European Journal of Physical Education and Sport Science
ISSN: 2501 - 1235
ISSN-L: 2501 - 1235
Available on-line at: www.oapub.org/edu

doi: 10.5281/zenodo.3864522 Volume 6 | Issue 4 | 2020

# EFFICACY OF PHYSICAL ACTIVITY COUNSELLING INTERVENTIONS DELIVERED IN PRIMARY CARE: A SYSTEMATIC REVIEW AND META-ANALYSIS

**Micky Olutende Oloo[1i],**
**Edwin Kadima Wamukoya[1],**
**Maximilla Wanzala[2]**
[1]Department of Health Promotion and Sports Science,
Masinde Muliro University of Science and Technology,
Kenya
[2]Department of Public Health,
Masinde Muliro University of Science and Technology,
Kenya

**Abstract:**
**Background:** Overwhelming success has been achieved in disease control through environmental interventions such as vaccinations and improved hygiene to increase life expectancy, many authorities in the field of preventative healthcare are of the opinion that too little has been done to target behavioral factors, particularly physical inactivity. Research suggests that the impact PA counselling in primary care could have at a population level is tremendous. However, gaps still exist between the science and the practice when it comes to prescribing exercise in the healthcare context**. Objectives:** To compare the effectiveness of health provider delivered interventions for physical activity (PA) promotion verses placebos or no or minimal intervention among community dwelling adults **Search methods:** We searched Cochrane Central Register of Controlled Trials in the Cochrane Library, Ovid MEDLINE(R), Embase Ovid, Web of Science, CINAHL (EBSCOhost) and SPORTDiscus (earlier dates to 10 January, 2020) electronic databases regardless of language or publication status using the optimal sensitive search strategy developed by The Cochrane Collaboration. We used Medical Subject Headings (MeSH) [1], Looked up words in text word, abstract, title [2], then Combined [1] with [2] using Boolean logic (OR) and then Set up proper filters. **Selection criteria:** Randomised controlled trials (RCTs) and cluster randomised trials. We excluded quasi-RCTs and cross-over trials. We will include studies comparing health provider counselling intervention to placebo or no counselling/exercise prescription. We excluded studies that had more than a 20% loss to follow-up if they did not apply an intention-to treat analysis. The studies were considered if the outcomes were measured on a continuous scale and

[i] Correspondence: email micky.oloo.mf@gmail.com

results reported in terms on mean change, confidence intervals of change, standard deviations or standard error and mean. Studies with dichotomous outcomes were excluded. **Data collection and analysis:** Two review authors (EKW and MW) independently carried out data extraction for each included record using a pro forma specifically designed for the purpose. We resolved differences in data extraction by discussion. We plotted the results of each trial as point estimates, using means and standard deviations for the continuous outcomes. Since studies reported different outcome measures but measured the same concept, we calculated the standardised mean difference (SMD) with 95% confidence interval (CI) using a random-effects models. **Main results:** There were 15,269 apparently healthy adults who participated in the 24 included studies. All studies recruited both genders. The stated age range of participants was from 18 to 80 years. Meta-analysis of data from these trials suggests that health provider-led physical activity counselling interventions in primary care may lead to increased self-report physical activity (SMD 0.11, 95% CI 0.04 to 0.17; participants = 13211; studies = 16; $I^2$ = 50%), total energy expenditure (SMD 0.20, 95% CI 0.13 to 0.27; participants = 3376; studies = 8; $I^2$ = 2%; overall effect P=0.00001) and systolic blood pressure at six showed a mean difference (MD) favouring health provider-led physical activity counselling interventions compared to usual care of -0.10 mmHg (SMD 0.27 95% CI 0.72 to 0.18;$I^2$ = 0%, overall effect P = 0.006) among patients. However meta-analysis showed that health provider-led physical activity counselling interventions in primary care did not lead to increased aerobic fitness (SMD 0.06, 95% CI -0.01 to 0.12; $I^2$ = 35%), body mass index (SMD -0.04, 95% CI -0.15 to 0.07) and total cholesterol (SMD -0.27, 95% CI -0.72 to 0.18 and Waist circumference (SMD -0.05, 95% CI-0.15 to 0.06; $I^2$ = 0%;overall effect P=0.24). Although there were limited data, there was no evidence of an increased risk of adverse events. **Authors' conclusions:** Counselling delivered by health providers, probably leads to similar or better physical activity outcomes for patient conditions (moderate-certainty evidence). However, these results must be interpreted with a degree of caution, recognising the variation in interventions reported within studies and the complex interplay of factors affecting outcomes. Several studies included multiple intervention methods, which made it difficult to tease apart which intervention components were the active ingredients

**Keywords:** physical activity counselling; interventions; primary care

## 1. Introduction

### 1.1 Plain language summary

#### 1.1.1 Physical activity counselling interventions for promoting physical activity

In order to initiate important increases in physical activity, both exercise prescription and counselling should be incorporated in standard practice. We recommend as a tool for primary health care physicians to promote PA, especially at health check and control visits, where more time can be allocated for the appointment. A more comprehensive

familiarization protocol or training sessions are needed to implement physical counselling to everyday practices and facilitate counselling cooperation. Self-monitoring of PA with expert feedback can be a useful and cheaper way of increasing especially the duration of overall weekly PA in the short term.

**1.2 Background**

Physical activity (PA) improves quality of life and health in clinical and non-clinical populations, including prevention and management of non-communicable diseases (NCDs) (Tero et al., 2017; Lee et al.m 2012) like diabetes (Thent, Das & Henry, 2013), lowers blood pressure and reduces the risk of coronary heart disease, hypertension and stroke (Soares-Miranda et al., 2016; Green et al., 2011), reduces risk of developing breast and colon cancer (Kimmel Haas & Hermanns 2014) and has beneficial effects on body weight, fat mass and central obesity (Wiklund, 2016). A study by Machio, 2012 showed that NCDs reduced labour force participation by 61% but with elimination of physical inactivity, life expectancy in Kenya was expected to increase by between 0.25-0.49 years (Lee et al., 2012). Regular PA can achieve parallel or greater effects on NCDs' risk factors than those achieved with drugs at a lower cost and with minimal adverse effects (Fiuza-Luces et al., 2013). In a landmark British Medical Journal paper examining the head-to-head effects of medication versus PA/exercise in chronic disease, Naci & Ioannidis, 2013 from Stanford University made a strong case for equivalent or superior effect of the health benefits of PA. In particular, PA interventions were more effective than drug treatment among patients with stroke and were as effective as medications for the prevention of diabetes and secondary treatment of NCDs (Naci & Ioannidis, 2013).

Research suggests that the impact PA counselling in primary care could have at a population level is tremendous. Goldstein et al., 1999 noted that the interaction between the high prevalence of sedentary behavior and the frequency of physician visits, coupled with primary care PA promotion had the potential to significantly impact the incidence of hypokinetic diseases such as heart disease, stroke, hypertension. In 2005, the American College of Preventive Medicine issued a position statement "*that primary care providers should incorporate PA counselling into routine patient visits*" (Jacobson other professional organizations echo the American Academy of Family Physicians, the American Academy of Paediatrics, the American College of Obstetrics and Gynaecology, the American Heart Association, National Institutes of Health, and the Surgeon General (Garry, Diamond & Whitley, 2002; Jacobson et al., 2005). The ACSM is yet another organization that recognizes and endorses the importance of primary care PA counselling through its initiative, "*Exercise is Medicine"* (Sallis, 2011). This initiative seeks to create awareness that "exercise is medicine" and should be prescribed accordingly. Previous Cochrane reviews found that PA interventions had a moderate effect on participation levels (Foster et al 2005; Richards, Hillsdon, Thorogood & Foster, 2013). This review seeks to give and overview of the effectiveness of all health provider delivered intervention in improving physical activity outcomes in adult populations. It also seeks to give more detail to a narrative review done by Oloo, Kweyu & Ashiali, 2017.

### 1.3 Description of the condition

It is evaluated that in 2008 physical inactivity caused 9% of untimely mortality and 5.3 million deaths around the world (Lee et al., 2012). This included between 6% to 10% of all deaths from major non-transferable maladies all-inclusive. It has been assessed that expanding the number of individuals that accomplish the WHO PA proposals by 10% or 25% would forestall more than 553,000 and 1.3 million deaths, separately, comprehensively every year (Lee et al., 2012). In Kenya, research shows that NCDs have been a growing problem over the years (WHO, 2017; Machio, 2012). In 2012 NCDs accounted for more than 50% of total hospital admissions and over 55% of hospital deaths in Kenya (Kenya Demographic and Health Survey, 2014). According to Kenya Demographic and Health Survey [KDHS] report (2014), over 61% of the population in Kenya did not engaged in exercise that caused an increase in their heart rate for at least 10 minutes continuously at work or during other activities. In the Western region of Kenya, results showed that 39.1% of women and 45.4% of men did not engage in PA at all (Grimstvedt et al., 2013). This corroborates with studies done in other parts of the world that showed most adults world- wide did not engage in PA at levels with the potential to yield benefits (Ding et al., 2016; Das & Horton, 2016; Hallal., 2012).

### 1.4 Description of the intervention

Studies have shown that HCPs have the best opportunity to question and counsel patients about the importance of PA since they can take advantage of the on-going care they provide to a large sector of the population and be influential in changing patients' behaviours. (Lobelo & de Quevedo, 2016; O'Brien et al., 2017; Lamming et al., 2017). Research has proven that HCPs have access to a large proportion of the sedentary population (Vuori et al., 2013; Matheson et al., 2011) and are therefore considered to be well positioned to champion the course of prevention of chronic diseases by prescribing PA during standard consultation (Teferi, Kumar & Singh, 2017; Matheson et al., 2011). In Western Kenya for example, Kakamega county which is the current study's setting is the second most populous county in Kenya with a nurse patient ratio of 86.37 per 100,000 people, which is higher than the national average of 51.5 per 100,000 people (HMIS, 2012). This is an indication that many HCPs in the county have access to a large proportion of the sedentary population.

### 1.5 How the intervention might work

Behavioural theories provide a foundation for an intervention that can explain the drivers of PA behaviour and potential pathways for change. It is now generally accepted that well designed PA interventions are based upon behavioral theories (Bartholomew et al., 2001). The majority of studies have adopted social psychology theories (Biddle & Mutrie, 2001). Some of the common theories used to explain behaviour include Social cognitive theory, Transtheoretical Model of behaviour change and Theory of planned behaviour. These theories have conceptual convergence and also share two common constructs (Biddle & Mutrie, 2001; Rodgers & Brawley, 1991). These constructs are the outcome

expectancy, which is the belief that the behaviour will lead to a specific outcome and social norms which refers to the influence of expected behaviour within a social group. With regards to PA behaviour, experts agree that variation in PA behaviour can be explained by factors operating at two levels, the intrinsic and extrinsic levels (Sallis, 2009). Intrinsic factors have received greater attention than the external factors in attempts to explain behavioral choices. Exercise prescriptions from HCPs will remind patients that PA is part of their treatment plan and should be adhered to with the same diligence with which their medication is taken (Grandes et al., 2009). Almost two-thirds of patients (65%) would be more interested in exercise and PA to stay healthy if advised by their HCP (Leemrijse et al., 2015), while 24% of patients will turn to fitness and health web sites for advice on exercise and PA but after consulting their HCP first (25%) (Derman et al., 2008). Researchers have confirmed that majority of people cite their HCP as their primary source of information regarding healthy lifestyle decisions (diet and exercise) (Lanhers et al., 2015; Leemrijse et al., 2015).

### 1.6 Why it is important to do this review

There is no "silver-bullet" to solve the global physical inactivity, an "all of the above" and "whole of society" approach, including PA counselling and referral in the health care context, will be required (Heath et al., 2012). While overwhelming success has been achieved in disease control through environmental interventions such as vaccinations and improved hygiene to increase life expectancy, many authorities in the field of preventative healthcare are of the opinion that too little has been done to target behavioral factors, particularly physical inactivity (Sallis, 2011). Once we have a better understanding of these factors, it will help better inform and target training efforts of current and future HCPs, and more importantly encourage HCPs to integrate exercise within standard consultation to further improve the health and well-being of clients. HCPs will begin to develop potentially better biopsychosocial treatment plans and interventions that include individually tailored exercise plans that help clients live increasingly holistic healthy lives. Understanding the effectiveness of these more traditional approaches to implementation should influence PA policy makers and professionals.

### 1.7 Objectives

To compare the effectiveness of health provider delivered interventions for physical activity (PA) promotion verses placebos or no or minimal intervention among community dwelling adults.

## 2. Methods

### 2.1 Criteria for considering studies for this review

#### 2.1.1 Types of studies

Randomised controlled trials (RCTs) and cluster randomised trials. We excluded quasi-RCTs and cross-over trials. We will include studies comparing health provider counselling intervention to placebo or no counselling/exercise prescription. We excluded studies that had more than a 20% loss to follow-up if they did not apply an intention-to treat analysis. The studies were considered if the outcomes were measured on a continuous scale and results reported in terms on mean change, confidence intervals of change, standard deviations or standard error and mean. Studies with dichotomous outcomes were excluded.

#### 2.1.2 Types of participants

Community dwelling participants men and women (18 years and above) randomly assigned. Studies that only assessed men or women alone were also included. Exclusion criteria included participants who had secondary hypertension, mental or physical illness serious enough to potentially influence the compliance with the study procedures, alcoholism, type 1 diabetes, current or planned pregnancy and history of myocardial infarction or stroke within the preceding 3 months.

#### 2.1.3 Types of interventions

Physicians, general practitioners, nurses, nutritionist in a primary care setting. Relevant interventions include, but not limited to:

- counselling or advice, or both;
- self-directed or prescribed exercise, or both;
- home based, telephone based or facility-based exercise, or both;
- written education or motivational support material, or both.

Telephone counselling. Intervention and comparators are eligible regardless of delivery or duration.

#### 2.1.4 Types of outcome measures

**A. Primary outcomes**

The primary outcomes of this review included data that assessed change between control and intervention for:

- self-report measures of PA behaviour. This was any physical activity outcome assessed by questionnaires (Total energy expenditure (Kcal/week or Kcal/day) and physical activity (minutes/week or hours/week))
- aerobic fitness (VO2max) (ml/kg/min or ml/min). It was either estimated from a submaximal fitness test or recorded directly from a maximal

**B. Secondary outcomes**

The secondary outcomes of this review included data for:

1. clinical anthropometric parameters. This was assessed by body mass index (BMI) and body circumferences (waist).
2. systolic blood pressure assessed in millimetres of Mercury (mmHg)
3. total cholesterol.

### 2.2 Search methods for identification of studies

#### 2.2.1 Electronic searches

We searched the following electronic databases regardless of language or publication status using the optimal sensitive search strategy developed by The Cochrane Collaboration (Lefebvre, 2011). We used the PICO (P-patient problem, I-intervention, C-comparison, O-outcomes) acronym to help keep focus on the key concepts. We Used Medical Subject Headings (MeSH) [1], Looked up words in text word, abstract, title [2], then Combined [1] with [2] using Boolean logic (OR) and then Set up proper filters e.g. randomised controlled trials. We also borrowed the search strategies of a previous systematic review (Richards, Hillsdon, Thorogood & Foster, 2013). The searches were based on the MEDLINE search strategy combined with the sensitivity- and precision-maximising version of the Cochrane Highly Sensitive Search Strategy for identifying RCTs (Lefebvre, 2011). We modified the search strategy to use in the other databases. Details of the search strategy are presented in the Appendices (Appendices)

1) Cochrane Central Register of Controlled Trials (January, 2020) in the Cochrane Library;
2) Ovid MEDLINE(R) and In-Process & Other Non-Indexed Citations and Daily (1948 to 17 January, 2020;
3) Embase Ovid (1980 to 29 January, 2020);
4) Web of Science (Search date: 18 January, 2020);
5) CINAHL (EBSCOhost) (Search date: January, 2020);
6) SPORTDiscus (1949 to 10 January, 2020.

#### 2.2.2 Searching other resources

We screened individual journals and conference proceedings (via handsearching). We also examined reference lists of all pertinent reviews and studies for published RCTs. We conducted a grey literature search to identify studies not indexed in the databases listed above. We used the following sources.

1) OpenGrey (www.opengrey.eu).
2) The web, e.g. Google (http://scholar.google.com)

### 2.3 Data collection and analysis

#### 2.3.1 Selection of studies

Two investigators (EKW and MW) independently screened titles and abstracts using an online systematic review toolkit, Covidence (https://www.covidence.org/). Relevant studies were selected based on search queries. The full-text articles of studies identified as being potentially eligible for the review were retrieved to further determine their

inclusion. We resolved all disagreements by discussion between the review authors to reach a consensus. Authors applied the following inclusion criteria to determine if the full paper was needed for further scrutiny.

1) The study was a RCT or a cluster randomised trial and aimed to examine the effectiveness of a health provider based PA counselling/exercise prescription to increase PA levels
2) The study allocated participants to the intervention or control group/placebo group using a method of randomisation
3) The study included adults aged 18 years and older and recruited community dwelling adults that were free of chronic disease or pre-existing medical conditions that would limit participation in PA;
4) The study had analysed the results by intention to treat analysis or used per protocol analysis but with an attrition of less than a 20% (Richards, Hillsdon, Thorogood & Foster, 2013).

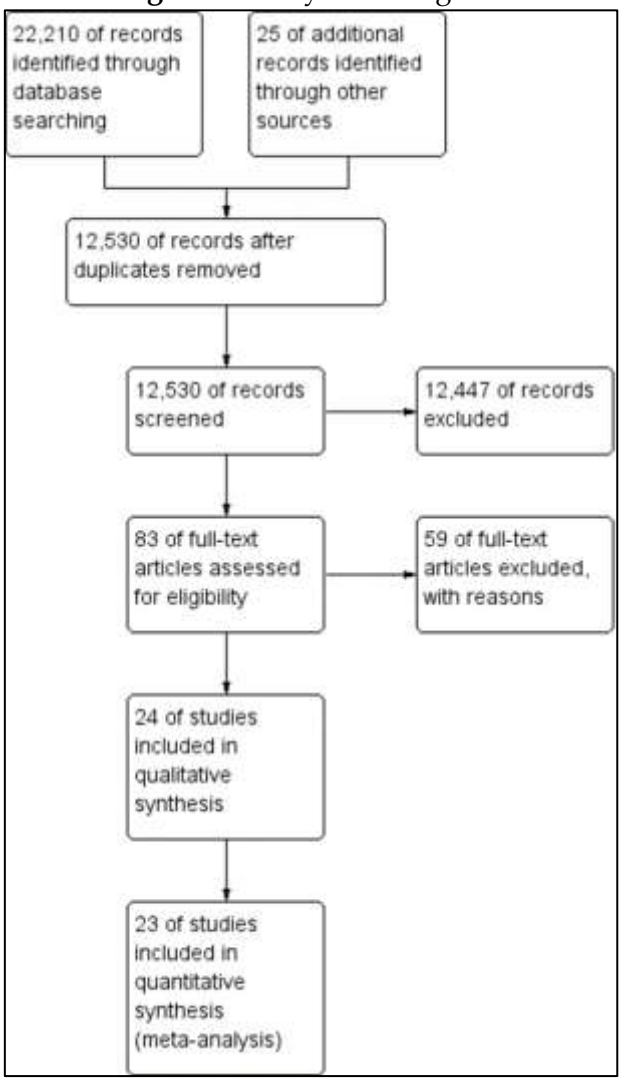
### 2.3.2 Data extraction and management

Two review authors (EKW and MW) independently carried out data extraction for each included record using a pro forma specifically designed for the purpose. We resolved differences in data extraction by discussion. The following characteristics from each study that met the inclusion criteria were extracted:

1) Description of study participants (e.g., sample size, demographic characteristics, country where study was performed);
2) Eligibility criteria for enrolment
3) Details about the intervention (e.g., length of intervention and follow-up, individual or group modality, behavioral techniques);
4) Assessment of risk of bias (e.g., study design, generation of allocation sequence, allocation concealment, blinding, loss to follow-up, inclusion of all randomised participants, incomplete outcome data addressed, and sample size calculation);
5) Outcome measures

We performed all statistical analyses using Review Manager 5.3 (Review Manager 5.3) software.

**Figure 1:** Study flow diagram




### 2.3.3 Assessment of risk of bias in included studies

Two review authors (MOO and EKW) assessed the risk of bias of included studies independently using Cochrane's 'Risk of bias' tool (Higgins, 2011). We assessed each study for risk of bias in each of the following domains: sequence generation, allocation concealment, blinding of participants and personnel (performance bias), blinding of outcome assessment (detection bias), incomplete outcome data and selective outcome reporting. We assessed the risk of bias associated with (a) blinding and (b) completeness of outcomes separately for self-reported outcomes and objective outcomes. Where there was disagreement between the review authors in risk of bias assessment, a third author (MW) was asked to independently appraise the study and discrepancies were resolved by consensus between all three authors. Risk was categorized as either "Low risk", "High risk", or "Unclear", and was listed in risk of bias tables broken down by trial. Studies at high risk of bias do appear in Data and analyses, but we suggest that readers use these data only to make decisions as to whether they would like to evaluate the intervention themselves in a more rigorous way. We do not believe the data support judgements about effect.

### 2.3.4 Measures of treatment effect

Statistical analysis was conducted according to Cochrane guidelines (Higgins 2011). We plotted the results of each trial as point estimates, using means and standard deviations (SDs) for the continuous outcomes. Since studies reported different outcome measures but measured the same concept, we calculated the standardised mean difference (SMD) with 95% confidence interval (CI) using a random-effects model. This strategy accounts for any potential heterogeneity that may occur following unique intervention approaches developed in various study settings.

### 2.3.5 Unit of analysis issues

We conducted statistical analysis using Review Manager 5.3 software and visualised the results using forest plots. When a study had more than one study arm relevant to this review, we examined the overall effects of the intervention versus control by combining the data from the related study arms and calculated the standardized mean difference using recommended approaches (Higgins, 2011).We also calculated individual study effects and then the pooled effect sizes with 95% CIs. As in the 2013 update, we used Cohen's three levels to guide our classification of the estimates of effect as small (< 0.5), medium (0.5 to < 0.8) or large (V 0.8; Cohen, 1988). In studies where data from all repeated follow up assessments after baseline were available, we combined results from all follow up assessments to get an average change in outcomes for analysis.

The authors corrected the sample size in the cluster randomised trials included according to the Cochrane handbook (Higgins, 2011). A common approach is to use external estimates obtained from similar studies. According to the Cochrane handbook, for continuous data only the sample size need be reduced; means and standard deviations should remain unchanged. The effective sample size of a single intervention group in a cluster-randomised trial is its original sample size divided by a quantity called the 'design effect'. The design effect was calculated using the following formulae 1 + (M – 1) ICC. For this study we used ICC from a study by Grandes et al., 2009 to compute the design effect.

### 2.3.6 Dealing with missing data

We applied the 'Risk of bias' criteria to exclude studies with a high risk of missing data, as they pose serious threats to validity (Higgins, 2011).Where appropriate, we contacted study authors for further information. If this was not possible, we reported the number of participants lost to follow-up. For studies that reported continuous data but did not report standard deviations, we calculated these values from other available data such as standard errors, or imputed them using the methods suggested in Higgins 2011.

### 2.3.7 Assessment of heterogeneity

First, we assessed whether studies were sufficiently homogeneous to be included in one comparison. We based this judgment on the similarity of the type of interventions, what the control condition was and the outcome. Statistical heterogeneity between results of

different studies was examined by $\chi^2$ tests. A $P$ value for a $\chi^2$ test of less than 0.10 indicated heterogeneity. An alternative approach to quantify the effect of heterogeneity is assessing the inconsistency among the results of studies with 95% uncertainty intervals. A value of 0% indicates no observed heterogeneity and a value greater than 50% indicates the presence of substantial heterogeneity. We noted potential sources of differences between studies where analyses had high heterogeneity

**2.3.8 Assessment of reporting biases**

To reduce possible publication bias, we employed strategies to search for and identify relevant unpublished studies for inclusion. These strategies included searching the grey literature and prospective trial registration databases to overcome time-lag bias.

**2.3.9 Data synthesis**

We carried out statistical analysis using the Review Manager software (Review Manager 5.3). Had it been reasonable to assume that studies were estimating the same underlying treatment effect, i.e. where studies were examining the same intervention, and the studies' populations and methods were judged to be sufficiently similar, we would have used fixed-effect meta-analysis for combining data. Since there was clinical heterogeneity sufficient to expect that the underlying treatment effects differed between studies, we used random-effects meta-analysis to produce an overall summary when an average treatment effect across studies was considered clinically meaningful. Where the average treatment effect was not clinically meaningful, we did not combine studies. When we used random-effects analyses, the results were presented as the average treatment effect with 95% confidence intervals

**2.3.10 Subgroup analysis and investigation of heterogeneity**

Although we identified heterogeneity with type of health provider (general practitioners and nurses), time of follow up, different interventions (face to face, telephone, Internet based) and with PA supervised verses unsupervised, we were unable to investigate subgroup analyses due to insufficient data.

**2.3.11 Sensitivity analysis**

We conducted sensitivity analyses to explore the effect of study quality, assessed by concealment of allocation, high attrition rates, or both, by excluding studies with a high or unclear risk of bias from the analyses in order to assess whether this made any difference to the overall result.

## 3. Results

### 3.1 Description of studies

The study selection process is summarized in Figure 1 according to PRISMA guidelines. See Characteristics of included studies; Characteristics of excluded studies; Characteristics of ongoing studies

#### 3.1.1 Results of the search

The database search identified 22, 210 studies. 12, 530 studies remained after duplicates were removed. We excluded 12,447 articles following a review of titles and abstracts and retrieved and assessed the full text of 55 articles. 24 randomised trials met the inclusion criteria, and we included them in this update. There were two ongoing studies which will be incorporated into the review at the next update. All searches were completed in January 2020.

#### 3.1.2 Included studies

There were 15,269 apparently healthy adults who participated in the 24 included studies. All studies recruited both genders. The stated age range of participants was from 18 to 80 years, and six of the studies specifically targeted adults aged 65 years or older (Goldstein et al., 1999; Kerse et al., 2005; Kolt et al., 2007; Mutrie et al., 2012; Petrella et al., 2003; Pfeiffer et al., 2001)

#### 3.1.3 Study design

Of the 24 included studies, 17 were randomised controlled trials (RCTs) (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Atay, Torman & Yaman, 2014; Fortier et al., 2011; Green et al., 2002; Hillsdon et al., 2002; Kolt et al., 2007; Lawton et al., 2008; Lewis & Lynch, 1993; Little et al., 2004; Mutrie et al., 2012; Norris et al., 2000; Pears et al., 2016; Petrella et al., 2003; Pfeiffer et al., 2001; Reid & Morgan, 1979; Swinburn et al., 1998; Sørensen et at., 2008; Tylor & Fox, 2005), including 7 cluster-RCTs (Elley et al., 2003; Goldstein et al., 1999; Grandes et al., 2009; Grandes et al., 2011; Kerse et al., 2005; Petrella, Lattanzio, Shapiro & Overend, 2010)

#### 3.1.4 Location and Setting

One study was conducted in Finland (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006), one study in Turkey (Atay, Torman & Yaman, 2014), five studies in New Zealand (Elley et al., 2003; Kerse et al., 2005; Kolt et al., 2007; Lawton et al., 2008; Swinburn et al., 1998), two studies in Spain (Grandes et al., 2009; Grandes et al., 2011 ), four studies in the United States (Goldstein et al., 1999; Green et al., 2002; Lewis & Lynch, 1993; Pfeiffer et al., 2001), three studies in the United kingdom (Hillsdon et al 2002; Little et al 2004; Mutrie et al., 2012; Pears et al., 2016; Tylor & Fox, 2005), four studies in Canada (Fortier et al 2011; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003; Reid & Morgan, 1979),

one study in the Pacific north (Norris et al 2000) and one study in Denmark (Sørensen et at 2008).

### 3.1.5 Types of intervention

The interventions in the study were all based in a primary care setting and were administered by either physician, general practitioners, doctors or nurses. The main modes of delivery of physical activity counselling were face to face, telephone, written or combined. Of the 24 included studies, 21 were face to face delivery (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Atay, Torman & Yaman, 2014; Elley et al., 2003; Fortier et al., 2011; Goldstein et al., 1999; Grandes et al., 2011; Hillsdon et al., 2002; Kerse et al., 2005; Lawton et al., 2008; Lewis & Lynch, 1993; Little et al., 2004; Mutrie et al., 2012; Norris et al., 2000; Pears et al., 2016; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003; Pfeiffer et al., 2001; Reid & Morgan, 1979; Sørensen et at., 2008; Tylor & Fox, 2005), five were either telephone delivered or combined with face to face (Green et al 2002; Hillsdon et al., 2002; Kolt et al., 2007; Lawton et al., 2008; Swinburn et al., 1998), 5 were either written only or combined with face to face delivery (Elley et al., 2003; Grandes et al., 2009; Lewis & Lynch, 1993; Little et al., 2004; Reid & Morgan, 1979).

### 3.1.6 Follow up periods

The follow up periods ranged from one month to 25 months. One study used a one month follow up (Little et al., 2004), five studies used a six months follow up period (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Atay, Torman & Yaman, 2014; Grandes et al., 2009; Norris et al 2000; Reid & Morgan, 1979), five studies used a 12 months follow up period (Hillsdon et al., 2002; Kerse et al., 2005; Kolt et al., 2007; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003), and 3 studies used a 24 months follow up period (Grandes et al., 2011; Green et al., 2002; Lawton et al., 2008). Other studies had variables follow up periods (Elley et al., 2003; Fortier et al., 2011; Goldstein et al., 1999; Lewis & Lynch, 1993; Mutrie et al., 2012; Pears et al., 2016; Pfeiffer et al., 2001; Swinburn et al., 1998; Sørensen et al., 2008; Tylor & Fox, 2005)

### 3.1.7 Outcomes

The primary out comes in the studies were: Total energy expenditure (Kcal/week or Kcal/day) reported in 8 studies (Atay, Torman & Yaman, 2014; Elley et al., 2003; Hillsdon et al., 2002; Kerse et al., 2005; Norris et al., 2000; Pears et al., 2016; Petrella, Lattanzio, Shapiro & Overend, 2010; Tylor & Fox, 2005), Self reported physical activity (minutes/week or hours/week) reported in 17 studies (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Elley et al 2003; Goldstein et al., 1999; Grandes et al., 2009; Grandes et al., 2011; Green et al., 2002; Kerse et al., 2005; Kolt et al., 2007; Lawton et al., 2008; Lewis & Lynch, 1993; Little et al., 2004; Norris et al., 2000; Pears et al., 2016; Pfeiffer et al., 2001; Swinburn et al., 1998; Sørensen et at 2008; Tylor & Fox, 2005,) Aerobic fitness VO2 Max (ml/kg/min or ml/min) reported in six studies (Fortier et al., 2011; Grandes et al., 2009; Grandes et al., 2011; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003;

Reid & Morgan, 1979), Systolic blood pressure(mmHg) reported in seven studies (Atay, Torman & Yaman, 2014; Elley et al., 2003; Hillsdon et al., 2002; Kerse et al., 2005; Lawton et al., 2008; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003) and Body Mass Index (BMI) reported in seven studies (Atay, Torman & Yaman, 2014; Fortier et al., 2011; Hillsdon et al., 2002; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003; Sørensen et at., 2008; Tylor & Fox, 2005). Other reported outcomes include Steps/walking, Body circumferences (waist, hip or limb), Total cholesterol and Blood Glucose.

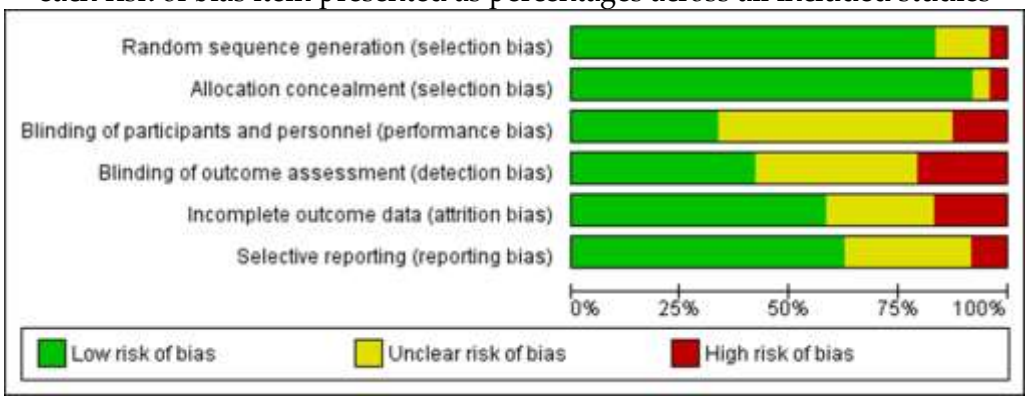
### 3.1.8 Excluded studies

After assessment, the following 31 studies were excluded: 3 did not have a control group (Activity, 2001; Armit et al., 2009; Hardcastle, 2012); 2 did not use a randomised control trial study/cluster randomised control trial design (Bucholz & Purath, 2007; Burn, Camaione & Chartterton, 2000); 3 reported the outcomes as odds ratios and could not be included in the meta analysis (Gao et al, 2016; Harland et al, 1999; Harrison, Roberts & Elton, 2004); 15 were not based in a primary care setup and intervention not health provider based (Bennet et al, 2008; Calfas et al, 1996; Cunningham et al, 1987; Duru et at., 2010; Kriska et al., 1986; Marcus et al., 2007; Pekmezi et al., 2016; Schröder et al,. 2018; Skår et al., 2011; Spittaels, Bourdeaudhuij & Vandelanotte, 2007; Steele, Mummery & Dwyer, 2009; Stralen et al., 2009; Wanner et al., 2009; William et al., 2006); 4 had inadequate data and attempts to reach the authors were futile (Bull & Jamrozik, 1999; Dubbert et al., 2002; Fortier et al., 2006; Jimmy & Martin, 2005); 5 reported different outcomes and/or used different interventions (Kastarinen et al., 2002; Long et al., 1996; Nymberg et al., 2018; Pinto et al., 1998; Rome, Ekdahl & Gard, 2009). See Characteristics of excluded studies.

### 3.2 Risk of bias in included studies

We prepared an assessment of risk of bias for each trial and illustrated final judgements for the ten criteria in Figure 2 and Figure 3. All studies had some methodological shortcomings, in most instances related to unclear risk of bias for different criteria.

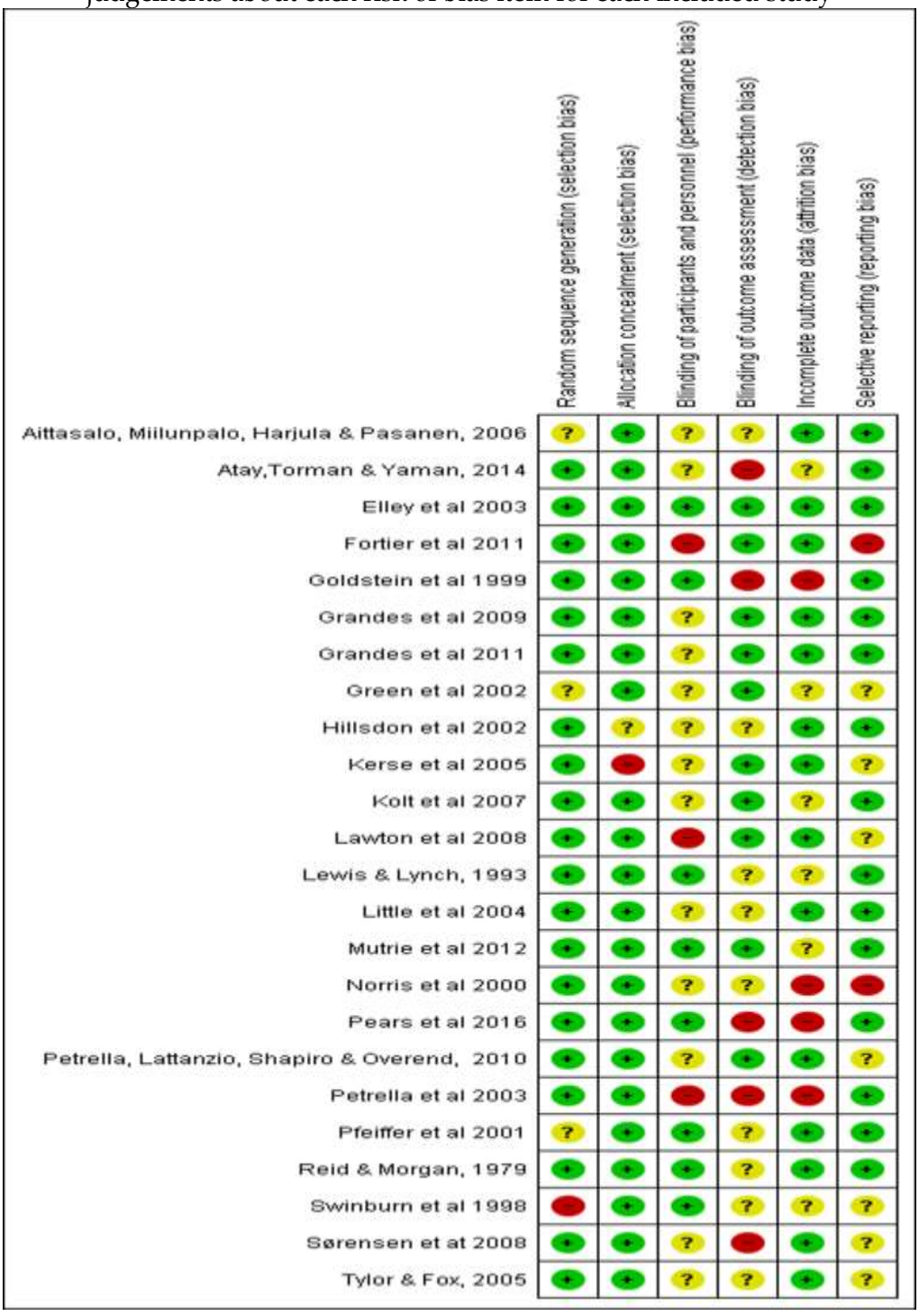
**Figure 2:** Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies



### 3.2.1 Allocation (selection bias)

Allocation bias was generally low in the review with 92% (22) of the studies having low risk. Only one study had a high risk of allocation bias. Most studies stated that participants or practices (in case of cluster randomisation) were assigned randomly, according to a computerised randomisation scheme. For one study, the risk of bias for random sequence generation was unclear owing to poor reporting.

**Figure 3:** Risk of bias summary: review authors' judgements about each risk of bias item for each included study



### 3.2.2 Blinding (performance bias and detection bias)

We judged the risk of performance bias as unclear because no information was available. Of the 24 studies included, 33% (8) studies had low risk, while 54% (13) were classified as having unclear risk because they did not provide sufficient information on blinding of outcome assessment. Only three studies had high risk of performance bias. We expect that patients and personnel were not blinded in these studies because the care provider constitutes the intervention. Whether this lack of blinding influences outcomes is unclear.

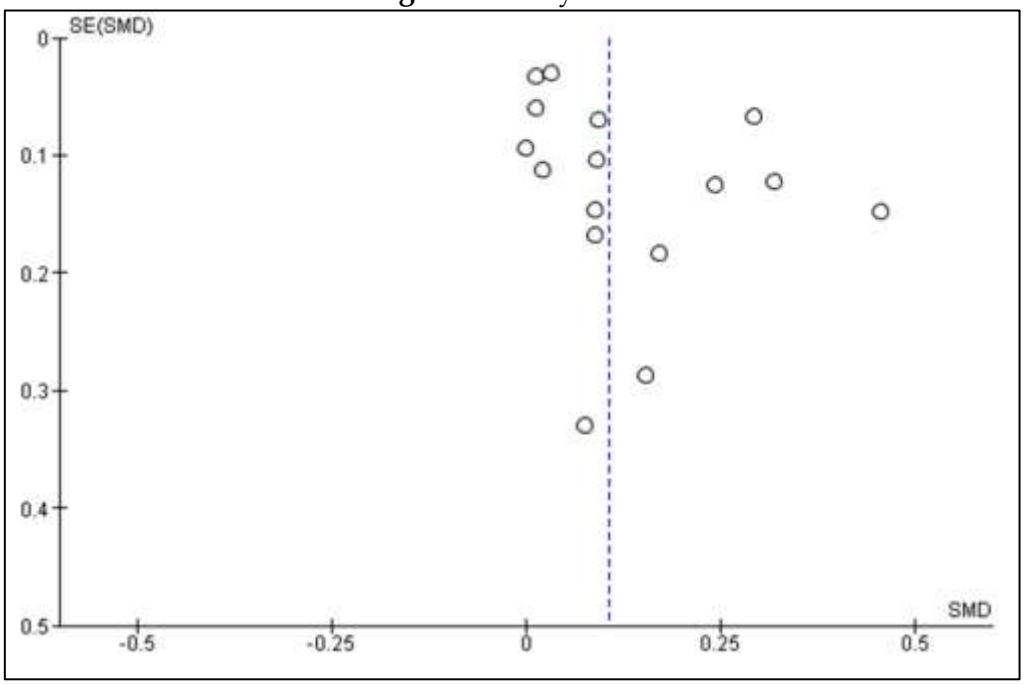
### 3.2.3 Incomplete outcome data (attrition bias)

In half of the studies, 80% or more of the initial participants completed the study. Risk of bias due to incomplete outcome data was unclear in six of the studies because of limited reporting about follow-up. Four studies were classified as having high risk of attrition bias due to high follow up losses and no explanation at all.

### 3.2.4 Selective reporting (reporting bias)

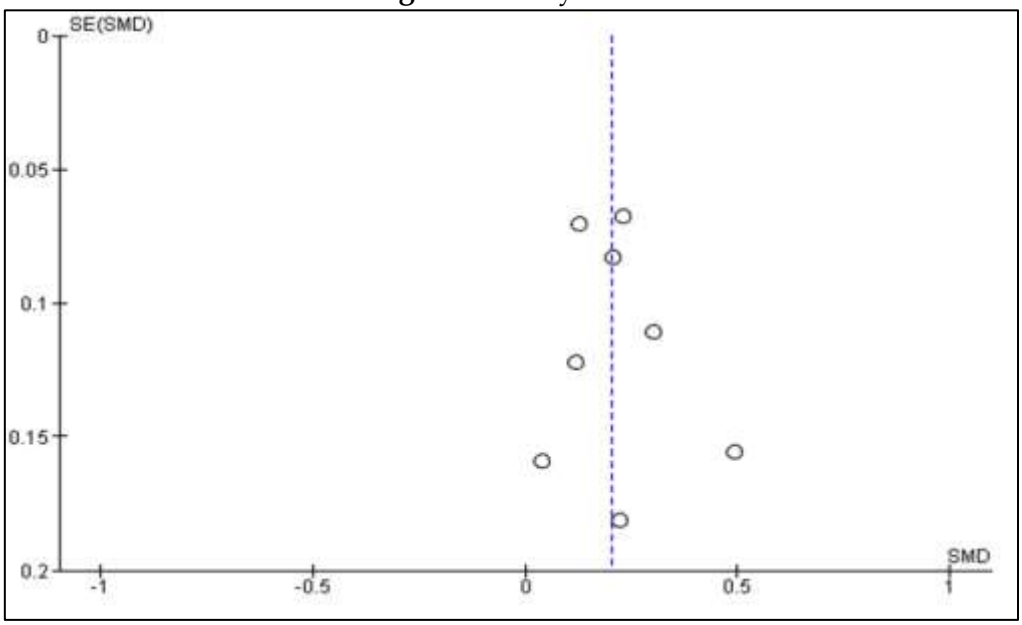
A protocol was available for each study, and 63% (15) of the studies reported predefined outcome measures. Absence of study protocols to confirm reporting of all intended outcomes led to the unclear judgement in seven of the studies.

**Figure 5:** Analysis 1.1



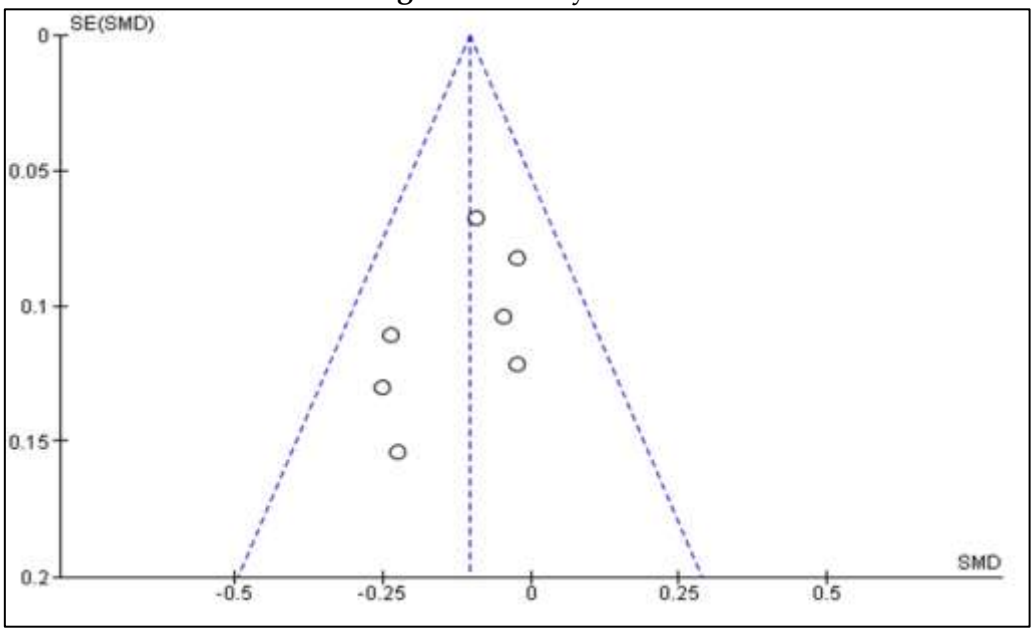

**Note:** Funnel plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.1 Self reports physical activity (minutes/week or hours/week)

**Figure 7:** Analysis 1.2




**Note:** Funnel plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.2 Total energy expenditure (Kcal/week or Kcal/day).

**Figure 11:** Analysis 1.5




**Note:** Funnel plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.5 Systolic blood pressure(mmHg). Effects of interventions

### 3.2.5 Publication bias

Meta-analysis may be vulnerable to publication bias if studies with less favourable results are excluded. A useful test for publication bias is based on the funnel plot, which compares intervention effects estimated from individual studies against a measure of study size. In the absence of bias, the plot resembles a symmetrical inverted funnel (Sterne, 2001).

In the current review, there was no clear evidence of funnel plot asymmetry for the main significant outcomes total energy expenditure (Kcal/week or Kcal/day) and systolic blood pressure (mmHg) (Figure 7; Figure 11), however there was some signs of publication bias on the outcome self reported physical activity (minutes/week or hours/week), this can be attributed to limited studies with small sample size. See funnel plot (Figure 5).

A total of 23 trials investigated patient outcomes (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Atay, Torman & Yaman, 2014; Elley et al., 2003; Fortier et al., 2011; Goldstein et al., 1999; Grandes et al., 2009; Grandes et al., 2011; Green et al., 2002; Hillsdon et al., 2002; Kerse et al., 2005; Kolt et al., 2007; Lawton et al., 2008; Lewis & Lynch, 1993; Little et al., 2004; Norris et al., 2000; Pears et al., 2016; Petrella, Lattanzio, Shapiro & Overend, 2010; Petrella et al., 2003; Pfeiffer et al., 2001; Reid & Morgan, 1979; Swinburn et al., 1998; Sørensen et al., 2008; Tylor & Fox, 2005)

We grouped patient outcomes into the following categories: self reported physical activity (minutes/week or hours/week), Total energy expenditure (Kcal/wee or Kcal/day), aerobic fitness VO2 max (ml/kg/min or ml/min), body mass index (Kg/m$^2$), systolic blood pressure (mmHg), total cholesterol (mmol/l) and waist circumference.

**Tables 1:** Summary of findings

**Patient or population:** primary care: A systematic review and meta-analysis
**Setting:** primary care setting
**Intervention:** Physical activity counselling in primary care setting
**Comparison:** Usual care or placebo

| **Outcomes** | **Anticipated absolute effects* (95% CI)** | | **Relative effect (95% CI)** | **№ of participants (studies)** | **Certainty of the evidence (GRADE)** | **Comments** |
|---|---|---|---|---|---|---|
| | **Risk with placebo** | **Risk with Physical activity counselling in primary care setting versus Usual care or placebo** | | | | |
| Self reports physical activity (minutes/week or hours/week) | The mean self reports physical activity (minutes/week or hours/week) was 0 | SMD 0.11 higher (0.04 higher to 0.17 higher) | - | 13211 (16 RCTs) | ⨁⨁⨀⨀ LOW [1 2 3 4 5] | |
| Total energy expenditure (Kcal/week or Kcal/day) | The mean total energy expenditure (Kcal/week or Kcal/day) was 0 | SMD 0.2 higher (0.13 higher to 0.27 higher) | - | 3376 (8 RCTs) | ⨁⨁⨁⨁ HIGH [1 3 4 6] | |
| Aerobic fitness VO2 Max (ml/kg/min or ml/min) | The mean aerobic fitness VO2 Max (ml/kg/min or ml/min) was 0 | SMD 0.06 higher (0.01 lower to 0.12 higher) | - | 8822 (6 RCTs) | ⨁⨁⨀⨀ LOW [3 7 8 9] | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Body Mass Index (BMI) | The mean body Mass Index (BMI) was 0 | SMD 0.04 lower (0.15 lower to 0.07 higher) | - | 2371 (7 RCTs) | ⊕⊖⊖⊖ VERY LOW [3] [9] [10] [11] | |
| Systolic blood pressure(mmHg) | The mean systolic blood pressure(mmHg) was 0 | SMD 0.1 lower (0.18 lower to 0.03 lower) | - | 2856 (7 RCTs) | ⊕⊕⊕⊖ MODERATE [3] [4] [12] [13] | |
| Total cholesterol | The mean total cholesterol was 0 | SMD 0.27 lower (0.72 lower to 0.18 higher) | - | 2459 (4 RCTs) | ⊕⊖⊖⊖ VERY LOW [1] [3] [11] [14] [15] [16] | |
| Waist circumference | The mean waist circumference was 0 | SMD 0.05 lower (0.15 lower to 0.06 higher) | - | 1469 (4 RCTs) | ⊕⊖⊖⊖ VERY LOW [3] [9] [12] [16] [17] | |

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
**CI:** Confidence interval; **RR:** Risk ratio; **OR:** Odds ratio;

**Grade Working Group grades of evidence**
**High certainty:** We are very confident that the true effect lies close to that of the estimate of the effect
**Moderate certainty:** We are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
**Low certainty:** Our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
**Very low certainty:** We have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] The studies with the highest weights only had unclear performance bias (single-blind). However, some studies noted that it was impossible to blind the personnel in this kind of intervention
[2] The variation of effect sizes in the study was too big (0.02-0.46). The I2 was moderate at 50% and the P-value of heterogeneity was statistically significant (p=0.01)
[3] A few variations in the intervention between studies. Some studies have a combined intervention of written and face to face counselling, telephone, and face to face counselling while others only focus on face to face,
[4] The confidence interval of the overall effect was not wide and the sample size was above the recommended of 400 and the pooled effect did not include a null effect
[5] The funnel plot was asymmetric indicating publication bias
[6] There was a good overlap of confidence intervals amongst studies, the I2 was low at 2% and the P-value of heterogeneity was not statistically significant (P=0.42)
[7] Only one study has signs of performance, detection, and attrition bias
[8] There was a good overlap of confidence intervals amongst studies, the I2 was moderate at 35% and the P-value of heterogeneity was not statistically significant (P=0.17)
[9] The confidence interval of the overall effect included the null effect and it was unclear if there was benefit or harm
[10] There was a high performance, detection, and attrition risk of bias in the studies that had the highest weight
[11] There were huge variations in the size and direction of the effects in the study (0.01 to-0.35). There was also a poor overlap of confidence intervals
[12] The studies with high weight had an unclear performance, detection and attrition risk of bias

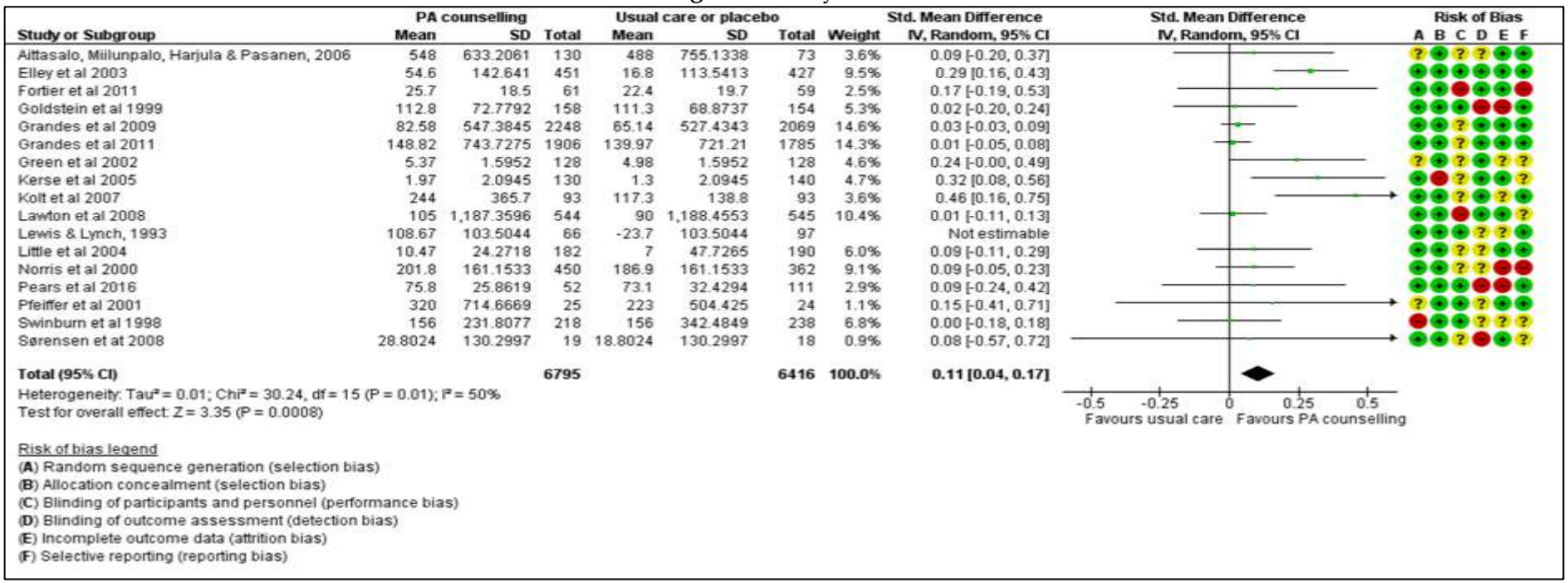
[13] There was a good overlap of confidence intervals amongst studies, all studies had consistency in direction of the effects, the I2 was low at 0% and the p-value of heterogeneity was not statistically significant (p=0.54)
[14] There was little to no overlap amongst confidence intervals, the I2 percentage was 96% indicating heterogeneity amongst studies
[15] There were huge variations in the size and direction of the effects in the study (0.09 to-0.75). There was also a poor overlap of confidence intervals
[16] The studies were below 10 hence we could not generate a funnel plot, however, some studies had signs of selective analysis and poor methodology
[17] There was a good overlap of confidence intervals amongst studies, all studies had consistency in direction of the effects, the I2 was low at 0% and the p-value of heterogeneity was not statistically significant (p=0.42)

**Note:** Physical activity counselling in primary care setting versus usual care or placebo compared to placebo in primary care: a systematic review and meta-analysis

### 3.2.6 Self reported physical activity (minutes/week or hours/week)

Sixteen trials evaluated self reported physical activity in minutes/week (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006; Elley et al., 2003; Fortier et al., 2011; Goldstein et al., 1999; Grandes et al., 2009; Grandes et al., 2011; Green et al., 2002; Kerse et al., 2005; Kolt et al., 2007; Lawton et al., 2008; Little et al., 2004; Norris et al., 2000; Pears et al., 2016; Pfeiffer et al., 2001; Swinburn et al., 1998; Sørensen et al., 2008). Meta-analysis of data from these trials suggests that health provider-led physical activity counselling interventions in primary care may lead to increased physical activity among patients. The pooled effect was positive and high favouring the physical activity counselling interventions over usual care/placebo (random effects analysis P=0.0008). Data show little evidence of statistical heterogeneity. Excluding from the meta-analysis a trial by (Lewis & Lynch, 1993) greatly changed the result. Results differed considerably in the sensitivity analyses (SMD 0.11, 95% CI 0.04 to 0.17; participants = 13211; studies = 16; $I^2$ = 50% Figure 4). A SMD of 0.11 represents a small difference between groups. The evidence is of low certainty (Summary of findings table 1) owing to a small confidence interval and moderate clinical heterogeneity, see, Analysis 1.1.

**Figure 4:** Analysis 1.1

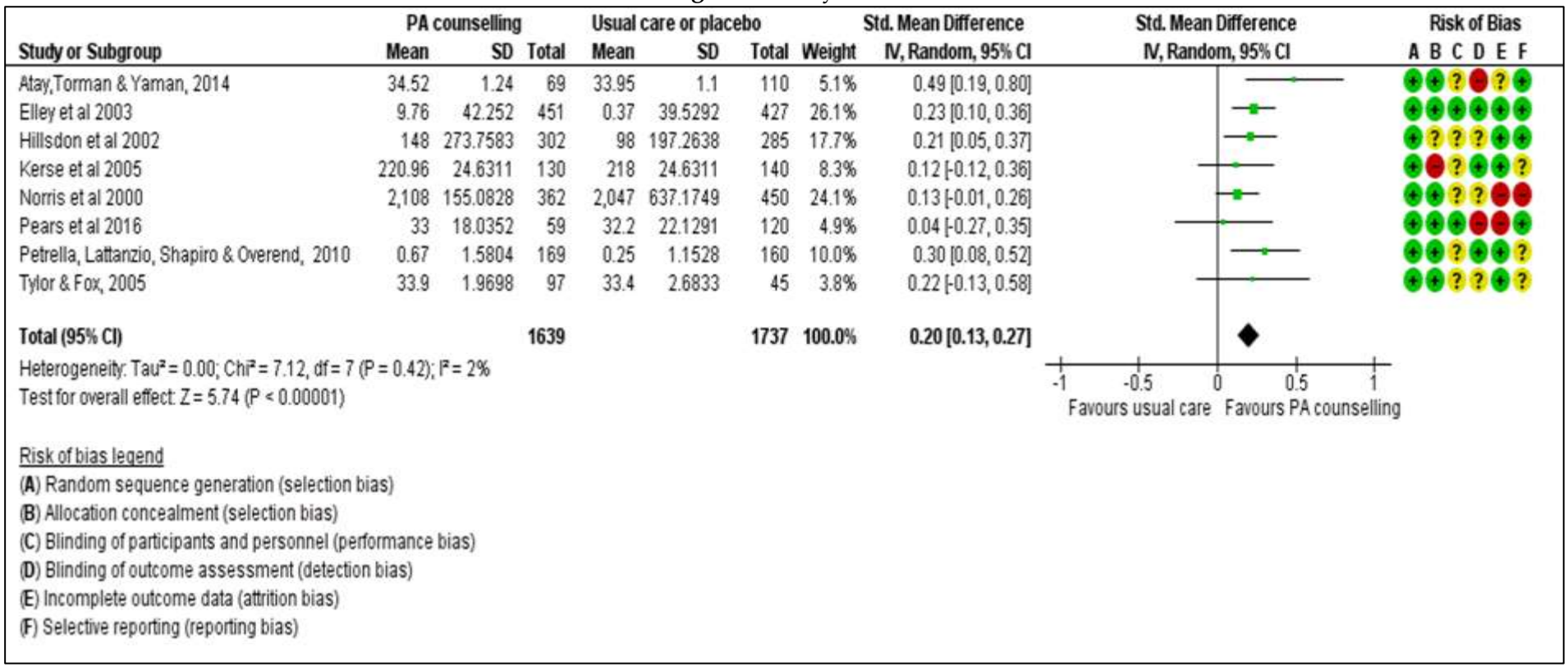



**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.1 Self reports physical activity (minutes/week or hours/week).

### 3.2.7 Total energy expenditure (Kcal/week or Kcal/day)

We found high-quality evidence, based on eight studies (Atay, Torman & Yaman, 2014; Elley et al., 2003; Hillsdon et al., 2002; Kerse et al., 2005; Norris et al., 2000; Pears et al., 2016; Petrella, Lattanzio, Shapiro & Overend, 2010; Tylor & Fox, 2005), that the use health provider-led physical activity counselling interventions in primary care may lead to increased total energy expenditure among patients when compared to usual care alone (SMD 0.20, 95% CI 0.13 to 0.27; participants = 3376; studies = 8; $I^2$ = 2%; overall effect P=0.00001) Figure 6. A SMD of 0.2 represents a small difference between groups and data show no evidence of statistical heterogeneity. See Analysis 1.2:

**Figure 6:** Analysis 1.2



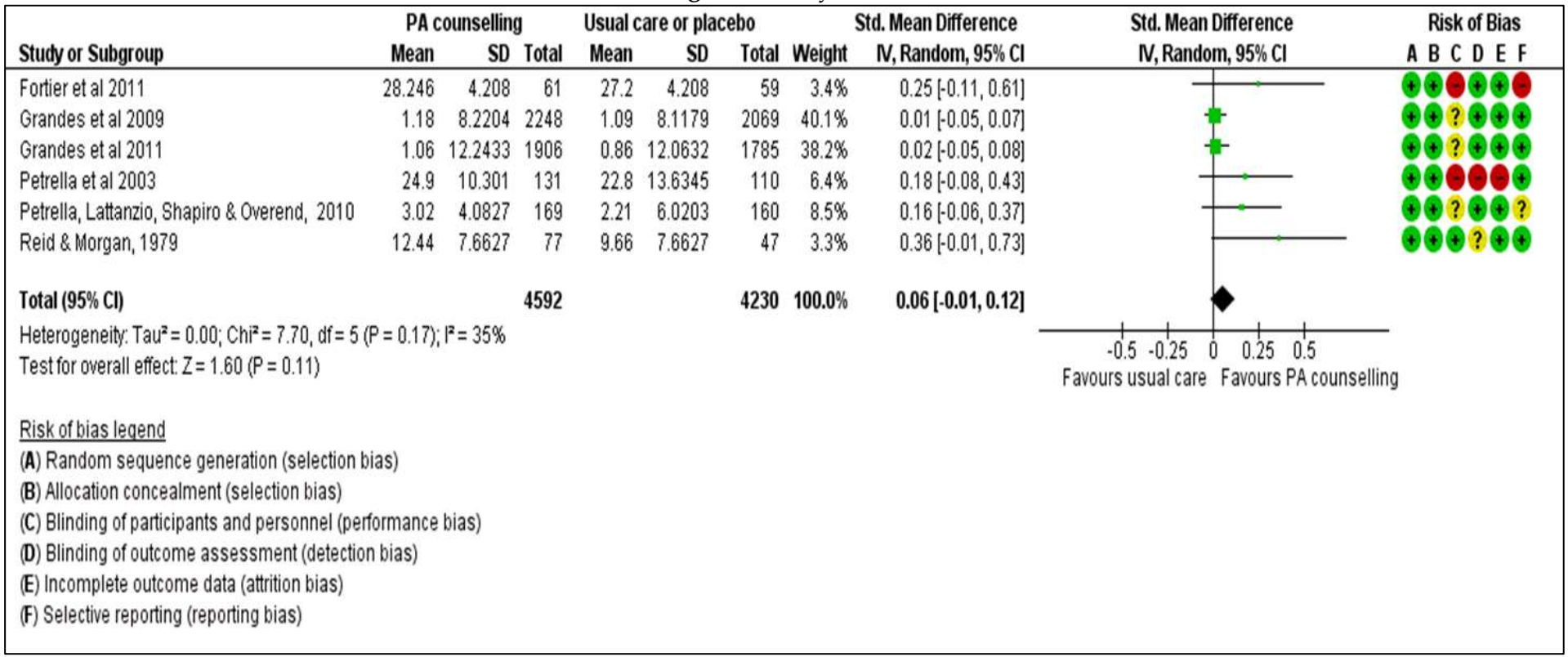

**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.2 Total energy expenditure (Kcal/week or Kcal/day).

### 3.2.8 Aerobic fitness VO2 Max (ml/kg/min or ml/min)

Sixt trials (8822 participants) evaluated aerobic fitness as VO2 max (Fortier et al., 2011; Grandes et al., 2009; Grandes et al., 2011; Petrella et al., 2003; Petrella, Lattanzio, Shapiro & Overend, 2010; Reid & Morgan, 1979). Meta-analysis of data from these trials suggests that health provider-led physical activity counselling interventions in primary care did not lead to increased aerobic fitness among patients(SMD 0.06, 95% CI -0.01 to 0.12; $I^2 = 35\%$) Figure 8. Data show moderate evidence of statistical heterogeneity. The pooled effect

was not significant (random effects analysis P=0.11). The evidence is of low certainty (Summary of findings, Table 1) owing to a large confidence interval and moderate clinical heterogeneity, see, Analysis 1.3:

**Figure 8:** Analysis 1.3

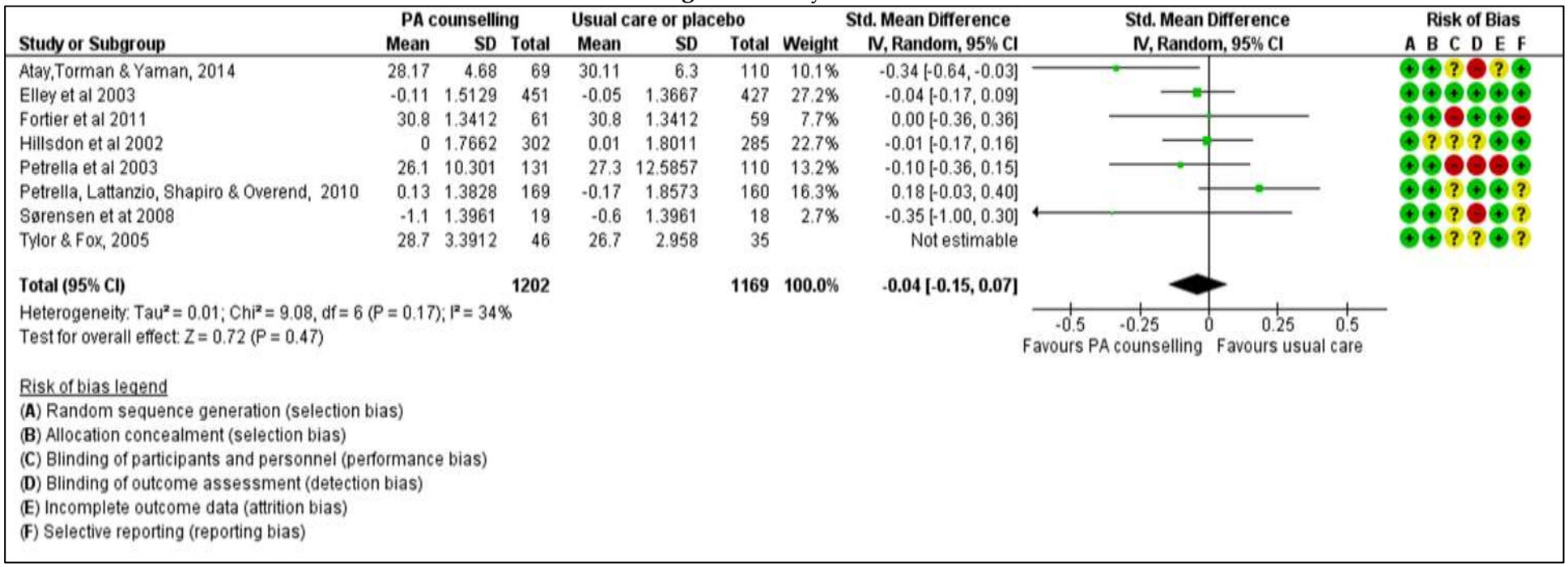



**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.3 Aerobic fitness VO2 Max (ml/kg/min or ml/min).

### 3.2.9 Body Mass Index (BMI) (Kg/m²)

Seven trials (2371 participants) investigated changes in body mass index (Atay, Torman & Yaman, 2014; Elley et al., 2003; Fortier et al., 2011; Hillsdon et al., 2002; Petrella et al., 2003; Petrella, Lattanzio, Shapiro & Overend, 2010; Sørensen et at., 2008; Tylor & Fox, 2005). All the trials provided sufficient data for a meta-analysis. Data show that there may be little or no difference in body mass index (SMD -0.04, 95% CI -0.15 to 0.07) Figure 9. For body mass index, the evidence is of very low certainty (Summary of findings, Table 1) owing to moderate inconsistency ($I^2$ = 34%; overall effect P=0.11; Random effects analysis) and imprecision (wide confidence interval). Results did not change considerably under Sensitivity analysis. See, Analysis 1.4:

**Figure 9:** Analysis 1.4

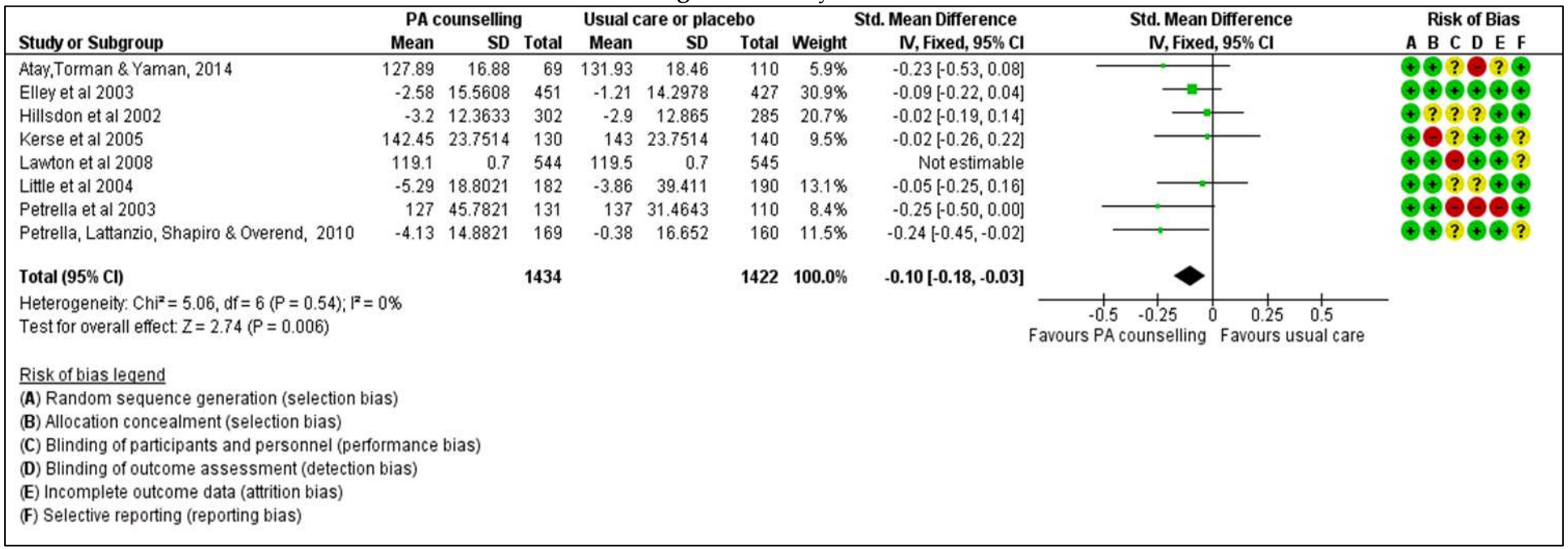



**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.4 Body Mass Index (BMI).

### 3.2.10 Systolic blood pressure (mmHg)

Eight trials (2856 participants) reporting systolic blood pressure at six showed a mean difference (MD) favouring health provider-led physical activity counselling interventions compared to usual care of -0.10 mmHg (95% CI -0.18 to -0.03; Analysis 1.5. The analysis was based on a fixed effects model. One study (Lawton et al., 2008) was excluded during sensitivity analysis. There was no heterogeneity amongst results of the remaining trials (Atay, Torman & Yaman, 2014; Elley et al., 2003; Hillsdon et al., 2002; Kerse et al., 2005; Little et al., 2004; Petrella et al., 2003; Petrella, Lattanzio, Shapiro & Overend, 2010) ($I^2$ = 0%, overall effect P = 0.006) Figure 10 .The evidence is of moderate certainty owing to a small confidence interval and small clinical heterogeneity (Summary of findings, Table 1).

**Figure 10:** Analysis 1.5

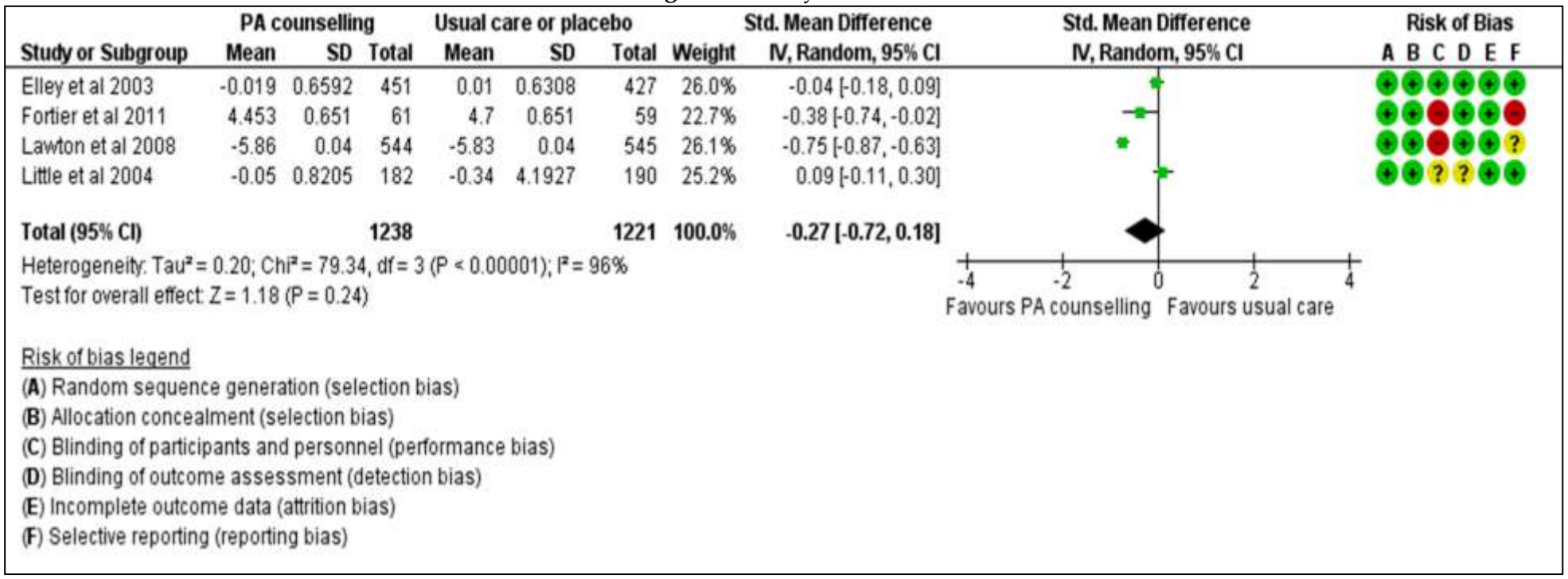



**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.5 Systolic blood pressure(mmHg).

### 3.2.11 Total cholesterol (mmol/l)

Four trials (2459 participants) investigated total cholesterol (Elley et al., 2003; Fortier et al., 2011; Lawton et al., 2008; Little et al., 2004).The meta-analysis suggests that there may be little or no difference between health provider-led physical activity counselling interventions in primary care and usual care (SMD -0.27, 95% CI -0.72 to 0.18; very low-certainty evidence; Analysis 1.6 ). For total cholesterol, evidence is of very low certainty (Summary of findings, Table 1) owing to strong inconsistency ($I^2$ = 96%;overall effect P=0.24; random effects analysis Figure 12 ) and a wide confidence interval, suggesting that the extent to which the total cholesterol differs between health provider-led physical activity counselling interventions in primary care and usual care varied with the context of physical activity. Results did not change considerably in the Sensitivity analysis.

**Figure 12:** Analysis 1.6

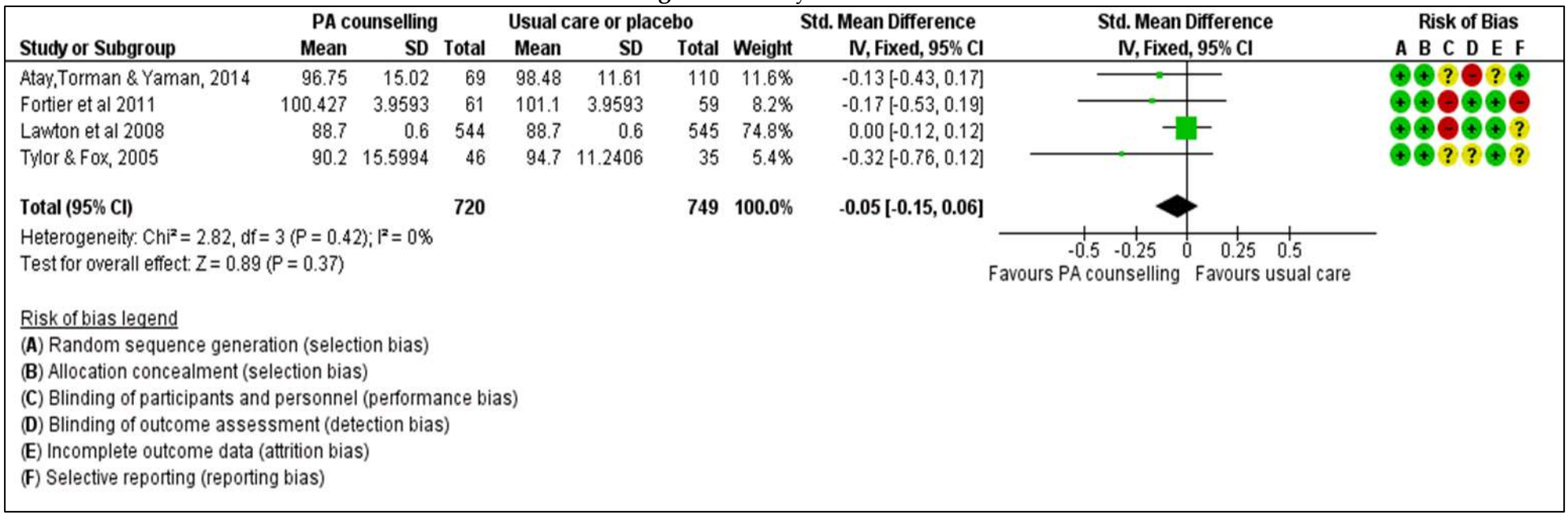



**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.6 Total cholesterol.

### 3.2.12 Waist circumference (cm)

Four trials (1469 participants) investigated waist circumference (Atay, Torman & Yaman, 2014; Fortier et al., 2011; Lawton et al., 2008; Tylor & Fox, 2005). All the trials provided sufficient data for a meta-analysis. The meta-analysis suggests that there may be little or no difference between health provider-led physical activity counselling interventions in primary care and usual care (SMD -0.05, 95% CI -0.15 to 0.06; very low-certainty evidence; Analysis 1.6 ). For total cholesterol, evidence is of very low certainty (Summary of findings, Table 1) owing to a wide confidence interval (imprecision), suggesting that the extent to which the waist circumference differs between health provider-led physical activity counselling interventions in primary care and usual care varied with the context of physical activity. There was no heterogeneity amongst study results ($I^2$ = 0%; overall effect P=0.24; fixed effects analysis). Results did not change considerably in the Sensitivity analysis.

**Figure 13:** Analysis 1.8




**Notes:** Forest plot of comparison: 1 Physical activity counselling in primary care setting verses Usual care or placebo, outcome: 1.8 Waist circumference.

## 4. Data and analyses

**Table 2:** Physical activity counselling in primary care setting verses Usual care or placebo

| Outcome or Subgroup | Studies | Participants | Statistical Method | Effect Estimate |
|---|---|---|---|---|
| 1.1 Self reports physical activity (minutes/week or hours/week) | 16 | 13211 | Std. Mean Difference (IV, Random, 95% CI) | 0.11 [0.04, 0.17] |
| 1.2 Total energy expenditure (Kcal/week or Kcal/day) | 8 | 3376 | Std. Mean Difference (IV, Random, 95% CI) | 0.20 [0.13, 0.27] |
| 1.3 Aerobic fitness VO2 Max (ml/kg/min or ml/min) | 6 | 8822 | Std. Mean Difference (IV, Random, 95% CI) | 0.06 [-0.01, 0.12] |
| 1.4 Body Mass Index (BMI) | 7 | 2371 | Std. Mean Difference (IV, Random, 95% CI) | -0.04 [-0.15, 0.07] |
| 1.5 Systolic blood pressure (mmHg) | 7 | 2856 | Std. Mean Difference (IV, Fixed, 95% CI) | -0.10 [-0.18, -0.03] |
| 1.6 Total cholesterol | 4 | 2459 | Std. Mean Difference (IV, Random, 95% CI) | -0.27 [-0.72, 0.18] |
| 1.8 Waist circumference | 4 | 1469 | Std. Mean Difference (IV, Fixed, 95% CI) | -0.05 [-0.15, 0.06] |

## 5. Discussion

### 5.1 Summary of main results

We included 23 randomised controlled trials (RCTs) and cluster randomised trials examining 15,069 patients aged between 18 to 80 years in this review. Sixteen studies examined self-reported physical activity in minutes/week (N =13211 randomised); eight studies examined total energy expenditure (N = 3376 randomised); six studies examined aerobic fitness (N=8822 randomised); seven trials examined body mass index (N=2371 randomised); seven trials examined systolic blood pressure (N=2856 randomised); four trials examined total cholesterol (N=2459 randomised) and four trials examined waist circumference (N = 2385 randomised). Findings suggest that counselling delivered by health providers, probably leads to similar or better physical activity outcomes for patient conditions (moderate-certainty evidence). However, these results must be interpreted with a degree of caution, recognising the variation in interventions reported within studies and the complex interplay of factors affecting outcomes. Several studies included multiple intervention methods, which made it difficult to tease apart which intervention components were the active ingredients. The effectiveness of the interventions was not significant in various outcomes. There was a paucity of data reporting on adverse events.

In summary:

1) Counselling delivered by health providers may lead to improved self reported activity, compared to usual care. However, the SMD was small (0.11).
2) Total energy expenditure and blood pressure outcomes are probably slightly improved in health provider counselling group.

3) There were no observed significant improvements in aerobic fitness, body mass index, total cholesterol and waist circumference
An overview can be found in (Summary of findings, Table 1)

**5.2 Overall completeness and applicability of evidence**

Several issues need to be considered when one is making judgements about the applicability of these findings in primary care setting. First, we were able to identify a moderate number of studies published up to 2014, which were sufficient to address all objectives of the review. These studies are highly varied in terms of types of health providers (physicians and nurses), healthcare setting, and geographical settings, and they examine counselling provided to general patient populations as well as to specific groups of patients, such as people with risk factors for non-communicable diseases. Next, we found a large variation in outcome measures. For a number of outcomes there were only a few contributing studies whereas for some other outcomes a relatively large number of studies contributed to the evidence. Furthermore, often details (such as type of health provider and type of intervention) were missing or unclear from study reports. Therefore, we were not able to conduct planned subgroup analyses. As a result, it is not possible to draw conclusions on the influence of health provider type and different types of the interventions on outcomes. In addition, all included studies were conducted in high-income country settings (Finland, Turkey, New Zealand, Spain, US, Canada, UK and Denmark). In other studies (Aittasalo, Miilunpalo, Harjula & Pasanen, 2006), patients were given an appointment but in others they were only advised to see their physicians (Elley et al., 2003), or included a longer time slot for physical activity consultations (Grandes et al., 2009). These differences in the interventions provided might have influenced study outcomes. Last, over the years, primary care services have changed considerably in many settings. However, we did not identify a trend in types of physical activity counselling interventions or in changes in outcomes assessed that might reflect changes in primary care services.

**5.3 Quality of the evidence**

We assessed the quality of evidence per outcome using the GRADEpro GDT. All studies had some methodological shortcomings, such as contamination and lack of blinding both patients and personnel, which sometimes led to downgrading of the evidence owing to risk of bias. Although lack of blinding is considered a shortcoming, blinding is often not possible for organizational interventions, such as administering of physical activity counselling and the impact of this on outcomes is unclear. Overall, there was low-quality evidence for outcomes because of high risks of bias, imprecision of effect estimates and inconsistent results. We downgraded our assessments of the quality of evidence produced by the included studies because of risk of bias, lack of blinding and use of subjective outcome measures (detection bias), lack of information on sequence generation (selection bias), and lack of information on allocation concealment (selection bias). Besides the paucity of usable data, the main quality concerns were use of subjective

outcome measures (detection bias), which occurred for all the interventions. Overall, reports were poorly written and, in some cases, it was difficult to follow the analyses, although the more recent studies have improved in their design and analyses.

**5.4 Potential biases in the review process**

The process of study selection, data extraction, and assessment of risk of bias of included studies was performed by two independent review authors and we resolved disagreements through discussion and consensus. Our search strategy was designed to maximise sensitivity (detecting relevant research) at the expense of specificity (excluding irrelevant research). Even so, relevant research proved difficult to identify, and some studies may have been missed. We conducted this review according to Cochrane standards. Therefore, we are confident in the quality of the review itself. Although publication bias cannot be ruled out in this area, it seems unlikely that this bias could be substantial. We were unable to assess the risk of publication bias adequately as there was a very limited number of studies assessing similar types of interventions and outcomes. We had considerable difficulty in classifying the interventions and we might have been too restrictive in combining studies. However, we believe that the broad categories of physical activity interventions that we made have resulted in a meaningful categorisation. Thus, we believe it is possible to get at least an impression of the effectiveness of interventions in the various outcomes.

**5.5 Agreements and disagreements with other studies or reviews**

Our results mirrored in other systematic reviews of PA interventions for older adults. A review by Hillsdon, Thorogood & Foster, 2013 found that there was a positive and moderate pooled effect on self-reported PA at 12 months (SMD 0.19; 95% CI 0.06 to 0.31), however the systematic review focused on face to face interventions only. A study by Conn (2011) reported on randomised and non-randomised PA interventions (n = 99,011 participants) and calculated a mean effect size for the comparison of intervention groups versus control groups that was positive but not significant (SMD 0.19; 95% CI –0.14 to 0.53). Hobbs (2013) reported an identical pooled effects size for PA interventions for older adults using counselling and lifestyle advice for self-reported PA outcomes (SMD 0.19; 95% CI 0.10 to 0.28). Foster et al. (2005) found that the effect of interventions on self-reported physical activity (19 studies; 7598 participants) was positive and moderate (pooled SMD random effects model 0.28 95% CI 0.15 to 0.41) as was the effect of interventions (11 studies; 2195 participants) on cardio-respiratory fitness (pooled SMD random effects model 0.52 95% CI 0.14 to 0.90). There was significant heterogeneity in the reported effects as well as heterogeneity in characteristics of the interventions. The heterogeneity in reported effects was reduced in higher quality studies, when physical activity was self-directed with some professional guidance and when there was on-going professional support.

### 5.6 Authors' conclusions

#### 5.6.1 Implications for practice

The review concludes that to initiate important increases in physical activity, both exercise prescription and counselling should be incorporated in standard practice. We recommend as a tool for primary health care physicians to promote PA, especially at health check and control visits, where more time can be allocated for the appointment. A more comprehensive familiarization protocol or training sessions are needed to implement physical counselling to everyday practices and facilitate counselling cooperation. Self-monitoring of PA with expert feedback can be a useful and cheaper way of increasing especially the duration of overall weekly PA in the short term.

#### 5.6.2 Implications for research

From the review conclusions, comprehensive approaches involving repeated interventions that include behaviour change techniques and booster programs might enhance the long-term effectiveness of advice given by physicians and other health care professionals. Future studies should address the modelling and evaluation of these new and complex interventions and also demonstrate what intensity of follow-up is needed for longer-term maintenance

**Acknowledgements**

The authors wish to acknowledge Prof Edwin Kadima Wamukoya, and Dr Maximilla Wanzala for their contributions and support during the writing of this review.

**Contributions of authors**

Micky Olutende Oloo, Prof Edwin Wamukoya and Dr. Maximilla Wanzala conceived the paper, designed and performed the review. All authors read and approved the final manuscript

**Declarations of interest**

The authors declare that they have no competing interests. The findings and conclusions presented in this manuscript are those of the authors and do not necessarily reflect the official position of Masinde Muliro University of Science and Technology

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**c. Studies awaiting classification**

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**c. Sources of support**
**Internal sources**

- No sources of support provided

**External sources**

- No sources of support provided

## B. Characteristics of studies

## a. Characteristics of included studies

### 1. Aittasalo, Miilunpalo, Harjula & Pasanen (2006)

| | |
|---|---|
| **Methods** | The study was a cluster randomised trial conducted in Finland during 2003–2004. |
| **Participants** | Physicians from 24 health care units (N = 67) were randomised to a prescription or a non-prescription group. The patients (N = 265) were assigned to the groups according to their physician. Every other patient of the non-prescription physicians received a pedometer and a physical activity log (MON) and feedback about their 5-day-recordings, the rest served as controls (CON). PA was assessed prior and 2 and 6 months after the physician's appointment with a questionnaire. All 20 – 65-year-old patients who made an appointment with the participating physicians were asked to participate. The inclusion criteria were 30 min of moderate-intensity PA on fewer than 4 days weekly and no perceived obstacles for PA |
| **Interventions** | The counselling proceeded according to the framework of 5 A's :Assessment of patients' prevailing PA habits and possibilities and willingness to increase PA, Advice on sufficient PA regarding health and potential personal benefits of PA, Agreement on PA goals and a weekly PA plan, Assistance at identifying PA barriers and suitable exercise services and Arrangements for a control visit with a precise date including a possibility to use a PA log for self monitoring. To support cooperation with other health care and exercise workers, ''Prex'' could be used as a referral to physiotherapists, nurses or exercise experts. |
| **Outcomes** | Number of overall weekly PA sessions, Number of at least moderate-intensity weekly PA sessions, Overall weekly PA in minutes, duration of at least moderate-intensity weekly PA in minutes |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Unclear risk | The receptionists were not able to give the screening questionnaire to all the patients intended, which may have caused selection bias |
| Allocation concealment (selection bias) | Low risk | The physicians were randomised into the study groups |
| Blinding of participants and personnel (performance bias) | Unclear risk | Single blinding as physicians were aware of the intervention. Secondly, lack of time and staff holidays prolonged the recruitment period, which may have increased variation in PA due to seasonal reasons. |
| Blinding of outcome assessment (detection bias) | Unclear risk | PA data were based on self-reports, which can be susceptible to over- or underreporting |
| Incomplete outcome data (attrition bias) | Low risk | Process evaluation was included in the analysis and the dropout rate of patients at both 2- and 6-month follow-up was small. |
| Selective reporting (reporting bias) | Low risk | All outcomes were reported |

## 2. Atay, Torman & Yaman (2014)

| | |
|---|---|
| **Methods** | The study was a randomised control trial with parallel groups |
| **Participants** | Primary care health centres from the metropolitan area of Antalya, Turkey, were chosen as study and intervention sites. Thirty nine primary health care centres were addressed and 33 centres were eligible for this study. Sedentary patients aged 50-70, without any physical health condition participated voluntarily to this study. |
| **Interventions** | Doctors from both groups attended the theoretical part (14 hours) of the exercise prescription course and the intervention group also attended the practical part of the course (10 hours). At the end of the 24 hours of training the doctors in the intervention group were given a "Doctor's Manual". In addition, the IG received instructional material and an equipment kit, which was prepared for the IG patients |
| **Outcomes** | The Seven Day Recall Physical Activity Questionnaire was used for estimating daily energy consumption. Physical performance measurements included the chair stand test, sit and reach test, back scratch test, balance test, and resistance band maximum repetition frequency test. In addition, heart rate, body weight, height, hip and waist circumference. measurements were also applied |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Volunteer participants were evaluated at the primary health care centres and recruited |
| Allocation concealment (selection bias) | Low risk | Participating doctors in the study were divided into two groups, IG and CG, by random selection |
| Blinding of participants and personnel (performance bias) | Unclear risk | There was single blinding in the study. Doctors were not blinded to the intervention |
| Blinding of outcome assessment (detection bias) | High risk | Turkish version of Borg's Scale (Rating of Perceived Exertion Scale-RPE) was given to the participants of the study to enable them to measure the intensity of their activity. There was a possibility of assessment bias |
| Incomplete outcome data (attrition bias) | Unclear risk | There was explanation on withdrawal from the study. Unclear if the authors used an intention to treat analytic approach |
| Selective reporting (reporting bias) | Low risk | All outcome results were reported |

## 3. Elley et al. (2003)

| | |
|---|---|
| **Methods** | Cluster randomised controlled trial. Practices were randomised before systematic screening and recruitment of patients. |
| **Participants** | The study setting was 42 rural and urban general practices in one region of New Zealand. Subjects were all sedentary 40-79-year-old patients visiting their general practitioner during the study's recruitment period |
| **Interventions** | General practitioners were prompted by the patient to give oral and written advice on physical activity during usual consultations. Exercise specialists continued support by telephone and post. Control patients received usual care. |

| | |
|---|---|
| **Outcomes** | Change in physical activity, quality of life (as measured by the "short form 36" (SF-36) questionnaire), cardiovascular risk (Framingham and D'Agostino equations), and blood pressure over a 12-month period |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Rolling recruitment of patients from each practice was spread evenly from April 2000 to April 2001. |
| Allocation concealment (selection bias) | Low risk | Participating general practices were stratified by size and computer randomised them at a distant site before recruiting patients |
| Blinding of participants and personnel (performance bias) | Low risk | Patients remained blind to whether they had been allocated to the intervention during screening for activity and enrolment. |
| Blinding of outcome assessment (detection bias) | Low risk | In measuring variables electronically, they used signed witness statements of verification to minimise assessor bias. |
| Incomplete outcome data (attrition bias) | Low risk | No patients were excluded after enrolment. All outcome analyses were by intention to treat, according to random allocation |
| Selective reporting (reporting bias) | Low risk | All variables assessed were reported regardless of significance |

## 4. Fortier et al. (2011)

| | |
|---|---|
| **Methods** | The study was a 25-week randomised controlled trial in Canada |
| **Participants** | Patients were recruited from a large community-based primary care practice in Ottawa, Canada. Men and women aged 18 to 69 years who were visiting the practice during the recruitment period for a scheduled nonemergency visit, who were identified as inactive (<150 min of physical activity per week: yes–no) through screening procedures in the waiting room before their visit, and who reported no uncontrolled medical conditions |
| **Interventions** | After receiving brief physical activity counselling from their provider, they were randomised to receive 6 additional patient-centred counselling sessions over 3 months from a physical activity counsellor (intensive-counselling group; n = 61), or no further intervention (brief-counselling group; n = 59).The brief intervention consisted of the first 4 As of the 7 As shared-care model for physical activity counselling in primary care: (A1) Addressing the subject of physical activity, (A2) Asking patients about their physical activity, (A3) Advising them to increase their physical activity while personalizing the benefits, and (A4) Agreeing on a 1-month leisure-time physical activity goal. |
| **Outcomes** | Physical activity was measured using self-report and accelerometer. |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|

| | | |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Between May and September 2005, trained research assistants recruited patients from a large community-based primary care practice in Ottawa, Canada |
| Allocation concealment (selection bias) | Low risk | The patients were randomly assigned to either the intensive-counselling group, which received an additional intervention from the physical activity counsellor, or the brief-counselling group, which received no further intervention. |
| Blinding of participants and personnel (performance bias) | High risk | Patient, research assistant, and physical activity counsellor blinding after randomizations was not feasible in this trial |
| Blinding of outcome assessment (detection bias) | Low risk | Research assistant who conducted the physical and metabolic testing was blinded to group assignment |
| Incomplete outcome data (attrition bias) | Low risk | In the intent-to-treat analyses was applied hence participants lost to follow up were also included in the analysis. |
| Selective reporting (reporting bias) | High risk | Only data for patients with at least 4 completed days (including at least 1 weekend day) for each time point were included in the statistical analysis |

## 5. Goldstein et al. (1999)

| | |
|---|---|
| **Methods** | The study design was a cluster randomised control trial. randomizations by practices and not by patients. Twenty-four community-based primary care medical practices were recruited into the study; 12 were randomised to the Intervention condition and 12 to the Control condition. The intervention period was 4-7 weeks. |
| **Participants** | The study was done in U.S. Potential subjects were contacted on the telephone to determine eligibility, obtain informed consent, and gather baseline information. We excluded patients who were too active (moderate exercise for ->30 minutes at least 5 days each week or vigorous exercise for -->20 minutes on at least 3 days per week), were not ambulatory, and those unable to provide information on the telephone. Three hundred and fifty-five patients were enrolled in the study (181 in the Intervention practices and 174 in the Control practices). The mean age of the sample was 65.6 years, a majority were women (65%), |
| **Interventions** | a patient-cantered counselling approach which emphasizes interviewing skills that permit tailoring of the counselling message. The counselling strategy utilizes the "5 As" (address the agenda, assess, advise, assist, and arrange follow-up). The intervention was based on the Transtheoretical Model which considers the individual's performance of the desired behavior and the intention to maintain or change this pattern of behavior |
| **Outcomes** | Assessment was done At baseline, 6 weeks, and 8 months. Physical Activity Scale for the Elderly (PASE): |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|

| | | |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Thirty-four physicians from 24 practices were recruited from Folio lists (20) of primary care practices (i.e. general internal medicine, family medicine) |
| Allocation concealment (selection bias) | Low risk | Practices were matched on whether they were solo or group practices; one practice in each pair of solo/group practices was randomised into the Intervention condition and the other into the Control condition |
| Blinding of participants and personnel (performance bias) | Low risk | The study employed a double blinded approach. Office staff at all practices attended a half-hour administrative session. If randomised to the Intervention condition, physicians attended a one-hour training session on physical activity counselling and provided physical activity counselling during a routine initial office visit and at a follow-up appointment scheduled within 4 weeks of the initial appointment. Physicians in the Control practices were not provided physical activity counselling training and were not expected to schedule patients for a follow-up visit for physical activity counselling. Patients were also blinded to the groups they belonged to and had no knowledge of existence of the intervention. |
| Blinding of outcome assessment (detection bias) | High risk | Assessment was done by trained staff however there was no blinding of the outcome assessors |
| Incomplete outcome data (attrition bias) | High risk | The final analysis excluded the patients that were lost to follow up and no explanation given to participants that did not complete the study. The study didn't employ an intention to treat analysis approach |
| Selective reporting (reporting bias) | Low risk | all outcomes were reported in the analysis despite the results being significant or not. |

## 6. Grandes et al. (2009)

| | |
|---|---|
| **Methods** | The study was a pragmatic, cluster randomised controlled clinical trial in Spain from October 2003 to December 2004. Fifty-six Spanish family physicians were randomised to either the intervention(n=29) or standard care (n=27) arm of the trial. The study took a duration of six months |
| **Participants** | The study was done at 11 public primary care centres with family physicians as allocation units. All 15 research groups of the Spanish Preventive Services and Health Promotion Primary Care Research Network were invited to participate. After signing a collaboration consent form, physicians were randomised to either the PEPAF or usual care (control) arm of the trial in a 1:1 ratio using computer-generated random numbers stratified by centre and provided by a central site. Family physicians recruited patients aged 20 to 80 years, who did not meet the recommended aerobic physical activity levels (moderate-intensity physical activity for30 minutes 5 d/wk. or vigorous intensity activity for 20 minutes 3 d/wk.). |
| **Interventions** | Physicians allocated to the PEPAF arm provided brief advice and educational materials to all patients and offered an additional 15-minute appointment to prescribe an individualized physical activity plan. Control group physicians delivered standard care and delayed any new systematic intervention related to physical activity until the end of the study |

| | |
|---|---|
| **Outcomes** | The primary outcome was 7-Day Physical Activity Recall (PAR) measured as moderate and vigorous activity (minutes /week) and moderate and vigorous activity (MET-h/week). Secondary outcome measures were maximum oxygen uptake ($\dot{V}O_2max$) |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | After meeting requirements for inclusion, 4317 patients were randomly included in the study. At baseline, both groups had similar values of outcome variables. |
| Allocation concealment (selection bias) | Low risk | physicians were randomised to either the PEPAF or usual care (control) arm of the trial in a 1:1 ratio using computer-generated random numbers stratified by centre and provided by a central site. |
| Blinding of participants and personnel (performance bias) | Unclear risk | Single blinded study as the assessors were not blinded to the intervention |
| Blinding of outcome assessment (detection bias) | Low risk | physicians assessed the patients' physical activity with assistance of a computerized algorithm. To avoid recruitment bias, candidates to be assessed by their physicians were systematically sampled by research nurses from the list of patients scheduled for consultation. |
| Incomplete outcome data (attrition bias) | Low risk | The study employed intention to treat analysis. The 6-month follow-up visit was completed by 81% of patients. Patients lost to follow-up did not differ between the groups, except by age and social class |
| Selective reporting (reporting bias) | Low risk | All outcomes assessed were reported regardless of significance of results. For example, there were no significant differences in secondary outcomes between groups |

## 7. Grandes et al. (2011)

| | |
|---|---|
| **Methods** | A 24 months pragmatic, cluster randomised clinical trial initiated in October 2003 |
| **Participants** | The study was conducted in eleven public primary care centres in Spain. Fifty-six general practitioners (GPs) were randomly assigned to intervention (29) or standard care (27) groups. They assessed the physical activity level of a systematic sample of patients in routine practice and recruited 4317 individuals (2248 intervention and 2069 control) who did not meet minimum physical activity recommendations |
| **Interventions** | Intervention GPs provided advice to all patients and a physical activity prescription to the subgroup attending an additional appointment (30%). A third of these prescriptions were opportunistically repeated. Control GPs provided standard care |
| **Outcomes** | Primary outcome measure was the change in self-reported physical activity from baseline to six, 12 and 24 months. Secondary outcomes included cardiorespiratory fitness and health-related quality of life. |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|

| | | |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | General practitioners were invited to participate through the Spanish Preventive Services and Health Promotion Primary Care Research Network |
| Allocation concealment (selection bias) | Low risk | a total of 70 family physicians as allocation units from 13 health centres were randomised before patient recruitment, in a 1:1 ratio using computer-generated random numbers provided by a central site |
| Blinding of participants and personnel (performance bias) | Unclear risk | The study was single blinded as general practitioners were aware of the intervention but the patients were not knowledgeable on the intervention being administered |
| Blinding of outcome assessment (detection bias) | Low risk | Trained nurses working in exercise laboratories who performed baseline and follow-up measurements at 6, 12 and 24 months were blinded to the allocation group of the participants |
| Incomplete outcome data (attrition bias) | Low risk | A telephone recall system was used to improve follow-up rates |
| Selective reporting (reporting bias) | Low risk | All outcomes were reported regardless of significance of results |

## 8. Green et al. (2002)

| | |
|---|---|
| **Methods** | This was a randomised controlled trial, conducted from 1997 to 1998, in Washington State U.S. |
| **Participants** | The setting was one family physician's patients in a suburban community. patients aged 18 to 65 who were recruited from a mailed health risk assessment. Patients were eligible for this study if they were inactive (exercised 15 minutes per day, even if they exercised daily) and specified that they were interested in increasing their exercise in the next 6 months |
| **Interventions** | Three sessions of telephone-delivered motivational counselling |
| **Outcomes** | Physical activity score (11-item Physician-Based Assessment and counselling for Exercise [PACE]) 6 months after the intervention |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Unclear risk | Patients were randomly recruited from 1330 adults |
| Allocation concealment (selection bias) | Low risk | A total of 316 patients were included in this study and were randomly assigned using a standard random number generator to be offered the intervention or be a control. |
| Blinding of participants and personnel (performance bias) | Unclear risk | This study was single blinded because patients were blinded to group allocation, but health providers were not blinded. |
| Blinding of outcome assessment (detection bias) | Low risk | All controls and all those in the intervention group (both those who received the intervention and those who did not) were questioned at baseline and 6 months after enrolment in the intervention by a study interviewer, independent of the health educator, who was blinded as to the participants' intervention status. |

| | | |
|---|---|---|
| Incomplete outcome data (attrition bias) | Unclear risk | Treatment groups were analysed as randomised in an intent-to treat analysis however In secondary analysis, differences between treatment groups were analysed as treatment received. |
| Selective reporting (reporting bias) | Unclear risk | All outcome results were reported regardless of significance. |

## 9. Hillsdon et al. (2002)

| | |
|---|---|
| **Methods** | The study was a randomised control trial in Wellingborough, England between February 1996 and May 1997 |
| **Participants** | Participants were included in the study if they were aged 45-64 years and had less than four occasions of moderate intensity physical activity.1658 middle aged men and women were randomly assigned to 30 minutes of brief negotiation or direct advice in primary care or a no-intervention control group. |
| **Interventions** | The interventions include direct advice group and brief negotiation. All intervention subjects were telephoned at set intervals following health check. The direct advice group received more advice about the importance of a physically active lifestyle |
| **Outcomes** | The primary outcome was self reported physical activity at 12 months. The secondary outcomes were blood pressure and body mass index |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | It was random recruitment |
| Allocation concealment (selection bias) | Unclear risk | Subjects were randomised by one of the authors to one of the three arms |
| Blinding of participants and personnel (performance bias) | Unclear risk | The study did not mention if the control groups were blinded to the intervention. |
| Blinding of outcome assessment (detection bias) | Unclear risk | The primary outcome was reported using self report. Bias is unclear |
| Incomplete outcome data (attrition bias) | Low risk | Missing blood pressure and weight and height measures at follow up were assigned the baseline values. |
| Selective reporting (reporting bias) | Low risk | All outcomes were reported |

## 10. Kerse et al. (2005)

| | |
|---|---|
| **Methods** | The study was a cluster randomised, controlled trial that was done over a 12months period |
| **Participants** | The study involved one hundred seventeen doctors in 42 primary care practices in the Waikato region of New Zealand. Two hundred seventy sedentary primary healthcare patients aged 65 and older participated. To be included, patients had to be participating in less than 30 minutes of at least moderate intensity physical activity on 5 or more days per week, be aged 40 to 80, and intend to stay in the region for at least 12 months. Patients were excluded if they were unable to comprehend the informed consent or were suffering from an unstable cardiovascular, debilitating, or progressive illness |

| | |
|---|---|
| **Interventions** | Patients in intervention practices prompted their primary care doctors or practice nurse to deliver brief activity counselling. A ''Green Prescription'' was written involving the negotiation of activity goals. Trained exercise specialists from a regional sports foundation gave follow-up telephone support over 3 months. |
| **Outcomes** | Leisure moderate and vigorous physical activity, total energy expenditure, systolic and diastolic blood pressure |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | All primary care doctors in the Waikato region were invited to participate hence randomization was guaranteed. |
| Allocation concealment (selection bias) | High risk | Patients and doctors could not be blinded to group allocation. |
| Blinding of participants and personnel (performance bias) | Unclear risk | Single blinded study as only participants were blinded to the intervention. The doctors in the control group were aware of the intervention however, received no prompt to deliver the Green Prescription |
| Blinding of outcome assessment (detection bias) | Low risk | Outcomes were ascertained by self-completion questionnaires at baseline and follow-up. |
| Incomplete outcome data (attrition bias) | Low risk | All outcomes were assessed using intention-to-treat analyses. Participants unable to follow up were assigned their baseline status on follow up, because this was considered the most conservative estimate |
| Selective reporting (reporting bias) | Unclear risk | All outcomes were reported including those with non-significant results e.g. blood pressure |

## 11. Kolt et al. (2007)

| | |
|---|---|
| **Methods** | The study was a randomised, controlled trial conducted over a 12-month period. |
| **Participants** | The study setting was three primary care practices from different socioeconomic regions of Auckland, New Zealand. One hundred and eighty-six low-active adults (aged 65) recruited from their primary care physicians' patient databases. |
| **Interventions** | The intervention was Eight telephone counselling sessions over 12 weeks based on increasing physical activity. Control patients received usual care. |
| **Outcomes** | Change in physical activity (as measured using the Auckland Heart Study Physical Activity Questionnaire) |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Those patients for whom physical activity was not contraindicated and were contactable at the address and telephone number on the practice database were invited to participate in the study via a letter from their primary care physician and follow-up telephone call from the practice where necessary. |

| | | |
|---|---|---|
| Allocation concealment (selection bias) | Low risk | After baseline assessment, one of the researcher's computer randomised (simple randomisation) participants to control or intervention groups. Another researcher (NG) generated the allocation sequence. |
| Blinding of participants and personnel (performance bias) | Unclear risk | Unclear if the participants were blinded of the intervention but the physicians were aware of the intervention. Hence it was single blinded |
| Blinding of outcome assessment (detection bias) | Low risk | The assessor was blinded to group allocation |
| Incomplete outcome data (attrition bias) | Unclear risk | Unclear how data from patients lost to follow up was handled. |
| Selective reporting (reporting bias) | Low risk | All outcome data was reported |

## 12. Lawton et al. (2008)

| | |
|---|---|
| **Methods** | The study was a randomised controlled trial. |
| **Participants** | The setting was in 17 primary care practices in Wellington, New Zealand. Eligibility criteria included women 40-74 who were physically inactive |
| **Interventions** | Brief physical activity intervention led by nurse with six-month follow-up visit and monthly telephone support over nine months. |
| **Outcomes** | Primary outcome measure was the proportion of those achieving the recommended 150 minutes of at least moderate intensity physical activity, as assessed by the long form of the physical activity questionnaire. Secondary outcomes included quality of life assessed with the short form 36 questionnaire (SF-36); weight, waist circumference, and blood pressure |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | A researcher not involved in the recruitment process carried out computer generated block randomisation |
| Allocation concealment (selection bias) | Low risk | After baseline measures the nurse opened sequentially numbered opaque envelopes containing the allocated treatment group (intervention or control). |
| Blinding of participants and personnel (performance bias) | High risk | There was no blinding of the general practitioner or the participants to the intervention. |
| Blinding of outcome assessment (detection bias) | Low risk | Nurses assessing participants at 12- and 24-month follow-up visits were blind to group allocation, and participants were asked not to discuss group allocation with the assessing nurse |
| Incomplete outcome data (attrition bias) | Low risk | They carried out an intention to treat analysis of all participants enrolled in the study according to allocation of randomisation |
| Selective reporting (reporting bias) | Unclear risk | All outcomes were reported |

## 13. Lewis & Lynch (1993)

| | |
|---|---|
| **Methods** | The study was a randomised control trial conducted from march through June 1991 in the US |
| **Participants** | The study was conducted in the ambulatory practice of the department of Family medicine, the University of Colorado Health Science Centre. Twenty-four family medicine residents were randomly assigned to the experimental or control groups. Subjects included outpatients age 18 and older who were scheduled to see a resident. A sample of 396 patients were admitted to the study. |
| **Interventions** | The intervention included 2 to 3 minutes of exercise advice, the distribution of an educational handout and the promise of a 1 month follow up phone call from a staff person. The exercise advice consisted of three steps of interaction with the patient, ASK about exercise, ASSESS the response and ADVISE accordingly. |
| **Outcomes** | physical activity minutes/session, PA Times/week, PA minutes/week |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Selection of participants was in a random sequence. |
| Allocation concealment (selection bias) | Low risk | family medicine residents were randomly assigned to the experimental or control groups |
| Blinding of participants and personnel (performance bias) | Low risk | The participants and family medicine residents in the control were blinded to the intervention |
| Blinding of outcome assessment (detection bias) | Unclear risk | The assessment was self reported. There risk of bias is unclear |
| Incomplete outcome data (attrition bias) | Unclear risk | Unclear how attrition was handled |
| Selective reporting (reporting bias) | Low risk | All outcome data was reported |

## 14. Little et al. (2004)

| | |
|---|---|
| **Methods** | The study design was randomised controlled (2 X 2 X 2) factorial trial. The study was approved by the Southampton and Salisbury Ethics Committees. Patients were randomly chosen from practice databases and were randomly assigned to one of eight groups. The study took 3 years from 1999 to 2002 |
| **Participants** | The study was conducted in Britain in four practice settings. Patients were included if they had one or more risk factors for coronary heart disease: a diagnosis by a GP of hypertension or hyperlipidaemia, a body mass index >25, or diabetes. Patients were excluded if they had coronary heart disease, if they were unable to perform moderate exercise (for example, if they had severe left ventricular failure), if they were unable to complete the questionnaire (for example, because of dementia). Participants who were both male and female and above the age of 18 years were included in the study. |
| **Interventions** | Patients could be assigned to no intervention, a single intervention, or any combination of interventions. The following interventions were used in the study. **Exercise prescription-**GPs briefly discussed the benefits of exercise, targets, how to start, and anticipating relapse, and wrote a prescription for 30 minutes, 5 times a week, of brisk walking (or equivalent). **Counselling session-** Nurses discussed the same issues as with exercise prescription. They also had a detailed motivational discussion (based on the theory of planned behaviour, which addresses attitudes and perceived behavioural |

| | |
|---|---|
| | control), identifying a precise time and place to start ('behavioural rehearsal'), and agreed and signed a contract. **Booklet-** The Health Education Authority booklet Getting active, feeling fit was used. |
| **Outcomes** | Assessments were carried out at baseline and after 1 month. The following data were collected: height; weight (using digital scales); blood pressure, using a semi-automated validated Omron oscillometer sphygmomanometer; cholesterol level, i.e. non-fasting total cholesterol, high density lipoprotein (HDL), and cholesterol/HDL ratio; the well-validated Godin questionnaire, which multiplies the number of episodes of exercise by relative energy expenditure in each; 'stage of change'; physical fitness/performance. The two measures of exercise performance used to assess fitness were: the Canadian Home Step Test, which took about 20 minutes to explain and perform; and the 6-minute walking test, in which participants are asked to walk or run as fast as they can around a circuit of known distance (200 meters on the pavement around the surgery) for 6 minutes, and the distance walked predicts health outcomes |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Patients were randomly chosen from practice databases if they had one or more risk factors for coronary heart disease |
| Allocation concealment (selection bias) | Low risk | Patients were randomly assigned to one of eight groups, which were defined by three intervention factors, in a balanced 2 X 2 X 2 factorial design, by opening a sealed opaque numbered envelope that had been prepared previously at the trial centre by the research nurse. |
| Blinding of participants and personnel (performance bias) | Unclear risk | The study has not clearly explained if the environment in which the participants was different, however it has tried to exclude participants with covariates that would possibly affect their fair participation. |
| Blinding of outcome assessment (detection bias) | Unclear risk | The assessors did not take part in the intervention, and made assessments without reference to the intervention group. Full blinding of assessors was not possible, given that this was an open trial, but patients were asked not to say what they had done |
| Incomplete outcome data (attrition bias) | Low risk | Missing values at follow-up were replaced with baseline measurements for intention-to-treat analyses, making the most conservative assumption that they would not have changed |
| Selective reporting (reporting bias) | Low risk | The study reported results despite being insignificant. For example there was relative increase in the cholesterol/HDL ratio in the prescription group compared to the control cholesterol/HDL ratio (0.34) in the control group, which may therefore be a chance finding, since the control group exercised less than the other groups |

## 15. Mutrie et al. (2012)

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|---|---|
| **Methods** | Two-arm (intervention/control) 12-week randomised controlled trial with a 12-week follow-up for the intervention group. |

| | |
|---|---|
| **Participants** | The setting was in one general practice in Glasgow, UK. Participants were aged ≥65 years. Inclusion criteria were living independently and not meeting current PA recommendations. |
| **Interventions** | The intervention group received two 30-minute physical activity consultations from a trained practice nurse, a pedometer and a walking programme. The control group continued as normal for 12 weeks and then received the intervention. Both groups were followed up at 12 and 24 weeks. |
| **Outcomes** | Step counts were measured by sealed pedometers and an activPAL TM monitor. Psychosocial variables were assessed and focus groups conducted. |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Random patients screen by GP and meet the inclusion criteria |
| Allocation concealment (selection bias) | Low risk | Randomization was performed using an ordered set of sealed envelopes containing group allocations. Allocations were made by an independent member of the research team and inserted into envelopes in a random order |
| Blinding of participants and personnel (performance bias) | Low risk | The nurse and a research assistant performing the randomisation were not blinded to group allocation but all other researchers were. |
| Blinding of outcome assessment (detection bias) | Low risk | The pedometer and activPAL were fitted by the practice nurse who had been appropriately trained by the research team. |
| Incomplete outcome data (attrition bias) | Unclear risk | Data processing errors resulted in loss of activPAL data for four participants (one intervention, three controls) at baseline and for one participant (intervention) at week 24. |
| Selective reporting (reporting bias) | Low risk | All outcome results were reported |

## 16. Norris et al. (2000)

| | |
|---|---|
| **Methods** | This study is a randomised controlled trial. Providers were randomised to the control or the intervention groups. All patients seeing a given provider were randomised into groups. The study took 6 months. |
| **Participants** | The study setting was Group Health Cooperative of Puget Sound, a staff model health maintenance organisation in Pacific Northwest. Study patients were recruited from consecutive patients, male and female of age 30 years or older, who called their primary care physician to schedule a well visit. Patients were excluded from the study if they had significant cognitive impairments, did not speak English, were pregnant or had cardiovascular, respiratory or metabolic disease |
| **Interventions** | Intervention physicians were trained to deliver exercise counselling protocol at the index visit, and one reminder telephone that occurred at 1 month. |
| **Outcomes** | The outcomes included Physical activity index (Kcal/week), Leisure score (0-130), Total physical activity (minutes/week), Total minutes walking per week |
| **Notes** | |

Risk of bias table

| Bias | Authors' judgement | Support for judgement |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Patients were recruited from consecutive age 30-year old's who called their primary care physician to schedule a well visit (a complete physical or female gynaecological investigation) |
| Allocation concealment (selection bias) | Low risk | Providers were randomised to the control or the intervention groups. The patient control and intervention groups were similar at baseline for demographic characteristics and physical activity levels. |
| Blinding of participants and personnel (performance bias) | Unclear risk | There was single blinding of the patients only, the physicians were not blinded on the intervention and were not monitored |
| Blinding of outcome assessment (detection bias) | Unclear risk | More part-time physicians were in the intervention group than the control group |
| Incomplete outcome data (attrition bias) | High risk | The authors analysed the data using the per protocol analysis approach and ignored data for patients who had been lost to follow up |
| Selective reporting (reporting bias) | High risk | The authors selectively reported data. When they realised that there were no significant differences in any of the measures of physical activity, they combined two interventions groups and reported the results verses a control comparison. |

## 17. Pears et al. (2016)

| | |
|---|---|
| **Methods** | The study was a randomised controlled trial in which interventions were delivered on a randomised weekly basis between May 2013 and February 2014 |
| **Participants** | Participants were recruited from eight NHS primary care practices in urban and rural areas in the East of England. Participants were eligible for the trial if they were eligible for the Health Check: i.e. aged 40–74 years and not previously diagnosed with heart disease, stroke, diabetes, or kidney disease. Participants were excluded if they had no working knowledge of English |
| **Interventions** | Participants were randomised to a Motivational (n = 83), Pedometer (n = 74), or Combined (n = 80) intervention, delivered immediately after a preventative health check in primary care, or control (Health Check only; n = 157). |
| **Outcomes** | physical activity, measured by accelerometer at 4 weeks. |
| **Notes** | |

Risk of bias table

| Bias | Authors' judgement | Support for judgement |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | The randomisation list was constructed by programming a simple block randomisation routine in R software |
| Allocation concealment (selection bias) | Low risk | Within each general practice, each calendar week of Health Checks was randomised to one of the four trial arms, such that all participants scheduled to receive a Health Check consultation in the same week were allocated to the same trial arm |

| | | |
|---|---|---|
| Blinding of participants and personnel (performance bias) | Low risk | Participants were blind to allocation at the time of booking their appointment, as were the practice staff who arranged the appointment |
| Blinding of outcome assessment (detection bias) | High risk | The general practitioners were aware of the intervention and hence possible bias |
| Incomplete outcome data (attrition bias) | High risk | Follow up data was analysed based on availability. hence the study did not apply an intention to treat analysis approach |
| Selective reporting (reporting bias) | Low risk | All outcome data was reported |

## 18. Petrella et al. (2003)

| | |
|---|---|
| **Methods** | randomised controlled trial; baseline assessment and intervention delivery with postintervention follow-up at 3, 6, and 12 months |
| **Participants** | Four large (5000 active patient files) academic, primary care practices: three in urban settings and one in a rural setting, each with four primary care physicians; two clinics provided the Step Test Exercise Prescription (STEP) intervention and two provided usual care control. A total of 284 healthy community-dwelling patients (72 per clinic) aged 65 years were recruited in 1998–1999 in Canada. Participants' mean age was 73 years |
| **Interventions** | Physicians in the intervention group were given published exercise counselling guidelines (exercise prescription instrument), a paper describing the benefits of exercise, guidelines for delivery and training in interpretation of the step test data to determine patient aerobic capacity (VO2max), including the prescription of an exercise training heart rate. Physicians in the control group were instructed to provide subjects with exercise counselling and prescription per their "usual care," with the addition of the ACSM guidelines and the benefits of exercise |
| **Outcomes** | The primary outcome measure was aerobic fitness (VO2max). Secondary outcomes included predicted VO2max from the STEP test, exercise self-efficacy (ESE), and clinical anthropometric parameters. |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Patients were identified in two ways over 6 months. First, over a 2-month recruitment period, clinic staff identified potentially eligible patients opportunistically from the regular daily register. Second, a clinic-produced list of patients meeting the eligibility criteria was utilized until 72 patients from each clinic were identified. All staff were blinded during recruitment. |
| Allocation concealment (selection bias) | Low risk | Subjects were then randomised to either STEP or control by a computer program, and scheduled to meet with a clinic family physician corresponding to their group assignment for exercise counselling |
| Blinding of participants and personnel (performance bias) | High risk | There was patient contamination because both STEP and control groups were given a list of available facilities for physical activity participation in their community. At all times, patients |

| | | |
|---|---|---|
| | | in both groups were free to choose where and how they would exercise |
| Blinding of outcome assessment (detection bias) | High risk | There was no blinding of outcome assessment because the same physicians who delivered initial counselling saw the patients at 3, 6, and 12 months, there was a possibility of positive assessment bias |
| Incomplete outcome data (attrition bias) | High risk | The study excluded results from participants lost to follow up, therefore did not employ intention to treat strategies |
| Selective reporting (reporting bias) | Low risk | All outcome results were reported even if they yielded non-significant results |

## 19. Petrella, Lattanzio, Shapiro & Overend (2010)

| | |
|---|---|
| **Methods** | study was a 12-month cluster randomised trial |
| **Participants** | The setting was forty family practices in 4 regions of Canada. Healthy, community-dwelling men (48%) and women (52%) with a mean (SD) age of 64.9 (7.1) years (range 55 to 85 years). There were a total of 193 participants in the intervention group and 167 in the control group. |
| **Interventions** | Intervention physicians were trained to deliver a tailored exercise prescription and a trans-theoretical behaviour change counselling program. Control physicians were trained to deliver the exercise prescription alone. |
| **Outcomes** | Predicted cardiorespiratory fitness, measured by predicted maximal oxygen consumption (pVO2max), and energy expenditure, measured by 7-day physical activity recall. |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Random selection of family physicians willing to participate |
| Allocation concealment (selection bias) | Low risk | Family physicians agreeing to participate were randomised to either the intervention or the control group. |
| Blinding of participants and personnel (performance bias) | Unclear risk | Single blinding because only patients were blinded to the intervention, however family physicians were fully aware of the intervention |
| Blinding of outcome assessment (detection bias) | Low risk | Assessment were collected by trained staff members; then step tests, supervised by the family physicians. |
| Incomplete outcome data (attrition bias) | Low risk | Dropouts were not replaced, as we performed an intent-to-treat analysis |
| Selective reporting (reporting bias) | Unclear risk | All outcomes were reported |

## 20. Pfeiffer et al. (2001)

| | |
|---|---|
| **Methods** | The study was a randomised control trial. Data were collected during a 28-week period from late February through August 1999. |
| **Participants** | During their regular office visits to the geriatrician, adults aged 60 years and older were informed of the study and invited to participate. It was conducted in a geriatric's ambulatory clinic, which is contiguous to a medical school in rural Appalachia, Ohio. |

| | |
|---|---|
| | Exercise baseline data were collected on 76 individuals; 49 (44 women and 5 men) of these individuals were enrolled in the study |
| **Interventions** | Enrolees were randomly assigned to either the green prescription group (n = 24) or the verbal advice only group (n =25) using a table of random numbers. The intervention involved exercise advice. This exercise advice was given verbally to all participants by the physicians. Then, the physician opened an envelope that indicated if that patient was in the group to receive further written exercise advice. Those patients placed in this group had their goals written on a green prescription form. |
| **Outcomes** | The outcomes were leisure physical activity in minutes/week and total physical activity in minutes /week |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Unclear risk | Unclear if there were participants were selected based on a random sequence |
| Allocation concealment (selection bias) | Low risk | Enrolees were randomly assigned to either the green prescription group or the verbal advice only group using a table of random numbers. |
| Blinding of participants and personnel (performance bias) | Low risk | It was single blinded as the physicians were aware of the intervention, however the patients were not aware |
| Blinding of outcome assessment (detection bias) | Unclear risk | The study was self reported. The possibility of bias in assessment of outcome is unclear |
| Incomplete outcome data (attrition bias) | Low risk | The analysis was also performed on an intention-to-treat basis assuming no change in exercise status for the two participants who withdrew from the study |
| Selective reporting (reporting bias) | Low risk | All outcomes were reported regardless of significant or non significant results. |

## 21. Reid & Morgan (1979)

| | |
|---|---|
| **Methods** | The study was a randomised control trial that was done within a six months period in Canada |
| **Participants** | One hundred and twenty-four firefighters, age 24 to 56, volunteered for this study. Through medical screening, authors evaluated the safety and suitability of participation for each of the volunteers. Fifteen per cent of those who came to the screening were ineligible for the study; those with contraindications for strenuous exercise and those already in physical activity programs were excluded from the study. |
| **Interventions** | The intervention included a printed exercise instruction and a ten-minute consultation with a physician, a one-hour period of film and discussion and knowledge of pulse taking, quantifying, recording of daily exercise, and reporting of this information. |
| **Outcomes** | Maximum oxygen uptake (VO2) was estimated on three occasions (start, three months, six months) using a bicycle ergometer, a stepwise increase of workload, and the Astrand nomogram |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|

| | | |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Participants randomly volunteered for this study. |
| Allocation concealment (selection bias) | Low risk | Participants were randomly allocated to a control and two treatment groups. |
| Blinding of participants and personnel (performance bias) | Low risk | Participants were randomly and received no further instructions on the intervention |
| Blinding of outcome assessment (detection bias) | Unclear risk | The participants self monitored, but the participants were blinded. Hence it was a single blinded study as physicians were not blinded to the intervention. |
| Incomplete outcome data (attrition bias) | Low risk | Missing cases were assigned a zero per cent change in their aerobic condition and are included in the analysis. |
| Selective reporting (reporting bias) | Low risk | All outcomes were reported |

## 22. Swinburn et al. (1998)

| | |
|---|---|
| **Methods** | The trial involved a randomised, controlled design conducted in a period of over a 13 weeks period |
| **Participants** | The study was carried out in two New Zealand urban centres (Auckland and Dunedin). Thirty-seven general practitioners underwent a training session on assessing and prescribing physical activity. They recruited patients who, in their judgment, were likely to benefit from an increase in physical activity and were able to increase their exercise over the following 6 weeks. Participants' mean age was 49 years (SD = 15). Participants (n = 491) were randomised to green prescription (n = 239) or verbal advice only (n = 252), |
| **Interventions** | Baseline data on exercise levels were collected by general practitioners using a standard questionnaire. For each participant, goals to increase physical activity (mainly centred around walking) were established. After the verbal advice had been given, the general practitioner opened an envelope that randomised the participant (within general practitioners) to having the goals written down or not. After 6 weeks, follow-up telephone interviews were conducted by trained interviewers using the same set of questions. |
| **Outcomes** | Physical activity duration in minutes/week |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | High risk | General practitioners recruited patients who, in their judgment, were likely to benefit from an increase in physical activity |
| Allocation concealment (selection bias) | Low risk | general practitioners randomised the participant into groups |
| Blinding of participants and personnel (performance bias) | Low risk | Participants were unaware of the intervention but general practitioners were not blinded |
| Blinding of outcome assessment (detection bias) | Unclear risk | Interviewers were unaware of the randomisation group of participants |
| Incomplete outcome data (attrition bias) | Unclear risk | Analysis was also performed on an intention-to-treat basis assuming no change in exercise status among those lost to follow-up. |

| | | |
|---|---|---|
| Selective reporting (reporting bias) | Unclear risk | All outcome data was available |

## 23. Sørensen et al. (2008)

| | |
|---|---|
| **Methods** | The study was conducted as a randomised trial in 2005–2006 |
| **Participants** | All the patients referred to the Exercise on Prescription scheme by their GP in the counties of Vejle and Ribe, Denmark, were eligible for the trial |
| **Interventions** | The high-intensive exercise on prescription group received 4 months of group-based supervised training and attended five motivational counselling sessions. The low-intensive group only attended four motivational counselling sessions. |
| **Outcomes** | Maximal oxygen uptake (VO2max), Body mass index (BMI), Physical activity (METh/day) |
| **Notes** | |

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | All patients referred to the Exercise on prescription scheme were eligible for the study and were offered participation in the randomised study during the baseline health profile by the physiotherapists |
| Allocation concealment (selection bias) | Low risk | Randomization was carried out by the first author by means of concealed envelopes containing the name of the group. |
| Blinding of participants and personnel (performance bias) | Unclear risk | The participants were blinded to the intervention, but it was impossible to blind the healthcare providers |
| Blinding of outcome assessment (detection bias) | High risk | The physiotherapists carried out the physiological measures and handed out questionnaires for the self-reported measures |
| Incomplete outcome data (attrition bias) | Low risk | missing data were replaced with the participant's mean value from the other questions |
| Selective reporting (reporting bias) | Unclear risk | All outcome results were reported |

## 24. Tylor & Fox (2005)

| | |
|---|---|
| **Methods** | The study was a randomised control trial. Randomization, on the basis of a random-numbers table, took place at the end of the first assessment, with a 7:3 greater likelihood of participants being referred into the exercise program than into the control group to offset anticipated non adherence to the exercise program |
| **Participants** | 142 patients were randomised women, ages 40–70 years, with one or more of three coronary heart disease (CHD) risk factors (i.e., being a smoker, hypertensive, or overweight) were identified from primary care medical records. There was no gender or age bias in U.K |
| **Interventions** | Patients were asked to visit the local leisure centre (exercise facility) and arrange an appointment for an introductory session to start a 10-week program with 2 sessions per week. At the end of the 10 weeks, a progress report was returned to the participant's GP, and participants were encouraged to maintain a physically active lifestyle |
| **Outcomes** | The outcome measures included total energy expenditure, moderate PA in minutes, Secondary outcomes included Body weight, BMI, fat, and heart rate |

| **Notes** | |
|---|---|

Risk of bias table

| **Bias** | **Authors' judgement** | **Support for judgement** |
|---|---|---|
| Random sequence generation (selection bias) | Low risk | Patients were randomly selected |
| Allocation concealment (selection bias) | Low risk | Randomization was done on the basis of random numbers into the intervention and control groups |
| Blinding of participants and personnel (performance bias) | Unclear risk | Participants were blinded to the intervention; however, the physicians were not blinded. |
| Blinding of outcome assessment (detection bias) | Unclear risk | Unclear who was assessing outcomes, hence unable to determine performance bias |
| Incomplete outcome data (attrition bias) | Low risk | For participants with missing data at the 16- and 37-week assessments, values were imputed from the previous assessment with complete data to perform an intent-to-treat analysis. |
| Selective reporting (reporting bias) | Unclear risk | Data on type, duration, and intensity of exercise in each session within the cardiovascular exercise room were self-reported on a card at each visit by participants within the scheme. Further scrutiny revealed that these data were often too subjective and incomplete to use in any data analysis. |

## b. Characteristics of excluded studies

### Activity (2001)

| **Reason for exclusion** | The study did not have a control group receiving usual care or placebo |
|---|---|

### Armit et al. (2009)

| **Reason for exclusion** | The study was excluded because it did not have a control group that received either a placebo or no intervention |
|---|---|

### Bennet et al. (2008)

| **Reason for exclusion** | The study was excluded because physical activity counselling intervention was administered by a counsellor and not a health provider |
|---|---|

### Bucholz & Purath (2007)

| **Reason for exclusion** | This descriptive exploratory study examined factors related to physical activity counselling practices |
|---|---|

**Bull & Jamrozik (1999)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded due to inadequate data for use in the meta analysis |

**Burn, Camaione & Chartterton (2000)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because it was a survey |

**Calfas et al. (1996)**

| | |
|---|---|
| **Reason for exclusion** | Intervention students attended 15 weekly 50-minute lectures led by 1 behavioral and 1 exercise science faculty member, hence was excluded because the intervention was not delivered by a healthcare provider. |

**Cunningham et al. (1987)**

| | |
|---|---|
| **Reason for exclusion** | The intervention was not primary care based |

**Dubbert et al. (2002)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because baseline data was missing on the variables of interest |

**Duru et al. (2010)**

| | |
|---|---|
| **Reason for exclusion** | The study used a faith-Based Physical Activity Intervention |

**Fortier et al. (2006)**

| | |
|---|---|
| **Reason for exclusion** | The study is missing results section. |

**Gao et al. (2016)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because the results were presented in odds ratios and standardized mean differences could not be computed in the meta analysis |

**Hardcastle (2012)**

| | |
|---|---|
| **Reason for exclusion** | Exclude because it was prospective study. It was also excluded because it lacked a control group |

**Harland et al. (1999)**

| | |
|---|---|
| **Reason for exclusion** | The outcomes were assessed based on frequency and percentages of participants who had improved, making the outcome dichotomous hence excluded. |

**Harrison, Roberts & Elton (2004)**

| | |
|---|---|
| **Reason for exclusion** | The outcome was reported as odds ratio and couldn't be included in the meta analysis |

**Jimmy & Martin (2005)**

| | |
|---|---|
| **Reason for exclusion** | The study is missing results necessary for conducting a meta analysis |

**Kastarinen et al. (2002)**

| | |
|---|---|
| **Reason for exclusion** | It was a randomised control trial which was assessing whether lifestyle counselling is effective in non-pharmacological treatment of hypertension in primary health care, hence could not answer the study objective |

**Kriska et al. (1986)**

| | |
|---|---|
| **Reason for exclusion** | The intervention was not primary care based |

**Long et al. (1996)**

| | |
|---|---|
| **Reason for exclusion** | The study did not have outcomes required in the intervention. |

**Marcus et al. (2007)**

| | |
|---|---|
| **Reason for exclusion** | The trial did not have a physician delivered intervention |

**Nymberg et al. (2018)**

| | |
|---|---|
| **Reason for exclusion** | The study was assessing the effect of mindfulness on adherence to physical activity on prescription |

**Pekmezi et al. (2016)**

| | |
|---|---|
| **Reason for exclusion** | The trial did not have a physician delivered intervention |

**Pinto et al. (1998)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because the outcomes were different from the outcomes required in the review |

**Rome, Ekdahl & Gard (2009)**

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because its primary outcome was direct and indirect costs of inactivity. |

**Schröder et al. (2018)**

| | |
|---|---|
| **Reason for exclusion** | The intervention was not health provider based. |

**Skår et al. (2011)**

| | |
|---|---|
| **Reason for exclusion** | This study examined the efficacy of two types of planning interventions (action plans and coping plans) in increasing physical activity levels when they are delivered via the Internet and not by a health care provider in primary care setting |

### Spittaels, Bourdeaudhuij & Vandelanotte (2007)

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because web-site-delivered physical activity intervention not involving a health provider or a primary care setting |

### Steele, Mummery & Dwyer (2009)

| | |
|---|---|
| **Reason for exclusion** | The study was excluded because the face to face or Internet delivered interventions were not administered from a primary care setting |

### Stralen et al. (2009)

| | |
|---|---|
| **Reason for exclusion** | Two tailored physical activity interventions, consisting of three tailored letters delivered during 4 months, were systematically developed by not administered from a primary care setting |

### Wanner et al. (2009)

| | |
|---|---|
| **Reason for exclusion** | The study employed an Internet approach in providing advice and not a health provider given intervention |

### William et al. (2006)

| | |
|---|---|
| **Reason for exclusion** | The interventions were not administered by a health provider or from a primary care setting |

## c. Characteristics of studies awaiting classification

## A. Characteristics of ongoing studies

### 1. Missud et al. (2019)

| | |
|---|---|
| **Study name** | Physical activity prescription for general practice patients with cardiovascular risk factors–the PEPPER randomised controlled trial protocol |
| **Methods** | The PEPPER clinical study is a randomised controlled trial to evaluate the efficacy over a period of 12months |
| **Participants** | 35 to 74-year old patients with cardiovascular risk factors. 140 patients will be recruited in 15 GP practices and randomised in the intervention group or in the control group where patients will receive verbal advice of physical exercise |
| **Interventions** | based on structured information delivery, a personalised written physical activity prescription |
| **Outcomes** | Primary outcomes include the total weekly energy expenditure, measured by an accelerometer in Metabolic Equivalent Task-minute (MET-min). Secondary outcomes will include Clinical and morphological measures |
| **Starting date** | Results were expected at the beginning of 2019. |
| **Contact information** | Correspondence: dmissud@gmail.com; laurent.connan@univ-angers.fr |
| **Notes** | |

**2. Rose et al. (2007)**

| | |
|---|---|
| **Study name** | The 'Women's Lifestyle Study', 2-year randomised controlled trial of physical activity counselling in primary health care: rationale and study design |
| **Methods** | The Women's Lifestyle Study is a two-year randomised controlled trial |
| **Participants** | Participants included women aged 40–74 years undertaking less than 150 minutes of at least moderate intensity physical activity per week recruited from primary care. |
| **Interventions** | This study used an enhanced green prescription (referred to in this trial as a 'Lifestyle script') that included telephone support from an RST exercise specialist over a nine-month period, and involved a face-to-face visit with the primary care nurse at six months to monitor progress and to provide additional support |
| **Outcomes** | Study measures were assessed at baseline, 12 and 24 months. The primary outcome measure was physical activity as assessed by the New Zealand physical activity questionnaire long form |
| **Starting date** | The results of the study were missing. Its unclear on the progress of dates |
| **Contact information** | Email: Sally B Rose* - sally.rose@otago.ac.nz;<br>Beverley A Lawton - bev.lawton@otago.ac.nz; C Raina Elley - c.elley@auckland.ac.nz; Anthony C Dowell - tony.dowell@otago.ac.nz; Anna J Fenton - anna.fenton@oxfordclinic.co.nz |
| **Notes** | |

## Appendices

### 1. COCHRANE CENTRAL Search strategy 2020

#1MeSH descriptor: [Physical Fitness] this term only
#2MeSH descriptor: [Physical Exertion] this term only
#3MeSH descriptor: [Physical Education and Training] explode all trees
#4MeSH descriptor: [Sports] explode all trees
#5MeSH descriptor: [Dancing] this term only
#6MeSH descriptor: [Exercise Therapy] explode all trees
#7physical* near activ*
#8physical* near train*
#9physical* near fit*
#10exercise* near train*
#11exercise* near activ*
#12exercise* near physical*
#13sport*
#14walk*
#15bicycle*
#16exercise* near aerobic*
#17((life next style*) near activ*)
#18life-style* near activ*
#19lifestyle* near activ*
#20((life next style*) near physical*)
#21life-style* near physical*
#22lifestyle* near physical*

#23#1 or #2 or #3 or #4 or #5 or #6 or #7 or #8 or #9 or #10 or #11
#24#12 or #13 or #14 or #15 or #16 or #17 or #18 or #19 or #20 or #21 or #22
#25#23 or #24
#26MeSH descriptor: [Health Education] this term only
#27MeSH descriptor: [Patient Education as Topic] this term only
#28MeSH descriptor: [Primary Prevention] this term only
#29MeSH descriptor: [Health Promotion] explode all trees
#30MeSH descriptor: [Behavior Therapy] this term only
#31MeSH descriptor: [Cognitive Therapy] this term only
#32MeSH descriptor: [Primary Health Care] this term only
#33MeSH descriptor: [Workplace] this term only
#34promot*
#35educat*
#36program*
#37#26 or #27 or #28 or #29 or #30 or #31 or #32 or #33 or #34 or #35 or #36
#38#25 and #37

**2. MEDLINE Ovid search strategy**
1. Physical Exertion/
2. Physical Fitness/
3. exp "Physical Education and Training"/
4. exp Sports/
5. Dancing/
6. exp Exercise Therapy/
7. exp Exercise/
8. (physical$ adj5 (fit$ or train$ or activ$ or endur$ or exertion$)).tw.
9. (exercis$ adj5 (train$ or physical$ or activ$)).tw.
10. sport$.tw.
11. walk$.tw.
12. bicycle$.tw.
13. ((exercise$ adj3 aerobic$) or aerobics).tw.
14. ((lifestyle or life-style) adj5 activ$).tw.
15. ((lifestyle or life-style) adj5 physical$).tw.
16. or/1-15
17. Health Education/
18. Patient Education as Topic/
19. Primary Prevention/
20. exp Health Promotion/
21. Behavior Therapy/
22. Cognitive Therapy/
23. Primary Health Care/
24. Workplace/

25. promot$.tw.
26. educat$.tw.
27. program$.tw.
28. or/17-27
29. 16 and 28
30. randomised controlled trial.pt.
31. controlled clinical trial.pt.
32. randomised.ab.
33. placebo.ab.
34. drug therapy.fs.
35. randomly.ab.
36. trial.ab.
37. groups.ab.
38. 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37
39. exp animals/ not humans.sh.
40. 38 not 39
41. 29 and 40
42. (200412$ or 2005$ or 2006$ or 2007$ or 2008$ or 2009$ or 2010$ or 2011$ or 2012$).ed.
43. 41 and 42

**3. EMBASE Ovid search strategy**

1. exp exercise/
2. fitness/
3. physical education/
4. exp sport/
5. dancing/
6. exp kinesiotherapy/
7. (physical* adj5 (fit* or train* or activ* or endur* or exert*)).tw.
8. (exercis* adj5 (train* or physical* or activ*)).tw.
9. sport*.tw.
10. walk*.tw.
11. ((exercise* adj aerobic*) or aerobic*).tw.
12. ((lifestyle or life-style) adj5 activ*).tw.
13. bicycle*.tw.
14. ((lifestyle or life-style) adj5 physical*).tw.
15. or/1-14
16. health education/
17. patient education/
18. primary prevention/
19. health promotion/
20. behavior therapy/
21. cognitive therapy/

22. primary health care/
23. workplace/
24. promot*.tw.
25. educat*.tw.
26. program*.tw.
27. or/16-26
28. 15 and 27
29. random$.tw.
30. factorial$.tw.
31. crossover$.tw.
32. cross over$.tw.
33. cross-over$.tw.
34. placebo$.tw.
35. (doubl$ adj blind$).tw.
36. (singl$ adj blind$).tw.
37. assign$.tw.
38. allocat$.tw.
39. volunteer$.tw.
40. crossover procedure/
41. double blind procedure/
42. randomised controlled trial/
43. single blind procedure/
44. 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36 or 37 or 38 or 39 or 40 or 41 or 42 or 43
45. (animal/ or nonhuman/) not human/
46. 44 not 45
47. 28 and 46
48. (200412* or 2005* or 2006* or 2007* or 2008* or 2009* or 2010* or 2011* or 2012*).dd.
49. 47 and 48
50. limit 49 to Embase

**4. Web of Science search strategy**

# 20 #19 AND #18
# 19 TS=(random* or blind* or allocat* or assign* or trial* or placebo* or crossover* or cross-over*)
# 18 #17 AND #8
# 17 #16 OR #15 OR #14 OR #13 OR #12 OR #11 OR #10 OR #9
# 16 TI= (promot* or educat* or program*)
# 15 TS=(workplace)
# 14 TS= (primary health care)
# 13 TS= (cognitive therap*)
# 12 TS= ((behaviour or behavior) NEAR/2 therap*)
# 11 TS= (health NEAR/2 promot*)

# 10 TS= (primary prevent*)
# 9 TS= ((health educat*) or (patient* educat*))
# 8 #7 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1
# 7 TS= ((lifestyle* or life-style*) NEAR/5 (activ* or physcial*))
# 6 TS= ((exercis* NEAR/2 aerobic*) or aerobic*)
# 5 TS= (sport* or danc* or walk* or bicycle*)
# 4 TS= (physical* educat*)
# 3 TS= (exercis* NEAR/5 (train* or physical* or activ*))
# 2 TS= (physical NEAR/5 (fit* or train* or activ* or endur* or exert*))
# 1 TS= (exercis* therap*)

**5. CINAHL Plus with Full Text EBSCO search strategy**

S34 S33 Limiters - Exclude MEDLINE records
S33 S31 and S32
S32 EM 20041201-20121010
S31 S20 and S30
S30 S21 or S22 or S23 or S24 or S25 or S26 or S27 or S28 or S29
S29 TX allocat*
S28 TX control*
S27 TX assign*
S26 TX placebo*
S25 (MH "Placebos")
S24 TX random*
S23 TX (clinic* N1 trial?)
S22 PT clinical trial
S21 (MH "Clinical Trials+")
S20 S10 and S19
S19 S11 or S12 or S13 or S14 or S15 or S16 or S17 or S18
S18 (TI promot* or educat* or program*) or (AB promot* or educat* or program*)
S17 (MH "Work Environment")
S16 (MH "Primary Health Care")
S15 (MH "Behavior Therapy+")
S14 (MH "Health Promotion")
S13 (MH "Preventive Health Care")
S12 (MH "Patient Education")
S11 (MH "Health Education")
S10 S1 or S2 or S3 or S4 or S5 or S6 or S7 or S8 or S9
S9 (TI sport* or walk* or bicycle* or exercis* or aerobic*) or (AB sport* or walk* or bicycle* or exercis* or aerobic*)
S8 (TI physical N5 (fit* or train* or activ* or endur* or exert*)) or (AB physcial* N5 (fit* or train* or activ* or endur* or exert*))

S7 (TI exercis* N5 (train* or physical* or activ*)) or (AB exercis* N5 (train* or physical* or activ*))
S6 (MH "Exercise+") or (MH "Therapeutic Exercise+")
S5 (TI (lifestyle* or life-style*) N5 (activ* or physical*)) or (AB (lifestyle* or life-style*) N5 (activ* or physical*))
S4 (MH "Sports+") or (MH "Dancing+")
S3 (MH "Physical Education and Training")
S2 (MH "Physical Fitness")
S1 (MH "Exertion")

**6. SPORTDISCUS search strategy**

1.'physical activity'
2.exercise
3.fitness
4.sedentary
5.housebound
6.aerobics or circuits or swimming or aqua or jogging or running or cycling or fitness or yoga or walking or sport
7.patient education
8.primary prevention
9.health promotion
10.behaviour therapy
11.cognitive therapy
12.primary health care
13.workplace
14.controlled
15.randomised
16.random-assignment
17.double-blind
18.single-blind
19.clinical
20.placebos
21.comparative
22.evaluation
23.study