American Journal of Epidemiology
Vol. 187, No. 5
DOI: 10.1093/aje/kwx337
Advance Access publication:
October 19, 2017

Systematic Reviews and Meta- and Pooled Analyses

Exercise for the Prevention of Low Back Pain: Systematic Review and Meta-Analysis of Controlled Trials

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Initially submitted July 14, 2017; accepted for publication October 5, 2017.

The aim of this systematic review and meta-analysis was to assess the effect of exercise in population-based interventions to prevent low back pain (LBP) and associated disability. Comprehensive literature searches were conducted in multiple databases, including PubMed, Embase, and the Cochrane Library, from their inception through June 2017. Thirteen randomized controlled trials (RCTs) and 3 nonrandomized controlled trials (NRCTs) qualified for the meta-analysis. Exercise alone reduced the risk of LBP by 33% (risk ratio = 0.67, 95% confidence interval: 0.53, 0.85; $I^2 = 23\%$, 8 RCTs, $n = 1{,}634$), and exercise combined with education reduced it by 27% (risk ratio = 0.73, 95% confidence interval: 0.59, 0.91; $I^2 = 6\%$, 6 trials, $n = 1{,}381$). The severity of LBP and disability from LBP were also lower in exercise groups than in control groups. Moreover, results were not changed by excluding the NRCTs or adjusting for publication bias. Few trials assessed health-care consultation or sick leave for LBP, and meta-analyses did not show statistically significant protective effects of exercise on those outcomes. Exercise reduces the risk of LBP and associated disability, and a combination of strengthening with either stretching or aerobic exercises performed 2–3 times per week can reasonably be recommended for prevention of LBP in the general population.

exercise; leisure activities; low back pain; primary prevention; referral and consultation; sick leave

Abbreviations: CI, confidence interval; ICC, intracluster correlation coefficient; LBP, low back pain; NRCT, nonrandomized controlled trial; RCT, randomized controlled trial; RR, risk ratio.

Worldwide, low back pain (LBP) is a common health problem (1). It is often recurrent, and in a small proportion of cases it becomes chronically persistent (2). Nearly one-third of cases can be attributed to occupational risk factors (2). However, the impact of ergonomic interventions in the workplace on the occurrence of LBP has generally been disappointing (2, 3). Of interventions at the individual level, only exercise for the spinal and abdominal muscles has been demonstrably effective in the prevention of LBP (2).

While previous systematic reviews of clinical trials (4–8) have indicated that exercise can prevent LBP, these reviews combined trials on primary prevention with other trials on secondary prevention (4–8). Furthermore, the numbers of trials on exercise that were included in those reviews were small, ranging from 4 to 8. Exercise may be effective for the secondary prevention of LBP, reducing not only its intensity (9, 10) but also its recurrence (11). However, it might also prevent initial development of LBP in people previously free of it.

In meta-analyses of prospective cohort studies, we found that leisure-time physical activity was associated with 10%–15% lower risk of chronic LBP (12) and lumbar radicular pain (13). Moreover, a meta-analysis of randomized controlled trials (RCTs) indicated that exercise reduces the risk of LBP in pregnancy by 10% (14). In a recent meta-analysis of 8 RCTs (11 reports), Steffens et al. (6) found that exercise prevented LBP by 35%–45% and sick leave due to LBP by 25%–75%, and they concluded that exercise combined with education was more likely to protect against the development of LBP than exercise alone. However, that investigation had several shortcomings (15). Interventions implemented in the general population and occupational populations were combined with interventions carried out in patient populations. Cluster-RCTs were analyzed without allowance for possible clustering effects. One trial was included twice in some meta-analyses, and some analyses employed fixed-effect rather than random-effects models. Furthermore, publication bias was neither explored nor considered in the interpretation of findings.

Our primary aim in the current meta-analysis of controlled trials was to determine the effect of exercise in population-based interventions designed to prevent LBP. In addition, as secondary objectives, we assessed the effects of exercise on intensity of LBP, disability due to LBP, health-care consultation for LBP, and sick leave due to LBP.

METHODS

Search strategy

We used the PRISMA statement (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) (16) when developing the review protocol. Literature searches were conducted in PubMed (National Library of Medicine, Bethesda, Maryland), Embase (Elsevier, Amsterdam, the Netherlands), and the Cochrane Library (The Cochrane Collaboration, London, United Kingdom) from their inception through June 2017 (see Web Table 1, available at https://academic.oup.com/aje). Additional searches were conducted in Google Scholar (Google LLC, Mountain View, California), ResearchGate (ResearchGate GmbH, Berlin, Germany), and ClinicalTrials.gov (National Library of Medicine). There was no restriction on language. Moreover, reference lists of articles included in the review and those of previous reviews on the topic were hand-searched.

Inclusion and exclusion criteria

The titles, abstracts, and (if necessary) full texts of relevant reports were screened by the first author (R.S.), who excluded those that clearly did not describe trials on the prevention of LBP by exercise. Those that remained were then assessed independently by 2 reviewers (R.S. and K.F.-H.) to identify population-based RCTs and nonrandomized controlled trials (NRCTs) that provided usable data on the effects of exercise in prevention of LBP, other than during pregnancy. Trials were eligible for inclusion in the review if they compared an exercise intervention with usual daily activities. Studies on spinal pain more generally (neck pain combined with back pain), clinically based studies in which participants all had LBP at baseline, and studies that did not report quantitative data with which to estimate a risk ratio were excluded. Any disagreements between the reviewers were resolved by discussion.

We approached the authors of several studies (17–27) to seek additional information, such as information on adjustment for clustering effects in cluster trials (21–27) and on the numbers of persons with LBP during the follow-up period in intervention and control groups (17–19). However, only 2 author groups (17, 20) provided us with additional data.

Assessment of study quality

Two reviewers (R.S. and K.F.-H.) independently graded the methodological quality of the RCTs using the Cochrane Collaboration's tool (28) and the methodological quality of the NRCTs using the Effective Public Health Practice Project tool (29). We assessed 5 sources of bias: selection bias, performance bias, detection bias, attrition bias, and reporting bias, and also the use of intention-to-treat analysis. Disagreements between raters were again resolved by discussion.

Meta-analysis

In our meta-analysis, we estimated relative risks for 4 dichotomized outcomes: 1) prevalent LBP during follow-up; 2) prevalent LBP with disability during follow-up; 3) health-care consultation for LBP during follow-up; and 4) a new episode of sick leave for LBP during follow-up. We also analyzed 2 continuous outcomes. We estimated mean differences between intervention and control groups in the intensity of LBP at the end of follow-up or the change in pain intensity during follow-up (see below) and standardized mean differences in disability due to LBP at the end of follow-up or change in disability score during follow-up (standardization was applied because individual studies had used different measures of disability).

Some clinical trials (23, 24, 30, 31) reported results only for continuous measures of pain or disability, in which case we estimated risk ratios for prevalent LBP or prevalent LBP with disability from mean number of days with LBP (30), mean pain intensity (31), or mean disability score (23, 24). We first calculated the standardized mean difference by dividing the difference between the mean scores for intervention and control groups by their pooled standard deviation. We then converted the standardized mean difference into an odds ratio using the logit method (32). Lastly, we converted the odds ratio to a risk ratio (33).

Where, at the start of a trial, the mean values for a continuous outcome measure were similar in intervention and control arms and did not differ between participants who were subsequently lost to follow-up and those who completed the trial, we took the difference between intervention and control groups at the end of follow-up as the measure of effect for our meta-analysis. However, where there were nontrivial differences between intervention and control groups at baseline (27), there were differences in the outcome measure at baseline between participants later lost to follow-up and those who completed the trial (31), or authors reported only changes from baseline to follow-up (17), we instead used as the measure of effect the difference between intervention and control groups in the change in the relevant measure from baseline to follow-up.

For 1 trial (20) in which the published analyses of days missed from school because of LBP and of health-care consultation for LBP were limited to participants who had experienced a new episode of LBP during the follow-up period, we reanalyzed the data to estimate differences between intervention and control groups for the full study sample. For this we used generalized estimating equations and defined correlation as "exchangeable," family as "binomial," link as "log," and vce as "robust." For another trial (25) that compared intervention and control groups with respect to changes in the prevalence of LBP and associated disability from 6 months preintervention to 6 months postintervention in a dynamic population of military conscripts, we re-estimated risk ratios from differences between the 2 groups during the 6-month postintervention period only.

There were 9 cluster clinical trials (20–27, 31), but only 1 study (20) had adjusted for possible clustering effects. We contacted the authors of the other 8 trials (21–27, 31) for adjusted results, but none were forthcoming. As an alternative, therefore, we estimated "design effects" on the variance of the outcomes from each trial, using the formula $1 + (M - 1)\text{ICC}$ (34), where M denotes the average cluster size and ICC the intracluster correlation coefficient. Previous studies had reported ICCs of 0.01 for LBP (35) and ICCs between 0.0 and 0.0053 for health-care consultation for LBP and sick leave due to LBP (36, 37). On this basis, we used ICCs of 0.02 for LBP and LBP with disability and ICCs of 0.01 for health-care consultation due to LBP and sick leave due to LBP. We multiplied the standard errors of risk ratios by the square root of the relevant design effect (34). For mean differences in pain intensity and disability due to LBP, we divided the sample size of each intervention group by the design effect (34).

Using Stata, version 13 (StataCorp LP, College Station, Texas), we performed random-effects meta-analyses to combine estimates from different studies (34). For standardized mean differences, we calculated Hedges' g (34), which weights each group's standard deviation by its sample size. Heterogeneity across the studies was assessed by means of I^2 statistics (38, 39). Funnel plots were used to explore publication bias, and Egger's regression test was used to examine funnel plot asymmetry (40). The potential for publication bias was deemed statistically significant if the P value was 0.10 or less (41). The trim-and-fill method was used to estimate the number of studies that were missing because of publication bias and to adjust the pooled estimates for publication bias (42). Other sensitivity analyses were performed with regard to study design, duration of follow-up period, and the methodological quality of included studies. Meta-regression (43) was used to test for differences in risk ratios between 2 or more subgroups.

RESULTS

Study search

The literature searches detected 8,800 relevant publications in PubMed, 10,697 in Embase, and 2,686 in the Cochrane Library (Web Table 1 and Web Figure 1). Preliminary screening reduced this yield to 78 trials. Thirty-four trials were on the prevention of LBP in general or occupational populations, conscripts, or schoolchildren, but we excluded 18 of them: 10 that were carried out in pregnant women, 1 on health promotion through physical activity, 1 on combined leg and back injuries, 1 on back pain/injury attributed to exercise, 3 in which the control group received an intervention to improve their knowledge of health and work conditions (i.e., health promotion), and 2 with insufficient data for estimation of a risk ratio. In 1 RCT (17), the control group received a single 1-hour lecture on general health. Since this minimal intervention has been shown to have no protective effect on LBP (6, 44), we retained the trial in our review. Thus, 16 trials ($n = 4{,}310$ participants) finally qualified for meta-analysis, including 13 RCTs (17, 18, 20, 23–25, 27, 30, 31, 45–48) and 3 NRCTs (21, 22, 26) (Web Tables 1 and 2).

Study characteristics

Of the 13 RCTs, 7 individually randomized the participants to intervention or control arms, 5 used cluster randomization, and 1 (27) employed individual randomization to compare education plus exercise with education alone and a cluster design to compare education plus exercise with a control group that received neither component of the intervention. All 3 NRCTs were cluster-controlled trials. Four studies were conducted in Japan, 3 in Denmark, 2 in Sweden, and 1 each in Canada, Finland, Italy, New Zealand, Thailand, the United Kingdom, and the United States. Sample sizes ranged from 30 to 901, and follow-up times ranged from 2 months to 24 months (Web Table 2). Five trials recruited participants who were free from LBP at baseline, and 11 recruited persons with or without LBP.

Type of intervention

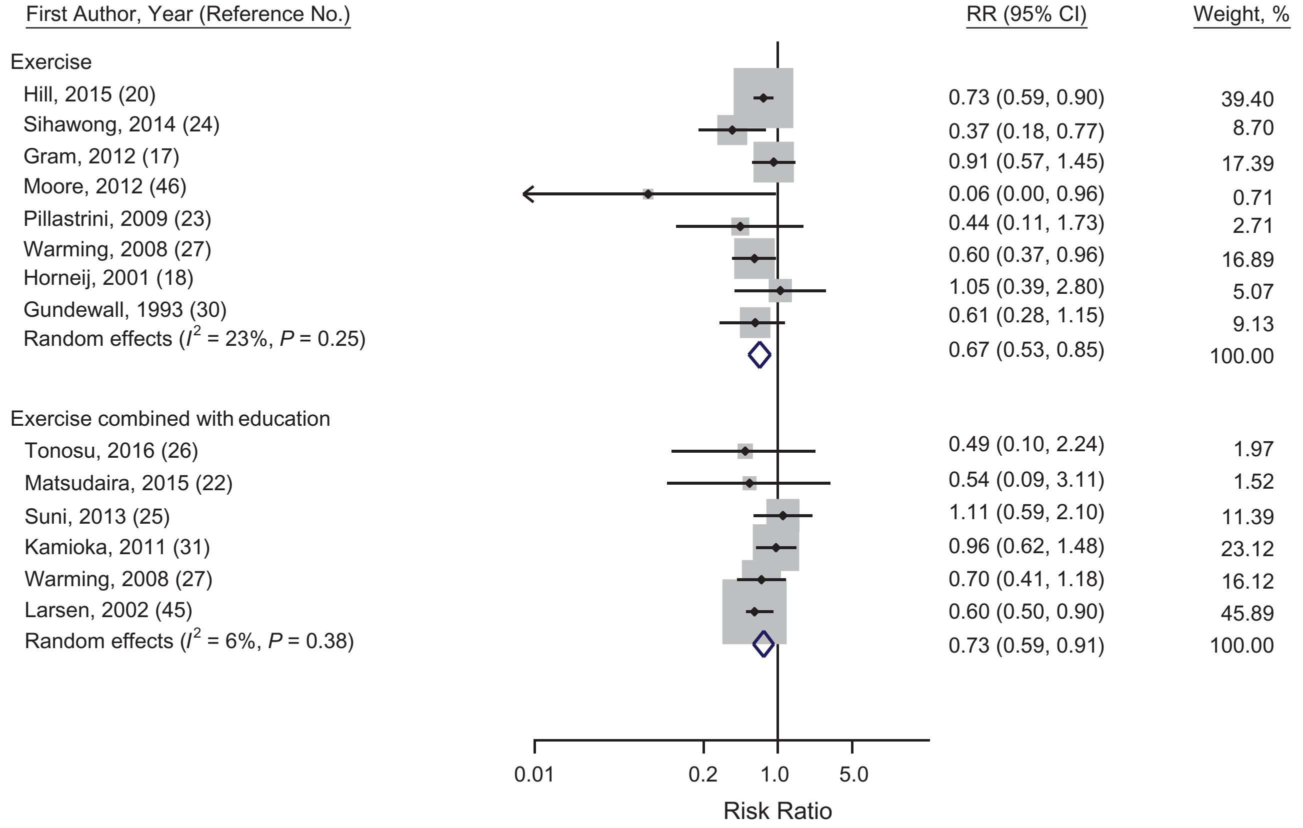
The interventions in the trials included stretching exercises for the spinal muscles (21, 22, 26, 31, 45), strengthening exercises (48), strengthening and stretching exercises (20, 23, 46), strengthening and aerobic fitness (17, 27), strengthening, endurance, and coordination exercises (30), stretching and endurance exercises (24), yoga (47), neuromuscular exercise to improve participants' control of movement in the lower back and enhance trunk muscular endurance and spinal stability (25), and a combination of posture and balance exercises, endurance and functional exercises, stretching exercises, and aerobic fitness (18). The durations of the interventions ranged from 3 weeks to 2 years. Some studies reported participants' levels of adherence to exercise, which ranged from 30% to more than 90%.

Study quality

Random sequence generation was adequate in 9 RCTs (Web Table 3). None of the trials blinded the participants or clinical personnel. Outcome assessors were blinded in only 1 trial, which used medical records to ascertain LBP. Losses to follow-up ranged from 0% to 50%. The sample attrition rate at follow-up was 20% or higher in 10 trials. All trials followed the intention-to-treat principle and retained participants who did not adhere to the intervention when analyzing outcomes.

Risk of LBP

Exercise alone. In a meta-analysis of 8 RCTs, exercise reduced the risk of LBP by 33% (risk ratio (RR) = 0.67, 95% confidence interval (CI): 0.53, 0.85; $I^2 = 23\%$, $n = 1{,}634$) (Figure 1). A funnel plot of the results of these 8 trials appeared to be asymmetrical (Web Figure 2), although Egger's test was nonsignificant ($P = 0.16$). The trim-and-fill method imputed 2 missing studies (Web Figure 3), and the risk ratio increased to 0.72 (95% CI: 0.54, 0.96) after adjustment for possible publication bias. Moreover, the effect of exercise did not differ (meta-regression $P = 0.20$) between the trials with a shorter follow-up time (≤9 months) and those with a longer follow-up time (≥12 months) (Web Figure 4). The risk ratio



First Author, Year (Reference No.)	RR (95% CI)	Weight, %
Exercise		
Hill, 2015 (20)	0.73 (0.59, 0.90)	39.40
Sihawong, 2014 (24)	0.37 (0.18, 0.77)	8.70
Gram, 2012 (17)	0.91 (0.57, 1.45)	17.39
Moore, 2012 (46)	0.06 (0.00, 0.96)	0.71
Pillastrini, 2009 (23)	0.44 (0.11, 1.73)	2.71
Warming, 2008 (27)	0.60 (0.37, 0.96)	16.89
Horneij, 2001 (18)	1.05 (0.39, 2.80)	5.07
Gundewall, 1993 (30)	0.61 (0.28, 1.15)	9.13
Random effects (I^2 = 23%, P = 0.25)	0.67 (0.53, 0.85)	100.00
Exercise combined with education		
Tonosu, 2016 (26)	0.49 (0.10, 2.24)	1.97
Matsudaira, 2015 (22)	0.54 (0.09, 3.11)	1.52
Suni, 2013 (25)	1.11 (0.59, 2.10)	11.39
Kamioka, 2011 (31)	0.96 (0.62, 1.48)	23.12
Warming, 2008 (27)	0.70 (0.41, 1.18)	16.12
Larsen, 2002 (45)	0.60 (0.50, 0.90)	45.89
Random effects (I^2 = 6%, P = 0.38)	0.73 (0.59, 0.91)	100.00

Figure 1. Results from a meta-analysis of 8 controlled trials on the effect of exercise alone on the risk of low back pain and 6 trials on the effect of exercise combined with education on the risk of low back pain. CI, confidence interval; RR, risk ratio.

was 0.61 (95% CI: 0.39, 0.96; $I^2 = 35\%$, $n = 726$) for 4 RCTs with a 0%–13% sample attrition rate and 0.69 (95% CI: 0.50, 0.96; $I^2 = 30\%$, $n = 908$) for 4 RCTs in which attrition was 20%–44%. Most of the trials included strengthening exercise as part of the intervention. The risk ratio was 0.50 (95% CI: 0.20, 1.26; $I^2 = 45\%$, $n = 809$) for 3 trials on strengthening and stretching exercises; 0.74 (95% CI: 0.49, 1.12; $I^2 = 33\%$, $n = 133$) for 2 trials on strengthening and aerobic exercises; and 0.71 (95% CI: 0.55, 0.91; $I^2 = 23\%$, $n = 942$) for 5 trials on strengthening and either stretching or aerobic exercises. Within the last group, the risk ratio was 0.31 (95% CI: 0.03, 3.16; $I^2 = 69\%$, 2 trials, $n = 738$) for daily exercise and 0.72 (95% CI: 0.52, 1.00; $I^2 = 1\%$, 3 trials, $n = 204$) for exercise 2–3 times per week.

Exercise combined with education. A meta-analysis of 6 trials (4 RCTs and 2 NRCTs) showed that exercise combined with education reduced the risk of LBP by 27% (RR = 0.73, 95% CI: 0.59, 0.91; $I^2 = 6\%$, $n = 1{,}381$). The protective effect of the intervention decreased after exclusion of the 2 NRCTs (RR = 0.77, 95% CI: 0.58, 1.02; $I^2 = 39\%$). There was no evidence of publication bias ($P = 0.78$; Web Figure 5). The trim-and-fill method imputed 1 missing study due to publication bias (Web Figure 6), but the estimate did not change after adjustment for publication bias (RR = 0.73, 95% CI: 0.59, 0.89). Furthermore, the effect of the intervention did not differ (meta-regression $P = 0.58$) between trials with shorter follow-up times (≤10 months) and those in which follow-up times were longer (Web Figure 4). Most of the trials had similar sample attrition rates. Four trials (22, 26, 31, 45) (Web Table 2) employed only stretching exercises, and the risk ratio for these 4 trials was 0.70 (95% CI: 0.53, 0.92; $I^2 = 10\%$).

Intensity of LBP

Exercise alone. Four trials measured pain intensity using a 10-cm visual analog scale. Among all participants (with or without LBP), pain intensity at follow-up was 0.5 cm lower in the exercise group than in the control group (mean difference = −0.52, 95% CI: −0.95, −0.09; $I^2 = 10\%$, 4 trials, $n = 452$) (Figure 2). There was no evidence of publication bias ($P = 0.75$; Web Figure 7). The trim-and-fill method did not impute any missing studies showing an absence of effect or harmful effect of exercise. However, it imputed 1 missing study showing a protective effect of exercise (Web Figure 8), such that the difference in mean pain intensity between the intervention group and the control group increased (RR = −0.54, 95% CI: −0.89, −0.20). The sample attrition rate was 0%–6% in 3 trials and 35% in 1 trial. Exclusion of the trial with the highest

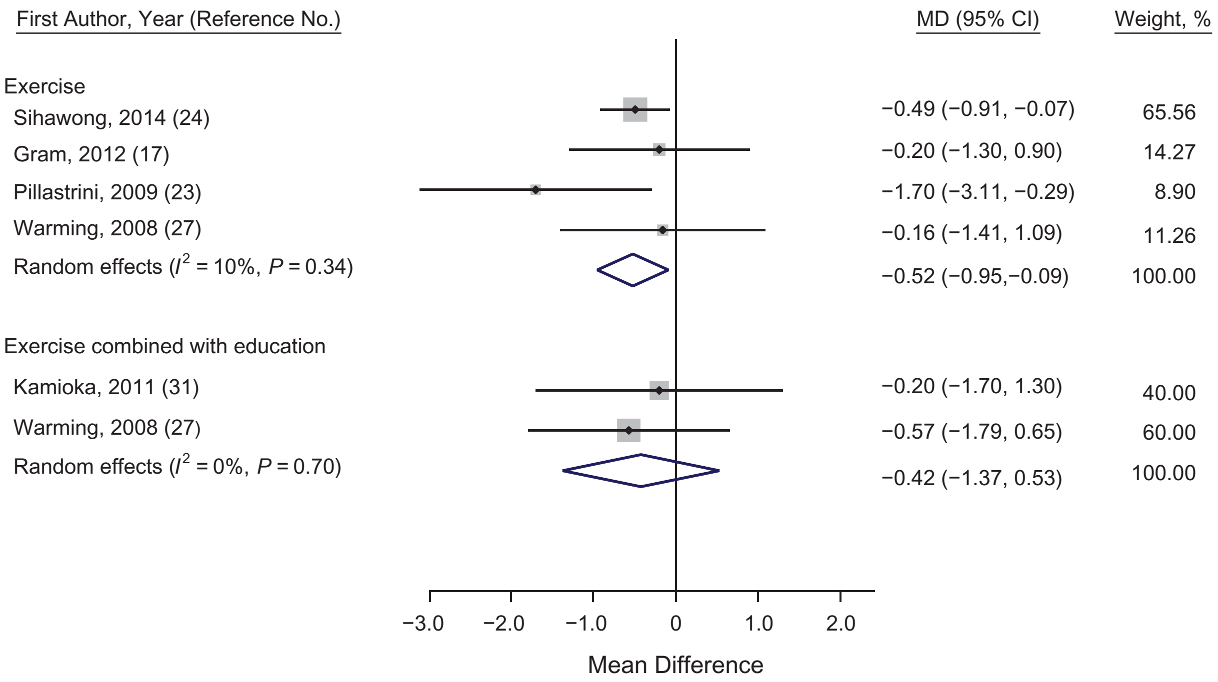


Figure 2. Results from a meta-analysis of 4 controlled trials on the effect of exercise alone on the intensity of low back pain and 2 trials on the effect of exercise combined with education on the intensity of low back pain. CI, confidence interval; MD, mean difference.

attrition rate did not change the result (RR = −0.61, 95% CI: −1.23, 0.00; $I^2 = 34\%$, 3 trials, $n = 384$).

Exercise combined with education. Only 2 cluster-RCTs provided estimates for pain intensity in intervention and control groups. Both trials measured pain intensity with a visual analog scale. Pain intensity was nonsignificantly lower in the intervention group than in the control group (mean difference = −0.42, 95% CI: −1.37, 0.53; $I^2 = 0\%$, $n = 133$) (Figure 2). The trim-and-fill method did not impute any missing studies showing no effect or harmful effects of the intervention, but it imputed 1 missing study showing a protective effect of the intervention. The summary mean difference was reduced to −0.57 (95% CI: −1.37, 0.23) after adjustment for funnel plot asymmetry.

Disability due to LBP

Disability as a dichotomized outcome. Exercise alone. The risk ratio from 5 trials that assessed disability due to LBP was 0.62 (95% CI: 0.42, 0.92; $I^2 = 63\%$, $n = 1{,}130$) (Figure 3). A funnel plot of these 5 trials was asymmetrical ($P = 0.01$; Web Figure 9). The trim-and-fill method imputed 2 missing studies attributable to publication bias (Web Figure 10), and the risk ratio increased to 0.78 (95% CI: 0.52, 1.18) after adjustment for publication bias. Summary risk ratios were 0.69 (95% CI: 0.50, 0.95; $I^2 = 0\%$, $n = 601$) for 2 trials with 0% and 6% attrition rates and 0.54 (95% CI: 0.24, 1.19; $I^2 = 79\%$, $n = 529$) for 3 trials with attrition rates of 20%–50%.

Exercise combined with education. Only 1 cluster-RCT (27) and 1 cluster-NRCT (26) examined the effect of exercise combined with education on disability from LBP. The risk ratio from these 2 trials was 0.62 (95% CI: 0.41, 0.96; $I^2 = 0\%$, $n = 253$) (Figure 3). The trim-and-fill method did not impute any missing study due to publication bias.

Disability as a continuous outcome. Exercise alone. Three trials reported mean values of disability due to LBP for exercise and control groups. Two trials (23, 24) used the Roland Morris Disability Questionnaire, and 1 (27) used a modified version of the Low Back Pain Rating Scale. The standardized mean difference for exercise was significant (Hedges' $g = -0.43$, 95% CI: −0.76, −0.09; $I^2 = 51\%$, $n = 387$). There was evidence of publication bias, however ($P = 0.07$). The trim-and-fill method imputed 2 missing studies due to publication bias, and the difference between exercise and control groups disappeared after adjustment for possible publication bias (standardized mean difference = −0.19, 95% CI: −0.53, 0.14).

Exercise combined with education. Only 1 cluster-RCT (27) reported a continuous estimate of effect for exercise combined with education. The intervention group had significantly lower disability scores than the control group at follow-up (mean difference = −2.56, 95% CI: −4.26, −0.86) after adjustment for the clustering effect. However, the difference was no longer significant after further adjustment for the difference in disability scores between the groups at baseline (mean difference = −2.47, 95% CI: −5.05, 0.11).

Health-care consultation for LBP

Exercise alone. A meta-analysis of 2 trials showed a nonsignificant protective effect of exercise (RR = 0.73, 95% CI: 0.24, 2.19; $I^2 = 53\%$, $n = 1{,}609$) (Figure 3) on health-care consultation for LBP. The effect was apparent in a cluster-NRCT (21) but not in a cluster-RCT (20). The trim-and-fill

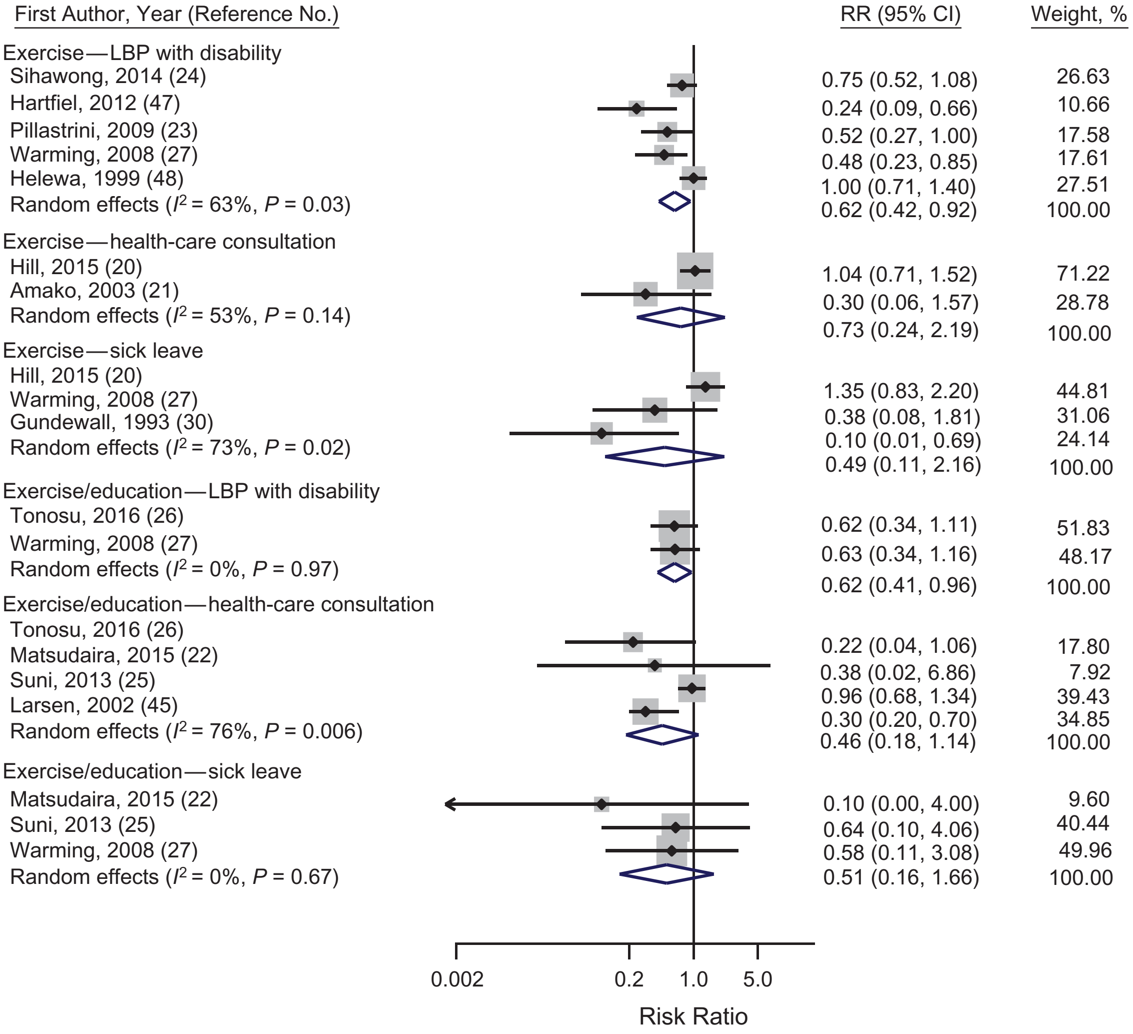


Figure 3. Pooled risk ratios (RRs) for the effects of exercise alone and of exercise combined with education on low back pain (LBP) with disability, health-care consultation for LBP, and sick leave for LBP. CI, confidence interval.

method imputed 1 missing study attributable to publication bias, and the risk ratio increased to 1.04 (95% CI: 0.38, 2.86) after adjustment for such bias.

Exercise combined with education. A meta-analysis of 4 trials showed a nonsignificant protective effect of exercise combined with education (RR = 0.46, 95% CI: 0.18, 1.14; $I^2 = 76\%$, $n = 1{,}207$) (Figure 3). Despite Egger's test being nonsignificant ($P = 0.35$), the funnel plot of the 4 trials was asymmetrical (Web Figure 11). Moreover, the trim-and-fill method imputed 2 missing studies due to publication bias, after adjustment for which the risk ratio increased to 0.89 (95% CI: 0.37, 2.16) (Web Figure 12).

Sick leave due to LBP

Exercise alone. The summary risk ratio from 3 trials that assessed sick leave was below unity, but not significantly (RR = 0.49, 95% CI: 0.11, 2.16; $I^2 = 73\%$, $n = 836$). The funnel plot was asymmetrical, and Egger's test was significant ($P = 0.098$; Web Figure 13). The trim-and-fill method imputed 2 missing studies due to publication bias (Web Figure 14), and the risk ratio increased to 1.35 (95% CI: 0.37, 4.92) after adjustment for publication bias.

Exercise combined with education. A meta-analysis of 3 trials that provided results on sick leave in relation to exercise combined with education showed no significant effect of the intervention (RR = 0.51, 95% CI: 0.16, 1.66; $I^2 = 0\%$, $n = 912$). The P value for Egger's test was 0.14, and the trim-and-fill method did not impute any missing study due to publication bias.

DISCUSSION

Our analysis indicated that in occupational and similar populations, exercises designed to strengthen the spinal muscles in combination with stretching or aerobic exercise can reduce the subsequent occurrence of LBP by approximately 30%, with decreases in the intensity of pain and associated disability as well. It suggested that the benefits extend to lower rates of health-care consultation and sick leave for LBP, although firm conclusions on this were precluded by limited statistical power and the possibility of publication bias. However, we found no indication that protective effects were larger when exercise programs were combined with education about back disorders, ergonomic principles, or exercise.

The intended focus of our review was the impact of exercise on LBP in the general and occupational populations, and therefore we excluded trials of secondary prevention in patients who already had the symptom. We did find a few studies (too few to be analyzed meaningfully as a subgroup) restricted to participants who initially were free from back pain, but for pragmatic reasons, most interventions targeted defined population groups (mainly occupational) without any attempt to screen out individuals who already had symptoms at the time of recruitment. Therefore, our conclusions do not relate strictly to the incidence of new LBP. They are, however, directly relevant to population-based preventive strategies. Care was taken to ensure that any imbalances between intervention and control groups in baseline levels of LBP and disability were taken into account in the analysis.

As well as lacking desirable statistical power for 2 of the secondary outcomes (health-care consultation and sick leave for LBP), our study had several other limitations. The trials on exercise plus education employed different educational programs. Moreover, as in many meta-analyses, there was a possibility of publication bias. We checked statistically for indications of such bias, and where effect estimates changed materially after adjustment for possible missing studies (e.g., for the impact of exercise on health-care consultation), results should be interpreted with caution.

Another problem was that many of the trials we reviewed had used a cluster design but had failed to allow for the clustering in their analysis. In the absence of further data, we attempted to adjust for possible overprecision in effect estimates, making assumptions about levels of intraclass correlation that were based on empirical findings from other studies conducted in occupational populations. Errors may have occurred in this extrapolation, but we think it unlikely that they would have had a major effect, and the confidence intervals presented should be more reliable than those that would have been derived without any allowance for clustering.

A further limitation was that adherence tended to be poorer in trials with longer periods of follow-up. Given that the trials were analyzed according to the intention-to-treat principle, this, if anything, would have tended to bias effect estimates towards the null, and it would not account for the significant reduction in LBP and disability that was found from exercise. Moreover, it means that the effects which were estimated are probably more representative of those which could be achieved in practice.

Despite these limitations, the observed benefit from exercise in reducing LBP and associated disability seems robust. The effect, which was based on results from 8 RCTs, was highly significant statistically, only minimally attenuated after adjustment for possible publication bias, and even stronger after exclusion of trials with higher attrition rates. Moreover, it was broadly consistent with that which was estimated in an earlier meta-analysis (6). Education appeared to have no additional beneficial effect on LBP, and the trials on exercise alone could therefore be pooled with those on exercise combined with education. Doing this produced a more precise estimate for the effect of exercise on LBP (RR = 0.71, 95% CI: 0.60, 0.83; $I^2 = 17\%$, 13 trials, $n = 2{,}929$).

In contrast to the previous review (6), we found no clear evidence of a reduction in sick leave attributed to LBP and no indication that the protective effects of exercise were enhanced when it was combined with education. These discrepancies may in part reflect differences in the inclusion criteria for trials—the earlier meta-analysis included some trials of secondary prevention in clinical populations as well as interventions in the general and occupational populations. In addition, as we noted in the Introduction, it had several methodological shortcomings.

In conclusion, this new meta-analysis suggests that a combination of strengthening and either stretching or aerobic exercises, performed 2–3 times per week, can reasonably be recommended for the prevention of LBP in the general population. Future research could usefully focus on the effectiveness of promoting spinal exercises in reducing demands for health care and work absence due to LBP.

ACKNOWLEDGMENTS

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The study was supported by the Finnish Ministry of Education and Culture (grant 253715).

We thank Dr. Julia J. Hill (20), Prof. Jennifer L. Keating (20), and Associate Prof. Bibi Valgerdur Gram (17) for providing us with additional results or data.

Conflict of interest: none declared.

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