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# Back schools for the treatment of chronic low back pain: possibility of benefit but no convincing evidence after 47 years of research—systematic review and meta-analysis

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**Abstract**

Back schools are interventions that comprise exercise and education components. We aimed to systematically review the randomized controlled trial evidence on back schools for the treatment of chronic low back pain. By searching MEDLINE, Embase, and Cochrane Central as well as bibliographies, we identified 31 studies for inclusion in our systematic review and 5 of these for inclusion in meta-analyses. Meta-analyses for pain scores and functional outcomes revealed statistical superiority of back schools vs no intervention for some comparisons but not others. No meta-analysis was feasible for the comparison of back schools vs other active treatments. Adverse events were poorly reported so that no reliable conclusions regarding the safety of back schools can be drawn, although some limited reassurance in this regard may be derived from the fact that few adverse events and no serious adverse events were reported in the back school groups in the studies that did report on safety. Overall, the evidence base for the use of back schools to treat chronic low back pain is weak; in nearly a half-century since back schools were first trialled, no unequivocal evidence of benefit has emerged.

**Keywords:** Back schools, Low back pain, Systematic review, Meta-analysis

## 1. Introduction

Low back pain is one of the most prevalent conditions encountered in clinical practice and is furthermore a common cause of work absenteeism and interference with work. Estimates of the point prevalence of low back pain range between 9% and 33%, 1-year prevalence estimates vary from 22% to 65%, and those for the lifetime prevalence range from 11% to 84%.[53,54] Although nonmalignant chronic painful disorders such as chronic low back pain tend to be a group of conditions with little directly associated mortality, recent work indicates that chronic pain may result not only in reduced quality of life but also in reduced life expectancy.[34] The burden of disease and cost associated with low back pain are therefore very high. The cost is comparable to other common disorders such as headache, heart disease, depression, or diabetes.[30] Analysing the Global Burden of Disease Study 2010, Murray et al.[39] reported that musculoskeletal disorders accounted for 6.8% of total disability-adjusted life years, with low back pain being responsible for about half of that amount; this was rather more than, for instance, diabetes mellitus (1.9% of disability-adjusted life years), major depressive disorder (2.5%), anxiety disorders (1.1%), drug use disorders (0.8%), or alcohol use disorders (0.7%). Low back pain is also a prominent cause of years lived with disability.[53]

Interventions for the treatment of chronic low back pain are therefore of considerable importance, to the individuals concerned and, because of the associated costs and productivity loss, to society at large. A number of treatment options exist from pharmacotherapy to physical therapy to various forms of exercise and instructions for back-friendly working practices. Back schools are educational and training programs with lessons given to patients or workers by a therapist with the aim of treating or preventing low back pain. They are a commonly used nonpharmacological intervention, especially within the occupational health setting. Despite the use of back schools since 1969,[57] their effectiveness in preventing or treating back pain has yet to be established unequivocally. A Cochrane review on this subject[22] summarised the evidence at the time and concluded that there was moderate evidence that back schools for patients with chronic low back pain in an occupational setting were more effective than other treatments and placebo or waiting list controls, but now more than a decade has passed since this last comprehensive evaluation and new studies as well as new guidance on conducting systematic reviews in the pain field[35] have been published in the meantime. More recent reviews that have included studies investigating back schools exist but are likewise now dated[5,6] or are not comprehensive, eg, by being restricted in geographical focus.[40]

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An up-to-date assessment of the efficacy and safety of back schools for low back pain is therefore now needed. This systematic review investigates the treatment of chronic low back pain by back schools. This review will not address the primary prevention of back pain or the treatment of acute or subacute back pain. Recent guidance about the choice of outcome measures to be investigated in systematic reviews in chronic pain[35] needs to be applied to studies investigating back schools to ensure that up-to-date standards are used and to ideally enable comparison with other current systematic reviews concerning treatments for chronic pain.

## 2. Methods

For inclusion in our systematic review, we accepted randomized controlled trials conducted in adults with chronic low back pain without any further restrictions as regards the condition as long as the trials investigated the intervention of back schools. Chronic pain is understood to be pain of at least 3 months' duration. Back school programs of different duration and content were accepted as long as they were educational and training programs with lessons given to patients by a therapist with the aim of treating low back pain. Any intervention, including no intervention, was accepted as a comparator.

We based our outcomes of interest on recent guidance[35] and extracted data accordingly. Pain outcomes of interest were the proportion of study participants with at least 50% pain reduction over study baseline, the proportion with at least 30% pain relief over baseline, the proportion below 30/100 mm on the Visual Analogue Pain Scale (VAS), and treatment group average scores of pain intensity or pain relief. As per the recent guidance,[35] the responder outcomes would have been our preferred outcomes of interest, but as such responder outcomes were not reported in the studies; we used treatment group average pain values for our meta-analyses.

Furukawa et al.[16] have reported on imputing response rates from mean and SD, and although this has utility for meta-analyses under certain circumstances, we did not use this approach in this review because none of our included studies reported pain responder rates, we did not know about the distribution of individual pain scores in the included studies, and the applicability of this imputation method for our specific circumstance of back schools for the treatment of back pain has not been proven.

Furthermore, we would have accepted both Numerical Rating Pain Scale and VAS scores for our meta-analyses, but the studies that were eligible for inclusion, compared back school treatments with no-treatment control groups, and reported mean values and SD all used the 0- to 10-cm VAS.

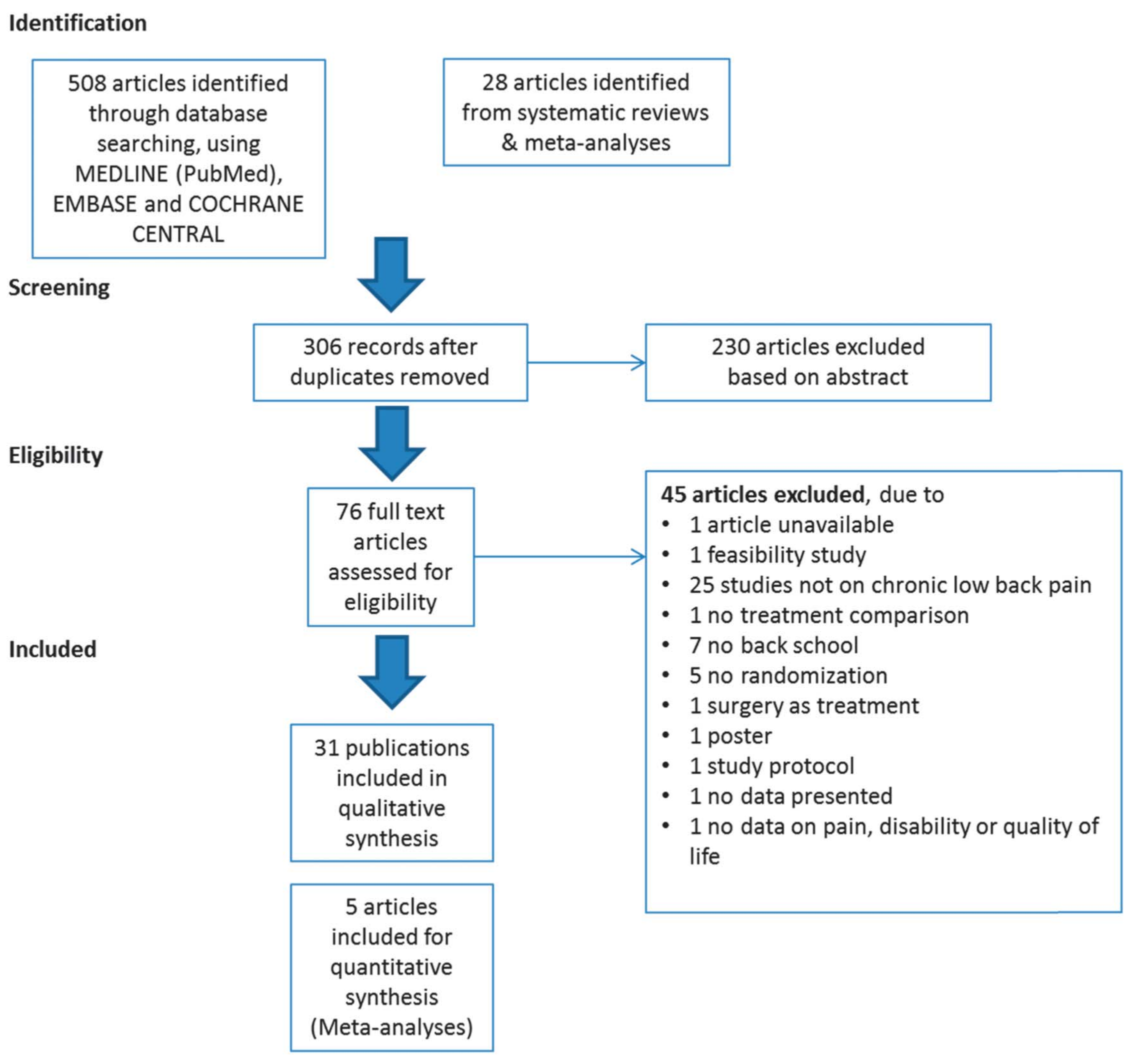


**Figure 1.** Study selection.

There is no consensus on a set of core work-related outcomes that should be included in studies and systematic reviews of interventions for painful conditions in working populations. Deriving such a set of core work-related outcomes would be worthwhile, but this is outside the scope of this article. For this review, we chose as our work and function-related outcomes of interest the number of workdays missed, pain interference with work, and pain interference with activities of daily living. We also sought data on quality of life measures and patient global impression. Furthermore, we sought data on safety outcomes: the proportion of trial participants with any adverse event, withdrawals due to adverse events, and the proportion with any serious adverse event.

Systematic database searches were conducted in MEDLINE (PubMed), Embase, and Cochrane Central. The search strategy for our MEDLINE search is detailed in Appendix 1. The other databases were searched in an analogous manner. The databases were searched from their inception with no time restriction. The date of the last search was October 9, 2015. Two reviewers screened all abstracts independently for inclusion or exclusion. In case of disagreement, the full article was examined. Electronic searches were supplemented by screening of the bibliographies of the articles identified for inclusion and of review articles in the field.

Data extraction was performed by one investigator and independently verified by another, disagreements were resolved by consensus. Data were extracted on the outcomes of interest and study characteristics including the number of patients, definition of chronic low back pain, duration of treatment, duration of follow-up, and types of interventions used in back school and comparator groups.

The risk of bias for each study was assessed with the Cochrane Risk of Bias tool, also in duplicate.[23] Study size was considered as high risk of bias if there were <50 study participants per treatment group, unclear risk of bias for 50 to 199 participants per group, and low risk of bias if there were >200 participants per group.

Given the expected clinical, methodological, and statistical heterogeneity between the studies, meta-analyses were performed with a random-effects model using RevMan software (Review Manager [RevMan] [Computer program], Version 5.3, Copenhagen, The Nordic Cochrane Centre, The Cochrane Collaboration, 2014). Most of the studies reported the measured outcomes for each visit and treatment group separately. We therefore normalized the measured values to their respective baseline measurements and compared treatment groups afterward using the mean difference and related SE, if calculable. The calculations were performed according to the recommendations in the *Cochrane Handbook for Systematic Reviews of Interventions*.[23] Studies reporting median and interquartile range were excluded from further analysis as no method of aggregating mean and median values seemed suitable. The Short Form (36) Health Survey (SF-36) items can be aggregated to eight scales that can be further aggregated to a physical (PCS) and a mental component score (MCS), from 4 scales each. If those 2 component scores were not reported in an article, we calculated them (as means and corresponding SE) from the scales.

PCS and MCS were calculated as means of the scales as described below:

$$PCS = \frac{PF + RP + BP + GH}{4} \qquad MCS = \frac{VT + SF + RE + MH}{4},$$

where, PF = physical functioning, RP = role-physical, BP = bodily pain, GH = general health, VT = vitality, SF = social functioning, RE = role-emotional, MH = mental health.

We also performed a change-point analysis based on the approach of Worsley[56] to assess whether an improvement in terms of risk of bias could be observed over time. To do this, we dichotomized the extracted information on risk of bias (low risk of bias = yes/no) and estimated a change point for each of the 6 domains based on likelihood ratio statistics. The proportion of studies having a low risk of bias until and after the change point is reported.

## 3. Results

Our literature search identified 306 potentially relevant articles of which 31[1,3,7,9–11,14,15,17–20,24,26–29,31–33,36–38,41–44,49–52] were included in our systematic review and of which 5 were included in our meta-analyses (**Fig. 1**). The 31 included studies differed with regard to the treatments being compared: 11 studies used usual care control groups, 9 studies used an active control other than back schools for comparison, 7 studies compared back schools with multimodal treatments, and 4 studies compared different types of back schools with one another (**Fig. 2**).

Regarding comparisons of one type of back school intervention vs a different type of back school intervention and comparisons involving back school as a part of a multimodal treatment approach, no independent treatment effect of back schools could be isolated from the data.

The active treatments in studies comparing back school vs other active treatments were so heterogeneous that we decided not to perform a meta-analysis for these studies. Therefore, only 11 studies comparing back schools vs usual care where considered for meta-analyses.

Because we did not include studies reporting median and interquartile ranges and excluded all studies that did not report data that could be used to calculate SE of mean differences, only 5 studies were finally included in our meta-analyses.

Comparing studies published at the time of the Cochrane review by Heymans et al.[22] in 2004 and studies published more recently, we noted that most of the studies with active control groups have been published recently. **Table 1** further details the baseline characteristics of these 31 included studies along with the reporting of our outcomes of interest. The studies were heterogeneous with regard to a number of parameters including treatment duration (varied from 1 to 10 weeks), duration of follow-up (from 4 to 130 weeks), and drop-out rates (between 0 and over 30%). There was

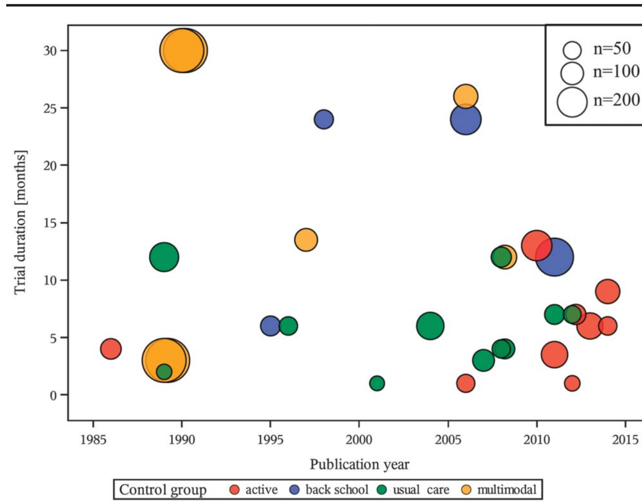


**Figure 2.** Visualization of all 31 included studies reporting on the efficacy of back schools, describing trial duration and total sample size (inset scale) over time. The comparator treatments to the back school treatments are colour-coded as shown in the figure.

**Table 1**

**Baseline characteristics of the 31 included studies.**

| Publication | N randomized | Dropout-rate (%) | Mean age (y) | Female (%) | Treatment A | Treatment B | Treatment C | Definition of chronic low back pain | Treatment duration | Follow-up | Outcomes of interest |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Klaber Moffett et al.[29] | 92 | 15 | 40.6 | 50.0 | Back school | Exercises | | >6 mo | 1 wk | 16 wk | NRS, ODI |
| Härkäpää et al.[19] | 476 | 4 | 44.9 | 37.0 | Inpatient treatment including back school | Outpatient treatment including back school | No treatment | Chronic or recurrent low back pain for at least 2 yr | 3 wk | 12 wk | NRS |
| Hurri et al.[26] | 204 | 8 | 45.8 | 100.0 | Back school | | No treatment | >12 mo | 4 wk | 52 wk | VAS, ODI |
| Keijsers et al.[28] | 40 | 25 | 49.7 | 60.0 | Back school | | Waiting list | >6 mo | 8 wk | 8 wk | VAS |
| Mellin et al.[32] | 476 | 5 | 44.9 | 36.5 | Inpatient treatment including back school | Outpatient treatment including back school | No treatment | Chronic or recurrent low back pain for at least 2 yr | 3 wk | 12 wk | |
| Härkäpää et al.[20] | 476 | 16 | 44.9 | 37.0 | Inpatient treatment including back school | Outpatient treatment including back school | No treatment | Chronic or recurrent low back pain for at least 2 yr | 3 wk | 130 wk | NRS |
| Mellin et al.[31] | 476 | 4 | 44.9 | 36.8 | Inpatient treatment including back school | Outpatient treatment including back school | No treatment | Chronic or recurrent low back pain for at least 2 yr | 5 wk | 130 wk | |
| Frost et al.[14] | 81 | 12 | 36.3 | 52.1 | Exercises + back school | | Back school | >6 mo somatic low back pain | 4 wk | 26 wk | ODI |
| Mucha et al.[38] | 56 | 0 | 34.5 | 100.0 | Back school | | No treatment | >3 mo | 10 wk | 26 wk | |
| Bendix et al.[3] | 123 | 16 | 42* | 72.8 | Multidisciplinary program including back school | Outpatient program including back school | Minimal treatment, no back school | >6 mo | 6 wk | 58 wk | NRS |
| Frost et al.[15] | 81 | 23 | 37.8 | 54.8 | Exercises + back school | | Back school | >6 mo of mechanical low back pain | 4 wk | 104 wk | ODI |
| Hodselmans et al.[24] | 28 | 14 | 35.5 | 50.0 | Back school | | Waiting list | >3 mo | 12 wk | 4 wk | RMDQ, SF-36 |
| Vollenbroek-Hutten et al.[52] | 163 | 13 | 39.0 | | Back school | | Usual care | >3 mo | 4 wk | 26 wk | RMDQ |
| Donzelli et al.[10] | 53 | 19 | 50.1 | 66.0 | Back school | Pilates | | >3 mo | 4 wk | 4 wk | VAS, ODI |
| Goldby et al.[18] | 302 | 29 | 42.0 | 68.5 | Spinal stabilization + back school | Manual therapy + back school | Education + back school | >3 mo | 10 wk | 104 wk | NRS, ODI |
| Kääpä et al.[27] | 132 | 2 | 46.3 | 100.0 | Exercises + back school + stress management | Physiotherapy | | Daily or nearly daily low back pain during last year | 8 wk | 112 wk | NRS, ODI |
| Tavafian et al.[49] | 102 | 11 | 43.8 | 100.0 | Back school | | Usual care | >3 mo | 1 wk | 12 wk | SF-36 |
| Andrade et al.[1] | 70 | 19 | 45.0 | - | Back school | | Waiting list | >3 mo | 4 wk | 16 wk | VAS, RMDQ |
| Ribeiro et al.[43] | 60 | 8 | 50.6 | 81.8 | Back school | | Usual care | >3 mo | 4 wk | 16 wk | VAS, RMDQ, SF-36 |
| Tavafian et al.[50] | 102 | 27 | 43.8 | 100.0 | Back school | | Usual care | >3 mo | 2 wk | 52 wk | SF-36 |

(continued on next page)

Table 1 (*continued*)

| Publication | N randomized | Dropout-rate (%) | Mean age (y) | Female (%) | Treatment A | Treatment B | Treatment C | Definition of chronic low back pain | Treatment duration | Follow-up | Outcomes of interest |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Van der Roer et al.[51] | 114 | 0 | 41.7 | 51.8 | Exercises + back school | Physiotherapy | | >3 mo | 32 sessions (A); 13 sessions (B) | 52 wk | NRS, RMDQ |
| Cecchi et al.[7] | 210 | 2 | 58.8 | 66.7 | Back school | Physiotherapy | Spinal manipulation | Back pain often to always in the last 6 mo | 4 wk | 56 wk | RMDQ, SF-36 |
| Meng et al.[33] | 382 | 6 | 49.9 | 64.2 | New back school | Usual care (“old back school”) | | Primary diagnosis of chronic low back pain *ICD-10-GM*: M51, M53, M54 | 4 wk | 52 wk | |
| Morone et al.[37] | 73 | 4 | 60.1 | 64.3 | Back school | | Usual care | >3 mo | 4 wk | 30 wk | VAS, ODI, WI, SF-36 |
| Sahin et al.[44] | 150 | 3 | 49.3 | 76.7 | Back school + exercises | Exercises | | >3 mo | 2 wk | 14 wk | VAS, ODI |
| Morone et al.[36] | 75 | 0 | 55.3 | 72.0 | Back school | Exercises using a “surface for perceptive rehabilitation”-tool | Usual care | >3 mo | 4 wk | 30 wk | VAS, ODI, WI |
| Paolucci et al.[41] | 30 | 0 | 58.6 | - | Back school | Exercises using a “surface for perceptive rehabilitation”-tool | | >3 mo | 4 wk | 4 wk | |
| Paolucci et al.[42] | 73 | 32 | 58.3 | 62.0 | Back school | | Usual care | >3 mo | 4 wk | 30 wk | VAS, ODI, WI, SF-36 |
| Garcia et al.[17] | 148 | 0 | 54.0 | 73.6 | Back school | McKenzie method (a classification-based treatment for low back pain) | | >3 mo | 4 wk | 26 wk | NRS, RMDQ |
| Costantino et al.[9] | 56 | 4 | 73.5 | 44.4 | Back school | Hydrotherapy | | >3 mo | 12 wk | 26 wk | RMDQ, SF-36 |
| Durmus et al.[11] | 127 | 5 | 53.1 | 100.0 | Back school + exercises | Exercises | | >3 mo | 12 wk | 38 wk | VAS, ODI, SF-36 |

* Median age.

NRS, Numerical Rating Pain Score; ODI, Oswestry Disability Index; RMDQ, Roland Morris Disability Questionnaire; SF-36, Short Form (36) Health Survey; VAS, Visual Analogue Pain Score; WI, Waddell Functional Index.

also some heterogeneity in the descriptions of the back school interventions themselves between the studies (Appendix 2).

An assessment of the risk of bias in the included studies using the Cochrane Risk of Bias tool shows relatively low risk of bias in the domains of random sequence generation and allocation concealment but relatively high risk of bias with regard to blinding, incomplete outcome data, and study size (**Fig. 3**). The risks of bias in the individual studies are detailed further in Appendix 3.

There were 10 studies[1,15,17,18,27,33,43,44,51,52] with a low risk of bias in terms of random sequence generation and allocation concealment, and this comparatively high-quality evidence merits a closer look. Two of these studies[17,44] compared a back school intervention with another active intervention. Garcia et al.[17] compared a back school against a McKenzie intervention. The McKenzie group showed an improvement in disability compared with the back school group. No difference in pain intensity could be observed. Sahin et al.[44] compared a combination of back school and exercises against exercises only. They observed a significant improvement in the back school group regarding the Oswestry Disability Index compared against exercises only. The results regarding pain are difficult to interpret because the comparisons presented in the article after the treatment and after 3 months favoured the back school group, although a larger decline from baseline could be observed in the exercise-only group. The authors conclude that back school plus exercises were more effective than exercises only.

Four studies[14,15,18,33] compared treatment regimens in which a back school intervention was part of all treatment arms. We cannot draw any conclusions regarding the efficacy or safety of back school treatments per se from these studies.

Three studies[1,43,52] compared back schools vs usual care or waiting list. Andrade et al.[1] compared a back school vs waiting list. They observed a significant reduction in the VAS score and in the Roland Morris Disability Questionnaire score in the back school group. Ribeiro et al.[43] compared back schools vs usual care. They observed a significant reduction in acetaminophen and anti-inflammatory intake in the back school group, but no difference regarding VAS, Roland Morris Disability Questionnaire, Beck Depression Inventory, or SF-36 (except for the SF-36 section "General Health" which favoured the back school treatment). Vollenbroek-Hutten et al.[52] compared back school vs usual care and did not observe a significant difference between groups.

Two studies[27,51] reported on the efficacy of multimodal treatments that included a back school intervention in one treatment arm. Because the impact of the back school per se on the overall treatment effect cannot be isolated from these data, we cannot draw any conclusions on the efficacy or safety of back school treatments from these studies.

As no study reported our preferred responder outcomes for pain intensity, we analysed treatment group average pain scores. **Figure 4** illustrates pain scores (0- to 10-cm VAS) after 1 to 2 and 4 to 6 months from baseline comparing back school treatments with control groups (no treatment). After 1 to 2 months, we found a statistically significant benefit of back schools over control. After 4 to 6 months, there was numerical improvement but no statistically significant difference. The differences in 0- to 10-cm VAS scores at these time points were 1.08 and 1.35 cm, respectively.

Only 4 studies reported on absence from work,[7,19,20,27] but not in a way that the data could be combined in a meaningful manner. However, some studies reported outcomes on functional or disability scales, the Roland Morris Disability Questionnaire, Oswestry Disability Index, Waddell Functional Index, and SF-36. The scores from the Roland Morris Disability Questionnaire are shown in a meta-analysis (**Fig. 5**). Again, there was a trend for back schools to be superior to no treatment, and this trend reached statistical significance for the scores after 1 to 2 months, missing significance for the scores after 4 to 6 months. Mean differences in the Roland Morris Disability Questionnaire score at these time points were 1.63 and 0.95, respectively.

The physical and mental component scores of SF-36 are illustrated in **Figures 6 and 7**. There was a trend for back schools to be numerically superior over no treatment for either component score at either time point investigated. For the physical component score, the differences after 1 to 2 and 4 to 6 months were 1.98 and 11.54, respectively. For the mental component score, the differences at these time points were 3.94 and 8.55. However, these differences in SF-36 component scores never reached statistical significance.

Adverse events were not commonly reported in the included studies. Where they were reported, we noticed an infrequency of adverse events and an absence of serious adverse events in the back school groups.

The change-point analyses revealed that significant improvements in terms of allocation concealment and incomplete data handling could be observed over time (**Table 2**). Until 1997, no study reported on allocation concealment procedures, whereas 57% of the subsequent studies described suitable allocation concealment methods resulting in a low risk of bias. Reporting and integration of incomplete data improved significantly after 2008 (from 33% to 90%, **Table 2**).

## 4. Discussion

We identified and synthesized information from 31 studies that reported on the efficacy and safety of back schools for the treatment of chronic low back pain. However, no firm conclusions can be

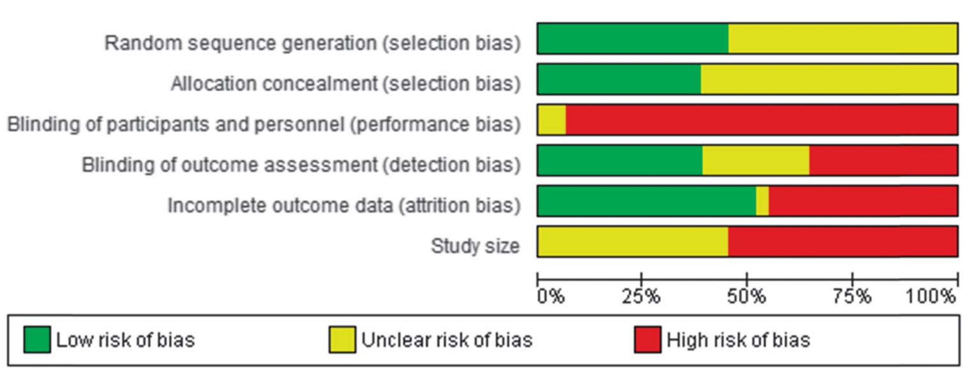


**Figure 3.** Risk of bias assessed using the Cochrane Risk of Bias tool.

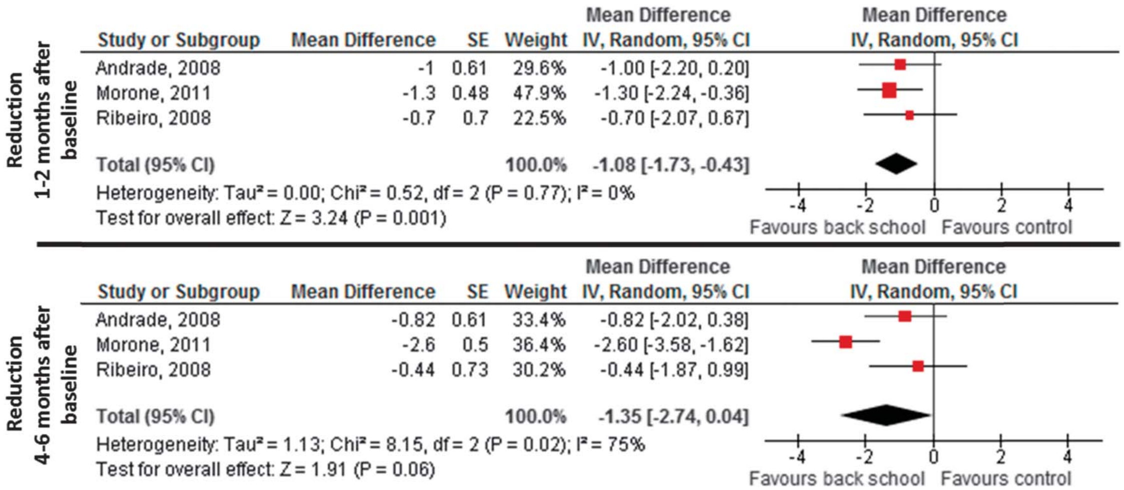


**Figure 4.** Changes in visual analogue pain scores (0- to 10-cm scale) in studies comparing participants taking part in back school programs with control groups (no treatment). CI, confidence interval, IV, inverse variance, Random, random-effects model.

reached, for the following reasons. First, the heterogeneity between the studies in terms of intervention characteristics and outcomes reported makes dependable information synthesis challenging. Second, our preferred responder pain outcomes were never reported and the work-related outcomes were infrequently and variably reported so that no meaningful information synthesis could be undertaken here either. Third, only 5 studies reported efficacy outcomes in a way amenable to meta-analysis. Finally, adverse event reporting was very poor, so that reliable conclusions regarding the safety of back schools cannot be drawn, although it is hard to imagine a high likelihood of serious adverse events, given that few adverse events and no serious adverse events were reported in the back school groups in the studies that did report on safety outcomes.

At first glance, the possibility of an educational and training intervention causing adverse effects seems unlikely, but it may not be as simple as that. There is a phenomenon, termed "risk compensation," whereby if workers are provided with protective equipment, they may paradoxically end up acting in a less safe manner in the workplace, because of a misconception as to the actual protection provided.[4,21] By the same logic, the possibility has to be entertained that, if workers participate in back school programs, they may subsequently overestimate the protection afforded by ergonomic lifting techniques and do more lifting than before and possibly experience adverse events associated with this greater strain. Such adverse events would be difficult to capture even under the best study conditions as they might present some time after the intervention and as subjects and investigators might not make the association.

With such tenuous evidence of efficacy as the present review demonstrates, the possibility needs to be entertained that the benefit seen is derived from some nonspecific interaction with treatment providers, ie, from context effects rather than through any genuine effect of back schools per se. There is evidence that, for pain, placebo can work better than no treatment,[25] and the possibility that that is what is happening here cannot be excluded.

Should future research efforts be devoted to the study of back schools? It seems legitimate to ask the question of whether it is time to stop looking at back school as a treatment for chronic back pain. If in the 47 years since 1969, no evidence of a clear benefit could be obtained; this might mean that there is none and that any apparent efficacy is the result of biases at work in a situation when trials were largely small and not double blind. Albert Einstein is sometimes credited with defining insanity as doing the same thing over and over again and expecting different results.

However, as the studies so far have typically not been using what we now know to be appropriate outcomes, perhaps such a stance would be premature. In a recent Cochrane review of

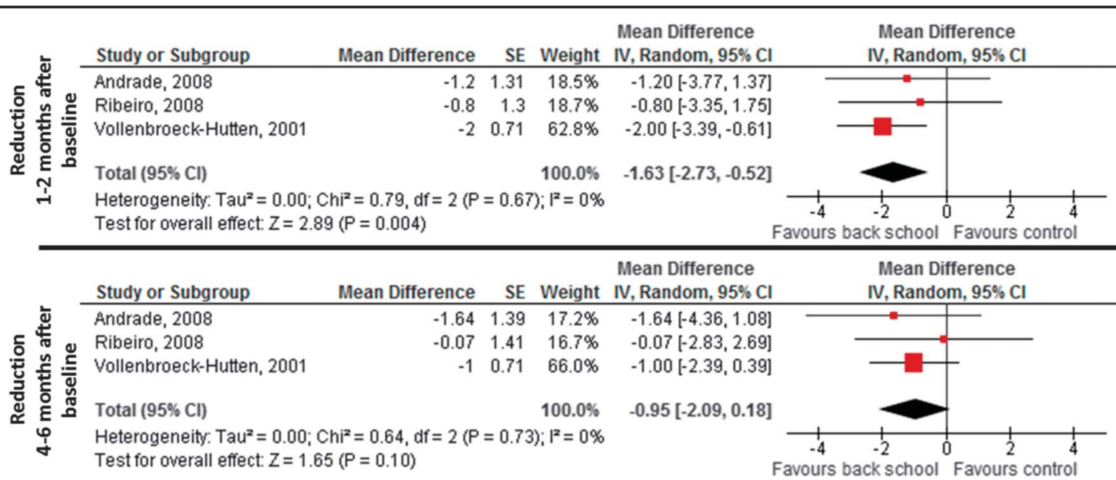


**Figure 5.** Changes in Roland Morris Disability Questionnaire scores in studies comparing participants taking part in back school programs with control groups (no treatment). CI, confidence interval, IV, inverse variance, Random, random-effects model.

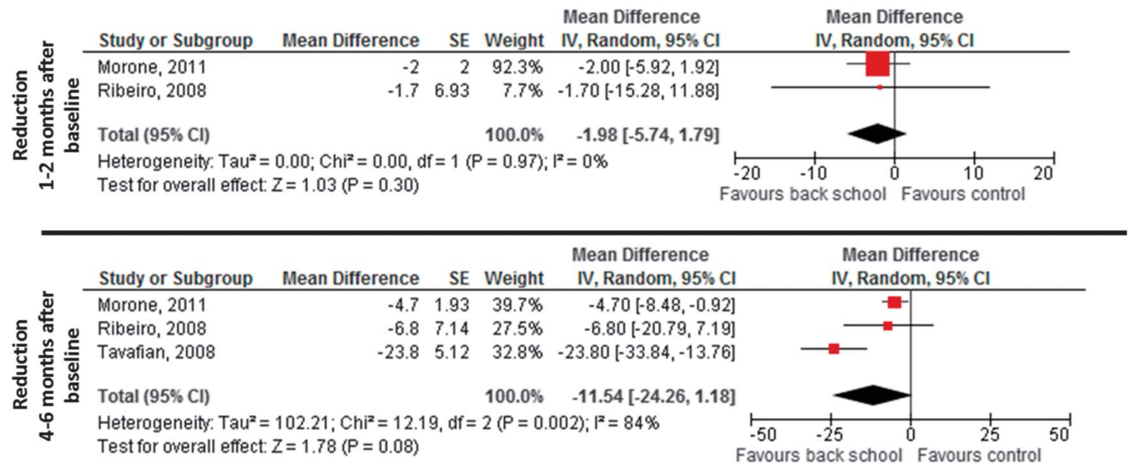


**Figure 6.** Changes in Short Form (36) Health Survey physical component scores in studies comparing participants taking part in back school programs with control groups (no treatment). CI, confidence interval, IV, inverse variance, Random, random-effects model.

vitamin D for chronic pain, there was uncertainty about benefit despite a number of trials having been conducted, and it was concluded that deficiencies in the trials done to date may at least inform the design of future trials.[47] Similarly, a Cochrane review of psychological therapies for the management of chronic pain called for different designs of trials in the future as the present trials failed to inform on some clinically important questions.[55]

For back school trials, there is evidence of improvements in study methodology occurring over time. Our change-point analyses revealed that a significant improvement in terms of allocation concealment and incomplete data handling could be observed. A reason for the improvement in these 2 domains may have to do with efforts undertaken to improve the reporting of clinical trials inspired by the publication of the CONSORT (Consolidated Standards of Reporting Trials) statement[2] and subsequent revisions thereof.[46] It must be remarked, however, that differences in reported study quality may result from better reporting in addition to better conduct of trials.

If the subject of the efficacy and safety of back schools is to be revisited in future trials, there would therefore be a real need to conduct not only more but better studies. Such future studies would ideally report on the recommended responder pain outcomes,[35] report on outcomes informative of work productivity and time lost from work, and also report on adverse events. Follow-up would need to be of adequate duration to allow conclusions to be drawn about the effectiveness of the intervention for a chronic painful condition. The studies published in the last decade had at most 1-year follow-up periods. A 1-year follow-up would typically be considered adequate for trials of an intervention but for back schools, there might be benefit that manifests over a much longer time frame as a result of behavioural change, and it may manifest as a delay or reduction in the deterioration of back pain with age, and associated greater participation in work and social life, rather than a short-term change in pain scores. Such longer term effects may well not be captured in a 1-year or shorter follow-up period and may be best captured by work-related and functional outcomes.

Trials comparing back schools against no treatment or waiting list controls would be helpful as they could inform about efficacy and safety in principle. Trials that investigate back schools as part of multimodal treatments would also be helpful, and perhaps more so, because they would inform about the utility of back schools in clinical practice where the established standard for the treatment of chronic pain is a multimodal approach.[45] It would furthermore be informative to have analyses stratified by baseline pain intensity to see where back schools or multimodal treatments involving back schools might be most appropriately used. Finally, as a number of different

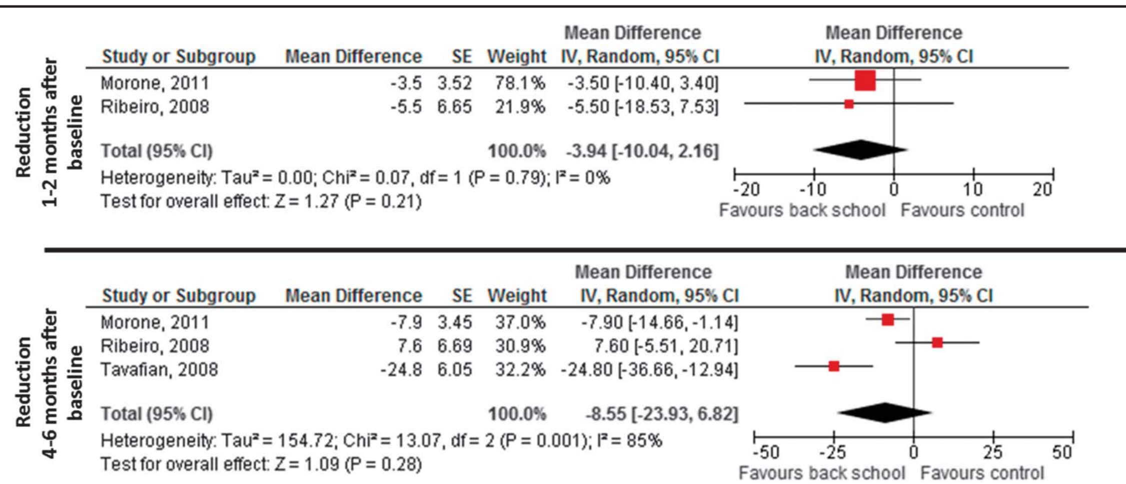


**Figure 7.** Changes in Short Form (36) Health Survey mental component scores in studies comparing participants taking part in back school programs with control groups (no treatment); CI, confidence interval, IV, inverse variance, Random, random-effects model.

Table 2

**Change-point analyses regarding low risk of bias for the 6 domains of the Cochrane Risk of Bias tool.**

| Domain | Low risk | Proportion of low risk (%) | Change point | Proportion of low risk until change point (%) | Proportion of low risk after change point (%) | *P* |
|---|---|---|---|---|---|---|
| Randomization | 14/31 | 45 | 1990 | 14 | 54 | 0.4299 |
| Allocation concealment | 12/31 | 39 | 1997 | 0 | 57 | 0.0070 |
| Blinding of patients | 0/31 | 0 | - | - | - | - |
| Blinding of assessors | 12/31 | 39 | 1990 | 14 | 46 | 0.6391 |
| Incomplete data | 16/31 | 52 | 2008 | 33 | 90 | 0.0276 |
| Other: size | 0/31 | 0 | - | - | - | - |

back school interventions are currently being used, future trials may assess different back school programs against one another.

Chou et al.[8] have a few years ago reviewed the evidence on nonpharmacological therapies for acute and chronic low back pain, finding that cognitive-behavioural therapy, exercise, spinal manipulation, and interdisciplinary rehabilitation were all moderately effective for chronic low back pain. This is the standard against which back schools would have to prove themselves.

Bias is a considerable concern in studies on back schools as double blinding will be challenging or simply impossible in such trials. Lack of double blinding can be associated with considerable bias in the form of an overestimate of efficacy, as we know from studies of interventions where double blinding is possible but has been inconsistently used.[48] As double blinding will be hard to achieve in studies comparing back schools with other interventions, it is important to bear this limitation in mind when interpreting the results and also to guard against other biases as best as possible.

Responder pain outcomes have been recommended for use in chronic pain clinical trials by a broad consensus group in 2008,[12] yet none of the studies we identified for inclusion in our systematic review had reported on such outcomes. Adverse event reporting was very poor, which is perhaps not surprising for studies investigating nonpharmacological interventions but nonetheless is a significant limitation of the evidence, especially because serious adverse events may be underreported when nondrug treatments are assessed.[13]

These limitations mean that we cannot report the preferred outcomes recommended for systematic reviews in chronic pain[35] that would have enabled ready comparison with other current systematic reviews in this field.

Strengths of this systematic review are that it adheres to what is now the established methodology for performing such reviews (duplication or independent verification of key steps in the review process and focus on outcomes of proven utility) and that it provides an up-to-date estimate of the efficacy and safety of back schools used in the treatment of chronic low back pain based on the highest quality evidence available, randomized controlled trials. The key limitation of our analysis is that this highest quality evidence available is still flawed by considerable between-study heterogeneity and by failure to report on the preferred responder pain outcomes and failure to report adequately on work-related outcomes and adverse events in the studies. The small number of studies and the methodological and statistical heterogeneity observed in some of our meta-analyses significantly limit the conclusions we can draw. Addressing the above limitations means to interpret our findings with caution.

## Conflicts of interest statement

M. Harden and B. Arendacka declare that they received salary support out of the project grant from the Federal Ministry of Education and Research, Germany. S. Straube declares honoraria from Oxford Medical Knowledge and an advisory board fee from Daiichi Sankyo, Inc.; all unrelated to this project. Sebastian Straube also declares travel support to attend a project meeting from the project grant from the Federal Ministry of Education and Research, Germany. H. Schröder declares that he has no conflicts of interest regarding this systematic review. X. Fan is a medical consultant for Millard Health and the Workers' Compensation Board Alberta; unrelated to this project. Xiangning Fan also declares travel support to attend a project meeting from the project grant from the Federal Ministry of Education and Research, Germany. R. A. Moore has received institutional grant support from RB and Grünenthal relating to individual patient level analyses in acute and chronic pain. He has attended boards with Menarini, Novartis (2014), and RB. None of this was related to back schools. T. Friede has received honoraria for consultancies (incl. memberships on data monitoring committees) within the past 3 years from Novartis, Biogen, AstraZeneca, Bayer, Johnson & Johnson, Janssen, Grünenthal, SGS, Quintiles, and Pharmalog; all unrelated to this project.

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## Appendix 1. MEDLINE (PubMed) Search Strategy

| | |
|---|---|
| #1 | randomized controlled trial [pt] |
| #2 | controlled clinical trial [pt] |
| #3 | randomized [tiab] |
| #4 | placebo [tiab] |
| #5 | drug therapy [sh] |
| #6 | randomly [tiab] |
| #7 | trial [tiab] |
| #8 | groups [tiab] |
| #9 | (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8) |
| #10 | (animals [mh] NOT humans [mh]) |
| #11 | (#9 NOT #10) |
| #12 | (back school [tw] OR backschool [tw] OR back-school [tw] OR back schools [tw] OR back-schools [tw] OR backschools [tw]) |
| #13 | (#12 AND #11) |

## Appendix 2. Description of the back school components

| Publication | Description of back school components |
|---|---|
| Klaber Moffett et al. 1986 | Anatomy and biomechanics of the spine, common causes of low back pain, exercises, ergonomic counselling, functional activities with minimal strain on the spinal structures |
| Härkäpää et al.[19] | Physiology of the back, etiology of low back pain, ergonomics, and exercises of proper working postures and movements |
| Hurri et al. 1989 | Education, exercises |
| Keijsers et al.[28] | Relaxation exercises, education, back exercises, correct posture, psychological and environmental factors |
| Mellin et al.[32] | As in Härkäpää et al.[19] |
| Härkäpää et al.[20] | As in Härkäpää et al.[19] |
| Mellin et al.[31] | As in Härkäpää et al.[19] |
| Frost et al.[14] | Discussion of patient's main problem, functional anatomy, simple applied body mechanics, advice regarding functional activities and exercise, relaxation techniques, ergonomic advice, prevention video, practical workshops |
| Mucha et al.[38] | Education, exercises, relaxation techniques |
| Bendix et al.[3] | "Theoretical class" according to Swedish back school principles as described by Zachrisson Forssell[57] |
| Frost et al.[15] | As in Frost[14] |
| Hodselmans et al.[24] | Treatment of mechanical capacity, treatment of energetic capacity, endurance, exercises, education |
| Vollenbroek-Hutten et al.[52] | Physiotherapy, exercises, education, occupational rehabilitation |
| Donzelli et al.[10] | Postural education exercises, respiratory education, muscular extension and strengthening exercises, mobilizing exercises, antalgic postures |
| Goldby et al.[18] | Q and A session covering anatomy, biomechanics and lifting, pathologies, and advice on education, exercise, and general fitness |
| Kääpä et al.[27] | Education, exercises |
| Tavafian et al.[49] | Education, self-awareness, exercises |
| Andrade et al.[1] | Education, muscular extension and strengthening exercises, handout |
| Ribeiro et al.[43] | Education, ergonomic guidelines relevant to back problems, exercises, relaxation |
| Tavafian et al.[50] | As in Tavafian[49] |
| Van der Roer et al.[51] | Not detailed |
| Cecchi et al.[7] | Education, relaxation techniques, postural and respiratory group exercises, individual back exercises |
| Meng et al.[33] | Education, exercises, social aspects, promotion of physical activity |
| Morone et al.[37] | Education, exercises, ergonomic use of the spine in daily life, spine stress coping strategies, handout |
| Sahin et al.[44] | Education, exercises, pain coping strategies |
| Morone et al.[36] | As in Morone[37] |
| Paolucci et al.[41] | As in Morone[37] |
| Paolucci et al.[42] | As in Morone[37] |
| Garcia et al.[17] | Education, exercises |
| Costantino et al.[9] | Education, muscular extension and strengthening exercises |
| Durmus et al.[11] | Education, exercises, pain coping strategies |

## Appendix 3. Risk of bias in individual studies

| | Random sequence generation (selection bias) | Allocation concealment (selection bias) | Blinding of participants and personnel (performance bias) | Blinding of outcome assessment (detection bias) | Incomplete outcome data (attrition bias) | Study size |
|---|---|---|---|---|---|---|
| Andrade 2008 | + | + | − | ? | − | − |
| Bendix 1997 | + | ? | − | + | − | − |
| Cecchi 2010 | ? | ? | − | + | + | ? |
| Costantino 2014 | + | ? | − | ? | + | − |
| Donzelli 2006 | ? | ? | ? | ? | + | − |
| Durmus 2014 | ? | ? | − | − | + | ? |
| Frost 1995 | + | ? | − | + | − | − |
| Frost 1998 | + | + | − | + | − | − |
| Garcia 2013 | + | + | − | + | + | ? |
| Goldby 2006 | + | + | − | + | ? | ? |
| Härkäpää 1989 | ? | ? | − | − | + | ? |
| Härkäpää 1990 | ? | ? | − | − | − | ? |
| Hodselmans 2001 | ? | ? | − | ? | + | − |
| Hurri 1989 | + | ? | − | − | + | ? |
| Kääpä 2006 | + | + | − | ? | − | ? |
| Keijsers 1989 | ? | ? | − | − | − | − |
| Klaber Moffett 1986 | ? | ? | − | + | − | − |
| Mellin 1989 | ? | ? | − | − | + | ? |
| Mellin 1990 | ? | ? | − | − | − | ? |
| Meng 2011 | + | + | ? | ? | + | ? |
| Morone 2011 | ? | ? | − | + | + | − |
| Morone 2012 | ? | + | − | + | + | − |
| Mucha 1996 | ? | ? | − | − | + | − |
| Paolucci 2012a | ? | ? | − | − | + | − |
| Paolucci 2012b | ? | + | − | − | − | − |
| Ribeiro 2008 | + | + | − | + | + | − |
| Sahin 2011 | + | + | − | + | + | ? |
| Tavafian 2007 | ? | ? | − | ? | − | − |
| Tavafian 2008 | ? | ? | − | ? | − | − |
| van der Roer 2008 | + | + | − | − | − | ? |
| Vollenbroek-Hutten 2004 | + | + | − | + | − | ? |

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