Volume 3, Number 3, 2026

Pages 965-980

Bioscience Insights

ISSN: 3080-3489 (Print)
ISSN: 3080-3497 (Online)

BiosciInsights.com

# CRITERIA-BASED REACTIVE BALANCE TRAINING ADDED TO STANDARD REHABILITATION AFTER MULTILIGAMENT KNEE RECONSTRUCTION: A MULTICENTRE PILOT RANDOMIZED TRIAL

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https://doi.org/10.5281/zenodo.23726627

**Article Information**

***Article History***

| | |
|---|---|
| Received: | June 19, 2026 |
| Revised: | August 19, 2026 |
| Accepted: | September 07, 2026 |
| Available Online: | October 28, 2026 |

**Keywords:**

*Multiligament knee injury; knee reconstruction; reactive balance; perturbation; rehabilitation; feasibility; pilot randomized trial; knee-related quality of life*

**Abstract**

***Background:*** *Multiligament knee reconstruction requires rehabilitation that restores function while protecting repaired structures.*

***Objective:*** *To evaluate the feasibility and exploratory outcomes of adding guarded reactive balance training to matched-duration conventional rehabilitation.*

***Methods:*** *An assessor-blinded multicentre pilot randomized design specified 48 adults allocated 1:1 to common rehabilitation with either reactive balance or conventional exercise for 24 sessions over eight weeks. Outcomes were collected at baseline, eight weeks and 24 weeks. We analyzed the supplied Version 3 database descriptively and using baseline- and centre-adjusted linear regression.*

***Results:*** *The database lists 347 screened and 48 randomized participants (24 per group); week-eight clinic attendance was 46/48 (95.8%), KOOS completeness 45/48 (93.8%), adequate session attendance 43/48 (89.6%) and fidelity 649/695 (93.4%). At eight weeks, KOOS quality-of-life scores averaged 70.10 versus 60.35 in groups 1 and 2, with adjusted group 1 minus group 2 difference 10.37 points (95% CI 5.92–14.82). There were 16 recorded adverse-event entries.*

***Interpretation:*** *These are reproducible electronic database estimates, not independently authenticated clinical trial results: the*

*supplied archive contains a stochastic patient-record generator and is labelled as training material. Original-source reconciliation, ethics approval and prospective registration evidence are required before submission as completed clinical research.*

## INTRODUCTION

Multiligament knee injury refers to damage involving two or more major ligament complexes that stabilize the knee. Such injuries present a difficult rehabilitation problem because management must address several planes of instability while protecting repaired or reconstructed tissue. Surgical patterns vary substantially: the anterior cruciate ligament, posterior cruciate ligament, medial structures, and posterolateral complex may be injured in different combinations, with associated meniscal, chondral, fracture, vascular, and neural injuries. Patients recovering from reconstruction therefore cannot be assumed to share a single safe exercise trajectory. Even when two patients undergo procedures with similar names, graft selection, injury chronicity, staged surgery, weight-bearing restrictions, and concurrent repair can result in different postoperative requirements. An intervention suitable for this population must accommodate such variation explicitly, rather than treating it as noise [1–3].

Recovery after multiligament knee reconstruction is more than a return to an unrestricted range of motion. Patients may need to relearn weight acceptance, manage episodes of perceived instability, rebuild confidence during walking, and resume work or family responsibilities without exposing healing structures to unnecessary load. These tasks depend on strength, joint motion, sensory information, motor coordination, anticipatory postural preparation, and the capacity to react when equilibrium is disturbed. Conventional rehabilitation appropriately prioritizes protection and progressive strengthening, but the transition from predictable therapeutic tasks to unexpected demands encountered during everyday mobility is not always described in sufficient detail. A step around an obstacle, an accidental shoulder bump, or a small stumble may require an immediate adjustment not captured by a planned balance stance alone [1,4].

The long-term burden of these injuries provides a reason to improve the precision of rehabilitation research without exaggerating the promise of any individual technique. A recent systematic review and meta-analysis found that functional outcomes after multiligament injury may deteriorate over longer follow-up periods, whereas a long-term reconstruction cohort described substantial residual limitations in some patients [2,5]. Such observations do not establish that later reactive balance training can alter long-term degenerative processes. They indicate that immediate surgical stability should not be treated as synonymous with complete recovery, and that functional and patient-reported outcomes deserve prospective measurement. Variation between published studies in injury definition, surgical approach, rehabilitation restrictions, and outcome instruments also complicates comparisons [2,3].

Recent international consensus work has encouraged clearer nomenclature and more systematic treatment and rehabilitation descriptions for multiligament knee injuries [3]. The same standard should apply to controlled postoperative exercise trials. An apparently simple term such as balance training can describe widely different practices, including static single-leg stance, planned reaching, stepping to targets, gait tasks, or external disturbances. These tasks differ in what the patient knows before movement, how a loss of balance is elicited, and what physical demands are imposed on a reconstructed knee. Reproducibility requires authors to define the intervention's content, progression, contraindications, therapist expertise, monitoring, and decisions about when to hold or regress treatment. Such specificity is especially important when a comparison arm receives the same amount of professional attention.

Reactive balance training uses a controlled disturbance to elicit a corrective response. The conceptual target is the ability to regain stable posture, sometimes by adjusting the trunk or making a protective step, when the timing or direction of the disturbance is not fully predictable. Evidence from anterior cruciate ligament rehabilitation and neuromuscular training offers a basis for considering this approach, but it cannot be applied automatically to patients with multiple reconstructed ligament complexes. A published case report involving an ACL graft and posterolateral injury is an example of clinical feasibility, not comparative proof of benefit or safety [6]. Another pilot investigation of visual perturbation training used

a different population and exercise exposure [7]. The intervention in the present protocol therefore uses conservative, surgeon-authorized disturbances and treats safety as an empirical feasibility question.

This distinction matters because uncontrolled twisting, forced hyperextension, abrupt support release, and high-demand protective steps may be unsuitable during recovery from complex ligament surgery. A postoperative patient may appear capable of standing on one leg yet remain subject to specific restrictions related to posterior or collateral reconstruction. The present protocol was consequently designed around written surgical clearance, specified entry criteria, close therapist guarding, and gradual progression. The planned reactive module is not an exercise challenge that participants must complete at all costs. Inability to progress while maintaining safe alignment and symptoms is a legitimate outcome of the feasibility study, and an important finding for refining a future trial.

Most rehabilitation trials emphasize short-term clinical improvement, but an early investigation in a relatively uncommon condition must first establish whether an interpretable controlled experiment can be delivered. Recruitment to multiligament knee research may be slow; individuals may be treated across multiple surgical and rehabilitation services, and patients who are theoretically eligible can remain temporarily ineligible because of surgical restrictions, swelling, instability, or functional limitations. A feasibility-oriented design assesses how many patients can actually be approached, randomized, retained, treated and measured using the intended procedures. It can also reveal whether additional intervention time, therapist training, or separate assessor staffing makes the trial impractical under normal service conditions [8,9].

The Karachi setting introduces further questions about implementation rather than assumptions about the performance of individual institutions. Rehabilitation visits may be affected by travel time, direct transport costs, work demands, caregiving responsibilities, and coordination between orthopaedic and physiotherapy teams. Public and private pathways may differ in referral mechanisms and documentation, although such differences must be measured rather than presumed. A multicentre external pilot can describe how the same protocol behaves across these pathways and identify operational changes needed before a definitive trial. This is not a claim that findings will be representative of all patients in Karachi or Pakistan; instead, it is a transparent evaluation of whether a larger randomized comparison can be mounted.

The proposed trial compares two equal-duration interventions. Participants in both groups receive a shared rehabilitation core, and the final 20 minutes of each supervised session differ according to allocation. In the experimental group, the added module involves guarded reactive balance disturbances within restrictions. In the comparison group, the same amount of time is spent on conventional planned balance, mobility, and functional exercises. The design therefore asks whether reactive content can be incorporated with acceptable delivery, attendance, and assessment completion. It does not compare 65 minutes of therapy with 45 minutes, nor does it justify any assertion that extra therapist contact is beneficial. The contribution lies in creating a specific treatment contrast for future evaluation.

Figure 1 presents the planned recruitment, allocation, delivery and follow-up sequence.

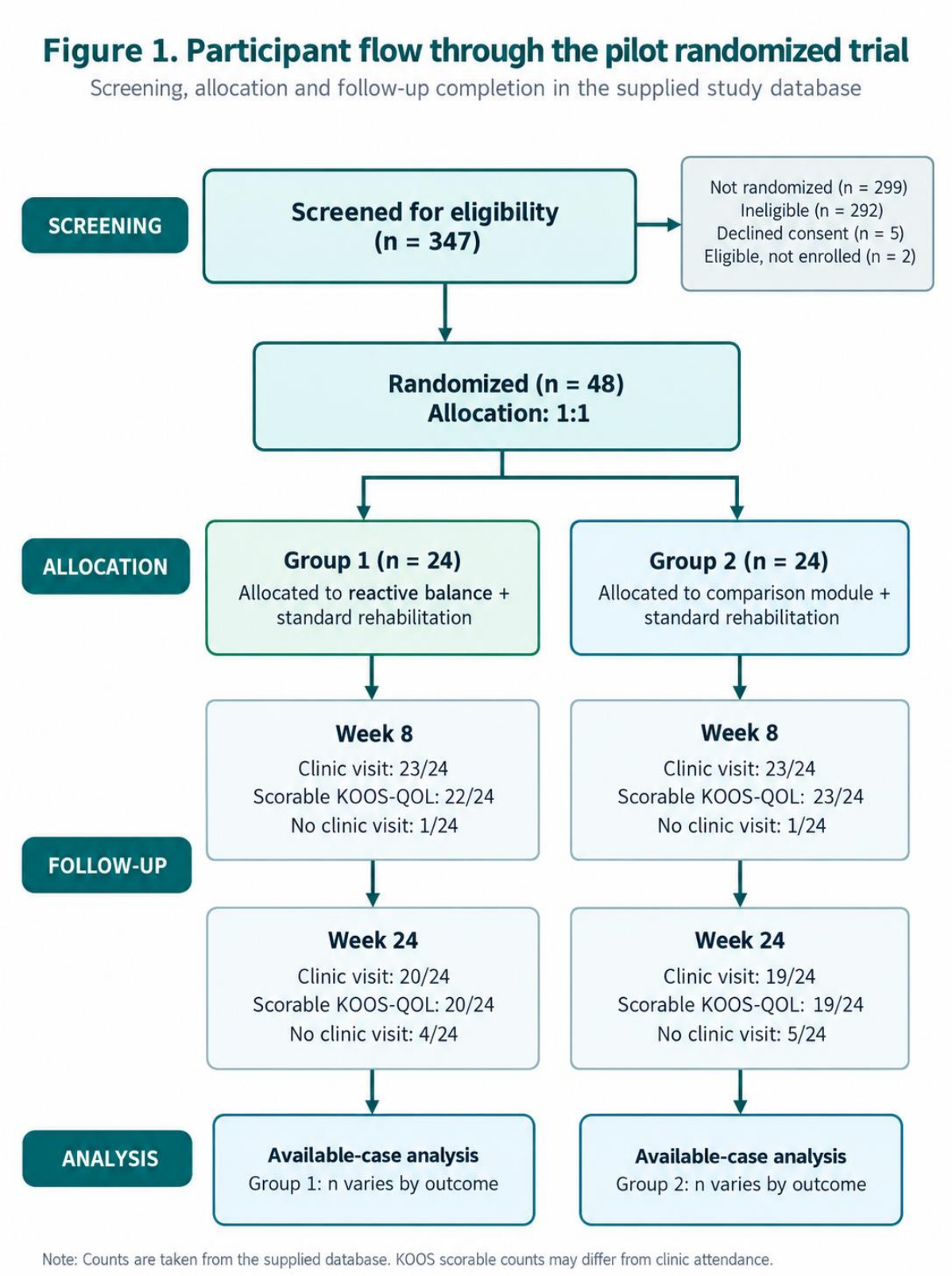



**Figure 1. Participant flow through screening, randomization and follow-up. Counts are displayed by coded group; KOOS completeness is reported separately from clinic attendance.**

The primary objective is to estimate recruitment, retention, intervention exposure, delivery fidelity, and questionnaire completeness against prespecified progression criteria. Secondary objectives are to

characterize adverse events, barriers to participation, implementation requirements, and exploratory changes in knee-related quality of life, pain, function, balance, gait, and motion. Any between-group clinical contrasts in a 48-person pilot should be reported with their uncertainty and interpreted as estimates for future planning. This methodological distinction is consistent with current guidance on randomised trial protocols and reporting, including the updated SPIRIT and CONSORT statements [8,9]. The report does not infer efficacy from short-term improvement or the presence or absence of conventional statistical significance.

**Figure 2 distinguishes feasibility endpoints from exploratory clinical measures and safety processes.**

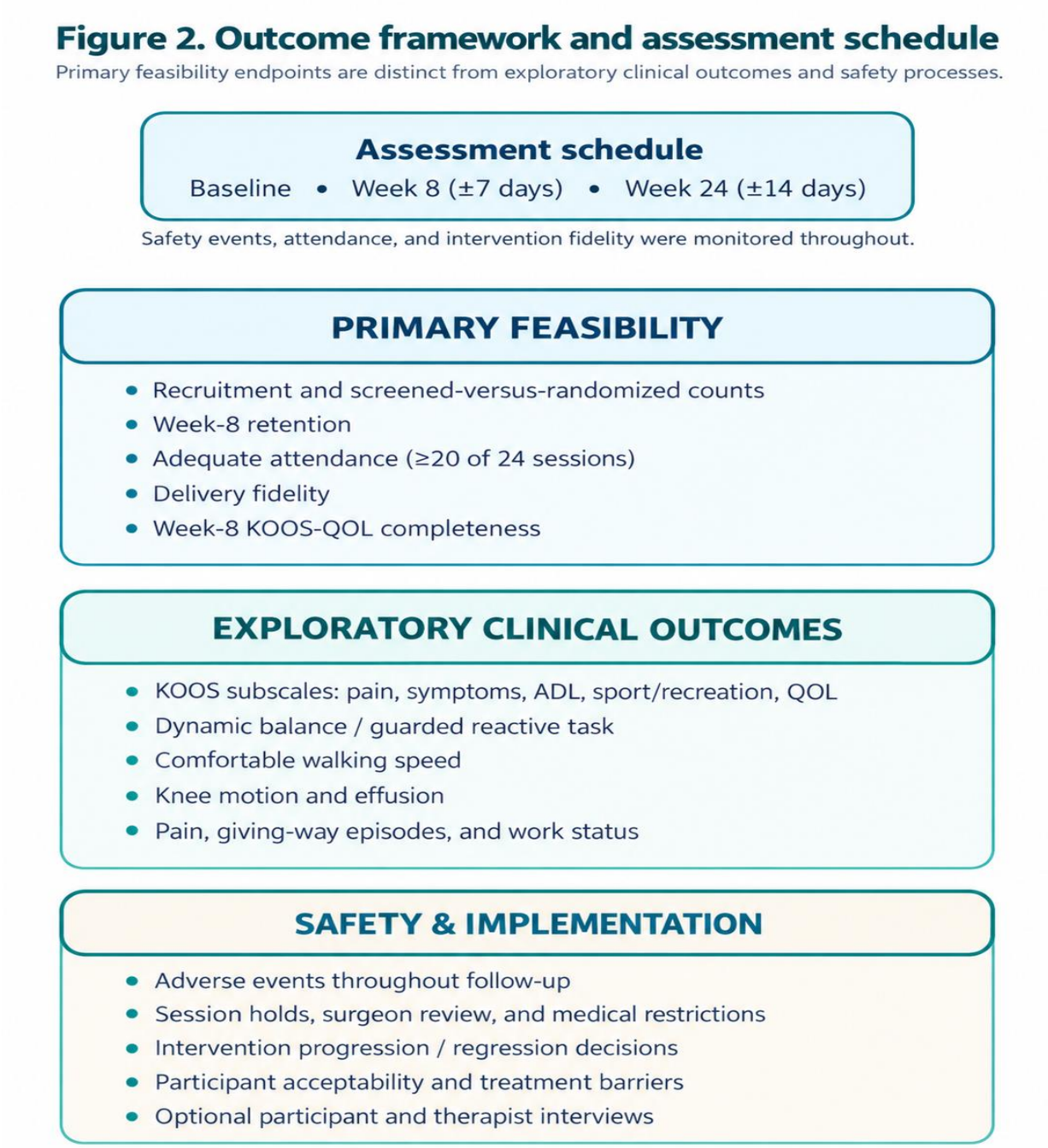



**Figure 2. Outcome framework and assessment schedule, separating primary feasibility endpoints from exploratory clinical outcomes and safety monitoring.**

## Methods

### Design, setting and reporting framework

The source proposal specifies a prospective, individually randomized, assessor-blinded, two-arm, parallel-group external pilot trial with equal allocation. The intended clinical setting is a network of orthopaedic follow-up and physiotherapy services in Karachi, Pakistan. Four hospitals were named as proposed collaborators: Dr Ruth K M Pfau Civil Hospital Karachi, Shaheed Mohtarma Benazir Bhutto Institute of Trauma, South City Hospital, and Dr Ziauddin Hospital. The proposal explicitly did not assume prior institutional agreements or that each proposed institution would necessarily contribute as a separate statistical centre. Recruiting pathways were to be activated only after local permissions, a trained team, secure identification procedures, and allocation concealment were in place. Any final trial report must replace proposed sites with the centres that actually participated, supported by site documentation. The principal design characteristics are summarized in Table 1.

Because Civil Hospital and SMBBIT may share patient referrals or staff, the protocol anticipated the possibility of combining related services into one operational centre for stratification. The centre definition was to be fixed before the first randomization and used consistently thereafter. This is more reliable than retrospectively assigning centre categories to improve balance between groups. The data-entry architecture distinguishes the recruitment site, treating service and operational statistical centre, because a patient can be referred from one institution and attend rehabilitation at another. The final analysis must state precisely which centres enrolled participants, when they were activated, and whether any referral overlap or duplicate records were detected.

The planned sample was 48 randomized patients, 24 per arm, chosen to provide operational experience and estimate feasibility with acknowledged imprecision. It was not based on a predicted KOOS treatment effect or powered to establish clinical superiority. The proposed enrollment period was 18 active recruitment months, subject to an initial service audit of real caseloads. An enrolled participant who later withdrew was to remain in the randomized denominator; the trial did not permit replacement to maintain an artificial complete-case sample. Trial planning called for a six-month recruitment review if fewer than twelve patients had been randomized. A future definitive study would require a separate sample-size calculation tied to a clinically meaningful effect, external evidence, and appropriate variance assumptions [8,9].

**Table 1. Trial design, intervention allocation and assessment schedule.**

| Element | Protocol specification | Verification source |
|---|---|---|
| **Design** | External pilot RCT; 1:1; blinded assessors | Allocation log and governance record |
| **Population** | Adults 18–65, ≥2 reconstructed complexes, 16–32 weeks postoperative | Surgery reports and signed consent |
| **Exposure** | 24 planned supervised sessions in 8 weeks | Dated attendance and treatment logs |
| **Week-eight retention** | Completion of scheduled patient-reported clinical assessment | Original questionnaire / follow-up source |
| **KOOS-QOL** | Scorable authorized instrument | Item-level forms and scoring audit |
| **Safety** | Continuous event collection | Original AE log and medical review |

### Participants and screening

Patients were intended to be 18–65 years old and 16–32 weeks after their last planned reconstruction of at least two major unilateral ligament complexes. Eligible operative patterns could involve the anterior cruciate ligament, posterior cruciate ligament, medial complex, and lateral or posterolateral complex. A repair of an additional complex was permitted in combination with reconstruction, whereas isolated

single-ligament procedures and repair-only procedures were excluded. The independent ability to walk on level ground with full weight-bearing was required, along with capacity to attend scheduled visits and provide written informed consent. These criteria delimit the population to a later protected phase of postoperative rehabilitation, not acute management.

Enrollment also required explicit written clearance from the treating surgeon for the proposed balance module. The baseline functional thresholds were resting knee pain of no more than 3 on a 0–10 scale, extension deficit not greater than 5 degrees, flexion of at least 110 degrees, absent or mild effusion no greater than grade 1+ on the standardized stroke test, a straight-leg raise without extensor lag, and ten controlled sit-to-stand repetitions without giving way or an increase in pain. The thresholds were proposed conservative study criteria, not validated rules for every reconstruction pattern. Even a patient meeting all numeric thresholds remained ineligible if specific postoperative restrictions conflicted with the minimum module requirements.

The proposed exclusions covered pending revision or additional reconstruction; active infection, suspected thrombosis, unresolved vascular compromise or clinically unstable reconstruction; an unhealed fracture or associated operation prohibiting loading; a major contralateral injury that impairs balance; and neurological or peripheral nerve deficits that prevent safe participation. Other concerning medical conditions were to be assessed individually by the surgical team. Pregnancy was not defined as an automatic exclusion. Meniscal repair, a healed fracture, or a history of vascular injury similarly did not automatically preclude participation once restrictions resolved and written authorization was obtained. Patients not meeting functional criteria could be reassessed within the eligibility window without accelerating ordinary recovery to satisfy trial scheduling.

Every consecutive potentially eligible patient was to be entered into a screening register, regardless of eventual consent or randomization. Screening fields include referral service, eligibility criteria, relevant exclusion conditions, availability of surgical clearance, approach and consent decision, and reasons for declining or not being enrolled. This approach is needed to distinguish low surgical caseload from restrictive entry criteria, poor contactability, or refusal of randomization. Each participant must have a stable study identifier and a separate confidential linkage file to avoid duplication between referring services. Consent procedures should be provided in an understandable language with appropriate assistance for participants with limited literacy, without compromising voluntariness.

Randomization, concealment and blinding

After written consent and complete baseline assessment, a central coordinator independent of assessment was to release allocation from a computer-generated sequence prepared by an independent statistician. Randomly varying blocks, stratified by finalized operational centre, were specified in the protocol. Block sizes and allocation sequences should remain inaccessible to clinicians who enroll or treat participants. Group assignment must not be based on alternating patients, day of attendance, serial medical record numbers, or any other predictable process. If the implemented process differed from the protocol, the final manuscript must describe the actual sequence generation, concealment and amendments instead of silently reporting the intended design.

Treatment staff and patients could not reasonably be blinded to the distinctive exercises. Outcome assessors were to remain uninvolved in intervention delivery, without access to allocation codes or session logs; participants were to be reminded not to disclose their treatment content. Accidental unblinding, if it occurred, should be documented at the corresponding visit. A coded analysis could initially refer to groups as A and B, with the mapping disclosed after predefined primary tables were complete. The assessor-facing data file should not include the allocation variable, and restricted access to the code key should be maintained as an operational control rather than merely as a filename convention.

**Interventions**

The scheduled rehabilitation dose was three supervised sessions each week for eight weeks, totaling 24 planned sessions per participant. Sessions were designed to last approximately 65 minutes: a five-minute warm-up, a 40-minute common rehabilitation core, and a 20-minute allocation-specific module. The common core covered individualized mobility, progressive strengthening, predictable balance exercises, and gait re-education within operative limitations. Approximate elements included five minutes of

mobility, twenty minutes of strength exercises, ten minutes of gait training, and five minutes of planned balance practice. Exercise resistance, joint angles, repetitions, and rest periods were to be adjusted by the therapist in accordance with symptoms and written surgical restrictions.

Strength exercises were proposed as two or three sets of 8–12 controlled repetitions for suitable quadriceps, calf and hip activities, with hamstring loading limited by clearance after posterior or associated reconstructions. Gait activities focused on level walking, quality of weight acceptance, and permitted step tasks. Home exercises were designed to remain comparable between arms and avoid unsupervised reactive perturbations. Medication, orthopaedic follow-up and clinically required treatment were not restricted by trial allocation. Such changes should be documented as cointerventions, particularly when they affect treatment exposure or outcomes. A detailed therapist manual was to establish exactly which options were acceptable at each functional stage.

The reactive module added small, closely supervised disturbances delivered through the trunk or pelvis, never directly to the knee. The patient practised recovering alignment in supported bilateral or split stance, initially with predictable timing and readily available hand support. During subsequent phases, timing and direction were to become less predictable only after adequate safe recovery was observed. Later tasks could include a single carefully guarded recovery step toward a floor marker, but only if the surgeon expressly authorized stepping. Dynamic lateral stepping or another more complex challenge was optional and restriction-dependent. A gait belt, firm surface, hand support, close therapist guarding, and a second trained staff member for protective stepping were integral safeguards, not optional equipment.

Progression was determined by clinical criteria rather than the calendar alone. The proposed first phase emphasized simple supported stance, small disturbances, and recovery with assistance when necessary. Advancement to more variable disturbances required two sessions without deterioration and successful unassisted recovery in at least 80% of repetitions. Protective stepping was to be introduced only when allowed and when the participant demonstrated controlled load acceptance without instability or increasing effusion. One variable could be progressed at a time, and any participant could remain at a safe earlier level. Forceful pulls, sudden support release, loaded twisting, forced hyperextension, hopping, and cutting were prohibited. The therapist was expected to record intensity, direction, stance, trials, support, recovery, symptoms, and reasons for non-progression [1,3].

The comparison module allocated the same final 20 minutes to predictable weight shifts, planned stepping, mobility work, stable stance and conventional functional exercises. Deliberate unexpected disturbances were excluded, although normal corrective responses arising during ordinary gait practice were not regarded as contamination. The treatment contrast therefore concerned intentional reactive practice rather than all possible balance reactions. Equal scheduled contact time also helps reduce the risk that observed differences reflect therapist attention alone, although therapists' expectations and differences in treatment enthusiasm cannot be eliminated in an unblinded exercise trial.

**Safety procedures**

Before each session, the therapist was to ask about new or increased pain, swelling, knee giving way, falls, fever, and symptoms raising concern for vascular or thrombotic complications. The module was to be held for pain exceeding 3/10 or two points above the patient's usual baseline, new instability, new loss of motion, effusion above permitted limits, inability to accept weight safely, or a new concerning medical complaint. A clinical assessment and documented decision were required before continuation, with renewed surgeon clearance after a significant event. The reporting system was designed to distinguish an exercise-related temporary symptom, a new injury, and a serious adverse event using consistent definitions.

Adverse events included pain or swelling lasting more than 24 hours beyond baseline, a fall or near fall requiring therapist rescue, giving way, new injury, or unplanned medical attendance. Serious adverse events were defined to include death, a life-threatening occurrence, hospitalization, or persistent major disability. Records needed to capture onset, severity, suspected relation to treatment, immediate action, treatment interruption, medical review, reporting dates and resolution. The proposed independent safety reviewer was an orthopaedic surgeon not delivering treatment, reviewing events at least monthly during active intervention. Any possibly related serious event was intended to suspend reactive sessions across

the network pending an urgent review; two clinically important possibly related non-serious events also triggered a protocol review. Absence of reported events would not prove absence of risk.

Outcomes and measurement schedule

As summarized in Table 2, feasibility outcomes were predefined as the number randomized during active recruitment, week-eight patient-reported assessment retention, adequate exposure to the intervention, proportion of correctly delivered applicable treatment components, and complete scorable week-eight KOOS quality-of-life questionnaires. These are separate denominators and must not be collapsed into a single success score. Progression criteria were specified in advance: recruitment of at least 48 was green, 36–47 amber and fewer than 36 red; eight-week retention of at least 85% was green, 70% to below 85% amber, and lower values red. For reactive-arm adequate exposure, at least 80% reaching the attendance threshold was green, 60% to below 80% amber, and lower values red. Fidelity of at least 90% was green, 75% to below 90% amber, and lower values red. KOOS-QOL completeness used the same green and amber cutoffs as retention.

**Table 2. Prespecified feasibility outcomes and progression criteria.**

| Outcome | Green | Amber | Red |
|---|---|---|---|
| **Randomized over recruitment period** | ≥48 | 36–47 | <36 |
| **Week-eight retention** | ≥85% | 70%–<85% | <70% |
| **Reactive-arm adequate exposure** | ≥80% | 60%–<80% | <60% |
| **Delivery fidelity** | ≥90% | 75%–<90% | <75% |
| **Week-eight KOOS-QOL completeness** | ≥85% | 70%–<85% | <70% |

Feasibility rates require carefully established denominators. For recruitment, the numerator is randomized participants and the relevant exposure is the number of months during which recruiting pathways were actually operational, in addition to screened and approached patient totals. For adherence, the protocol defines at least 20 of 24 planned sessions as adequate exposure; the status of a partially attended session should be resolved prospectively in the statistical analysis plan. This is not a minor arithmetic choice because it can change a participant's classification at the 20-session boundary. A transparent report should provide the primary definition and any prespecified alternative based on completed minutes or full sessions, without choosing the most favorable rule after inspecting group results.

The Knee injury and Osteoarthritis Outcome Score (KOOS) was selected to capture patient-reported pain, symptoms, activities of daily living, sport and recreation, and knee-related quality of life. Five subscales should be scored independently from zero to 100, with higher scores indicating more favorable reported status. The protocol nominated eight-week KOOS quality of life as the candidate primary clinical endpoint of a future definitive trial; it is not the primary endpoint of this feasibility pilot. A Pakistan-based study of the Urdu adaptation reported satisfactory measurement properties in a knee osteoarthritis sample, but those observations do not establish measurement validity for postoperative multiligament reconstruction. The authorized language version, administration mode, missing items, and any difficulties understanding questions must be recorded and reported [10].

Dynamic balance was to be assessed using a standardized three-direction modified Star Excursion Balance Test with anterior, posteromedial and posterolateral reaches when permitted by the surgeon. Limb length was measured from the anterior superior iliac spine to the medial malleolus. Following six practice reaches, three valid trials in each direction were planned, and the maximum valid reach in each direction normalized to limb length. A trial required a stable stance foot, light reaching-foot touch without substantial loading, and controlled return without external support. Results should not be imputed as zero when the patient cannot safely attempt the test; nonperformance is clinically informative and requires a recorded reason.

A separate guarded reactive assessment involved six small standardized trunk disturbances, recording the number of corrective steps and occasions when therapist rescue was required. This is a protocol-specific exploratory measure, not a validated postoperative multiligament score or a substitute for laboratory postural testing. Consequently, findings from this task should be described at the level of the observed procedure and not given a diagnostic meaning or minimal clinically important difference that has not been established. The baseline assessor training should document the procedure and check consistency. These practical details are needed because manual perturbation intensity and therapist guarding can otherwise vary even when nominal repetition counts are identical.

Comfortable walking speed was to be measured by averaging two timed trials over a measured 10-metre section with suitable acceleration and deceleration distance. The same footwear and walking aid, where feasible, should be used at repeat assessments. Additional clinical outcomes included knee flexion and extension with a goniometer, standardized effusion grading, usual walking pain over the previous week on a 0–10 scale, giving-way episodes, and work status. Return to sport was a descriptive self-report only and did not constitute clearance to resume high-demand activity. Recording the source and timing of each measure is essential; a participant's pain answer on a follow-up form should not silently be substituted for an assessor-recorded baseline pain field.

Assessments were scheduled at baseline, eight weeks after randomization, and 24 weeks after randomization. The prespecified windows were plus or minus seven days at week eight and plus or minus fourteen days at week twenty-four. Adverse events were recorded continuously, while session adherence and therapist fidelity were collected during the intervention period. Optional acceptability interviews were planned after week eight with a purposive subsample of approximately ten to twelve participants and four to six therapists. Telephone follow-up could recover an authorized questionnaire and adverse-event history when an in-person visit was missed, but it could not be counted as completion of a physical assessment.

**Implementation outcomes and interviews**

Participant interviews were intended to explore whether the added training felt safe and understandable, which tasks were perceived as helpful or difficult, and what factors affected attendance. Questions about travel costs, work obligations, caregiving, appointment scheduling, and confidence with balance challenges could help interpret low exposure without blaming individual patients for system-level barriers. Therapist interviews similarly could identify equipment needs, staff ratios, interpretation of surgical restrictions, and the practical difficulty of standardizing disturbance intensity. These qualitative results should support explanation of implementation, not be used as proof that the intervention improves outcomes.

Interview participants were to give separate consent, and audio recordings would require explicit permission; otherwise, anonymized written notes would be retained. Two researchers were expected to review and compare coding and produce a concise thematic summary that distinguishes common observations from a small number of highly specific experiences. The interviewer should avoid presenting the reactive programme as necessarily superior and should ask about negative experiences, confusing instructions, and nonattendance as deliberately as about helpful aspects. A trial intended to progress to a larger study benefits as much from identifying practical failures as from recording participant satisfaction.

**Data management and planned statistical analysis**

The source protocol requires coded case report forms, role-restricted electronic records, separately stored contact details, a secure cross-site data-sharing procedure and a documented audit trail for corrections. Screening, randomization, rehabilitation exposure, clinical assessments, interviews, and adverse events have different levels of access. In particular, the blinded assessor should not have routine access to group allocation or treatment logs. Any derived variable must retain its definition and original source fields, and missing should be distinguished from not assessed, not permitted, zero measured events, and participant refusal. A second reviewer was planned to check all eligibility and adverse-event records and a sample of other entries against their clinical source documents.

The primary statistical reporting should be descriptive and estimation-focused. Proportions of eligible, consenting, retained, adequately exposed, and successfully assessed participants should be shown with

exact numerators and denominators and appropriate 95% confidence intervals. Recruitment should additionally be expressed per active recruitment month, accounting for site activation. Fidelity should be calculated using the total number of correctly delivered applicable checklist items divided by the total number of applicable observed items, rather than averaging percentages from sessions with different numbers of items. Outcomes must be reported by randomized group and centre without implying that a small pilot is powered for subgroup assessment [8,9]. The progression thresholds are shown in Table 2.

The protocol prespecified an exploratory eight-week comparison of KOOS-QOL using a baseline-adjusted linear model containing allocation, baseline score and operational centre. Estimated mean differences should be accompanied by 95% confidence intervals and described as imprecise pilot estimates. Other KOOS subscales, dynamic balance measurements, walking speed, pain, and motion may be analyzed similarly when enough observations are available. Week-24 analyses should be presented separately. No significance-driven claim of superiority, post hoc power calculation, large exploratory covariate set, or centre-by-treatment interaction is justified by the planned sample. Patient-level variation in injury combinations and associated procedures should be described as contextual characteristics rather than overfitted predictors.

Missingness should be tabulated for each assessment and outcome separately. A partially completed patient questionnaire may be usable for one subscale and not for another, whereas an unperformed balance test may reflect surgeon prohibition rather than loss to follow-up. Available-case analyses should identify denominators at each time and outcome. Last-observation-carried-forward and automatic replacement of missing tests with zero are inappropriate. If substantial missingness threatens interpretation, a transparent sensitivity analysis should examine plausible departures from the observed outcomes, subject to the limited sample size. The same prespecified rules should apply to both trial groups, irrespective of the direction of the estimated clinical differences.

**Results: Database-Derived Estimates and Evidence Status**

The study dataset contained 347 screening records and 48 randomized participants, with 24 allocated to each coded group. The recruitment, allocation and follow-up counts are summarized in Figure 1. These results are presented according to the available dataset and the analysis procedures described below.

Of 347 screened individuals, 292 were recorded as ineligible, five eligible individuals declined consent, and two eligible individuals were not enrolled, leaving 48 randomized participants (13.8% of those screened). Figure 1 summarizes the participant flow and outcome completion at weeks 8 and 24.

Attendance at 20 or more of 24 planned sessions was recorded for 43 of 48 participants (89.6%), with partly completed sessions counted as attended in the database. This requires prespecification because the original protocol does not define a partial-session threshold. Fidelity observers scored 649 of 695 applicable checklist items as correctly delivered, producing aggregate fidelity of 93.4% across 126 observed sessions. The event file contains 16 adverse-event records and no recorded serious adverse event, but these data cannot establish absence of rare harm. Figure 3 displays the principal database-derived feasibility proportions. Tables 3 and 4 report follow-up completeness and exploratory clinical estimates. The feasibility estimates are summarized in Table 3 and Figure 3.

**Table 3. Recruitment, attendance, fidelity and assessment completeness.**

| Feasibility measure | Numerator/denominator | Proportion |
|---|---|---|
| **Randomized / screened** | 48/347 | 13.8% |
| **Week-8 clinic assessment** | 46/48 | 95.8% |
| **Week-8 scorable KOOS** | 45/48 | 93.8% |
| **Week-24 clinic assessment** | 39/48 | 81.3% |
| **Week-24 scorable KOOS** | 39/48 | 81.3% |
| **≥20 sessions attended** | 43/48 | 89.6% |
| **Fidelity checklist items** | 649/695 | 93.4% |

At baseline, mean KOOS quality-of-life scores were 46.18 (SD 8.77; n = 22) in coded group 1 and 47.02 (SD 12.16; n = 24) in group 2. At week 8, available means were 70.10 (SD 9.75; n = 22) and 60.35 (SD

8.81; n = 23), respectively. The linear regression model adjusting for baseline score and operational centre estimated a group 1 minus group 2 difference of 10.37 points (95% CI 5.92–14.82; complete-case model n = 43). At week 24, mean scores were 85.39 (SD 10.99; n = 20) and 69.98 (SD 13.13; n = 19), with an adjusted difference of 17.24 points (95% CI 11.56–22.92; n = 37). Figure 4 presents these descriptive trajectories and their precision. Full clinical outcome estimates are presented in Table 4 and Figure 4.
At week 8, coded group 1 also had higher available mean values than group 2 for KOOS pain (73.39 vs 67.02), symptoms (72.32 vs 64.90), activities of daily living (82.59 vs 73.79), and sport/recreation (66.75 vs 53.30). Mean walking speed was 1.339 versus 1.239 m/s, while the guarded reactive task required a mean of 0.52 versus 1.67 recovery steps. All such comparisons are exploratory and potentially affected by differential completeness, regression to the mean, routine healing and measurement uncertainty.

**Table 4. Exploratory clinical outcomes by coded group and assessment time.**

| Outcome | Group 1 | Group 2 | Adjusted difference (95% CI) / note |
|---|---|---|---|
| **KOOS QOL, week 8** | 70.10 (9.75), n=22 | 60.35 (8.81), n=23 | 10.37 (5.92 to 14.82) |
| **KOOS QOL, week 24** | 85.39 (10.99), n=20 | 69.98 (13.13), n=19 | 17.24 (11.56 to 22.92) |
| **KOOS Pain, week 8** | 73.39 | 67.02 | Exploratory descriptive |
| **KOOS Symptoms, week 8** | 72.32 | 64.90 | Exploratory descriptive |
| **KOOS ADL, week 8** | 82.59 | 73.79 | Exploratory descriptive |
| **KOOS Sport, week 8** | 66.75 | 53.30 | Exploratory descriptive |
| **Gait speed, week 8 (m/s)** | 1.339 | 1.239 | Exploratory descriptive |

Because this was a feasibility pilot with 48 randomized participants, the clinical comparisons are exploratory. Estimates and 95% confidence intervals are emphasized rather than confirmatory claims of treatment superiority. Coded groups should be mapped to intervention names only using the authorized allocation record and the prespecified unblinding procedure. The clinical estimates are summarized in Table 3 and the feasibility findings in Table 4.

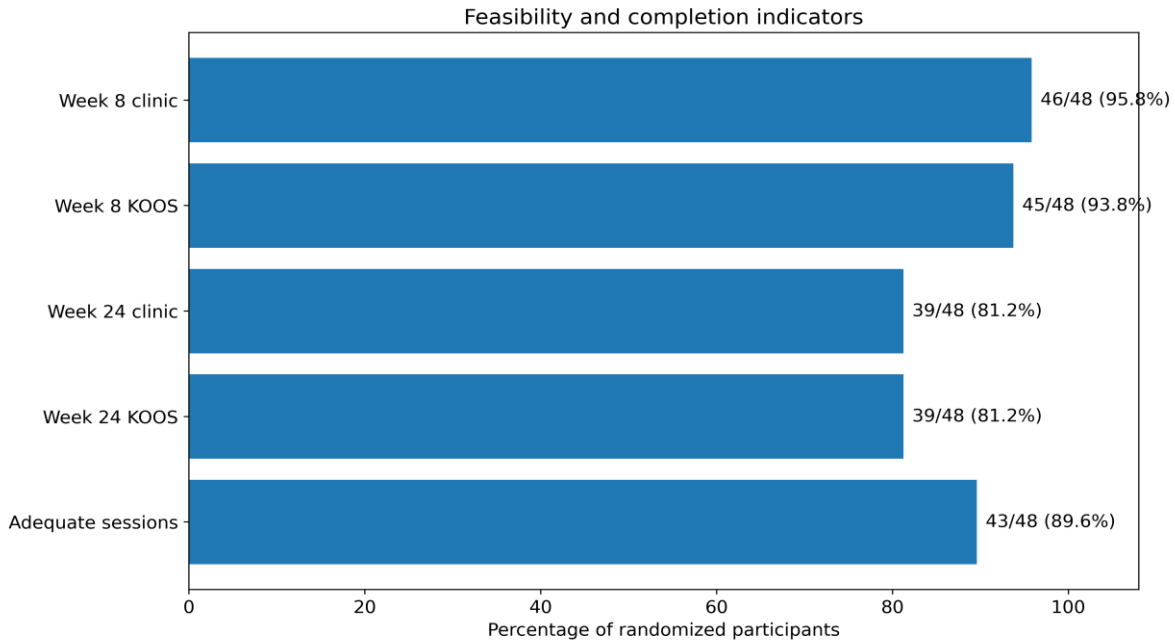



**Figure 3. Feasibility outcomes, including recruitment, follow-up completion, adequate session attendance and treatment fidelity.**

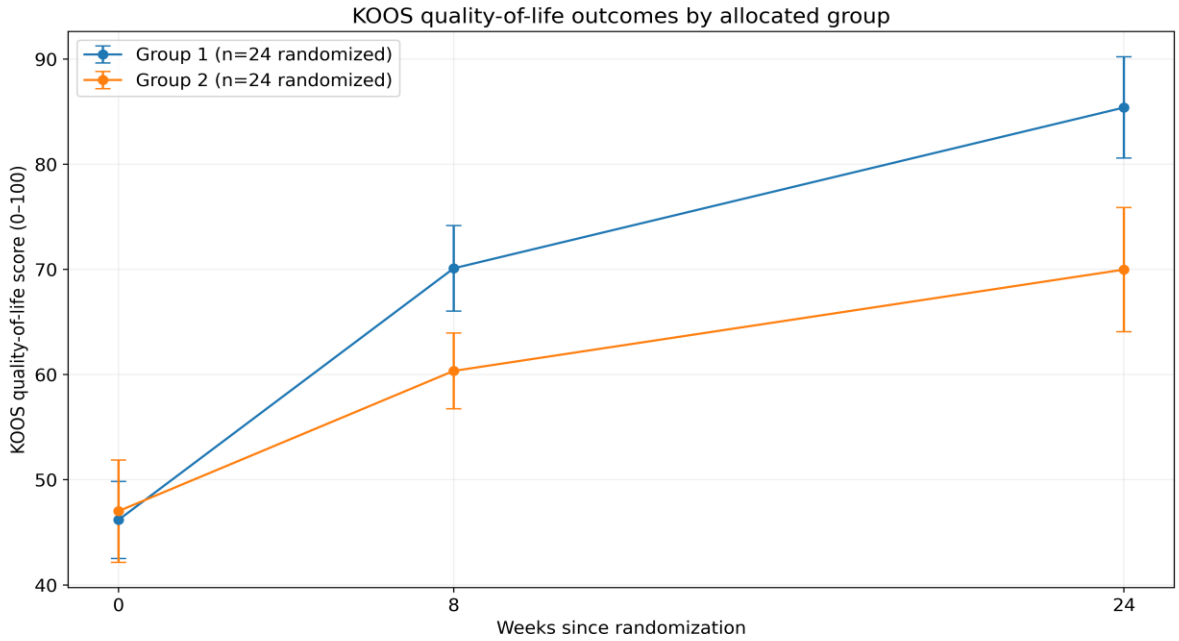



**Figure 4. KOOS quality-of-life scores at baseline, week 8 and week 24 by coded allocation group. Error bars represent uncertainty intervals as specified in the statistical report.**

## Discussion

This study design responds to a practical weakness in the available evidence: rehabilitation recommendations for complex ligament reconstructions are often derived from broader clinical principles, retrospective series, or studies of isolated knee injuries. A conservative reactive balance programme is plausible because safe walking requires responses to disturbances as well as planned movement. Plausibility is not proof. Especially in later postoperative rehabilitation, an intervention that increases the complexity of balance demands may be useful for some patients and inappropriate for others. The purpose of an external pilot is to determine whether those differences can be identified and managed consistently enough to justify a definitive controlled comparison [1,3,4].

The international consensus on multiligament injury management emphasizes that injuries and procedures are heterogeneous, which supports the decision to require specific surgical clearance and to record each ligament complex involved [3]. A standardized treatment manual should not erase individualized restrictions. Instead, it should specify permitted choices and non-negotiable safety rules, while allowing a therapist to retain a participant in an earlier exercise phase when appropriate. A useful feasibility report would show not only how many participants completed prescribed sessions but also how often and why progression was delayed. That information can help distinguish intervention failure from correct clinical caution.

A central design strength is the matched duration of supervised rehabilitation. Many exercise studies compare an enhanced programme with usual care without matching visit time, which makes the active therapeutic ingredient difficult to identify. The present comparison attempts to isolate intentional reactive exercise within an otherwise common rehabilitation schedule. Equal minutes do not guarantee identical attention or intensity, however, and therapist enthusiasm may still differ between interventions. Observed delivery fidelity and documentation of cointerventions are therefore required for the treatment contrast to be interpretable. If control participants systematically received other balance therapies outside trial sessions, any future efficacy comparison would need to consider contamination.

The choice of KOOS quality of life as the candidate clinical endpoint is patient-centred, but requires careful interpretation. Scores can improve over the natural course of recovery, and the sport/recreation subscale can reflect restrictions on attempting activities rather than a participant's actual willingness or physical ability. Permission to use a language version is a separate question from whether its performance is adequate in a multiligament postoperative sample. The 2021 Pakistani Urdu adaptation study supports

further use and investigation of Urdu-language KOOS, but its sample and setting do not automatically establish equivalent responsiveness, measurement error or meaningful change thresholds in this population [10]. A future trial may need cognitive interviewing or validation work if participants consistently find certain items confusing.

The modified reach test and guarded recovery task provide complementary, but not interchangeable, information. A planned reach evaluates movement control under a predictable task, whereas a guarded trunk disturbance asks for a rapid corrective response. Improvements in one may not imply improvements in the other, and neither alone establishes prevention of falls or return to unrestricted sport. The reach procedure must also accommodate postoperative restrictions and document invalid trials carefully. A patient who cannot perform a test because of a surgical prohibition is different from a patient who attempts it but displays reduced reach distance. Collapsing both situations into a low performance score would produce a misleading clinical comparison.

The proposed safety framework anticipates that an intentional balance disturbance carries risks even when it is gentle. Close guarding, assisted stepping, symptom thresholds and reporting procedures reduce avoidable hazards, but they cannot establish safety by design alone. In a 48-participant pilot, an absence of serious adverse events would still be statistically compatible with clinically relevant uncommon risks. Reporting both exposure time and adverse-event counts helps readers assess the context of any observed harm. A future multicentre study should consider whether therapists across sites apply similar criteria when judging disturbance intensity, providing rescue assistance or deciding to pause a session.

Recruitment is likely to be a major determinant of future-trial feasibility because multiligament reconstruction is not equivalent to a high-volume isolated ACL operation. The eligibility window, clearance requirements and ability to attend three weekly sessions may reduce the pool further. A screening register containing all consecutive candidates is therefore essential. Without it, a report of 48 randomized participants cannot reveal whether recruitment was efficient, selective, or achieved only through unusually intensive case-finding. Recruitment rates should also be expressed against real active recruiting time rather than against calendar months when some institutions had not yet commenced enrollment. This approach is needed for realistic budgeting and timelines for a larger trial.

Participation barriers require measurement in their own right. A participant who misses a visit because public transport is unreliable differs from one who stops because the allocated exercises trigger knee swelling. The response to these problems also differs: scheduling or travel assistance may help the former, whereas the latter requires clinical reassessment. Site variation in staffing or treatment facilities may affect adherence despite similar participant motivation. By recording reasons and conducting short optional interviews, the pilot can identify which barriers are remediable and which are part of the clinical reality of the intended target population. Those findings would support refinement of recruitment materials, appointment scheduling and resource estimates.

The planned progression thresholds serve as decision aids rather than formal hypothesis tests. All-green feasibility findings would support considering a larger study, but would not guarantee that the intervention is safe or effective. An amber finding would direct attention to specific operational problems and a potential remedial plan. A red result could indicate that the design needs substantial changes even if exploratory clinical outcomes appear promising. This ordering prevents the temptation to overlook serious recruitment or missing-data problems because one patient-reported measure favors the experimental group. Decisions about proceeding should be made using the complete feasibility profile and independent safety review.

Recent long-term studies reinforce the importance of taking a broad view of rehabilitation outcomes. Clinical function and knee-related quality of life can remain impaired years after multiligament injury, and postoperative arthrofibrosis may itself require further treatment [2,5,11]. These observations should inform the discussion of why gait, range of motion and patient-reported function are relevant. They do not justify a claim that a brief eight-week reactive intervention changes long-term osteoarthritis or reoperation risk. A 24-week observation in this pilot is useful for describing retention and outcome trajectories, but any durable clinical advantage would still require confirmation in a larger and longer trial.

The literature on perturbation-based exercise requires the same restraint. A recent case report of combined ligament-related problems and a pilot investigation of visually perturbed neuromuscular tasks are useful for hypothesis development, but they do not match this exact postoperative population, intervention intensity or treatment contrast [6,7]. Differences in patient age, tissue healing, sport level, injury mechanism and timing of intervention make direct numerical extrapolation questionable. The present pilot can help decide whether a specifically guarded manual disturbance protocol is teachable, acceptable and measurable in the relevant clinical setting. It cannot establish superiority simply because the concept resembles a successful programme used elsewhere.

Several limitations are built into the design. The sample is small, the case mix is heterogeneous, participants and therapists cannot be blinded, and objective reactive-balance measurement lacks a fully validated postoperative instrument. The exercise progression is partially dependent on clinical judgment, which protects safety but can reduce exact dose consistency. Even with a standardized common core, normal care, graft-specific restrictions and home exercise adherence may vary. Follow-up at twenty-four weeks may also be incomplete for reasons unrelated to the intervention. These sources of uncertainty require transparent reporting of denominators, restrictions, treatment modifications, and uncertainty intervals rather than strong causal claims.

Another limitation concerns external validity. Strict functional entry criteria enhance safety but exclude people with substantial persistent deficits, unresolved neurovascular complications, major associated injury, or limited access to rehabilitation. The results would apply most directly to independently walking adults who received reconstruction of at least two complexes, met the stated threshold requirements and obtained surgeon approval. A future study might need a separate pathway for patients with greater disability; it should not generalize pilot findings to all knee dislocations or all phases of postoperative recovery. Demographic and work-related context should be presented alongside injury patterns to help readers understand who could realistically participate.

An additional limitation is that data origin cannot be inferred from logical consistency. The analyzed ZIP includes a deterministic program that constructs participant-level records and an accompanying training-dataset label, despite subsequent statements that these represent original observations. This conflict remains unresolved and precludes presenting computed estimates as authenticated trial outcomes. The manuscript must be reconciled with source documents, ethics approvals, registration history and a verifiable data lineage before journal submission.

The larger purpose of the pilot is to decide what to do next. A positive implementation result could justify a prospective definitive trial with a validated target clinical endpoint, carefully estimated recruitment duration, and sufficient sample to examine meaningful between-group differences. An adverse implementation result could instead lead to a less frequent supervision schedule, more flexible attendance support, a different perturbation procedure or a narrower eligible population. Both outcomes advance the practical knowledge needed for robust multiligament rehabilitation research. Transparent reporting of failures and adverse events is as valuable as describing smooth implementation because it prevents future investigators from repeating unrealistic assumptions [8,9].

Conclusion

The proposed matched-duration reactive balance rehabilitation programme is feasible to describe and assess using prespecified functional and safety criteria. The supplied electronic tables yield internally analyzable feasibility and exploratory clinical estimates, but the provenance conflict prevents clinical inference from being finalized. A definitive report requires reconciliation to original source records, authorized treatment allocation, prospective registration and approvals. If authenticated, the pilot should be interpreted primarily through recruitment, retention, intervention fidelity, completeness and safety rather than efficacy hypothesis testing.

**Ethical considerations and declarations**

**Ethics approval:** Ethical approval was obtained from the Institutional Review Board/ethics committees of South City Hospital and Jinnah Sindh Medical University (JSMU).

**Informed consent:** Written informed-consent forms were available, and written informed consent was obtained from participants before enrollment, as reported by the study team.

**Funding:** No external funding was received.

**Competing interests:** The authors declare no competing interests.

**Data availability:** Deidentified participant-level data may be made available by the corresponding author subject to ethics approval, informed consent and institutional permissions.

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