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REVIEW

Does the use of small aids during patient handling activities lead to a decreased occurrence of musculoskeletal complaints and diseases? A systematic review

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Abstract
Purpose Patient handling increases the risk of musculoskeletal complaints and diseases among healthcare workers. Thus, the use of small aids for patient handling is recommended. Small aids are non-electrical and handy assistive devices that support caregivers during patient handling. To date, there is no evidence about the clinical efficacy of small aids. Hence, the objective of this systematic review was to systematically analyze whether the use of small aids during patient handling leads to a decreased occurrence of musculoskeletal disorders.
Methods A systematic literature search was carried out. The review process was done independently by two reviewers. Methodology was assessed with the "Downs and Black checklist" and the "Risk of Bias tool." Quality of evidence was determined with the GRADE method.
Results One randomized and two non-randomized trials were included. Three comparisons of intervention assessing the lumbar spine and shoulder joint were investigated. A statistically significant improvement of the 7-day prevalence of low back pain and shoulder pain was achieved within the intervention group over time of questionable clinical importance in a study with comparisons made between small aids and usual practice or mechanical aids. No comparison between the intervention group and control group at follow-up was made. Each trial showed an insufficient methodology and a high risk of bias. Quality of evidence was low for disability scores and very low for pain outcomes.
Conclusions To date, there is no convincing evidence (from low-quality studies) for the preventability of musculoskeletal complaints and diseases by the use of small aids. The literature also lacks evidence for the opposite. Generalizability of the study results is further debatable due to the different populations and settings that were investigated. Robust, high-quality intervention studies are necessary to clarify the clinical efficacy of small aids in healthcare work.
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Keywords Ergonomics · Small aids · Healthcare worker · Musculoskeletal · Systematic review

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Introduction

Prevalence of musculoskeletal complaints and diseases is more in nursing staff than in other occupational groups (Hofmann et al. 2002; Schneider et al. 2006). A 12-month prevalence of musculoskeletal complaints and diseases of

58–78 % was determined among nursing staff (Koppelaar et al. 2013; Tinubu et al. 2010). The lumbar spine, the cervical spine, and the shoulder joints are the most affected body parts (Tinubu et al. 2010; Trinkoff et al. 2002). The 12-month prevalence of musculoskeletal complaints of the lumbar spine ranges from 44 to 61 % among nursing staff (Karahan et al. 2009; Tinubu et al. 2010), in intensive care units even up to 90 % (June and Cho 2011). For problems of the cervical spine and the neck, a 12-month prevalence of 28–46 % was reported (Tinubu et al. 2010; Trinkoff et al. 2002), for problems of the shoulder joints of 35 % (Trinkoff et al. 2002). Work-related musculoskeletal complaints and diseases are also common in physical therapists (Alperovitch-Najenson et al. 2014).

Cumulative spinal loads caused by occupational manual material handling (and patient handling as a part of it) and working postures with forward bending are associated with lumbar disk herniation (Seidler et al. 2003, 2007, 2009) and lumbar chondrosis (Bolm-Audorff et al. 2007; Seidler et al. 2001, 2009, 2011) in a positive dose–response relationship. In nursing staff, patient handling is one of the most relevant risk factors for lumbar complaints (Engkvist 2008; Engkvist et al. 2000; Yassi and Lockhart 2013). Patient handling tasks that are associated with low back pain are as follows: repositioning a patient in bed (Smedley et al. 1995); transferring a patient between bed and chair (Smedley et al. 1995); pulling a patient up the bed (Karahan et al. 2009); and bending to lift a patient from the floor (Karahan et al. 2009). Transferring a patient in a wheelchair, bed, hoist, trolley, or commode increases the risk of neck and shoulder pain in nurses (Smedley et al. 2003). Patient handling is also a risk factor for work-related musculoskeletal disorders among physical therapists (Campo et al. 2008) and occupational therapists (Rice et al. 2011).

Several intervention strategies exist to improve the burden and varying consequences of patient handling (Dawson et al. 2007; Hignett 2003; Tullar et al. 2010). One is the selection and acquisition of adequate work equipment which should be part of a multimodal prevention approach according to the German Social Accident Insurance Institution for the Health and Welfare Services [Berufsgenossenschaft für Gesundheitsdienst und Wohlfahrtspflege (BGW)] (BGW 2013). A Cochrane review investigated the effects of manual material handling advice and training with and without aids, but patient handling as a part of manual material handling was not examined separately, which is the focus of this systematic review (Verbeek et al. 2011). Conclusions regarding the training and provision of equipment for patient handling only are differing: one review reports conflicting and another moderate evidence about the efficacy of transfer devices (Dawson et al. 2007; Hignett 2003). There are two types of transfer devices for patient handling: mechanical aids and small aids (BGW 2013). The two aforementioned systematic reviews investigated transfer devices as a whole and did not make a difference between these two types of equipment (Dawson et al. 2007; Hignett 2003). A systematic review about the effects of mechanical aids does exist (Vieira and Miller 2008), but to the authors knowledge there is no systematic review which surveys the efficacy of small aids related to the occurrence of musculoskeletal complaints and diseases for persons who conduct patient handling activities.

Since an explicit definition of small aids is not known, the following definition was carried out by the review authors. Thus, small aids are assistive devices that support caregivers during patient handling. Small aids work non-electric and are of handy size, so that they can be stored and transported easily. Small aids are bed ladders, anti-slide mats, slide boards/transfer boards, turn tables, handling belts/gait belts, transfer mats, slide sheets, and slings among others (BGW 2013; GUV 2007; Hignett et al. 2003). According to the results of experimental studies, small aids can improve biomechanical outcomes (Elford et al. 2000; Jager et al. 2013). To date, there is no evidence whether such changes of biomechanical loads are accompanied with an improvement of the clinical efficacy. Hence, only studies that considered clinical outcomes, more precisely musculoskeletal complaints and diseases of the lumbar spine, cervical spine, and the shoulder joints, were included in this systematic review.

The aim of this systematic review was to investigate whether the use of small aids leads to a decreased occurrence of musculoskeletal disorders of the lumbar spine, the cervical spine, and the shoulder joints in persons who transfer patients.

Methods

A systematic review was conducted according to the PRISMA statement (Moher et al. 2009). The study protocol was published on the "International Prospective Register of Systematic Reviews" (PROSPERO) prior to the study conduct (PROSPERO registry number: CRD42014009767) (Freiberg et al. 2014).

Inclusion and exclusion criteria

Inclusion and exclusion criteria were compiled based on the PICOS criteria (population, intervention, comparison, outcome, and study design) (Moher et al. 2009). Persons who transfer patients on a regular basis (nursing staff, physical therapists, occupational therapists, caregiving volunteers, and family members), aged 15–70 years, working in all healthcare facilities where patient handling activities are conducted, were included. The use of small aids

during patient handling activities, as individual measure or as part of a multimodal intervention, was considered as intervention. For the comparison patient handling with the routinely method used, without aids or with mechanical aids was allowed. Also, similar interventions as used in the intervention group but without the use of small aids were considered as comparison. Furthermore, studies without a control intervention (before–after studies) were included. Musculoskeletal complaints and diseases of the lumbar spine, the cervical spine, and the shoulder joints were included as outcomes for the "clinical efficacy" of small aids. Therefore, the original definitions of the authors of each trial were used. Excluded were studies with biomechanical outcomes or with perceived exertion as outcome. Randomized and non-randomized controlled trials and before–after studies, published in all languages, were included. Commentaries, expert opinions, editorials, case reports, case series, and narrative and systematic reviews were excluded.

Search strategy

A systematic electronic literature search was conducted using the following databases from their date of origin up to May 14, 2014: MEDLINE (via PubMed); EMBASE (via Ovid interface); AMED (via Ovid interface); CINAHL (via EBSCOhost); and PEDro (via their homepage: http://www.pedro.org.au). The systematic electronic search did not apply any language restrictions. The search string was created sensitively by combining the keywords with Boolean operators and adapted respectively to each database. A full search strategy including all keywords is available on PROSPERO (PROSPERO registry number: CRD42014009767) or on request (Freiberg et al. 2014).

A hand search in the reference lists of all included studies after title and abstract screening and in the reference lists of related key articles supplemented the electronic search.

Study selection

Titles and abstracts of the studies as well as full texts were subsequently screened independently by two reviewers (AF and UE) with regard to the a priori defined research question, in line with the defined inclusion and exclusion criteria. Disagreements were resolved by discussion in consensus conferences. With lack of agreement, a third reviewer (MG) was consulted. The title and abstract screening process was piloted beforehand. Excluded studies of the full text screening process were documented as tables with exclusion criterion.

To assess the agreement between the two reviewers for title and abstract screening and full text screening, the proportion of observed agreement and Cohen's kappa were calculated (Cohen 1960).

Data extraction

Data extraction from included studies was done independently by two reviewers (AF and UE) using a standardized data extraction sheet and discussed subsequently in consensus conferences. The data extraction process was piloted beforehand.

Methodological quality assessment

The assessment of the methodology of included studies was done independently by two reviewers (AF and UE). Disagreements were discussed in consensus conference, and in case of a lack of agreement, a third reviewer (MG or AS) decided about evaluation. Randomized trials were assessed with the "Risk of Bias tool" (Higgins and Green 2011); non-randomized trials, with an adapted "Downs and Black checklist" (Downs and Black 1998) where only questions about internal validity (questions 14–26) were considered. These 13 questions contain six of the seven domains of bias of the "Risk of Bias tool" (random sequence generation, allocation concealment, blinding of participants and personnel, blinding of outcome assessment, incomplete outcome data, and selective outcome reporting), aside from "other bias." The answer options "0" and "1" of the "Downs and Black checklist" were adapted to the judgments of risk of bias ("high risk," "low risk," "unclear risk") of the "Risk of Bias tool." Such an adaption was also described in a Cochrane protocol about interventions to support return to work for patients with coronary heart disease (Euler et al. 2013).

Additionally, the risk of bias within and across studies was determined, based on the assessment of each study with the "Risk of Bias tool." For that purpose, the following relevant key domains were defined: random sequence generation, allocation concealment, blinding of outcome assessment, and selective outcome reporting. The risk of bias within a study was "low" if all key domains were judged to be at "low risk," was "unclear" if at least one key domain was judged to be at "unclear risk," and was "high" if at least one key domain was judged to be at "high risk." The risk of bias across studies was determined as follows: The risk of bias across studies was "low," if the risk of bias within all studies was at "low risk;" was "unclear," if the risk of bias within at least one study was at "unclear risk" and was "high," if the risk of bias within at least one study was at "high risk."

In case of missing information for judgment of methodological quality of a study, the lead author was contacted. Reasons for study quality rating were documented in an

appraisal form. The methodological quality assessment process was piloted beforehand.

Data analysis and data synthesis

Study results were analyzed descriptively for each comparison of intervention and each associated outcome. The quality of evidence was determined with the GRADE approach (Grading of Recommendations, Assessment, Development and Evaluation) by two reviewers (AF and UE) for each comparison of intervention and each associated outcome (Atkins et al. 2005).

Results

Study selection

Results of study selection are summarized in Fig. 1.

The electronic search identified 1072 hits. After duplication cleansing, 934 titles and abstracts were screened, of which 30 studies were included in full text screening. Twenty-seven full texts were excluded because the intervention, the study design, or the outcome did not meet the inclusion criteria of this systematic review. No additional search results were identified through hand search. Three studies were included in qualitative data synthesis (Hartvigsen et al. 2005; Muto et al. 2008; Yassi et al. 2001). The conduct of a meta-analysis was not possible due to heterogeneity of outcomes and the small number of studies. Agreement among reviewers regarding screening of titles and abstracts yielded a proportion of observed agreement of 0.96 and a Cohen's kappa of 0.36 (fair) and regarding screening of full texts a proportion of observed agreement of 0.97 and a Cohen's kappa of 0.84 (almost perfect).

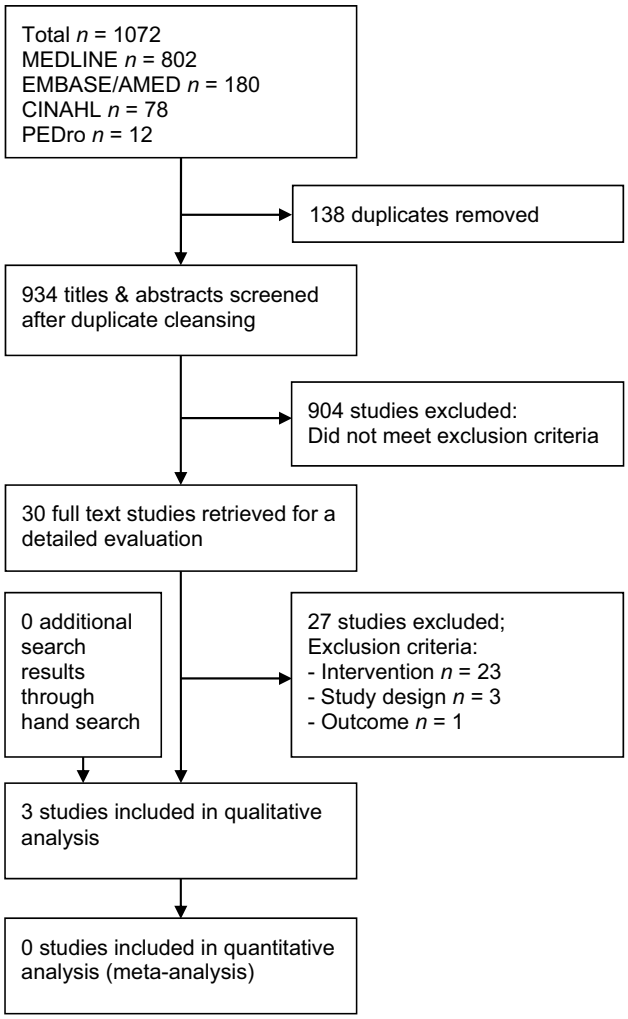

Fig. 1 Flow diagram of the study selection

Study characteristics

Three studies were included for further evaluation (Hartvigsen et al. 2005; Muto et al. 2008; Yassi et al. 2001). Hartvigsen et al. (2005) investigated in a non-randomized controlled trial with a 2-year follow-up whether an intensive ergonomic education program [which included provision of and training in patient handling with small aids (sheets, slings)] was superior to a one-time ergonomic education session in improving the 12-month prevalence of low back pain in nurses and nursing aids. The study was conducted in home care institutions. Muto et al. (2008) conducted a non-randomized controlled trial in prefectural schools for disabled children with teachers and nurses to examine the effect of the use of small aids (mat with attached handles, trousers with knee pads, waist holding belt) compared to no use of small aids in patient handling in preventing musculoskeletal pain of the lumbar spine and the upper arm region. Follow-up comprised 4–6 months. Yassi et al. (2001) executed a three-armed randomized controlled trial in an acute and tertiary care hospital with nurses and nursing aids with a follow-up of 12 months. One arm comprised an education in the use of and provision with small aids (transfer belt, sliding device), and the other two arms comprised usual practice of patient handling or patient handling with mainly mechanical aids. These latter two arms served as control groups for this systematic review. The 7-day prevalence of low back pain and shoulder pain and disability of the lumbar spine and the upper extremities were outcomes of interest.

Hartvigsen et al. (2005) and Muto et al. (2008) compared baseline characteristics of the intervention group and the control group. Study groups in the study of Hartvigsen et al. were similar for all but one characteristic (self-rated health) including the outcome of interest (number of

days with low back pain during past year). In the study of Muto et al., demographics were similar between groups. Prevalence data of the outcome of interest were different between groups but reached no statistical significance (low back pain: $p = 0.065$, upper arm pain: $p = 0.366$). A comparison of baseline characteristics was not reported in the study of Yassi et al. (2001).

Detailed study characteristics and risk of bias of each study can be seen in Table 1.

Methodological quality assessment

After communication with the lead authors of the studies of Hartvigsen et al. (2005) and Muto et al. (2008), existing uncertainties about methodology could be resolved. Most of the questions about internal validity of the "Downs and Black checklist" of these two non-randomized controlled trials were judged as "low risk." Four of the seven risk domains of the "Risk of Bias tool" in the randomized controlled trial of Yassi et al. could only be judged with an unclear risk of bias because relevant study information was missing: method of randomization for "random sequence generation," execution of "allocation concealment," information about a non-responder, or an intention to treat analysis for "incomplete outcome data," and information about a study protocol for "selective reporting." The lead author did not reply to questions about these domains (Yassi et al. 2001). All three studies received a "high risk" for "other bias." Yassi et al. did not report any characteristics of participants. In the study of Hartvigsen et al., compliance measure was missing. Muto et al. used an own questionnaire for data collection with no available information about accuracy.

Methodological quality assessment with the "Downs and Black checklist" is shown in Table 2 and with the "Risk of Bias tool" in Table 3.

Risk of bias was high in each study, because at least one key domain in each study was at "high risk." As a result of this, risk of bias across studies was high.

Study results

The following comparisons of intervention were identified:

- Provision of and intensive education in patient handling with small aids versus one-time ergonomic education (small aids vs. one-time ergonomic education) (Hartvigsen et al. 2005).
- Provision of and education in patient handling with small aids versus no intervention or usual practice (small aids vs. no intervention or usual practice) (Muto et al. 2008; Yassi et al. 2001).
- Provision of and education in patient handling with small aids versus provision of and education in patient handling with mechanical aids (small aids vs. mechanical aids) (Yassi et al. 2001).

Small aids versus one-time ergonomic education

In the study of Hartvigsen et al. (2005), no statistically significant difference in 12-month prevalence of low back pain at 2-year follow-up between intervention and control group was found for the comparison of provision of and intensive education in patient handling with small aids with one-time education. The authors did not provide any prevalence data, but referred to the results of a χ^2 test ($p < 0.88$). Musculoskeletal complaints and diseases of the cervical spine and the shoulder joints were not evaluated (Hartvigsen et al. 2005).

Small aids versus no intervention or usual practice

For the comparison of provision of and education in patient handling with small aids with no intervention or usual practice, one study by Muto et al. (2008) demonstrated an increase in 1-month prevalence of low back pain at follow-up in the intervention group (from 57.1 to 61.9 %, +4.8 percent points, $p = 1.000$) as well as in the control group (from 27.8 to 55.6 %, +27.8 percent points, $p = 0.063$) with no statistically significant difference within each study group over time. In the same study, the 1-month prevalence of upper arm pain decreased in the intervention group (from 47.6 to 23.8 %, −23.8 percent points, $p = 0.063$), but increased in the control group (from 33.3 to 38.9 %, +5.6 percent points, $p = 1.000$) at follow-up, but these changes were also not statistically significant. It should be noted that these comparisons were made only within groups over time, but not between groups at follow-up (Muto et al. 2008). In another study by Yassi et al. (2001), the 7-day prevalence of low back pain as well as shoulder pain decreased statistically significant in the intervention group ($p = 0.012$), but not in the control group ($p > 0.05$) at 1-year follow-up. The authors did not compare the prevalence data at follow-up between groups. No statistically significant differences of disability of the lumbar spine and the upper extremities, measured with the Oswestry Low Back Pain Questionnaire and the disabilities of the arm, shoulder, and hand (DASH) were found within and across groups, respectively. Complaints and diseases of the cervical spine were not investigated.

Small aids versus mechanical aids

Only one study by Yassi et al. (2001) examined the comparison of the provision of and education in patient handling with small aids with the provision of and education

Table 1 Summary of study characteristics

References	Place	Design	Setting	Population	Intervention	Comparison	Outcome	Risk of bias
Hartvigsen et al. (2005)	Aarhus, Denmark	NRCT; FU: 2 yr.	Home care institutions	Occupation: Nurses Nursing aids Sex (% female): 100 Age ($\bar{X}$): I: 44.6 yr. C: 44.4 yr. *n* baseline: I: 171 C: 145 *n* follow-up: I: 140 C: 115 FU-rate in %: I: 82 C: 79	Provision of and intensive education in patient handling with small aids: Provision of small aids (plastic sheets and slings) Intensive education in transfer techniques, biomechanics and patient handling with small aids (plastic sheets and slings) Motivation in using small aids	One-time ergonomic education: One-time 3-h education in transfer techniques No provision of small aids	12-month prevalence of low back pain (measured with the question about the number of days with low back pain during the past year)	High
Muto et al. (2008)	Shizuoka, Japan	NRCT; FU: 4–6 mo.	Prefectural schools for disabled children	Occupation: Teachers Nurses Sex (% female): 74 Age ($\bar{X}$): I: 34.6 yr. C: 34.8 yr. *n* baseline: I: 21 C: 20 *n* follow-up: I: 21 C: 18 FU-rate in %: I: 100 C: 90	Provision of and education in patient handling with small aids: Provision of small aids (mat with attached handles, trouser with knee pads, waist holding belt) Instruction to use small aids during patient handling	No intervention or usual practice: No provision of small aids	1-month prevalence of low back pain (measured as presence or absence of low back pain within the past month) 1-month prevalence of upper arm pain (measured as presence or absence of upper arm pain within the past month)	High

Table 1 continued

References	Place	Design	Setting	Population	Intervention	Comparison	Outcome	Risk of bias
Yassi et al. (2001)	Manitoba, Canada	RCT; FU: 1 yr.	Acute and tertiary care hospital with three types of service: Medical Surgical Rehabilitation	Occupation: Nurses Nursing aids *n* baseline: I: 116 C_1: 103 C_2: 127 *n* follow-up: I: 85 C_1: 82 C_2: 94 FU-rate in %: I: 73 C_1: 80 C_2: 74	Provision of and education in patient handling with small aids: Provision mainly of small aids (transfer belt, sliding device) One-time 3-h ergonomic education inclusive education in patient handling with small aids Motivation in using small aids	No intervention or usual practice (C1): Training in use of aids of regular use (total body lift, sliding device) Ergonomic training only upon request Provision of sliding device only upon request Provision of and education in patient handling with mechanical aids (C2): Provision mainly of mechanical aids Sliding devices in each room One-time 3-h ergonomic education inclusive education in patient handling with mechanical aids	7-day prevalence of low back pain (measured on a VAS 0–100) 7-day prevalence of shoulder pain (measured on a VAS 0–100) Disability of the lumbar spine (measured with the Oswestry Low Back Pain Disability Questionnaire) Disability of the upper extremities (measured with the disabilities of the arm, shoulder and hand)	High

C control group, *FU* follow-up, *I* intervention group, *mo.* month, *n* number of participants, *NRCT* non-randomized controlled trial, *RCT* randomized controlled trial, *VAS* visual analog scale $\bar{X}$ mean, *yr.* year

Table 2 Methodological assessment of the non-randomized controlled trials with the "Downs and Black checklist"

Question	Hartvigsen et al. (2005)	Muto et al. (2008)
14	HR	HR
15	LR	HR
16	LR	LR
17	LR	LR
18	LR	LR
19	HR	LR
20	LR	HR
21	LR	LR
22	LR	LR
23	HR	HR
24	HR	HR
25	LR	UR
26	LR	LR

Questions "Downs and Black checklist"

14. Was an attempt made to blind study subjects to the intervention they have received?

15. Was an attempt made to blind those measuring the main outcomes of the intervention?

16. If any of the results of the study were based on "data dredging," was this made clear?

17. In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case–control studies, is the time period between the intervention and outcome the same for cases and controls?

18. Were the statistical tests used to assess the main outcomes appropriate?

19. Was compliance with the intervention/s reliable?

20. Were the main outcome measures used accurate (valid and reliable)?

21. Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case–control studies) recruited from the same population?

22. Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case–control studies) recruited over the same period of time?

23. Were study subjects randomized to intervention groups?

24. Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?

25. Was there adequate adjustment for confounding in the analyses from which the main findings were drawn?

26. Were losses of patients to follow-up taken into account?

HR high risk, *LR* low risk, *UR* unclear risk

in patient handling with mechanical aids. There was a statistically significant decrease in low back pain and shoulder pain at 1-year follow-up in the intervention group ($p = 0.012$) which was not prevalent in the control group ($p > 0.05$). A comparison of prevalence data at follow-up was not reported. Disability scores of the lumbar spine and upper extremities did not differ within and across study groups. The outcome "cervical spine" was not considered.

Qualitative evidence synthesis with GRADE

Quality of evidence was low for all disability scores and very low for all outcomes regarding pain (Atkins et al. 2004). Table 4 illustrates the detailed evaluation of each comparison of intervention and the corresponding outcomes. The criterion "inconsistency" was not determined, because for each comparison of intervention only one study was available.

Discussion

Since the use of small aids for patient handling is recommended to staff in nursing sector and to caregiving family members (BGW 2013; GUV 2007) and small aids can improve biomechanical loads (Jager et al. 2013), an extensive systematic literature search was conducted to evaluate the clinical efficacy of small aids. Even if the search strategy was consciously chosen sensitive and 934 titles and abstracts were screened, only three studies fit the inclusion criteria (Hartvigsen et al. 2005; Muto et al. 2008; Yassi et al. 2001). This indicates that not much research is available on the topic, yet. The study by Muto et al. (2008) was the only one that focused their research on the clinical efficacy of small aids. The other two investigated small aids only as part of a multimodal intervention program (Hartvigsen et al. 2005; Yassi et al. 2001).

A statistically significant effect was only observed for the improvement of the 7-day prevalence of low back pain and shoulder pain within the intervention groups over time for the comparisons between small aids and usual practice or mechanical aids. The authors did not compare these prevalence data between the intervention group and the control group at follow-up (Yassi et al. 2001). No statistically significant effects were observed in the other two studies. The question is whether truly no effects of small aids regarding the defined outcomes could be observed or whether there was a failure to observe real effects due to the insufficient methodology of included studies. The statistically significant effect in the study of Yassi et al. is, moreover, of questionable clinical importance, because the change measured on a visual analog scale ranges from 0 to 100, was less than 10 points, but needs at least 13 points to have clinical importance (Todd et al. 1996).

It is difficult to determine whether an under- or overestimation of study results is existent in the study by Muto et al. (Muto et al. 2008). On the one hand, the sample size and the power of the study were very low, which could have led to a concealment of a true intervention effect (Altman

Table 3 Methodological assessment with the "Risk of Bias tool"

Bias	Hartvigsen et al. (2005)	Muto et al. (2008)	Yassi et al. (2001)
Random sequence generation (selection bias)	HR	HR	UR
Allocation concealment (selection bias)	HR	HR	UR
Blinding of participants and researchers (performance bias)	HR	HR	HR
Blinding of outcome assessment (detection bias)	LR	HR	HR
Incomplete outcome data (attrition bias)	LR	LR	UR
Selective reporting (reporting bias)	LR	LR	UR
Other bias	HR	HR	HR

HR high risk, *LR* low risk, *UR* unclear risk

and Bland 1995). On the other hand, they did not refer to the number of participants at baseline, but to the number of participants at follow-up in their outcome assessment, so that an overestimation of the effect of intervention is possible.

To compare the results of the intervention and the control group in the study of Muto et al. (2008) at follow-up would have been misleading, because 1-month prevalence of low back pain and upper arm pain at baseline were different in these groups but without a statistical significance. Hence, it was interesting to look at the changes of 1-month prevalence within the groups. Thereby, it was obvious that the increase in low back pain was much higher in the control group (+27.8 percent points, $p = 0.063$) compared to the intervention groups (+4.8 percent points, $p = 1.000$). An exclusive look at the 1-month prevalence of low back pain at follow-up would have created the impression that it was somewhat lower in the control group compared to the intervention group (55.6 and 61.9 %). Based on the 1-month prevalence of upper arm pain, a decrease in the intervention group (−23.8 percent points, $p = 0.063$) and an increase in the control group (+5.6 percent points, $p = 1.000$) were determined.

In regard to the follow-up time of such intervention studies, two issues should be discussed. Follow-up should be as long as for the development of outcomes of interest required. Since this takes a while, a measure of compliance of the use of small aids should be included, since non-compliance over such a long time could influence long-term study results. The study duration in the study by Muto et al. (2008) of 4–6 months was possibly too short to detect a real effect of the intervention, but a measure of compliance was conducted (personal information). The 2-year follow-up in the study of Hartvigsen et al. (2005) was probably long enough for outcome development. No measure of compliance was executed (personal information), but since the intervention comprised measures of motivation for the personnel for using small aids during the study period, it is assumed that compliance was high. The follow-up of 1 year in the study of Yassi et al. (2001) seems to be appropriate, but information about measures of compliance was missing.

Due to the nature of their non-randomized design, the studies of Hartvigsen et al. (2005) and Muto et al. (2008) were assessed as "high risk" in the domains "random sequence generation" and "allocation concealment." Blinding of participants and personnel was not performed in any of the included studies, and therefore, that domain was judged as "high risk." But it should be mentioned that such a blinding is very difficult to ensure in this kind of intervention studies (Verbeek et al. 2011).

According to the results of the evaluation with the GRADE approach, quality of evidence was very low for all outcomes regarding pain, which means that any estimate of effect is very uncertain and was low for all disability scores, which means that further research is very likely to have an important impact on the confidence in the estimate of effect and is likely to change the estimate (Atkins et al. 2004).

Only a few small aids (transfer belts, sliding devices, mats with attached handles, plastic sheets, and slings) were considered in included studies. So no statement can be made about the clinical efficacy of other small aids like bed ladders, anti-slide mats, slide boards, and turn tables. It is also not possible to determine whether physical therapists, occupational therapists, caregiving family members, and volunteers would have shown the same effects like the evaluated population of nurses, nursing aids, and teachers.

The three included studies of this review were, among others, also considered eligible in a Cochrane review about manual material handling advice and assistive devices for preventing and treating back pain in workers (Verbeek et al. 2011). Relating to the outcome "low back pain," Verbeek et al. determined the same results as found in this systematic review. Due to additional data they had received from Jan Hartvigsen, they were able to calculate an odds ratio for the 12-month prevalence of low back pain for the comparison of provision of and intensive education in patient handling with small aids versus one-time ergonomic education

Table 4 Quality of evidence with GRADE

Comparison/ outcome	References	Study design	Risk of bias	Inconsistency	Indirectness	Imprecision	Publication bias	Quality of evidence
Provision, intensive education SA versus one-time ergonomic education/LBP (12-month prevalence)	Hartvigsen et al. (2005)	1 NRCT	Serious	–	Not serious	Serious	No	Very low
Provision, education SA versus no intervention or usual practice/LBP (1-month prevalence)	Muto et al. (2008)	1 NRCT	Very serious	–	Not serious	Very serious	No	Very low
Provision, education SA versus no intervention or usual practice/LBP (7-day prevalence)	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Serious	No	Very low
Provision, education, SA versus no intervention or usual practice/ Oswestry Low Back Pain Questionnaire	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Not serious	No	Low
Provision, education SA versus no intervention or usual practice/shoulder and upper arm pain (1-month prevalence)	Muto et al. (2008)	1 NRCT	Very serious	–	Not serious	Very serious	No	Very low
Provision, education SA versus no intervention or usual practice/Shoulder and upper arm pain (7-day prevalence)	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Serious	No	Very low
Provision, education SA versus no intervention or usual practice/DASH	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Not serious	No	Low

Table 4 continued

Comparison/ outcome	References	Study design	Risk of bias	Inconsistency	Indirectness	Imprecision	Publication bias	Quality of evidence
Provision, education SA versus provision, education MA/ LBP (7-day prevalence)	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Serious	No	Very low
Provision, education SA versus provision, education MA/ Oswestry Low Back Pain Questionnaire	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Not serious	No	Low
Provision, education SA versus provision, education MA/shoulder and upper arm pain (7-day prevalence)	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Serious	No	Very low
Provision, education SA versus provision, education MA/DASH	Yassi et al. (2001)	1 RCT	Very serious	–	Not serious	Not serious	No	Low

DASH disabilities of the arm, shoulder and hand, *LBP* low back pain, *MA* mechanical aid, *NRCT* non-randomized controlled trial, *RCT* randomized controlled trial, *SA* small aid

that was not statistically significant [OR 1.12 (95 % CI 0.71–1.75)] (Verbeek et al. 2011).

Conclusions

Despite an extensive systematic literature search, only few studies about the clinical efficacy of small aids are existent. These demonstrate, in some extend, a statistically significant benefit of small aids of questionable clinical importance in reducing musculoskeletal disorders of the lumbar spine and the shoulder joints associated with patient handling within the study groups, but not across study groups. All included studies showed an insufficient methodology and a high risk of bias. Quality of evidence was low for all disability scores and very low for all outcomes regarding pain. No conclusion can be drawn for complaints and diseases of the cervical spine.

Generalizability of study results of this review is debatable, because different populations (nurses, nursing aids, and teachers) and settings (home care institution, acute and tertiary care hospital, and prefectural school for disabled children) were utilized. It can be assumed that personnel in various healthcare institutions and different caregivers like nursing staff, therapists, or caregiving family members are exposed to different exposures (Engkvist et al. 2000; Simon et al. 2008; Tullar et al. 2010).

There is currently no convincing evidence of the preventive benefit of small aids, i.e., that the use of small aids decreases musculoskeletal complaints and diseases in patient-handing persons. Evidence of the opposite, i.e., that the use of small aids increases musculoskeletal disorders, is also missing. Thus, there is a strong need for further robust research to investigate the utility of small aids in patient handling. With the implementation, training and use of small aids, in future, their clinical efficacy should be examined in form of evaluations or—better yet—in form of intervention studies. Therefore, the authors propose to conduct a cluster-randomized controlled trial, with a sufficiently long follow-up of at least 1 year. The intervention should include a measure of compliance for the use of small aids. The intervention group should receive training in patient handling with all kinds of small aids and subsequently be equipped with

them. The control group should work with their usual practice of patient handling. For better generalizability, settings and populations should be stratified. Then, these results should be incorporated into practice guidelines to improve the prevention of musculoskeletal complaints and diseases due to patient handling in daily practice (Kuijer et al. 2014).

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Compliance with ethical standards

Conflict of interest The authors declare that they have no conflict of interest.

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