Journal of
Functional Morphology and Kinesiology

MDPI

Perspective

Before Changing an Exercise Prescription: A Delivery-First Approach to Apparent Nonresponse

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Abstract

A training outcome below expectation creates an immediate decision: continue the program, change its delivery, repeat the assessment, alter the prescription, or seek clinical review. These actions require different evidence. This perspective specifies a candidate six-step review for that decision episode. The proposed review uses established records of exercise prescription, completed exercise, internal load, and measured outcomes as its primary inputs. The six steps confirm safety and the target, verify the apparent shortfall, reconstruct the completed exercise, assess strain and feasibility, select a justified action, and set the next review or stopping point. Each step defines an output and conditions for proceeding, pausing for information, or leaving the decision unresolved. Additional monitoring is justified by whether its possible results could change the action at acceptable cost. Constructed pulmonary rehabilitation and warm-up examples show how missing exercise records and uncertain measurements can lead to different decisions from the same initial report of poor response. A proposed three-arm evaluation compares usual care with scheduled review, a simple checklist and the full review, with common care resources, information access and scheduled assessments. The primary contrast estimates functional benefit over the checklist across all enrolled participants; a prespecified secondary analysis addresses participants with a confirmed shortfall before allocation. Interpretation incorporates the frequency of triggered reviews, uncertainty, harms and monitoring burden. The contribution is an operational decision intervention and a design for testing its incremental value. Its effectiveness remains unknown; failure to demonstrate worthwhile benefit should lead to simplification or rejection.

Keywords: exercise prescription; decision-making; intervention fidelity; achieved dose; measurement uncertainty; monitoring burden; pragmatic evaluation

Check for updates

Academic Editor: Olivier Hue

Received: 16 October 2026
Revised: 6 November 2026
Accepted: 10 November 2026
Published: 12 November 2026

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1. The Next Decision After a Disappointing Outcome

At a scheduled exercise review, a patient or athlete reports that the expected improvement has not occurred. The practical problem is what to do next. A harder prescription,

better access to the existing program, another measurement and clinical reassessment are different interventions. Selecting between them requires a credible description of the shortfall and of the exercise that actually occurred. This perspective addresses that bounded decision episode. "Delivery-first" refers to checking that the exercise is actually completed before routine prescription escalation, rather than to the literal order of every review task. The safety and the credibility of the apparent shortfall are assessed before the completed exercise is reconstructed.

Session attendance alone may provide an incomplete exposure record. Contemporary oncology studies separately record prescribed and achieved resistance work, interruptions, modifications and tolerance [1–3]. Conversely, an absent device entry need not establish that no exercise occurred: monitoring adherence and exercise adherence are different variables [4]. A poor outcome following poorly documented exercise therefore supports a different conclusion from a poor outcome after verified work and a credible assessment.

Reporting studies identify another distinction. Incomplete intervention descriptions in prehabilitation, rheumatology and inpatient respiratory exercise research limit what a reader can reconstruct [5–7]. A meta-analysis of aerobic training in cardiovascular disease found no significant association between reporting completeness and the magnitude of improvement in flow-mediated dilation [8]. Separately, an audit of exercise-science studies identified persistent concerns about risk of bias [9]. These findings concern different properties of the evidence and do not establish that more complete documentation improves patient outcomes. The proposed intervention must therefore be tested for the decisions and outcomes it changes.

Data-informed exercise decisions already exist in rehabilitation, and recent sport-science frameworks address monitoring uncertainty and actionability [10,11]. This perspective contributes a specified six-step decision procedure, an explicit category for unresolved decisions, and a comparative evaluation designed to determine whether the additional structure provides worthwhile benefits relative to its time and resource costs.

2. Scope and Source Selection

This conceptual perspective proposes a decision intervention and a design for its evaluation. Literature selection was purposive rather than systematic. P.J. and A.M. identified publications, with selection discussed by H.N. and G.S. Sources were selected for their relevance to the decision following an exercise outcome below expectation.

The evidence serves four purposes: describing planned and completed exercise, assessing observation credibility, distinguishing observed change from individual treatment response, and designing a fair test of an added decision process. The bibliography comprises 112 distinct publications spanning 2013–2026. Foundational conceptual and methodological publications establish the existing approaches; contemporary intervention studies, measurement research and clinical guidance inform their application to the present decision.

Targeted source checks were completed on 4 November 2026 using terms concerning exercise adherence, intervention fidelity, achieved dose, measurement error, individual response, monitoring decisions and trial evaluation. These checks and source-specific searches do not establish exhaustive coverage. Complete query histories, retrieval counts and discovery routes are unavailable; consequently, no reproducible screening total, pooled effect or formal certainty assessment is reported.

Sources are used according to their designs. Completed-dose studies describe exposure; reporting assessments describe available information; device studies quantify particular observation properties; methodological papers support an analysis or design; and clinical guidance supplies care standards. Protocols describe planned methods and are never treated as effectiveness results. The two worked cases are constructed examples. All numerical

decision margins below are candidate protocol choices requiring prospective agreement, not thresholds validated by this perspective.

The two schematic figures were developed using ChatGPT (OpenAI, GPT-6), CorelDRAW Graphics Suite 2026 (v27.2), and Adobe Photoshop 2026 (v27.11). ChatGPT assisted with organizing the conceptual structure, drafting descriptive labels and arranging the sequence of review tasks. CorelDRAW was used for vector drawing, layout and alignment of graphical elements. Adobe Photoshop was used for high-resolution image editing, contrast adjustment and final formatting for publication. The authors reviewed the figures for scientific accuracy and consistency with the manuscript.

3. What the Proposed Review Adds to Existing Practice

External work and internal load are established components of the training process [12,13]. Exercise-oncology reporting also distinguishes prescribed and completed dose [14]. The Consensus on Exercise Reporting Template (CERT) and the Template for Intervention Description and Replication (TIDieR) describe intervention content, delivery, tailoring and implementation [15,16], with recent exercise-specific guidance explaining their application [17–19]. The proposed record uses those descriptions when a particular prescription decision arises; it does not replace the full reporting standards or rename their constructs.

Precision exercise medicine already addresses response variability and the evidence required for individualized prescription [20]. Individual-response methods distinguish noisy change from treatment effects [21,22]. The Medical Research Council (MRC) framework connects intervention development, context, program theory, implementation and evaluation [23,24]; the Consolidated Framework for Implementation Research (CFIR) supplies complementary implementation concepts [25]. Contemporary monitoring frameworks additionally ask whether a metric is actionable and whether acting on it improves an outcome [11,26,27]. Table 1 identifies what is borrowed and what remains to be tested.

World Health Organization (WHO) guidance supplies population-level physical activity recommendations [28]; it does not determine a symptomatic patient's session workload. American College of Sports Medicine (ACSM) guidance supports exercise testing and individualized prescription [29]. The ATS/ERS statement defines pulmonary rehabilitation as assessment followed by individualized therapies, including exercise, education and behavior change [30]. Current American Thoracic Society (ATS) recommendations establish pulmonary rehabilitation care in the proposed trial [31]. These standards apply in every arm; the experimental question concerns the added review structure.

Table 1. Established approaches and the limited proposed contribution.

Existing Approach	What It Already Supplies	Use and Unresolved Question Here
Training load and completed dose [12–14]	Distinguishes external work, internal load and completed exposure.	Established training-load and completed-dose measures provide inputs to the proposed review.
CERT, TIDieR and exercise reporting [15–18,32]	Intervention content, delivery, tailoring and implementation descriptions.	Existing intervention descriptions inform prescription decisions; full reporting standards remain applicable.
Precision exercise and individual response [20–22,33]	Response variability, observed change and replicated comparisons.	The review retains uncertainty when selecting an action without claiming to estimate individual causal treatment effects.

Table 1. *Cont.*

Existing Approach	What It Already Supplies	Use and Unresolved Question Here
MRC-based development and CFIR implementation [23–25]	Program theory, context, implementation and evaluation.	The proposed contribution is a defined decision procedure and its evaluation, rather than a general implementation framework.
Monitoring and actionability frameworks [11,26]	Connects observation quality, interpretation, action and outcomes.	The proposed procedure specifies six review tasks and tests their incremental value against a simpler process.
Activity and clinical guidance [28–31]	Population activity goals; clinical assessment, prescription and progression.	Population goals do not set a patient session workload. Clinical guidance defines care in every arm.

The experimental addition is the compulsory review sequence and its decision record. It states which uncertainty prevents a routine change, which further observation could resolve it, and when review should stop. Experienced practitioners may already make all of these judgments. The proposed comparison therefore tests reproducibility and incremental benefit over competent usual care and a simpler checklist, rather than superiority over an artificially uninformed comparator.

4. Information Needed for a Defensible Decision

4.1. Minimum Information for a Prescription Decision

Four established records supply the inputs. The prescription states the intended mode, workload, frequency, duration and progression. The completion record describes achieved work in corresponding units and retains reasons for changes. Internal load records relative strain or tolerability during that work. The outcome record identifies the agreed target, assessment procedure and review interval. These established measures are examined together to support the interpretation of a specific exercise-prescription decision [13].

The completion denominator must remain visible. Original and clinically revised prescriptions are retained separately, so that an appropriate adaptation is not mistaken for a missing session or hidden by changing the denominator. Exercise-relative dose intensity can summarize achieved resistance-exercise volume relative to prescribed volume [34]. For the proposed review, the underlying sets, repetitions and load would also be retained when they could change the next decision; this is a proposed documentation choice, not a finding attributed to that source. Aerobic exercise records should describe the corresponding mode, duration and intensity, as illustrated by digitally monitored treadmill exercise [35]. Realized versus planned training load provides another adherence measure [36]. The review would record attendance, completed work, modifications, withdrawal and missing information separately, using the relevant definitions reported in the underlying intervention studies [37,38].

Delivery fidelity and adaptation also require separate descriptions. Fidelity concerns delivery of the specified intervention; adaptation describes what was deliberately changed and why. Recent process studies show the importance of documenting both rather than treating every deviation as either failure or successful personalization [39,40]. A process record can identify the question to investigate without establishing that correcting the recorded discrepancy will improve function.

Comparator delivery needs the same scrutiny. If usual care is incompletely described or the full review receives more equipment, contact or support, an apparent advantage cannot be attributed to its decision structure alone [32,41–43]. This is why the proposed trial shares information access and clinical resources across all arms and checks actual delivery, not allocation labels alone.

Assessments of physical activity trials and non-pharmacological concussion interventions examine methodological rigor and reporting completeness [44,45]. Reviews of shoulder rehabilitation and exercise for older adults address reproducibility and attention to training principles, respectively [46,47]. These sources support checking what an intervention description permits a reader to reconstruct; they do not identify the cause of an individual patient's outcome.

4.2. Credibility of the Apparent Shortfall

Before interpreting a change, specify what was measured. Device agreement, within-session consistency, between-session reliability, responsiveness and meaningful change answer different questions. A reliable metric can be insensitive to the intended target; correlated instruments can have different means; and a change judged important can remain imprecisely measured. Table 2 links these observation properties to an action rather than to an undifferentiated monitoring score [48,49].

In field walking, instructions, familiarization and repeat testing can affect the recorded result. A randomized study supports standardized instructions and repeated testing [50]. A separate COPD study found further improvement during a third test in some patients, indicating that two tests may not eliminate learning effects for everyone [51]. The proposed evaluation would use two tests at each scheduled assessment, with the greater distance as the prespecified summary, as detailed in Section 7.1. This pragmatic choice would be examined during feasibility work and would not be presented as proof that learning effects had been eliminated. Any change to the testing procedure would be finalized before definitive-trial recruitment and applied consistently across groups and assessment occasions.

Jump measurement is similarly conditional on the selected metric and acquisition procedure. Reporting recommendations address the choice of countermovement-jump variables [52], while validation work examines agreement and reliability for a particular portable force-plate system [53]. Other studies estimate repeatability and detectable differences for specified strength and jump measures [54,55]. Processing and calculation choices can change the measured quantity or its error [56,57], and remote assessment methods require their own validity and reliability evidence [58]. These sources inform measurement standardization; they do not establish a universal threshold for retaining a warm-up strategy. The worked case therefore uses local familiarization and a prospectively chosen worthwhile contrast.

Missingness must be attached to the relevant record. Verified completion, verified noncompletion and unknown work are different states. A diary that was not completed may prevent assessment of intensity or progression, while a wearable may contain partial days or unusable intervals [59–62]. Missing information should prompt a justified ascertainment plan, not automatic assignment of zero exercise or exclusion from beneficial care.

Measurement properties also depend on the target population and use. For patient-reported outcomes, recent methods emphasize identifying the construct, population, administration and intended interpretation before selecting an instrument [48,63]. Performance-test validation likewise needs the relevant procedure and setting [64,65]. An estimate from a different device, disease group or test interval can inform planning but does not establish the uncertainty of this person's current decision.

Table 2. Information that can change the next action.

Potential Information	Decision It Can Change	Evidence and Limit on Interpretation
Target and patient preference	Choose which shortfall to investigate and which outcomes or costs matter.	Decision quality and clinical benefit require separate assessment [66,67].
Test conditions and repeated performance	Repeat an assessment when a procedural or learning effect could alter the conclusion.	Standardize instructions and summary rules; learning and protocol effects are setting-specific [51,65,68].
Device, metric and processing	Avoid interpreting a change caused by a different instrument or calculation.	Agreement and error are metric- and procedure-specific [52,53,56].
Worthwhile change and measurement uncertainty	Decide whether the interval supports a worthwhile contrast, rules it out or remains unresolved.	Importance and detectable change are not interchangeable [49,69].
Original, revised and achieved work	Distinguish an appropriate modification from a completion deficit before routine escalation.	Preserve denominators and component-level records [1,35].
Missing exercise or monitoring records	Seek targeted information or retain an unknown-exposure state.	Missing diary entries, partial wearable records and verified noncompletion are different observations [59,62].
Strain, symptoms and feasibility	Seek clinical assessment, adapt permitted work or address an execution barrier.	Exercise participation and tolerability should be described using the distinct measures reported in the relevant intervention studies [2,37].
Cost of another observation	Collect only information capable of changing a justified action within the available time and resources.	Record staff and participant effort prospectively; usefulness is not established by data volume [70,71].

4.3. Observed Change and Individual Treatment Response

A pre-to-post difference includes treatment-related change, background change, day-to-day variation and measurement error. It is not an observed individual causal treatment effect. Atkinson and Batterham distinguish true response variation from apparent differences due to ordinary within-person variability [33]. A conventional population-level starting analysis subtracts the control group's change-score variance from the intervention group's variance. When positive, the square root of that difference is commonly termed the standard deviation of individual responses. Its assumptions and uncertainty must accompany the estimate [21,33]. A negative variance-difference estimate is not a negative biological standard deviation.

This calculation does not identify a stable personal responder phenotype. Similar variances do not prove identical treatment effects, and extra variance can reflect unequal achieved exposure or observation error. Hecksteden and colleagues address the design needed to investigate individual exercise responses [72]; Senn distinguishes components of variation that require replication to separate [73]. Recent methodological treatments and repeated-intervention studies further distinguish an observed change, response repeatability and a participant-by-treatment interaction [74,75]. Repeated comparisons are especially relevant to reversible short-term decisions; chronic training requires attention to carryover and the feasibility of a control or washout [22].

Treatment selection is a further inference. The Predictive Approaches to Treatment effect Heterogeneity (PATH) Statement concerns assessment and reporting of variation in treatment effects relevant to clinical decisions [76]. A response label from one exercise episode is insufficient to establish that one prescription is preferable to another for that person. The proposed review records the evidence supporting the next action and retains unresolved uncertainty rather than claiming a validated treatment-selection model.

The empirical result also depends on endpoint and design. A four-week trial found poor agreement between response classifications based on measured peak oxygen uptake and accessible surrogates [77]. Other recent analyses examine response variance, replication and classification under different protocols [78–80]. The defensible practical statement is conditional: no clear improvement on the specified test after the recorded exposure. It does not identify the cause of that shortfall.

Verified completion likewise does not prove that the prescription supplied an adequate stimulus. Reuter and colleagues compared continued moderate training with increasing intensity at matched energy expenditure after a shared initial training phase [81]. That design makes the actual stimulus relevant to interpretation, but its healthy participants and endpoint-specific classification do not justify automatic escalation in symptomatic rehabilitation. Clinical progression remains governed by the relevant care standard [29,31].

4.4. Decision-Relevant Information and the Cost of Additional Monitoring

Before adding a test or log, state which possible result would change the action, which action would follow, and by when the information is needed. If every plausible result leads to the same appropriate action, the extra observation has little immediate decision value. Recent work on monitoring actionability and rehabilitation decisions provides a basis for making that question explicit [11,82].

An unresolved decision is a legitimate output. Further measurement is justified when it can distinguish reasonable alternatives at acceptable cost; otherwise the record should state the uncertainty, the action supportable now and the next review point. Patient preferences concern both the intended benefit and the burden of obtaining information. Decision-process quality, treatment benefit and consultation cost require separate assessment [66,67].

Monitoring costs include staff and participant time, equipment, data processing, missing-record follow-up and barriers to participation. They should be measured rather than assumed negligible [70,71,83]. The proposed minimum can use ordinary clinical records and a paper ledger. Optional tests are added only for a defined decision and removed when their information no longer changes a justified action.

5. A Six-Step Intervention for Prospective Testing

Figure 1 and Table 3 specify the same six numbered steps. The arrows are decision routes between review tasks, not a physiological pathway or an alternative sequence to the table. Immediate safety concerns can end the routine review at Step 1 or interrupt any later step. The rules specify a candidate intervention for evaluation; they are not a validated clinical algorithm.

The output is an action with a reason, retained uncertainty and a review plan. The options include continuing the current program, repairing a delivery problem, repeating an assessment, modifying the prescription within clinical guidance, seeking clinical review, or leaving routine escalation unresolved. A verified exposure record and an apparently unchanged outcome are insufficient on their own to select the direction or size of a dose change.

Routine progression is considered only after the review target and observation are credible, achieved work is sufficiently known, tolerability is acceptable, the planned assessment interval is appropriate, and progression is permitted by the existing clinical or sport-specific plan. A completion discrepancy directs attention to delivery; an observation problem directs attention to measurement. The procedure does not convert either into a diagnosis of biological nonresponse.

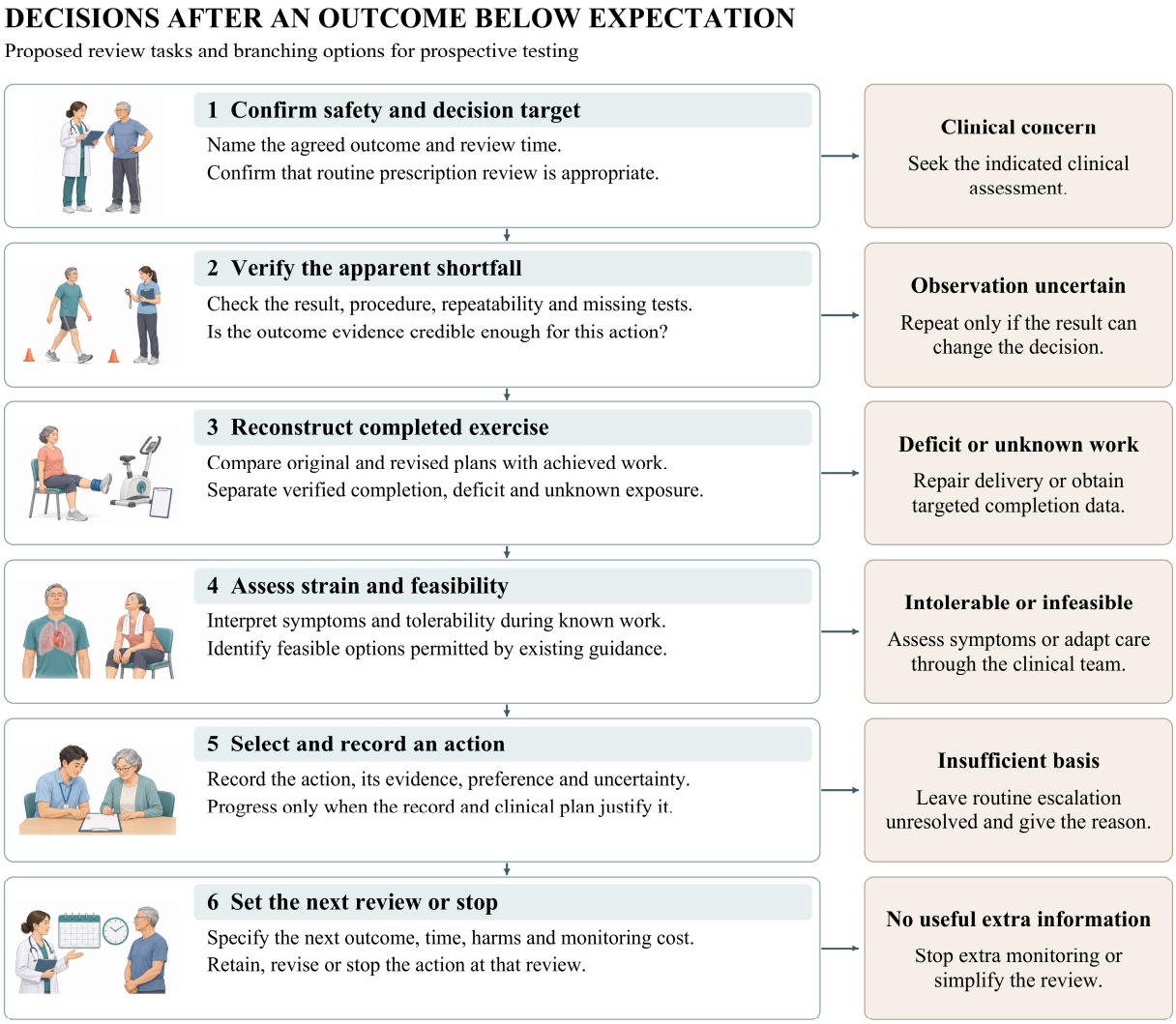


Figure 1. Proposed decision routes after an outcome below expectation. The six numbered nodes match Table 3. Arrows connect review tasks and candidate actions; they do not represent physiological effects. A clinical concern can interrupt any step. Pausing for information and leaving a routine change unresolved are explicit outputs. The routes require prospective evaluation. CorelDRAW Graphics Suite 2026 (v27.2), Adobe Photoshop 2026 (v27.11), and ChatGPT (OpenAI, GPT-6) were used in the development and design of this figure.

Table 3. Proposed six-step review and branching conditions.

Step and Decision Question	Input	Recorded Output	Continue, Pause, or Stop
1 Confirm safety and decision target	Clinical status; agreed outcome; planned review time; current prescription.	Named target and whether routine prescription review is appropriate.	Clinical concern: seek the indicated assessment and stop routine escalation. Otherwise proceed.
2 Verify the apparent shortfall	Raw result; comparable conditions; error and repeatability; missing tests.	Interpretable shortfall or observation uncertainty.	If a credible repeat can change the action, obtain it. If not feasible, retain uncertainty rather than label nonresponse.

Table 3. *Cont.*

Step and Decision Question	Input	Recorded Output	Continue, Pause, or Stop
3 Reconstruct completed exercise	Original and revised plans; achieved work; attendance; reasons and missing records.	Verified completion, verified deficit or unknown exposure.	A verified deficit directs attention to delivery. Unknown exposure calls for a minimal ascertainment plan or an unresolved decision.
4 Assess strain and feasibility	Strain and symptoms during known work; interruptions; practical barriers.	Permitted and feasible options under existing guidance.	Intolerability or clinical deterioration: assess and adapt through the clinical pathway. Do not infer dose adequacy from completion alone.
5 Select and record an action	Credible outcome; exposure; guidance; preference; retained uncertainty.	Continue, repair delivery, remeasure, modify within guidance, refer, or leave unresolved; give the reason.	Progress only when the record, tolerance and existing plan justify it. State what further result would change the action.
6 Set the next review or stop	Action; review date; repeated target; completion; harms and burden.	Retain, revise or stop the selected action.	Review at the agreed point. Stop extra monitoring when it cannot change a justified action, or when its cost or harm outweighs its value.

Support for delivery also requires evaluation. Reviews and trials examine adherence-support strategies, alternative delivery modes and added educational or behavioral components [84–86]. Acceptability, contact and improved documentation can change without corresponding functional benefit. Matching those resources in the experimental arms is therefore necessary to identify the contribution of the review itself [83,87].

6. Constructed Cases Showing the Next Decision

6.1. Pulmonary Rehabilitation: Attendance Is Not Completed Work

Consider an adult with stable chronic obstructive pulmonary disease, COPD, in a pulmonary rehabilitation service following current guidance [29,31]. The agreed aerobic component is 20 min at an individualized workload on three occasions weekly for four weeks: 240 planned minutes. The patient attends 10 of 12 sessions, but only 120 min within the prescribed workload range are documented. Attendance is 83%; recorded completion against the original plan is 50%. These are arithmetic illustrations, not escalation cutoffs.

At Step 1, the team confirms that no new clinical concern requires a different care pathway and that functional walking is the agreed target. At Step 2, it checks whether the baseline and follow-up tests used the same instructions, assistance and summary procedure. If those records are inadequate and a repeat could change the action, it repeats the assessment before treating the apparent shortfall as established [50]. A missing test record and a verified unchanged outcome are not interchangeable.

At Step 3, the 120 min gap requires explanation. Session notes identify interrupted bouts and transport-related late arrival. Step 4 considers the strain and symptoms recorded during the work actually completed. The proposed next action is to make the agreed work feasible, or adapt it through the clinical team, with a recorded reassessment date. Increasing nominal minutes while the missing work persists would answer a different question. Respiratory exercise studies and delivery evaluations provide context for component-level records and feasible modes of care [7,88].

Temporal interpretation also matters. In a lung-cancer cohort, an early activity decline preceded later trajectories associated with symptoms, expectations and self-efficacy [89].

In the Braining feasibility study, acceptability, attendance and successful monitoring were distinct findings [90]. These observations motivate asking when and why participation was interrupted; they do not establish the benefit of a particular support change for this COPD patient.

Good pulmonary rehabilitation practice can already reach this decision. An attendance-only review might interpret 83% attendance as adequate exercise exposure and favor increasing the prescription. The proposed review instead identifies 50% verified completion and prioritizes resolving the delivery shortfall before routine escalation. This contrast concerns an incomplete decision record, not a disagreement with established pulmonary rehabilitation guidance. The proposed advantage is that the shortfall, evidence and uncertainty are recorded reproducibly. The trial must determine whether that requirement improves outcomes over ordinary guidance and a simple ledger, including when alternative delivery modes are feasible [31,88].

6.2. A Warm-Up Decision: A Positive Mean Can Remain Unresolved

Consider an athlete deciding whether to retain a conditioning activity intended to produce post-activation performance enhancement (PAPE) before countermovement-jump assessment. A published experiment in 35 moderately trained adults did not detect a condition-by-time interaction at the four-minute assessment [91]. That result concerns its tested conditions and timing; it does not show that the intervention was poorly delivered or identify a stable individual nonresponder.

The proposed comparison uses six randomized matched pairs of sessions, comparing the conditioning activity with the athlete's usual warm-up. Each session uses the same device, timing and average of three jumps before and after the condition. The paired outcome is the conditioning change minus the control change. Delivery deviations and missing tests are retained in the record; any replacement session follows a rule written before data collection. Measurement studies inform which metrics and procedures require standardization [52,54,56].

The following values are hypothetical. Repeated familiarization sessions estimate a typical error of 1.0 cm for the three-jump session mean; the athlete and practitioner choose 1.5 cm as a worthwhile paired benefit. A mean contrast of 2.0 cm and between-pair standard deviation of 1.2 cm give an approximate two-sided 95% t interval of 0.74–3.26 cm over six pairs. The between-pair uncertainty is not replaced by that familiarization error estimate.

The candidate rule retains the strategy only when the interval's lower limit exceeds 1.5 cm and its cost is acceptable. If its upper limit is below 1.5 cm, the strategy is not retained for that target. An interval touching or spanning 1.5 cm is unresolved; additional testing requires an agreed information value and a new fixed analysis plan, not repeated significance checking. Here the positive mean does not justify retention. Recent repeated-comparison research supports distinguishing this uncertainty from a single observed improvement [22,75]. A decision based only on the positive mean difference might favor retaining the conditioning activity. In contrast, the prespecified decision rule leaves retention unresolved because the confidence interval crosses the 1.5 cm worthwhile-benefit threshold. This illustrates a different decision from a mean-only interpretation, not from established measurement-aware coaching.

7. Testing the Value of the Decision Process

7.1. Three Arms with a Common Standard of Care

A proposed pragmatic cluster-randomized evaluation would recruit outpatient pulmonary rehabilitation services and adults with stable COPD. Follow-up would extend from baseline to week 12. Services would be randomized to reduce contamination between

review approaches. A feasibility phase would estimate recruitment, the proportion meeting the review criterion, record availability, cluster sizes, outcome variance, intracluster correlation and monitoring burden before the definitive protocol and sample size were registered [92–94].

A fixed participant cohort would be enrolled at each service before allocation. During a common four-week period, all services would provide guideline-based rehabilitation and collect the same minimum source records: the baseline functional target, expected week-4 progress, prescribed and completed exercise, symptoms, adaptations and adverse events. Neither the Arm B checklist nor the Arm C six-step review would be introduced during this period. Service allocation would occur only after week-4 assessments and review-eligibility classifications had been completed and locked for the enrolled cohort. Arm-specific training and review procedures would then begin. This timing would prevent the assigned review structure from influencing the measurements used to define the shortfall subgroup.

At baseline, week 4 and week 12, assessors unaware of allocation would administer two six-minute walk tests using standardized instructions and encouragement, the same course and a prespecified approach to walking aids and supplemental oxygen. Tests conducted on the same day would be separated by at least 30 min and adequate clinical recovery. Both distances would be recorded, with the greater distance used for the scheduled assessment. Clinically necessary deviations would be documented. The two-test procedure would balance repeatability and assessment burden [50], while acknowledging evidence of residual learning during a third test in some patients with COPD [51]. Feasibility work would assess this choice before the definitive protocol was fixed.

The expected week-4 progress would be documented at baseline using a common target-setting procedure. A confirmed shortfall would require an interpretable week-4 result below that prospectively specified target; it would trigger the allocated review. Missing or non-comparable observations would be classified as uncertain, with a common ascertainment procedure and fixed assessment window specified before recruitment. Uncertain cases would not be coded as having no shortfall. They would remain in the all-participant analysis, receive clinically indicated care and have their subsequent ascertainment and review use reported separately.

Arm A would receive usual care with scheduled review and ordinary clinical notes, following ACSM prescription and current ATS pulmonary rehabilitation guidance [29,31]. Arm B would receive the same care plus a brief review checklist covering the target, planned and completed work, repeated outcome, adverse events and next review date. Arm C would receive the same care plus the six-step procedure, including discrepancy explanations, uncertainty, pause/continue/stop conditions and a documented action. All arms would share access to the underlying records, equipment, care options and scheduled assessments. Additional work generated by the allocated approach would be measured rather than assumed equal.

The primary estimand would remain the participant-average C-minus-B difference in six-minute walk distance at week 12 across the enrolled cohort, adjusted for baseline, regardless of review eligibility or subsequent workflow adherence. This would estimate the value of adopting the review policy across a service. A prespecified secondary estimand would address the same contrast among participants with a confirmed shortfall established before allocation, with adjustment for baseline and week-4 distance. This subgroup would be defined by eligibility, not by receipt of or adherence to the allocated review. Its findings would complement rather than replace the primary result. C versus A would be secondary; other comparisons would be exploratory, with multiplicity addressed in the analysis plan.

Referrals, treatment changes and discontinuation of the review would remain permitted. Outcomes would be sought after these events, with a treatment-policy approach

used where the outcome remained defined. The protocol would specify separate handling of death or other events that made walking distance undefined, and would distinguish such events from missing observations [95,96]. Participant weighting and cluster-aware estimation would be aligned with the estimands [97]. Prespecified covariate adjustment and inference would account for randomization strata, service clustering and the number of clusters [98].

Sample-size planning would distinguish the expected treatment effect from the minimum worthwhile incremental benefit. It would incorporate the expected frequency of triggered reviews, plausible effects among eligible participants, uncertainty in these assumptions, unequal cluster sizes, intracluster correlation and attrition. Simulations would assess both power for the primary contrast and the probability of meeting the benefit decision rule in Section 7.3. The hypothetical worked cases would not supply empirical effect-size estimates.

Effect modification would mean a difference in the specified C-versus-B causal contrast across levels of a prespecified characteristic on the chosen effect scale; prognostic association or an association with exercise participation alone would not establish it [76]. Interaction analyses would be secondary, limited in number and prespecified, with missing modifier data addressed explicitly [99]. Reasons for missing outcomes and service withdrawal would be reported by arm. Sensitivity analyses would examine departures from the prespecified missing-data assumptions, particularly when symptoms or delivery difficulties predicted missingness [95,100].

7.2. Benefit, Burden and Independently Judged Decisions

The worthwhile incremental benefit should reflect the active comparator and the population included in the estimand. Both B and C would receive pulmonary rehabilitation. For scale, current ATS guidance reports an approximately 44 m mean advantage of pulmonary rehabilitation over control [31], while a recent synthesis estimates an approximately 25 m minimum important difference for six-minute walk distance in respiratory conditions [49]. Neither quantity directly defines the worthwhile between-group increment from adding a decision procedure to otherwise similar rehabilitation.

No empirical estimate of the review-trigger frequency or the C-versus-B effect is available for this procedure. For provisional planning only, a trigger frequency of 50% would be examined alongside sensitivity scenarios of 25% and 75%. If the incremental effect were confined to triggered participants, with no spillover or effect in other participants, the population-average effect would approximately equal the trigger proportion multiplied by the effect among triggered participants. Thus, a 30 m population-average advantage would imply a 60 m advantage among triggered participants at a 50% trigger frequency, or 120 m at a 25% frequency. These calculations illustrate dilution and are not observed treatment effects.

A 10 m population-average advantage is proposed instead as a provisional minimum worthwhile increment for discussion with patients and services. At a 50% trigger frequency, it would correspond to a 20 m increment among triggered participants. It is approximately one-quarter of the reported rehabilitation-versus-control effect and reflects the narrower contribution expected from decision support added to active care. An illustrative planning alternative of a 30 m effect among triggered participants would yield a 15 m population-average effect at the same frequency; this expected-effect scenario is distinct from the 10 m decision threshold. Neither assumption is established by existing evidence. Feasibility estimates, plausible treatment effects and stakeholder judgments about benefit relative to burden would inform their revision before definitive-trial registration. The full confidence

interval would be reported, with clinical importance distinguished from measurement uncertainty and statistical evidence of benefit [49,69].

Decision quality would be a separate secondary endpoint. Before allocation, an independent panel would establish an adjudication rubric and acceptable action ranges using external clinical guidance. A data-management team separate from the panel would prepare a standardized case extract from the common pre-decision source records. Each extract would contain the same categories of clinical, exercise and assessment information, with unavailable items explicitly marked. Allocation labels, service and clinician identifiers, arm-specific fields, step order and free-text workflow rationales would be excluded. The chosen action would be presented using a common action vocabulary. Adjudicators would remain unaware of allocation, workflow adherence and subsequent clinical outcomes.

The panel would judge whether the action was defensible from the information available when it was selected. More than one action could be acceptable. Documentation length, completion of the six steps and the persuasiveness of an arm-specific narrative would not contribute to the score. Extraction consistency and inter-rater agreement would be assessed. After scoring each case, adjudicators would record their allocation guess, including an "unable to tell" option, confidence and reasons; these findings would be reported as a check on possible unblinding, without treating chance-level guessing as proof of successful blinding.

Patient understanding and preference concordance would be assessed separately from functional benefit [66,82]. Patient-reported functional limitation would be a secondary clinical outcome measured with a prespecified instrument appropriate to the population.

Delivery fidelity would record achieved work, legitimate changes, interruptions and ascertainment. Workflow fidelity would record whether the allocated review was used. These would remain process outcomes, distinct from clinical improvement [101,102]. Staff time, participant effort and resource use would be collected prospectively, including training, preparation, review delivery and missing-record follow-up [70,71]. A provisional limit of 10 additional staff minutes per enrolled participant per week relative to B would apply over the eight-week period from the week-4 review to week 12. Total incremental staff time, including allocated training costs, would be divided by all enrolled participants and the eight-week period; time per triggered review would also be reported. This service-level allowance would require agreement before registration and would not imply that spending the full allowance was justified by any positive outcome. Participant burden and resource costs would be evaluated separately. Adverse events would have prespecified definitions, ascertainment schedules and denominators, with independent safety stopping procedures [103]. More exercise, more complete records or fewer unsupported decisions alone would not establish functional benefit. The comparative design is summarized in Figure 2, and its candidate evaluation criteria are presented in Table 4.

Table 4. Candidate evaluation criteria and consequences.

Evaluation Item	Prespecified Candidate	Consequence for the Proposed Review
Primary comparison and outcome	C versus B; participant-average, baseline-adjusted six-minute walk distance at week 12 across the enrolled cohort; service-level randomization after common week-4 assessment.	Estimates the service-level value of the review policy, including participants without a triggered review.
Triggered-review analysis	Prespecified secondary C-versus-B contrast among participants with a confirmed shortfall before allocation; report trigger frequency and uncertain eligibility separately.	Examines the population directly eligible for the decision intervention without selecting participants by subsequent review adherence.

Table 4. *Cont.*

Evaluation Item	Prespecified Candidate	Consequence for the Proposed Review
Worthwhile functional benefit	Provisional population-average target of 10 m; report the estimate and two-sided 95% confidence interval.	Upper limit below 10 m: candidate target ruled out. Estimate ≥10 m and lower limit >0: provisional benefit criterion met. Other results: insufficient evidence for adoption under this rule.
Staff and participant burden	Provisional maximum of 10 additional staff minutes per enrolled participant weekly over weeks 4–12, including allocated training costs; report time per triggered review, participant effort and resource costs.	Upper confidence limit for mean additional staff time must not exceed the agreed allowance; participant burden and costs must also be acceptable.
Safety	Defined events, surveillance, denominators and independent stopping criteria.	Unacceptable harm can reject the procedure irrespective of the functional estimate.
Independent decision quality	Panel unaware of allocation, workflow adherence and subsequent outcomes; standardized extracts of common pre-decision data; record allocation guesses and inter-rater agreement.	Assesses action defensibility without rewarding arm-specific documentation or conformity to the six-step sequence.
Delivery and workflow processes	Achieved work, legitimate modifications, ascertainment and use of the assigned review.	Process improvement may explain an effect but cannot substitute for functional benefit.
Simplification	Subsequent component-removal evaluation only after credible evidence of benefit; any noninferiority margin justified from the established benefit, its uncertainty and the acceptable loss.	Retain a simpler procedure only if it preserves an acceptable share of benefit with lower burden and acceptable safety.

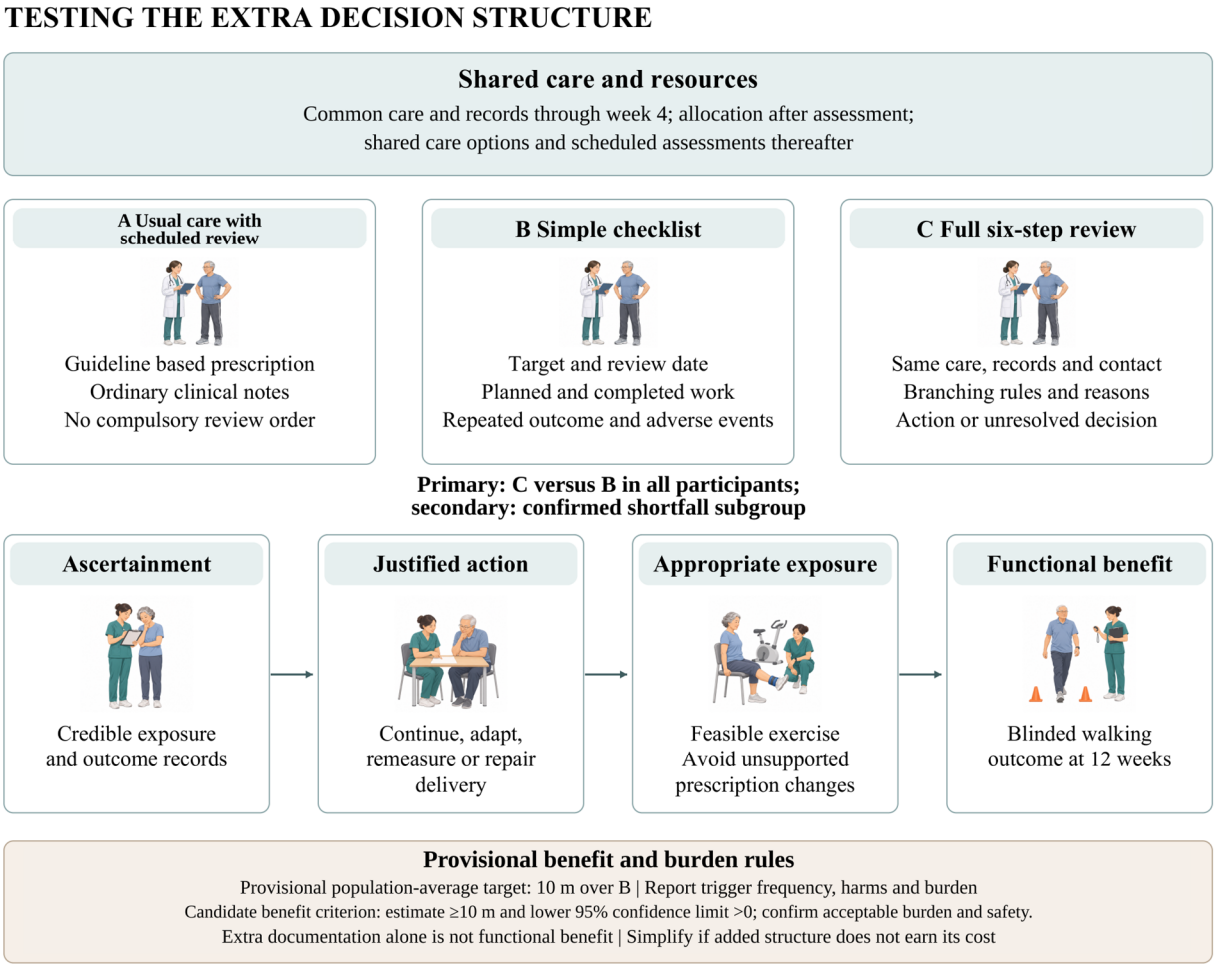


Figure 2. Testing the incremental value of the proposed decision process. A common four-week care and assessment period precedes service allocation and the introduction of the assigned review. All

arms share clinical guidance, care options, source-data access, equipment and scheduled assessments. The primary C-versus-B contrast concerns all enrolled participants; a prespecified secondary contrast concerns those with a confirmed shortfall before allocation. Trigger frequency and subgroup effects inform population-level planning. The arrows represent hypotheses, not validated effects. The 10 m worthwhile increment and staff-time allowance are provisional planning choices requiring justification before registration. CorelDRAW Graphics Suite 2026 (v27.2), Adobe Photoshop 2026 (v27.11), and ChatGPT (OpenAI, GPT-6) were used in the development and design of this figure.

7.3. When to Reject or Simplify the Full Review

The following decision criteria would be finalized before definitive-trial registration. If the upper limit of the two-sided 95% confidence interval for the primary C-minus-B contrast were below 10 m, the candidate worthwhile population-average benefit would be ruled out at that precision, although a smaller benefit could remain possible. A point estimate of at least 10 m together with a lower confidence limit above zero would meet the provisional benefit criterion. Other results would provide insufficient evidence for adoption under this rule; failure to meet the criterion would not establish equivalence or absence of any effect.

Meeting the provisional criterion would support consideration of implementation and further evaluation only if harms, participant burden and resource costs were acceptable and the upper confidence limit for additional staff time did not exceed the agreed allowance. An interval extending below 10 m would still leave uncertainty about whether the true benefit exceeded that threshold, even when the provisional criterion was met. A lower confidence limit above 10 m would provide stronger evidence of worthwhile benefit, but would not be a mandatory requirement. Broad adoption would require confirmation across service configurations. A favorable subgroup result would not, by itself, override an unfavorable primary service-level result.

A subsequent simplification study could remove extra decision fields while preserving the basic exposure and outcome record. A noninferiority comparison would be justified only after credible evidence of benefit from the full review over the brief checklist. Its margin would be specified from the established benefit, the uncertainty around that benefit and the maximum loss considered acceptable by patients and services. A fixed 10 m margin is not proposed because it could permit loss of the entire candidate incremental benefit. Comparator fidelity, missing outcomes, safety, burden and alignment between the margin and estimand would require explicit assessment [104–106].

Component selection is an empirical question. Optimization approaches can test which fields or review tasks contribute to outcomes within affordability and feasibility constraints [107–109]. A field should not survive merely because it is biologically plausible or completes a conceptual taxonomy. The protocol, primary contrast, analysis, safety rules and amendments should be publicly specified before recruitment, using current trial protocol and reporting standards [100,110–112].

8. Interpretation, Limits and Implementation Costs

The procedure changes how evidence is reviewed; it does not identify every cause of poor training response. Delivery, observation and clinical problems can coexist. A low completion ratio can reflect an unrealistic plan, an appropriate clinical adaptation, inaccessible care or incomplete ascertainment. High completion can coexist with an insufficient stimulus, unsuitable target or genuine variation in benefit. The action record must retain these alternatives without assigning an unobserved cause.

The central limitation is the absence of a completed test of incremental value. More structured reasoning may reproduce good ordinary practice, displace useful clinical discussion or increase data burden. Evidence that decision aids, implementation strategies or

monitoring systems affect one process cannot establish the effectiveness of this particular six-step intervention [67,102]. A fair comparison therefore includes a competent simple checklist and equivalent care resources.

Purposive literature selection is vulnerable to selection bias and does not establish comprehensive coverage. Population-specific reliability, adherence and feasibility findings require local examination before transfer. Process evaluations and adherence research identify contextual barriers that may influence participation [60,84]. The proposed evaluation should therefore record access to monitoring, staff support and repeated assessment, and examine who contributes complete records. Missing information would remain an ascertainment problem rather than evidence of insufficient effort.

Even a favorable result from the proposed COPD trial would not establish effectiveness or justify adoption in athletic settings. The PAPE example is constructed and does not validate either the warm-up strategy or the review procedure. Sport-specific evaluation would require prospectively specified outcomes, standardized measurement, acceptable burden and replicated within-athlete comparisons where the intervention is reversible [22,75]. Evaluating the decision procedure itself would additionally require comparison with a simpler decision approach under equivalent information and resources. Repeated warm-up comparisons alone could estimate the warm-up effect, but would not demonstrate the incremental value of the six-step review. Chronic training applications would require designs that address carryover and longer adaptation periods.

This perspective focuses on decisions following exercise outcomes below expectation. Its contribution is a specified six-step review, an explicit record of the selected action and remaining uncertainty, and an evaluation designed to test its incremental value over simpler practice. The approach builds on established concepts of exercise prescription, delivered dose, internal load, and outcome measurement.

9. Conclusions

Before routine escalation after a disappointing exercise outcome, a practitioner needs enough credible information to select a justified next action. This perspective specifies six review steps and the conditions for proceeding, pausing for information or remaining unresolved. It proposes testing that structure against usual care with scheduled review and a simple checklist, using common care resources and source information. Functional benefit, uncertainty, harms and monitoring burden determine whether the extra structure is worthwhile. The review should be simplified or rejected if those comparisons do not support its added value.

Author Contributions: P.J.: conceptualization, methodology, investigation, writing—original draft preparation, writing—review and editing, visualization, and project administration. A.M.: conceptualization, methodology, investigation, writing—original draft preparation, writing—review and editing, and visualization. A.E.: conceptualization, methodology, writing—original draft preparation, and writing—review and editing. L.P.: conceptualization, methodology, writing—original draft preparation, and writing—review and editing. H.N.: methodology, investigation, writing—review and editing, and supervision. G.S.: conceptualization, methodology, investigation, writing—review and editing, supervision, and project administration. All authors have read and agreed to the published version of the manuscript.

Funding: This research received no external funding.

Institutional Review Board Statement: Not applicable.

Informed Consent Statement: Not applicable.

Data Availability Statement: No new data were created or analyzed in this study. Data sharing is not applicable.

Acknowledgments: The article processing charge (APC) will be covered by the Faculty of Kinesiology, University of Zagreb, Croatia.

Conflicts of Interest: During the preparation of this manuscript, the authors used ChatGPT (OpenAI; GPT-6) for grammatical correction and refinement of academic English. CorelDRAW (v27.2), Adobe Photoshop (v27.11), and ChatGPT were used in the development and design of the figures. All AI-assisted textual and visual modifications were critically reviewed and verified by the authors to ensure scientific accuracy, consistency with the cited evidence, and preservation of the intended scientific meaning. The authors have reviewed and edited the output and take full responsibility for the content of this publication.

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