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Precision Exercise Prescription for Lumbar Spinal Stenosis: A Randomized Feasibility and Implementation Study with a Replicable Multimodal Protocol

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Abstract

Background: Lumbar spinal stenosis (LSS) with neurogenic claudication is associated with reduced walking tolerance, functional limitations, and impaired daily functioning. Although exercise-based rehabilitation is recommended as a first-line conservative treatment, many published interventions remain insufficiently described, limiting reproducibility and clinical application. **Objective:** To provide a detailed and replicable description of an individualized multimodal exercise and walking-extension protocol for patients with LSS, and to report key implementation outcomes. **Methods:** This single-centre randomized controlled study included patients with LSS allocated to an experimental or usual-care control group. The intervention consisted of a 12-week supervised multimodal exercise program delivered twice weekly in small groups, combined with a home-based component and a walking extension plan. Outcomes focused on feasibility, adherence, fidelity, safety, and acceptability. The primary clinical outcome of the overarching trial was the Oswestry Disability Index (ODI). **Results:** A total of 60 participants completed the study. The intervention was feasible and generally well accepted. Attendance to supervised sessions was moderate, while compliance with walking diaries was limited. The protocol was delivered largely as planned, with individualized adaptations based on symptom response and movement quality. No serious adverse events were reported. **Conclusions:** This study provides a structured and clinically applicable framework for individualized exercise prescription in LSS. The protocol demonstrates good feasibility and acceptability, although interpretation of outcomes is limited by high attrition and variability in adherence. The findings should be interpreted with caution given the feasibility design, attrition, and variability in adherence.

Keywords: neurogenic claudication; physical therapy; rehabilitation program; walking limitation; functional capacity; conservative treatment; clinical study

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1. Introduction

Walking impairment is the dominant functional complaint in many patients with lumbar spinal stenosis (LSS) and one of the main reasons for seeking care. Despite the considerable clinical burden of LSS, evidence for conservative management remains limited. Current reviews and guidelines generally support non-surgical treatment as a first-line approach, yet the certainty of evidence for many nonoperative interventions is still low.

The strongest support appears to be for multimodal care combining exercise with manual therapy, with or without education, whereas the effectiveness of other conservative approaches remains uncertain [1,2].

While exercise-based rehabilitation is widely recommended for patients with LSS and neurogenic claudication, the literature remains highly heterogeneous in terms of intervention content, dose, progression, and outcome reporting [3–5]. This heterogeneity, together with insufficient description of treatment protocols, makes it difficult to identify which intervention components are most beneficial and limits reproducibility across studies and clinical settings. At the same time, some studies have reported clinically meaningful improvements in pain, disability, and walking-related outcomes following supervised, multimodal, or individualized exercise programs [6].

Patients with neurogenic claudication do not represent a uniform clinical group. Symptom severity, walking tolerance, exercise capacity, and day-to-day variability differ substantially between individuals, suggesting that effective rehabilitation may require individualized rather than fixed exercise prescription. Therefore, advancing conservative care for LSS depends not only on evaluating outcomes, but also on providing clear, reproducible, and clinically applicable descriptions of how exercise interventions are designed, tailored, progressed, and implemented [7,8].

Despite the growing use of exercise-based interventions in patients with lumbar spinal stenosis, there remains a need for structured, clinically applicable programs that can be implemented in real-world settings. The present paper addresses the gap in detailed and replicable descriptions of individualized exercise protocols by outlining key tailoring principles, progression criteria, modification strategies, and implementation outcomes relevant to clinical practice.

The aim of this study was to evaluate the feasibility and potential effectiveness of the intervention. It was expected that participants in the intervention group would demonstrate improvements in functional outcomes and symptom severity compared to the control group.

2. Materials and Methods

This was a single-centre, prospective, randomized controlled study. After eligibility confirmation and baseline assessment, patients were randomized using Research Randomizer (version 4.0) to a control and an experimental group. Due to the nature of the intervention blinding of participants was not feasible, while outcome assessors were blinded to group allocation. Outcome assessment was performed by assessors who were not involved in the intervention and were unaware of group allocation (single-blinded assessment). Allocation was performed using a computer-based randomization procedure; however, no formal allocation concealment mechanism (e.g., sealed envelopes) was implemented, which may represent a potential source of selection bias. Participants were allowed to continue with usual care during the study period. Co-interventions were not restricted or systematically quantified. The interventions in the experimental group were conducted over 12 weeks. We conducted an intermediate assessment to track progress at week eight, and a final assessment at week twelve to evaluate outcomes.

All participants were recruited from an orthopaedic spinal department at the University Medical Centre Ljubljana between September 2024 and May 2025. Eligible participants were screened based on predefined inclusion and exclusion criteria (Table 1). Participants were recruited consecutively.

Table 1. Inclusion and exclusion criteria.

Inclusion Criteria	Exclusion Criteria
Aged between 50 and 80	Early onset of stenosis symptoms (inability to walk 250 m)
A diagnosis of LSS confirmed by magnetic resonance imaging (MRI)	Associated spinal defects (spondylolisthesis of a high degree)
Clinical symptoms associated with LSS (neurogenic claudication)	Vascular claudication
Ability to reach the gold standard of walking distance (250 m)	Diabetic polyneuropathy
Being an appropriate candidate for LSS surgery as determined by an orthopaedic surgeon	Presence of a neurological disease affecting the patient's functionality
Signed consent for participating in the study	Fibromyalgia or systemic inflammatory diseases

2.1. Participant Flow

A total of 218 patients from an orthopaedic spinal department, University Medical Centre Ljubljana were assessed for eligibility, of whom 131 were excluded (45 did not meet the inclusion criteria, 61 declined to participate, and 25 were excluded for other reasons). A total of 87 participants were randomized, with 43 allocated to the experimental group and 44 to the control group. Consequently, patients were scheduled for baseline assessment and allocated to four cohorts between September 2024 and June 2025. Participants were enrolled gradually on a rolling basis to facilitate scheduling and the organization of exercise groups. During the follow-up period, 12 participants in the experimental group and 15 in the control group were lost to follow-up due to reasons including health issues, time constraints, non-response, and failure to attend assessments.

The most common reasons for dropout included pain-related problems (LSS symptoms and other musculoskeletal conditions), other health conditions, family or work-related obligations, missed assessment appointments, and voluntary withdrawal due to fear of symptom exacerbation. Consequently, 31 participants in the experimental group and 29 in the control group completed the study and were included in the final analysis. The dropout rate was calculated based on randomized participants and was approximately 31%, with 27 of 87 randomized participants not completing the study.

A total of 60 participants were included in the final analysis (31 in the experimental group and 29 in the control group). The sex distribution was comparable between groups, with a predominance of female participants (64.5% in the experimental group and 65.5% in the control group).

The mean age was slightly higher in the experimental group (68.1 $\pm$ 5.55 years; mean BMI 30.43 $\pm$ 6.42 kg/m^2) compared to the control group (65.5 $\pm$ 8.46 years; mean BMI 29.67 $\pm$ 5.10 kg/m^2). Participants in both groups had diverse educational backgrounds, with the majority having completed secondary or higher education.

A higher proportion of participants in the experimental group were retired (90.3%) compared to the control group (55.2%), where a larger proportion remained employed.

In the experimental group, attendance averaged 16.65 sessions (SD 3.10), with a median of 16 sessions (IQR 5; range 10–22). Six participants (19.4%) attended at least 80% of the planned sessions.

Participants were given the opportunity to make up missed sessions at alternative time slots; however, for various reasons, this option was not always used. Absences were generally reported in advance. The most common reasons were symptom exacerbation on the day of training, poor weather conditions and transportation difficulties, medical appointments, other health problems or illnesses, and caregiving responsibilities for family members (Figure 1).

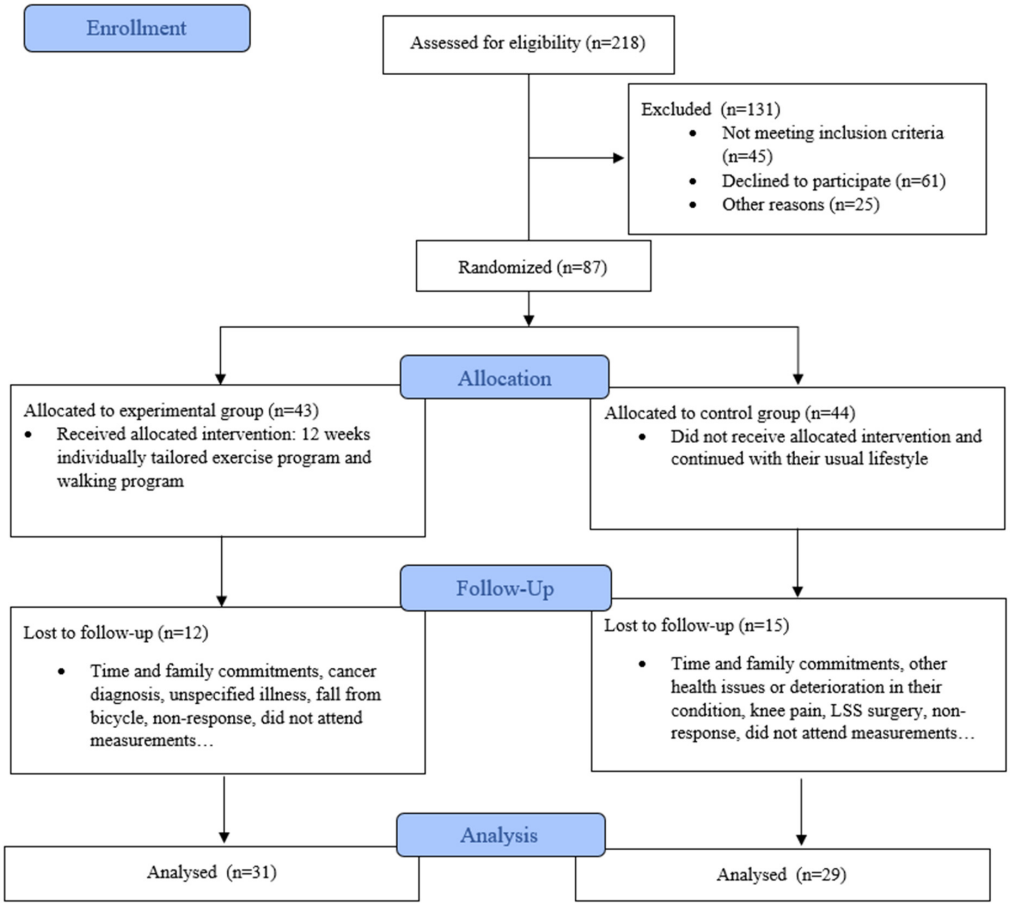


Figure 1. Consort Flow Diagram showing study enrollment.

The study was conducted in accordance with the Declaration of Helsinki and its later amendments and was approved by the Slovenian National Medical Ethics Committee (approval number: 0120-177/2023/3). All participants provided written informed consent prior to participation. The study was not prospectively registered, which represents a methodological limitation. The study was reported in accordance with the CONSORT guidelines for randomized trials (Table 2).

Table 2. TIDieR-based description of the individualized multimodal exercise and walking-extension intervention.

TIDieR Item	Description
Brief name	Individualized multimodal exercise and walking-extension protocol for lumbar spinal stenosis (LSS) with neurogenic claudication.
Why (rationale)	Intervention was designed to improve walking tolerance, functional capacity, and symptom management in patients with LSS and neurogenic claudication through a multimodal approach combining stabilization, mobility, balance/coordination, and muscular endurance training, together with progressive walking exposure. Individual tailoring was used to account for symptom variability, functional limitations, and differences in exercise tolerance.
Materials	Pilates equipment (Reformer, Wunda Chair, Spine Corrector), mats, BOSU, TRX, large exercise balls, small Pilates balls, Pilates ring, kettlebells, sticks/bars, dumbbells/free weights, elastic bands, treadmill, and step-monitoring tools (e.g., smartwatch or pedometer). Home-exercise logs and walking/step diaries were used to support self-monitoring.

Table 2. *Cont.*

TIDieR Item	Description
Procedures	The main circuit emphasized stabilization, muscular endurance, balance, and coordination. Warm up consisted of 7–15 min on a bike or treadmill walking (Borg RPE 10–14 or conversational pace). Strength/endurance exercises were typically prescribed at approximately 30–50% of estimated 1 RM, for 8–15 repetitions, progressing from one set during weeks 1–4 to up to three sets during the final 4 weeks. Rest periods were self-paced. Isometric holds were progressively increased across weeks. Balance and coordination exercises typically included 3–5 tasks performed for 10–30 s, with two repetitions and 1–2 min rest between efforts. Mobility block was based on the FAMO method. Each session included up to 8 different exercises, which were repeated to facilitate motor learning.
Who provided	The intervention was supervised and delivered by a kinesiologist. Instructions for the home-based component were explained, demonstrated, and reinforced during supervised sessions.
How (mode of delivery)	The program was delivered in person through supervised small-group sessions. The home-based component was supported by verbal and practical instruction, written guidance, where applicable, and regular review of diaries with feedback.
Where	Supervised sessions were conducted in a facility equipped with Pilates apparatus, functional training equipment, and a treadmill. Home-based exercises and walking targets were performed in the participants' home and outside.
When and how much	Participants were scheduled to complete 24 supervised sessions over 12 weeks (2 sessions/week). Daily home exercise and daily monitoring of walking and/or step counts were recommended throughout the intervention period.
Tailoring	The intervention was individualized using symptom- and performance-based decision rules. A readiness/symptom gate was applied before and during exercise selection and progression. A technique gate was used to modify or stop an exercise if movement quality deteriorated or symptoms increased beyond acceptable limits. Exercises were progressed when tasks were performed with good control and low perceived difficulty, by increasing load, repetitions, hold duration, complexity, or task challenge. Walking targets were tailored according to baseline activity level, with staged weekly progression for participants below the predefined step threshold.
Modifications	No protocol-level modifications were planned beyond prespecified individual adaptations. Any exercise or walking modifications made during delivery were symptom-driven and consistent with the tailoring algorithm.
Fidelity planned	Fidelity was planned through a predefined session structure, standardized core intervention components, and a symptom-based tailoring algorithm. Each session was intended to include a warm-up, a mobility block, and multimodal exercise circuit, with home practice and walking adherence reviewed where applicable. Consistency was supported by being delivered by the same kinesiologist, small-group supervision, predefined progression/regression rules, and attendance recording.
Fidelity actual	Actual fidelity was assessed using attendance records, diary completion, and documentation of the delivery of key intervention components, exercise modifications, stop events, and adverse events. The intervention was delivered largely as planned, with individualized adaptations applied according to symptom response, movement quality, and exercise tolerance.

2.2. Procedures

Throughout the study, testing procedures were conducted in the same facility, by the same researchers, with the same equipment at a similar time of the day. All assessments were conducted on weekends, when participants were not scheduled to attend training sessions. Participants who met all inclusion criteria and provided written informed consent

underwent baseline, intermediate, and final assessments according to a predefined protocol. Participation in the study posed a potential risk of health deterioration due to physical exertion; however, sufficient rest was provided between each physical performance test to minimise the risk. All the benefits and risks were explained to each participant prior to the enrolment in the study.

Each assessment consisted of two parts. In the first part, participants completed the Oswestry Disability Index (ODI), the Swiss Spinal Stenosis Questionnaire, and the SF-36 quality-of-life questionnaire. Demographic data and subjective pain intensity using a visual analogue scale (VAS) were also recorded, followed by measurements of body height and body mass.

In the second part, participants completed a battery of functional and physical performance tests. The 6 Min Walk Test (6 MWT) was performed first and also served as a warm-up for the subsequent assessments. Perceived exertion was assessed immediately before and after the test using the 10-point Borg Rating of Perceived Exertion scale. This was followed by the Functional Reach Test, the Single-Leg Stance Test, the Sit-and-Reach Test, and the McGill trunk endurance test battery. The order of testing was standardized across all measurement sessions. All assessments were conducted under standardized conditions and, where possible, performed by the same assessor to reduce measurement variability.

2.3. Structure and Progression of the Training Program

The program consisted of 24 exercise classes held twice weekly for 12 weeks. Each class was about 75 min in length and was led by a qualified kinesiologist (experienced in prescribing and guiding this kind of exercise). Attendance at each class was documented by the kinesiologist. During the initial four weeks, training focused on posture correction, basic movement patterns, and identifying individual physical limitations. In the subsequent eight weeks, exercise intensity and task demands were progressively increased. Exercise-induced soreness was typically delayed (24–48 h after exercise), localized to the exercised muscles, and not associated with neurological symptoms, whereas symptom aggravation reflected an immediate or activity-related increase in familiar LSS symptoms. Group-based exercise may have contributed to motivation and adherence through social interaction and peer support, although this effect was not formally assessed in the present study.

Approximately 50% of the sessions emphasized stabilization exercises with Pilates-based elements incorporated into the intervention programme. The mobility component was partly based on the FAMO fascia movement® method [9], which emphasizes dynamic mobility, fascial elasticity, and coordinated movement patterns. This approach was selected to complement the multimodal intervention by supporting movement variability and symptom-sensitive mobility, rather than relying solely on static stretching techniques.

Considerable attention was also devoted to static and dynamic balance. About 30% of the sessions targeted muscular endurance, with an emphasis on lower-limb strength, using fundamental movement patterns.

The warm-up was performed on a stationary bicycle or by walking on a treadmill and lasted 7–15 min. Warm-up intensity was prescribed using Borg's 15-point rating of perceived exertion scale (6 = very, very light exertion; 20 = very, very hard), targeting between 10 and 14, or a self-selected pace at which participants were still able to maintain a conversation.

The main part of the session consisted of strength, balance, and coordination exercises delivered as circuit training or station-based training using different pieces of equipment and Pilates devices. Each session included mostly 8 different exercises, and 1–3 exercises were always repeated from the previous session to facilitate motor learning. Training load

was set at approximately 30–50% of the estimated one-repetition maximum (1 RM). Direct 1 RM testing was not performed due to safety considerations in this older symptomatic population. Training intensity was therefore prescribed using a submaximal, symptom-guided approach, analogous to a repetition-in-reserve model, where the final repetitions were perceived as challenging but could be completed with correct technique and without symptom exacerbation.

The number of repetitions per exercise ranged from 8 to 15. We started with one set per exercise during the first 4 weeks and progressed to three sets in the final 4 weeks. If an exercise proved too difficult for a participant or could no longer be performed with proper form, the exercise was stopped and modified. Conversely, if exercises became too easy, the load was increased and/or the exercise was progressed using different equipment. Rest intervals between exercises and sets were self-paced and were not restricted. Symptom aggravation was defined as an increase in familiar LSS-related symptoms (e.g., neurogenic claudication, radiating pain), whereas expected exercise-induced soreness was characterized by delayed, localized muscular discomfort without neurological symptoms. This distinction was used to guide exercise modification or termination during supervised sessions. The intervention was delivered according to a predefined structure, with progression individually adapted based on participants' symptom response and tolerance.

Each session systematically included exercises aimed at improving hip mobility, pelvic stability, lumbar spine flexion, thoracic spine extension and thoracic mobility, as well as lower-limb strength. The main goal of every session was to increase muscular endurance. To develop muscular endurance, we used isometric contractions with a progressive increase in contraction duration. The duration of individual isometric holds was increased by 1 s each week, starting at 5 s holds with 5 repetitions in week 1, progressing to 10 s holds with 5 repetitions in week 6, and reaching 20 s holds by week 12; between these time points we progressively adjusted the number of repetitions and hold durations to maintain an appropriate training stimulus. Other strength exercises were performed with a continuous concentric phase, with emphasis on correct breathing and appropriate muscle activation.

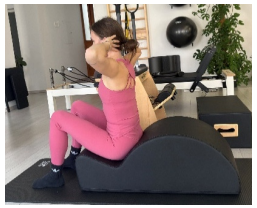
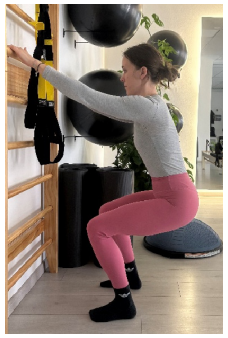
Coordination and balance exercises were designed to develop both gross motor skills (e.g., walking with changes in direction) and fine motor skills (e.g., aiming at a target). Tasks were organized as individual, repetitive, or combined tasks. The goal was for participants to perform the tasks correctly, on time, efficiently, adaptably, and reproducibly. In each session, 3–5 different tasks were performed, with as many repetitions as needed for the participant to learn the task, or until fatigue.

Balance exercises were carried out standing and walking, on reduced bases of support, with changes in surface compliance and under varying sensory conditions. The duration of individual balance tasks was set between 10 and 30 s, or until participants could no longer maintain the balance position, with two repetitions per task and 1–2 min of rest between repetitions.

To illustrate the progression framework more clearly, two representative exercises are presented below (Table 3).

Exercise progression was guided by predefined criteria based on movement quality, symptom response, and task tolerance. For example, the squat exercise was progressed from a supported version using wall bars, to a TRX-assisted squat, and finally to a squat performed on a BOSU. This progression was intended to increase lower-limb strength, coordination, postural control, and dynamic balance by moving from greater external support and stability toward greater loading and balance demands. Progression was introduced only when the participant demonstrated adequate technique, symptom control, and confidence in movement. If symptoms increased, trunk or knee alignment deteriorated, or balance became unsafe, the exercise was regressed or stopped.

Table 3. Exercise Progression Framework.

	Exercise	Targeted Function	Initial Version	Progression 1	Progression 2	Progression Criteria	Modification/Stop Rule
1.	Spine flexion »	Trunk control, segmental flexion, and lumbopelvic coordination				Progress when task is completed with good pelvic control and no symptom aggravation	Modify or stop if lumbar pain increases or pelvic control is lost
2.	Squat	Lower-limb strength, postural control, coordination, and dynamic balance				Progress when the participant demonstrates adequate technique, symptom control, and confidence in movement	Regress if symptoms increase, trunk or knee alignment deteriorates, or balance is compromised

A similar principle was applied to the spine flexion ("C curve") exercises, which were used to improve trunk control, segmental spinal flexion, and lumbopelvic coordination. The exercise was initially introduced in a simpler, more supported form with reduced range and explicit cueing, and was then progressed to more controlled and functionally demanding versions as participants improved. Progression was allowed when the task could be completed with good pelvic control and without symptom aggravation. If lumbar pain increased or pelvic control was lost, the exercise was modified or discontinued. Together, these examples illustrate the symptom-sensitive and individualized progression model that underpinned the intervention protocol.

2.4. Home-Based Exercise and Walking Extension Plan

Participants were gradually educated and supervised throughout the intervention to support the independent and individualized performance of selected exercises in the home setting. Exercises deemed appropriate for home practice were demonstrated and explained by the kinesiologist during supervised sessions, and participants were encouraged to note or record them to facilitate accurate execution at home.

Participants were instructed to complete the home exercises once daily and to document the exercises performed, including the number of sets and repetitions, in an exercise diary. If prescribed exercises were not completed, they were asked to record the reasons. The amount of home exercise performed was not recorded separately for each participant. Home practice was reviewed at the following supervised session, and additional explanation was provided when necessary.

Participants in the experimental group were asked to record adherence to the walking-extension component using a daily diary provided at the first supervised session. Participants who owned a smartwatch used it to monitor walking distance, whereas those without such devices used pedometers to track daily step counts. Diaries were reviewed after the first week to establish baseline walking activity. Based on step-based physical activity recommendations for older adults, participants whose average daily step count was below 7500 steps were assigned to an additional walking extension component. Evidence suggests that approximately 7000–10,000 steps/day is a reasonable reference range for older adults, with 7000 steps/day representing a pragmatic and achievable target for many individuals.

During weeks 1–2, these participants were encouraged to increase their baseline step count by 500 steps on at least 3 days per week. During weeks 3–4, the same increment was targeted on at least 5 days per week. During weeks 5–6, the target was increased to 1500 steps above baseline on at least 3 days per week, and during weeks 7–12, to 1500 steps above baseline on at least 5 days per week. Participants also received practical recommendations regarding the timing and location of walking, as well as encouragement to walk with a companion when appropriate to enhance adherence and motivation. At the end of the intervention, they reported whether the prescribed targets had been achieved and, if not, the reasons for not meeting them on specific days. They also documented the onset of neurogenic claudication symptoms and whether another form of aerobic activity had been performed instead of walking. This step-based self-monitoring approach is consistent with the growing use of daily step counts and walking performance metrics in lumbar spinal stenosis research and with broader physical activity guidance emphasizing achievable, progressive increases in ambulatory activity.

The walking-extension component was informed by step-based physical activity recommendations for older adults and by the use of daily step counts as a functional walking metric in individuals with lumbar spinal stenosis. Progressive targets were therefore prescribed using individualized increments above baseline walking activity [10,11].

2.5. Control Group

The control group continued their usual lifestyle and symptom-management strategies for lumbar spinal stenosis (LSS). Some participants received physiotherapy within the public healthcare system according to the standard clinical practice. They had also been advised by an orthopedic specialist to seek additional conservative care, including manual therapy, trunk-stabilizing exercises, and aerobic activity within their individual capacity.

Participants in the control group were not enrolled in the exercise intervention and were not introduced to the walking-extension program. They were not restricted from seeking or receiving additional physiotherapy or other conservative care during the study period; any such co-interventions were allowed as part of usual care and were not standardized within the study protocol.

All participants were offered individual consultations, during which medical records and imaging findings were reviewed, general recommendations were provided, and selected lumbar unloading exercises were demonstrated.

2.6. Implementation Outcomes

As the present paper focuses on intervention description and implementation rather than clinical effectiveness, these outcomes were used to characterize the practical delivery of the program in a real-world rehabilitation setting.

Feasibility referred to the ability to deliver the planned 12-week intervention within the available organizational and clinical conditions. This included the implementation of twice-weekly supervised small-group sessions, delivery of the home-based exercise component, and use of the walking-extension plan.

Adherence was assessed by attendance at supervised sessions and by completion of the home-exercise and walking diaries. Where available, adherence to prescribed walking targets was also considered. Home-based exercise volume was reviewed through diary use and participant feedback, although it was not quantified separately for each participant.

Fidelity referred to the extent to which the intervention was delivered as intended. This included the delivery of predefined core session components, namely warm-up, mobility exercises, and the multimodal main exercise circuit, as well as the application of the planned session structure and tailoring principles. In this study, fidelity was defined as consistency in delivering the core elements of the intervention rather than identical exercise prescription across all participants.

Adaptations included symptom- or performance-based modifications made during program delivery. These involved changes in exercise position, range of motion, load, complexity, volume, or rest duration in response to symptom behavior, movement quality, or exercise tolerance. When applied according to the predefined tailoring rules, such adaptations were considered an integral part of the individualized intervention rather than protocol deviations.

Safety was monitored through documentation of adverse events, symptom aggravation, and exercise cessation or modification events that occurred during supervised sessions or during home practice were reported.

Acceptability referred to participants' willingness and ability to engage with the intervention. This was considered in relation to attendance at supervised sessions, participation in home-based exercise, completion of diaries, and engagement with the walking-extension component throughout the intervention period.

To improve clarity, key implementation outcomes are summarized as follows: feasibility of the intervention was achieved, adherence to supervised sessions was moderate, fidelity of delivery was generally high, safety was confirmed with no serious adverse events,

and acceptability was overall good, although variability in adherence to home-based and walking components was observed (Table 4).

Table 4. Summary of implementation outcomes.

Outcome	Summary
Feasibility	The 12-week supervised program was delivered within available clinical and organizational conditions.
Adherence	Attendance to supervised sessions was moderate; adherence to home-based and walking-extension components was variable.
Fidelity	Core intervention components were delivered largely as planned, with individualized adaptations according to symptom response and movement quality.
Safety	No serious adverse events were reported.
Acceptability	The intervention was generally well accepted, although diary completion and walking-extension monitoring were limited.

2.7. Statistical Methods Including Sample Size Calculation

The primary outcome of the study was assessed using the Oswestry Disability Index (ODI, version 2.1), a widely used measure of back pain-related disability scored from 0 to 100, with higher scores indicating greater disability [12]. Sample size calculation was based on the expected between-group difference in change in ODI score, using a two-sample *t*-test for independent groups, with 80% power, a significance level of 0.05, and a beta error of 0.20. A clinically important mean difference of 20 points and a standard deviation of 28.6 were assumed [3]. The estimated total sample size was 76 participants, including an anticipated 20% dropout rate, corresponding to 38 participants per group. Data were formatted using Microsoft Office Professional Plus 2019 (Microsoft Corporation, Redmond, WA, USA) and statistically analyzed using IBM SPSS Statistics, version 26 (IBM Corp., Armonk, NY, USA).

Data distribution was assessed using the Shapiro–Wilk test prior to analysis. Depending on distribution, appropriate parametric or non-parametric tests were applied. Within-group differences across measurement time points in the control and experimental groups were analyzed using the paired-samples *t*-test. Between-group differences over time were analyzed using repeated measures ANOVA to evaluate the effect of the intervention at baseline, 8 weeks, and 12 weeks. In cases where the assumption of sphericity was violated, the Greenhouse–Geisser correction was applied. When the data were not normally distributed, the non-parametric Friedman test was applied, followed by post hoc pairwise comparisons using the Wilcoxon signed-rank test with Bonferroni correction. For selected outcome measures, a 30% improvement was used as an indicator of clinically meaningful change.

Effect sizes in the ANOVA were estimated using partial eta-squared (ηp^2). Following Cohen's guidelines, ηp^2 values of approximately 0.01 were interpreted as small, 0.06 as medium, and $\geq$0.14 as large. A 30% improvement was used as a threshold for clinically meaningful change, as this level is commonly considered indicative of minimal clinically important difference in musculoskeletal and pain-related outcomes [13,14]. The final analysis was conducted on participants who completed the intervention (per-protocol analysis). The level of statistical significance was set at $p < 0.05$.

3. Results

The Walking Extension Program

The walking diary was introduced with the intention of monitoring daily walking activity as accurately as possible and supporting the individualized walking-extension component of the intervention. Nevertheless, compliance with diary completion was

limited, reducing the ability to analyse the walking extension component. The mean number of recorded steps increased significantly between the first and final week ($p < 0.05$).

The walking diary was structured into two parts. During the first week, participants recorded their usual walking activity, including the date, day of the week, time of day, walking distance, and/or daily step count, in order to establish baseline ambulatory activity before progression was introduced.

In the subsequent phase, the diary was used as part of a 12-week walking extension program, in which participants documented daily step counts and whether they had achieved the prescribed walking target.

Although 19 of 31 (61.29%) participants in the experimental group were identified at baseline as not reaching the recommended average of 7500 steps per day, 14 of 19 (73.68%) returned completed step diaries at the end of the program. This limited the available data for analysis of the walking extension component.

Daily step count increased from a mean of 3705.10 steps (SD 2185.22; median 3016.50, IQR 3675.93) before the intervention, to 6237.26 steps (SD 3133.47; median 5766.07, IQR 5009.25) after the intervention.

Overall, the results suggest a trend toward improvement in functional outcomes and symptom severity in the intervention group compared to the control group. The most pronounced changes were observed in measures of walking capacity and functional performance (Table 5).

Table 5. Changes in primary outcomes over time.

Test	Group	Baseline (μ ± SD)	8 Weeks (μ ± SD)	12 Weeks (μ ± SD)	Δ (Baseline-12 Weeks)	*p* (Baseline-8 Weeks)	*p* (Baseline-12 Weeks)	*p* (8–12 Weeks)
6 MWT	EKS (n = 30)	390.93 ± 108.22	472.07 ± 91.48	496.02 ± 93.79	+105.09 m (26.9%)	**0.001**	**<0.001**	0.070
	CON (n = 27)	382.44 ± 125.57	379.64 ± 85.56	393.20 ± 99.89	+10.76 m (2.8%)	0.952	0.418	0.051
ODI	EKS (n = 31)	35.41 ± 12.37	28.82 ± 10.99	25.67 ± 9.66	−9.74 (27.5%)	**<0.001**	**<0.001**	**0.024**
	CON (n = 29)	44.15 ± 14.27	40.36 ± 18.36	41.45 ± 20.38	−2.7 (6.1%)	**0.026**	0.227	0.600

Note: Values are presented as mean ± standard deviation. Δ indicates the change from baseline to 12 weeks. 6 MWT = 6 Min Walk Test; ODI = Oswestry Disability Index; EKS = experimental group; CON = control group. Within-group differences were analyzed using paired tests. Statistical significance was set at $p < 0.05$.

In the experimental group, improvements in the 6 min walk test (6 MWT) were observed over time, with statistically significant changes between baseline and 8 weeks ($p = 0.001$) and between baseline and 12 weeks ($p < 0.001$), while no significant change was observed between 8 and 12 weeks. In the control group, no statistically significant changes were observed across time points.

For the 6 min walk test (6 MWT), repeated-measures ANOVA with Greenhouse–Geisser correction ($\varepsilon = 0.638$) showed a significant main effect of time ($F(1.28,70.15) = 15.89$, $p < 0.001$, $\eta p^2 = 0.224$) and a significant time × group interaction ($F(1.28,70.15) = 11.09$, $p = 0.001$, $\eta p^2 = 0.168$), indicating a different pattern of change between groups. A significant main effect of group was also found ($p = 0.005$), with higher walking distances in the experimental group. The experimental group increased walking distance by 105.09 m, whereas the control group showed minimal change (10.76 m).

For the Oswestry Disability Index (ODI), the experimental group showed a consistent reduction in disability across all time comparisons, whereas the control group demonstrated a statistically significant improvement only between baseline and 8 weeks, with no further changes thereafter. The Oswestry Disability Index (ODI) showed a significant main effect of time ($F(2,116) = 14.61$, $p < 0.001$, $\eta p^2 = 0.20$) and a significant time × group interaction ($F(2,116) = 4.13$, $p = 0.019$, $\eta p^2 = 0.07$), indicating a greater reduction in disability in the

experimental group compared to the control group. A significant main effect of group was also observed ($F(1,58) = 11.60$, $p = 0.001$, $\eta p^2 = 0.17$), with consistently lower ODI values in the experimental group. The mean reduction in ODI in the experimental group was 9.74 points.

These findings indicate a pattern of improvement in the experimental group; however, given the study design and limitations, results should be interpreted with caution.

4. Discussion

The present study evaluated the feasibility and potential effectiveness of a structured, multimodal exercise program in patients with lumbar spinal stenosis. Participation in the intervention was associated with improvements in functional outcomes and symptom severity compared to the control condition. However, these findings should be interpreted cautiously, as the study was not designed to establish definitive clinical effectiveness, but rather to explore feasibility and implementation in a real-world context. The study design reflects a pragmatic approach, focusing on implementation in a real-world clinical setting rather than controlled efficacy conditions.

The intervention was deliberately designed to reflect routine rehabilitation practice, combining supervised sessions, individualized progression, home-based exercise, and a walking extension component. This is clinically relevant, as ambulation is central to the symptom experience of neurogenic claudication and represents a key functional outcome in LSS [15]. The protocol aimed to extend rehabilitation beyond supervised sessions and support functional adaptation in daily life, aligning with current recommendations that conservative management should address function, symptom behavior, and participation rather than pain alone [1,16].

The intervention was based on the premise that rehabilitation in LSS should be individualized, symptom-sensitive, and progressively dosed. In this population, excessive or poorly tailored loading may reduce adherence and effectiveness. The combination of multimodal exercise and walking extension was therefore intended to address the multidimensional impairments commonly observed in LSS, including reduced walking capacity, impaired muscular endurance, altered balance, and limited mobility [17,18]. This approach is consistent with current evidence suggesting that effective exercise interventions in LSS are typically multimodal, supervised, and functionally oriented [19,20].

The observed improvements may be explained by several complementary mechanisms. Flexion-based positions may increase the cross-sectional area of the spinal canal and reduce neural compression. Improvements in muscular endurance may delay fatigue during walking, while repeated exposure to symptom-limited activity may enhance tolerance and reduce symptom sensitivity over time [17,21].

The results are generally consistent with previous literature supporting the use of exercise-based interventions in this population [3,22,23]. However, the present study extends existing knowledge by focusing on a structured and clinically applicable program designed for implementation in routine practice [6,24]. This pragmatic approach reflects real-world rehabilitation settings, where interventions are often delivered alongside usual care and adapted to individual patient needs. From a clinical perspective, these findings suggest that a structured, supervised, and multimodal exercise program may represent a feasible and potentially beneficial conservative treatment option for patients with lumbar spinal stenosis. The group-based format and individualized progression may further support adherence and engagement, although these aspects were not formally assessed [18,25].

Several important methodological limitations of this study should be explicitly acknowledged, as they may influence the interpretation of the findings. First, the study was

not prospectively registered, which represents an important methodological limitation and may affect transparency and reproducibility. Second, the relatively high attrition rate represents an important limitation. Although the required sample size was achieved among participants who completed the intervention, attrition may introduce bias when those who discontinue differ systematically from those who remain. In the present study, it is plausible that individuals with greater symptom severity, lower exercise tolerance, or lower motivation were more likely to withdraw. Consequently, the final sample may represent a subgroup of participants more responsive to the intervention, potentially leading to an overestimation of treatment effects.

A formal comparison between completers and non-completers was not performed, which further limits the ability to determine the direction and magnitude of this potential bias. Attrition bias is a well-recognized threat to internal validity in randomized controlled trials, particularly when dropout is substantial or related to outcomes [26]. Missing outcome data were not imputed, and therefore the results should be interpreted with caution, particularly given the high attrition rate.

In addition, the study was not prospectively registered, which represents an important methodological limitation and may affect transparency and reproducibility.

Second, the analysis was conducted using a per-protocol approach, which may further increase the risk of bias compared to an intention-to-treat analysis; however, a full ITT approach was not feasible due to the extent of missing outcome data. The use of a per-protocol analysis limits the internal validity of the study and may result in an overestimation of treatment effects. Therefore, the findings may be a feasible and potentially beneficial approach in a real-world setting rather than efficacy under controlled conditions.

Third, participants in the control group were allowed to continue usual care, including physiotherapy and other conservative treatments, which may have introduced confounding and reduced the interpretability of between-group comparisons.

In addition, the intervention was based on the premise that LSS rehabilitation should be individualized and symptom-sensitive. Methodological limitations include a high attrition rate, which may introduce bias if those who discontinued differed systematically from those who remained. Furthermore, the lack of an equal-attention control group may have contributed to a Hawthorne effect.

Group-based exercise may have contributed to motivation and adherence through social interaction and peer support, although this effect was not formally assessed in the present study. Co-interventions in the control group were allowed but not systematically recorded, which represents a potential source of confounding and limits the interpretation of between-group differences. This limitation should be considered when interpreting the observed effects.

Fourth, although allocation was randomized, formal allocation concealment procedures were not implemented, which may have introduced a risk of selection bias. While participant blinding was not feasible, the use of blinded outcome assessors and standardized testing procedures helped reduce the risk of measurement bias.

Adherence to the walking-extension component was limited due to incomplete diary returns, which substantially restricts the interpretability of this component. Therefore, no firm conclusions can be drawn regarding its independent contribution to the observed outcomes. Future studies should consider more objective monitoring strategies, such as wearable devices, automated step-count recording, or regular reminders, to improve adherence monitoring and reduce missing data.

Taken together, these methodological limitations suggest that the findings should be interpreted as reflecting the effectiveness of the intervention within a real-world clinical context rather than its isolated efficacy under controlled conditions.

The FAMO-based mobility component was included as a pragmatic approach to structuring mobility exercises, based on its compatibility with the multimodal, movement-oriented design of the program and the expertise of the therapist, and was not selected based on evidence of superiority over conventional mobility or stretching approaches.

Future research should aim to confirm these findings using more rigorous designs, including improved retention strategies, implementation of allocation concealment, systematic monitoring of co-interventions, and the use of intention-to-treat analyses. In addition, further investigation of adherence and dose–response relationships would help clarify the mechanisms and optimize intervention delivery. This introduces uncertainty regarding the extent to which observed between-group differences can be attributed solely to the intervention.

In conclusion, the present study suggests that a structured, multimodal exercise program may be a feasible and clinically relevant approach for improving function and symptoms in patients with lumbar spinal stenosis. However, given the methodological limitations, these findings should be interpreted cautiously given the feasibility design, attrition, and per-protocol analysis.

5. Conclusions

The findings of this study suggest that a structured, multimodal exercise program may be a feasible and clinically applicable approach in patients with lumbar spinal stenosis, primarily from an implementation perspective rather than definitive clinical effectiveness. However, these results should be interpreted with caution due to several methodological limitations, including a high attrition rate, the use of a per-protocol analysis, lack of allocation concealment, and limited control of co-interventions. As a result, the observed effects may overestimate the true impact of the intervention.

Within these constraints, the study provides preliminary insights into the feasibility and practical application of structured exercise-based programs in real-world settings. Future research using more rigorous randomized controlled designs, including intention-to-treat analyses and improved methodological control, is needed to confirm these findings.

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Abbreviations

The following abbreviations are used in this manuscript:

CV	Coefficient of Variation
DOMS	Delayed Onset Muscle Soreness
EKS	Experimental Group
CON	Control Group
LBP	Low Back Pain
LSS	Lumbar Spinal Stenosis
MCID	Minimal Clinically Important Difference
ODI	Oswestry Disability Index
RCT	Randomized Controlled Trial
SPWT	Self-Paced Walking Test

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