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Rehabilitation After Lumbar Disc Surgery

An Update Cochrane Review

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Study Design. Cochrane systematic review of randomized controlled trials.

Objective. To evaluate the effects of active rehabilitation for adults after first-time lumbar disc surgery.

Summary of Background Data. Several rehabilitation programs are available for individuals after lumbar disc surgery, however, little is known about the efficacy of these treatments.

Methods. Search strategies were performed on CENTRAL (The Cochrane Library 2007, Issue 2) and MEDLINE, EMBASE, CINAHL, and PsycINFO up to May 2007. All randomized controlled trials without language limitations were included. Pairs of review authors independently assessed studies for eligibility and risk of bias. A meta-analysis was performed with clinically homogeneous studies. The GRADE approach was used to determine the quality of evidence.

Results. Fourteen studies were included, 7 of which had a low risk of bias. Most programs were only assessed in 1 study. Statistical pooling was only completed for 3 comparisons in which exercises started 4 to 6 weeks postsurgery: exercise programs *versus* no treatment, high *versus* low intensity exercise programs, and supervised *versus* home exercises. We found low quality evidence (3randomized controlled trials [RCTs], N = 122) that exercises are more effective than no treatment for pain at-short-term follow-up (weighted mean difference [WMD]: −11.13; 95% CI: −18.44 to −3.82) and moderate evidence (2 RCTs, N = 102) that exercises are more effective for functional status on short-term follow-up (WMD: −6.50; 95% CI: −9.26 to −3.74). None of the studies reported that exercises increased the reoperation rate. We also found low quality evidence (2 RCTs, N = 103) that high intensity exercises are slightly more effective than low intensity exercise programs for pain in the short-term (WMD: −10.67; 95% CI: −17.04 to −4.30) and moderate evidence (2 RCTs, N = 103) that they are more effective for functional status in the short-term (standardized mean difference [SMD] −0.77; 95% CI: −1.17 to −0.36). Finally, we found low quality evidence (3 RCTs, N = 95) that there were no significant differences between supervised and home exercises for short-term pain relief (SMD: −1.12; 95% CI: −2.77–0.53) or functional status (3 RCT, N = 95; SMD −1.17; 95% CI: −2.63–0.28).

Conclusion. Exercise programs starting 4 to 6 weeks postsurgery seem to lead to a faster decrease in pain and disability than no treatment. High intensity exercise programs seem to lead to a faster decrease in pain and disability than low intensity programs. There were no significant differences between supervised and home exercises for pain relief, disability, or global perceived effect. There is no evidence that active programs increase the reoperation rate after first-time lumbar surgery.

Key words: low back pain, post surgery, lumbar disc surgery, systematic review, rehabilitation, exercises. **Spine 2009;34:1839–1848**

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The results of a Cochrane review can be interpreted differently, depending on people's perspectives and circumstances. Please consider the conclusions presented carefully. They are the opinions of review authors, and are not necessarily shared by The Cochrane Collaboration.

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The lumbosacral radicular syndrome (LRS) is characterized by radiating pain over an area of the buttocks or legs served by 1 or more lumbosacral nerve roots combined with nerve root tension or neurologic deficit. The prevailing view is that the condition is most commonly caused by a lumbar disc prolapse; however, other pathologies may also cause LRS. It is estimated that there are between 60,000 and 75,000 new cases of LRS in The Netherlands each year,[1] for which the direct and indirect costs are estimated at 1.6 billion US $ per annum.[2] Many patients with LRS are treated conservatively, but surgery is a common option in patients with persistent symptoms. In The Netherlands, with a population of about 16 million people, it is estimated that 10,000 to 11,000 operations are performed each year due to the LRS[1] but surgery rates vary across countries. An international comparison showed that the rate of back surgery in the United States was at least 40% higher than in any other country and was more than 5 times those in England and Scotland.[3] But even within 1 country (*i.e.*, the United States), considerable regional variations are reported.[4] The reported success rate of lumbar disc surgery varies from 60% to 90%.[5–8] Differences between these studies with regard to inclusion criteria, indications for surgery and

definitions of success, may account for the wide range in success rate. Still, these figures show that the results of surgery are unsatisfactory in 10% to 40% of the patients still having symptoms. These persisting symptoms mainly consist of pain, motor deficits, decreased functional status, inability to return to work, or any combination. Recurrence of lumbar disc prolapse ranges from 3% to 12% in patients who underwent disc-surgery for the first time, resulting in these cases in reoperation.[9]

Further treatment is often recommended after lumbar disc surgery (*e.g.*, physiotherapy, rehabilitation programs), but there are persistent controversies about this topic. Should all patients receive further treatment? Or should only the patients who still suffer from persisting symptoms after surgery? The necessity and duration of activity restrictions after lumbar disc surgery is still controversial. Although several active rehabilitation/physical fitness programs are available and instruction protocols for patients to return to work after lumbar disc surgery have been suggested, there are still concerns that these interventions may cause reinjury, reherniation, or instability.[10–12] Unfortunately, little is known about the effectiveness of these treatments. In this updated review, we systematically evaluated the effectiveness of active treatments that are used in rehabilitation after first-time lumbar disc surgery.

This article is based on a Cochrane review first published in The Cochrane Library 2008, Issue 4 (information is available at: http://www.thecochranelibrary.com/). Cochrane reviews are regularly updated as new evidence emerges and in response to feedback, and The Cochrane Library should be consulted for the most recent version of the review.

■ Materials and Methods

The objectives of this systematic review were to determine if active rehabilitation after lumbar disc surgery is more effective than no treatment, and to describe which type of active rehabilitation is most effective.

First, we clustered treatments according to the start of treatment:

1. Active rehabilitation that starts immediately postsurgery,
2. Active rehabilitation that starts 4 to 6 weeks postsurgery,
3. Active rehabilitation that starts more than 12 months postsurgery.

For every cluster, the following comparisons were investigated:

Active rehabilitation *versus* no treatment, placebo or waiting list control,

Active rehabilitation *versus* other kind of active rehabilitation,

Specific intervention in addition to active rehabilitation *versus* active rehabilitation alone.

Criteria for Considering Studies for This Review

Types of Studies. Only randomized controlled trials were included in this review.

Types of Participants. Subjects who had first-time lumbar disc surgery due to a lumbar disc prolapse and aged between 18 and 65 years were included. All types of surgical techniques for lumbar disc herniation (*e.g.*, standard discectomy, microdiscectomy, laser discectomy, and chemonucleolysis) were included.

Types of Interventions. Trials with 1 or more types of active rehabilitation programs aiming at functional restoration (improvement in functional status and return to work) were included. Examples of treatments that were considered are (supervised) exercise therapy, functional restoration programs, or rehabilitation-oriented approaches in insurance medicine. Treatments solely targeting pain relief (*e.g.*, medication) or improvement of physical outcomes such as strength or flexibility were excluded.

Types of Outcome Measures. Trials were included if they used at least 1 of the 4 primary outcome measures that we considered to be important, that is pain (*e.g.*, Pain Intensity visual analogue scale [VAS]), a global measure of improvement (*e.g.*, overall improvement, proportion of patients recovered, subjective improvement of symptoms, Global-perceived effect scale), back-pain specific functional status (*e.g.*, Roland-Morris Disability Questionnaire, Oswestry Disability Index), and return-to-work (return-to-work status, days off work). Outcomes of physical examination (*e.g.*, spinal range of motion, straight-leg raise range of motion, or muscle strength), behavioral outcomes (*e.g.*, anxiety, depression, pain behavior) and generic functional status (SF-36, Nottingham Health Profile, Sickness Impact Profile) were considered as secondary outcomes. Other outcomes such as medication use and side-effects were also considered.

Search Methods for Identification of Studies

All relevant trials meeting our inclusion criteria were identified by:

A search in CENTRAL (The Cochrane Library 2007, Issue 2),

A computer-aided search of the MEDLINE (from 1966 to May 2007), EMBASE (from 1988 to May 2007), CINAHL (2000 to May 2007), and PsycINFO (from 1984 to May 2007) databases using the search strategy recommended by the Editorial Board of the Cochrane Back Review Group.[13] Specific search terms for low back pain, lumbar disc surgery, and postsurgery treatment were added. No language restriction was used. The complete search strategies are outlined in Appendix 1 (see Supplemental Digital Content 1, http://links.lww.com/A1326).

Screening of references given in relevant reviews and identified trials.

Screening of personal bibliographies and communication with experts in the field.

Data Collection and Analysis

Study Selection. Pairs of review authors independently selected the studies to be included in this systematic review by applying the selection criteria to the studies that were retrieved by the literature search. Consensus was used to solve disagreements concerning selection and inclusion of studies and a third review author was consulted if disagreements persisted.

Risk of Bias Assessment. The criteria recommended in the updated method guidelines for systematic reviews in the Cochrane Back Review Group (CBRG) were used (Table 1).[13]

Table 1. Criteria for the Risk of Bias Assessment

Adequate method of randomization: A random (unpredictable) assignment sequence. Examples of adequate methods are computer-generated random numbers table and use of sealed opaque envelopes. Methods of allocation using date of birth, date of admission, hospital numbers, or alternation should not be regarded as appropriate.
Adequate concealment of randomization: Assignment generated by an independent person not responsible for determining the eligibility of the patients. This person has no information about the persons included in the trial and has no influence on the assignment sequence or on the decision about eligibility of the patient.
Blinding of patients: The review author determines if enough information about the blinding is given in order to score a "yes." If blinding of patients was not feasible, we assessed this item to be positive if the credibility of applied treatments was evaluated and treatments were equally credible and acceptable to patients.
Blinding of care providers: The review author determines if enough information about the blinding is given in order to score a "yes."
Blinding of outcome assessment: The review author determines if enough information about the blinding is given in order to score a "yes."
Drop-out during intervention period (<10%) AND withdrawal during follow-up period (<20%): The no. of participants who were included in the study but did not complete the observation period or were not included in the analysis must be described and reasons given. If the percentage of drop-outs during the intervention period does not exceed 10% AND withdrawal during follow-up does not exceed 20% and does not lead to substantial bias, a "yes" is scored.
Intention-to-treat analysis: All randomized patients are reported/analyzed in the group to which they were allocated by randomization for the most important moments of effect measurement (minus missing values), irrespective of noncompliance and co-interventions.
Similarity of baseline characteristics: To receive a "yes," groups have to be similar at baseline regarding demographic factors, duration and severity of complaints, percentage of patients with neurological symptoms, and value of main outcome measure(s).
Cointerventions avoided or equal: Cointerventions should either be avoided in the trial design or be similar between the index and control groups.
Compliance: The review author determines if the compliance to the interventions is acceptable, based on the reported intensity, duration, no. and frequency of sessions for both the index intervention and control intervention(s).
Identical timing outcome assessment: Timing of outcome assessment should be identical for all intervention groups and for all important outcome assessments.

Pairs of review authors independently assessed the risk of bias of included studies. We decided not to blind studies for authors, institution, or journal because the review authors who assessed the risk of bias were familiar with the literature. A consensus method was used to solve disagreements and a third review author was consulted if disagreements persisted. If the article did not contain enough information to assess all the risks of bias (*i.e.*, if 1 or more criteria were scored "unclear"), the review authors contacted the study authors for additional information. The risk of bias assessment form was mailed to all study authors and they were also asked whether they agreed with the risk of bias assessment. Each criterion was assessed as "positive," "negative," or "unclear." Studies with a low risk of bias were defined as RCTs that fulfilled 5 or more of the risk of bias criteria.

Data Extraction. Pairs of review authors independently extracted data from the studies using a standardized form (RO extracted data from all studies, except from the study on which he is the first author). All pairs of review authors who extracted the data, first piloted the data extraction form by using 2 RCTs on back pain without surgery. The domains that were assessed for data extraction were characteristics of patients and interventions, and results on primary and secondary outcome measures.

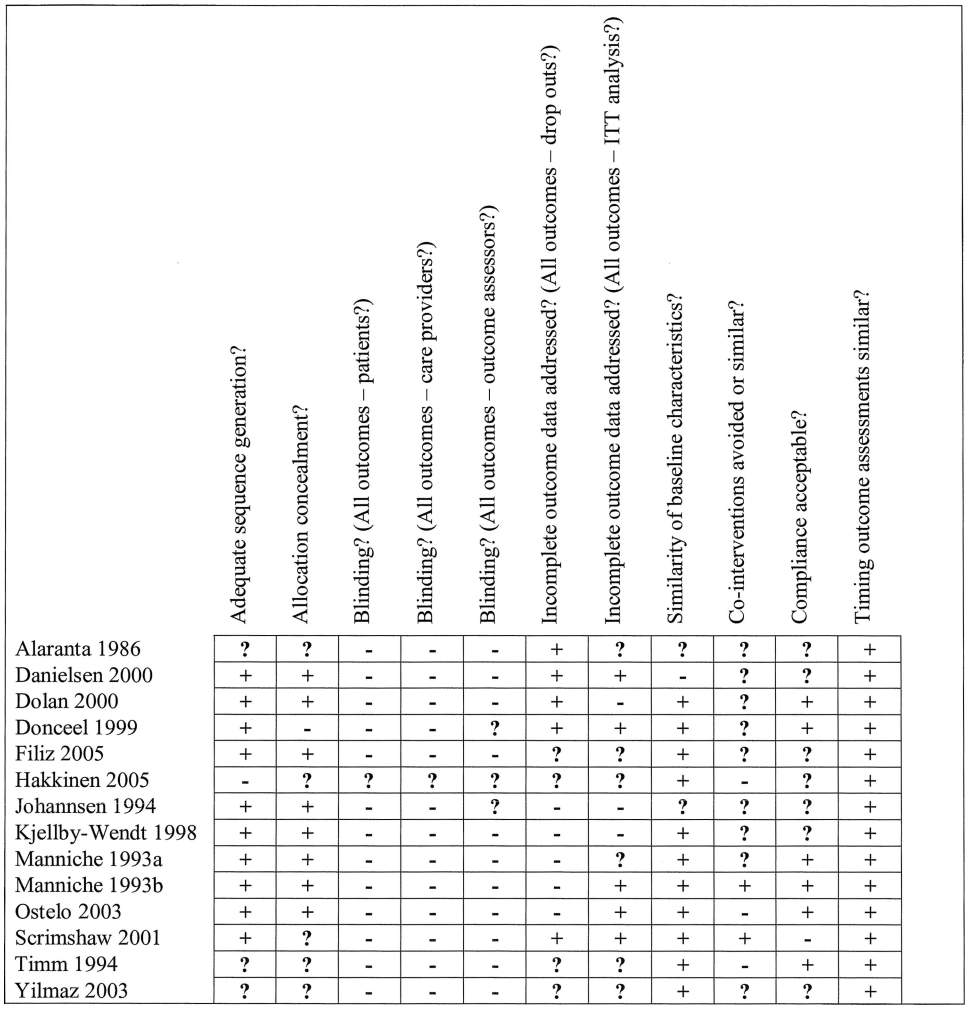
Data Analysis and the GRADE Approach. If studies were clinically homogeneous regarding study population, types of treatment and reference treatments, and outcomes and measurement instruments, a meta-analysis was performed. If possible, we calculated the WMD because this improves the interpretability of the results. If a WMD was not possible the SMD was calculated. For the comparisons where studies were too heterogeneous, no meta analysis was performed. The Editorial Board of the CBRG recommends presenting the overall quality of the evidence using the GRADE approach. The quality of the evidence on a specific outcome is based on the study design, the potential for bias, consistency of results, directness (generalizability), precision (sufficient data), and potential reporting bias for the results across all studies that measure that particular outcome. The overall quality is considered to be high when RCTs with a low risk of bias provide consistent, generalizable results for the outcome, and reduces by 1 level when 1 of the factors described above are not met.[14] In the case of only 1 study measuring an outcome, we considered the data to be "sparse" and subsequently labeled the evidence as "low quality evidence." To improve the readability of this review, a GRADE table was only completed when we completed a meta-analysis. If only 1 study was present for a given comparison, the results were described in the text.

■ Results

Study Selection

The searches from the original review (until 2000) yielded 427 studies in MEDLINE, 414 in EMBASE, and 135 in The Cochrane Library. The first selection of this part of the search was based on keywords, title, and abstract, resulting in the inclusion of 11 RCTs and 4 CCTs. After reading of the full text articles, 2 studies were excluded because 1 study evaluated intraoperative epidural corticosteroids[15] and 1 evaluated the use of parenteral Ketorolac during wound closure.[16] Neither intervention matched our definition of active rehabilitation, yielding a total of 9 RCTs and 4 CCTs for the original review. For this update, we searched the same databases plus CINAHL from 2000 until May 2007, but in line with the updated guidelines, only RCTs were included, yielding a total of 3059 hits. The first selection, based on title and abstract, resulted in 10 papers: 5 new RCTs, 1 long-term follow-up of an already included RCT[17], and 1 published protocol.[18] Reference checking yielded another long-term follow-up of an already included RCT.[17] After reading the full text articles, 1 RCT was excluded because this study was not an effectiveness study.[19] Four studies that were included in the original review were excluded because they were not randomized.[20–23] Therefore, this updated systematic review includes a total of 14 randomized controlled trials.

Description of Studies. From the 14 included studies, 2 RCTs assessed the effectiveness of programs that started immediately after surgery: 1 RCT focused on neural mobilization,[24] and 1 RCT assessed the effectiveness of intensive exercises.[17] The majority of trials focused on treatments that started 4 to 6 weeks postsurgery. Two



	Adequate sequence generation?	Allocation concealment?	Blinding? (All outcomes – patients?)	Blinding? (All outcomes – care providers?)	Blinding? (All outcomes – outcome assessors?)	Incomplete outcome data addressed? (All outcomes – drop outs?)	Incomplete outcome data addressed? (All outcomes – ITT analysis?)	Similarity of baseline characteristics?	Co-interventions avoided or similar?	Compliance acceptable?	Timing outcome assessments similar?
Alaranta 1986	?	?	-	-	-	+	?	?	?	?	+
Danielsen 2000	+	+	-	-	-	+	+	-	?	?	+
Dolan 2000	+	+	-	-	-	+	-	+	?	+	+
Donceel 1999	+	-	-	-	?	+	+	+	?	+	+
Filiz 2005	+	+	-	-	-	?	?	+	?	?	+
Hakkinen 2005	-	?	?	?	?	?	?	+	-	?	+
Johannsen 1994	+	+	-	-	?	-	-	?	?	?	+
Kjellby-Wendt 1998	+	+	-	-	-	-	-	+	?	?	+
Manniche 1993a	+	+	-	-	-	-	?	+	?	+	+
Manniche 1993b	+	+	-	-	-	-	+	+	+	+	+
Ostelo 2003	+	+	-	-	-	-	+	+	-	+	+
Scrimshaw 2001	+	?	-	-	-	+	+	+	+	-	+
Timm 1994	?	?	-	-	-	?	?	+	-	+	+
Yilmaz 2003	?	?	-	-	-	?	?	+	?	?	+

Figure 1. Summary of risk of bias.

trials[25,26] included 3 arms, 1 of which was a no treatment arm, yielding 2 comparisons per RCT. For 3 comparisons assessing the effectiveness of interventions starting 4 to 6 weeks postsurgery, a meta-analysis could be performed: exercise program *versus* no treatment (comparison 2A); high intensity programs *versus* low intensity programs (comparison 2B.1); and supervised exercise program *versus* home exercises (comparison 2B.2). For all other types of interventions (or programs) that started 4 to 6 weeks postsurgery, there was only 1 study per comparison. Finally, 2 studies assessed treatment regimens that started later than 12 months after the surgery.

Effects of Interventions. Seven of the 14 included studies were assessed as having a low risk of bias. Care providers could not be blinded due to the nature of the interventions. Because blinding of patients is also often hampered (for similar reasons), we redefined this item (as mentioned in method section). In addition, the credibility or acceptability of applied treatments was not evaluated in any study. In 6 studies, the randomization (method AND concealment) was not described adequately and in 8 studies, the compliance to the rehabilitation program was inadequate or not assessed. There was a general lack of published details concerning cointerventions: only 2 studies explicitly provided information on cointerventions. These methodologic shortcomings in the conduct and reporting of studies suggest considerable potential for bias in half of the included trials (results of individual trials are provided in Figure 1). Overall judgment of quality of evidence is provided in GRADE tables (Tables 2–4).

Effectiveness of Rehabilitation Programs

Comparisons Among Rehabilitation Programs That Start Immediately After Surgery

1A. Treatment *Versus* no Treatment, Placebo or Waiting List Control.
No trials were identified.

1B. Treatment *Versus* Other Kinds of Treatment.

One small (N = 60) RCT with a high risk of bias[17] compared an intensive exercise program consisting of increasing daily activities, home training (mobilization, trunk strengthening), and later mainly intensive muscle strengthening exercises and cardiovascular exercises with a control group that received no increasing daily activities, exercises only once a day and no promotion of

Table 2. Overall Judgement of Quality of Evidence (Grade System). Question: Should Exercise (Started 4–6 Weeks Post-Surgery) *Versus* No Treatment Be Used in Post-Lumbar Disc Surgery?

Quality Assessment							Summary of Findings			
							No. of Patients			
No. of Studies	Design	Limitations	Inconsistency	Indirectness	Imprecision	Other Considerations	Exercise	No Treatment	Effect Absoulte (95% CI)	Quality
Pain (*post treatment*)‡										
5	Randomized trial	Serious*	Serious†	No serious indirectness	No serious imprecision	None	77	45	WMD −11.1 (−18.4 to −3.8)	⊕⊕○○ Low
Functional status (*post treatment*)§										
4	Randomized trial	Serious*	No serious inconsistency	No serious indirectness	No serious imprecision	None	68	34	WMD −6.5 (−9.3 to −3.7)	⊕⊕⊕○ Moderate

*Unsure of randomization, concealment, blinding of patients, care providers, or outcome assessors, intention-to-treat analysis, co-interventions, compliance, drop-outs.
†Statistical inconsistency.
‡Measured with: VAS; 0 = no pain; range of scores: 0–100; better indicated by less.
§Measured with: Modified Oswestry Index; 0 = no disability; range of scores: 0–50; better indicated by less.

cardiovascular exercises. The results showed that there were no statistically significant differences for pain, global perceived effect, and sick leave outcomes. Only secondary analyses (leg pain on a VAS in a subgroup of patients with sciatica, and some clinical outcome measures) showed some small differences in favor of the intensive exercise program. However, these were *post hoc* analyses that do not allow firm conclusions. At the 5- to 7-year follow-up, there were no relevant differences in the number of patients that had leg pain (VAS) or back pain (VAS), or days of sick leave. There was 1 reoperation (3.4%) in the intervention group and 2 reoperations (6.5%) in the reference group. There is low quality evidence illustrating that there is no difference in the long-term for global perceived effect, pain or return-to-work between an intensive exercise program and a less active program.

1C. Specific Intervention in Addition to a Treatment Program *Versus* Treatment Alone.

One small RCT with a low risk of bias[27] (N = 59) evaluated the effectiveness of adding neural mobilization to standard postoperative care consisting of isometric and dynamic exercises (progress as tolerated). The aim of the neural mobilization was to maintain interplane mobility of the neural structures by stimulating gliding surfaces, and consisted mainly of repeated through-range straight leg raises. The published RCT[24] reports on 81 patients, but only 59 patients received a standard laminectomy, whereas the others underwent a fusion. For this review, we only analyzed the 59 laminectomy patients.[27] There is low quality evidence from this study (N = 59) that neural mobilization is not effective as an adjunct to standard postoperative care on pain (mean difference: −6.8; 95% CI: −22.2–8.6 on a 0–100 VAS) and functional status (mean difference: 4.5; 95% CI: −7.2–16.2 on 0–100 Quebec Disability Scale) after 6 weeks follow-up. For these outcome measures, as well as for overall improvement, there were also no differences after 12 months. No data were presented on reoperation rates.

Comparisons Among Rehabilitation Programs That Start 4 to 6 Weeks Postsurgery

2A. Exercise Programs *Versus* no Treatment (Table 2).

Table 3. Overall Judgement of Quality of Evidence (Grade System). Question: Should High Intensity Exercise Programs (Started 4–6 Weeks Post-Surgery) *Versus* Low Intensity Exercise Programs Be Used in Post-Lumbar Disc Surgery?

Quality Assessment							Summary of Findings			
							No. of Patients			
No. of Studies	Design	Limitations	Inconsistency	Indirectness	Imprecision	Other Considerations	High Intensity Exercise Programs	Low Intensity Exercise Programs	Effect Absoute (95% CI)	Quality
Pain (*short term*)‡										
2	Randomized trial	No serious limitations	Serious*	No serious indirectness	Serious†	None	59	44	MD −10.67 (−17.0 to −4.3)	⊕⊕○○ Low
Function (*short term*)§										
2	Randomized trial	No serious limitations	No serious inconsistency	No serious indirectness	Serious†	None	59	44	SMD −0.77 (−1.2 to −0.4)	⊕⊕⊕○ Moderate

*Statistical inconsistency.
†Sparse data.
‡Measured with: VAS; 0 = no pain; range of scores: 0–100; better indicated by less.
§Measured with: Modified Oswestry Index; 0 = no disability; range of scores: 0–50; better indicated by less.

Table 4. Overall Judgement of Quality of Evidence (Grade System). Question: Should Supervised Programs (Started 4–6 Weeks Post-Surgery) *Versus* Home Exercises Be Used in Post-Lumbar Disc Surgery?

Quality Assessment							Summary of Findings			
							No. of Patients			
No. of Studies	Design	Limitations	Inconsistency	Indirectness	Imprecision	Other Considerations	Supervised Programs	Home Exercises	Effect Absoute (95% CI)	Quality
Pain (*short term*)‡										
3	Randomized trial	Serious*	Serious†	No serious indirectness	No serious imprecision	None	45	45	SMD −1.1 (−2.8 to 0.5)	⊕⊕○○ Low
Functional status (*short term*)§										
3	Randomized trial	Serious*	Serious†	No serious indirectness	No serious imprecision	None	45	50	SMD −1.2 (−2.6 to 0.3)	⊕⊕○○ Low

*Uncertainty for all risk of bias criteria.
†Statistical inconsistency.
‡Measured with VAS; 0 = no pain; range of scores: 0–100; better indicated by less.
§Measured with Modified Oswestry Index; 0 = no disability; range of scores: 0–50; better indicated by less.

One very small (N = 20) RCT with a low risk of bias[28] and 2 small (total N = 102) RCTs with a high risk of bias[25,26] compared an exercise program with no treatment. Two studies[25,26] were 3-armed RCTs with 1 no-treatment arm and 2 exercise arms, yielding 2 comparisons for each study. Outcome was only measured at the post-treatment follow-up. For treatments that start 4 to 6 weeks postsurgery, there is low quality evidence (3 RCTs with 5 comparisons, N = 122) that exercise programs are more effective than no treatment on short-term follow-up for pain (WMD: −11.13; 95% CI: −18.44 to −3.82 on a 0–100 Pain VAS), and moderate quality evidence (2 RCTs with 4 comparisons, N = 102) in favor of exercise programs for functional status on short-term follow-up (WMD: −6.50; 95% CI: −9.26 to −3.74 on the 0 to 50 Modified Oswestry Disability Index). None of the included studies reported that these active programs increased the reoperative rate.

2B. Treatment *Versus* Other Kinds of Treatment

2B.1. High Intensity Exercise Programs *Versus* Low Intensity Exercise Programs (Table 3).

Two RCTs with a low risk of bias (total N = 159)[29,30]; and 1 small (N = 42) RCT with a high risk of bias[26] compared intensive exercise programs with mild exercise programs. There are sparse data reporting no statistically significant differences on overall improvement at short-term follow-up[30] and at 6 and 12-month follow-up.[29]

There is low quality evidence (2 RCTs, N = 103) that high intensity exercise programs are slightly more effective for pain in the short-term compared with low intensity exercise programs (WMD: −10.67; 95% CI: −17.04 to −4.30 on a 0–100 Pain VAS), and moderate quality evidence (2 RCTs, N = 103) in favor of high intensity exercise programs compared with low intensity exercise programs for functional status in the short-term (SMD: −0.77; 95% CI: −1.17 to −0.36). Long-term follow-up results for both pain and functional status were contradictory. Results for sick leave, which could not be pooled, were also contradictory: Danielsen *et al*[29] reported no significant differences in sick leave during the 1-year follow-up (high intensity: mean 18.5 weeks (SD: 14.3) *vs.* 22.0 weeks (SD: 18.6) for low intensity, while Filiz *et al*[25] reported that patients in the high intensity programs returned to work sooner (mean after 56 days, SD: 18.6) as compared with the low intensity program (mean after 75 days, SD: 24.9). Danielsen *et al*[29] reported 1-year reoperative rates that were negligible.

2B.2. Supervised Exercise Program *Versus* Home Exercises (Table 4).

Three small (total N = 142) RCTs with a high risk of bias[25,26,31] compared supervised exercise programs to home exercise programs. There are sparse data from 1 trial[31] showing no differences on global perceived effect (4-point scale) at both the post-treatment and the 3-month follow-up. There is low quality evidence (3 RCTs, N = 95) that there were no significant differences between supervised exercise program and home exercises on short-term pain relief (SMD −1.12; 95% CI: −2.77 to 0.53). There are sparse data from 1 trial[31] that there were no differences between groups on long-term pain relief. For functional status, there is low quality evidence (3 RCTs, N = 95) that there were no short-term differences between supervised exercise programs and home exercises (SMD −1.17; 95% CI: −2.63 to 0.28). For the long-term, there are only sparse data[31] reporting no significant differences between groups. One small (N = 40) trial[31] reported reoperative rates that were negligible in both groups.

2B.3. Multidisciplinary Rehabilitation Program.

Only 1 RCT with a high risk of bias (N = 212)[32] compared a multidisciplinary rehabilitation program that consisted of sessions with a physical therapist, psychiatrist, occupational therapist, psychologist, social worker and an intensive back school with usual care. There is low quality evidence that at 1-year follow-up, there were no statistically significant differences between groups for global perceived effect, sick leave or reoperative rates (3.7% in both groups).

2B.4. Rehabilitation in the Occupational Setting.

One large (N = 710) RCT with a low risk of bias[33] compared a multidisciplinary rehabilitation-oriented approach intervention, coordinated by medical advisers of a social security sickness fund for a patient population with mandatory insurance, with usual care. Because there was only 1 RCT, there is low quality evidence that suggests that a rehabilitation-oriented approach by the medical advisers of social security is more effective than usual care on return-to-work at long-term follow-up.

2B.5. Behavioral treatment.

One RCT with a low risk of bias (N = 105) compared a behavioral graded activity (BGA) program with standard physiotherapy.[34] There was low quality evidence (1 RCT only) that in the short-term there was a clinically relevant and statistically significant difference of 19% in global perceived recovery in favor of the physiotherapy program, but there were no differences on the long-term. There was also low quality evidence that there were no differences (short-term or long-term) in pain (VAS), functional status (RDQ), or return-to-work. This trial also included a cost effectiveness analysis that suggested that the behavioral program was associated with higher costs during the 1-year follow-up.

2B.6. Stretching and Strengthening Training.

One RCT with a high risk of bias[35] included 126 patients, 2 months after their first lumbar disc surgery if they were not pain free (Pain VAS, >10 mm). The intervention group received a 12-month home exercise program after 1 instruction session. Patients were instructed to stretch and perform stabilization exercises and to perform strength training (instructed to perform 2 series of exercises twice a week). The control group received identical instructions, except for the strength training. There is low quality evidence that after 12 months there were no clinically relevant or statistically significant differences in pain (VAS) and disability (Oswestry Disability Index).

2C. Specific Intervention in Addition to a Treatment Program *Versus* Treatment Alone.

No RCTs were identified.

Comparisons Among Rehabilitation Programs That Start More Than 12 Months Postsurgery

3A. Treatment *Versus* no Treatment, Placebo or Waiting List Control.

No RCTs were identified.

3B. Treatment *Versus* Other Kinds of Treatment.

One RCT with a high risk of bias[36] compared physical agents with joint manipulations, high-tech exercise, low-tech exercise and no treatment in 250 workers after first-time surgery. There is low quality evidence that low-tech and high-tech exercise might be more effective in improving low back functional status as compared to physical agents, joint manipulations, or no treatment.

3C. Specific Intervention in Addition to a Treatment Program *Versus* Treatment Alone.

One small (N = 62) RCT with a low risk of bias[37] added hyperextension to an intensive exercise program. On short-term measurement, there was a statistically significant improvement in functional status, which was no longer present at long-term follow-up. There were no reoperations. There is low quality evidence that adding hyperextension to an intensive exercise program might not be more effective than intensive exercise alone on overall improvement or functional status outcomes.

■ Discussion

Fourteen RCTs were included in this systematic review. The studies were heterogeneous with regard to timing, ranging from starting 2 days postsurgery up to more than 12 months postsurgery. The duration and intensity of the interventions also differed widely. Due to this clinical heterogeneity, statistical pooling of the results was considered appropriate in only 3 comparisons: exercise program *versus* no treatment (comparison 2A); high intensity programs *versus* low intensity programs (comparison 2B.1); and supervised exercise program *versus* home exercises (comparison 2B.2).

The effectiveness of many different rehabilitation programs has been assessed, but the majority of the programs were tested in only 1 study. The results showed that adding neural mobilization to an exercise program was not effective for pain and functional disability in the short- and long-term. For treatments that started 4 to 6 weeks postsurgery, the pooled results suggest that exercise programs lead to a faster decrease in pain as compared with no treatment, and that high intensity programs, also starting 4 to 6 weeks postsurgery, lead to a faster decrease in pain than low intensity programs. But these results should be interpreted cautiously as these are only supported by low quality evidence. However, the observation that exercise programs lead to a greater improvement in disability as compared to no treatment, and that high intensity programs, lead to a greater improvement in disability than low intensity programs is supported by moderate quality evidence. Mainly due to sparse data, the results with regard to long-term follow-up could not be pooled. None of the included studies reported that these active programs increase the reoperative rate. Because of these negligible rates, we concluded that it is not harmful to return to activity after lumbar disc surgery and consequently, that it is not necessary for patients to stay passive after lumbar disc surgery. This is in line with 1 study,[12] where the authors concluded that lifting postoperative restrictions after limited discectomy led to shortened sick leave without increased complications.

Based on pooled results, we found some evidence suggesting that supervised training is not more effective than home-based training in the short-term. But again, these results should be interpreted cautiously (only low quality

Table 5. Clinical Relevance Assessment

Study	Patients	Interventions	Relevant Outcomes	Size of Effect	Benefit and Harms
Alaranta 1986[32]	N	N	Y	N	N
Danielsen 2000[29]	Y	Y	Y	Y	Y
Dolan 2000[28]	Y	Y	Y	Y	?
Donceel 199933	Y	Y	?	Y	?
Filiz 2005[25]	Y	Y	Y	N	N
Hakinnen 2005[35]	N	Y	Y	N	N
Johanssen 1994[31]	Y	Y	Y	N	N
Kjellby-Wendt 1998[17]	Y	Y	Y	N	N
Manniche 1993[37]	Y	Y	Y	?	?
Manniche 1993[30]	Y	Y	Y	?	?
Ostelo 2003[34]	Y	Y	Y	N	N
Scrimshaw 2001[27]	Y	Y	Y	N	N
Timm 1994[36]	Y	Y	N	?	?
Yilmaz 2003[26]	Y	N	Y	Y	?

The description of each item of the table is provided below:
1. Are the patients described in detail so that you can decide whether they are comparable to those that you see in your practice?
2. Are the interventions and treatment settings described well enough so that you can provide the same for your patients?
3. Were all clinically relevant outcomes measured and reported?
4. Is the size of the effect clinically important?
5. Are the likely treatment benefits worth the potential harms?

evidence). Moreover, the compliance, both of the home exercise programs and the supervised programs, is poorly reported in these studies, further hampering an adequate interpretation. Hakkinen *et al*[35] assessed the adherence rates to home exercise programs that lasted 12 months. It was demonstrated that after 2 months, the adherence rates dropped from 50% to 60% of the target, dropping further to only 30% in the last 6 months. This seems to suggest that more intensive supervision needs to be in place for long-term rehabilitation, to maintain patients' motivation. One RCT with a low risk of bias[33] assessed an intervention of medical advisers of a social security sickness fund on a patient population with mandatory insurance. These medical advisors coordinated a multidisciplinary rehabilitation-oriented approach. The results of this study indicate that an intervention aimed at an active rehabilitation policy, encompassing gradual work resumption, information, early mobilization, and early contact with the medical adviser increased the probability of return-to-work for these patients. Although this is only 1 RCT in a specific setting (approaches like this are highly dependent on the social security system), these results look promising. The results are in agreement with the view that patients do not need to have their activities restricted and that aiming at an early reactivation is an effective approach.[12] Furthermore, this study highlights the need for more than just exercising if an intervention aims at early return to work. Further research is needed to assess whether these types of interventions are (cost-) effective.

Regarding bio-psychosocial aspects of postsurgery rehabilitation, it has been suggested that high-intensity programs confront patients with their fears and insecurities and that they learn that symptoms related to training are not necessarily dangerous.[37] In this updated review, we included a RCT with a low risk of bias that assessed the effectiveness of a BGA program, which focused on bio-psychosocial aspects.[34] The results of this study indicated that there were no differences between the BGA program and standard physiotherapy. As of yet, there seems to be no convincing evidence to use bio-psychosocial-oriented approaches in the rehabilitation of patients after first-time disc surgery. Despite that, it could be hypothesized that, as both treatment arms in this RCT were active treatment programs, the results of this RCT also show that active programs do not increase the reoperative rate or that patients need to have their activities restricted after their first lumbar surgery.

In this systematic review, all surgical techniques were included *a priori*. Patients included in the studies had all received standard discectomy or microdiscectomy. A recent systematic Cochrane review showed that there were no significant differences in effectiveness between these 2 approaches.[38,39] Therefore, it is unlikely that different surgical techniques have biased the results of this systematic review. Another important issue regarding surgery needs to be discussed. Although it was not the main focus of the current systematic review, it is important to know the indication for surgery, because indications might change over time, with potential consequences for rehabilitation. Unfortunately, the description of the indication for surgery in the included studies is scarce. In 4 RCTs (out of the 14), no description of the indication was given at all (only that there had been a first time surgery) and in 7 RCTs, the only description was that patients were operated for the first time because of a lumbar disc prolapse, without further information. In only 3 RCTs it was stated that the signs and neuromuscular symptoms and dysfunction should be elicited by a lumbar disc prolapse (or herniation) confirmed by an imaging technique. Future studies on rehabilitation should include more details on this issue in their design (Table 5).

This updated review differs in some aspects from the original review. In this update, only RCTs were included and in total 5 new studies were included. Therefore, it was now possible to pool the data in 3 comparisons. In addition, we used the GRADE approach in this update, as recommended by the Editorial Board of the CBRG, whereas in the original review the "levels of evidence" approach was used. The GRADE approach gives an overall grade of the quality of the evidence, in which the study design, risk of bias, consistency of results, directness (to the population in question), and the precision of results across all studies that measure that particular outcome are taken into account. Pooling the data and applying the GRADE approach slightly changed the underpinning quality of evidence of the results compared with the original review. In the original review, based on the "levels of evidence," we concluded that for treatments that start 4 to 6 weeks postsurgery there is strong evidence (level 1) that high intensity programs are more effective on functional status. In this updated review this is underpinned with moderate grade of evidence. This change in strength of the evidence may seem somewhat inconsistent. But in our original review, we clearly stated that although it was concluded, based on the "levels of evidence" that the evidence was "strong," this conclusion was based on only 2 studies with a low risk of bias. If results depend greatly on the system of summarizing the evidence, this means that the conclusions cannot be interpreted as absolutely convincing.

An important topic for future research is the identification of relevant subgroups. The goal of lumbar disc surgery is to relieve the leg pain of patients. Kjellby-Wendt and Styf[17] presented a positive outcome for a subgroup with residual leg pain in favor of early active training. But numbers were too small and no firm conclusion could be drawn. This raises the question of whether patients with residual leg pain should be treated differently than patients without residual leg pain. Another important topic for future trials relates to return-to-work or sick leave. Although various trials measured return-to-work or sick leave, the results of the current review should be interpreted with caution, with the exception of Donceel *et al*,[33] because this study was specifically set up in an occupational setting to improve return-to-work. One difficulty when interpreting the return-to-work (or sick leave) results is that it is often unclear how many patients were employed (or not) at baseline and if that was comparable between groups. A second difficulty relates to the method of measuring return-to-work (or sick leave), which was rarely described. Future RCTs should include appropriate measures of return-to-work. Maybe even more importantly, full economic evaluations should be performed alongside these trials to assess the cost-effectiveness and cost-utility of rehabilitation programs after lumbar disc surgery.

Although we conclude that it is not harmful to return to activity after lumbar disc surgery, and therefore, it is not necessary for patients to stay passive after lumbar disc surgery, it is still unclear what exact components should be included in rehabilitation programs. High intensity programs seem to be more effective but they could also be more expensive. Therefore cost-effectiveness analysis should be performed to assess whether intensive rehabilitation programs, if started early after surgery, lead to a reduction in costs in terms of less healthcare utilization or earlier return to work. Future research should also focus on the implementation of rehabilitation programs in daily practice. Should all patients be treated postsurgery or is a minimal intervention with the message "return to an active lifestyle" sufficient, with only patients that still have symptoms 4 to 6 weeks postsurgery requiring rehabilitation programs? The cost-effectiveness of this approach needs to be investigated. In conclusion, more research is needed on the (cost-) effectiveness of rehabilitation after first-time disc surgery.

■ Key Points

- A systematic review of randomized controlled trials was performed to assess the effectiveness of active rehabilitation programs after first time lumbar disc surgery.
- When exercises are compared with no treatment at the short-term follow-up, there is low quality evidence that they are more effective for pain and moderate evidence that they are more effective for functional status.
- When high intensity exercises are compared with low intensity exercises at the short-term follow-up, there is low quality evidence that high intensity exercises are slightly more effective for pain, and there is moderate evidence that they are more effective for functional status.
- There is low quality evidence that there were no significant differences between supervised and home exercises for short-term pain relief or functional status.

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