mass index showed high levels of heterogeneity. The three studies included in this comparison recruited different populations in different settings using different recruitment methods. Given the limited research available, the possibility of publication bias is unclear.

Potential biases in the review process

While we attempted to minimise bias in the selection of studies, collection of published data, and analysis for the review, our searches were limited to electronic databases, and as a result we have only included published studies. In future updates of this review we will attempt to identify additional, unpublished data. At the time of this review, we were unable to obtain relevant unpublished data from one of the authors of the included studies. Further, assessment of selective outcome reporting was limited as only one protocol was identified for the included studies and only

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one study was listed on a trial registry database. In addition, due to limited data we were unable to assess publication bias using funnel plots. However, the studies that we identified were mixed in their results, both positive and negative, which leads us to believe that publication bias may not have affected our identification of studies significantly.

Agreements and disagreements with other studies or reviews

To our knowledge, only one other review has included health programmes including pedometers in a workplace context (Bravata 2007). In their meta-analysis of 27 pedometer-based programme evaluations involving randomised controlled trials and observational studies across a range of settings (five were in a workplace setting), Bravata 2007 indicated that on average such programmes increased step counts by 27%, decreased body mass index by 0.38 kg/m² and systolic blood pressure by 3.8 mmHg. However, they reported only small increases in physical activity associated with the five pedometer interventions within a workplace setting. In addition, they found that having the intervention in a setting other than the workplace was more beneficial. They attributed this outcome to the recruitment of staff who were already physically active, and suggest that workplace interventions may have a broader health benefit if sedentary employees were targeted. An alternative explanation is that there was limited evidence to synthesise in their meta-analysis as they included only five studies. In comparison, our review reported inconclusive results for physical activity.

AUTHORS' CONCLUSIONS

Implications for practice

There was insufficient evidence to assess the effectiveness of pedometer interventions in a workplace setting for increasing physical activity and improving subsequent health outcomes.

Implications for research

Given our results, alongside the findings by Bravata 2007 (as discussed in the Agreements and disagreements with other studies or reviews section), we believe there is a need for more high quality randomised controlled trials to assess the effectiveness of pedometer interventions in the workplace, to add to our knowledge of their effectiveness in other contexts. Future studies should allocate time and financial support to reducing attrition and thereby reducing incomplete outcome bias. Future studies should also think innovatively about blinding participants and personnel (especially those who undertake outcome assessment), for example through the use of active intervention controls to blinded participants (these could be minimal or full-scale, depending on the aims of the study), and regular outcome screening for all employees. Outcome assessments should be conducted by personnel not involved in implementing the interventions. Future studies should aim to report a core set of outcomes to improve the comparability of results across studies. We recommend this core set of outcomes to be: total physical activity in METs, total time sitting in hours and minutes, objectively measured cardiovascular disease risk factors (body mass index, waist circumference, blood pressure, resting heart rate), a measure of quality of life, and injury. The collection of biochemical measures (blood glucose, blood cholesterol (total, HDL-C, LDL-C), blood triglycerides) would be optimal, but the prioritisation of reduced attrition would be more beneficial to study quality. We also recommend that studies assess benefits in the long term and undertake subgroup analyses. Future studies should also prioritise the publication of protocols prior to data collection to reduce selective reporting.

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* *Indicates the major publication for the study*

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CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Dishman 2009

Methods	Cluster-randomised controlled trial **Aim:** To evaluate the efficacy of Move to Improve, a social-ecologic intervention delivered at the workplace to increase leisure-time physical activity
Participants	**Population description:** Employees of The Home Depot, Inc., based at divisional offices, subsidiaries, call centres, and distribution centres, none of which dealt directly with customers **Intervention group:** Eight worksites, 885 participants (mean cluster size 111, range 49 - 387) **Control group:** Eight worksites, 557 participants (mean cluster size 70, range 42 - 126) **Location:** USA (Arizona, California, Colorado, Florida, Georgia, Illinois, Louisiana, Maryland, Texas) and Canada (Toronto) **Inclusion criteria:** Employees without overt cardiovascular, pulmonary, or metabolic disease **Recruitment:** Twenty sites in diverse regions were identified as eligible for the study because they could be paired on number of employees and nature of work. Sixteen worksites agreed to participate, were paired and randomly assigned. Recruitment of volunteers within each worksite was performed via e-messages, onsite flyers, inter-office mail, face-to-face meetings, and posters developed and delivered by site co-ordinators who recruited and supervised team captains. All employees who completed baseline testing received an incentive (e.g. t-shirt, lunch cooler) **Demographics:** Age: range 19-64 years, mean (SD) 36.2 (9.8) years. Gender: 31% male. Ethnicity: white (60%), black (25%), Asian (3%), Pacific Islander or Native American (1%), or other (11%); 7% identified themselves as Hispanic or Latino Highest level of education: high school graduate (9%), some college or technical training (34%), associate degree (12%), bachelor degree (31%), postgraduate work or degree (14%) Job title: non-manager/supervisor (45%), supervisor (8%), manager/senior manager/director (12%), other (35%)
Interventions	A collaborative effort with the Building Better Health (BBH) program, a pre-existing health promotion programme operating at approximately 1700 Home Depot locations **Duration:** Two-month pre-intervention phase, followed by a 12-week intervention **Intervention** ***Pre-intervention phase:*** Project staff consulted with senior management at each worksite to discuss project objectives and to review the site selection criteria and expectations for participation. An employee was selected as a site co-ordinator during the first month of installation ***Intervention phase:*** Adapted from the Director's 50th Anniversary Physical Activity Challenge implemented at the CDC in Atlanta 1. Organisational action • Senior management endorsement received at the beginning of the project. Middle managers encouraged to support employee participation.

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	• Joint employee-management steering committees established at the sites, consisting of 8 - 10 employees, including a site co-ordinator and a representative from each major participating work unit. The committee met regularly during both phases and was responsible for implementation of the intervention components. • Site co-ordinators: received orientation, project requirement training, a Handbook that served as the implementation manual and weekly contact with project staff throughout both phases. 2. Goal setting • Individual and team goals were self set, specific regarding performance and time, challenging but realistic and attainable, and easily assessed. • Participant Handbook: detailed the components, benefits and incentives, participant responsibilities and timing of the intervention. It contained six sequential, bi-weekly tools to guide and assist the participant through the intervention: (1) goal-setting, (2) overcoming obstacles, (3) sedentary temptations, (4) avoiding relapse, (5) staying motivated, and (6) keep on moving. Each tool was a practical application of behaviour modification principles built around goal-setting theory. • *Personal goals:* graduated increases in the accumulation of 10-minute blocks of moderate-to-vigorous physical activity (MVPA) and pedometer (style Yamax SW 200) steps each week, evaluated and adjusted weekly. Targeted toward meeting or exceeding established public health recommendations for physical activity: ≥150 minutes each week of (MVPA and/or ≥10,000 pedometer steps each day. • *Team goals:* Employees formed teams, usually based on organisational and workgroup structures. Team size ranged from five to 20 members with a mean of nine members. Team captains were responsible for motivating participants to set goals and earn points for their team, while serving as liaisons between participants and site co-ordinators. Team captains collected work sheets including outcome measures and revised goals every two weeks. Posters that recorded and compared team goal attainment were displayed in break rooms and were updated every two weeks by the site co-ordinator. • *Organisational goals:* Established by the steering committee at each worksite. Participation objective at each worksite was 50% of all employees. Goal attainment objective was that 75% of participating employees would accumulate 150 minutes of MVPA and 10,000 steps per day, or both on at least nine of the 12 weeks during the intervention. 3. Incentives • Participants received a lunch bag if they completed the bi-weekly goal-setting and assessments until the six-week mid-point, and a programme t-shirt if they did so through all 12 weeks. • Each member of every team that had 75% of its members reach the goal attainment target received an embroidered "winning logo" t-shirt as an incentive. • Team captains received another incentive if their teams met this goal. • Site co-ordinators received incentives based on site participation and one site received a recognition plaque and a free catered lunch for employees having the greatest participation. 4. Environmental prompts • Signage that encouraged physical activity and its health benefits, emphasised the target goals for minutes and steps, and illustrated opportunities to be active, such as parking and walking, taking walk breaks, and climbing stairs.

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	• Posted throughout the worksite in places with high employee traffic (e.g. break rooms, bathrooms and points of decision such as elevators and stair wells). • Changed bi-weekly to vary the messages within the same themes. **Control:** Usual treatment control condition, including completion of the CDC health-risk appraisal and monthly newsletters describing the health benefits of physical activity. This provided a minimal treatment comparator for the intervention that has been shown to have modest or no effects on physical activity. Control sites had a programme director that dispensed monthly educational messages after baseline data collection
Outcomes	***Physical activity*** *:* • **International Physical Activity Questionnaire (IPAQ) short form.** Hourly participation each week in activities rated according to multiples of metabolic equivalent of task (METs) units. METs is a measure of energy consumption and one MET is equal to the energy produced at a standard resting metabolic rate obtained during quiet sitting (Ainsworth 2000). Can assess frequency and duration of moderate ($\geq$ 4 METs) and vigorous ($\geq$ 8 METs) physical activity and walking. Reliability and criterion validity judged against accelerometry is comparable to other self report measures. • **Number of people meeting US *Healthy People 2010 recommendations*** for moderate or vigorous physical activity. ***Other outcomes not reported in this review:*** • Perceived management support (Likert scale); • Employee involvement (Likert scale); • Physical activity diary and pedometer (style Yamax SW 200) steps (intervention group only); • Satisfaction, confidence, commitment and intention (1 - 4 scale, intervention group only).
Statistical analysis	**Imputation of missing data:** Latent growth modelling imputation and latent transition analysis were undertaken using full-information likelihood procedures for selected variables within those returning at follow-up. Imputation was not undertaken for those lost to follow-up. The imputed data were not used in this Cochrane review **Adjustment for clustering:** As there was no substantive difference in models after covariate adjustment using the Huber-White sandwich estimator procedure, unadjusted models were presented. Hence, raw data presented were not adjusted for clustering **Sample size calculation:** Authors reported that the sample size provided adequate statistical power for latent growth model tests
Notes	Supported by Health Protection Research Initiative grant DP 000111 from the CDC. Authors stated no other financial disclosures

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"16 worksites agreed to participate and were paired and randomly assigned" "Each of the paired sites was randomly assigned... to the intervention or a health ed-

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		ucation control condition using computer-generated random numbers. Based on past worksite intervention studies, the expected intervention enrolment and retention rates were approximately 35% and 50%, respectively, so recruitment goals at intervention and control sites were set at a 2:1 ratio."
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	No information provided. Although worksites were geographically dispersed and contamination was unlikely, it was unclear whether the usual treatment control group received additional information to their usual health promotion programmes. Additionally, recruitment bias may have occurred as it was unclear whether employees knew which allocation (intervention or health education control) they were offered
Blinding of outcome assessment (detection bias) Physical activity outcomes	High risk	All outcomes were self reported by the participants. Participants receiving the intervention would have been aware of goals set and the intention of the intervention, and may have been likely to overestimate outcomes
Incomplete outcome data (attrition bias) All outcomes	High risk	Initial allocations were 885 to the intervention, and 557 to the control. "Percent loss to follow-up ranged from 25% to 47% at intervention sites and 32% to 56% at control sites." Reasons for loss to follow-up were not given. The authors reported that in the control group, participants who were lost to follow-up had slightly higher baseline physical activity scores than those who remained
Selective reporting (reporting bias)	Unclear risk	All outcomes described in the published papers were reported. Study protocol was not available
Other bias	High risk	Following cluster-randomisation, recruitment of individuals in each worksite was voluntary. Volunteers were permitted to join the study after allocation and baseline measurement, at which point the nature of the intervention would have been clear. More participants joined the control group

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		(138) after baseline than the intervention group (23). Volunteers for the intervention may have been more motivated to change than volunteers for the less active control programme

Maruyama 2010

Methods	Randomised, cross-over study - first phase only. **Aim:** To investigate the effectiveness of a worksite-based programme, Life Style Modification Program for Physical Activity and Nutrition (LiSM10!®), on metabolic parameters in middle-aged male Japanese white-collar workers requiring health guidance based on regular health check-up results
Participants	**Population description:** Office workers belonging to the health insurance association of the Nichirei Group Corporation, aged 30 to 59 years **Intervention group:** 52 participants **Control group:** 49 participants **Location:** Tokyo and surrounding area, Japan **Inclusion criteria:** "male, office employees of the Nichirei Group Corporation, aged 30 to 59 years, with risk factors for developing metabolic syndrome, including one or more abnormalities involving serum lipids, glucose levels and blood pressure, with visceral obesity (umbilical circumference: 85 cm or more) and/or BMI ≥ 25. Abnormal levels were defined as: triglyceride (TG) ≥150 mg/dL and/or HDL-cholesterol (HDL-C) ≥40 mg/dL, systolic blood pressure ≥130 mmHg and/or diastolic blood pressure ≥85 mmHg, fasting glucose ≥110 mg/dL and/or HbA1c ≥5.5%." **Recruitment:** Individuals at risk were identified at regular medical check-ups conducted by the Tokyo Health Service Association (Shinjuku-ku, Tokyo), not involved in the study. 800 male employees were informed about the study, 319 showed interest in participating and agreed to use of their data. After a detailed explanation of the programme, 115 agreed to participate **Demographics:** Age: range 30 - 59 years, mean (SD) for control 35.5 (8.1), for intervention 43.1 (7.7) years Gender: 100% male.
Interventions	**Duration:** Four months **Intervention:** The LiSM10!® programme was designed to promote healthy dietary habits and physical activity 1. Professional contact/counselling • Monthly individual contact with a dietitian and a physical trainer, both certified health councillors for this programme. • Baseline: Twenty-minute session with dietitian, including self assessment of consumption of beneficial foods (Group A: fish, soy products, green/yellow/white vegetables, mushrooms/seaweed/konnyaku)) and foods recommended to be decreased (Group B: large servings of grains, confectionaries, sweet drinks, fatty meats, butter/margarine/dressing, eggs/liver, fried foods, pickles, soup, alcohol). The dietitian gave advice on the impact of consumption of each food group, and participants were assisted to identify and record an action plan to monitor and change dietary habits or

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	both, targeting specific foods based on stages of change theory. Ten-minute session with physical trainer, including discussion of baseline physical activity measurements. Participants were assisted to identify and record an action plan to increase physical activity based on pedometer steps or other lifestyle changes. • Months 1 and 2: Ten-minute session with each counsellor, who assisted the participant to review the month's achievements against their action plan, consider reasons for the results and effective strategies to improve, and if necessary revise the plan. • Month 3: Counsellors provided advice via the website described below, including review of progress and revision of goals. 2. Personal web page • Enter current weight; targeted food intake and physical activity; and upload data from computer-linkable pedometer (style Omron HJ-7101T) for self monitoring throughout the study. The data obtained were automatically presented in figures. • Discuss awareness of their lifestyles for self monitoring throughout the intervention period. • Family members and counsellors could make comments and note their impressions of the data on the self monitoring page. 3. Goal setting • Dietary action plans: In food group A, one, two and three items selected to be increased by 20 (38.5%), 24 (46.2%) and three (5.8%) participants, respectively. Top items were white vegetables, green/deep-yellow vegetables and mushrooms/seaweed/konnyaku. In food group B, one, two, three and four items selected to be decreased by 20 (38.5%), 19 (36.5%), three (5.8%) and two (3.8%) participants, respectively. Top items were confectionaries, alcoholic drinks, sweet drinks, large servings of grain and butter/margarine/dressing/mayonnaise. • Physical activity action plans: All participants decided to count steps. 32 (61.6%) decided to walk more than 10,000 steps daily. **Control:** No intervention
Outcomes	***Physical activity*** *:* • **Pedometer steps (average steps per day over one week).** A pedometer (Walking style HJ-7101T Omron Health Care Co., Ltd. Japan) was used to count the number of steps in a week. Participants wore the pedometer at the waist from the time they woke up until they went to bed. Control group participants were only given the pedometers for two periods (at baseline and at follow-up) of one week to allow outcome measurement. At the end of the week, pedometers were returned to the study staff. Intervention group participants periodically uploaded data from pedometer electronically via website. ***Anthropometrics*** *:* measured by Tokyo Health Service Association (Shinjuku-ku, Tokyo) , not involved in the study • **Body mass index (BMI**): Weight(kg)/height(m)^2. • **Waist circumference**: cm, umbilical circumference measured during the late exhalation phase in the standing position. • **Blood pressure**: mmHg, measured using an automatic blood pressure manometer with the subject in the seated position. ***Biochemical measures:*** Fasting, obtained and blinded measurements were conducted in the laboratory of the Tokyo Health Service Association (Shinjuku-ku, Tokyo), not involved in the study

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	• Total cholesterol (TC): mg/dL, enzymatic method; • High-density lipids (HDL): mg/dL, direct method; • Low-density lipids (LDL): mg/dL, Friedewald equation; • Triglycerides (TG): mg/dL, enzymatic method; • Fasting plasma glucose (PG): mg/dL, hexokinase-UV method. ***Other outcomes not reported in this review:*** • **Additional biochemical measures:** HbA1c: %, enzymatic method; fasting insulin: μU/L, chemiluminescence immunoassay; insulin resistance, homeostasis model assessment (HOMA-IR), calculated as PG (mg/dL) × insulin (IRI) (μU/L) 405); aspartate aminotransferase (AST), alanine aminotransferase (ALT) and gamma-glutamyl transferase (γ-GTP): IU/L, UV and L-γ-glutamyl-3-carboxy-4-nitroanilide substrate methods; uric acid: mg/dL, uricase method; • **Food intake:** Current targeted food intake entered on a website for self monitoring; • **Lifestyle:** A questionnaire on lifestyle, habitual food intake, the stages and self efficacies of changes in their habitual food intakes and efforts to increase physical activity.
Statistical analysis	**Imputation of missing data:** Not reported to have been undertaken by authors **Adjustment for clustering:** Adjustment not required as participants were recruited individually **Sample size calculation:** "Sample size was calculated to detect the intervention effect of a 10% change within the group and between groups, using 0.05 for the alpha and 0. 20 for the beta error. The necessary sample size was 45 subjects in each group."
Notes	"This study was supported by a grant from the International Life Sciences Institute Japan. We appreciate the collaborative efforts of the Meiji Dairies Corporation, Suntory Holdings Limited and Nichirei Foods Inc." Authors stated no financial interest in the subject matter, materials, or equipment

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	"A randomization code with equal numbers of alternative groups was generated from a list of all participants, using software SPSS (ver.15) at Waseda University."
Allocation concealment (selection bias)	Low risk	"The Nichirei Inc staff members managing the study and contacting participants were not involved in this randomization process. "
Blinding of participants and personnel (performance bias) All outcomes	High risk	"…as the participants received detailed explanations of the objectives and other aspects of this study, blinding to group assignments was not possible." No information was given about blinding of personnel,

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		but it is unlikely that this was undertaken Awareness of the purpose of the study may have led control group participants to behave differently during the study, which may have affected the outcomes
Blinding of outcome assessment (detection bias) Physical activity outcomes	Low risk	Participants in the intervention group uploaded pedometer data electronically via a website. Control group participants returned the pedometer to study staff, and it is likely that results were electronically recorded. Due to electronic linkable pedometers, the uploading of incorrect pedometer steps are unlikely to be influenced by the lack of blinding
Blinding of outcome assessment (detection bias) Disease risk factor outcomes	Low risk	No additional information was given about blinding of outcome assessors. Anthropometric and blood test results are also objectively measured and unlikely to be affected
Incomplete outcome data (attrition bias) All outcomes	High risk	Of 52 participants randomised to the intervention, four were lost to follow-up, and an additional four did not provide postintervention pedometer steps. Of 49 participants randomised to the control, two were excluded after baseline due to abnormal blood tests indicating possible hyperlipidaemia, eight were lost to follow-up, and an additional 17 did not provide pedometer steps The authors did not conduct intention-to-treat analysis. The reasons for most missing data were not available. For all outcomes, attrition is likely to be large enough to affect the observed results
Selective reporting (reporting bias)	Unclear risk	All outcomes described in the published papers were reported. Study protocol was not available
Other bias	Unclear risk	The control group walked on average around 1,000 fewer steps per day at baseline than the intervention group. This difference could indicate that the intervention group was already at a high level of activity and less likely to achieve significant increases (less room to move), or it could indicate a highly active or motivated group

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		who were more likely to achieve significant increases

Morgan 2011

Methods	Cluster-randomised controlled trial **Aim:** To evaluate the feasibility and efficacy of a workplace-based weight loss programme that targeted overweight and obese male shift workers
Participants	**Population description:** Overweight or obese male adult shift workers at Tomago Aluminium, one of Australia's largest producers of aluminium, employing around 1200 staff **Intervention group:** Two clusters of multiple crews (15 crews across both groups), 65 participants **Control group:** Two clusters of multiple crews, 45 participants **Location:** The industrial suburb of Tomago, 13 km northwest of Newcastle, New South Wales, Australia **Inclusion criteria:** Overweight or obese (BMI between 25 and 40 kg/m²) men aged 18 - 65 from Tomago Aluminium without a history of major medical problems such as heart disease in the last five years, diabetes, orthopaedic or joint problems that would be a barrier to physical activity, recent weight loss of ≥ 4.5 kg, or taking medications that might affect body weight **Recruitment:** Individual recruitment via a staff email and through promotion at crew meetings, by crew leaders and health staff. Participating crews were allocated to clusters based on the timing and rotation of shifts worked, to avoid contamination within the worksite **Demographics:** Age: range 18-65 years, mean (SD) 44.4 years (8.6) years. Gender: 100% male. Socio-Economic Indexes for Areas (SEIFA, based on residence postcode): 1 - 2 (lowest category) 7.9%, 3 - 4 18.0%, 5 - 6 52.8%, 7 - 8 18.0%, 9 - 10 (highest) 3.4%
Interventions	**Duration:** 3.5 months (14 weeks) **Intervention:** The Workplace POWER (Preventing Obesity Without Eating like a Rabbit) programme is based on Social Cognitive Theory and behaviour change strategies. Adapted from a previous internet-based weight loss programme, 'SHED-IT' 1. Professional contact/counselling • Information session by male researcher: 1 x 75-minute face-to-face session. • 60 minutes covered education about energy balance, the challenges of shift work relating to diet and physical activity, weight loss tips, and behaviour change strategies including self monitoring, goal setting and social support. • 15-minute technical orientation during information session to familiarise and teach participants how to use the website. 2. Personal web page • Study website: publicly accessible, free weight loss site http://www.calorieking.com.au. Weekly enter weight, submit online daily eating and physical activity diaries for the first four weeks, for two weeks in the second month and for one week in the third month. • Website user guide. • Weight loss handbook.

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	• Pedometer (style Yamax SW 200). 3. Feedback • Website data given in seven weekly individualised feedback documents via email over the three months from the research team. • Each sheet gave weekly summary of results, suggested personalised strategies to address weight loss, reduce energy intake and increase energy expenditure. • A research team email was available for questions, which were answered weekly by two research assistants with qualifications in health and physical education or nutrition and dietetics. 4. Incentives • Group-based financial incentive. • The crews with the highest mean percentage weight loss after one month and at the conclusion of the programme were given a AUD 50 gift voucher per person to be spent at a local sporting equipment store. **Control:** Received the intervention at 14 weeks (wait-list control group)
Outcomes	***Physical activity*** *:* • **Leisure-time physical activity.** Self reported. Measured using a modified version of the Godin Leisure-Time Exercise Questionnaire. "*How many times per week do you engage in strenuous, moderate, and mild physical activity for a minimum of 10 minutes per session?*" The total leisure activity score was calculated by: (N·MET) moderate + (N·MET) strenuous +(N.MET) mild where N = (number of bouts per week lasting ≥ 10 minutes multiplied by the time in minutes) for each category. • **'Workday' and 'usual' physical activity**. Self reported. "*(i) How much do you incorporate physical activity into your workday (during breaks, active commuting to and from work)?"* scored on a five-point scale from 1) none to 5) a great deal; and "*(ii) Is the amount of activity you did in the past month less, more, or about the same as your usual physical activity habits?*" scored from 1) I am now much less active to 5) I am now much more active. ***Anthropometrics*** *:* • **Body weight**: kg, measured with men wearing light clothing, without shoes on a digital scale to 0.1 kg (Model no. UC-321PC, A&D Company Ltd., Tokyo, Japan). • **Body mass index (BMI**): Weight(kg)/height(m)^2, measured to 0.1 cm using a stadiometer (model KaWe 44440; Medizin Technik, Mentone Education Centre, Morrabin, Australia). • **Waist circumference**: cm, measured level with the umbilicus with a non-extensible steel tape (KDSF10-02, KDS Corporation, Osaka, Japan). • **Blood pressure and resting heart rate:** mmHg and beats per minute, measured using a NISSEI/DS-105E digital electronic blood pressure monitor (Nihon Seimitsu Sokki Co. Ltd.,Gunma, Japan). ***Quality of life*** *:* • **Health-related quality of life:** 12-Item Short Form Health Survey physical and mental scales. ***Adverse effects:*** • **Injuries at Work:** On-site incident and injury recording system at Tomago Aluminium for the 12-month period before and after programme commencement. All work-related injuries are reported by employees and recorded in an electronic database. Injuries can be the result of a single workplace exposure or event, or the result of a cumulative exposure over time. Injuries excluded were those that occurred on the

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	journey to and from work. ***Other outcomes not reported in this review:*** • **Selected dietary variables:** Specific foods (fruit, vegetable, bread) and beverages (milk, cola, soda, diet and alcohol). • **Physical activity and dietary cognitions:** Self ef fi cacy, pros and cons, behavioural intention, attitudes, stage of change. • **Daytime sleepiness:** Epworth sleepiness scale, which is a valid measure of general daytime sleepiness. • **Workplace productivity or presenteeism:** Work Limitations Questionnaire (WLQ) short-form (the degree to which health problems interfere with the performance of job tasks and to estimate the related productivity loss. The WLQ generically assesses presenteeism, is validated and highly reliable). • **Absenteeism:** Personal illness or non-work-related injury were recorded in an electronic database, presented as hours of leave. Carer's leave was excluded from the analysis. Absences for the three-month period before and after programme commencement. • **Feasibility:** Recruitment (achievement of target sample size), retention (retention rates at follow-up) and attendance (at information sessions). • **Adherence to self monitoring:** Calculated from website usage data. • **Research team emails:** Frequency and topic
Statistical analysis	**Imputation of missing data:** "Analyses were performed using PASW Statistics 17 (SPSS Inc. Chicago, IL)." "Mixed models were used to assess all outcomes (primary and secondary) for the impact of group (Intervention and Control), time (treated as categorical with levels baseline and 14 weeks) and the group-by-time interaction, these three terms forming the base model. This approach was preferred to using baseline scores as covariates, as the baseline scores for subjects who dropped out at 14 weeks were retained making this an intention-to-treat analysis." The intention-to-treat analyses reported are used in this Cochrane review **Adjustment for clustering:** "To examine potential clustering of effects at the crew level, crew was nested within both the treatment and treatment-by-time terms as fixed effects and these terms were used in the final models." These adjusted results are used in this Cochrane review **Sample size calculation:** "Based on 90% power to detect a significant weight loss (primary outcome) difference between groups of 3 kg, assuming SD=5 (P=0.05, two-sided) , and a correlation between pre and post scores r=0.80, a sample size of 41 participants for each group was needed."
Notes	"Funding Source: This study was funded by Tomago Aluminium and the Hunter Medical Research Institute. Tomago had no involvement in study design, analysis and interpretation of data or the decision to submit the manuscript for publication. Simon Mitchell from Tomago reviewed the drafted manuscript for accuracy and also organised the data collection at Tomago." "Ethics approval was obtained from the University of Newcastle Human Research Ethics Committee and the project was supported by Tomago Aluminium management." No other potential conflict of interests stated by the authors Australian New Zealand Clinical Trials Registry Number: ACTRN12609001003268

Risk of bias

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Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	"The random allocation sequence was generated by a computer-based random number-producing algorithm to ensure an equal chance of work crews being allocated to each group, without restriction." "As crews were randomly allocated based on crew shift clusters, we had an uneven number of men in intervention and control conditions."
Allocation concealment (selection bias)	Low risk	"To ensure concealment, the sequence was generated by a statistician. Randomization and participant study arm assignment was completed by a researcher who was not involved in the assessment of participants and the allocation sequence was concealed when enrolling participants by work crew. Participants were enrolled by Health Services staff at Tomago."
Blinding of participants and personnel (performance bias) All outcomes	High risk	"Men were randomly allocated to one of two groups: the Workplace POWER (Preventing Obesity Without Eating like a Rabbit) programme or a 14-week wait-list control group. Men worked in crews (n = 15) and were randomly allocated in four crew clusters based on the timing and rotation of shifts worked, to avoid contamination within the worksite." It is unclear whether participants in the control group knew if they were allocated to the 14-week wait-list control group. The waiting period may have given opportunity to change their behaviour in anticipation of the programme commencement Some personnel would have been aware of participants' allocation through provision of tailored support during the trial
Blinding of outcome assessment (detection bias) Physical activity outcomes	High risk	Physical activity outcomes were self reported. It is possible that participants in the intervention group, knowing that the intervention had begun and the direction of expected change, may have exaggerated activity levels, although obvious outliers were excluded from the analysis

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Blinding of outcome assessment (detection bias) Disease risk factor outcomes	Low risk	"Participants and assessors were blind to group allocation at baseline assessment." It is unclear who performed these outcome measures, and whether they were blinded at follow-up; however these outcomes are sufficiently objective to present a low risk of bias
Blinding of outcome assessment (detection bias) Quality of life outcomes	High risk	Quality of life outcomes were self reported. It is possible that participants in the intervention group, knowing that the intervention had begun and the direction of expected change, may have reported more favourably to quality of life outcomes. However, quality of life may not be perceived by participants as a direct outcome of this intervention, and hence, may not be as vulnerable to bias as an outcome directly related to the intervention goals, such as physical activity
Blinding of outcome assessment (detection bias) Adverse event outcomes	Low risk	"Injury data were sourced from an on-site incident and injury recording system at Tomago Aluminium for the 12-month period before and after programme commencement."
Incomplete outcome data (attrition bias) All outcomes	High risk	One hundred and ten participants were randomised (65 Intervention, 45 Control) . Nineteen were lost to follow-up (10 Intervention, nine Control) due to unavailability for testing and one due to employee termination (Intervention). Reasons for unavailability were not described "Six men were identified as outliers in the total MET minutes variable and were omitted from the physical activity analyses as their reported physical activity levels were not plausible." In the physical activity analyses, only 75 participants were included (allocation and reasons for additional exclusions not stated) For anthropometric analyses, the authors undertook a mixed model approach "as the baseline scores for subjects who dropped out at 14 weeks were retained making this an intention-to-treat analysis." Imputation of follow-up scores for missing participants

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		was not described At least 18.2% of the baseline sample were missing, but in some analyses up to 31.8% were missing, with the balance between intervention and control group and the reason unclear. This level of incomplete data is enough to pose a high risk to the results
Selective reporting (reporting bias)	Low risk	All outcomes described in the published papers and trial registration were reported at three months. Trial registry data stated that outcomes would be measured six-month follow-up, and that the wait-list control group would be offered the intervention immediately following that point. However, the study reported that wait-list control received the intervention at 14 weeks, and no results at six months were reported, so we assumed that six-month data collection was not undertaken. Despite this, the results reported at three months remain at low risk
Other bias	Low risk	As cluster-randomisation was utilised for a small number of clusters, there is an increased chance of baseline imbalance between the randomised groups in terms of either the clusters or the individuals. The use of a wait-list control is likely to have minimised the risk of selection bias within clusters. In addition, appropriate adjustment for clustering in statistical analyses was undertaken

Talbot 2011

Methods	Randomised controlled trial **Aim:** To compare the effects of a pedometer-based behavioral intervention (Fitness for Life, FFL) and the traditional army physical fitness (TRAD) programme on physical activity, aerobic fitness, and chronic heart disease risk factors in healthy adult men and women in the Army National Guard (ARNG) who had failed the Army Physical Fitness Test (APFT)
Participants	**Population description:** Volunteer, part-time Army National Guard (ARNG) members, who had failed the two-mile run component of the Army Physical Fitness Test **Intervention group:** 84 participants **Control group:** 72 participants **Location:** Maryland and Washington, DC, USA. **Inclusion criteria:** Volunteer part-time Army National Guard members who had failed

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	the two-mile run component of the Army Physical Fitness Test (APFT), with 12 months or longer before re-enlistment or retirement, and who had no history of CHD or stroke, were not currently taking hypertensive or cholesterol-lowering medications, had not been pregnant within the previous six months, were not post-menopausal or currently taking hormone replacement therapy, or had no major musculoskeletal disorders. Post-menopausal women and women on hormone replacement therapy were excluded owing to their small numbers and potential confounding effect on serum lipids **Recruitment:** From a pool of 261 ARNG soldiers who failed the run portion of the APFT, 156 met the criteria for inclusion in the study and were randomised into two groups **Demographics:** Age: Mean (SD) for intervention 32.7 (10.1); for controls 32.8 (8.3) years Gender: 68.8% male for intervention, 80.4% for controls. Race: Intervention 50% white, 39% African-American, 7% Asian/Pacific Islander. Controls 31% white, 50% African-American, 7% Asian/Pacific Islander Education: Intervention 27% high school graduate, 50% some college, 7% college graduate, 8% some postgraduate, 8% advanced degree. Controls 31% high school graduate, 50% some college, 2% college graduate, 15% some postgraduate, 2% advanced degree
Interventions	**Duration:** 24 weeks (12 weeks of conditioning and 12 weeks of maintenance) **Intervention:** The Fitness for Life (FFL) programme was designed specifically for the reserve components of ARNG and Reserve to teach soldiers usually working a full-time civilian job and a part-time military job to incorporate moderate intensity physical activity (PA) into their daily lives 1. Professional contact/counselling • Counselling sessions: discussed various activities to increase their daily step count. • Weeks one to four: During brief weekly telephone counselling (< five minutes), pedometer logs were reviewed and feedback provided. • Weeks five to 12: Weekly booster telephone calls, monthly support meetings. • Weeks 13 to 24: Monthly maintenance meetings were continued; telephone calls were tapered to every two weeks, then monthly to increase autonomy. • Monthly group meetings: held to provide support, emphasise relapse prevention, and encourage self monitoring of steps. 2. Goal setting • Pedometer (style Yamax SW 200) worn for self monitoring of their daily steps. Central focus used to motivate and monitor steps through setting step goals, maintaining a daily step log, and promoting activities to increase steps. • Weeks one to four: focused on accumulating daily steps through short bouts of walking combined with behavioral-based PA counselling. • Weeks five to 12: focused on increasing the intensity of soldiers' activities. Soldiers were taught to rate their perceived exertion while performing moderate-to-high intensity daily activities in their target heart rate range, defined as 60 to 90% of predicted maximum heart rate, calculated as 220 − age. Using the Rating of Perceived Exertion scale, participants gauged the intensity of their activities by means of feedback from their target heart rate. The pedometer continued to be the central focus of behavioral strategies for setting step goals. • Weeks 13 to 24: focused on sustaining gains in the amount of steps and intensity of PA. Participants were expected to continue using the pedometer to monitor their PA. Relapse prevention, self monitoring, and reinforcement were continually

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	emphasised during the maintenance phase. 3. Environmental prompts • Motivational postcards: mailed weekly (weeks one to four) and then bi-weekly to suggest various ways to increase steps. 4. Feedback • Through counselling sessions. • Taught how to gauge the intensity of their activities by means of feedback from their target heart rate. **Control:** The army physical fitness (TRAD) programme follows Army Regulation 350-41, with recommendations detailed in the army's Field Manual 21-20. The programme consists of 12 weeks of high-intensity conditioning, defined as 75 to 80% of maximum heart rate and 12 weeks of maintenance 1. Professional contact/counselling • A Master Fitness Trainer, an ARNG soldier who had completed a two-week reserve component training course, oversaw the 12-week training programme. • 60-minute briefing. • A brief reminder call was made before each monthly meeting. • Monthly group meetings. 2. Goal setting • Instructed to perform vigorous physical fitness training three to six days per week, including three 30-minute sessions of aerobic training and three 30-minute strength training sessions, performed unsupervised separately or combined, during their normal work day or during leisure time. • Supplementary booklet on the TRAD.
Outcomes	***Physical activity*** *:* • **The seven-day physical activity recall (PAR) interview.** Amount of time spent asleep (1.0 metabolic equivalent of task (METs) units) and in moderate (four METs), hard (six METs), and very hard (10 METs) intensity physical activity for the previous weekdays and weekend. The seven-day PAR is moderately correlated with an accepted standard measure for cardio-respiratory fitness, the VO_2 max which is the maximum capacity of an individual's body to transport and use oxygen during incremental physical activity. It was assumed that they spent the remaining time in light activities (1.5 METs). To estimate energy expenditure per week, the average number of minutes at each activity level was multiplied by the respective MET value for an estimate of light, moderate, hard, and very hard physical activity in kcal/kg. *Total physical activity* was the sum of *Moderate Intensity*, *Hard Intensity* and *Very Hard* Intensity physical activity because both interventions were designed to increase these three activity levels, but not *Low Intensity*. • **Pedometer steps (assumed to be: average steps per day over one week).** A pedometer (Digiwalker, Yamax SW 200; New Lifestyles, Lees Summit, MO) was used to count the number of steps in a day, which were recorded in a daily pedometer log. This pedometer's accuracy is within 1% of the actual step count on a 4.88 km sidewalk course. Participants wore the pedometer at the waist from the time they woke up until they went to bed. Control group participants were only given the pedometers for two periods (at baseline and at follow-up, length of testing time unknown) to allow outcome measurement. At the end of the week, pedometers were returned to the study staff. Intervention group participants retained the pedometer throughout the intervention

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	Anthropometrics : • **Weight and height:** Measured using a digital scale, with participants in gym shorts and t-shirt without shoes. • **Body mass index (BMI**): Weight(kg)/height(m)^2. • **Waist circumference**: Measured during the late exhalation phase in the standing position. • **Blood pressure**: Measured using an automatic digital monitor (Model 6009; American diagnostic, Tokyo, Japan) on dominant arm at heart level while participants were seated. Three measurements were taken at one- to two-minute intervals, and the mean of the two closest readings was reported. Cuff sizes reflected the circumference of the participant's arm. Extreme values were checked by trained personnel, who repeated the digital recording and then recorded blood pressure manually. ***Biochemical measures:*** Fasting venipuncture from the anterior cubital fossa. The samples were allowed to clot at room temperature, then centrifuged at 3,000 rpm for 15 minutes, and the resulting serum was removed and stored at −80°C until analysis. The lipid panel was analysed using the Cholestech LDX system analyser (Cholestech, Hayward, CA), with a sensitivity of 0.8%. All assays were conducted at Johns Hopkins Bayview Campus in the General Clinical Research Center • Total cholesterol (TC); • Triglycerides; • Low-density lipoprotein cholesterol (LDL-C); • High-density lipoprotein cholesterol (HDL-C); • TC:HDL-C ratio were calculated. ***Other outcomes not reported in this review:*** • **Army Physical Fitness Test (APFT):** a standardised measure of cardiorespiratory fitness and muscular endurance according to standardised protocols detailed in Chapter 14 of the Army Field Manual 21-20. This is a three-event physical performance test consisting of the number of standard Army push-ups performed in two minutes; the number of standard Army sit-ups performed in two minutes; and the time to complete a two-mile run.The APFT scoring is a normative-based scale based on age and gender.
Statistical analysis	**Imputation of missing data:** "We used expectation-maximization for imputation estimates of missing data in the group of protocol completers, with SPSS Missing Value Analysis 16.0. The missing data for individual variables ranged between 1% and 19%. Missing data were determined to be missing at random, meeting expectation-maximization assumptions." Hence, expectation-maximisation was undertaken within those returning at follow-up. Imputation was not undertaken for those lost to follow-up **Adjustment for clustering:** Adjustment not required as participants were recruited individually **Sample size calculation:** "Based on the predicted effect of the intervention with projected 40% attrition, we estimated a total sample size of 156 ARNG soldiers to demonstrate a 10% improvement in APFT scores (effect size, d = 0.65) and PA (effect size d = 0.56) at an α of 0.05 and a power of 0.80."
Notes	"We also acknowledge the Johns Hopkins Bayview General Clinical Research Center (which is funded by Department of Health and Human Services, National Institutes of Health [NIH], National Center for Research Resources, no. 5 M01 RR0279) for providing core laboratory and data management support and equipment, and the Intramural Research Program of the NIH, National Institute on Aging. Funding for this project

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	was provided by Triservice Nursing Research Program, Johns Hopkins Bayview General Clinical Research Center, the Intramural Research Program of NIH, National Institute on Aging." No other potential conflict of interests stated by the authors	
Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Study described as a "randomized controlled trial". No further information given on the method used to generate the random sequence
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding of participants and personnel (performance bias) All outcomes	High risk	No information provided. It is unlikely that personnel delivering the specific intervention programmes would have been blinded. Participants may have been unaware of the differences between programmes on allocation, but being colleagues in the National Guard, it is possible that communication among participants would have occurred that identified the nature of each intervention Although both interventions were active physical training programmes, it is possible that participants may have had a preference for the traditional training programme (control) over the new (pedometer-based) intervention, perhaps contributing to the higher drop-out rate in the pedometer group
Blinding of outcome assessment (detection bias) Physical activity outcomes	High risk	Physical activity outcomes were self reported. It is possible that participants in either study group, knowing that the intervention had begun and the direction of expected change, may have exaggerated activity levels. Although this applies to both groups, it may have applied more in one group than another. For example, if participants had stronger belief in the effectiveness of one programme over another (e.g. traditional versus new intervention), or depending on participants' experience of the intervention. Participants in the pedometer arm may have felt greater pressure to report

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		increased steps per day. Participants in the control arm may have felt greater pressure to report increased periods of higher-intensity physical activity
Blinding of outcome assessment (detection bias) Disease risk factor outcomes	Low risk	No information is provided on blinding of assessment of these outcomes, but they are sufficiently objective to make detection bias unlikely
Incomplete outcome data (attrition bias) All outcomes	High risk	One hundred and fifty-six met the criteria and were randomised. 62 (40%) were lost to follow-up (at 12 weeks: 33 Intervention, 19 Control; at 24 weeks: three Intervention, seven Control) due to deployment (15), left ARNG (21), personal reasons (22), injury not related to the study (three), passing Army Physical Fitness Test (one). Reasons were provided, but not for each group "ARNG soldiers who dropped out of the training programme had lower baseline APFT scores, suggesting that less fit individuals require more motivation to complete such a program". This indicates the possibility that data were not missing at random Results were reported for participants remaining for the entire study. "We used expectation-maximization for imputation estimates of missing data in the group of protocol completers", but data were not imputed for participants who withdrew At least 40% of the baseline sample was missing and the reasons were not very specific. This level of incomplete data is enough to pose a high risk to the results
Selective reporting (reporting bias)	Unclear risk	Results for some outcomes measured were not reported (total physical activity, heart rate, triglycerides, very low density lipoprotein), but the reported results included a range of significant and nonsignificant effects, favouring both programmes. It is unlikely that the outcomes omitted were selected based on their results. Study protocol was not available

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Other bias	High risk	Baseline data were reported separately for participants who completed or withdrew from the study, so it is difficult to assess the true levels of baseline imbalance. Of those who completed the study, those in the intervention group performed lower levels of moderate and very high intensity physical activity at baseline, which could indicate that the control group was already at a high level of activity and less likely to achieve significant increases (less room to move), or it could indicate a highly active or motivated group who were more likely to achieve significant increases. A number of smaller baseline imbalances were also observed in other outcomes

CDC: Centers for Disease Control; CHD: coronary heart disease; MET: metabolic equivalent of task; MVPA: moderate-to-vigorous physical activity; PA: physical activity

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Aittasalo 2004	Pedometers were not used throughout the intervention period.
Bassey 1983	Pedometers were not used throughout the intervention period. Controls also received a pedometer
Brehm 2011	Pedometers were not used throughout the intervention period. No physical activity measured as an outcome
Brooke-Wavell 1996	Pedometers were not used.
Della 2010	Controls also received a pedometer.
Erfurt 1991	Pedometers were not used. No physical activity measured as an outcome
Furukawa 2003	Accelerometers were used. Controls also received an accelerometer. Participants could not view step count.
Gilson 2007	Controls also received a pedometer.
Gilson 2008	Pedometers were not used throughout the intervention period.
Hultquist 2005	Not in a workplace setting. Participants could not view step count.

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(*Continued)*

Härmä 1988a	Pedometers were not used.
Härmä 1988b	Pedometers were not used.
Iwane 2000	Controls also received a pedometer.
Johannesson 2010	Controls also received a pedometer. No physical activity measured as an outcome
Kennedy 2007	Pedometers were not used.
Lee 1997	Pedometers were not used.
Mackey 2011	Controls also received a pedometer.
Molde 2003	Not in a workplace setting. Pedometers were not used.
Moreau 2001	Not in a workplace setting.
Motl 2005	Not in a workplace setting. Pedometers were not used.
Murphy 2006	Pedometers were not used throughout the intervention period. Controls also received a pedometer
Mutrie 2002	Pedometers were not used.
Naito 2008	Pedometers were not used throughout the intervention period.
Oja 1991	Not in a workplace setting. Pedometers were not used.
Polzien 2007	No physical activity measured as an outcome.
Puig-Ribera 2008	Controls also received a pedometer.
Serwe 2011	Controls also received a pedometer.
Slootmaker 2009	Accelerometers were used.
Speck 2001	Controls also received a pedometer.
Sternfeld 2009	Not in a workplace setting.
Terry 2010	Pedometers were not used.
Torstensen 1998	Not in a workplace setting. Pedometers were not used.
Tucker 2011	Participants could not view step count.
Tudor-Locke 2004a	Not in a workplace setting.

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(*Continued*)

Van Berkel 2011	Pedometers were not used. Accelerometers were used.
Vincent 2009	Not in a workplace setting.
Wing 1996	Pedometers were not used.

Characteristics of studies awaiting assessment *[ordered by study ID]*

Adams 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Ainsworth 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Aittasalo 2012

Methods	Randomised controlled trial **Aim:** To evaluate a six-month intervention to promote walking in office workers using pedometers and email messages
Participants	**Population description:** Voluntary and insufficiently physically active employees at 20 office-based worksites. Worksite specifics not described **Intervention group:** 123 participants **Control group:** 118 participants **Location:** Southern Finland **Inclusion criteria:** Respondents to the baseline questionnaire were eligible if they volunteered for the study and were insufficiently physically active for cardio-respiratory health (less than 150 minutes of moderate-intensity physical activity or less than 75 minutes of vigorous-intensity physical activity per week accumulated from fewer than three days a week) and perceived no restrictions for physical activity

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	Recruitment: by 10 occupational health care units from 20 worksites with 2,230 employees **Demographics:** Age: mean (SD) for control 45.3 (9.1), for intervention 44.1 (9.4) years Gender: for control 22% male, for intervention 13% male. Highest level of education: for control Basic 11%, Polytechnic or vocational school 75%, University degree 32%; for intervention Basic 7%, Polytechnic or vocational school 79%, University degree 37% Married: for control 96%, for intervention 99%.
Interventions	**Duration:** Six months **Intervention:** • A one-hour preliminary meeting in each worksite held by a researcher and providing information on the intervention as well as on health benefits and recommendations of PA and walking. The use of stairs was emphasised from the aspect of health and easy applicability. The employees were also supplied with walking leaflets, pedometers (Omron, Walking Style II) and printed logbooks. • Self monitoring of physical activity with the pedometer and logbook. • The baseline average number of daily steps was used for the step goals, which were prompted monthly by the logbooks and email messages sent from occupational health care units. **Control:** No intervention received until 12 months when a one-hour seminar took place and participants were given pedometers, logbooks, walking leaflets and additional training session options
Outcomes	***Physical activity*** *:* • **Walking:** adapted from the **International Physical Activity Questionnaire (IPAQ Long)** (walking at work, walking for transportation, walking for leisure) and an additional question on walking stairs. • **Other:** Vigorous- and moderate-intensity leisure PA other than walking. ***Sedentary behaviour:*** • Sedentary time during working and non-working day adopted from the **International Physical Activity Questionnaire (IPAQ Long)**. ***Anthropometric*** *:* • Height and weight. ***Quality of life*** *:* • Self reported health status (good, fairly good, average, fairly poor, poor). ***Subjective work ability:*** • Subjective estimation of present work ability compared with the lifetime best (scale 0 - 10). ***Other outcomes not reported in this review:*** • Reach, effectiveness, adoption, implementation, maintenance and costs.
Notes	***Statistical analysis:*** **Imputation of missing data:** Not reported/undertaken by authors. **Adjustment for clustering:** Not undertaken. **Sample size calculation:** "According to the power calculations (significance level of 0.05, power of 80%) 175 participants in each group totaling 350 participants were needed to detect the 30% between-group difference in change in the weekly minutes of total walking."

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Barrington 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Berkel 2011

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Bors 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Bort Roig 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Bort Roig 2012a

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Buman 2011

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Butler 2004

Methods	Randomised controlled trial **Aim:** To investigate the effect of feedback from a pedometer as an intervention strategy to increase adherence to a walking programme
Participants	**Population description:** Voluntary, 45 - 65-year olds. (Unknown if employees were recruited or whether it was conducted within a worksite setting - queried through author correspondence) **Intervention group:** 17 participants **Control group:** 16 participants **Location:** Not reported **Inclusion criteria:** Voluntary, inactive (not exercising three times 30 minutes per week), not hypertensive, able to walk unaided for 30 minutes **Recruitment:** Not reported **Demographics:** Age: range 45 - 65 years, mean (SD) 52 (1.21). Gender: 15% male.
Interventions	**Duration:** One month **Intervention:** • Given normal pedometers and shown how to access the step count display. • A modified version of the National Heart Foundation's "Just Walk It" walking programme was promoted. Information included risks of being inactive, potential benefits of becoming more active, suggestions on fitting walking into daily life. • A goal of walking for 30 minutes on all or most days of the week for the first two weeks and increasing this to 40 minutes during the second two weeks. This target was also expressed as a step count (3,000 and 4,000 steps). **Control:** The same intervention was given except the step count display was obscured and the walking goals were

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	not expressed as a step count
Outcomes	***Physical activity*** *:* • **Pedometer steps.** An individually calibrated pedometer (style not reported), with the step count display obscured, was used to count the number of steps in a week during all waking hours at baseline. Intervention group participants were then given normal pedometers and shown how to access the step count display. Control group participants continued to use the obscured display pedometers. ***Other outcomes not reported in this review:*** • **Adherence:** percentage of participants who met the target step count each fortnight. • **Motivation reasons:** such as wanting to experience health benefits (motivated) or tiredness (unmotivated).
Notes	***Statistical analysis:*** **Imputation of missing data:** Not reported/undertaken by authors. **Adjustment for clustering:** Adjustment not required as participants were recruited individually **Sample size calculation:** Not reported/undertaken by authors.

Claus 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

De Cocker 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Devine 2011

Methods	
Participants	
Interventions	

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Devine 2011 (*Continued*)

Outcomes	
Notes	Full text article still awaiting assessment.

Devine 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Hekler 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Hunter 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Hunter 2012a

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Ikenouchi-Sugita 2013

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Ingram 2011

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Jun 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Kazi 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Kessler 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Kim 2013

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Leibiger 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Linde 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Petersen 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Prestwich 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Puhkala 2011

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Thorndike 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Tucker 2011a

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Viester 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

Wierenga 2012

Methods	
Participants	
Interventions	
Outcomes	
Notes	Full text article still awaiting assessment.

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Characteristics of ongoing studies *[ordered by study ID]*

Pillay 2012

Trial name or title	Steps that Count!
Methods	Randomised controlled trial **Aim:** To investigate the effectiveness of a ten-week pedometer-based worksite health promotion programme (Steps that Count!) and individualised email-based feedback to effect physical activity behavioral change (protocol)
Participants	**Population description:** Employed adults, further description not reported. **Intervention group:** Not reported. **Control group:** Not reported. **Location:** Selected worksite settings based in the province of KwaZulu-Natal, RSA, South Africa **Inclusion criteria:** Employees attending the wellness event and willing to participate were eligible if aged between 21 to 49 years; identified as being in the contemplation stage of the Transtheoretical Model towards improved physical activity; and who have a contract with their employer until end of the twelve-week measurement period. In addition, participants must not be pregnant; not be under diagnosis or treatment of cancer; not have any other condition that makes physical activity difficult or impossible; be non-compliant for three or more days during the pre-intervention blinded wearing of a pedometer **Recruitment:** To be undertaken through a Health Risk Appraisal available to all employees attending a corporate wellness event **Demographics:** Age: mean 32 (SD 8). Gender: 50% male
Interventions	**Duration:** Three months **Intervention:** • Participants will be encouraged to steadily increase their steps by approximately 10% per week until the target of at least 30 minutes of aerobic steps is achieved and maintained until the end of the intervention. • Un-blinded pedometer (Omron HJ 750 ITC), data to be uploaded bi-weekly. • Bi-weekly email: Individualised feedback will be given via a personalised email and will include information on the average daily steps accumulated; the number of days (if any) that aerobic steps were accumulated, and the volume thereof; the highest number of steps per day accumulated by the individual over the past two weeks; the category within which the average steps per day fall; general supportive and motivational messages; and a few strategies to achieve the step goals. **Control:** No intervention received.
Outcomes	***Physical activity*** *:* • **Pedometer steps:** Participants (intervention and control groups) to wear a blinded pedometer (Omron HJ 750 ITC), attached to the left or right hip during weeks one and twelve. Data will be downloaded electronically, and the pedometer output will be expressed as steps/day. Step counts will be classified as aerobic (at least 60 steps/min, minimum duration of one minute) and nonaerobic (less than 60 steps/min and less than one minute duration or both). Total time spent accumulating aerobic steps in minutes/day (aerobic time) and the number (in hours) of sedentary time will be calculated ***Anthropometrics*** *:* Specifics not reported • **Body mass index (BMI)** • **Percentage body fat (%BF)** • **Waist circumference (WC)** • **Blood pressure:** systolic and diastolic

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Pillay 2012 (*Continued*)

Starting date	Not reported
Contact information	Julian David Pillay, UCT/MRC Research Unit for Exercise Science and Sports Medicine, Faculty of Health Sciences, University of Cape Town, Cape Town, South Africa. E-mail: pillayjd@dut.ac.za
Notes	***Statistical analysis:*** **Imputation of missing data:** Not reported **Adjustment for clustering:** Not reported **Sample size calculation:** "A sample size of 30 participants per arm of the study is required to ensure 80% statistical power and with a p-value set at <0.05. However, if a modest improvement of 1 500 steps/d is considered, a sample size of approximately 85 participants per arm is required. In order to achieve this, 1200 employees attending wellness events will be targeted. Of these, a minimum of 480 employees (40%) will be identified to be in the contemplation stage of the Transtheoretical Model"

Thøgersen-Ntoumani 2010

Trial name or title	Step by Step
Methods	Randomised controlled trial **Aim:** To assess the feasibility and effectiveness of a 16-week lunchtime walking intervention to increase (and sustain) walking behaviour, improve general and work-related wellbeing, and enhance work performance levels in insufficiently physically active non-academic University employees
Participants	**Population description:** Non-academic, insufficiently physically active adults employed full-time at a large University **Intervention group:** 35 participants **Control group:** 40 participants **Location:** West Midlands, UK. **Inclusion criteria:** Healthy, mobile, 18- to 65-year-old, full-time employees (non-academic), engaging in less than 30 minutes of moderate intensity physical activity on five days per week (i.e. insufficiently physically active), with no significant auditory or visual problems and no severe musculoskeletal disorders that prevent them from engaging in physical activity. Medical clearance was requested for those who reported any cardio-vascular disease or back pain preventing them from exercising **Recruitment:** • Open stall in a one-day health fair taking place at the University; collected email addresses and sent a link to an online survey; • Staff University newspaper; • Brief messages on the back of all staff pay-slips; • Brief messages on electronic totems (information stands) at the main University campus; • University-wide electronic newsletters and departmental newsletters; • Posters and flyers at targeted University locations (e.g., refectories, staff bar, main administrative centre of the University); • University induction sessions for new staff;

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	• University web-based information portal for all employees; • Website targeted to interested participants. **Demographics:** Age: range 18 - 65 years.
Interventions	**Duration:** Four months (10-week intervention phase followed by a six-week independence phase) **Intervention:** Designed using an autonomy supportive exercise leader style and to meet national recommendations of 150 minutes per week of physical activity 1. Professional contact/counselling • 10-week intervention phase: 3 x 30 minutes per week group lunchtime walks, facilitated by a nationally qualified walk leader (e.g. already walk leader-trained by nationally recognised organisations, such as Natural England), maximum 12 participants per group. Registration required through doodle via website with days (Mondays to Thursdays) and times (12.30 or 1.15 pm) offered. Different route for each of these walks had been mapped and could be viewed through the website. • Six-week independence phase: participants were encouraged to form informal walk groups. • Six-week independence phase: encouraged to make use of the walk routes they had been made aware of during the 10-week phase, as well as explore new ones. • Six-week independence phase: encouraged to contact the research team if they needed 2. Goal setting • 10-week intervention phase: Challenge to accumulate 60 minutes of walking during the week-ends. • 10-week intervention phase: Pedometers (style Yamax Digi-Walker 351) the week prior to the start of the intervention. • 10-week intervention phase: A motivational booklet; educational information about adoption and maintenance of physical activity (e.g. identifying/countering barriers and goal-setting principles); "Am I on track?" table; logbook; record of participants' personal reasons for walking; record of favourite walks; new places/areas to walk. 3. Personal web page 4. Environmental prompts • 10-week intervention phase: Two per week autonomy-supportive text messages (times were randomly allocated) were sent to the participants via a smart phone (Nokia 2730 Classic). Self-Determination Theory principles (e.g. offering choice, supporting individual volition, minimising pressure and control, acknowledging participants' perspectives and feelings, and providing a meaningful rationale for engaging in walking) informed the tone. • Six-week independence phase: Three per week autonomy-supportive text messages (times were randomly allocated). Self-Determination Theory principles (e.g. offering choice, supporting individual volition, minimising pressure and control, acknowledging participants' perspectives and feelings, and providing a meaningful rationale for engaging in walking) informed the tone. **Control:** Received intervention at 10 weeks (delayed treatment control group). No intervention received during control period and were asked to continue their usual behaviours. They knew that they would be contacted in a few months regarding the start of their programme. smart phones received at the beginning of their control period, but no text messages, as the phones were also used as a monitoring tool to survey work-related wellbeing
Outcomes	***Physical activity :*** administered via internal post self report questionnaire at baseline and four months • **International Physical Activity Questionnaire (IPAQ) short form.** Continuous or categorical. Hourly participation each week in activities rated according to multiples of metabolic equivalent task units (METS). Reliability and criterion validity judged against accelerometry is comparable to other self report measures. ***Quality of Life:*** administered via internal post self report questionnaire at baseline and 16 weeks • **Current health perceptions**: One item from the MOS SF-36.

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	• **Satisfaction With Life Scale** • **Subjective Vitality scale** • **Positive and Negative Affect Schedule** ***Other outcomes not reported in this review:*** • **Two kilometre field-based walking tests:** "participants were instructed to walk 2 km (e.g. five laps) on an outdoor 400 metre track as fast as they could with a steady pace. After each lap, participants were provided prompts on the number of laps left to be completed." • **Work-related well-being scales** (administered via internal post self-report questionnaire at baseline, 16 weeks and 4 months): a job satisfaction scale, the Job Affect Scale (participants rate their levels of affect during the past week, which can be categorised into four factors: enthusiasm, relaxation, nervousness and fatigue at work), a 16-item instrument developed specifically for the present study (participants rate their own levels of work quality in the past four weeks), overall perceptions of work performance (one item regarding the past four weeks taken from the WHO-HPQ). • **Manager-rated scales** (administered at baseline and at 16 weeks): manager-rated work quality, managers' views WHO-HPQ (work quality in the previous four weeks), three most important characteristics of the employee's job(s) and subsequent rating of the employee on those characteristics (qualitative). • **Smart phone 'real time' questionnaire:** a momentary, real time work-related affect which was administered via a smart phone. The Job Affect Scale and a single question relating to perceived daily work load was administered twice weekly. • **Work Extrinsic and Intrinsic Motivation Scale**
Starting date	Results were expected in January 2011.
Contact information	Dr Cecilie Thøgersen-Ntoumani, School of Sport and Exercise Sciences, University of Birmingham, United Kingdom. E-mail c.thogersen@bham.ac.uk
Notes	***Statistical analysis:*** **Imputation of missing data:** Not reported **Adjustment for clustering:** Adjustment not required as participants were recruited individually **Sample size calculation: "**This was a feasibility trial as specified by the MRC guidelines for designing complex interventions. Consequently the sample size was determined by a consideration of the results of King, Ahn, Oliveira, Atienza, Castro, and Gardner who reported a large effect of an 8-week physical activity intervention on minutes per week in moderate intensity physical activity. We also consulted the corporate partner to confirm a realistic target number for a feasibility study. Thus, we aimed to recruit a total sample of 68 participants given an effect size of d = .70, statistical power of 80% at a significance level of 5%, with a potential loss to follow up of 25%." ***Notes:*** "This work was supported by The BUPA Foundation (grant number TBF08004)." Authors stated no competing interests

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DATA AND ANALYSES

Comparison 1. Pedometer programme vs 'no intervention' control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 CVD risk factor: Body Mass Index (BMI; kg/m²)	2	197	Mean Difference (IV, Random, 95% CI)	-0.92 [-1.82, -0.02]
2 CVD risk factor: Waist circumference (cm)	2	196	Mean Difference (IV, Random, 95% CI)	-3.35 [-8.34, 1.65]
3 CVD risk factor: Systolic blood pressure (mmHg)	2	197	Mean Difference (IV, Random, 95% CI)	-3.11 [-8.39, 2.17]
4 CVD risk factor: Diastolic blood pressure (mmHg)	2	197	Mean Difference (IV, Random, 95% CI)	-1.14 [-3.45, 1.16]

CONTRIBUTIONS OF AUTHORS

RFP co-ordinated the review, assessed each paper at each stage, undertook data extraction, risk of bias assessment and drafted the text. MC, SC and AP assessed one third of the papers at the title/abstract stage and the full-text stage. MC also undertook data extraction and risk of bias assessment, and oversaw drafting of tables and data analyses. All authors reviewed, amended and approved the final text.

DECLARATIONS OF INTEREST

None of the researchers have a commercial for-profit interest in the outcomes of this review. Two researchers (RFP, AP) would like to disclose that they are undertaking an independent research study, titled the Global Corporate Challenge® (GCC®) Evaluation Study, which will evaluate the impact of a workplace pedometer intervention, which will not be included in this review. The GCC® study is partially funded by the Australian Research Council (ARC) and the Foundation for Chronic Disease Prevention^{TM} in the Workplace, which is associated with the Global Corporate Challenge®.

SOURCES OF SUPPORT

Internal sources

- Australasian Cochrane Centre, School of Public Health and Preventive Medicine, Monash University, Australia.

Salary support for MC.

- Department of Epidemiology and Preventive Medicine, Monash University, Australia.

RFP is supported by a Monash Departmental Scholarship.

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External sources

- Victorian Health Promotion Foundation (VicHealth), Australia.

AP has received a VicHealth Public Health Fellowship. VicHealth is a government-funded health promotion agency.

- The Australian Research Council (ARC), Australia.

The Australian Research Council (ARC) is partially funding two researchers (RFP, AP) to undertake an independent research study, titled the Global Corporate Challenge® (GCC®) Evaluation Study, which will evaluate the impact of a workplace pedometer intervention and will not be included in this review.

- The Foundation for Chronic Disease Prevention^{TM} in the Workplace, Australia.

Two researchers (RFP, AP) are undertaking an independent research study, titled the Global Corporate Challenge® (GCC®) Evaluation Study, which will evaluate the impact of a workplace pedometer intervention and will not be included in this review. This evaluation study is partially funded by the Foundation for Chronic Disease Prevention^{TM} in the Workplace, which is associated with the Global Corporate Challenge®

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We altered the order of some methods to match the order within the main text.

Intervention duration categories in Subgroup analysis and investigation of heterogeneity have been amended to coincide with follow-up duration categories in Primary outcomes.

Some outcome descriptors have been changed for clarity and accuracy. Body composition outcomes are now referred to as anthropometric. Hypertension outcomes are now referred to as blood pressure. Biomedical outcomes are now referred to as biochemical.

Given the availability of the other body composition measures, body weight was not assessed as a secondary outcome in Secondary outcomes.

As described in the Searching other resources section, we asked the Cochrane Public Health Group and the Cochrane Heart Group to search their trial registers for relevant trials. However, this request was only given a month prior to submission which did not allow enough time for the searches to be co-ordinated.

As noted in the Excluded studies section, an additional study (Racette 2009) was excluded because, although random allocation was used, only one workplace cluster was allocated to each of the intervention and control arms, which was not considered adequate to reduce the risk of imbalance of confounders between these two arms. This was an additional criterion not originally planned at the protocol stage.

As outlined in the Quality of the evidence section, the quality of evidence was assessed using the GRADE system.

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